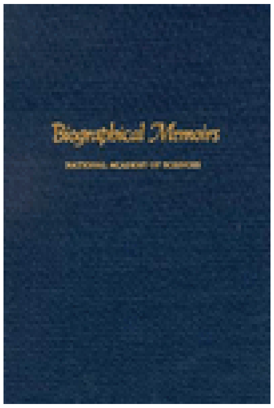

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VOLUME 66

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PREFACE

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

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PETER H. RAVEN
Home Secretary

Biographical Memoirs

VOLUME 66

Myron L. Bender.

MYRON LEE BENDER

May 20, 1924–July 29, 1988

BY FRANK H. WESTHEIMER

YRON LEE BENDER played a major role in bringing enzymology within the compass of chemistry and made outstanding contributions to our understanding of reaction mechanisms in organic chemistry and enzymology. In particular, he and his coworkers unscrambled the kinetics of the action of the serine proteases. They showed how to reconcile the rate data with a two-step mechanism for the hydrolytic process, wherein an enzyme molecule is acylated as it cleaves a peptide bond, and subsequently is regenerated when the acylated enzyme is hydrolyzed. Since the proteases constitute one of the leading systems in the study of enzymes at a molecular level, Bender's research was of great importance to the development of bio-organic chemistry.

While this body of work probably constitutes the most important of Bender's achievements, his earlier contribution to the detailed mechanism of the hydrolysis of esters would alone be sufficient to provide his work with lasting distinction.

Later, he and Koshland working independently invented a chemical procedure to convert the single serine residue in the protease subtilisin to a cysteine and thereby tested the importance of that single change in enzyme structure on the catalytic activity of that enzyme—a sort of site-directed mutagenesis prior to the time when this objective could be achieved by the methods of molecular biology. Additionally, he and his coworkers prepared interesting models for enzymic processes. He made numerous other contributions to mechanistic organic chemistry and mechanistic enzymology and trained an influential group of bioorganic chemists. He was honored for his elegant science, notably by election to the National Academy of Sciences, and was active and inventive throughout his life.

BIOGRAPHICAL

Bender was born and grew up in St. Louis, Missouri. He took his undergraduate degree at Purdue University and obtained his Ph.D. in chemistry there with H. B. Hass. He bore a birthmark (not unlike that of Mr. Gorbachev) on his face, an angioma that affected his circulatory system and presumably led to glaucoma. It may even have been related to the strokes he suffered late in life. But neither the birthmark, the glaucoma, nor his strokes affected his spirit, his friendships, or his originality, and his strokes interfered only temporarily with the development of his research. He never complained, and his scientific productivity was enormous.

In 1952, while he was on the staff at the Illinois Institute of Technology, Bender married Muriel Schulman. It was a splendid marriage. Muriel was a loving, loyal, and helpful wife, who accompanied him to meetings in the United States and abroad. The Benders had three fine sons who survive them; obviously he and Muriel enjoyed each other's company and that of their family. Their marriage bond was true to the end; Muriel was ill at the time of Myron's death, and survived him by only a few weeks.

ORIGINAL RESEARCH

After Purdue Bender spent a postdoctoral year at Harvard with Paul Bartlett and then won an Atomic Energy Commission postdoctoral fellowship, which he exercised in 1950 in my laboratory at the University of Chicago. He arrived with an original research plan—a method to test for the reality of the tetrahedral intermediate that had long been postulated in the hydrolysis of the esters and amides of carboxylic acids. Prior to Bender's work the experimental evidence for this postulate was rather indirect. Bender offered a firm experimental basis for the tetrahedral intermediate. He carried out the hydrolysis of ethyl benzoate and other esters marked with ^{18}O in the carbonyl oxygen atom and showed that the remaining starting material lost label as the reaction progressed.[1] This is what would be predicted if the formation of the tetrahedral intermediate is reversible and if the intermediate is sufficiently long-lived to undergo proton transfer before decomposition. In fact, the demonstration of oxygen exchange into the unreacted ester during hydrolysis would be hard to explain without a tetrahedral intermediate. This work comes as close to a proof of mechanism as can be found in physical-organic chemistry (see Figure 1). Furthermore, the reaction is an essential one in both chemistry and biochemistry.

THE TETRAHEDRAL INTERMEDIATE

In Figure 1 the primed rate constants (e.g., k_1') for species substituted with ^{18}O and are only slightly smaller than the constants (not primed—e.g., k_1) for compounds carrying the normal isotope. Exchange of isotopically-labeled oxygen, here designated as O, into the residual, unhydrolyzed ester takes place provided that k_{-1}' (and, of course, k_{-1}) and k_2 (and, of course, k_{-2}) are not small compared to k_4 and k_4'.

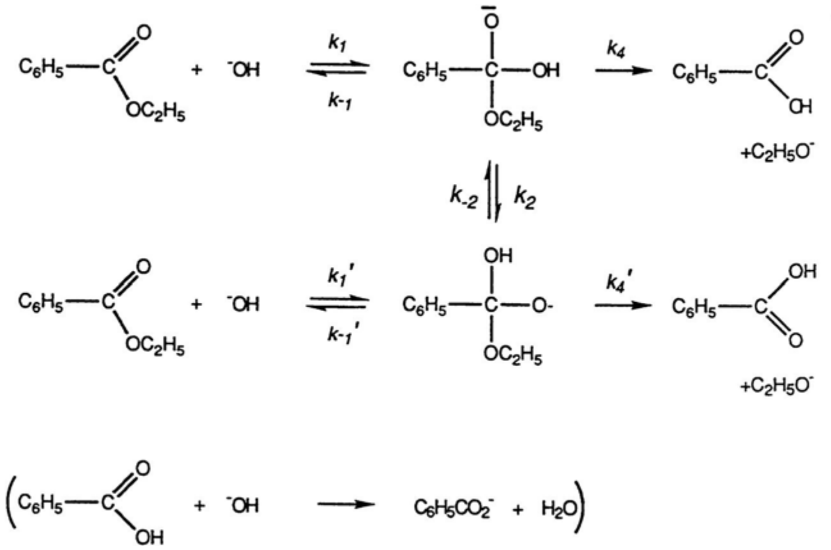

FIGURE 1 The exchange of oxygen between ester and solvent during alkaline hydrolysis.

Bender released the isotopic oxygen by pyrolysis of the ethyl benzoate to give CO_2 for mass-spectrometric analysis (see Figure 2).

This research was ideally suited to the time and place. In 1951 mechanistic chemistry was coming into its own with an interested community of physical-organic chemists ready to examine and applaud new initiatives. At the University of Chicago, Harold Urey and his collaborators had constructed an isotope ratio mass spectrometer—this was before accurate mass-spectrometers were commercially available—and Urey was willing to arrange for analyses of Bender's samples. Bender could not readily have developed his idea in many other places in the world. The project solved an important problem and brought Bender instant recognition from the community of physical-organic chemists. He subsequently enlarged the research in this area.[2-4]

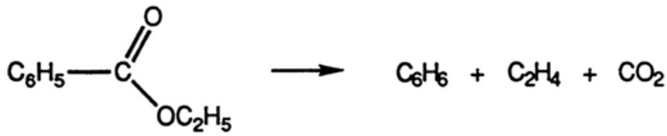

FIGURE 2 Pyrolysis of ethyl benzoate.

The conclusions of this work are correct, have largely been confirmed, and have proved of great importance in the development of physical-organic chemistry, while one must note that some of the later work[5] has not gone entirely unchallenged.[6] The data, both from Bender's laboratory and from that of his critics, show that the exchange of isotopic oxygen in many cases accompanies the alkaline hydrolysis of esters and it is reasonable that, in some instances, the ratios of rate constants in the scheme above are such as to obscure that exchange. Since the exchange does occur in a number of examples it offers firm evidence supporting a tetrahedral intermediate in ester hydrolysis. This is the point that Bender made, a point that contributed so much to the development of reaction mechanisms in organic chemistry.

ILLINOIS INSTITUTE OF TECHNOLOGY

Subsequent to this brilliant start on research Bender was appointed an instructor at the University of Connecticut. He was there for only one year. Fortunately for the progress of bio-organic chemistry, however, Bender immediately was appointed to the staff of the Illinois Institute of Technology. There he continued his investigations of the hydrolyses of esters and amides[7] and in 1954-55 published the first of his papers on the hydrolysis of esters catalyzed by alpha chymotrypsin.[4, 7]

He investigated intramolecular catalysis in the hydrolysis of esters and amides, demonstrated the imidazole catalysis[8] in the hydrolysis of *p*-nitrophenyl acetate, and investigated the enzyme-catalyzed exchange of ^{18}O between solvent and carboxylic acids.[9] Most significantly, he and his coworkers demonstrated spectrophotometrically the existence of an acyl enzyme intermediate in the chymotrypsin-catalyzed hydrolysis of *o*-nitrophenyl cinnamate.[10]

A two-step mechanism (or perhaps one should say, a two-step pathway) for the chymotrypsin-catalyzed hydrolysis of esters and amides had previously been developed on the basis of the work of A. K. Balls and Brian Harley and their coworkers. Balls[11] noted that chymotrypsin and trypsin were stoichiometrically inactivated by a nerve gas (diisopropyl fluorophosphonate). He also stoichiometrically acylated chymotrypsin by treating the enzyme at low pH with *p*-nitrophenyl acetate or *p*-nitrophenyl pivalate.[12] His work identified the specific serine residue at the active site of the enzyme.[13]

Hartley and Kilby[14] demonstrated that, when chymotrypsin acts on *p*-nitrophenyl acetate, a "burst" of nitrophenol is released that is stoichiometric with the quantity of chymotrypsin that is employed. These experiments, like those of Balls, strongly suggested that a specific hydroxyl group in chymotrypsin is acylated during enzymatic catalysis and that this hydroxyl group is regenerated when the acetyl ester of the enzyme is subsequently hydrolyzed (see Figure 3).

Bender's spectroscopic demonstration[10] that a cinnamate ester of chymotrypsin is formed during the enzymic hydrolysis of *o*-nitrophenyl cinnamate fits with, and strongly reinforces, the earlier work of Balls and Hartley. Of course, as one examines the Balls-Hartley pathway more closely, one realizes that each step in the formation or decomposition of an ester or amide presumably proceeds through a

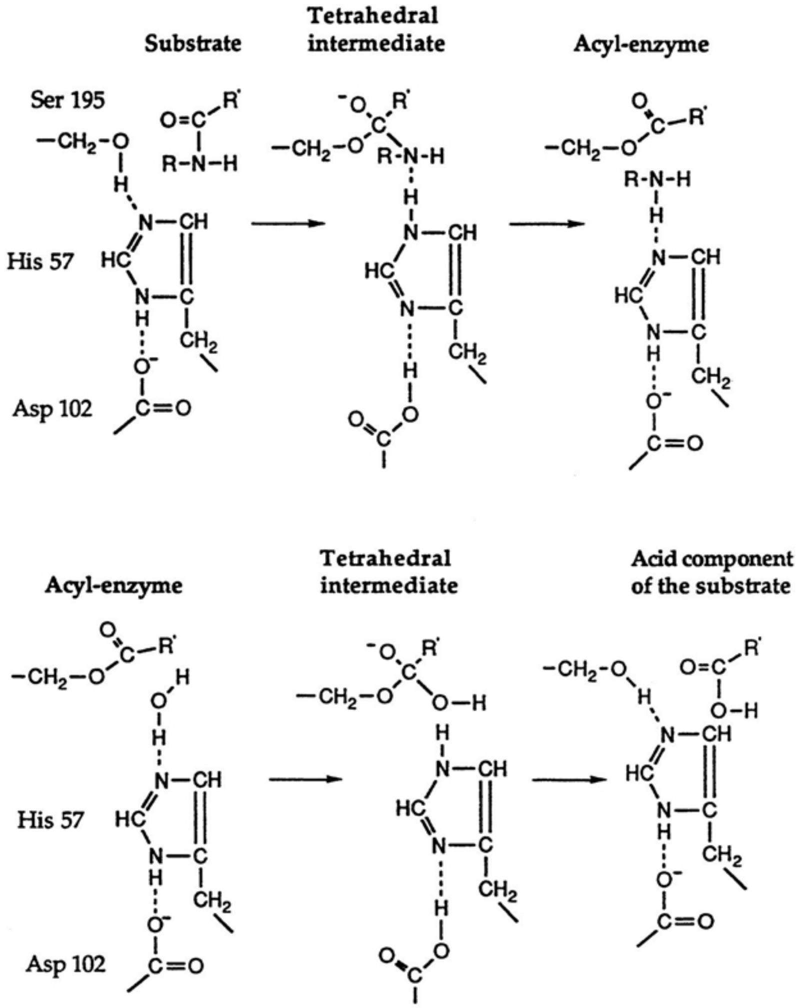

FIGURE 3 The mechanism of action of chymotrypsin.

tetrahedral intermediate of the type that Bender had demonstrated for the non-enzymic hydrolysis of esters. However, since the nucleophile for the serine proteases is a serine hydroxyl group rather than a water molecule, the type of experiment that Bender invented cannot be applied to the enzymic processes. One must accept the tetrahedral intermediate by analogy, rather than demonstrate it by experiment.

Today many more details of the mechanism are known. In particular, the participation of a histidine residue as a base in the mechanism (as shown in Figure 3) comes from the work of Shaw and his coworkers,[14] who showed the *N*-tosylphenylalanyl chloromethyl ketone reacts stoichiometrically with a histidine residue of the enzyme. The histidine serves to pull a proton from the hydroxyl group of the essential serine residue, and thus makes it much more nucleophilic. Subsequent X-ray crystallographic studies[16] confirmed in every detail the mechanism of action of the serine proteases that had been developed through a study of the chemistry and kinetics (see below) of the process, and disclosed an additional feature—the participation in the active site of an aspartate residue along with those of histidine and serine. The function of the aspartate is apparently to form a hydrogen bond to the N-H proton of the histidine and make it more nucleophilic.

NORTHWESTERN UNIVERSITY

In 1960 Bender was appointed an associate professor at Northwestern University and was soon promoted to professor. In the few years after he was appointed he and his coworkers—in particular Burt Zerner and F. J. Kézdy—put the mechanism of action of the serine esterases on a firm footing.

KINETICS

Zerner and Bender[17,18] reconciled the reaction kinetics for the action of the serine esterases with the two-step pathway shown in the equations above. A vast body of kinetic data had been published[19] on the enzymatic hydrolyses of esters and amides, but attempts to interpret these data had served only to confuse the literature. The two-step mechanism for the enzymatic hydrolysis of esters and amides that had been suggested by Balls' labeling experiments and Hartley's "burst" experiments could, however, be confirmed by reaction kinetics. Scientific theory is always best established when the same conclusion can be obtained in two or more entirely different ways. The importance of Bender and Zerner's kinetic analysis can scarcely be overestimated. The serine esterases/peptidases occupy a special place in the history of mechanistic enzymology, and these kinetics are essential to the understanding of these processes. Some scientists today write as if all the mechanisms of enzyme action were established by X-ray crystallography. With respect to the serine proteases—perhaps the most important example of mechanistic enzymology—X-ray crystallography largely confirmed what had already been established by protein chemistry and enzyme kinetics.

The kinetic analysis to the two-step mechanism led to the equation shown below:

$$E + S \underset{k_{-1}}{\overset{k_1}{\rightleftarrows}} ES \xrightarrow{k_2} \underset{+P_1}{ES'} \xrightarrow{k_3} E + P_2$$

Here P_1 is ammonia, an alcohol, or a peptide. P_2 is an acylated amino acid or peptide residue. E is the serine esterase or peptidase. ES is a Michaelis complex and ES' is the acylated enzyme.

$$k_{cat} = \frac{k_2 k_3}{k_2 + k_3} \text{ and } \mathrm{K_M} = \mathrm{K_S} k_3 / (k_2 + k_3)$$

$$\text{where } \mathrm{K_S} = \frac{k_1}{k_{-1}}$$

These equations can be simplified when either the first step (acylation of the enzyme to form an acylated enzyme as intermediate) or the second step (hydrolysis of this intermediate) is clearly rate-limiting.

For amides where $k_3 \gg k_2$, $k_{cat} = k_2$, and k_2 is different for and characteristic of each substrate; further, $K_M = K_S$ (binding constant).

For esters, however, where $k_2 \gg k_3$, $k_{cat} = k_3$, and k_{cat} is therefore the same for all esters of any particular acid, while K_M is smaller than K_S, and does not represent the binding constant of substrate to enzyme. The predictions from these equations could be tested from the mass of data that had already been accumulated and from specific experiments that Bender and Zerner designed to test them.

In particular, the formulation predicts that, at saturating substrate concentrations, all esters of *N*-acetyltryptophan, for example, will react at the same rate (i.e., the rate of the hydrolysis of the chymotryptic ester of acetyltryptophan). In other words, the value of k_{cat} will be the same for all these esters. On the other hand, the Michaelis constant will correctly represent the affinity of the substrate for the enzyme. When, however, the first step is rate limiting (as in the hydrolysis of the tryptophanyl amide) each substrate will react at a different maximal velocity and the Michaelis constant will correctly represent the binding of substrate to enzyme.

Bender and his coworkers demonstrated that the abundant data for the hydrolysis of various substrates by chymo trypsin accord with these conclusions. Some of the data for derivatives of *N*-acetyltryptophan are assembled in Table 1. Many more data are in the original papers.

TABLE 1 N-Acetyl-L-Tryptophan Derivatives

	$k_{cat'}$ sec^{-1}	$K_M \times 10^5$, M
Amide	0.026	730
Ethyl ester	26.9	9.7
Methyl ester	27.7	9.5
p-Nitrophenyl ester	30.5	0.2

Note that the ethyl ester and the *p*-nitrophenyl ester of acetyl-tryptophan are hydrolyzed with the same rate constant, but with vastly different Michaelis constants, whereas the amide reacts much more slowly, but with a much larger Michaelis constant. These data are consistent with rapid reaction of esters with the enzyme, followed by rate-limiting hydrolysis of a common intermediate, whereas the amide is hydrolyzed with rate-limiting acylation of the enzyme.

Bender had previously provided spectroscopic evidence for the formation of an acetylated enzyme with unnatural substrates. Now Kezdy and Bender[22] answered some of the last objections to this mechanism when they demonstrated spectroscopically the formation of an intermediate with natural substrates and showed that the decomposition of the acyl-enzyme intermediate occurred at a rate consistent with that calculated by the kinetic scheme of Bender and Zerner. It was primarily on the basis of these studies of enzyme mechanism that Bender was elected to the National Academy of Sciences in 1968. Undoubtedly his detailed kinetic analysis of an enzyme-catalyzed reaction constitutes one of his major scientific achievements.

SITE-DIRECTED MUTAGENESIS

Along with these studies of reaction kinetics and the spectroscopic identification of acyl-enzyme intermediates, Bender made several other significant contributions to enzymology. In particular, Polgar and Bender[20] invented a kind of site-directed mutagenesis. Their methodology differs from, and is entirely independent of, modern work with nucleic acids and is much less general and convenient than the latter. But they accomplished their work many years before it was possible to manipulate nucleic acids, and Polgar and Bender's research, together with that of Neet and Koshland (see below), demonstrated the importance of making small changes in the structures of enzymes. Until this time enzyme mechanisms had been tested by changing the substrate; now they could also be tested by changing the structure of the catalyst.

Polgar and Bender converted subtilisin into thiosubtilisin by chemical transformation of the essential serine residue to a cysteine. The same chemistry was independently achieved by Neet and Kosland.[21] Today this type of transformation is accomplished almost routinely through synthesis and expression of appropriately modified nucleic acids. In 1968 the modification of a single amino acid in an enzyme was an important departure and illustrated the type of information about enzyme action that could be obtained by specific substitution of one amino acid by another. Thiosubtilisin turned out to be a much poorer enzyme than subtilisin—in fact, it hardly qualifies as an enzyme at all—but the research in 1966 demonstrated the importance of the precise nature of enzyme-substrate interactions.

MODEL SYSTEMS

Concurrent with his research on enzymes, Bender initi ated a series of studies of model systems. This work included the rapid internal reactions of phthalates and such molecules and internal catalytic reactions that more closely resemble enzymic action. He chose to work with cyclodextrins, whose cavities serve as binding sites for substrates. His research followed and enormously amplified the prior studies of Fritz Cramer[23] and his coworkers. Bender (and more or less concurrently, several other researchers) attached various functional groups to the cyclodextrin to catalyze the hydrolysis of esters).[24, 25, 26 and 27] Similarly, he worked with bicyclic systems, where the molecular geometry placed catalytic sites close to ester linkages, much as they must be in the active site of serine esterases.[28] In the structure shown in Figure 4, for example, Bender used an imidazole residue to simulate the histidine in chymotrypsin, and a carboxylate residue, properly placed, to simulate the aspartate in chymotrypsin.

Most models for esterases are active only in the hydrolysis of highly activated esters such as *p*-nitrophenyl esters; this model hydrolyzed an ordinary ester. It presented a fast internal reaction that effected the hydrolysis of an aliphatic ester.[22] Of course, this was not catalysis, since the reaction is stoichiometric, and is an internal process; a large number of fast internal reactions are well known. This study demonstrated that our understanding of the groups needed for catalysis is accurate and sufficient. In particular, Bender and his coworkers showed the importance of the aspartate residue in the catalytic triad of the serine esterases. They synthesized a model[28] similar to that shown in Figure 4, but lacking the carboxylic acid residue, and then showed that the rate of hydrolysis of the ester could be increased 2500-fold by the addition of 0.5 *M* benzoate ion. They thus provided a kinetic verification for the efficacy of the catalysis that had been postulated, on the basis of X-ray structure, for the aspartate residue of the catalytic triad. In this case, contrary to most of our understanding of the mechanism of action of the serine esterases, X-ray analysis preceded chemistry. The work on benzoate catalysis was carried out shortly before Bender's death and showed how active he was right up to the end of his life.

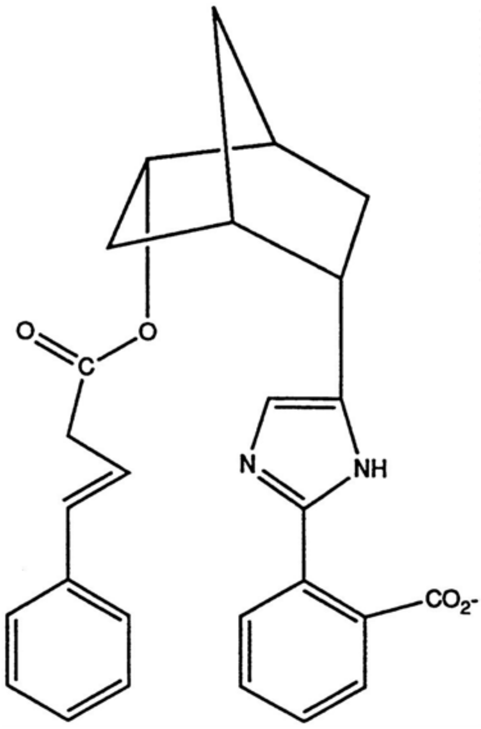

FIGURE 4 A model for a serine esterase.

In summary, the two-step mechanism for the serine esterases was postulated on the basis of good evidence by Brian Hartley and A. K. Balls and the essential serine had been identified. But the mechanism was strongly reinforced by Bender's spectroscopic studies, and a proper understanding of the reaction kinetics came directly from his work. Furthermore, the detailed mechanism and understanding of enzymic hydrolysis rest in substantial part on Bender's prior work on the oxygen exchange that accompanies ester hydrolysis. In fact, our understanding of the mechanism of enzymic hydrolysis of esters and amides comes in large part from Bender's probing experiments and critical examinations of the resulting data. All of these accomplishments distinguish Myron Bender as a major contributor to the development of bio-organic chemistry in our time.

THE WRITER THANKS PROFESSORS Jack Kirsch and Jeremy Knowles for their helpful suggestions concerning this manuscript.

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Courtesy of the University of Wisconsin, Madison

R. A. Brink

ROYAL ALEXANDER BRINK

September 16, 1897–October 2, 1984

BY OLIVER E. NELSON, JR., AND RAY D. OWEN

OYAL ALEXANDER BRINK, over a long career, was a major contributor to the development of genetics and to the improvement of major crop plants through the application of genetic principles. He is best remembered for his last major contribution, the identification and investigation of paramutation in maize—a fascinating phenomenon that contradicts the genetic axiom that contrasting alleles always segregate unaltered from their association in a heterozygous individual.

His basic contributions to genetics began with his appointment to the faculty at the University of Wisconsin in 1922 and continued for many years after his retirement in 1968. Using maize as his principal experimental organism, Alex Brink and his students demonstrated that a gene could be expressed postmeiotically in the pollen grain, reported the first explanation for semi-sterility, and mapped many mutants. In investigations of seed failure in interspecific crosses, Brink and D. C. Cooper demonstrated the critical role of the endosperm in normal seed development.

His laboratory also made early and important contributions to the study of transposable elements by showing that the unstable *P-vv* allele of maize resulted from the insertion of a transposable element, *Mp,* in a functional *P* allele and that *Mp* was the same element reported earlier by Barbara McClintock and designated as *Activator (Ac)*.

Alex Brink also had an abiding interest in agricultural productivity and the improvements that genetics could provide. He preferred to refer to these efforts as adaptive rather than applied research. Soon after his arrival as a young faculty member at the University of Wisconsin, he started the hybrid corn breeding program for the state. Subsequently, research with colleagues resulted in the breeding of a nonbitter (coumarin-free) sweet clover and the production of the first reliably winter-hardy alfalfa, Vernal, which has had great economic importance for the northern states. On the day of his death, October 2, 1984, sixteen years after his formal retirement from the faculty of the University of Wisconsin, he had spent the morning in his experimental plots endeavoring to identify the corn mutant lines that would best serve for homegrown supplementation to enhance desirable fermentation in alfalfa-hay silage. His report of this venture was posthumously published in *Maydica* (1984).

Over the sixty-three years following the publication of his first paper (1921), he made numerous other important research and conceptual contributions to basic and applied genetics. But his clear-sightedness, uncompromising standard of excellence, and leadership found equally profound expression in other spheres as well. It was his influence, more than any other, that guided the development of the Department of Genetics at Wisconsin to its early and continuing preeminence (he was chairman from 1939 to 1951), and he served the broader university community as well. He was a memorable teacher, not the kind whose disorganization and obfuscation of the subject are effective because they force students to work out confusing matters for themselves, but clear, firm, concise and thoughtful, encouraging students to extend their perceptions and knowledge on the basis of understanding he had helped them to achieve at the start.

He was a statesman in the external world of science, too, and he served genetics in many ways, for example as president of the Genetics Society of America in 1957, as president of the American Society of Naturalists in 1963, and as managing editor of *Genetics* (1952–57). He was elected to the National Academy of Sciences in 1947 and to the American Academy of Arts and Sciences in 1960. He became professor emeritus at Wisconsin in 1968. That was not, for him, a signal that he had retired, but that he had more time for intense research. At one point he wrote, “One of my reactions to advancing age has been an increasing reluctance to sacrifice a diminishing future by becoming preoccupied with the past.” He participated in the meetings of the genetics societies of America and Canada in Vancouver in August 1984—less than two months before his death—and, upon receiving the Morgan Medal there, he delivered a brief statement in part relating his work, begun some thirty-five years earlier, to the still-challenging problem of “the meaning of transposable elements for make-up of the chromosomes.”

Born of generations of farming people on a dairy farm near Woodstock in Ontario, Canada, Alex attended a one-room country school and, at age eleven, passed the selective written entrance examination for high school, the Collegiate Institute in Woodstock, a school with a rigid curriculum and exacting standards. Up early to help with milking and farm chores, he walked to the railroad for the five-mile ride to school. After school he headed back to the farm, where chores left him drowsy and fatigued. He neglected his homework, and his instructors' disfavor took the form mainly of ignoring him.

At the end of the first year he failed the final test and dropped out of school to work on the farm. He was then only twelve years old. Fortunately, promotion in school was not based on attendance or term work, but on passing final exams in each subject at the end of the year. When he reentered high school, his father excused him from morning and evening chores two weeks before the examinations, and he crammed for the tests. He regularly passed them. To the surprise of his instructors, he gained first-class honors in the provincial examinations at the end of both the second and fourth years. “There was no joy and little satisfaction in my high school experience,” he wrote later. “I was taking it for granted that eventually I would enter college, although no one was encouraging me to do so.”

He registered at the Ontario Agricultural College in Guelph in 1914, just after the start of World War I and near his seventeenth birthday. Many of his classmates enlisted in the Canadian army in their freshman year, but in November 1914 he contracted typhoid fever, which was misdiagnosed at first as appendicitis. He barely survived the mistaken surgery and acute infection when thrombosis in his left leg kept him on crutches for a slow recovery and physically unfit for military service.

He returned to Guelph in February 1915, managed to catch up, and completed his freshman year. He graduated in 1919 in chemistry and physics, ranking second in a class of twenty-four.

During the summer, he worked for the Chemistry Department at a soil experiment station, for the Physics Department surveying farms for tile drainage, and as an assistant to two county agricultural representatives in northern Ontario. In his spare time during the last summer before his senior year he made, on his own initiative, a field study of the glacial geology and soils of the Kaministikwai valley. During Christmas vacation he visited Cornell University to inquire about graduate work in soil science. Members of the staff encouraged him to apply, but the university ruled that his undergraduate degree would not qualify him for full graduate admission. He was in debt and could not afford to make up an academic deficiency. Following a summer course in the milling and baking laboratory of Ontario Agricultural College, he took a job in the testing laboratory of Western Canada Flour Mills in Winnipeg.

At Winnipeg he was given charge of mapping the milling and baking quality of the wheat crop in Manitoba, Saskatchewan, and Alberta, and was responsible for quality control of the 5,000 barrels of flour made daily. The work was "interesting, instructive, and rather well paid," but he felt it "unlikely that it would continue to be satisfying as the cycle of operations was repeated with each successive wheat crop." Besides, his desire for further academic study continued, and he had developed a compelling interest in plant physiology and genetics. He concluded that the University of Illinois had the courses he wanted. Illinois would accept the Ontario Agricultural College degree, and there was no out-of-state tuition fee, an important consideration because he had only his salary savings, and the Canadian dollar was heavily discounted in the United States.

He registered in 1920 in agronomy, with C. F. Hottes, a plant physiologist in the Department of Botany, as his professor. He developed a close association that year with J. A. Detlefesen, a Harvard graduate who had studied with W. E. Castle and who taught genetics in the Animal Husbandry Department at Illinois. Brink remembered Detlefesen as "a superb teacher. His first semester course in general genetics was one of the most informative and stimulating in my experience." In his second semester he did research with Detlefesen on selection for heritable change in frequency of recombination in *Drosophila* and decided that plant ge netics was to be his field. Detlefesen wrote to E. M. East at Harvard about him, and Alex accepted "with alacrity" an appointment as an Emerson Fellow with "a stipend that exceeded Harvard's annual tuition fee by $50.00."

He entered East's laboratory at the Bussey Institution as a candidate for a Harvard D.Sc. degree in June 1921, and he worked there through the summer and the following academic year and summer. Developing methods for the cultivation of pollen on artificial media, he applied his techniques to the study of physiological effects of the waxy gene in corn and its action as an agent of control in development. His D.Sc. degree was awarded in 1923.

In 1922 he accepted an appointment as an assistant professor of genetics at Wisconsin. In the same year, he married Edith Margaret Whitelaw. The couple had one son, Andrew W. Brink, now a professor of English at Trinity College, University of Toronto, and an adopted a daughter, Mrs. Margaret Alexandra Ingraham. Following the death of Mrs. Brink in 1961, Alex Brink married Joyce Hickling in 1963.

Very soon after his arrival at Wisconsin, Brink became aware of some disappointment with the Department of Genetics on the part of others at the university. The department had been founded in 1910 in a college devoted to the principle of fostering science in the interest of agriculture. It was to be a core department contributing to, as well as using, a broad pool of knowledge of the developing science of heredity—the first department of its kind in the United States. L. J. Cole, its first professor, had served alone from 1910 to 1919, when E. W. Lindstrom joined the faculty as a plant geneticist. Lindstrom left to found a new department at Iowa State, and Brink was his replacement.

Devoted as he was to the highest standards of basic research, Brink recognized that the other aspect of the de partment's obligation, use of a broad pool of knowledge in the interest of agriculture, was not being competently served. He knew, from earlier associations with East, G. F. Shull, and D. F. Jones, about the promise of hybrid corn, a technology that was beginning to find application elsewhere in the Midwest, but had been resisted at Wisconsin. “I resolved in 1923,” he wrote later, “to give first priority to getting a hybrid field corn breeding program started at Madison.” He achieved the close cooperation of the departments of agronomy and, later, plant pathology, and by 1925 the program was well under way, funded by a grant from the Purnell Act. N. P. Neal, who as a graduate assistant under Brink had become associated with the program, was given leadership in 1931, and Brink left the successful project in his competent hands. This program profoundly affected Wisconsin agriculture and forever dispelled doubts about the ability of genetics to serve farm interests. An informative and interesting narrative of the Wisconsin development can be found in *The Hybrid Corn Makers* (1947).

In 1926 Brink initiated a program of alfalfa breeding that was to absorb much of his attention for a quarter of a century. It had been widely recognized that if dairy farmers could grow a high-protein roughage like alfalfa, their feeding cost could be reduced substantially. But the crop was unreliable, mainly because of winter killing; in 1928, for example, two successive severe winters had reduced the hay acreage by more than one-third from the 1926 level. With his background in soil science and a concern for soil conservation that continued throughout his life, Brink felt strongly that a productive sod crop like alfalfa was a primary requirement in any general scheme to conserve Wisconsin farm soils. He joined a group stimulated by the passage of the Soil Conservation Act during the Roosevelt administration, which was seeking to open the way to a new era in land use practices. In addition, attention had to be given to the development of bacterial wilt-resistant strains of alfalfa. Brink had earlier been impressed with the amount of genetic variability in available varieties. Little real genetic work had been done with the plant, however. Later he wrote, "The application of genetic principles to breeding alfalfa for greater winter hardiness and disease resistance gave promise not only of contributions to the solution of a major agricultural problem, but also of providing opportunity for exploratory efforts in basic plant genetics research. The balance would be shifted toward the latter if the preliminary attempts to establish myself in the field were encouraging."

His efforts proved conspicuously successful in every respect. There was a series of papers on the genetics of alfalfa—resistance to bacterial wilt, the mechanism of pollination, embryo mortality in relation to seed formation, and differential survival of strains under an ice sheet. It was from the survivors of this icy encapsulation that the breeding material was selected for the program that culminated in the release for seed increase in 1953 of the winter-hardy, wilt-resistant variety, Vernal. It was soon widely grown in the north-central United States and in Canada and by 1971 had added a yearly average of $20 million to the value of alfalfa in Wisconsin. Further development by Dale Smith, of the Department of Agronomy, made three cuttings regularly possible per year, and the total gain to Wisconsin's farmers from Vernal alfalfa in the interval between 1954 and 1971 was about $1 billion. Vernal became a leading variety of alfalfa in Canada as well as in northern United States and represented, in Brink's own mind, his major direct contribution to agriculture.

There was another important, nongenetic consequence of this period of dedication to alfalfa improvement. In 1932 the dean of the College of Agriculture, C. F. Christiansen, invited his faculty to propose projects that could use federal unemployment funding to accomplish objectives useful to Wisconsin agriculture. Brink, aware through his alfalfa breeding of a need throughout Wisconsin for liming soil on which alfalfa could be grown, suggested that the college sponsor a work relief program through which agricultural lime could be made available at cost to farmers. The suggestion was taken up with enthusiasm; projects were set up in 1933, but very shortly they encountered legal obstacles. District attorneys ruled that counties could not engage in business in competition with private industry.

In the face of some administrative reluctance to “rock the boat,” Brink on his own initiative obtained the help of Paul Raushenbush, then an administrative officer for the Wisconsin unemployment compensation program, in arranging for the drafting of a bill to be submitted to the state legislature to authorize county boards to engage in the production and sale of agricultural lime as an unemployment relief measure. The bill passed and became law in June 1933. A statewide program was then inaugurated; by 1934 more than one thousand projects had been approved, and nearly ten thousand workers were employed in the production of lime and marl and in the hauling of paper mill sludge. By 1935 about 840,000 tons of these materials had been made available to farmers at less than half the usual cost. With further extensions about thirty million tons of agricultural lime were distributed in Wisconsin under the Federal Emergency Relief Administration and the Works Progress Administration relief measures, and county programs continued after the work relief support had been discontinued. The contribution of these programs to the productivity of Wisconsin agriculture is immeasurable.

In the early 1930s sweet clover could be recognized as a potentially useful forage crop that had, however, two compelling disadvantages. First, it was relatively unpalatable, with a bitter, stinging taste; second, it was often poisonous when "spoiled." In 1933 Brink discovered a nonbitter strain in a species from China (*Melilotus dentata*), which although markedly deficient in its adaptation to Wisconsin conditions, became involved as the starting point in two major lines of sweet clover research at Wisconsin. One line of investigation, of which W. K. Smith became leader, aimed to incorporate the nonbitter trait into the more useful forage species, *M. alba.* This was accomplished, although not without difficulty, since the seedlings resulting from the interspecific cross were chlorophyll-deficient and soon died. Grafts of these seedlings onto plants of a third species, *M. officinalis,* allowed one scion to progress to the flowering stage so that it could be backcrossed by *M. alba.* One of the seven plants resulting from this backcross proved to be heterozygous for the nonbitter trait, so a strain of *M. alba* that was homozygous for this characteristic could be established.

The other line of research had its origin in somewhat earlier observations by others: that spoiled sweet clover hay produced a blood-clotting deficiency in animals that ate it. In 1938 Brink and Smith published the results of a study showing that bitterness of the fresh forage and toxicity of spoiled sweet clover hay had a common basis in coumarin (1938). It was in following up that clue that K. P. Link and his associates in biochemistry were able to isolate the hemorrhagic agent Dicumarol and to implement its application in such diverse contexts as an anticoagulant in human surgery and, through a derivative trade-named Warfarin, as a rat poison. Link was a colorful and dynamic personality, often dramatic in his oral presentations. As a result, some popular misconceptions about the origins of this aspect of the research became prevalent in the public press; Link's Harvey Lecture (1944) is more accurate.

Over the same interval that his contributions to adaptive research and service were coming to fruition, Brink's explorations at the forefront of basic genetic knowledge also proceeded at a remarkable pace. There followed, during 1924–29, a series of pioneering papers on gene expression in, and environmental effects on, the development of the pollen grain, especially in corn, including the demonstration (contemporaneously with M. Demerec) that the *Wx* allele was activated postmeiotically (1924). Several of the major papers were published in *Genetics*; his review provides a clear and thoughtful summary (1929). In 1927 he described the first case of semi-sterility in maize. C. R. Burnham, who had taken his Ph.D. with Brink, observed in 1930 that the semi-sterility involved a reciprocal translocation. Brink and Cooper (1931) confirmed the cytological observation, identified markers for the linkage groups involved in the translocation, located the translocation breakpoints, and characterized the recombinants in terms of their cytological and genetic behavior. Early in 1935 they published a confirmation of the 1931 reports by Creighton and McClintock in corn and Stern in *Drosophila* of the relationship between genetic crossing over and physical exchange between homologous chromosomal segments (1935).

Over the years Brink, his students, and collaborators contributed substantially to the building up of linkage maps in corn. With his long-standing interest in the developmental genetics of seeds, it was natural for him to make use of the advantages of species other than corn. One was, of course, alfalfa. He became interested in the phenomenon of frequent early seed collapse, especially in relation to self-pollination as compared with cross-pollination. Careful study indicated that this type of seed failure was attributable to inadequacy in what emerged as an essential role of the endosperm. The angiosperm ovule, characteristically low in food reserves at fertilization, depends on the translocation of nutritive materials in which the endosperm plays an essential role. The generality of seed failure owing to abortion of the endosperm was established in studies of a large number of plant genera. The work, done mainly with Cooper, culminated in a large, historic monograph on *The Endosperm in Seed Development* (1947). Extending the techniques he had earlier invented for the cultivation of pollen in vitro, Brink and his collaborators set out to cultivate immature excised embryos on artificial media. The work with barleyrye hybrid embryos was especially rewarding. The hybrid embryos regularly died during ordinary development, but when they were dissected from the seeds and grown on an artificial nutrient medium they could survive through the seedling stage to maturity. They found provocative indications, too, of at least one “embryo factor” that, when added to the medium, promoted the growth of otherwise abortive immature embryos.

As early as 1948 Brink had initiated studies on unstable alleles, especially variegated pericarp *P-vv* in corn, and in 1952 he interpreted the instability as resulting from a discrete element, *Modulator (Mp)*, that inhibits pigment formation when present at the *P* locus, with mutation involving transposition of *Mp* away from the *P* locus and its assortment to offspring as a unit separate from that locus (1952). The postulated process was similar to that reported by McClintock in 1950 for the *Ac/Ds* transposable element family, and two years later Barclay and Brink (1954) reported that *Mp* was operationally indistinguishable from McClintock's *Ac.* In the 1950s detailed and elegant analyses of *P-vv* showed that when *Mp* excised from the *P* locus, it was most frequently reinserted in the same chromosome and at a loca tion closely linked to *P*. From these closely linked locations reinsertions of *Mp* into *P* at apparently different sites can easily be isolated, thus constituting in effect almost a locus-specific mutagenic system. Greenblatt and Brink (1969) then showed that the locations of transposed *Mp's* were in accord with the hypothesis that an *Mp* excised from *P* during replication of the *P* locus moved to an unreplicated portion of the same chromosome and then was replicated in phase with that segment of the chromosome.

During the late 1950s the focus of the laboratory shifted almost entirely to a study of the intriguing phenomenon of paramutation at the *r* locus in maize, which Brink first reported in 1956. A functional allele at the *r* locus is required for the production of anthocyanin pigments in the aleurone layer of the endosperm and in some tissues of the plant. In paramutation, *R* alleles of one class (e.g., the standard *R-r* allele) are predictably and markedly reduced in their pigmenting potential following recovery from heterozygotes with a second class of *R* alleles of which the unstable *R-st* allele is a prime example. The *R-st* allele is completely unaffected. The affected alleles (paramutant or *R′* alleles) can revert partially toward their original pigmenting potential under certain conditions, hemizygosity or heterozygosity with a null allele. As in the case of *P-vv* a meticulous series of investigations by Brink and his students elucidated many of the genetic aspects of paramutation. A 1973 review by Brink summarized these investigations.

Alex Brink lived to see the first results of investigations into the molecular structure and behavior of the transposable elements, and he followed these with interest. It will not be long before there are also molecular explanations for paramutation.

On leave in 1960 to work at University College, London, and at Oxford, Brink collected and expressed his thoughts on a broader developmental-genetic subject in a remarkable review titled "Phase Change in Higher Plants" (1962). The primary phenomenon reviewed was the abrupt switch in potential of perpetually embryonic meristems from a juvenile to an adult type of growth. One observes such changes in the needles of juniper trees and the shape of ivy leaves.

Phase change in higher plants involves an alteration during development whereby one type of growth along an axis is succeeded more or less sharply by another that contrasts sharply with the first. . . . This is not a unique ontogenic phenomenon; similar distinctive alterations, manifested in a wide variety of ways, also occur among animals and other classes of plants. . . . There are significant reasons of a general nature for thinking that the chromosomes embody an apparatus for the primary regulation of gene action during development. . . . The point of view here developed leads to the suggestion that the chromosome characteristically serves what may be called a paragenetic, as well as a genetic, function. . . . The chromosome, assumed to carry an unchanging complement in somatic cells, also possesses other, more labile components by which gene action is regulated during development. . . . The "individuality" of a somatic cell is implicit in the paragenetic state of the chromosomes present. . . . Future investigation will show whether the assumption of an additional link, involving chromosome substances with paragenetic properties, and the beginning of the chain between gene and end product, will aid in reversing the trend recognized as separating the study of heredity from that of development, and so putting the two disciplines on convergent paths.

These thoughts were too far ahead of their time to be widely appreciated, and in any case, their realization depended on later definition in molecular terms by others. His use of the term "paragenetic" here was meant to cover a broader context than "paramutation," which he coined to define a specific phenomenon.

Over much of this period he was devoting time and effective attention to the development and guidance of his department and to the affairs of the university. The accomplishments in this context, of which he was most proud in the end, were the recruitment of young Joshua Lederberg to his faculty, the subsequent formation of the Department of Medical Genetics, and the eventual unification of the departments of genetics and medical genetics into the Laboratory of Genetics. Fifty-seven students over the years completed their Ph.D. training under him, some of them as joint majors with other faculty members. He described the deepest satisfaction in his academic life as seeing "a graduate student effectively launched on a rewarding professional career," and a good many students in addition to those who claimed him as their major professor had him to thank for that concern.

WE HAVE MADE EXTENSIVE use of a brief autobiography written by Alexander Brink in the last years of his life. A copy of this autobiography was given to each of us by Dr. Brink. We are indebted also to Mrs. Joyce Brink for other information.

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S. C. Chandler

SETH CARLO CHANDLER, JR.

September 16, 1846–December 31, 1913

BY W. E. CARTER AND M. S. CARTER

ETH CARLO CHANDLER, JR., IS best remembered for his research on the variation of latitude (i.e., the complex wobble of the Earth on its axis of rotation, now referred to as polar motion). His studies of the subject spanned nearly three decades. He published more than twenty-five technical papers characterizing the many facets of the phenomenon, including the two component 14-month (now referred to as the Chandler motion) and annual model most generally accepted today, multiple frequency models, variation of the frequency of the 14-month component, ellipticity of the annual component, and secular motion of the pole. His interests were much wider than this single subject, however, and he made substantial contributions to such diverse areas of astronomy as cataloging and monitoring variable stars, the independent discovery of the nova T Coronae, improving the estimate of the constant of aberration, and computing the orbital parameters of minor planets and comets. His publications totaled more than 200.

Chandler's achievements were well recognized by his contemporaries, as documented by the many prestigious awards he received: honorary doctor of law degree, DePauw University; recipient of the Gold Medal and foreign associate of the Royal Astronomical Society of London; life member of the Astronomische Gesellschaft; recipient of the Watson Medal and fellow, American Association for the Advancement of Science; and fellow, American Academy of Arts and Sciences. Considering this prominence, one might ask why it is just now, three quarters of a century after his death, that Chandler's biographical memoir is being written. This is actually two questions: “Why was it not written many years ago by a contemporary?” and “Why write it now, so many years after his death?”

Unfortunately, the answer to the first question is probably related to certain controversies in which Chandler became involved. Chandler's formal education reached only graduation from high school and he had virtually no theoretical background in astronomy or physics. However, he was a talented observer and an extraordinarily adroit computer, and he reported his observational and computational results with total disregard for conflicting accepted theory. As associate editor and later editor of the *Astronomical Journal,* Chandler had little difficulty publishing and often included extensive commentaries in his technical papers. Chandler's comments undoubtedly proved particularly irritating to certain individuals simply because of his close association with Benjamin Pierce, B. A. Gould, and A. D. Bache. Just a few decades earlier these three scientists had joined forces in a highly publicized dispute over an attempt to develop a national observatory that ended in failure and left many personal animosities (James, 1987).

The answer to the second question (Why now?) is more certain and pleasant. The recent development of very long baseline interferometry (VLBI) has improved the measurement of Earth orientation, including polar motion, length of day, universal time (UT1), precession, and nutation by two orders of magnitude. New information about the interior structure of the Earth, motions of the plates that form the surface of the Earth, and improved understanding of the interactions among the oceans, atmosphere, and solid Earth have been derived from the highly accurate VLBI observations (Carter and Robertson, 1986). But contemporary researchers using high-speed digital computers and analysis techniques not even known in Chandler's day have found it difficult to develop a better model of polar motion. Recognition of the sheer volume of the computations that Chandler performed by hand and the completeness with which he was able to characterize the complexities of polar motion (not to mention the vast quantities of computations in his research of variable stars, comets, and minor planets) has brought a renewed appreciation of his achievements (Mulholland and Carter, 1982; Carter, 1987). His work has clearly withstood the test of time, and the minimal documentation afforded by this biographical memoir is long overdue.

BIOGRAPHICAL INFORMATION

Seth Carlo Chandler, Jr., was an eighth-generation American born in Boston, Massachusetts, on September 16, 1846. His father was a member of the firm of Roby and Company, dealers in hay, coal, and other produce. Seth Carlo, Jr., was one of six children. He attended the English High School at Boston, graduating in 1861. During his last year in high school Chandler became associated with Benjamin Pierce, of the Harvard College Observatory, for whom he performed mathematical computations. Upon graduating he became a private assistant to B. A. Gould, one of the best known American astronomers of that time. Gould was assisting the U.S. Coast Survey in developing improved procedures for the determination of astronomic longitude, and in 1864 Chandler joined the survey as an aide.

In 1866 Chandler was assigned to an astronomic survey party, where he served as an observer and performed computations. He participated in a historic determination of the astronomic longitude at Calais, Maine, in which the new trans-Atlantic cable was used to relate the local clock to the master clock at the Royal Greenwich Observatory, England. His party also traveled by ship to New Orleans to make longitude determinations, again using telegraph signals to synchronize the local clock with the Coast Survey's master clock. It was an exciting period in geodetic astronomy and the young Mr. Chandler had the opportunity to learn the latest computational techniques, develop his observational skills, and acquaint himself with state-of-the-art instrumentation.

When Chandler fell in love with Carrie Margaret Herman, he decided to leave the Coast Survey and accept a position in New York City, as an actuary with the Continental Life Insurance Company. In October 1870 they were married and during the next six years their first three daughters were born: Margaret Herman in 1871; Caroline Herman in 1873; and Mary Cheever in 1876. Chandler corresponded regularly with his old mentor B. A. Gould, who had moved to Argentina to establish the Cordoba Observatory. With Gould's encouragement Chandler published his first technical paper, on the development of an analytical expression for computing a person's life expectancy from his current age, an alternate method to actuarial tables.

In 1876 Chandler moved his young family to Boston, where he continued his actuarial work as a consultant to the Union Mutual Life Insurance Company of Boston. In 1880 the Chandlers' fourth daughter, Elizabeth, was born. The same year Chandler renewed his association with the Harvard College Observatory, and in 1881 moved into a brand new house in Cambridge, within a short walking distance of the observatory. Three more daughters were to be born while the Chandlers lived on Craigie Street: Abbie in 1883; Eunice in 1888; and Helen Osgood in 1893. Chandler spent many of the most enjoyable hours of his life at the eyepiece of his telescope, which was mounted in a cupola atop the roof of this house. It was also from this house that he carried on the duties of associate editor of the *Astronomical Journal,* while B. A. Gould was editor, and later as editor after Gould's death. He used his own funds to help continue to publish the journal during difficult financial periods. In 1909 he turned the editorship over to Lewis Boss, but continued to serve as an associate editor.

When his father died in 1888 Chandler purchased his grandfather's place near Strafford, Vermont, and built a new summer home on the site. The Chandlers spent many relaxing times at their summer home. As a hobby Chandler designed and built model sailboats, which he raced on a small spring-fed pond on the front lawn. He relished the task of computing improved shapes for the hulls and was quite proud of his achievements.

In 1904 the Chandlers moved to a new home in the small town of Wellesley Hills, today a residential suburb of Boston, where he died on December 31, 1913. The Chandler homes in Cambridge, Wellesley Hills, and Strafford all are still standing, and the latter two are still owned by his descendants.

INVENTING THE ALMUCANTAR

While Chandler worked at the Coast Survey he used an instrument called a zenith telescope to determine the astronomical latitude and longitude of stations. This instrument was equipped with spirit (bubble) levels to reference the readings to the local vertical. Level vials with sensitivities of 1-2 seconds of arc per division were, and even today can be, quite finicky, changing sensitivity and behavior with temperature, age, stresses from mounts, and other unknown causes. Chandler set out to develop an instrument for determining astronomic latitude free of these leveling problems. His goal was to build an instrument that would automatically be aligned very precisely with the local vertical by the force of gravity.

Chandler considered two possible approaches: suspending the instrument like a pendulum, and floating it on mercury. He tested instruments of both designs, concluding that the flotation approach presented lesser mechanical problems. His next step was to have a small (45 millimeter diameter objective lens), relatively inexpensive instrument built. He was quite pleased with the performance of this first instrument, concluding from his analysis of observational data that "its accuracy seemed to be limited by its optical rather than its mechanical capacity" (Chandler, 1887). Encouraged by this success, he designed the larger aperture (100 millimeter diameter objective lens) instrument shown in Figure 1. The rectangular structure at one end of the horizontal axis was the mercury floatation bearing that kept the telescope constantly pointed to any angle of elevation set by the observer. The circle traced out in the sky when the instrument was rotated in azimuth (i.e., a small circle parallel to the horizon) is called an almucantar, and Chandler adopted this as the name of his new instrument.

By the time that the full scale Almucantar was completed Chandler had moved to Cambridge, and he immediately mounted the instrument on a pier on the grounds of the Harvard Observatory, near the main dome. During the period from May 1884 through June 1885 Chandler used the Almucantar to make latitude determinations on more than fifty nights. He carefully reduced the observations and published the resulting time series, pointing out that the values "exhibited a decided and curious progression throughout the series" for which he could identify no instrumental or personal cause (Chandler, 1887 and 1891a).

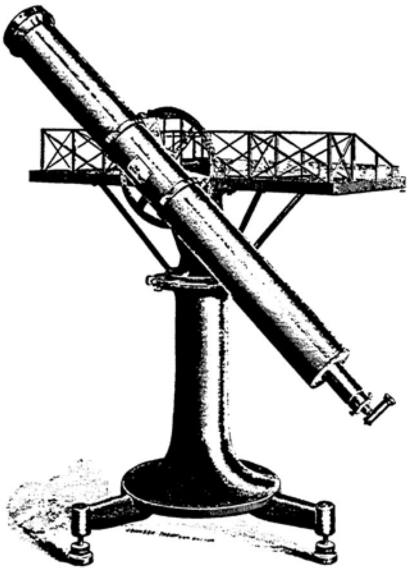

FIGURE 1. Sketch of the Almucantar used by Chandler to detect polar motion.

DISCOVERY OF POLAR MOTION

About 1765 Leonhardt Euler, a Swiss mathematician studying the dynamics of rotating fluid bodies, developed equations that suggested the Earth might wobble slightly about its axis of rotation. Such a wobble (free nutation) would result in periodic variations of the astronomic latitudes of all points on Earth. The expected period of the variation of latitude was approximately ten months. Several astronomers attempted to detect the phenomenon during the succeeding century, without success.

In 1888 German astronomer Friedrich Küstner (1888) published the results of his research on the constant of aberration, reporting that his analysis indicated that the latitude of the Berlin Observatory had changed during the period of the observing campaign. Küstner's observations were made during the same period as Chandler's Almucantar observations (i.e., 1884-85), but were not continuous enough to detect any periodicity in the variation of latitude. However, he argued strongly that the apparent change in latitude was real and his evidence was sufficiently convincing that the International Geodetic Association (now the International Association of Geodesy) organized a special observational campaign to verify his discovery. Küstner subsequently refined his analysis, finding a total variation in latitude of 0.5 seconds of arc, but giving no value of the period or direction of the motion of the pole (Küstner, 1890).

Chandler reexamined his Almucantar observations, as well as more recent observations made in Berlin, Prague, Potsdam, and Pulkova, and found a periodic variation of latitude, with a total range of about 0.7 seconds of arc and a period of 427 days, approximately 14 months (Chandler, 1891a and 1891b). The 40 percent discrepancy between the 305-day period predicted by theory and the 427-day observed period was quickly explained by Simon Newcomb as being the consequence of the "fluidity of the oceans" and the "elasticity of the Earth" (Newcomb, 1891).

There was some level of disagreement within the scientific community, which continues today, as to who should be credited with the discovery of polar motion, Küstner or Chandler. When the Royal Astronomical Society of London awarded Chandler the Gold Medal, it specifically made note of Küstner's contribution to the discovery, and many scientists (particularly European scientists) continue today to credit Küstner. However, the 14-month wobble of the pole is universally referred to as the Chandler motion, and there is no argument that Chandler illuminated the complex nature of the phenomenon, dominating the subject for decades.

LEARNING THE COMPLEXITIES OF POLAR MOTION

Chandler continued to analyze historical observations and in 1892 (Chandler, 1892a) reported that it appeared the period of the polar motion had changed, increasing from approximately twelve months to fourteen months, during the previous century. Newcomb (1892) responded: “The question now arises how far we are entitled to assume that the period must be variable. I reply that, perturbations aside, any variation of the period is in such direct conflict with the laws of dynamics that we are entitled to pronounce it impossible.” Chandler (1892b) vigorously defended his analysis, pointing out that the accepted theory had already been modified once to agree with the observations and suggesting that the new theory might still be incomplete. He continued his analysis of the observations without regard to theoretical constraints, but soon discarded the model that included a secular variation in the period of the free nutation in favor of a model consisting of two periodic components, the 427-day term and a superimposed 365-day (annual) term (Chandler, 1892c). The annual motion could easily be attributed to seasonal relocations of the masses of the atmosphere, ground water, and snow cover.

There seems to be no record of Chandler ever revealing just how he had thought to investigate a two-component polar motion model, but it might well have been triggered by the results of a study made by his old friend B. A. Gould of variations of the latitude at the Cordoba Observatory (Gould, 1892). Gould found that he could detect the 14-month variation in the latitude that Chandler had “shown to exist at other places,” only after subtracting an annual variation. He concluded that “in the absence of any indica tion as to its (the annual term's) origin, it may be attributed to instrumental causes, or to terrestrial ones.” Chandler did not refer to Gould's results in defending the reality of the annual component in the variation of latitude. Rather, he argued that it was highly unlikely that seasonal variations in temperature would affect the measurements from observatories located at nearly equal latitudes but widely differing longitudes, in just such a way as to yield a consistent phase for the annual term. And, since such was the case for several observatories located in the northern hemisphere, “We may dismiss forever the bugbear which undoubtedly led many to distrust the reality of the annual component . . .” (Chandler, 1893).

SECULAR MOTION OF THE POLE

During the very same period that Chandler was doing the laborious computations required for him to formulate his two-component model of polar motion, and to use the model to correct historical observations (a subject to which we will return later), he also became embroiled in an argument with George C. Comstock concerning the latter's claim to have detected a secular drift of the pole (Comstock, 1892). There is not space to go into the details here, but Chandler showed that the rather large secular motion suggested by Comstock, 0.044 seconds of arc per year, was simply not supported by existent observations. He concluded that any secular motion must indeed be no larger than about one-tenth of that amount. The best estimate of the secular motion today, based on more than eighty years of observations made by the International Latitude Service, is about 0.003 seconds of arc per year in a direction roughly 130 degrees different from Comstock's model, but consistent with Chandler's bound.

RE-REDUCING HISTORICAL OBSERVATIONS

The Almucantar proved to be a very accurate instrument, as evidenced by Chandler's detection of polar motion from only thirteen months of observations, but it did not represent a fundamental technological breakthrough. Optical-mechanical instrumentation of essentially equal quality had been used for astronomical observations for well over a century. Chandler was quite aware of this and rather than mounting a new observational program of his own, or waiting for new observations to become available from other sources, he spent much of his energy re-reducing existing data, deriving some remarkable results. For example, by rereducing zenith-tube observations made by the British astronomer Samuel Molyneux and zenith-sector observations made by the British astronomer James Bradley nearly two centuries earlier, Chandler was able to determine polar motion values for the period 1726 to 1731. Figure 2 is a plot of the results (Chandler, 1901c). In reporting this work Chandler recounted how Bradley had noticed anomalies in the observations that he had "unsuccessfully endeavored to trace to an instrumental source." It seems that Bradley had become concerned about a rapid change in the observed latitude between March and September 1728, and performed tests to determine if this was caused by the replacement of a wire on the plummet used to check the collimation of his telescope. From Chandler's vantage point (i.e., knowing the nature of polar motion) it was clear that the variation in the latitude had been quite real. He exclaimed, "So near did Bradley come to the discovery of the polar motion! Thus coincidentally can we now trace our knowledge of it to the same immortal work that gave us the aberration and nutation."

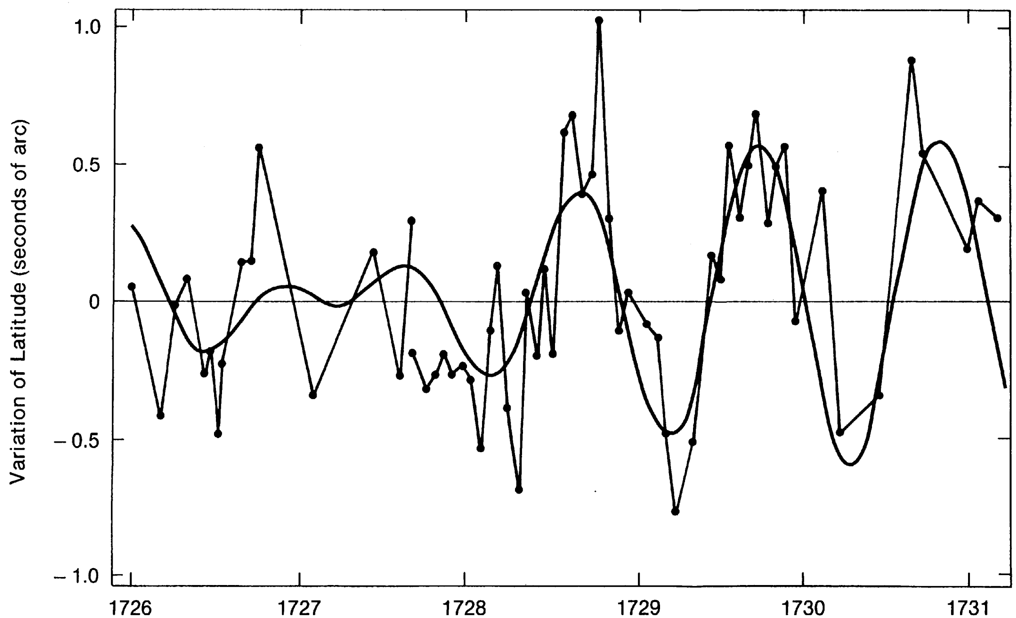

FIGURE 2. Chandler's figure showing the variation of latitude during the period 1726 to 1731, determined by rereducing observations by Molyneux and Bradley. The dots connected by straight lines are the observed values, and the smooth line is the path computed by Chandler using 14-month and annual components.

FROM HISTORICAL RESULTS TO PREDICTIONS

Chandler continued to reduce and analyze historical observations, eventually piecing together thirty-seven short series from various observatories to form a nearly continuous record of polar motion beginning in 1820. As he refined his analysis he became convinced that the 14-month motion was not a simple, single-frequency oscillation, but was itself a complex motion involving two or more components. In 1901 he announced the discovery of a 436-day component that was considerably smaller than the 428-day component, but whose reality was "beyond reasonable doubt" (Chandler, 1901a). The beating of two components of such nearly equal frequencies but disparate amplitudes would generate a very distinctive pattern in the motion of the pole. Chandler described the effect as follows: "These fluctuations are embraced in a cycle of about 57 periods, or 67 years. (The period) . . . remains during five-sixths of the cycle between its mean value and the upper limit, or between 428.5 and 431.4 days; then suddenly shortens to minimum, 415 days, and immediately rapidly lengthens. Similarly the variations of radius of motion are singularly asymmetrical." Figure 3 is a copy of Chandler's sketch in which he plotted the observed motions of the pole and the motion calculated from his model (Chandler, 1901b). Based on these findings Chandler predicted "We shall soon have a . . . test of the law in its operation on the period which . . . ought to shorten to the minimum value, 415 days, within the next few years. Of course, an accurate prediction cannot be made as to when this interesting phase will become perceptible, because the length of the harmonic cycle, which depends on the difference of the two component periods, is imperfectly defined by existing observations." Even with this caveat, Chandler had taken a bold step in predicting the occurrence of such a pronounced variation in the motion of the pole. His pre diction did not come true in 1910, nor by his death in 1913, and it was soon forgotten by the scientific community. But some fifteen years later, circa 1926, an event just such as Chandler had predicted did occur.

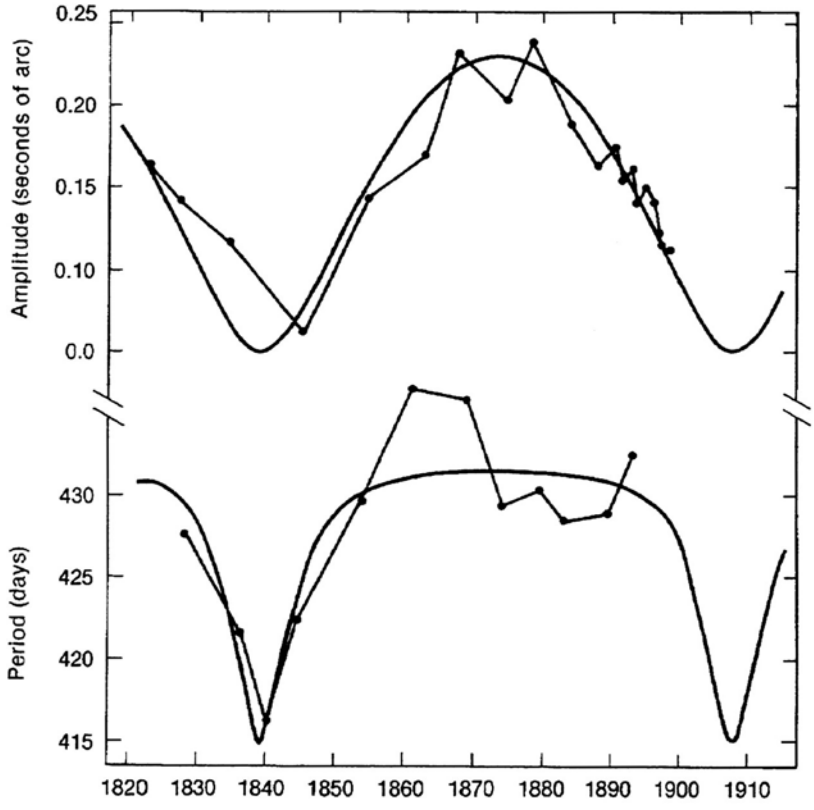

FIGURE 3. Chandler's figure showing the variation in the amplitude and period of the 14-month component of polar motion. The dots connected by straight lines are the observed values, and the smooth lines are the values computed by Chandler using 428- and 436-day components.

Based on numerical analysis of seventy-eight years of polar motion observations by the International Latitude Service, Dickman (1981) summarizes reports of a large change in the phase and amplitude of the Chandler component during the period from about 1925 to 1940. According to Dickman the behavior of the Chandler motion could not be interpreted to be the result of noise in the data, because that would have caused the phase to vary erratically, rather than in the smooth systematic manner indicated by his analysis. The observed variation in phase could be modeled as a sudden and temporary change in the Chandler frequency of 0.003 cycles per year, equivalent to a period of 418 days, but Dickman preferred to think of the motion as resulting from the beating of two components. Totally independent investigations of two disjoint (in time, instrumentation, and observing locations) polar motion time series could hardly have agreed more closely. Could it be that Chandler's prediction will yet prove to be essentially correct, if somewhat less accurate than one might hope? Ironically, if this phenomenon is periodic, it should next occur circa 2010, precisely one century after Chandler's original prediction.

VARIABLE STARS

Chandler released his first catalogue of 225 variable stars in 1888. As a rigorous observer of variable stars, discoverer of many, and enthusiastic computer of the elements of their variations, he was personally responsible for the elements of 124 of the 160 periodic stars in this, the first of three, full-scale catalogues. The second was published in 1893 and contained 260 stars. The third, in 1896, had 393 stars. Between the publication of each of the full catalogues, Chandler also published several supplements and revisions so that the newest discoveries would be available to the community as soon as possible. This work required that Chandler spend thousands of hours observing and recording his results, a task that he attacked with his usual extraordinary enthusiasm.

The rules governing the nomenclature of variable stars were still inconsistent, so Chandler introduced a blueprint of his own. In his system the designation of a variable star was obtained by dividing the number of seconds in its right ascension for the year 1900 by ten. This scheme insured that the numeration would not be disturbed by the addition of new discoveries. This system was used in all of Chandler's work, but it is not used today.

Chandler's work on cataloging variable stars led him to many new discoveries, including a correlation between their color and the period of their variability. Following clues given by Lalande and Schönfeld (Chandler, 1890), he determined that the Algol-type stars are strikingly white; the very short-period stars range from nearly colorless to yellow; and that those of longer and longer periods show a color of deeper and deeper red. In fact he noted that variable stars with a period of over 400 days consist solely of red stars. Chandler also detected correlations between the periods of the variable stars and the average range of the variations of their brightness. For periods under two months, the maximum brightness could be expected to be about three times the minimum, for periods of four to eight months, maximum brightness was about thirty times the minimum, and for periods longer than eight months maximum brightness was sixty times the minimum. But, perhaps the most important contribution Chandler made to the study of variable stars was his encouragement to other astronomers to participate in the observing program, especially in the southern hemisphere. He wrote a number of papers urging astronomers to enter this useful study, even including detailed observing instructions.

In 1894 Chandler published a paper (Chandler, 1894) contending that the photometric results obtained by Harvard College with their new meridian photometer left “an im pression of distrust whether any of these observations are suitable for any precise or critical purpose.” It may seem ironic that the man who had such success with his newly invented Almucantar would be such an outspoken critic of the first products of the new meridian photometer. But careful examination of these papers shows that Chandler's criticisms were directed primarily at the apparent lack of care taken in making the observations and the resulting poor reliability of the results obtained, not at the basic design of the instrument. His concerns were justified by the fact that fifteen out of the first eighty-six telescopic variables observed showed serious errors. Always a man to solve puzzles, Chandler was able not only to report these errors, but also to deduce correctly the probable cause for each of them.

Edward Pickering, then director of the Harvard Observatory, was concerned that Chandler's criticisms might weaken support for the photometric research and responded publicly (Jones and Boyd, 1971). Chandler's colleagues, John Ritchie, Jr., and B. A. Gould, entered the dispute on his behalf. For the next several months the debate raged, fueled chiefly by Ritchie, in local newspapers and in the *Astronomische Nachrichten,* where Pickering accused Chandler of personal animosity. In the supplement to his second catalogue, Chandler entered several variables in the southern sky based on examinations of the Harvard College Observatory photographs made at its Boyden Station in Arequipa. He again stated his distrust in the accuracy of the positions and identifications, but relied on the assurances of Pickering that each instance had been confirmed by independent examination. Yet still, he marked those stars with the initials H.C.O. to signify that the observatory in question was the sole authority.

COMPUTATIONAL SKILLS LEAD TO DIVERSE STUDIES

Chandler's exceptional computational skills led him into diverse facets of astronomy. He was known to spend countless hours at his desk working his computations with speed and vigor, often foregoing food and sleep in pursuit of his goal. He computed the orbital elements and generated an ephemeris for any comet for which he could obtain adequate observations. His investigations of the orbit of comet 1889d allowed him to identify it as the previously discovered Lexell's comet (Chandler, 1889). He calculated and recalculated the constant of aberration, publishing sixteen papers on this subject alone. He even used his mathematical skills to speculate on the size of the two newly discovered moons of Mars (Chandler, 1877). His values were too small by about a factor of two because he assumed much too high albedos, but until space probes visited other bodies in the solar system a century later there was little information from which he could obtain better estimates.

In 1898 the minor planet Eros (first called Witt's planet or planet DQ) was discovered. Since Eros' orbit approached Earth more closely than any other minor planet, it could be used to determine more accurately the distance to the sun. The exact orbit of this planetoid needed to be calculated as soon as possible. Chandler wrote to Pickering asking him to launch a search for the planetoid in the Harvard collection of photographic plates. Using Chandler's rough ephemeris, the Scottish astronomer Wilhelmina Fleming was able to recover an image of Eros on a plate from 1893. Chandler was impressed by this “inspired find” and was able to calculate a more exact orbit (Jones and Boyd, 1971). This more exact orbit produced more observations on plates from 1894 and 1896. This cooperation repaired the rift between Pickering and Chandler and friendly relations were restored.

During the search for previously undetected observations of Eros in 1898, Chandler wrote a letter to the editor of *The Observatory* regarding the name of the tiny new planet (Chandler, 1898). He suggested the name "Pluto," the only one of the six surviving children of Saturn that had not yet been assigned to major planets or members of the asteroid belt. Chandler noted that "Pluto under his older name, Hades, was the invisible or unknown, the God of Darkness." Although the name was not used at the time, this suggestion may have influenced the naming of the current planet Pluto after its discovery in 1930.

THE TELEGRAPHIC CODE

One of the many obstacles associated with astronomy that Chandler saw fit to tackle was the important task of telegraphic transmission of scientific data. "In astronomy," Chandler stated, "where accuracy is of vital importance, the details of a message are such that they are of little interest to the ordinary operator, and afford no means of correcting mistakes by the context" (Chandler, 1881). In 1881 he and John Ritchie, Jr., devised a code that could be used to telegraph important data. The code made use of a readily available dictionary, the 1876 edition of *Worcester's Comprehensive Dictionary*. Using this code, no numbers had to be transmitted. All numbers could be represented as words. For example, the number 16,718 was replaced with the 18th word on page 167 (electrize), or April 14d. 10h. 48m. could be represented by the number 134.45 (134.45th day of the year) or the 45th word on page 134 (Crush) and so on. During the first fourteen messages of comet discoveries in the months following the implementation of the code, frequent errors were detected in transmission, but the control words were easily corrected by the receiver. Based on the success of this new system the Smithsonian Institution re linquished control of the Department of International Exchange of Astronomical Information for the United States to the Harvard College Observatory where this important activity continued for more than eighty years (Jones and Boyd, 1971).

IN CONCLUSION

Seth Carlo Chandler, Jr., was a talented, enthusiastic scientist whose career spanned a half century and included many exciting new discoveries in diverse facets of astronomy. But his era was quickly followed by discoveries in science that shone so brightly as to make his achievements pale in comparison. The discoveries of special relativity, general relativity, and quantum mechanics early in the twentieth century completely redefined the understanding of our universe, making many of the discoveries of the nineteenth century seem insignificant. Observatories continued to track the motion of the pole without interruption even during the two world wars, but this activity was considered an operational requirement for treating geodetic and astrometric measurements, with little scientific importance or excitement. The discovery of polar motion was eventually reduced to a brief anecdote in which Chandler was generally described as a wealthy merchant from Boston, an amateur who had just happened upon polar motion. At least one author went so far as to speculate that Chandler probably had little idea of the importance of his discovery.

Ironically, Chandler was very active in the amateur astronomy community of his time. He was, for some time, president of the Amateur Astronomers' Club of Boston. Many of his earlier writings were published in the *Science Observer*, which was directed primarily toward the amateur community and gave detailed directions on how certain observations could be conducted by amateurs. He clearly believed that amateurs could make a strong contribution to the discovery and monitoring of variable stars, one of the primary subjects of interest in his era, and he published detailed descriptions of the observing techniques and special instrumentation he developed. However, Chandler had worked directly with the leading astronomers of his time beginning in high school and did in fact make his living for some years in geodetic astronomy. He knew very well the importance of his findings and published extensively. Fortunately, his contributions have survived and his work has regained much of the recognition it once enjoyed and deserves.

AN EXTENSIVE COLLECTION OF Chandler's private and professional correspondence has been assembled and is available on microfilm from the American Institute of Physics, Center for History of Physics, 335 East 45th Street, New York, New York 10017.

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JULE GREGORY CHARNEY

January 1, 1917–June 16, 1981

BY NORMAN A. PHILLIPS

ULE CHARNEY WAS one of the dominant figures in atmospheric science in the three decades following World War II. Much of the change in meteorology from an art to a science is due to his scientific vision and his thorough commitment to people and programs in this field.

In 1946 he married Elinor Kesting Frye, a student of logic and semantics with H. Reichenbach at the University of California at Los Angeles. They had two children, Nora and Peter. Nicolas, Elinor's son from her previous marriage, assumed the last name of Charney. Their marriage lasted almost twenty-one years. In 1967 Jule married Lois Swirnoff. Lois is a painter and color theorist and was a professor at UCLA and Harvard. Their marriage lasted almost ten years. Jule shared the last years of his life with Patricia Peck, a photographic artist with roots in New York City and Venice. His last illness was lung cancer, from which he died in Boston on June 16, 1981.

THE BUDDING MATHEMATICIAN

Jule was born on New Year's Day 1917 in San Francisco. His parents, Stella and Ely Charney, had immigrated early in the century from White Russia, where the lot of Jewish citizens was difficult. Each of them had taken up work in the New York garment industry, but later met and married in St. Louis. After a brief stop in Denver, they moved to Los Angeles in 1914. Employment difficulties forced a temporary move for several years to San Francisco, where Jule was born. He spent most of his youth in Los Angeles with one important exception. This happened at the age of fourteen, when his mother, temporarily estranged from his father, moved back to New York. Jule later recalled that he did not like New York, but he also remembered that it was here at a relative's home that he came upon Osgood's book on calculus. Calculus was not taught in any of the usual high schools in the country, but exposure to this book and the realization that he could solve the problems excited his interest in science.

Mother and father were fervent socialists, especially Ely, who took an active role in union affairs. Stella favored a more leftist position than that held by her husband. Home political discussions were frequent. Along with this stimulating background, Jule read widely and voraciously in the public library during grade school. He was exposed to music in his early years through a small family collection of records (Caruso, Galli-Curci, Tchaikovsky, etc.), but he never received any musical training. Nevertheless, music was a source of enjoyment throughout his life. One of his amusing recollections in later years was of having played games with the young prodigy Yehudi Menuhin on top of Yehudi's apartment building, and in using this fact many years later to establish an element of mutual recognition with the world famous violinist.

His last three high school years were spent at Hollywood High School after the family moved from Boyle Heights in east-central Los Angeles. By graduation in January 1934 he had already familiarized himself through independent reading with most of the standard material on the differential and integral calculus, and it seemed that he was already on the way to a career in mathematics or theoretical physics. He attended the Los Angeles campus of the University of California instead of the scientifically well-established campus at Berkeley, because of UCLA's nearness and the absence of any advice about the senior campus to the north. His undergraduate years emphasized both mathematics and physics (although Jule later complained about the lack of theoretical physicists at UCLA), and he began to be recognized as a likely candidate for the first doctorate in mathematics from the Los Angeles campus. He became a member of Phi Beta Kappa and a University Fellow in 1939 shortly after he started his graduate work under T. Y. Thomas. A master's degree followed in 1940 and he soon completed a paper, “Metric Curve Spaces.” Thomas considered this suitable material for a doctoral thesis, but Jule had a lower opinion of its merit; he never began the final write-up for submission as a thesis.

Thomas led a seminar that included treatment of fluid turbulence and one day invited J. Holmboe from the newly formed meteorology group in the Physics Department to talk. Having introduced Jule to the idea of meteorology as a field with some scientific possibility, Holmboe invited Jule in the spring of 1941 to be his assistant and to participate in the meteorology training program taking shape at UCLA and other universities under sponsorship of the army and navy. At this time the war in Europe and tensions in the Pacific had progressed far enough that university students began to consider various options for useful service. Seeking advice, Jule visited T. von Karman and was counseled to pursue meteorology over work in the aeronautics industry since the latter was becoming too much of an engineering subject for a person of Jule's theoretical inclination. Since this option had also been made easy by Holmboe's offer, it was the logical choice; in 1941 Jule became a teaching assistant and student in the meteorology program at UCLA.

A NEW LIGHT IN METEOROLOGY

In 1941 only a few U.S. universities offered meteorology as an academic discipline, although greater interest in the field was being stimulated by an expanding military's need for weather forecasters. The leader of the small meteorology group at UCLA (then a part of the Physics Department) was J. Bjerknes, who had recently arrived from Norway. He was very well known in the meteorological world for the description of cold and warm fronts he had put forth in Bergen about the time of Jule's birth. J. Holmboe was a younger Norwegian who was at ease with these concepts and had somewhat more familiarity with fluid dynamics.

M. Neiburger, on the other hand, had been educated under C.-G. Rossby at the Massachusetts Institute of Technology. Rossby preferred a more analytic approach to atmospheric and oceanic motions, in which fluid dynamics was applied to simplified models of the atmosphere and ocean. In 1939, for example, he had pursued a recent idea of Bjerknes that the variation with latitude of the Coriolis parameter (twice the angular velocity of the earth times the sine of the latitude) played an important role in the eastward migration of the large-scale circulation systems. Rossby used a simple model of a purely horizontally moving homogeneous atmosphere to arrive at a quantitative formula for the speed at which these systems (now called Rossby waves) would move from west to east in such an idealized atmosphere. Although these flow patterns were correlated with weather systems, weather forecasting throughout the world was still done by extrapolating the day-to-day behavior of pressure systems as they were depicted on daily weather maps of surface weather observations. Even Rossby's formula—at the few places it was known—had only a limited role because there was no evidence for deciding the level in the atmosphere at which his model should be applied. Furthermore, in 1941 measurements in the troposphere were too few to define the flow pattern over the hemisphere at one instant. (The novel *Storm*, published by G. Steward in 1943, gives a necessarily romantic, but otherwise realistic picture of meteorological practice at that time.)

During the next ten years Jule Charney brought about a profound change in this primitive procedure. In collaboration with J. von Neumann he was to show how the newly developed electronic computer could be used to make forecasts by numerical integration of the hydrodynamical equations of motion, beginning with the observed picture of those motions that had then become available from a greatly expanded network of daily radiosonde stations. The basic premise of this physically based procedure was not new, having been stated by V. Bjerknes in the early years of the century and even attempted partially by L. Richardson during World War I. It had, however, lain dormant for twenty-five years.

Part of Jule's assignment was to teach a course in synoptic meteorology—the construction of weather maps based on surface observations of pressure, temperature, wind, and weather. In his 1980 conversations with G. Platzman, Jule recalled his distaste for this subjective procedure with its emphasis on elegant drawing of isobars and fronts. He admitted, though, that it was in 1941 the only way for students to become familiar with atmospheric motions and behavior. (His performance as a teaching assistant was evidently acceptable; his small class of students in this subject successfully manipulated his campus-wide election as King of the Mardi Gras—a precursor of many academic honors to come!) Jule also taught a course in atmospheric radiation as a substitute for J. Kaplan (where he recalled being just one lecture ahead of the class) and assisted in preparing notes for Holmboe's lectures on basic principles of fluid dynamics of the atmosphere.

Jule's university social life was happy. M. Wurtele, a fellow meteorology student, recalls that Jule shared a house on Kelton Avenue with several other students and enjoyed a lively social life. Fortunately, a mistaken diagnosis in childhood that he had a heart problem had been corrected in his teens. Jule had since learned to ski and play tennis, sports that he was to enjoy until the last several years of his life. Somewhere along the way he acquired experience in games of chance, a skill that was exercised much later on night watches during one of the two Indian Ocean ship expeditions in which he participated. (After Jule's death B. Taft recalled that Jule was the only scientist he knew who could play poker nightly with the ship's crew, win their money consistently, and never engender the slightest ill will.)

With his mathematical background Jule was not attracted by the descriptive reasoning used by Bjerknes and Holmboe. Fortunately, however, Neiburger exposed him to Rossby's papers early in Jule's assistantship. This is not to say that Rossby used completely deductive reasoning—the simple models that he constructed to describe the atmosphere and ocean were based on intuition instead of rational simplification (and were often resisted by fellow meteorologists on that ground). Rossby and Charney exchanged many letters in the ten years preceding Rossby's death in 1957. (In the Charney files at the Massachusetts Institute of Technology there are forty-two letters from Rossby and twenty-three from Charney.) In one of them Rossby described his own teaching method: “Perhaps I occasionally sought to give, or inadvertently gave, to the student a sense of battle on the intel lectual battlefield. If all you do is to give them a faultless and complete and uninhabited architectural masterpiece, then you do not help them to become builders of their own." This philosophy also characterized Rossby's papers and seems to have had a permanent effect on Charney's thinking.

Around 1944 or 1945 Charney began to view himself as qualified to consider a thesis in meteorology. He gradually formulated his goal to be a theory of the instability of the average west-to-east flow in middle latitudes of the atmosphere. These zonal westerlies increase with speed from ground to around ten kilometers because the average air temperature below that level typically increases from pole to equator. This choice of topic was influenced by his exposure to Bjerknes' semi-quantitative description of the wavelike patterns in the upper atmosphere, three or four of Rossby's papers, and his exposure in the lecture series by Thomas to the idea of instability in fluid flows as a mathematical problem. This choice was his, with no guidance from the faculty.

The perturbation equations for atmospheric flow are intricate when allowance is made for a basic state containing a non-uniform current. Furthermore, even a resting atmosphere can sustain propagation of sound waves and of gravity waves, as well as the more recently recognized Rossby waves. To arrive at a tractable mathematical problem, Jule found it necessary to make a set of consistent approximations in his derivation of the final governing differential equation. In his 1980 recorded conversations with G. Platzman, Jule recalled with fresh enthusiasm the occasion when this process had reached a tractable state in his mind, with a recognizable standard second-order differential equation. It is easy now to forget that this type of reasoning was not then common in any branch of science. That Charney accomplished this, and without help from any established fluid dynamicist, is early evidence of his insight.

After much hand calculation Jule was able to find a curve of zero growth rate that separated unstable waves of short horizontal wavelengths from longer stable waves. He also calculated how the wind, temperatures, and pressure fields were organized in an unstable wave, and this picture agreed well with observed features of the upper waves. The thesis was quickly published and accepted as an explanation for this phenomenon even though few meteorologists were then familiar with this level of mathematics. Later studies have shown that the complete solution is more complicated than Jule thought in 1946, but his solution did contain the most important aspects. Most significantly, his thesis satisfied Jule's high critical standards and convinced him that he was indeed capable of original research of high caliber in meteorology. His ensuing commitment to meteorology as a permanent career was of major importance to the development of atmospheric science.

NUMERICAL WEATHER PREDICTION AND PRINCETON

In the months before his thesis defense in the spring of 1946 Charney explored several avenues for a postgraduate fellowship, having in mind that he was, in spite of his thesis, a newcomer to fluid mechanics. He was awarded a National Research Council fellowship, tenable in Europe, and he made plans to visit H. Solberg in Oslo (who had been the leading mathematician in the Norwegian school) and G. I. Taylor in Cambridge, England. Fortunately, Jule and Elinor called on Rossby at the University of Chicago en route. Rossby was leading the department into its heyday with field investigations of thunderstorms (under H. Byers), discovery of the jet stream (under E. Palmen and H. Riehl), application of group velocity to meteorological and oceanic wave propagation (under Rossby), and simulation of atmospheric motion in experiments with rotating differentially heated "dishpans" (under D. Fultz).

The two men hit it off at once and Rossby, with his extraordinarily persuasive powers, had no difficulty in persuading Jule to postpone his fellowship and stay at the university for almost a year. The two men had many discussions both together and with other faculty and the many foreign visitors Rossby brought to Chicago to open the channels of communication that had been interrupted by the war. Jule later viewed this year as the most formative experience in his professional life.

A major event soon occurred when Rossby arranged for Jule to attend a meeting that J. von Neumann was to hold in August 1946 at the Institute for Advanced Study in Princeton. The subject was the application of electronic computers to weather forecasting. Von Neumann had recently recognized weather prediction as a prime candidate for application of electronic computers, in particular the new computer that was being built to his specification at the Institute. (In his 1980 interview with Platzman Jule suggested that von Neumann's interest in weather prediction originated from von Neumann's acquaintance with V. Zworykin at nearby RCA. F. Nebeker, however, points out in his Princeton University thesis that it was Rossby who suggested to von Neumann that the Institute for Advanced Study should submit a proposal for meteorological funding to the navy's Office of Research and Invention, and that this had been done by May 1946.)

About a dozen of the leading dynamical meteorologists in the United States attended, including Rossby. Most of them knew that L. Richardson had attempted during World War I to integrate the hydrodynamical equations for the atmosphere with finite-difference methods for a single time step, but had obtained an absurdly large value for the rate of change of surface pressure. Neither the official minutes nor Jule's notes of the meeting record anything of material or even inspirational value at that time. But from the vantage of hindsight it is possible to see that the presentation by navy Lieutenant R. Elliott consisted of approximated equations that had some similarity to the quasi-geostrophic theory that Jule was to formulate in the next several years. The similarity is clear, however, only to someone who knows what to look for, because Elliott's derivation was ad hoc and his computation scheme was involved and ill-posed. It is not surprising that Elliott's work was not pursued by the small meteorological group that von Neumann collected.

Thus, the only important result of this meeting was to acquaint Jule Charney with the fact that John von Neumann was a man with considerable feeling for physical problems and that a rational theory for the large-scale motions of the atmosphere would receive a strong welcome at Princeton, with a good likelihood of being applied on the new computer. Jule's files show that shortly after returning to Chicago he went so far as to write a letter to von Neumann exploring the possibility of coming to Princeton, but he never mailed it.

Jule and Elinor sailed for Norway in the spring of 1947. Their first stop was at Bergen, the intellectual home of the Norwegian frontal concept since World War I. Here Jule met the English theoretical meteorologist E. Eady. Eady had independently derived a theory of the instability of the west wind belt containing the same physical mechanism as that in Jule's thesis, but in a simpler form. They became good friends and Eady later spent a part of a year with Jule at Princeton.

Upon arrival in Oslo, Jule found a long letter from J.

Bjerknes containing many practical suggestions on travel and other aspects of living in postwar Norway. Bjerknes also arranged for Elinor to receive some money by helping C. Godske with the language for his contribution to the new English edition of the *Physikalische Hydrodynamik* that had been published in 1933 by the Bergen meteorological group. This must have been a welcome addition to the fellowship stipend. The solicitude continued when Bjerknes wrote in late November to discuss not only the possibility of coming back to a faculty position at UCLA, but of faculty appointments at other universities as well! It is easy to understand Jule's long, deep respect for J. Bjerknes.

Jule did not take long to discover how to modify the hydrodynamical equations for separating the meteorologically relevant large-scale motions from the faster acoustic and gravity waves that were also contained in the equations and which were demonstrably at the root of Richardson's difficulty; the year of gestation at Chicago had done its job well. As he wrote in his 1948 paper, “On the Scale of Atmospheric Motions”:

The motion of large-scale atmospheric disturbances is governed by the laws of conservation of potential temperature and absolute potential vorticity, and by the conditions that the horizontal velocity be quasi-geostrophic and the pressure quasi-hydrostatic.

This formulation, which Jule first stated in a letter to P. Thompson in November 1947, was justified by a careful scale analysis of the terms in each of the hydrodynamic equations for momentum, for mass, and for entropy. This scale analysis was similar in principle to the consistent approximation steps Jule had been led to in arriving at the governing differential equation for his thesis. The set of prediction equations that results from the above prescription is nowadays called the quasi-geostrophic theory. It al lows only "slow" advective-type motions without acoustic or gravity waves (i.e., it acts as a low-pass filter).

The quasi-geostrophic theory is probably the most rewarding development in meteorology and oceanography since World War I. Numerical weather forecasting is now based on a more complete set of dynamic equations, but Jule's quasi-geostrophic system was necessary for the first several years of computer work; it is still used for theoretical studies of atmospheric motion. It is possible to detect procedures in earlier literature that bear some resemblance to parts of Jule's system, but they are scattered and have little intellectual continuity. Jule was not familiar with most of them in 1947, but this was probably an advantage. It is also true that in 1947 other meteorologists were close to formulating the quasi-geostrophic system (A. Eliassen in Oslo had already done so), but Jule had in effect proved with his scale analysis that this system was a consistent approximation for large-scale atmospheric motions.

In early 1948 von Neumann invited Jule to head the meteorology group in his Electronic Computer Project, whose financial support came from the Office of Naval Research. Arrangements were also made for A. Eliassen to come from Oslo for a year, to be followed by R. Fjørtoft. These Norwegian meteorologists were well trained in both hydrodynamics and descriptive meteorology, while earlier members of the group, J. Freeman, G. Hunt, P. Queney, and Thompson as well, were primarily theoreticians and left Princeton for other commitments.

For three years Jule and Elinor lived in the Institute compound—a collection of wooden rowhouse barracks that had been moved to the Institute grounds from use elsewhere. These were occupied by the many one-year temporary members of the Institute, mostly young mathematicians and physicists, with a sprinkling of more established professors in the humanities. Located within a brief walk of the computer building and the main Institute building, this was a stimulating place for Jule and Elinor to live and for Jule to begin work on what was clearly going to be a milestone in atmospheric science. Von Neumann, although often away from the Institute, was an eager listener and willing participant in Jule's thinking. John and Klari von Neumann were gracious hosts and the Charneys soon met the Oppenheimers and other permanent members at the Institute, as well as faculty at Princeton University. It seems reasonable that this cosmopolitan milieu and the earlier year at Chicago did much to equip Jule for the powerful domestic and international advocacy roles he was to play later in the creation of the National Center for Atmospheric Research and the Global Weather Experiment.

During the first year Jule took several major steps in preparation for predicting flow patterns with a computer. First, with the quasi-geostrophic system he investigated the important question of how large a volume of atmosphere surrounding a forecast point must be considered for a twenty-four-hour forecast. This was answered by appeal to the three-dimensional group velocity of Rossby waves in a uniform current from the west. But the full three-dimensional geostrophic system, straightforward as it was, was still too demanding for von Neumann's computer. Jule showed how, by an intelligent system of vertical averaging, the full system could be reduced to a simple approximate statement that vorticity was advected horizontally at a certain objectively defined level in the atmosphere. This was denoted as the equivalent barotropic level, located about 5 kilometers above sea level. Jule put forth this greatly simplified system as the first nonlinear system for numerical weather prediction, to be followed by future systems with more vertical detail as experience and computer development would justify.

Since the computer was not available, Charney, with the collaboration of Eliassen, took the simplification process one step further. They linearized the barotropic equation to treat perturbations on a uniform flow in a narrow westeast channel and expanded Rossby's frequency formula into a Green's function that would give a twenty-four-hour forecast of the initial flow pattern by simple weighted longitudinal integration of the initial distribution of the isobaric height at the barotropic level. Tests gave very promising results, indicative of success to come when the new computer could be applied to the full nonlinear vorticity equation. (These linear results were so striking that when Jule sent them to Rossby in Stockholm, J. Namias, then chief of the long-range forecast section of the U. S. Weather Service on visit to Rossby's institute, wrote immediately to H. Wexler in Washington, urging that the Weather Bureau contact Charney at once to start operational testing of this linear method. J. Smagorinsky became involved in this effort.)

Charney and Eliassen exploited this linear model further by inserting the effect of flow over mountains and the effect of turbulent friction in the air near the ground. These effects were not of major importance in a forecast for one day, but when the enhanced equations were solved for a stationary perturbation field the resulting pattern was amazingly similar to the time-averaged perturbation field at the five-kilometer level. (The manner in which friction was represented in the equations was conceived by Eliassen and has become known as Ekman pumping, referring to the Swedish oceanographer who first presented the mathematics for the effect of the earth's rotation of frictionally driven currents near the surface of the ocean.) Many later studies with more detail in latitude and height—including one by

Charney—have shown that the effect of mountains on large-scale atmospheric flow is not as straightforward as was assumed here by Charney and Eliassen. (For example, they used a somewhat narrow zonal channel, which reduces considerably the dispersive aspect of Rossby waves.) But the gods smiled!

Since work on von Neumann's computer had progressed more slowly than hoped, Weather Bureau chief F. Reichelderfer, at von Neumann's request, wrote in September 1949 to General Hughes, Chief of Army Ordnance, for permission to use the ENIAC computer at Aberdeen Proving Grounds. Von Neumann had developed a technique for using Fourier sums with cyclic input and output of punched cards that allowed the nonlinear vorticity equation to be integrated on the ENIAC, whose internal storage was small.

The first one-day, nonlinear prediction was made in April 1950. It required the round-the-clock services of Charney, Fjørtoft, J. Freeman, G. Platzman, and J. Smagorinsky, and, largely because of ENIAC breakdowns, more than twenty-four hours to execute. The results of several such forecasts were quickly published in Rossby's journal *Tellus.* Their overall character was good. Jule sent copies of the forecasts to L. Richardson in the United Kingdom. Richardson was a committed pacifist who had abandoned his early numerical work on forecasting and, since 1920, had worked on devising mathematical models to understand and prevent war. Richardson asked his wife to judge whether initial (*a*) or forecast (*d*) maps best resembled the verification maps (*b*). He reported her verdict to Charney:

Thus (*d*) has it on average, but only slightly. This, although not a great success of a popular sort is anyway an enormous scientific advance on the single, and quite wrong, result in which Richardson (1922) ended. So far I have only had time to glance at your five papers. To comment on them now would be rash; but to defer comment would be to risk never making any; for I have other urgent duties.

Richardson died several years later.

The first computations on the Institute computer were made in the summer of 1952, followed closely by models that contained two and then three levels of information in the vertical. The latter successfully predicted a case of intense winter storm development. This development was an example of the instability that Jule had described in his thesis and was convincing vindication of his graduate work seven years earlier. This was the peak of Jule's interest in personally pursuing numerical weather prediction, although he was to make several theoretical contributions in later years.

The meteorology group (in particular, the writer) used a simple quasi-geostrophic model to simulate the so-called general circulation—the manner in which latitudinal variation of solar heating generates zonal winds and how this, through Jule's instability process, gives rise to cyclones and anticyclones, which in turn modify the zonal winds into belts of westerlies and trade winds. Besides predicting westerlies and trades, this experiment indicated that the fronts described by Bjerknes were not the source of large-scale storm development, but were created by the developing unstable wave. This showed that Bjerknes' recent emphasis on the wave in the free atmosphere and Jule's fixing upon this wave instead of fronts as the basic instability element was correct. This type of numerical experiment has blossomed nowadays into elaborate computer simulations of the atmosphere (and the oceans), which include detailed radiation calculations, modeling of the hydrological cycle and, in some instances, chemical interactions. These are used to estimate anthropogenic effects on mean temperature from changes in carbon dioxide or dust from thermonuclear ex plosions and on ozone from changes in hydrofluorocarbons and nitrogen oxides.

At its maximum, Jule's meteorology group consisted of Jule plus four meteorologists, several programmers, and a secretary. In addition there was a constant stream of short-term visitors from the United States and abroad, some Jule had invited, and others who requested to see firsthand what these new developments were like. Jule also took seriously the responsibility to report frequently the group's progress at scientific meetings.

The new prediction method spread rapidly, with assistance from the Princeton group. In August 1952 von Neumann organized a meeting to consider operational use of the new method by the weather services of the United States, and by February 1954 the computer had been selected for what was known as the Joint Numerical Weather Prediction Unit, representing the Weather Bureau, Air Force, and Navy. The Princeton group assisted in teaching the new methods and computer usage to representatives from the three services. By 1954 groups also had started at the Air Force's Cambridge research laboratory, at Rossby's international institute in Stockholm, and independently, at the British Meteorological Office. In late 1955, shortly after the general circulation numerical experiments had been digested, von Neumann and Charney encouraged the establishment of a special unit in the Weather Bureau to exploit this technique. J. Smagorinsky led this effort, which has culminated in the Geophysical Fluid Dynamics Laboratory of the National Oceanic and Atmospheric Administration at Princeton University.

Jule's creative interests continued apace. He had some familiarity with the existing theory of large-scale motions in the ocean from his reading of Rossby's papers from the 1930s and from more recent acquaintance with H. Stommel and W. Munk. (H. Sverdrup was also on Jule's thesis committee.) Jule first applied Eliassen's development of the Ekman theory to show how the effect of wind stress at the top of the ocean should be used as a boundary condition on the quasi-geostrophic interior motions of the ocean. This step brought to fruition the original observations of ice drift that F. Nansen had made at the turn of the century, which had led to Ekman's theory. An even more dramatic step was an inertia theory for the Gulf Stream. H. Stommel and W. Munk had presented theories in which the width of the stream—much narrower than the ocean—depended on a poorly known artificial friction parameter. After much discussion with Stommel, Charney showed how the conservation of potential vorticity in the water mass moving slowly westward in the ocean interior should lead to a narrow boundary current at the western coast of the ocean with geostrophic balance in the streamwise velocity. (A similar theory was published almost simultaneously by G. Morgan.)

Jule had now been at the Institute for seven years and was thirty-eight years old. He received a five-year appointment as a member of the Institute at the end of 1951, but was not a permanent member. Von Neumann had become an atomic energy commissioner with heavy duties in Washington and it was clear that the Electronic Computer Project, with its applied flavor, was not to be a permanent feature of the Institute; pure mathematics was the preferred science there. Oppenheimer was unable to promise a permanent membership to Jule, although both men respected each other and the mutual benefit that the Institute and Jule had on one another. The situation became more urgent when von Neumann developed cancer. By this time Jule realized the importance to him of contact with experimental and observational work and began inquiries about a university appointment. The universities responded favorably, some with remarkable alacrity. The Massachusetts Institute of Technology was the winner and offered a package deal including the writer and the retention at MIT of E. Lorenz. It was characteristic of Jule that he recommended to the MIT administration that V. Starr and Lorenz should be promoted as part of this enhancement of the meteorology department. The move was made in the summer of 1956, with a special personal grant of $5,000 to Jule from the Institute at Oppenheimer's urging.

ORGANIZATIONAL CONTRIBUTIONS

After his move to MIT Jule could shed much of his responsibility for progress in numerical weather prediction. Together with W. Malkus in the Mathematics Department he at once organized an informal seminar on geophysical fluid dynamics. This seminar was held fortnightly on late Friday afternoons and gradually involved people from the meteorology, oceanography, geophysics, and applied mathematics groups at MIT, Harvard, and Woods Hole, with frequent participation from Yale, Brown, and the University of Rhode Island. They were held at a different institution each time and the long automobile trip naturally demanded a social hour for decompression afterwards. This seminar lasted for twenty-two years and was the major means of informal communication between people in New England working on this subject.

The fame of his scientific work, however, quickly led to increasing demands for his service as an advisor and committee member. The most permanent of these was an appointment in 1957 to the National Academy of Sciences' Commission on Meteorology, a commitment that was to last, in one form or another, for fourteen years. This commission had been established a year earlier by D. Bronk, with Rossby and L. Berkner as co-chairmen. T. Malone recalls that Bronk did this in response to a request from F. Reichelderfer, who hoped for advice on strengthening research in the Weather Bureau, and because Bronk, as a sailor, had been dissatisfied with weather prediction since the 1938 hurricane! In November 1957 Jule made a report to the commission that emphasized the presence of three new factors in meteorology—satellites that observe the atmosphere on a global basis, instrumented aircraft and radar that scrutinize the details of small weather systems, and the electronic computer that helps digest the new data. Although not acted on at this time, this idea can be recognized in the later call by Jule for the global atmospheric research program.

At about this time, L. Berkner (Rossby had died in the summer of 1957) thought of creating a national research center for atmospheric science. Malone recalls that Berkner first charged him and Jule to give a prompt, but considered, reaction. After intense deliberation they returned with a favorable report and then other meteorologists (H. Houghton, for example) reviewed the concept. One of the early worries was whether the new center would weaken the university departments or whether the departments would stifle a new center. Malone and Charney were then charged to visit a significant sample of universities and established research centers for reaction and suggestions. They did so, with positive results.

Jule played no role in the organizational meetings that followed, but he was very active in assisting Malone in the more technical meetings that described the activity the new center would conduct above and beyond that done at universities. These initial steps, when supplemented by the organizational drive of the leading department chairmen, led to incorporation of the University Corporation for Atmospheric Research in March 1959 and the formation soon after of the National Center for Atmospheric Research (NCAR), with financial support from the National Science Foundation. Jule visited NCAR several times during its early years, but most of his extended visits away from MIT were to other centers.

He became a councillor of the American Meteorological Society in 1959 and was given responsibility for the scientific program at the fortieth anniversary meeting of the society in December 1960. He selected "motions of the upper atmosphere" as the central topic, partly in response, one may suppose, to his revived interest in the vertical propagation of large-scale wave energy. He also served as publications commissioner for several years.

Jule's outgoing nature and open mind quickly led to friendly acquaintance with many of the leading faculty members and administrators at the Institute and elsewhere in the Boston area. In early 1960 he was appointed to the Atmospheric Sciences Panel of the President's Science Advisory Committee. A year later he had discussions with B. Rossi from the Physics Department at MIT, who was advising J. Wiesner (science advisor to President Kennedy) on the possible peaceful uses of outer space. Jule arranged a meeting with several other meteorologists at which it was suggested that satellites could improve weather forecasting. This met with ready acceptance and was referred to by Kennedy in his State of the Union message and in his September 1961 speech to the United Nations. U.N. Resolutions 1721 and 1802 followed, asking first the World Meteorological Organization and then the International Council of Scientific Unions to develop plans to this end in operational practice and research.

Jule recalled that soon after, at a meeting of the American Meteorological Society dealing with international cooperation in meteorology, he was struck with the fact that, although meteorology was presumably a global science, the lack of global observations prevented examination of this important aspect, and that correction of this observational gap could be a very fruitful aspect of international cooperation in meteorology—even if it must be confined to a limited time as an observational experiment.

I recall that Jule pondered deeply on the commitment from him that a serious follow-through on this idea would entail. But having decided that it was a job that he should do, he pursued it with evangelical zeal, almost until the yearlong global observational experiment finally started in December 1979. He devoted considerable time in traveling, speaking, devising ways to arrive at meaningful specifications for an observing system, and helping to formulate the first set of plans.

In 1966 he became leader of the National Academy of Sciences' Committee on the Global Atmospheric Research Program—GARP, as it came to be called—and held this demanding position until 1971. He was active in several working groups, even to the extent of being scientific director for one month of the preliminary GARP tropical observing experiment in Barbados. It was at this time that he cemented a productive and long-lasting working arrangement with NASA's Goddard Laboratory, first in New York City, and then in Greenbelt, Maryland. In the meantime his other research continued at full intensity, as may be judged from his publications. He maintained his educational commitments at MIT in spite of this furious outside activity and even accepted non-trivial appointments to committees for NCAR, the American Geophysical Union, the American Meteorology Society, and several ad hoc committees for the National Academy of Sciences.

Jule and Lois took sabbatical leaves in the academic year 1972-73 and spent the first part at Cambridge, England.

During this period Jule gave considerable thought to how higher frequency gravity wave motions might be generated by nonlinear interactions among Rossby waves, but finally gave up.

The Charneys then stayed at the Weizmann Institute in Tel Aviv, where Jule worked on a theory of desertification. (A loss of vegetation would increase the ground albedo and reflect more solar radiation back to space. The reduction in insolation absorbed by the ground would then decrease the local heating of the air by convection. This in turn would reduce the mean upward motion of air, resulting in reduced rainfall and a tendency toward further decrease in vegetation.) His interest in this topic was stimulated by the drought in the Sahel and by his fond recollection of spring trips to the Mojave Desert with his parents. This was his first visit to Israel, although he had received several invitations. His parents were not religious and Jule himself seems never to have taken up any part of his ancestors' faith. However, in several letters from Tel Aviv to friends back home, he described his trip as a "moving experience" and referred respectfully to "the toughness" of the Israeli.

The last months of this sabbatical were devoted to leading a summer workshop in Venice. This annual event had been started several years earlier by R. Frassetto of the *Institut per lo studio della dynamica delle grandi masse* and the oceanographer A. Robinson from Harvard. The drive behind this workshop was to help reduce flooding in Venice; the successful operation of a massive floodgate project would need accurate prediction of water level in the upper Adriatic. The fluid dynamical model developed by the Harvard group had treated the influence of tides and atmospheric wind and pressure as known forcing functions on the Adriatic. The success of this model shifted the emphasis to predicting the atmospheric wind and pressure. Jule's workshop was therefore on meso-scale meteorology, which, although somewhat different from the large-scale problems that had occupied him, was very important for the mountainous orography in the Italian area. In following years he continued to influence Italian science by sponsoring and working with several Italian students and postgraduates in his National Science Foundation project at MIT.

All the above activities illustrate Jule's sense of responsibility as a scientist in matters for which he had some unique insight and power, where he would be expected to lead and for which it was reasonable to expect success. His personal sense of responsibility was broader, however, as most of his friends can attest. The most ambitious of these efforts began in May 1970 after the invasion of Cambodia by U.S. forces and the tragedy at Kent State on May 4. Jule, Lois, and S. Luria conceived the idea of soliciting money from academic people to support antiwar candidates in the upcoming elections. With the help of A. Robinson and other Cambridge faculty members, they organized the Universities National Antiwar Fund. Chapters were organized at several hundred campuses and enabled UNAF successfully to solicit the equivalent of a day's salary from thousands of people. In this way about $250,000 was donated to dozens of carefully selected antiwar candidates in the primaries and the November election.

TWENTY-FIVE YEARS OF RESEARCH AND TEACHING

At MIT Jule continued to be a prolific creator of new ideas on the dynamics of atmospheric motion. Space here allows only a short description of the most significant.

During Jule's brief stay in Chicago in 1946-47 C.-G. Rossby had emphasized the existence of internal modes of oscillation for Rossby waves, and in 1948 Jule had used the vertical component of the group velocity in a resting atmosphere to estimate the rate at which influences from above the observed atmospheric volume would corrupt a forecast in the region below. This continued to occupy part of his thinking; for example, in a 1954 letter to D. Martin at Oxford Jule mentioned his interest in studying the upward propagation of Rossby wave energy. In 1961 this interest crystallized into a paper with P. Drazin. This time the important effect of a west-to-east flow in the undisturbed atmosphere was acknowledged. In this paper and in the Charney-Platzman conversations Jule states that the main goal was to show that this propagation is inhibited (i.e., little energy of this type reaches the very high atmosphere), so that a corona, or extremely high temperatures, would not be produced by viscosity. In 1982 a more direct proof of this was suggested by R. Lindzen and M. Schoeberl. The 1961 Charney-Drazin paper is, however, most important for two other less dramatic but more tangible results:

1. Subject to the limitations of WKB analysis, Rossby waves cannot propagate latitudinally or vertically if the wave moves either eastward or too rapidly westward relative to the basic zonal current.
2. Rossby waves will have no nonlinear effect on the basic state unless there is some non-conservative aspect to their motion.

The first of these gave an immediate qualitative explanation of the near absence of Rossby waves in the trade winds of low latitudes and in the westward flow that characterizes the summer stratosphere. Both results have since been extended and amplified in many ways by theoretical and observational scientists, although Jule's attention was quickly attracted again to another aspect of quasi-geostrophic motion.

In 1961 the traveling MIT-Woods Hole seminar heard a presentation by M. Stern on an extension of the Rayleigh condition for stability of a plane-parallel fluid flow (that the vorticity be monotonic). Stern had extended this to the case of a rotating homogeneous fluid with a free surface. Charney's interest was ignited by this; he and Stern then showed that an internal jet in a rotating atmosphere must be stable if the potential vorticity is monotonic and the temperature at the ground is uniform. The former condition is usually satisfied in our atmosphere; the latter is not and is therefore an important element for storm formation.

Hurricanes engaged Jule's attention ever since his car was damaged by a falling tree in 1954 as Hurricane Carol passed over Woods Hole. His first attempts at a numerical model for hurricane motion were unsatisfactory, however. In 1962 when A. Eliassen was a visitor to Jule's National Science Foundation project at MIT, he and Jule returned to the subject, this time considering the question of how hurricanes grow into strong vortices. In his conversations with Platzman Jule recalled that it was K. Ooyama (also a visitor at MIT) who first pointed out that the simple vertical stability considerations traditionally applied to explain individual cumulus clouds did not apply as a whole to the much larger hurricane cloud system. Jule and Eliassen then directed their approach to recognize that the storm was in a state of near dynamic balance and that it must be the frictionally induced indraft of air near the ocean surface that supplied water vapor and latent heat to the vortex. (As an example of Jule's intensity, I recall that much of the final work on this problem took place in the last part of Eliassen's visit, when Jule arranged for them to go off into the New England forests to avoid distraction.)

In 1971 Jule published a short paper on a subject that seems highly abstract, yet is of deep practical significance: the spectrum of large-scale quasi-geostrophic motion in the atmosphere. Kolmogorov and Obukhov in the Soviet Union showed many years earlier that the inertia spectrum of three-dimensional homogeneous isotropic turbulence varied as the minus 5/3 power of the wave number. This is the type of motion created in a wind tunnel and in more recent years has been found under windy conditions in the layer of air near the ground. Several theoreticians had in the meantime considered two-dimensional turbulence as a pure mathematical abstraction (there seemed to be no way to create it experimentally!) and had arrived at a steeper law— wave number to the minus 3 power. Such a flow would seem much smoother to an observer than would the conventional wind tunnel pattern.

Jule was able to present a convincing argument that a system governed by the dynamics of his quasi-geostrophic equations would have a spectrum like that of the hypothetical two-dimensional case, even though its motions are three-dimensional. The practical significance of this (although not emphasized by Jule) is that a weather map constructed from scattered observations makes more sense under the minus 3 law than it does under the minus 5/3 law and all of meteorology leading up to Jule's appearance in 1940 was based on such maps. Without these maps there would have been no meteorological group at UCLA!

Jule's last major research was performed in 1978, only a few years before his death. It had all the pathbreaking characteristics of his previous work. This time it was the result of a seminar he conducted at UCLA where he proposed that they jointly study dynamical models that might explain long-time variability in the atmosphere. The outcome was a joint paper with J. DeVore—a member of the class—on a dynamical theory of blocking. (This meteorological term refers to the sporadic occurrence of large high-pressure systems in preferred positions in middle and high latitudes. By lasting long enough and moving slowly enough they influence the overall hemispheric weather pattern for several weeks. There was no accepted theory for them in 1978.)

The basic dynamical model that Jule suggested to the class was the same as the one he and Eliassen had used thirty years earlier in 1948. But since nonlinearities were presumably important in this problem the class employed a Galerkin-like technique that E. Lorenz and B. Saltzman used in other nonlinear fluid problems—the use of severely truncated trigonometric series. Mountains were included. The results showed that under certain conditions the system would have two stable states, one corresponding to a strong west-to-east current with traveling waves and the other corresponding to a weaker zonal current with large-amplitude quasi-stationary waves. The latter resembled blocking. This research initiated much further exploration of this subject by other fluid dynamicists—an activity that always followed Jule's major papers.

Jule's teaching load was never more than one course and his lecture performance was often halting. But his stellar performance as a mentor for his thesis students more than made up for these defects, if such they were. At times considerable effort was required on his part to avoid interrupting their progress with his travels. His National Science Foundation project (typically funded for three years at a time) always included support for about five graduates in addition to a postdoctoral visitor. He shared supervision of some students with other faculty, but Jule was the sole supervisor of most of them, especially in later years when his GARP duties diminished.

He made a special effort to entrain into atmospheric science students and postdocs who had been educated in re lated fields, such as fluid mechanics and applied mathematics, and this often required as much personal attention as a beginning thesis student. In several instances the efforts he made to support and educate young people from abroad have had a major impact on the atmospheric sciences in their homeland. He worked hard, for example, to support several students affected adversely by the military takeovers in Argentina.

The housekeeping-type committee work associated with academic life was not to his taste and he successfully avoided it. However, he always took a keen interest and personal responsibility in the faculty selection process and served as department head from 1974 to 1977. His appointment as Sloan Professor in 1966 led ultimately to a modest stipend for his personal use. Some of this he dedicated to enhancing the computing facilities for the department, which at that time had no disposable money. In the isolation of Princeton he found it necessary to start a small reading collection in meteorology and related fields. The National Science Foundation continued some support for this in Cambridge and the “Charney reading room” across the corridor from his office became the main library for students, faculty, and visitors in meteorology and physical oceanography.

Jule will certainly be remembered for his research in atmospheric and oceanographic science and for his insight and initiative in the global atmospheric research program. But future chroniclers may well rank his students as an equally great contribution. R. Goody said it well at the memorial service for Jule held in 1989: “As a teacher Jule molded the thoughts of several generations of students. We shall be completing his thoughts and building upon them for a long time to come.”

A. ELIASSEN, R. ELLIOTT, G. Golitsyn, T. Malone, G. Platzman, A. Robinson, and L. Swirnoff helped me on many points in correspondence and conversation.

Jule's personal papers are archived at MIT as “Manuscript Collection MC 184.” They consist of about 27 cubic feet of records, fully archived. I thank Ms. H. W. Samuels and her staff at the Institute for help in examining this collection.

The National Center for Atmospheric Research published a verbatim transcript of an interview with Charney recorded in August 1980, *Conversations with Jule Charney* by George W. Platzman. NCAR/ TN-298 +Proc (1987).

The American Meteorological Society published a memorial volume titled *The Atmosphere—A Challenge* and subtitled “The Science of Jule Gregory Charney,” edited by R. Lindzen, E. Lorenz, and G. Platzman (1990). Besides a full list of his honors, publications, and appointments it contains eleven essays on Charney and his work, reprints of five landmark papers, a series of photographs, and an edited version of the interview with Platzman. I made considerable use of the essay by M. Wurtele on Charney's youth.

The development of interest in numerical weather forecasting at Princeton is described by F. Nebeker in chapter 5 of his thesis, *The 20th Century Transformation of Meteorology*, Princeton University (1989).

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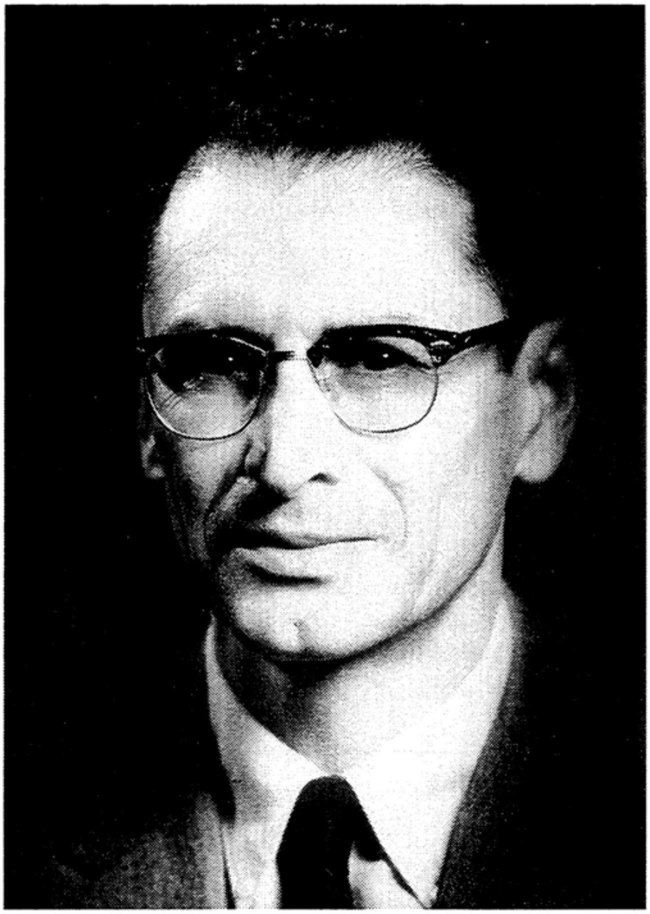

Eugene Feenberg

EUGENE FEENBERG

October 6, 1906–November 7, 1977

BY GEORGE PAKE

HROUGHOUT HIS LONG, PRODUCTIVE career Eugene Feenberg demonstrated a steadfast dedication to theoretical physics. His pursuit of research served not only to advance the science of many-body physics, but it also was for him a great source of fulfillment and inner contentment. He pioneered in applying non-relativistic quantum mechanics to realistic microsystems. Through proficient development and application of approximation methods, he advanced nuclear theory and contributed importantly to the theory of quantum fluids. Quiet, warm, and conscientious, he was an inspiration to his students and colleagues.

Among the young American-Jewish theoretical physicists who contributed so much to U.S. physics in the 1930s and 1940s, Gene Feenberg was somewhat unique in his western rather than urban eastern U.S. origins. He was born on October 6, 1906, in Fort Smith, Arkansas, to Polish emigrant parents, Louis and Esther Feenberg. Louis came to the United States in 1883, later returning to New York for a visit during which he met and married Esther Siegel. The Feenbergs had moved to Texas and then to Fort Smith from South Dakota. Gene was the first graduate of Fort Smith High School to attend college. He went to the University of Texas at Austin, where he majored in mathematics and phys ics. After only three years he graduated first in his class in 1929. Upon the urging of one of his professors, C. P. Boner, Gene applied to Harvard University, where he studied for a doctorate in physics under E. C. Kemble.

Gene's graduate student career was not routine. With the Great Depression settling upon the United States, his father found himself unable to provide financial support for his son's graduate study. Kemble and other professors helped Gene find part-time employment in a Raytheon laboratory in Cambridge —the first of his two experiences with industrial physics.

In 1931 Harvard awarded Gene a Parker Traveling Fellowship with which he studied in Europe for a year and a half. During the summer of 1931 he worked at Raytheon. At this time his father became ill and died. Gene then left for Europe in the fall. Although he later said that he probably was not mature enough to take full advantage of the European study opportunity, Gene spent some time in Munich with Sommerfeld's group, in Zurich with Pauli's institution, and in Rome with Fermi's group. In 1933 he went to Leipzig for a few months, just as the Nazis were seizing power. I have been particularly moved by a copy of a 1933 letter Gene wrote to Professor Kemble in which he described his observations of Nazi anti-Semitic violence in the streets. Over the nearly thirty years I knew Gene Feenberg, fifteen of them as a faculty colleague, I never saw this quiet, peaceful, and reasonable man angry. He was the epitome of thoughtful, tranquil, and balanced wisdom. Yet we all know, from the unfolding of history, how thoroughly justified was the indignation Gene expressed over Nazi violence and persecution when he wrote to Edward Kemble from Leipzig on April 4, 1933, “I walked the crowded streets Saturday boiling with anger; violent emotions visible on my face, I suppose, for passers whispered to be calm.” An American fel low student had persuaded Gene, somewhat against his better judgment, to go out into the streets. Understandably, the events in Germany led Harvard to recall Gene to the campus. There he completed his thesis for Kemble, treating the problem of the quantum scattering of slow electrons by neutral atoms. An important element of this work was the first statement and proof of the quantum optical theorem.

Upon completion of his Ph.D., Gene took an instructorship at Harvard for two years. During this period he began pursuing ("in a relaxed way," he later said) some calculations on nuclear forces. Van Vleck recalled that his own interest in the binding energies of hydrogen and helium isotopes led him to suggest that Gene pursue some neutron-proton force calculations using Gaussian functions. Upon hearing this suggestion, Gene reached into his desk drawer and produced for Van Vleck the calculations he had already made. Gene expressed some doubt that the work was worth publishing, but Van Vleck convinced him that it was. Here we have a prime example of a Eugene Feenberg trait: extreme modesty concerning his own achievements and the value of his work.

In 1935 Gene went to the University of Wisconsin for a year and continued work on nuclear structure and energy levels. In 1936 he collaborated with Gregory Breit in a significant publication noting the charge independence of nuclear forces. At Wisconsin he met Eugene Wigner, with whom he collaborated and helped prepare their 1937 paper on the structure of nuclei between helium and oxygen, showing the importance of the symmetry of the wave function in binding p-shell nuclei. Gene characterized his year at Wisconsin as one of "solid achievement. I got deeply committed to the strong interactions between the like particles."

When Gene moved to the Institute for Advanced Study at Princeton for a two-year fellowship (1936-38) he continued research on the p-shell with Melba Phillips and worked on energy-level spacing in collaboration with John Bardeen and Lloyd Motz. In 1938, on the recommendation of such leading lights in physics as I. I. Rabi, Edward Kemble, and Eugene Wigner, New York University hired Gene for its Washington Square College, where he rose to the rank of associate professor. During World War II, although sought for work at Los Alamos, he took leave of absence to work on radar research at the Sperry Gyroscope Company and advanced the theory of klystron tubes.

In 1946 Gene joined the faculty of Washington University in St. Louis. There he drew on the background of his studies of isomerism and nuclear structure, of assigning orbital configurations based upon spins and moments, and the nature of nuclear beta-decay transitions to provide the foundations for building a modern shell theory of the nucleus. His research in this field led to his book *Shell Theory of the Nucleus*, which was published in 1955.

His first book, however, was *Notes on the Quantum Theory of Angular Momentum* (1953), which I co-authored. When I arrived as a junior faculty member of the Washington University Physics Department in 1948 I found that the department followed the custom of assigning incoming graduate students, however inexperienced in physics they might be, immediately to a research group. Consequently, about eight new graduate students were waiting for me to launch them in nuclear magnetic resonance research even though no such research was yet in place there.

My approach to this problem was to institute a series of seminars for my new research students in which we began to explore this accelerating research activity, then confined mostly to Harvard, Stanford, and Oxford universities. A chal lenge inherent in this situation was that these students scarcely knew any classical mechanics or electromagnetic theory, to say nothing of the rudiments of quantum theory. But some of the experiments we were planning required an understanding—or at least an awareness—of the energy levels of nuclear magnetic dipoles and nuclear electric quadrupoles in laboratory magnetic fields and in crystalline electric fields. The standard quantum mechanics course of that day generally would not develop those energy level calculations, which derive from the quantum properties of angular momentum, and many treatments of the topic resorted to group theory (again, not a ready tool brought by beginning students).

I do not recall precisely how Gene became aware of my dilemma in trying to bring my crew of graduate students sufficiently up to speed in their physics to be able to understand and describe the experimental program we were launching in nuclear magnetic resonance. Perhaps in the brown bag lunch sessions I used to have with Gene and Henry Primakoff (who then shared an office) I may have described my predicament and my frustration with the problem of tutoring the students to a suitable level of understanding. In any event, Gene came to me one day and offered to present a series of lectures in my seminar on the quantum theory of angular momentum, culminating in developing the nuclear moment energy level expressions. This was a real answer to our needs.

Over a period of several weeks Gene presented a seminar series of lectures that succinctly worked out the quantum theory of angular momentum, ending in a session (the final chapter of the subsequent little book) on the applications to nuclear moments and transition probabilities. I took very careful notes during those seminars. Then, to have a little “text book” for future students who would be recruited to the group, I wrote up the lecture notes and reproduced them in mimeographed form. The local roving representative of a publishing company must have seen or heard about those lecture notes, because he dropped by my office one day and suggested that his company might like to publish them. Gene was agreeable, and in 1953, our fifty-six-page booklet appeared—hard cover and all. Later, because the demand exhausted the first printing, Addison-Wesley permitted the Stanford University Press to reprint the book in paperback.

Gene's voluntary effort to help me and my new research students with his seminar presentations is a perfect example of his generosity and human interest in others. The efficiency of his formulation and presentation of the material was characteristic of his sharp mind and his tendency to get right to the heart of the matter. His physics was direct, clear, and concise—truly elegant!

In 1955, shortly after the appearance of our little book, Gene published *Shell Theory of the Nucleus*, building on his extensive research of this topic. Then he began to concentrate on bound-state perturbation theory. Some of this research is treated in volume two of Morse and Feshbach's *Methods of Theoretical Physics.* He also produced another set of mimeographed notes under the title *Notes on Approximation Methods in Elementary Quantum Theory.* These notes appeared in the early 1960s, but this time no publishing house volunteered its services and they have remained unpublished.

The next major thrust of his research was application of the method of correlated basis functions (CBF) to describe the ground state and lower excited states of strongly interacting many-particle systems. CBF theory is useful for nuclear matter and for the helium liquids. Gene focussed on the latter, and a decade of this work led to his *Theory of Quantum Fluids* (1969). It is fitting that the CBF methods he pursued for the helium fluids were subsequently applied with success by others to nuclear problems, the domain to which he had earlier contributed so much through his shell theory research. Gene also had a pedagogical interest in the theory of special relativity and he occasionally published on the topic.

In his family life he was a devoted husband and father. He married the former Hilda Rosenberg; their two sons, Andrew and Daniel, both have successful professional careers, for which their father not only provided encouragement but also served as a superb role model. Gene was equally effective as a professional example to his research students, who became something of an extended family for him. His students developed a great respect for his incisive approach to physics and for his personal integrity; they readily acquired a deep affection for him. For his students as well as his collaborators, colleagues, and friends, Eugene Feenberg was a sympathetic source of sensible counsel.

When I became chairman of his department at the age of twenty-eight, I could always rely on him for balanced, sane advice. Although he strictly adhered to teaching and research, his keen observation of administrative and political matters and his high ethical standards conferred admirable wisdom, and he imbued his students with a sense of responsibility for the greater social good. It is interesting to note a few of his students who went on to larger responsibilities. George Trigg became the editor of *Physical Review Letters.* Chia-Wei Woo is the president of the new Hong Kong University of Science and Technology. Walter Massey has just completed service in the directorship of the U.S. National Science Foundation.

Gene exercised the highest standards of excellence and integrity, both in his professional work and in his personal life. He was a quiet, self-disciplined man, who used words economically but with great precision, elegance, charm, and wit. If he had any fault at all, I would say he was modest to excess. In closing this brief biography, I affirm and commend to you the thoughts of four colleagues[1] who wrote in 1979:

Secure in his family and in himself, he was guided in all relationships by a profound sense of the importance of individual human dignity. He cared deeply for his many students, who remained close to him long after they left St. Louis, and for the many talented collaborators and colleagues which a rich career brought to him. To all who were fortunate enough to know him, Eugene Feenberg was a very special human being.

NUMEROUS INDIVIDUALS AND SOURCES assisted me in preparing this brief biography. Chief among those is Mrs. Hilda Feenberg, who supplied me with many materials. Among the resources upon which I drew heavily were the transcript of an oral history interview of Eugene Feenberg by Spencer R. Weart of the Center for the History of Physics, and an introductory forward to the April 1979 issue of *Nuclear Physics*, which was designated a Feenberg commemorative issue. Mr. Herbert Weitman furnished the photograph and Professor John W. Clark contributed substantial help on a number of factual matters.

NOTE

1. K. A. Brueckner, C. E. Campbell, J. W. Clark, and H. Primakoff. *Nuclear Physics* A317:iv (1979).

PERSONAL HISTORY

Born October 6, 1906
B.A. and M.A., University of Texas, 1929
Ph.D., Harvard University, 1933
Instructor, Harvard University, 1933-35
Lecturer, University of Wisconsin, 1935-36
Fellowship, Institute for Advanced Study, 1936-38
Assistant professor, then associate professor, New York University (Washington Square College), 1938-46
Engineer, Sperry Gyroscope Company (on leave from NYU), 1941-45
Associate professor, then professor, Washington University, 1946-75
Visiting Higgins Professor of Physics, Princeton University, 1953-54
Wayman Crow Professor of Physics, Washington University, 1964-75
Visiting professor of physics, The State University of New York at Stony Brook, spring semester 1969
Lecturer at Escuela Latino Americana de Fisica, Universidad Nacional Autonoma de Mexico, July 1-19, 1974
Elected to the National Academy of Sciences, 1975
Professor emeritus, Washington University, 1975-77
Died November 7, 1977

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R E G

ROBERT EDWARD GROSS

July 2, 1905–October 11, 1988

BY FRANCIS D. MOORE AND JUDAH FOLKMAN

URING A REMARKABLE CAREER as one of America's great pioneers of surgery Robert E. Gross made many contributions that have altered the practice and understanding of surgery, pediatrics, and cardiology throughout the world. These include the performance for the first time of successful major surgery on the great vessels near the heart, with the ligation of a patent ductus arteriosus on August 26, 1938; the first successful corrective surgery of coarctation of the aorta in 1945; the performance of several other important, innovative surgical procedures; and one of the largest series in the world of successful open heart repairs of congenital anomalies of the heart in infants and children. In 1941 Dr. Gross joined his professor, Dr. William E. Ladd, in publishing the first textbook on surgery in children, defining for the first time this new field of learning and practice.

For more than forty years Robert Edward Gross was engaged in pediatric surgery at Harvard Medical School and Children's Hospital in Boston, Massachusetts. From 1947 to 1966 he was the William E. Ladd Professor of Child Surgery at Harvard Medical School and the surgeon-in-chief at that hospital.

Gross was born in Baltimore, Maryland, on July 2, 1905, the son of Charles Jacob and Emma Houch Gross. His fa ther was manager of the Stieff Piano Company, a firm that had been owned by his grandmother's family. Robert had five sisters and two brothers with whom he grew up in Baltimore. As a boy, Robert was described as a sensitive, somewhat shy tinkerer, an avid reader, a keen observer, and an enjoyer of the outdoors. During his summer holidays in high school he journeyed first by train and boat and later by jalopy to a central Minnesota farm to work. There he strengthened his instinctive concern for living things and nature's ways. These rewarding summertime experiences led to his choice of Carleton College in Northfield, Minnesota, for his next step in education. He had yet to form any firm career plans, but enjoyed chemistry and was considering a career in chemistry. As a Christmas gift from a friend he received Harvey Cushing's biography of Sir William Osler. According to Gross he nearly dropped out of college to find the time to read this book. By the spring of this first year he had decided to enter medicine and to attend Harvard Medical School, because his newly found author-idol, Dr. Harvey Cushing, was teaching there. He graduated in 1927 with honors from Carleton, Phi Beta Kappa, and entered Harvard Medical School.

At the medical school Gross was one of the top students of his class, much stimulated by a fourth year elective surgical service with William E. Ladd at Children's Hospital, as well as the teachings of Cushing, who was in the last five years of his tenure at Harvard. After receiving his M.D. degree in 1931, with honors, and becoming a member of Alpha Omega Alpha, Gross took a postgraduate training period of two years with Dr. Simeon Burt Wolbach. Wolbach was the George Cheever Shattuck Professor of Pathology and chairman of the Department of Pathology at Harvard Medical School, and chief of the departments of pathology at the Peter Bent Brigham, Boston Children's, and Lying-In hospitals. Wolbach was to become a great friend and confidant of Gross, with whom he enjoyed important professional relationships, particularly the opportunity to study fresh human material at autopsy, including many congenital anomalies, but most especially those of the heart and great vessels. In addition, Wolbach provided Gross with his only regular relaxation—early morning horseback rides on weekends near his home in Framingham, Massachusetts. Wolbach kept a stable of riding horses in Framingham, the town where Gross later was to establish his home.

In 1933 Gross entered a surgical training program under Dr. Elliott Cutler at Peter Bent Brigham Hospital. Cutler was the successor to Harvey Cushing as Moseley Professor. The influence of Cutler naturally included special attention to surgery of the heart and great vessels, because in 1924 he had performed operations on a few patients suffering from mitral stenosis. Several years later he published a review of cardiac surgery, including a prediction that one of the first successful procedures around the heart would be the ligation of the patent ductus arteriosus. This rather common anomaly (failure of closure of an artery essential in fetal life) condemned children to heart failure, infections, and an early death.

In 1937, as a resident at Peter Bent Brigham Hospital, Gross was appointed by Cutler to the position of George Gorham Peters Traveling Fellow. For nine months this enabled him to visit the most active surgical centers of Britain and the continent. In Edinburgh he worked at the Royal Hospital for Sick Children and in the laboratories of Sir David Wilkie.

After returning to the Harvard medical community in 1938, he was appointed chief resident in surgery under Ladd at the Boston Children's Hospital. There he was stimulated by a pediatric colleague, Dr. John Hubbard. Working in the laboratory and autopsy room the two young men, one a pediatrician and the other a surgeon, worked out a surgical approach for the ligation of the patent ductus arteriosus. During August of 1938, while Ladd was on holiday, but with the approval of the acting chief, Dr. Thomas Lanman, Gross performed the first successful ligation of the patent ductus arteriosus. In an unpublished autobiographical document written in 1987 (now in Judah Folkman's library) Gross described several remarkable events in his career, including the following:

As time went on there was an urge to attack several blood vessel anomalies which had been seen previously at autopsies during my training two years before. The first consideration had to do with possibly attacking a ductus arteriosus that had remained patent. How could one be closed off surgically? It had never been accomplished anywhere before. Possible surgical approaches to a ductus were practiced on humans in the autopsy room and on animals. After deciding clearly on the best approach, there seemingly would be no difficulty or risk in ligating a patent ductus. Our first operation for such a procedure was performed on a seven-year-old girl on August 26, 1938. The postoperative course was uneventful and without worry. The next morning she was up and out of bed and around the ward. She was discharged in ten days. Our patent ductus operations were always done through a left antero-lateral approach through the third intercostal space.

Eleven children were operated upon satisfactorily for ductus closure by ligation. The twelfth was a fourteen-year-old girl also treated by ligation. She was well at the time of hospital discharge. Two weeks after that, there was a party for her at her home. While dancing with friends, she suddenly collapsed on the floor and was instantly dead! The family permitted an autopsy examination, which showed that the ductus ligature had cut through, permitting massive hemorrhage. I never again ligated a ductus. All subsequent patients were handled by careful local dissection placing double clamps on the ductus, then cutting the ductus in half and meticulously closing each end by suturing. This became the standard technique, giving completely satisfactory results. It was used with total satisfaction up through the last ductus operation I performed, which was number 1,610, in March 1972.

The patient who had the operation recalled at the age of fifty-eight that in the early 1980s she visited Dr. Gross in his retirement home in Vermont. She stated, "We reminisced about our surgery and he laughed and said, 'You know, Lorraine, if you hadn't made it, I might have ended up here in Vermont as a farmer.'"

Almost immediately Gross began his studies on surgical correction of congenital narrowing or coarctation of the aorta. Gross described this in the same autobiographical note:

As a next endeavor, attention became concentrated in 1938 on the possibility of surgical treatment for coarctation of the aorta. I was joined in this endeavor by an outstanding assistant resident, Dr. Charles Hufnagel. In the laboratory, we practiced on dogs to find the best way to remove a short segment of abdominal aorta, bringing the remaining ends together for a satisfactory anastomosis. It was not at all difficult to establish a good arterial channel. But, alas, some of the animals had extensive hind-leg paralysis postoperatively. I was so discouraged that orders were given to abandon the whole project.

In mid-April 1939, Dr. Clarence Crafoord, from Sweden, visited Harvard, and Dr. Elliott Cutler asked me to show him around the Brigham and the Children's hospitals and also our experimental laboratory across the street. This I had the pleasure of doing. We ended up in the surgical research area. Dr. Hufnagel, extremely competent in research activities, summarized for Dr. Crafoord various projects that were in progress. Finally, he brought four dogs out of their cages and let them run freely. He said to me, "Dr. Gross, each of these dogs has had resection of an abdominal segment of aorta, and anastomosis of the remaining ends." I asked him how was it that these dogs could run? He answered, "They were done under general anesthesia and then laid down on a bed of ice. Through an anterior abdominal incision we did the usual aortic resection and reanastomosis." It was wonderful to have someone in the laboratory who could think properly! Besides my learning how to attack resections in humans accompanied by hypothermia, I certainly got into the habit of listening to young men!

The European war was still on, and all thoughts of attacking coarctations in humans had to be put off. Immediately after the global conflict, within a few days of each other, we in Boston and Crafoord in Sweden did a successful repair of a coarctation in humans. Since then, over a span of 26 years, 825 patients with coarctations were operated upon by us. In 104 of these, it was necessary to insert a graft to fill in a rather long gap.

Although coarctation of the aorta is a relatively rare congenital anomaly, this resection of the aorta with anastomosis, and in some cases with the interposition of a preserved human aorta homograft, became the model on which surgery for aortic aneurysm (pathologic enlargement) in the adult was based. Within a few years these procedures were carried out in large numbers all over the world for thousands of older patients, some of them in the elderly age groups suffering from aneurysm of the aorta.

In his approaches both to patent ductus arteriosus and to coarctation of the aorta Robert Gross demonstrated to the world that anatomical study and a carefully planned surgical approach (with extensive rehearsals both in the autopsy room and in the experimental animal) could result in successful treatment of very ominous and previously forbidding diseases of the heart and of the great vessels. In operations on these large vessels, which contain blood under tremendous pressure, even a momentary lapse of technique could lead to an instantly fatal outcome. These events were widely heralded and immediately recognized throughout the world as amongst the first successful operations on what we now call cardiac surgery even though they were procedures carried out on the great vessels within an inch or two of the heart, rather than on the heart itself.

Other innovative procedures carried out by Gross included the correction of an anomalous arterial ring around the esophagus. Again, his own words tell the story of these developments:

In June 1945 our attention was drawn to a teenage boy who had difficulty in swallowing and also had rather noisy respiratory sounds in the chest.

Roentgenographic studies quickly and clearly showed a pulsating vessel behind the esophagus, pressing upon it. Also, there was a pulsation on the anterior surface of the trachea. These facts could very clearly be substantiated by roentgen studies with a swallow of barium, and also by injecting a little radiopaque material down the trachea.

At surgical exploration, there was an amazing finding of a "double aortic arch," the first part of the ascending aorta splitting, with half going up and across behind the esophagus and the other limb going up in front of the trachea, both branches meeting on the left side to form the descending aorta. The anterior arch was of much larger size than the rear one. It was not at all difficult to divide the posterior arch and suture closed each end thereof. This completely freed the esophagus from its compression. The anterior arch, being the larger of the two, was intentionally saved. Severance of the posterior arch had relieved tension on the anterior limb and allowed it to swing free from the trachea. It was all a surprise, and a very happy outcome. This case opened the way for studying and identifying other patients with anomalous and troublesome pressure on the esophagus or the trachea, or both simultaneously. The list of patients coming to operation included those with double aortic arch, anomalous position of the left subclavian artery, anomalous left carotid artery, aberrant right subclavian artery, and others. All of these could be satisfactorily operated upon with excellent results. Between June 1945 and September 1971, we operated upon 165 of these patients with arterial anomalies without fatality and with very satisfactory results.

Gross was also one of the first to operate successfully on tiny newborn babies for life-endangering anomalies of the circulatory system. He writes of some of these experiences:

Some cardiac anomalies are so serious that they must be repaired within two or three days after birth. An example of this is the so-called "total anomalous pulmonary vein drainage." Essentially, the critical condition is that the lungs become greatly congested because pulmonary veins do not take away the oxygenated blood and deliver it to the left side of the heart. A perfect example of what could be accomplished was shown on March 2, 1967, when a 36-hour-old baby in great respiratory distress was brought into the hospital. The cardiologists were immediately "on the ball." Appropriate studies showed tremendously congested lungs, almost certainly indicating very poor drainage of blood through the pulmonary veins. Opera tion was undertaken within a few hours after admission. Our small pump-oxygenator was employed very satisfactorily. There was no difficulty in raising the heart forward and upward, getting an excellent view of what was behind it. There was no vein drainage into the left side of the heart. All the pulmonary veins gathered into a single large trunk, running downward through the diaphragm. It was not at all difficult to ligate the lowest part of this trunk just at the diaphragm, and then open the trunk above this, so it could be widely anastomosed into the back of the left auricle. There was a fast and very satisfactory recovery.

The child grew in a rapid and completely satisfactory way. She was active and a very good student at school. To have this wonderful youngster, at the age of 17 years, voluntarily come in to see me, say "hello," and give thanks was gratifying beyond description. She was bright, active, rather athletic, a superb student in her fourth year of high school and headed for college. This all made me thank the Lord for what could be accomplished by surgery.

In preparing his team for operations on the heart itself Gross was influenced by the previous work of Dr. John Gibbon, Dr. Kirklin, Dr. Lillehei, and Dr. Blalock in developing pump-oxygenator equipment for the adult. Gross himself was gifted in the use of tools; he kept in his operating room a special tool kit, which the nurses had painted gold and indicated that it was for his use only. In developing a pump-oxygenator adapted to use in tiny babies, these mechanical skills of Gross became especially important. He tells the story:

One of the most successful projects in the laboratory was to devise and build a small pump-oxygenator so that surgical repair of some of the serious cardiac anomalies in babies could be successfully accomplished. In the basement of the laboratory there was a very good machine shop. And we fortunately obtained a top-flight machinist who was very productive and who stayed with us until he was 80 years old (Mr. Fred Savage). We drew up sketches and specifications of what we wanted, and he had great skill in building what we needed. The pump-oxygenator was only 18 inches long, 10 inches wide, and 10 inches high. It had a horizontal glass enclosure and carried a horizontal rotating shaft driven at variable speeds. This rotating shaft carried rotating metal discs driven by a variable-speed motor. Blood could be pumped through the machine, picking up oxygen, and then returned to the patient.

It was with this device that Gross carried out his extensive open cardiac repairs on newborn babies or very young infants with congenital heart disease.

Despite this abiding interest in cardiovascular disease Gross did not lose his longstanding focus on the broad issues of surgical care of the infant. In 1941 he coauthored with William E. Ladd a book titled *Abdominal Surgery of Infancy and Childhood.* This defined the field of pediatric surgery for the first time and was the standard textbook in the field for many years. It was not merely a scholarly clinical report and academic statement; it was also a casebook based on detailed study of a large number of patients cared for by Ladd and Gross and their colleagues and analyzed from the point of view of embryology, pathology, and pediatrics. Many of these disorders were for the first time grouped into recognizable categories or syndromes that now could be recognized and analyzed throughout the world. Gross expanded this book considerably and published it in 1953 as *Surgery of Infancy and Childhood.* It became a classic and was published in more languages than any other medical text at the time.

There were many difficulties in early attempts to open the heart for repair. Prior to the development of the extra-corporeal pump-oxygenator Gross moved towards a solution for some cases by designing in 1952 an ingenious rubber well, which could be temporarily sutured to the atrium so that blood could well up in it. At the low pressures of blood in the atrium, blood could accumulate in the well without overflowing. He then could operate on atrial septal defects in a “deliberate and unhurried manner, albeit under a pool of blood and without direct vision.”

Despite an intense research orientation, Gross, in per forming new operations in life-endangering conditions and in expanding and defining the field of children's surgery, was a humane physician, insisting that each surgeon look after every aspect of his own patients' welfare, never entrusting such to others. He was personally acquainted with all of his patients and their families and set a high standard for the ethical and humane practice of pediatric surgery.

In 1966 Gross resigned his position as surgeon-in-chief at Children's Hospital to take on a position heading up cardiac research as director of the cardiac program at Boston Children's Hospital. At that time Dr. Robert H. Ebert, dean of the faculty of medicine at Harvard Medical School, said:

It is fitting that this program, which has such great potential in helping young heart patients, should be headed by Dr. Gross who already has contributed so much in this field. His new hospital appointment is in recognition of the many advances in pediatric cardiac surgery, which have come about because of his work, and as a logical progression in his splendid career.

Honors awarded to Dr. Gross were numerous. He is the only physician to receive the Albert Lasker Award twice. In 1954 he was given this honor with the citation: “Whereby surgery upon the heart and great vessels was at last removed from the realm of the experimental trial and placed upon a firm clinical basis.” He shared the award with Dr. Alfred Blalock and Dr. Helen Taussig, also pioneers in the surgical correction of children's cardiac disorders and anomalies. In 1959 he was given the Lasker award again, this time for his “foremost role in the extension of surgery to the relief or cure of other cardiovascular defects.”

His honors included the Gold Medal of the American Surgical Association and honorary degrees from Carleton College (Minnesota), Suffolk University (Boston), and the universities of Louvain (Belgium), Turin (Italy), and Sheffield (England). He was elected to the American Pediatric Hall of Fame and was awarded the Sheen Award of the American Medical Association, the Rudolf Matas Vascular Surgery Medal, and the Roswell Park Memorial Medal. In 1957 he was made a Grand Officer of the Order of Leopold by the Belgian government, in part because of his successful operation on fifteen-year-old Prince Alexander. Greece bestowed on him the Gold Cross of the Royal Order of the Phoenix. In 1970 he was awarded the Henry Jacob Bigelow Award of the Boston Surgical Society, its highest honor.

Gross was a founder, member, and president of many surgical and pediatric societies. He was a founder and the first president of the Society for Pediatric Surgery and a founder of the Board of Thoracic Surgery and the American Board of Surgery. He was made a member of the Academy of Surgeons of France and the British Association of Pediatric Surgeons. He was president of the Massachusetts Heart Association.

The Robert E. Gross professorship of pediatric surgery was established in 1985 at the Harvard Medical School and Dr. William Hardy Hendren was appointed the first incumbent.

In 1984 Gross received an honorary degree from Harvard University. His citation read: “With keen mind and compassionate hands, this brilliant surgeon has brought health to the youth of the world.”

A shy man, Gross eschewed ostentation of any kind. He refused to meet with newspaper reporters, even at the time of such remarkable events as his treatment of President Kennedy's baby in a hyperbaric oxygen chamber. This shyness was sometimes mistaken for aloofness. That such was not the case is demonstrated by the fact that he was referred to as “my doctor” by his patients throughout his life. When they grew up and had children of their own they consulted him first whenever anyone in their family was sick. It was not unusual for him to make an evening house call on a postoperative child. Medical students, usually in awe of him, found a sympathetic teacher who called them “son.” He also protected his residents from the distraction of financial worry. His personal check often appeared when a resident's baby was born or when a wife was ill, or an invitation was extended to stay at his home temporarily until the crisis was over. Gross' reserve, however, was not born of timidity. For thirty-five years of his life he was the epicenter of a surgical revolution and set in motion the development of cardiovascular surgery, establishing new principles used today throughout the world, both in the repair of congenital anomalies in children, in the surgery of infancy and childhood, and the application of many of these principles in the adult.

In the later years of his life Gross was severely incapacitated by a back ailment. Despite this, he continued to attend clinical meetings, celebrations, and teaching sessions of the Harvard Medical School and Children's Hospital. He died quietly at a nursing home in Plymouth, Massachusetts, on October 11, 1988.

Dr. Gross was elected to the National Academy of Sciences in 1975. His career epitomizes the remarkable combination of science, art, skill, and clinical achievement that characterizes leadership in surgery. His scientific research was not carried out at a laboratory bench, but in the clinics, operating rooms, autopsy rooms, and animal laboratories of Harvard Medical School and Children's Hospital. The opinions held of Dr. Gross by persons in medicine and pediatrics are exemplified by the comment of Alexander S. Nadas, long a colleague of Dr. Gross and cardiologist-in-chief, emeritus, at Boston Children's Hospital: “Dr. Gross was a remarkable, extremely innovative surgeon. His impact on the field of cardiovascular surgery is immeasurable.

He challenged the belief that the human heart was beyond repair, and brought heart surgery from the experimental laboratory to clinical reality."

HONORS AND DISTINCTIONS

DEGREES

1927	B.A., Carleton College
1931	M.D., Harvard University, Medical School

HONORARY DEGREES

1951	D.Sc., Carleton College
1959	M.D., Honoris Causa, Louvain University
1961	M.D., Honoris Causa, Turin University
1962	D.Sc., Suffolk University
1963	D.Sc., University of Sheffield
1984	D.Sc., Harvard University

HOSPITAL AND UNIVERSITY APPOINTMENTS

1934-36	Instructor in pathology, Harvard Medical School
1937-39	Instructor in surgery, Harvard Medical School
1939-40	Junior associate in surgery, Peter Bent Brigham Hospital
1939-42	Associate in surgery, Harvard Medical School
1939-46	Associate visiting surgeon, Children's Hospital, Boston
1940-46	Senior associate in surgery, Peter Bent Brigham Hospital
1942-47	Assistant professor of surgery, Harvard Medical School
1947-88	Ladd Professor of Children's Surgery, Harvard Medical School
1947-67	Surgeon-in-chief, Children's Hospital, Boston
1952	Surgeon-in-chief, pro-tempore, Ohio State University
1967-72	Surgeon-in-chief, cardiovascular surgery, Children's Hospital, Boston

PROFESSIONAL AND HONORARY SOCIETIES

1953	Honorary member, Reno Surgical Society
1955	Honorary member, Dallas Southern Clinical Society
1956	Honorary member, Buffalo Surgical Society
1958	Honorary appointment, American National Red Cross, North Shore chapter
1961	Honorary fellow, Spokane Surgical Society
1967	Honorary citation, Barnstable County chapter, Massachusetts Heart Association

FOREIGN SOCIETIES	
1959	Officer of the Order of Leopold, Belgium
1959	Honorary officer of the International Red Cross, Belgium
1960	Honorary member, Pediatric Society of Guatemala
1964	Honorary member, La Bocedad de Cirurgia Pediatrica de Mexico
1968	Honorary member, Surgical Infantil Argentina Society
1973	Honorary fellow, Royal College of Surgeons of England
COMMITTEES	
1954-55	Director, American Heart Association
1958-60	Director, American Heart Association
1960	President, Massachusetts Heart Association
1963-64	President, American Association for Thoracic Surgery
1969-70	Board of directors, Massachusetts Heart Association
1970-71	First president, American Pediatric Surgical Association
EDITORSHIPS	
1970	Editorial board, *Surgery*
AWARDS	
1940	F Mead-Johnson Award, American Academy of Pediatrics
1940	Rudolf Matas Vascular Surgery Award, Tulane University
1954	Children's Service Award, Toy Manufacturers of America
1954	Albert Lasker Award, American Public Health Association
1956	Roswell Park Gold Medal, Buffalo Surgical Society
1957	Gold Medal, Louisville Surgical Society
1959	Laeken Award, Brussels, Belgium
1959	Gold Medal, Detroit Surgical Association
1959	Billroth Medal, New York Academy of Medicine
1961	Gold Medal Award, Golden Slipper Square Club of Philadelphia
1962	Award of the Brotherhood Temple Ohabei Shalom, Brookline
1965	William E. Ladd Medal Award, Surgical Section, American Academy of Pediatrics

1965	Gold Cross, Royal Order of the Phoenix of the Greek Government
1968	Dennis Browne Gold Medal, British Association of Pediatric Surgeons
1969	Dr. Rodman E. Sheen and Thomas G. Sheen Award, American Medical Association
1970	Alfred Jurzykowski Medalist, New York Academy of Medicine citation with Dr. Farber and Dr. Neuhauser and the Children's Hospital Medical Center
1970	Henry Jacob Bigelow Memorial Medal
1971	Tina Award, Foundation for Children, Houston, Texas Certificate of Award, 26th annual Philadelphia book show, presented to W. B. Saunders Company for *An Atlas of Children's Surgery* by Dr. Robert E. Gross
1973	Distinguished Service Medal, American Surgical Association

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1953 With E. Watkins, Jr., A. A. Pomeranz, and E. I. Goldsmith. A method for surgical closure of interauricular septal defects. *Surg. Gynec. & Obst.* 96:1-23.

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1959 With S. Farber and L. W. Martin. Neuroblastoma sympatheticum. A study and report of 217 cases. *Pediatrics* 23:1179-91.

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HARRY GRUNDFEST

January 10, 1904–October 10, 1983

BY JOHN P. REUBEN

ARRY GRUNDFEST'S CONTRIBUTIONS to (and his influence on) the field of neurophysiology were extensive, touching all corners of the field, providing inspiration and direction to more than 100 young scientists, and proposing mechanisms for how membrane electrical events determine cellular processes. These include conduction and excitation in nerve fibers, chemical and electrical signalling between excitable cells, and excitation-contraction coupling.

Considering the breadth of his contributions and the well over 500 publications describing many of them, it is difficult within an introductory paragraph or two to select what may be considered the overriding contributions. For this reason I emphasize that the following is my personal selection and clearly others may have a different point of view.

A concept that he put forth and that evolved over numerous publications was the heterogeneity of cellular membranes and the role this attribute of membranes plays in determining cellular activities. Initially his interpretation of currentclamp data led to the proposal that cell membranes are a composite of two types of membranes, electrically excitable and electrically inexcitable. This concept gave rise to the view that membranes are mosaic structures in which different regions of the bounding cell membrane possess varying amounts of a given type of ionic conductance. This concept of the bounding membrane was incorporated into models to describe synaptic, secretory, and contractile signalling processes.

The mosaic structure of membranes has been well substantiated by present-day techniques, which allow for biochemical identification and separation of various segments of cell membranes and classification of the ion channel types associated with them. Examples are the tubular system in muscle and synaptosomal fraction of nerve cells.

Another and equally important contribution was his establishment of a laboratory and training program, based on his philosophy, that taught and inspired a large number of scientists whose names and research are well known by those in the field of neuro/electrophysiology.

Harry Grundfest was born on January 10, 1904, in Minsk, Russia. His father Aaron, a rabbi, and mother Gertrude had four children, three boys and one girl, before coming to this country in September 1913. Harry graduated from high school in Kearny, New Jersey, in 1921 and then entered Columbia College where he received his A.B. degree in 1925, M.A. in 1926, and his Ph.D. in zoology and physiology in 1930. The parents emphasized the value of a higher education and consequently both brothers became medical doctors and his sister a biologist.

In January 1926 he married Rose Danzig, who became recognized as a fine artist and sculptress. They had one child, daughter Brooke, who maintained the family tradition, receiving her Ph.D. degree in anthropology.

SCIENTIFIC CAREER (1929-1954)

Harry's fellowship years were spent at the laboratory of biophysics at Columbia University (National Research Council, 1929-30) and the Johnson Foundation for Medical Physics at the University of Pennsylvania (National Research Council and Johnson Fellow, 1930-32).

His research during these early years was concerned with the excitatory processes of the visual system (spectral sensitivity of sunfish), nerve-muscle complex, and muscle. His first instructorship began at Swarthmore College (1932-33) and continued in the Department of Physiology at Cornell University Medical School through 1935. Here he laid the foundation for the classical studies with H. S. Gasser by publishing several papers on the excitatory properties of nerve bundles and their compound action potentials.

Much of his research during the first part of his tenure at the Rockefeller Institute (1935-45) was done in collaboration with H. S. Gasser. Their work was a milestone in neuro-physiology, determining for the first time the different nerve fiber types whose action potentials compose the compound spike of nerve bundles, and describing the relationship between nerve fiber diameter and conduction velocity.

World War II temporarily changed the course of Harry's research. At the time he was national secretary of the American Association of Scientific Workers. In this capacity he wrote an article titled “Science in the War” that appeared in the *Weekly Science Page*, Science Service, Washington, D.C. He backed his words in this article by taking a leave of absence from Rockefeller to spend several years working on government projects. The research was done at two sites, the Climatic Research Unit at Fort Monmouth Signal Laboratories and the Wound Ballistic Unit at Princeton University. This work dealing with nerve regeneration and wound damage to the peripheral and central nervous system was mainly described in restricted reports, but some of this material was published in biological journals.

At the end of the war Harry returned to his alma mater to stay for the remainder of his career. Promotions came rapidly from research associate, Department of Neurology (1945), to assistant professor (1947) and associate professor of neurology (1949). Much of his research during this period was done in collaboration with David Nachmansohn and concerned the role of acetylcholine (Ach) in excitation and conduction in nerve and muscle.

The view that Ach was directly involved in the excitation and conduction processes was held by Nachmansohn throughout his career. While this concept received support from their collaborative research, subsequent findings by Harry as well as many other neurophysiologists were incompatible with this proposed role for Ach.

Harry's activities beyond research during this period included membership on the advisory committee for planning the biology building for the Weizmann Institute and chairperson of the medical advisory board of the Hebrew University and Hadassah (1950-54). He traveled to Israel in 1950 to deal with matters concerning the organization of the medical center. Subsequent planned trips had to be cancelled due to the government's denial of his passport renewal. The latter was a product of the “dark ages” era for our country, which was under the influence of Senator Joseph McCarthy and his supporters.

In 1953 Harry was summoned to appear before the McCarthy Committee. He testified that he was not a communist, but he refused to discuss other affiliations, political views, and those of friends and colleagues by invoking the first, fifth, and sixth amendments to the Constitution. Research continued throughout the McCarthy era in spite of the government's withdrawal of funds and lack of support from some faculty members and previous collaborators. Without the strong backing of Houston Merritt, who was both chairperson of the Department of Neurology and dean of the medical school at the time, he might not have survived the unwarranted persecution with his university position intact.

PURPURA YEARS (1954-1961)

Research activities were not diminished in spite of the obstacles placed in Harry's path by the government and some factions in the medical school. In fact, research on electrocortical phenomena was pursued at a rapid pace and, as a consequence, numerous publications appeared during this period. Over forty papers were published by Grundfest, Purpura, and colleagues between 1955 and 1951. This prolificacy was predictable for it was a product of the collaboration of two individuals with comparable enthusiasm and drive for deciphering the basis of the electrical activity monitored topically from cerebral and cerebellar cortices.

Both Harry and Dominick Purpura shared the belief that the dominant contribution to the cortical electrical activity was from excitatory and inhibitory postsynaptic potentials (e.p.s.p. and i.p.s.p.) at the level of the dendrites, a belief that was well supported by research findings in subsequent years. Part of their approach, described by Harry in a number of review articles, was based on the view that neurons in the mammalian central nervous system (CNS) have the same basic properties as peripheral junctional systems that are more readily characterized in lower animals. Furthermore, an axiom of this theorem is that the complexity of the CNS is not due to special properties of the neurons, but results from the multiplicity of cells and their interconnections.

Another contribution of the Grundfest-Purpura era was the mode in which they used pharmacological agents to obtain information about the system under investigation. This approach laid some of the foundation stones for present day neuropharmacology. Agents like ω-amino and ω-guanidino compounds were viewed as having specific syn aptic activities and thus were used (in particular GABA) as tools to dissect electrical signals composed of i.p.s.p. and e.p.s.p. This work provided the impetus for investigating the action of these compounds on the crustacean neuro-muscular junctions (n.m.j.). This research, which led to the conclusion that GABA was the transmitter for the inhibitory n.m.j., was done in part by myself under Harry's guidance during my tenure as a Grass Fellow at the Marine Biological Laboratories in Woods Hole, Massachusetts, in 1958. I joined the laboratory as a postdoctoral fellow the following year and stayed on as a member of the department for the remainder of Harry's career.

KRAMER BUILDING YEARS (1961-1976)

The McCarthy Committee hearings and the black list were behind him and the renaissance in neurophysiology at Columbia College of Physicians and Surgeons began. Space in the medical center was limited and, with the increase in personnel following reinstatement of government funds, additional facilities were needed. This was attained by renting the upper story of a building located one block east of the medical center and owned by Kramer Medical Supply Company. The latter's offices and business were on the ground floor of the building.

While housed in the Kramer Building about eighty postdoctoral fellows and four predoctoral fellows worked, trained, and completed requirements for Ph.D. degrees. Most of these fellows were from foreign countries, including Argentina, Brazil, Colombia, England, Israel, Japan, Mexico, Poland, Russia, Scotland, Switzerland, Venezuela, and Yugoslavia. Colleagues who were familiar with this international make-up of the laboratory often referred to the second floor as an annex of the United Nations.

Harry's design of the floor space in many ways reflected his philosophy regarding the teaching and training of young scientists. Eight research rooms surrounded a large central area that contained the following prioritized items: a full wall-length blackboard, a library containing most of the current biological journals, and an H-shaped arrangement of tables and chairs.

This spatial arrangement of the laboratories around the central room maximized interactions amongst the fellows and was not a favorable design for encouraging timidity. The blackboard was occupied most of the time. New findings, schemes, and equivalent membrane circuits were chalked across the board. Discussions about interpretations arose and debates ensued. These debates frequently carried over to the lunch period when most of the fellows assembled around the tables. This central area also served as a seminar room for more formal presentations by visiting scientists.

On some occasions science was not on the lunch menu and politics and philosophy were on tap. In view of the international make-up of the group, U.S. foreign policies were a subject commonly discussed. While Harry did not hesitate to initiate or join in ongoing discussions and debates (science or other subjects), he more frequently orchestrated them.

Before leaving this subject it is necessary to comment about the lunch scenes in the central room. Something that never failed to draw the attention of new members and first-time visitors was the placement of several gallon jars on the center table just before the lunch period. The contents could be properly prepared only by a special store located somewhere on the lower east side of Manhattan. The contents, called barrel-cured Kosher sour dill pickles, were delivered by the store at regular intervals. A special pickle fund (money from private industry, not government funds) provided these items for all who wished to partake in lunch at Kramer's. This specialty of the house was not part of Harry's teaching philosophy; rather, as some said, it was his attempt to lose weight (the only food he consumed at lunch). I personally believe the latter was an excuse and the real basis for this practice was Harry's great love for properly prepared pickles. Purpura prefers to call this phase of Harry's career the Great Pickle Period.

During the Great Pickle Period the number of researchers at a given time ranged from fifteen to twenty-five. Keeping the group supplied with adequate electrophysiological equipment was a demanding job.[1] To this end, Harry enlarged his medical electronics facility, which was not located in the Kramer Building, but occupied space in the recently built Black Building, a part of the medical school complex. This space housed one of the best medical electronic laboratories in neurophysiology. George Katz and Ernie Amatniek, electronic engineers, were household names in the field. Amatniek's tenure ended shortly after the Kramer period began and Katz headed the lab throughout the remainder of Harry's career.

While fulfilling all the demands for equipment design and construction, Katz, under Harry's advisorship, completed his requirement for his Ph.D. degree in physiology (1964). With part of his time now devoted to biological research, Katz had his responsibilities in the electronic area alleviated by the addition of two more engineers, Sidney Steinberg and Daniel Benamy, as well as three associates.

Harry's input in the development of electrophysiological equipment was significant as one might gather by noting his name on the membership list of the American Institute of Electrical Engineers and the Institute of Radio Engineers, as well as publications dealing with the design of D.C. amplifiers. Another demand placed on the engineering divi sion that required a fair bit of ingenuity was the design of twenty to thirty research setups to be highly portable.

Every year towards the end of May, two large moving vans appeared in front of the Black and Kramer buildings to move the research equipment to the Marine Biological Laboratories in Woods Hole. The yearly pilgrimage to Woods Hole was considered by Harry to be a major part of his training program. Neurophysiology was at its pinnacle in the 1960s and electrophysiological research at the Marine Biological Laboratories was highly competitive and carried out at a fever pitch.

The experience for the trainees was heightened by Harry's seminar series held on Monday nights. The tempo of these meetings was on the same level as that of the research and these meetings became known as the Monday Night Fights. The moderator was referred to affectionately by some as Papa Bear, a title that I believe was bestowed upon him by Ellen Grass, president of the neurophysiology Grass fellowship program at the laboratories. The list of speakers for the Monday Night Fights included some of the finest scientists in the field. A few names that will be familiar to most include A. Hodgkin, A. Huxley, I. Tasaki, K. C. Cole, S. Kuffler, and H. Huxley. The latter four speakers were regular summer residents and trainees were able to interact with them on a frequent and informal basis.

Research in the early part of the Kramer Building period, as it was for all periods in Harry's career, made use of numerous types of excitable cells from a wide variety of animals and tissues. Marine fish, primarily but not exclusively electric, provided a source of electroplaques as well as secretory, sensory, and neuronal cells. Axonal studies used squid, crayfish, and lobsters. Insects, frogs, lobsters, crayfish, and fish provided neuromuscular and muscle preparations.

The problems under investigation were as diverse as the preparations described above and for this reason it is difficult to designate areas of research with specific years in this period of Harry's career. There was, however, an emphasis on structure-function studies between 1960 and 1970. The roots for the latter began to grow rapidly after Michael V. L. Bennett joined the laboratory of neurophysiology in 1957. A few years after Harry introduced Bennett to electric fish and electroplaques (*Scientific American,* October 1960), the tempo of research on these preparations reached a peak. Interest in the field was also heightened by the publication of numerous papers dealing with the electrophysiological and structural properties of electroplaques by Harry, Mike, and colleagues.

An outgrowth of this work, in which Mike Bennett was also the primary investigator, was the study on the properties of cell-cell junctions. Both electrotonic and chemical synapses were investigated using electrophysiological and morphological approaches. The latter involved extensive electron microscopic investigations of both types of junctions, mainly in preparations from fish. This aspect of the work was carried out by George Pappas, a member of the Department of Anatomy and a longtime collaborator. These studies identified and classified cell-cell junctions, determined the mode of signalling between the cells, and emphasized the physiological significance of electrotonic junctions.

Structure-function studies on single muscle fibers also began during this period. The focus of these investigations was on the role played by transverse tubules, diads, and triads in the excitation-contraction-coupling (ECC) process. This work was initiated shortly after Lucien Girardier came to work[2] with Harry. Collaboration with Philip W. Brandt (then an assistant professor and currently a full professor in the Department of Anatomy) provided the essential electron microscopic skills. This, however, was only one of Brandt's skills.

Brandt's other skills made him a primary contributor to the numerous research projects on muscle in the subsequent years. Initiation of this work represented a rekindling of Harry's earlier interest in muscle[3] and it laid the foundation for the final phase of his career in which muscle physiology was dominant (1970-83). However, to reiterate, with Harry's broad interests and knowledge, a single theme cannot be assigned to any segment of his career. To further emphasize this point, the titles of three papers that he authored and that were published in 1975 are listed: “History of the Synapse as a Morphological and Functional Structure,” “Physiology of Electrogenic Excitable Membranes,” and “The Role for Elementary Properties of Sensory Receptors in Transduction and Coding Information.”

A finding that strongly supported Harry's view that cell membranes are a mosaic of ion permeabilities was the observed change in light scattering by single muscle fibers subjected to conditions that cause an outward movement of chloride ions. The change in light scattering was due to localized and osmotically induced swellings (accumulation of salt within a restricted volume) along specific portions of the transverse tubular system (diads). Since the degree of swelling and light scattering was found to be a function of the magnitude of chloride efflux or the intensity of an inward current applied through an intracellular electrode (filled with a chloride salt), the tubular membranes forming the diads with the cisterna of the sarcoplasmic reticulum must contain a predominance of chloride channels. This was the first demonstration of the heterogeneity of ion channel distribution in cell membranes that merely required a cell and the use of a microscope. This finding gave rise to the chan neled current proposal for the ECC processes. That is, the flow of current through the anion-permselective membranes at the diadic junctions will cause local accumulation and depletion of given ions within the gap-junction volume and the ionic change serves as a signal for Ca release from the sarcoplasmic reticulum. This study led to the investigation of the steps following the release of Ca that are involved in the regulation of the interaction between actin and myosin.

A systematic study of the role played by Mg, ATP, Ca, and MgATP in regulation of contraction in skinned muscle fibers ensued. This work showed that force developed in the absence of Ca as MgATP was increased from zero (rigor state) to a maximum at ~3.0 μM (50 percent of the force-generating capability of the fiber) and then declined to a minimal level (~10 μM). These data led to an empirical formulation describing force generation in which the contractile proteins could exist in one of three states: rigor (zero MgATP), force generating (single bound MgATP), and inhibited (two bound MgATP). By assuming that Ca can only interact with the rigor and single bound MgATP state, the antagonistic effects of Ca and MgATP over a wide range of concentrations (encompassing the physiological levels) could be accurately predicted. Since a quantitative model of force generation has not been developed, this treatment is the only available means for predicting force as a function of MgATP and Ca concentrations.

I cannot end this brief description of Harry's scientific contributions without commenting about his teaching and guidance of those who were fortunate enough to have worked with him. In all the letters written by those who worked with him for his *festschrift* in 1972 and his memorial in 1983 there was a common sentiment expressed regarding the time spent with him. This was most elegantly stated in a letter to me from Dominick Purpura, who at the time was unable to leave Stanford University to attend the memorial at Columbia University's College of Physicians and Surgeons:

To paraphrase Oster, there are those present who will feel, and no exaggeration when I say, that to have know Harry Grundfest was, in the deepest and truest sense of the phrase, a liberal education. Whatever way my days decline, I felt and feel, tho' left alone, his being working in mine own: The footsteps of his life in mine. . . . Tho' much is taken, much abides.

HONORS AND AWARDS

Besides his membership in the National Academy of Sciences, Grundfest was an elected member of the Physiological Society of London and the Japanese Physiological Society. He was an honorary member of the Czechoslovak Medical Society.

He was awarded the degree of doctor honoris causa by the University of Geneva in Switzerland and he received the Claude Bernard Medal of the Sorbonne, as well as the Physicians and Surgeons Distinguished Service Award from Columbia University.

Of particular note was the award he received from the Japanese government. The Order of the Rising Sun is the highest award given to foreigners and seldom is bestowed on U.S. scientists. His recognition by the Japanese physiologists is well evidenced by the more than twenty postdoctoral fellows who traveled to this country to work with him.

NOTES

1. Readers unfamiliar with this period in electrophysiology would not be aware of the sparsity of commercially available equipment that was adequate for the needs of the researchers.

2. In the collection of letters for Harry's *festschrift* Lucien Girardier recalled how he soon learned after joining the laboratory that young scientists came to work with him rather than under him.

3. H. Grundfest. Summation of two subliminal stimuli in single muscle fibers. *Am. J. Physiol.* 105:42.

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LOUIS GEORGE HENYEY

February 3, 1910–February 18, 1970

BY PETER H. BODENHEIMER

OUIS HENYEY, ONE OF THE eminent members of the faculty of the Department of Astronomy at the University of California, Berkeley, during the period 1947-70, is best known for his pioneering research in the field of stellar structure and evolution. The numerical technique he developed for the solution of the equations of stellar structure, known worldwide as the Henyey method, resulted in breakthroughs in research and has since become the standard tool in the field.

His interests included a variety of other problems in theoretical astrophysics and ranged from diffuse interstellar matter to radiative transfer to nuclear physics to cosmological models. He had practical interests in astronomical spectroscopy, optical design, and electronic computing. He played an important part in the education of graduate students and, as a teacher, he was characterized by exacting standards as well as warm relationships with his students. Major administrative posts included service as chairman of the Berkeley Astronomy Department, director of the Leuschner Observatory, director of the Berkeley computer center, and president of the Astronomical Society of the Pacific. He was elected to the National Academy of Sciences in 1968.

Louis Henyey was born in McKees Rocks, Pennsylvania, on February 3, 1910. His parents, Albert and Mary Henyey, were immigrants from Hungary. He attended West High School in Cleveland, Ohio, where he received his diploma in June 1927. He then studied at the Case School of Applied Science in Cleveland, where he obtained his B.S. degree in 1932 and his M.S. degree in 1933. During his Case years he collaborated in research projects with his mentor J. J. Nassau. Henyey married Elizabeth Rose Belak, born in Budapest, on April 28, 1934; they had three children: Thomas Louis, Francis Stephen, and Elizabeth Maryrose.

He spent the years 1934-37 as a graduate student at Yerkes Observatory of the University of Chicago, where he served as an assistant astrophysicist and earned his doctorate in 1937, with a mathematical thesis on the topic of reflection nebulae. In 1937 he was appointed instructor at Yerkes. During the year 1940-41 he took a leave of absence as a Guggenheim fellow to study under Hans Bethe at Columbia University. There he concentrated on the application of quantum mechanics to astrophysical problems. In 1942 he was appointed assistant professor at Yerkes, a position he held until 1947.

During much of this time at Yerkes Observatory he collaborated in astronomical research with Jesse Greenstein. From 1943 to 1945 he also worked under a contract to the Office of Scientific Research and Development under the supervision of the National Defense Research Council. As technical representative he was in charge of the work at Yerkes under this contract, which involved the design and construction of optical instruments of military value.

In 1947 he accepted a position as assistant professor in the Department of Astronomy at the University of California, Berkeley. He was promoted to associate professor the following year and to professor in 1954. At Berkeley he became head of his own research group in the field of stellar evolution and he supervised and collaborated with numerous graduate students, postdoctoral fellows, and scientific visitors. He died unexpectedly of a cerebral hemorrhage on February 18, 1970.

During his years at Yerkes Henyey worked primarily in the area of the physics of the diffuse gas in interstellar space, considering problems, for example, of the reflection of starlight from clouds of gas in space, the absorption and reddening effects of such clouds on the light from background stars, and the theory of spectral line formation in gaseous nebulae. He developed considerable expertise in the field of optics. He and J. L. Greenstein, in collaboration with director Otto Struve, invented a wide-field camera suitable for studying the diffuse gas in space, which they used as a basis for several papers in a collaboration that was to last several years. As Greenstein[1] remarks, “His perfectionism blended with my somewhat coarser energy into confidence that we could finish everything we tried.” The combination of observational and theoretical work involved both elaborate analytic radiative transfer calculations as well as observations of the spectra of emission and reflection nebulae, for example, those of the North America nebula and the γ Cygni nebula.

During the war years, however, most of the effort of those staff members who remained at Yerkes (including Henyey and Greenstein) went into design of lenses and optical systems for military application under the Office of Scientific Research and Development. An optical shop was established there so that the optical devices designed by the group could be tested. The designs involved viewing and fire control optics and fast wide-angle cameras. Henyey developed original designs and became an expert in complicated lens and mirror systems and their aberrations. As Greenstein describes,[1] much laborious numerical calculation with the mechanical calculators of the time was required. The optical group at Yerkes was regarded as one of the most skillful and ingenious in the United States at the time. After the war Henyey remained in charge of the optical shop at Yerkes.

Sturla Einarsson, chair of the Berkeley Astronomy Department in 1947, considered it necessary to rebuild the department and to develop expertise in modern astrophysics.[2,3] Henyey was the first in a series of new appointments, taking the position left open by the resignation of C. D. Shane, who became director of Lick Observatory. He joined faculty members Cunningham, Einarsson, Meyer, and Trumpler in that department. He was followed in the next few years by Otto Struve, John G. Phillips, and Harold F. Weaver.

After arrival at Berkeley, Henyey became interested in the area of stellar structure and evolution. This field, one of the cornerstones of modern astrophysics, was restricted at the time by the lack of adequate computational facilities. The calculation of the evolution of a star, including all the physical effects required for meaningful comparison with observation, is simply not possible to do by analytical methods. Henyey spent the year 1951-52 on leave of absence at Princeton University, where he was involved in classified defense work. There he had contacts with J. von Neumann and learned about general numerical methods for solving nonlinear problems. He also developed links with the Livermore Radiation Laboratory, which had probably the world's most powerful computational facility during the 1950s. There, in collaboration with scientists at the laboratory, he was able to develop specific numerical methods for stellar evolution and to publish several papers on the subject.

He was active in the effort to provide the Berkeley campus with a powerful computational facility, spending a considerable amount of time as chair of the operations sub committee of the computer center. In 1958 he became the first director of the Berkeley computer center, and although he resigned that post in the following year to become chair of the Berkeley Astronomy Department, he continued to serve on the center's advisory committee. During the next several years when development of larger and faster computers was very rapid the center was able to upgrade its equipment to keep abreast of the best university centers in the country.

Louis Henyey is most remembered for two major scientific contributions. First, he developed a method for automatic solution of the equations of stellar evolution, suitable for electronic computers and applicable to a wide range of physical conditions and phases in the lifetime of a star. Second, he made a significant new calculation of the evolution of stars during their early history when gravitational contraction provides the main energy source, and during the transition phase when nuclear energy takes over from the gravitational source. The method, described succinctly in one line by Henyey,[4] is “an iterative procedure which is essentially a multidimensional generalization of Newton's method for finding the root of a function.” For the calculation of the structure of a star at a given point in time, four differential equations are generally solved for four unknown functions, for which two boundary conditions are specified at the center and two at the surface.

Until about 1960 most investigators in the field used the “fitting” method to solve the equations. This procedure involves repeated inward and outward trial integrations starting from both boundaries and adjustment of the boundary parameters until the solutions match at some interior fitting point. The Henyey method involves dividing the (spherical) star into a set of Lagrangian zones, expressing the differential equations as difference equations on the finite grid, setting up an initial guess for all values of the unknown variables at all of the zones, expanding the difference equations in terms of first-order corrections to all the variables, and solving, by matrix inversion, for all the corrections simultaneously. The result is a second approximation to the actual solution. The process is repeated until it converges. If there are N zones in the star, and 4 dependent variables whose variations through the star are to be solved for, the method essentially involves solving 4N linear equations for the 4N unknown corrections to the basic variables. Once the model has converged, the time is advanced and the next model in the evolutionary sequence is computed using the previous model as first guess. The advantage of the method is that it is more efficient and has better convergence properties than the earlier methods. It provides solutions, for example, during advanced phases of stellar evolution where the earlier methods fail. It is flexible in the sense that the choice of physical approximations can be as simple or as complex as the investigator desires.

The basic principle of the method was published in *The Astrophysical Journal* in 1959, but the terse style of that paper made it difficult to understand. Apparently the power of the method was not appreciated by the international community of researchers in the field until Henyey, at the request of Professor Schwarzschild, then president of Commission 35 (stellar constitution) of the International Astronomical Union, gave a lucid presentation of it before a meeting of the commission during the general assembly of the IAU held in Berkeley in the summer of 1961.[5] Several groups around the world then began to develop their own versions of the method. The result was an outpouring of new results during the decade of the 1960s, a significant amount of progress in finding agreement between theoretical calculations and observational phenomena, extensions of the theory into problems that had not even been considered before, and, in general, a substantial advancement of the field of stellar evolution.

The major paper on calculation of stellar evolution during the gravitational contraction phase[4] appeared in *Publications of the Astronomical Society of the Pacific* in 1955. It established Henyey as a leader in the area of numerical solutions of systems of nonlinear differential equations. It was significant not only because it was the first application of the new method but because it was able to go beyond earlier restrictive assumptions and produce more accurate solutions during this phase than had been possible previously. This paper recently has been recognized as a significant contribution. As part of the centennial volume of the *Publications of the Astronomical Society of the Pacific* it was reprinted[6] and accompanied by a review article[7] on the subject of the evolution of young stars by Steven Stahler of the Massachusetts Institute of Technology.

By 1955 it had been well established that the principal energy source of the stars during most of their lifetime was the nuclear fusion of hydrogen into helium. The evolutionary phase during which a major portion of this conversion takes place is known as the main-sequence phase. At the time, observational evidence was beginning to become available regarding the earlier pre-main-sequence phase, during which the stars are not hot enough in their interiors to allow nuclear reactions to generate significant energy. Today it is recognized that stars form in the cores of clouds in interstellar space, where the typical density of matter is twenty orders of magnitude lower than that in stars. Once gravitational instability sets in, the protostar collapses in near freefall until internal temperatures and pressures become high enough so that the collapse is stopped and the star approaches hydrostatic equilibrium. At this point maximum temperatures fall around 1-2 million K, far too low to result in nuclear reactions. Therefore, the star contracts slowly, radiating and heating at the expense of its gravitational energy, until the central temperature reaches about 10 million degrees, at which point the onset of nuclear burning occurs. The calculation of Henyey, LeLevier, and Levée (1955) addresses the latter part of this phase of gravitational contraction in quasi-hydrostatic equilibrium, and, in particular, the transition to the main sequence, during which nuclear burning gradually replaces the gravitational energy source. Their calculation was the first to treat this transition properly.

The calculations were performed on the UNIVAC computer at the Livermore Radiation Laboratory. The full timedependent equations of stellar structure were solved, including hydrogen-burning nuclear reactions, approximate radiative opacities, and provision for a central convection zone that would be induced by highly temperature-sensitive nuclear reactions. However, a possible surface convection zone was not allowed for, and the models remained almost completely radiative. The results showed that as a star contracts and heats in the interior, the luminosity gradually increases and the surface temperature increases until nuclear reactions become important. As the nuclear energy source gradually replaces the gravitational energy source the track changes direction and the star gradually declines in luminosity and decreases slightly in surface temperature before settling onto the main sequence. Henyey's calculations included several different masses ranging from 0.65 to 2.3 times the mass of the Sun. The calculation indicated that the lifetime of a star of a solar mass in the contraction phase was about 3×10^7 years.

The surface temperatures of the model stars at the beginning of the calculation fell in the range 2000-4000 K.

Henyey was aware that in the atmospheres of stars cooler than about 6000 K the ionization of hydrogen generates a surface convection zone. In the same year that Henyey's paper was published F. Hoyle and M. Schwarzschild[8] published an important paper, which traced the evolution of a star beyond the main-sequence phase to the red giant phase and showed that provision for a surface convective layer was crucial. With it the models made a transition from evolution at almost constant luminosity on the way over to the giant branch to rapidly increasing luminosity and slowly varying surface temperature in the giant branch itself; these theoretical models agreed with observations. It was demonstrated in 1961 by Professor Hayashi of Kyoto University[9] that stars, in their evolution prior to the main sequence, exhibit roughly the inverse behavior. When contraction starts at a radius several times the main-sequence value, convection dominates the structure so that the evolution proceeds at nearly constant effective temperature and decreasing luminosity. As the star contracts and heats, a radiative zone develops near the center and gradually includes more and more mass. The star then makes a transition to an evolutionary path of gradually increasing luminosity and increasing temperature, which is what Henyey calculated. However, he missed the earlier convective portion of the tracks because of the surface boundary condition he used. He has often been criticized for this omission. In fact, his tracks are valid for the final approach to the main sequence as long as the mass is greater than about 0.6 solar masses, but not for the earlier phase of the evolution when the surface temperature is less than about 4000 K. The convective boundary condition was not included in the 1955 version of the code, first, because there were numerical problems at the surface, and second, because he was not satisfied with the very approximate nature of the theory of convection in stellar surface layers.

For several years Henyey distrusted Hayashi's result, which was originally based on a number of approximations and, for example, did not include possible superadiabatic effects in surface convection zones. He wished to see a confirmation based on a full numerical solution. Before Hayashi made his discovery of the nature of pre-main-sequence evolution, Henyey had been in the process of developing an advanced stellar evolution computer code that took into account a wide variety of physical processes, including a detailed “mixing length” theory of the surface convection zones on cool stars. He invited Karl-Heinz Böhm, an expert on astrophysical hydrodynamics, and his wife Erika BöhmVitense, an expert on the theory of convection in stellar atmospheres, to spend some time at Berkeley to work out the atmosphere problem. Mahendra Vardya, who spent several years at Berkeley, developed a detailed atmosphere computer code to join with the Henyey stellar evolution program. However, it turned out to be quite difficult to get the combined code into full operation, because of its great complexity, the use of assembly language, and the limited power and storage of computers at the time. Other investigators were already beginning to learn about the Henyey method and, using somewhat simpler versions than Henyey's own, were getting results. Some of them were able to prove that Hayashi was right. The superadiabatic effects, while present, were not sufficiently important to vitiate the result.

My role as a graduate student in Henyey's research group was to investigate the Hayashi problem and to determine whether the result was sensitive to initial conditions. For example, if the assumed initial model was largely radiative rather than convective, would Hayashi's tracks still result? Of course, the actual initial condition is determined by events during the preceding protostar collapse phase, which at that time (1964) was just beginning to be studied.

My calculations started with an artificially chosen entropy distribution that produced a radiative region in most of the model. Nevertheless, on a very short time scale the surface regions of the model relaxed to conditions very close to those predicted by Hayashi. Although the interior radiative zone persisted for some time, its presence did not appreciably affect the evolution of the surface temperature and luminosity, which proceeded according to Hayashi's theory. By this time there was also convincing observational evidence supporting Hayashi's evolutionary tracks. For example, essentially no stars have surface temperatures below 2500 K, in agreement with the theory. Also, the agreement between observed main-sequence lithium abundances as a function of surface temperature and my calculation of lithium depletion during the pre-main-sequence contraction was a strong point in favor of the existence of deep convection zones in the early phases.

Henyey insisted on as much accuracy as possible in describing the physical processes in the stellar interior, and he was always pushing the available computing power to the limit. In the early 1960s his computer program, nicknamed STEVE by his research group, had a degree of sophistication well beyond that of most codes at the time. Approximations were, of course, necessary, but the code followed, nevertheless, with non-equilibrium reaction rates, the time dependence of several nuclear species involved in hydrogen and helium burning. It also had a separate model atmosphere calculation that was fitted to the interior solution, and it had a detailed equation of state that was correct over a wide range of physical conditions. Some of the refinements were probably unnecessary in view of the inher ent uncertainties in both theory and observations, and the various complications resulted in some delays in progress.

Although he was recognized as a world leader in the field of stellar evolution, he was at times criticized for failing to publish voluminously. In part, this reticence followed out of his high standards and self-critical attitude. Nevertheless, the fundamental importance of his work was internationally recognized.

He did publish three papers in collaboration with members of his research group in 1964-65. The first of these described the new version of the mathematical method, discussed the physics that went into the calculations and presented a flow chart of the overall computer program. The second paper presented several calculations of the late pre-main-sequence and main-sequence evolution of a star of 2.3 solar masses, with different assumed initial abundances of the elements carbon, nitrogen, and oxygen and a comparison of the results with the present observed properties of the star Sirius. The third paper considered the physical and mathematical approach to the calculation of the model atmosphere boundary condition that was combined with the stellar evolution program, identified several uncertain parameters in convection theory, and tested the influence of those parameters in the pre-main-sequence and post-main-sequence evolutionary phases of a star of five solar masses. These results helped to establish that the Hayashi phenomenon was insensitive to uncertainties in the treatment of stellar surface layers.

The stellar evolution project produced a number of further research papers during the late 1960s, but he did not include his name as co-author on many of them. During that time he concentrated on studies in the rigorous theory of radiative transfer in stellar atmospheres and on the diffusion of energy in the stellar interior and its relation to the larger question of why stars develop a red-giant structure.

Henyey was very generous with the time, assistance, and recognition he gave to his graduate students. During the mid-to-late 1960s he had more students than any other faculty member in the department, and his relations with them were unusually good. He treated them with respect and as intellectual colleagues and encouraged them (also with financial support) to attend scientific meetings to present their results. On several occasions he invited groups of them to his home, which was beautifully located in the hills overlooking San Francisco Bay and was surrounded by a very fine garden to which he devoted much attention. The meetings were sometimes social affairs, but at other times they involved discussions of particularly difficult problems with the stellar evolution code.

Several papers were published during this period by his students under their own names even though he himself played a major role in the development and testing of the numerical techniques and physical input that led to those results. For example, I published three papers[10,11,12] under my own name in 1965 and 1966. The results were based on a version of the Henyey code and they concerned the depletion of lithium, deuterium, and beryllium during pre-main-sequence evolution, as well as the effect of initial conditions on the theoretical tracks during the early pre-main-sequence phase. In 1968 Jack Forbes, who worked long and hard on the development of the Henyey code, published his thesis results[13] on the effect of mass loss during post-main-sequence stellar evolution.

Robert Benson[14] used the code to study the evolution of close binary star systems in which mass is transferred from one component to the other. He was the first to show that the star that accepts mass evolves out of thermal equilib rium, expands rapidly, and soon comes into contact with its companion, a crucial realization in the theory of close binary evolution.

Silvia Torres-Peimbert[15,16] studied the effects of enhanced (above solar) metal abundance on the evolution of stars and discussed the effects of metal abundance on cluster ages, which are found by fitting observed Hertzsprung-Russell diagrams to the results of stellar evolution calculations.

Erik Simpson[17] completed his thesis on the effects of semiconvection on the evolution of massive stars. He discussed which version of the theory was most likely to be correct through a comparison with observed cluster stars.

Three students jointly published a paper[18] in which they used the Berkeley stellar evolution program to produce solar models and to compare the theoretical neutrino fluxes observable at the Earth to the early experimental results. Another student project involved a comparison of Berkeley stellar evolution isochrones with the Hertzsprung-Russell diagrams of several galactic clusters to determine their ages.[19] The age of the Pleiades found in this investigation was generally accepted by observers and it has not been substantially modified in more recent determinations.

Other students of Henyey performed thesis research on projects that were closely related to stellar evolution but did not involve direct use of the computer program. Roger Ulrich[20] developed a theory of convection that was an alternative to the mixing-length approach. It examined the coupling, through mass motions, between adjacent layers with different properties in the superadiabatic region of an atmosphere.

Richard Hillendahl[21] developed his own hydrodynamic code with radiation transport to describe the oscillations of Cepheid variables and the generation of extended envelopes and mass loss in luminous stars. William G. Mathews[22] did a pioneering study on the numerical treatment of expanding regions of ionized hydrogen associated with hot, young, and massive stars. William B. Hubbard[23] solved difficult problems in transport theory in dense plasmas; this work later became of fundamental importance in the development of detailed tables of thermal conductivity for stellar matter under electron-degenerate conditions.[24] These tables are still the standard source for electron-conduction opacities in stellar evolution calculations. All in all, the Henyey research group produced very respectable scientific results during the period 1965-70.

Those Berkeley graduate students who did not choose to do research in areas related to stellar evolution also benefitted from Henyey's expertise and high standards. For over twenty years he was the mainstay in the graduate teaching program in theoretical astrophysics, giving the basic courses in stellar structure and evolution, fundamental principles of physics, and occasionally stellar atmospheres. He earned his unofficial title of “the tiger” with his generally rigorous standards of instruction and by being “fierce” on, for example, qualifying exams. Nevertheless, he was often regarded by the students as their best friend on the faculty. His lecture courses were very well organized. He was extremely clear and at times brilliant. He provided an excellent set of lecture notes for his students. And he covered both the fundamental physical principles of a subject as well as the latest results at the frontiers of research. The courses were generally aimed at the level of the best students.

Henyey also devoted considerable energy to departmental and university affairs. When Otto Struve took a leave of absence in 1959 to become director of the National Radio Astronomy Observatory, Henyey took over his position as chair of the Astronomy Department and director of the Leuschner Observatory for the next five years. He took on the chairmanship during a time of rapid, nationwide expansion of the field of astronomy. Several new faculty members and research associates joined the department during this time, and the number of graduate students also increased substantially.

One particular area of research that Henyey strongly supported was radio astronomy. A radio astronomy laboratory had been established on the Berkeley campus during the late 1950s and an observing site had been set up at Hat Creek in northern California. He served on the advisory committee of the laboratory from 1961 to 1965. About the time he took on the chairmanship, the department moved from the old Leuschner Observatory buildings near the north gate of the campus to new, modern quarters on the top floor of Campbell Hall. Henyey was the departmental representative on the Campbell Hall subcommittee, which had an important role in planning and designing the new building. He was also heavily involved in moving the Leuschner Observatory itself to a new, more suitable, but still close-by site in the Berkeley hills. He was instrumental in locating the new site, obtaining funding from the National Science Foundation, and in designing the new 30-inch reflecting telescope that was put into operation there. He generally took on more than his share of the local administrative responsibilities, and he discharged his duties thoroughly, carefully, and with meticulous attention to detail. His judgment and opinions were highly valued among his colleagues.

In summary, Professor Louis Henyey was an excellent scientist who was interested less in building his personal scientific reputation than in service to others, and more interested in scientific rigor than rapid, superficial publication. He had a wide range of interests in astrophysical subjects ranging from highly theoretical to very practical; in par ticular, he played a key early role in the development of electronic computing as a technical aid in astronomy. His major accomplishment was the development of an automatic computational method for the calculation of stellar evolution. As a result, there was rapid progress in this field, which brought it onto a quantitative level and into a state where useful comparisons with many different types of observations could be made.

I THANK THE BERKELEY Astronomy Department and, in particular, Ms. Mary Brunn, for providing the photograph and other historical information from the faculty files. Although this article is based primarily on my personal experience as a graduate student in the Berkeley department from 1960 to 1965, I have also benefitted from Professor Greenstein's vivid review,[1] from comments by Professor Martin Schwarzschild and from an article by Berkeley professors John G. Phillips, Ivan R. King, and L. V. Kuhi that appeared in the University of California volume *In Memoriam* in May 1977.

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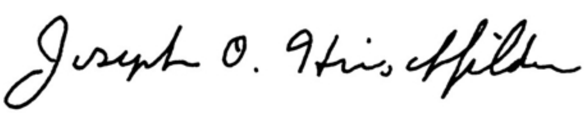

JOSEPH OAKLAND HIRSCHFELDER

May 27, 1911–March 30, 1990

BY R. BYRON BIRD, CHARLES F. CURTISS, AND PHILLIP R. CERTAIN

OSEPH OAKLAND HIRSCHFELDER WAS one of the leading figures in theoretical chemistry during the period 1935-90. His sustained research program not only spanned five and one-half decades but a wide number of scientific areas as well: chemical kinetics, chemical applications of quantum mechanics, combustion, nuclear explosions, kinetic theory of gases, intermolecular forces, structure of liquids, and laser chemistry. He was elected to the National Academy of Sciences at the relatively early age of forty-two and he was chosen to be a fellow of the American Academy of Arts and Sciences at age forty-eight. At age sixty-five he received the National Medal of Science from President Gerald Ford "for his fundamental contributions to atomic and molecular quantum mechanics, the theory of the rates of chemical reactions, and the structure and properties of gases and liquids." Despite his exalted standing in the field of chemical physics, he was a very approachable and gregarious individual. He always insisted on being called "Joe," and he was always thus addressed by colleagues, students, secretaries, and janitors. It would seem unnatural for us to refer to him in any other way, even in this rather formal summary of his illustrious career.

The account below was prepared by three of his former graduate students and colleagues, based to a large extent on personal recollections. Additional information can be obtained from published sources[1, 2,3,4 and 5] as well as from articles that contain a certain amount of autobiographical material.[6,7,8,9 and 10]

Joe was born in Baltimore, Maryland, on May 27, 1911, the son of Arthur Douglas and May Rosalie (Straus) Hirschfelder. Of his family and early childhood, Joe has written:[7]

My paternal great grandparents emigrated from Germany to California in 1843. . . . Both my grandfather and father devoted their lives to medical research. Grandpa was the first child born in Oakland; he graduated in the first class at the University of California and became the first Professor of Clinical Medicine at Stanford. Dad entered the University of California at the age of 13. After receiving his MD, he joined the medical faculty of Johns Hopkins University, where he was the first doctor in the United States to use an electrocardiogram. . . . Later he became very much interested in the colloid chemistry associated with the physiological effects of drugs and accepted a Professorship in Pharmacology at the University of Minnesota.

Thus, I was born in Baltimore and grew up in Minneapolis. When I was five years old, Dad built a chemistry lab for me in the basement of our home. When I was ten, he took me to an American Chemical Society Meeting in Los Angeles. And when I was 15, I helped Dad determine the distribution of colloidal particles in a Zsigmundy ultramicroscope—my contribution was to suggest a correction factor for the convection currents produced by passing street cars.

Joe was an undergraduate at the University of Minnesota from 1927 to 1929 and at Yale from 1929 to 1931. During this period he found he was not particularly suited to experimental sciences and decided to do theoretical work. He was attracted to Princeton since it was possible to take a double Ph.D. in theoretical physics and chemistry. His chief physics mentor was Eugene P. Wigner, and his chemistry supervisors were Henry Eyring[6] and Hugh S. Taylor.[7] After receiving his Ph.D. in 1936, he spent an additional year as a postdoctoral fellow with John von Neumann at the Institute for Advanced Study, during which time he also continued his work with Eyring and Taylor. Joe found Princeton a most exciting place, and he was a dedicated and diligent student; he worked on a variety of research problems including the polarizability of the hydrogen molecule and hydrogen-molecule ion (suggested to him by E. U. Condon); the separation of rotational coordinates from the N-particle Schrödinger equation (with Wigner); the energy of the H_3 molecule and the H_3 molecule ion (with Eyring and Rosen); the a priori calculation of the reaction rate between atomic and molecular hydrogen (with Eyring); a free-volume theory of liquids (with Eyring); and some applications of the virial theorem to the scaling of wave functions. Of the latter work Joe had this to say:[8]

My discovery of the hypervirial theorem is curious: In 1932 when I was a graduate student, I doodled with derivatives of the Schrödinger equation and obtained a variety of seemingly useless relations which I carefully saved in my files. Then 28 years later, when I had to give a paper at a symposium in honor of Jack Kirkwood, I studied these doodles and found that I had discovered a generalization of the virial theorem!

In 1937 he went to the University of Wisconsin as a Wisconsin Alumni Research Foundation research associate; in 1940 he became an instructor in chemistry and physics, and in 1941 he was named an assistant professor in the Chemistry Department. During the five-year period between his arrival in Madison and his departure for activities related to the war, he continued his research on the applications of quantum mechanics to intermolecular forces and chemical kinetics, and he started a program devoted to intermolecular forces and properties of gases. It was during this period that he published his first paper with C. F. (Chuck)

Curtiss; Chuck did his senior thesis with Joe, and this was the beginning of many years of fruitful collaboration.

In 1942 Joe's academic career was interrupted by military research. For about two years he was with the National Defense Research Committee (NDRC) in Washington, D.C., where he worked as head of the Interior Ballistics Group on a wide variety of problems, such as the thermodynamics of propellent gases and the fluid dynamics and combustion in the barrels of guns, mortars, and rockets. In 1944-45 he was a group leader at the Los Alamos scientific laboratory. In 1945-46 he was head of theoretical physics at the Naval Ordinance Test Station at Inyokern, California, and in 1946 he served as the chief phenomenologist at the Bikini atomic bomb tests. These wartime experiences exposed Joe to a myriad of practical problems, which forced him to become a theoretician with a strong interest in experimental facts and phenomena. Thereby he was able to impart to his graduate students a genuine concern for being able to interpret theoretical results in a form useful to experimentalists and engineers.

During the atomic bomb project Joe worked with Hans Bethe and John Magee on the dynamics of nuclear explosions, including the formation of the fireball and the shock wave. He also studied range-energy relations for the penetration of high-energy protons, multiple Klein-Nishina scattering, and the prediction of fallout from nuclear blasts. During the next few years he chaired a five-member board of editors in preparing the 456-page book *The Effects of Atomic Weapons* (1950).

In 1946 he returned to Madison to become a full professor in the Department of Chemistry, a post he was to fill until his retirement. He started out by establishing the University of Wisconsin Naval Research Laboratory; he served as the director until 1959, when it was reorganized as the

University of Wisconsin Theoretical Chemistry Institute. He arranged to have a separate one-story, cinder-block building constructed on the UW campus several blocks west of the chemistry building. By 1950 he had between eight and ten students doing theoretical work, a somewhat smaller number doing experimental work, and five or six "computresses" (a group of young women with mathematics degrees, whom he had trained to be extremely responsible and accurate in making numerical computations using electromechanical desk calculators, tables of tabulated functions, graph paper, and French curves). No other professor in the Department of Chemistry had such a large entourage. Joe was among the first of the "entrepreneurial professors" on the campus; of course, financing such a large group and maintaining the building and equipment demanded of Joe enormous amounts of time and energy. In those days Joe's energies seemed unbounded as he supervised (very closely) his graduate students, organized meetings, attended committee meetings in Washington, presented papers at scholarly societies, and consulted for private industry and government agencies. Despite this whirlwind of activities he still had time to organize picnics, play tennis, and be very friendly and hospitable to students and colleagues. He seemed to be living life at about one and one-half times the speed of normal mortals.

Joe was one of the first theoretical chemists to engage in large-scale numerical computations. We have mentioned his computresses above, who enabled him and his graduate students to attack numerically a number of otherwise intractable problems. Joe was extremely insistent on accuracy in computing, and when he and his students published any kind of tabular results, he wanted to be 100 percent sure that tables of computed values could be trusted. He set very high standards in the field of chemical computations;

he often said that if you turn out one table of numbers that cannot be trusted, no one will trust any of your subsequent work and it will all be regarded as useless. He insisted that all students devise clever ways to check numerical results.

Joe had a very ambitious agenda for his laboratory, including both theoretical studies (equation of state for gases and liquids, transport property calculations, flames, shockwave phenomena, intermolecular forces) and experimental projects (flame velocities, critical phenomena, phase equilibria, interferometry). One of the main activities in the group was the development of theories of flames and combustion, an extremely difficult task involving the solution of the equations of change along with information on transport properties and chemical reaction rates. According to Professor Roger A. Strehlow of the University of Illinois, one publication from that period, “The Theory of Flame Propagation” (1949), had an enormous impact on subsequent flame structure studies. The work on flames also resulted in a key paper on the integration of stiff equations (1952), one of the earliest publications dealing with what is now called singular perturbation theory.

The UW Naval Research Laboratory was an exciting place to be a graduate student because of the tremendous stimulation provided by a very active professor, many other excellent students to interact with, and a constant stream of visitors; it was a wonderful experience to have the chance to meet such illustrious figures as Jack Linnett (from Oxford), Jan de Boer (from Amsterdam), Mel Green (from Princeton), Henry Eyring (from Utah), Ilya Prigogine (from Brussels), Jack Kirkwood (from Cal Tech), Al Matsen (from Texas), and many, many others. Joe's students were most fortunate to mature in such an inspiring environment. They were lucky to have a research adviser who knew personally all the key figures in his areas of interest. Joe's lectures on quantum mechanics and statistical mechanics were peppered with personal references to the people who had made the major contributions to these subjects. Students got a kick out of comments like: “Hans Bethe was telling me just the other day that . . .” or “Joe Mayer showed me a nifty way to derive this . . .” or “Johnny von Neumann suggested that . . .”

Joe was not regarded as a polished classroom lecturer and he did not always prepare his lectures carefully; however, what he lacked in preparation and organization was more than made up for by his buoyant and boyish enthusiasm and his clear perspective of the direction of movement of the field as a whole.

By 1950 he had decided that it was time to summarize some of the work done in his laboratory and combine this information with that from other research centers. In the summer of 1950 he invited several of his former students to collaborate with him in this adventure. The first draft of the book was done in 1950-51, and the second draft was prepared in 1951-52. The reviewing, editing, and proofreading took about a year and a half; Joe had the habit of making extensive changes in the page-proof stage of books and articles, with the result that the proofreading took longer than normal. In the spring of 1954 the treatise “The Molecular Theory of Gases and Liquids” was published. MTGL, as the book was soon nicknamed, appeared in a second corrected printing in 1964, and a Russian translation was published in 1961. In 1974 a list was published in *Current Contents* of the most cited books in physics and chemistry, and MTGL ranked fourth on the list. Forty-one years after its publication the book is still in print.

The MTGL book made it evident that further progress could not be made in the calculation of physical properties until more is known about intermolecular forces. Conse quently, Joe began to concentrate his research efforts in that area. He became interested in the possibilities of making a priori calculations of the forces between molecules. He was a pioneer in this field before the technological development of computing facilities, which has now so greatly expanded the field. He investigated the use of hypervirial theorems, developed ideas on the use of the perturbation and variational theories to obtain upper and lower bounds, and perturbation theory as applied to almost degenerate states.

In the early 1960s, as the National Aeronautics and Space Administration took up President Kennedy's challenge to put a man on the moon, Joe was invited to submit a proposal to convert the UW Naval Research Laboratory into the Theoretical Chemistry Institute (TCI) to investigate the intermolecular forces and chemical dynamics. This allowed for tremendous expansion of both staff and facilities. It also allowed the creation of an experimental program in molecular beam reactive scattering, led by Richard B. Bernstein, closely allied to theoretical research. This was characteristic of Joe's approach to science: an interdisciplinary emphasis with theory tied closely to experiment. It was also during this period that he collaborated with P. O. Löwdin in the development of the concept of natural spin orbitals.

From 1963 to about 1970 TCI grew rapidly, with a large number of graduate students, postdoctoral fellows, and visiting scientists coming to Madison. In many ways the sheer magnitude of the effort firmly established theoretical chemistry as an essential component in all major chemistry departments.

After the Apollo program, funding shifted from NASA to the National Science Foundation. Gradually the support shifted from a block grant for TCI to grants for individual investigators. During the 1970s Joe began a longtime association with the University of California, Santa Barbara, where he interacted with chemists, physicists, and engineers. His research interests turned to the interaction of light with matter, and the nonlinear effects associated with intense lasers. On the occasion of his retirement in 1981, Joe wrote:[8]

Intermolecular forces have been my principal interest for the last 44 years. Thus, after the publication of MTGL, I studied all kinds of intermolecular forces and their relativistic corrections and Born-Oppenheimer derivations. In order to calculate the interaction energies, I worked on variational and perturbation techniques applied to nondegenerate, degenerate, and almost-degenerate problems. However, I found that perturbation theory applied to practical electron exchange problems is a mathematical whirlpool so that I started to go around in circles and got sucked in, even deeper. Thus, I decided to make a big change in my research and study the dynamics of molecules with moving nuclei either in the presence or in the absence of external electromagnetic fields.

Joe said that after his retirement he wanted to continue doing research until he “lost his marbles.”[10] From 1981 on, he kept interacting with colleagues and pursuing scholarly activities, splitting his time between Madison in the summer and Santa Barbara in the winter.

A good example of the vitality of Joe's intellect was his response to receiving radiation treatments for a tumor on his spine. Intrigued by how the radiation could destroy the tumor without damaging his spinal cord, he asked the medical physicists about the equations used to focus the radiation. The equations reminded him of those used in weather satellite tracking, and consequently Joe put the medical physicists in touch with a former postdoctoral associate, Robert Pyzalski, then working in meteorology. Pyzalski was able to be very helpful to the medical physicists, and indeed, meteorology lost him to medical physics, where his research could be devoted to helping mankind.

Joe's contributions were recognized by a number of important awards. He was elected to the National Academy of Sciences in 1953, the American Academy of Arts and Sciences in 1959, the Norwegian Royal Society in 1965, and the Royal Society of Chemistry of Great Britain in 1981. He received the Debye Award of the American Chemical Society in 1966, the Edgerton Gold Medal of the Combustion Institute in 1966, the National Medal of Science in 1976, and the Silver Medal of the American Society of Mechanical Engineers in 1981. He also received honorary degrees from Marquette University (1978) and the University of Southern California (1980).

In 1953 Joe married Elizabeth (Betty) Stafford Sokolnikoff, a much-admired textbook author and mathematics professor on the University of Wisconsin campus. Betty shared with Joe a love of travel and an intense interest in people. The two of them shared the hospitality of their home with hundreds of visitors from all over the United States and abroad. Betty followed closely the activities of Joe's students and their families; with her encyclopedic knowledge of the names and faces of theoretical chemists and physicists from all over the world, she was invaluable. She was his constant and devoted companion, and his exacting proofreader. They were a scintillating and fascinating couple.

Joe's scientific output included more than 250 scientific papers, several edited volumes, various chapters in handbooks, and the MTGL treatise. He directed the Ph.D. theses of thirty-nine students and collaborated with over 100 postdoctoral students and visiting professors. His scientific progeny is currently active in more than fifteen fields of science and engineering—attesting to his own extremely broad-ranging interests. Joe himself recognized no boundaries between scientific fields. If it was science, it was ipso facto interesting and worthy of study. To Joe, science was life itself, and he remained active scientifically until several weeks before his death on March 30, 1990.

Joe's strong feelings on science education are best reflected by his own words:[9]

In industrial and government laboratories, interdisciplinary problems are solved by task forces composed of people having different skills and backgrounds. Frankly, I am very much concerned that the training which we give our students is so highly specialized that they are not prepared to tackle problems that are not closely connected with their theses. It is important that our students develop sufficient breadth that they can explain their ideas to people with different backgrounds. This is essential if they are to become useful members of an interdisciplinary task force.

As just one example of Joe's roving scientific mind, we cite his 1976 publication with Howard and Lightfoot (of the Chemical Engineering Department at the University of Wisconsin) on a hydrodynamic separation technique for optical isomers; this was prompted by his observation of the behavior of sea shells when he was hiking on the beach at Sanibel Island, Florida, during a quantum chemistry conference.[6]

He will long be thought of as one of the founding fathers of the field of theoretical chemistry. He will also be remembered because, when you'd run into him on the street or in the hall, he'd start out by saying, “Gee, am I ever having fun with our new theory of . . . .”

THE AUTHORS THANK Mrs. Elizabeth Hirschfelder for checking the manuscript and making some changes in the text.

NOTES

1. Biographical sketch. In *McGraw-Hill Modern Scientists and Engineers.* New York: McGraw-Hill (1980):65-67.

2. Joe retires. *Badger Chemist* 28:1 (1981). Additional information in 4:9 (1956), 5:3 (1957), 10:12 (1963), 13:4 (1966), and 16:3 (1969).

3. C. F. Curtiss, R. B. Bird, and P. R. Certain. *J. Phys. Chem.* 86:6A-8A (1982); on p. 8A there is an "academic genealogy" showing Joe's Ph.D.'s (his "academic children"), his "academic grandchildren," etc.

4. Obituary. *New York Times.* March 31, 1990, p. 11.

5. A. J. Ihde. *Chemistry as Viewed from Bascom's Hill.* Department of Chemistry, University of Wisconsin-Madison (1990):542.

6. J. O. Hirschfelder. Henry Eyring, 1901-1982. *Ann. Rev. Phys. Chem.* 34:10-16 (1983).

7. J. O. Hirschfelder. My adventures in theoretical chemistry. *Ann. Rev. Phys. Chem.* 34:1-29 (1983).

8. J. O. Hirschfelder. Some new directions in molecular quantum mechanics. *J. Phys. Chem.* 86:1045-52 (1982).

9. J. O. Hirschfelder. The scientific and technological miracle at Los Alamos. In *Reminiscences of Los Alamos, 1943-1955.* Edited by L. Badash, J. O. Hirschfelder, and H. P. Broida. Dordrecht, Netherlands: D. Riedel Publishing Company (1980):67-88.

10. Television interview with J. O. Hirschfelder by Channel 27 (WISC) in Madison, Wisconsin. June 1981.

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FREDERICK KAUFMAN

September 13, 1919–July 6, 1985

BY MICHAEL F. GOLDE

REDERICK KAUFMAN WAS a leader in the field of gas-phase chemical kinetics and its application to the understanding of atmospheric and combustion processes. He figured prominently in the national, and later international, debate concerning the possible impact of the chlorofluorocarbon class of compounds on the stratospheric ozone layer. His stance on this issue was typical of his clearsightedness and integrity as a scientist: legislation concerning production and use of these compounds Should be based on reliable experimental data and computer models. He urged moderation and caution until the reliability of this information could be established. Subsequent actions, first, to ban the use of freons in aerosol propellants, and second, to control more stringently the production of the potentially most hazardous chlorofluorocarbons, were based on the careful program of chemical kinetic measurements conducted by him and others in laboratories around the world.

Many of these studies were made possible by Kaufman's pioneering work in the 1950s, which adapted the venerable discharge-flow technique of Wood and Bonhoeffer into a modern tool for gaining information on the rates and products of elementary reactions, the simple building blocks of complex reaction mechanisms. Until his death in 1985 he remained at the forefront, exploiting new developments to ensure that the discharge-flow technique remained a highly versatile tool for obtaining bulk kinetic data and of equal importance to the following generation of complementary state-to-state techniques.

Fred Kaufman was born in Vienna and his twin loves of music and chemistry had already emerged by 1938, when, following the annexation of Austria by Hitler, his family emigrated to Panama. Sadly, an accident there to his hands, which required extensive surgery, forced him to abandon hopes of a professional career as a concert pianist. He visited the United States in 1940 for medical treatment and moved to Baltimore the following year. Focusing on a scientific career, he began evening undergraduate courses at Johns Hopkins University while continuing to work full time. In 1944, under a new program, he began graduate work at Johns Hopkins and received his Ph.D. in 1948, bypassing the undergraduate degree. His research advisor was Alsoph Corwin, in the area of chemical kinetics in solution.

He began work in the combustion section of the U.S. Army's Ballistic Research Laboratories at Aberdeen Proving Ground and rose to the position of chief of the Chemical Physics Branch. Although also engaged in pyrolysis and other high-temperature combustion studies, his interest in exploring the underlying elementary gas-phase reactions increased during the early 1950s. With the award of a Rockefeller Public Service Award in 1955, he was able to spend a year in the Department of Physical Chemistry at Cambridge University, which Professor Norrish had established as a center for kinetics studies. There he began his pioneering discharge-flow studies with a survey of reactions of oxygen atoms. Also in this decade he began his long association with the Combustion Institute, and advances in the understanding of elementary gas reactions were regularly reported at the biennial symposia. In 1964 he was invited to move to Pittsburgh, with the establishment of the Space Research Coordination Center at the University of Pittsburgh. The decision was not easy to make, particularly as the Kaufmans' oldest son Roy was already married and settled in Baltimore. However, there were exciting plans afoot in Pittsburgh (unfortunately never realized) for a massive new research center to be built in Panther Hollow, a valley on the edge of Schenley Park and close both to the university and to Carnegie Tech, and Kaufman, enjoying the anticipated challenges, accepted the position of full professor in the Chemistry Department. With his wife Klari and younger son Michael he took up residence in the prosperous community of Squirrel Hill and their house, infused by the warmth of their marvelous personalities, rapidly became and remained an oasis for colleagues, students, and friends.

The Space Research Coordination Center (SRCC) was created with funding from the National Aeronautics and Space Administration (NASA) to promote studies in the natural and social sciences, engineering, and health areas concerned with the aerospace field. Several prominent scientists were immediately attracted to the SRCC, including Thomas Donahue, an aeronomer and the first director of the SRCC, who was already on the faculty of the Pitt Physics Department, and the physicists Wade Fite, Manfred Biondi (moving across town from the Westinghouse Corporation), and Edward Zipf. Kaufman was the sole chemist in the five-story building and, although a new chemistry building became available in 1974 and provided much-needed space for the previously widely scattered department, he chose to keep his office and laboratories in the SRCC building.

The move to Pittsburgh confirmed a shift in the focus of his research from combustion problems to the chemistry of the atmosphere, in particular the stratosphere. His involve ment in all scholarly activities rapidly broadened and included advisory service on panels and committees of the National Academy of Sciences, NASA, AFOSR, National Science Foundation, and NRC, and he became director of the SRCC from 1974, chairman of the Chemistry Department between 1977 and 1980, University Professor in 1980, and president of the Combustion Institute in 1982. In 1979, the year in which he was elected to the National Academy of Sciences, he was chosen as the speaker to represent the faculty honored in that year's University of Pittsburgh honors convocation.

He was also fully involved in the teaching program at Pitt, having a preference for the general chemistry courses, while his advanced graduate course in chemical kinetics, presented every second year, provided an excellent introduction to the theory and practice of that field for a succession of physics and chemistry graduate students and post-doctoral fellows. He won Outstanding Educator of America awards in 1971 and 1975.

In 1984 his sixty-fifth birthday was honored by special symposia at Harvard University and at the University of Pittsburgh. However, it was an anxious time for his family and friends, because of illness that struck the previous year. The intensity and commitment of his research effort did not abate, however, and it was at a conference that his last illness started, leading to his death in July 1985.

Kaufman's major research contributions were in the areas of combustion and atmospheric science. By the 1950s it had become clear that combustion, for instance in flames, comprised a complex array of simpler reaction steps involving atoms and radicals. Rather than attempting to characterize these reaction steps, and thus the complete mechanism, from observation of flames, Kaufman was among those who realized the importance of determining quantitative data for each elementary reaction in isolation from competing reactions. To form the relevant atoms and radicals he revived the discharge-flow technique dating from the 1920s and developed techniques to allow quantitative detection of these species, thus allowing rate constants for their reactions to be measured. Initially at the Ballistic Research Laboratories and then at Cambridge University in 1955-56, he characterized gas phase titration reactions of N and O atoms, in particular that of oxygen atoms with nitrogen dioxide, and monitored their progress by chemiluminescence (i.e., ultra-violet and visible light emitted during the reactions). In a major publication arising from his work at Cambridge, Kaufman showed very clearly the broad range of elementary reactions. Thus the reaction of oxygen atoms with NO was found to be termolecular, requiring a third-body or chaperon to stabilize the nascent hot nitrogen dioxide molecule. In contrast to several fast oxygen-atom reactions, those with nitrous oxide and carbon dioxide occurred far below the collision rate. In addition, each chlorine molecule was able to consume several oxygen atoms by way of a chain reaction, which much later was recognized as a key sequence in the removal of stratospheric ozone by chlorofluorocarbons.

The discharge-flow technique was rapidly and ingeniously exploited by Kaufman and others, such as Schiff, using mass-spectrometric detection, and Westenberg, who succeeded in measuring absolute concentrations of radicals using electron spin resonance. However, a more sensitive general detection technique was needed and Kaufman in 1961 applied resonance ultra-violet absorption to the detection of the hydroxyl radical OH formed by the important titration reaction of hydrogen atoms with nitrogen dioxide. The key was the use of a source lamp that emitted OH radiation, thus optimizing overlap of the emission and absorption line profiles. The use of OH in this pioneering experiment was prophetic, because it became established later as the most important radical in neutral combustion and atmospheric chemistry.

This detection technique evolved during the next decade to the more sensitive resonance fluorescence, in which the fluorescence following the photon absorption event was monitored, and then to laser-excited fluorescence, in which the resonance lamp was replaced by a much more intense tunable laser light source. With these improvements a powerful technique was in place to allow a vast range of elementary reactions to be studied.

The heart of combustion and atmospheric processes, as with biological processes, is the chemistry of carbon, hydrogen, oxygen, and nitrogen. Kaufman's work was focussed remarkably tightly on key reactions of small molecular fragments containing just these atoms, with only a few excursions into studies involving chlorine, hydrogen chloride, and hydrogen fluoride. A partial listing of the reactions that he and his coworkers studied includes most of the fundamental reactions of combustion and atmospheric chemistry:

- Recombination of hydrogen, oxygen, and nitrogen atoms;
- Combination of oxygen with nitrogen atoms;
- Reactions of hydrogen atoms with O_2 and HO_2—of oxygen atoms with O_2, NO, NO_2, O_3 and HO_2—of nitrogen atoms with NO—of electronically excited nitrogen atoms and molecules with O and O_2—of hydroxyl radicals with OH, HO_2, O_3, CH_4 and several Cl-substituted methanes— and of the charged species O_2^+, NO^+ and H_2O^+.

The primary goal of these studies was the rate constant, which is closely related to the probability that an encounter of the reagent molecules would lead to reaction. Kaufman consistently sought to achieve direct measurements, in which the concentrations of the reagent species were known from measurement rather than inferred from modeling calculations, and in which interfering secondary reactions were avoided or rendered unimportant. This required a constant search for new titration reactions to generate the radicals of interest. In addition, ultra-sensitive detection techniques were required, because in some cases secondary reactions could be controlled only by using extremely low radical concentrations. A particularly challenging example concerned the reaction of OH with HO_2, which is a major sink for HO_x species in the atmosphere and which engrossed the chemical kinetics community for much of the 1970s and 1980s. Both reagents are unstable radicals and susceptible to self-reaction and other competing reactions. Initial rate investigations were indirect and produced widely scattered values of the rate constant. In his first publication on this reaction, in 1978, Kaufman carefully discussed the criteria for successful modeling of the reaction system and was characteristically cautious about the validity of the derived rate constant. Three years later his laboratory established a much more direct route for the investigation of this important reaction.

Through the succession of reliable measurements from his laboratory and his outspoken criticism of less direct approaches to rate determination, Fred achieved a unique position within the chemical kinetics community. He represented the highest of standards and helped instill in his younger colleagues a similar spirit. He rapidly became involved in the debate concerning possible ozone depletion through artificial introduction of chemicals into the atmosphere. The relevant chemistry was believed at the time to be dominated by gas-phase reactions involving ozone and oxygen atoms with hydrogen-, nitrogen-, and halogen-containing molecules and radicals, and could be modeled by combining the results of investigations of individual elementary reactions with independent information on species abundances in the atmosphere and gas transport. Kaufman was one of the scientists called on to testify before congressional committees concerning the possible impact of supersonic transport engine exhaust gases on the ozone layer. Later he served on several panels, such as the National Academy of Sciences' Committee on Impacts of Stratospheric Change, which were particularly concerned with the long-term effects on the ozone layer of the release of chlorofluorocarbons. His contributions were recognized by the dedication of the massive 1985 atmospheric ozone report to his memory. His special role in the chemical kinetics community was likewise recognized with the posthumous award in 1987 of the Polanyi Medal by the gas kinetics group of the Royal Society of Chemistry.

Kaufman's role in the area of combustion chemistry was somewhat different. Because of the very large number of elementary reactions involved and the high temperatures of flames, the input rate data were less precise than in the atmospheric chemistry models. Kaufman served in part as a unique resource to the modeling community. His advice was invaluable in assessing the likely reliability of rate data, but he was also adamant in demanding proper sensitivity analysis and assessment of uncertainties in the conclusions drawn from the analysis of the data. Many were the times when his eagle eye caught a suspicious-looking rate constant in a speaker's presentation slide; there followed an incisive and pointed question. His purpose was never to belittle the author, but rather to instill the same critical approach to the analysis of data as he insisted on in his own laboratory.

Fred participated in the biennial symposia of the Combustion Institute for over two decades. He rose over the years to be a member of the executive committee, vice-president from 1978 to 1982, and president from 1982 until his death. He was the plenary lecturer at the 19th Symposium in 1982. Interestingly, in the 1980s his research turned again to fundamental kinetics problems in combustion chemistry, with a major study of the reaction of hydrogen atoms with oxygen molecules (which controls the second explosion limit of the hydrogen-oxygen reaction) and a survey of reactions of the methoxy radical, CH_3O.

Collaboration with his SRCC colleagues, especially Wade Fite and Fred Biondi, helped to spur his involvement in two related areas of reaction rate measurements, namely ion-molecule reactions and reactions of electronically excited species. In the first of these areas, his group undertook a major study of reactions of ions with water and of water ions, H_2O^+, with several neutral molecules. Equally important to understanding of the upper atmosphere were investigations of the rates and products of reactions of excited nitrogen atoms and nitrogen molecules, especially with oxygen atoms and molecules.

As mentioned already, one of Kaufman's innovations in these studies was the use of resonance radiation absorption to monitor the concentration of the reactive atom or radical. These applications were complemented by more fundamental measurements of the radiative transition probabilities for species such as hydrogen, nitrogen, and oxygen atoms and hydroxyl radicals. This work and the special nature of the SRCC led to one of the most unusual and interesting projects of his career. The catalyst was James Anderson, who came to Pitt as a postdoctoral associate of Tom

Donahue. Anderson's doctoral research involved use of resonance radiation to measure the hydroxyl radical in the atmosphere and he was intrigued by Kaufman's applications of the technique. From the resulting collaboration of Anderson, Donahue, and Kaufman was born the plan to measure by resonance absorption the oxygen and nitrogen atom densities in the upper atmosphere. The experiment, which also involved graduate student Terry Rawlins and Bob Hudson of the Goddard Space Flight Center, was implemented on the Apollo-Soyuz space mission of 1975, the light sources and detectors being mounted on Apollo and reflectors on Soyuz, both flying at an altitude of 225 kilometers. In the time-honored way of experiments, the first attempt, with a spacecraft separation of 150 meters, yielded no signal except for possible weak resonance fluorescence; however, on the following orbit with the craft now 500 meters apart, excellent absorption and fluorescence data were obtained.

How did Fred Kaufman conduct his research? From the late 1960s he rarely was active in the laboratory, but he was nevertheless in control of each project. He was always accessible to his group and would listen carefully to each student's tale of success or woe. He was invariably courteous but one sensed his irritation when the experiment failed to cooperate for whatever reason. He met at least once a week with his entire group and a lively discussion would inevitably ensue. This intense involvement in science extended outside the laboratory; whether at conferences, dining a visitor, or sitting hunched over the telephone, he always had the goal of a full understanding of the problem at hand. Under his tutelage, his students blossomed—some would take longer than others but usually he had the intense satisfaction of seeing yet another mature scientist leave the laboratory and move on to make his or her mark elsewhere, normally still in gas kinetics or related areas. Of his forty or so pre- and postdoctoral associates, some seven are currently in academic posts, nine involved in government research laboratories, and most of the remainder in other high-technology research positions.

Kaufman's immersion in chemistry made him sensitive to the major questions in the field. A significant portion of his research was devoted primarily to improving fundamental understanding of how reactions occur and how molecules gain and lose energy. If he could be said to have been fascinated by a single chemical species, that species was nitrogen dioxide; it reached center stage in 1958 with his eleventh publication and featured also in one of his last, in 1985.

Throughout his career he puzzled over the strong fluorescence from the lowest group of electronically excited state of NO_2. First, he established the mechanism of combination of oxygen atoms and nitric oxide into these states, discovering the intricate competition between radiation by these states and their collisional deactivation to lower energy, non-emitting states. Beginning in 1966 his group populated the same states by exciting nitrogen dioxide with visible light, later using the temporal development of the fluorescence to gain analogous information but in more detail. These were among the first investigations of the mysterious communication between quantum states, later known as IVR (intramolecular vibrational relaxation), and thus Kaufman helped set the scene for one of the major research fields of the 1980s. Ironically, although (with sulfur dioxide) among the first molecules to be studied in this way and although the subject of a vast number of investigations, nitrogen dioxide still conceals many of the secrets of the dynamical properties of these excited states.

Another area that Kaufman entered relatively late but exploited to bequeath future generations a fascinating ar ray of still poorly understood data was that of infrared chemiluminescence of the products of chemical reactions. Initially set up to study infrared emission from nitrogen dioxide from reactions of nitric oxide with oxygen atoms and with ozone, this system was used to measure rates of vibrational relaxation of many vibrational states of hydrogen chloride and hydrogen fluoride and deuterated analogues in collision with various molecular species. In agreement with well-established theoretical models the probability per collision of relaxing the first excited level was quite small for most collisional partners. However, the probability was observed to increase with vibrational quantum number to near unity for most polyatomic relaxers. This unexpected behavior remains a challenge to theorists; there is a strong implication that IVR is efficient in the collision complex.

It is perhaps surprising that Kaufman did not extend his research to include theoretical calculations beyond minor incursions into transition-state theory and the bond energy-bond order model. He doubtless felt that it was his role to obtain the relevant mechanistic information experimentally and he gained much insight into particular reactions through ingenious use of isotopic substitution or careful searches for key reaction products. Although he referred in print to his nontheorist mind, he was keenly aware of the status of theory regarding thermal reaction rates. His graduate course on chemical kinetics was dominated by description and critical discussion of theories of bimolecular and unimolecular reactions. The phrase “critical discussion” perhaps expresses well his approach: just as we have seen that he expected experimentalists to defend their experimental approach and critically to assess the reliability of their conclusions, so he wished to apply the same standards to the work of theorists, in particular regarding the validity of conclusions drawn from theoretical calculations. This constituted an almost impossible demand on theorists and led to a slight prickliness in his relations with some in that community. However, in the late 1970s when his concern led him to organize a symposium around this subject, the response was overwhelming and the three-day event attracted a brilliant group of experimentalists and theorists. The symposium was titled: "Current Status of Kinetics of Elementary Gas Reactions: Predictive Power of Theory and Accuracy of Measurement." It also paid special attention to the compilation and critical evaluation of rate data. This unusual occasion, with its sharp focus on the general subject of thermal rate data, was clearly a great success, as measured by developments in the 1980s. Thus, in this area as in so many others the colossal influence of Fred Kaufman is felt, and it is appropriate to end this account with his introductory remarks in the *Journal of Physical Chemistry*,[1] which reported on this meeting. They are as valid fifteen years on as they were at the time.

My reason for calling this meeting was the need to take stock of the present state of the field of elementary gas reaction kinetics: to assess the accuracy of measurement techniques; to discuss the compilation and evaluation of rate data; and, most importantly, to examine the predictive power of theory. The stock-taking was stimulated by recent advances in the direct experimental measurement of elementary atom or radical reaction kinetics and by the preoccupation of theorists with problems of detailed state-to-state dynamics at a time when the demand for rate constants, measured, calculated or guessed, is growing rapidly in such diverse fields as atmospheric chemistry, combustion, and pollution. For these reasons, the subject matter of the symposium was sharply focused on thermal reaction rates of neutral (electronic) ground-state species, not because state-to-state dynamics or excited state reactions or ion reactions are any less interesting, but because thermal reactions have recently been treated with much benign neglect. They did, after all, form the foundation of reaction rate theory in the 1920s and 1930s, yet have only recently become open to direct experimental measurement of good accuracy.

A large number of key questions ought once again to be asked and their answers examined in the light of laboratory results. They include the following: validity and the limitations of transition state theory; potential energy surfaces, how to calculate them (ab initio versus semiempirical) and what detail is required (in a cost-benefit analysis sense); classical trajectory calculations; quantum corrections based on one-, two-, and three-dimensional theory; nonequilibrium effects in two-body reactions; energy transfer in dissociation/recombination reactions and its dependence on excitation energy, molecular complexity, and temperature; prediction of rate parameters over large temperature ranges for widely different molecular complexity or for series of reactants differing only in substituent effects; implication of energy disposal information for thermal rate constants; critical test cases presently available or to be developed for theory-experiment comparison.

The reader of this journal issue must decide which of these and other issues have been brought closer to successful resolution. My own brief appraisal would begin with the statement that the meeting seemed a useful and successful exercise, that it should probably be repeated in a few years, and possibly become a regularly scheduled event, albeit an infrequent one.

In assessing the present state of affairs in the three topical areas, it is probably fair to say that the greatest progress has been achieved in the first area under discussion, that of experimental measurements. Here the wide use of highly sensitive detection techniques (resonance fluorescence, laser induced fluorescence, laser magnetic resonance, molecular beam sampling mass spectrometry, etc.) and the wide range of atom or radical generation techniques (photolytic, discharge, thermal, chemical, etc.) has made it possible to make measurements on vastly more reaction systems than ever before, and to do so in a direct manner (i.e., without recourse to classical methods of fitting analytical data to proposed mechanisms). The major experimental methods, flash photolysis and discharge-flow for the low temperature range and shock tube for high temperatures, continue to dominate the scene. Other methods (e.g., very low pressure pyrolysis [VLPP]) are making major contributions, especially for bond fission reactions of large molecules. Hybrid techniques (e.g., discharge flow shock tube and extensions to high temperatures [high temperature fast flow reactor]) are successfully bridging the gap between the widely separated temperature regimes of earlier studies. The realistic appraisal of experimental error still leaves much to be desired and we are fortunate in having the fine review paper by Cvetanovic, Singleton, and Paraskevopoulos to help us put our house in order. Experimental rate measurements of elementary reactions have certainly "arrived" and their future looks very bright indeed, both in regard to improved accuracy and to wide applicability to reaction systems.

The second topical area, compilation and critical evaluation of rate data, suffers greatly from being underfunded. The papers devoted to this field and the ensuing discussion show the urgent need for increased support. This is due both to the proliferation of experimental studies and to increased "user" pressure, mainly for modeling calculations in atmospheric chemistry, combustion, or pollution studies. Rate data evaluation is a relatively small, inexpensive activity, but it is in great demand by many groups: by experimentalists to keep abreast with the field; by theorists to have reliable results to guide and check their calculations; and by modelers to provide them with input parameters for computer codes. It is clear, of course, that compilation and evaluation spans a wide spectrum and that different "customers" may have very different requirements. Yet the overall need for faster progress on all fronts (i.e., for greater funding support) seems well substantiated.

The third area, predictive power of theory, makes up almost two-thirds of the symposium and of the published papers. It is also the most difficult to assess in a broad, overall sense. There has been clear progress on all fronts. Ab initio, three-dimensional, fully quantum calculations of the dynamics of some simple systems ($H + H_2$), routine three-dimensional classical trajectory calculations on many systems, ab initio and semiempirical potential energy surfaces, testing of various approximate theories against exact calculations in the easily accessible one-dimensional format (mainly for A + BC reactions), development of improved statistical theories of dissociation-recombination reactions, and continued application of transition state theory, particularly in its thermochemical variant (Benson, Golden), with excellent success to a host of complicated systems. To my nontheorist mind, many major questions remain unsettled: How extrapolatable are one-dimensional concepts and findings to the real world? What is the present and near-future accuracy of ab initio potential energy surface calculations and what impact can they be expected to have on elementary reaction rate calculations? How many and what kind of scaling parameters are needed in the characterization of semiempirical surfaces for thermal rate constant calculations? How are quantum (tunneling) effects best approximated in complex reaction systems? How serious are the necessary overestimates of equilibrium (transition state) theory rate constants due to "recrossing" ef fects, due to non-uniqueness of the transition state, due to specificity of energy disposal? What is a conservative estimate of the predictive power of thermochemical kinetics? As good as a factor of 2 or 3 in the Arrhenius A factor? The list of questions could be lengthened almost indefinitely, but enough. There is clearly much more work to be done. What impresses me, however, is the general usefulness and resilience of simple transition state theory which, after early triumphs went into a lengthy eclipse only to reemerge as a surprisingly accurate (and sometimes as the only) tool of the gas phase kineticist.

Lastly, the symposium did achieve its major goal: to bring experimentalists and theorists together and to show that the field of thermal elementary reaction kinetics is alive and well.

NOTE

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DANIEL SANFORD LEHRMAN

June 1, 1919–August 27, 1972

BY JAY S. ROSENBLATT

ANIEL S. LEHRMAN DIED in Santa Fe, New Mexico, in the early morning of a day late in August 1972 at the age of fifty-three. He was scheduled in a few days to give a major address at the American Psychological Association meeting in Hawaii and had prepared for the trip characteristically by collecting lists of birds he wanted to see in Hawaii and by arranging bird watching expeditions with several resident ornithologists. The immediate cause of death was a heart attack. His obesity over many years had weakened his heart. Because Dan had been able to keep his deteriorating heart condition from his closest friends and colleagues, his death came as a shock to all of us. He was unable to change his way of living. With his characteristic optimism and boundless energy, he continued to live his life to the fullest despite his declining strength. He kept up his travel throughout the world and especially to places like Kenya where he could see animals in the wild. He gave many talks at universities, visited colleagues, enjoyed the finest eating places, and attended conferences worldwide.

In the last years of his life he received many honors. He became a Fellow of the Salk Institute in La Jolla, where he spent several months a year. He was elected a member of the National Academy of Sciences and the Society of Ex perimental Psychologists, and he was a Fellow of the American Academy of Arts and Sciences. In addition, he held a coveted lifetime Research Career Award from the National Institute of Mental Health to pursue his research.

Among his other accomplishments, he founded the series *Advances in the Study of Behavior* with Evelyn Shaw and Robert A. Hinde in 1963 and was its editor until his death. He also served a three-year term as associate editor of the *Journal of Comparative and Physiological Psychology.*

Dan's reputation as a research scientist was acknowledged, but he was also an excellent teacher and lecturer. As a lecturer and teacher, he influenced a whole generation of students in animal behavior in this country and abroad, in addition to those who studied with him at the Institute of Animal Behavior. As a research scientist, he initiated a new era in the study of hormone-behavior relations, which emphasized the important role social interactions play in reproductive behavior and physiology. As a theorist, he had a strong influence on how psychologists and zoologists thought about questions of instinctive behavior, behavioral development, and the evolution of behavior among animals.

The center of Dan's activities was the Institute of Animal Behavior at Rutgers University in Newark. Dan established the Institute and served as its director until his death. The Institute had its beginnings in 1954 in his own laboratory, which was located on the top floor of what had been a brewery. There he began his research on the neuroendocrine basis of reproductive behavior in the ring dove. In 1958 the laboratory was enlarged and moved to a nearby building in which two floors were renovated to accommodate additional researchers including my own laboratory. In 1959 the university Board of Governors authorized the Institute of Animal Behavior to grant a Ph.D. in psychology with a specialization in psychobiology. Over the next sev eral years the Institute gained its independence from the Graduate Psychology Department in New Brunswick, the location of the main university campus. The Institute moved again in 1968 to its present, greatly expanded quarters. These expansions of the Institute were funded by the National Science Foundation and the Ford Foundation at the urging (and with the enthusiastic support) of William C. Young.

From the late 1950s through the early 1970s until his death, Dan recruited additional staff for the Institute. Characteristically he chose scientists whose research supplemented and complemented his interest in both the naturalistic study of behavior, primarily in birds, and the experimental analysis of social and reproductive behavior and physiology in birds and mammals. A list of the faculty he recruited shows that his aim was to establish a multidisciplinary staff of scientists representative of the most active areas of research in ethology and comparative psychology and in neuroethology and behavioral biochemistry. They included Colin G. Beer (a New Zealander recently from Oxford, where he took his degree with Niko Tinbergen), who established a field station at Brigantine, New Jersey, to study laughing gulls and other shorebirds, and Ernst W. Hansen (a recent student of Harry Harlow), who established a colony of rhesus monkeys at the Institute for studying primate social behavior, a newly developing area in animal behavior. I joined the staff after studying with Theodore C. Schneirla and Lester R. Aronson at the American Museum of Natural History. I established a rat colony for studying the hormonal basis of maternal behavior. Barry R. Komisaruk, a graduate of the Institute and a recent postdoc of Charles H. Sawyer, was recruited to set up an electrophysiology-neuroendocrinology laboratory. Harvey H. Feder, a student of William C. Young and a recent postdoc of Geoffrey Harris, established a steroid biochemistry laboratory for studying action of hor mones at brain sites mediating sexual behavior. Monika Impekoven was recruited as an associate of Colin Beer; she was a former student of Tchanz from Switzerland and she studied pre- and post-hatching behavioral development in birds. Mei-Fang Cheng, from Taiwan, recently from the University of Pennsylvania, was recruited as Dan's research associate to study the ring doves.

Through Dan's efforts the Institute was awarded center support grants by the National Institute of Mental Health to support basic research and administration. We were awarded training grants to support the training of students and postdoctoral fellows. Dan made the Institute a national and international center for animal behavior by inviting visiting scientists to spend six months to a year in residence at the Institute to be available for discussion with students and faculty. In addition, he established a colloquium series, which continues to the present day, where leading scientists from throughout the world, passing through the New York area, present their research. As an example, Niko Tinbergen, later to win the Nobel Prize as co-founder of ethology, gave a series of talks at the inauguration of the new Institute building in 1968. Dan was popular with (and highly respected by) the administration of the university, which fully supported the Institute.

The location of the Institute in Newark was always a subject of curiosity. On several occasions Dan was invited to move it to New Brunswick. However, out of his loyalty to the college that had generously supported the Institute during its early years, he insisted that the Institute remain at the Newark campus, then mainly an undergraduate college. For a similar reason he refused offers from other universities, Harvard University among them, to move the Institute. Dan's entire twenty-two year career was spent at Rutgers University in Newark.

Dan attended public schools around the New York area and the elite Townsend Harris High School. His education at City College was interrupted by four years of army service and he received his B.S. degree in 1946 with majors in biology and psychology. Because of his intense interest in bird watching, he was an erratic college student. Several times he would be suspended for missing classes, often during the spring bird migration, only to be reinstated. He became an expert ornithologist, which he credited to the influence of a scoutmaster who remained a close friend throughout his life. His ambition in his early teens was to become the warden of an animal preserve and live in a small cottage at the edge of a woods guarding the animals. It was not until quite late that he realized he could make a career of his love of bird watching. At the urging and help of T. C. Schneirla he was admitted to the graduate program in psychology at New York University and received his doctoral degree in 1954. His doctoral thesis was on parental care in the ring dove and specifically on the effect of experience and the role of the crop gland as a source of stimulation motivating parental regurgitation feeding of the squabs.

Dan began doing research as a teenager under the eminent herpetologist G. Kingsley Noble, curator in the Department of Experimental Biology (later the Department of Animal Behavior) at the American Museum of Natural History. In 1938, at the age of nineteen, Dan published his first research paper on egg selection behavior during incubation in the laughing gull. The research was done at New Jersey's Brigantine Wildlife Preserve, which later became the Brigantine Field Station of the Institute of Animal Behavior. As a teenager at the museum he met Theodore C. Schneirla, William Etkin, Frank Beach, Ernst Mayr, Libby Hyman, and Niko Tinbergen (who was visiting the United States). He was drawn increasingly to the study of evolu tionary biology, particularly reproduction and specifically the influence of hormones on reproductive behavior. Dan acknowledged that he was most influenced in his theoretical approach to animal behavior by T. C. Schneirla, who was, until his death in 1968, curator of the Department of Animal Behavior, and under whom Dan did his doctoral research at the museum from 1948 to 1954.

Dan had varied research and teaching experience before he came to Rutgers University. He worked for a period at Haskins Laboratory on the development of prosthetic devices for the handicapped and studied facial perception in the blind. He held a summer fellowship in a newly established program in animal research at the New York Zoo and worked alongside Nicholas Collias, a leading student of animal behavior. In 1947 he began to teach at City College as a fellow and lecturer. This was an important period in his life, because during this period he developed close relationships with clinical psychologist and psychoanalyst Max Hertzman and with Jules Nydes, his own psychoanalyst. They served as role models for him and Dan acknowledged their influence on his thinking and especially on his understanding of what motivated him as well as other scientists to engage in the study of animal behavior, a subject that emerged in his later writings. Dan joined the Psychology Department in the Newark College of Arts and Sciences at Rutgers University in 1950. However, he continued to teach in the evening at City College until the early 1960s, because he said he recruited some of the finest students for the Institute from his evening classes.

Dan began his research on the behavioral interactions during reproduction between male and female ring doves in 1954. He was able to show that the consequence of these behavioral interactions and the endocrine responses they evoked gave rise to each of the phases of the reproductive cycle in the ring dove. This research established for the first time that behavioral stimulation by one animal could elicit an endocrine response in another, a phenomenon that is commonplace now, but at that time had not been clearly established. The research had originated from Dan's naturalistic observations of the interactions between mates during reproductive behavior among the birds he observed in the field and the recent discovery by Geoffrey Harris of a humoral pathway between the brain and the pituitary gland. This link between the neural and endocrine systems enabled behavioral stimulation to reach the brain through the sensory systems eventually to stimulate the release of substances in the brain that would be carried to the pituitary gland where they would cause the release of pituitary hormones. Dan, studying ring doves in this country, and Robert Hinde, studying canaries at Cambridge University, grasped the significance of this discovery for the study of reproductive behavior and physiology among birds. The field of behavioral neuroendocrinology can be said to have grown out of the studies of Lehrman and Hinde, who became close friends and communicated frequently about their research during this period, while exchanging personnel between the Institute of Animal Behavior and the Sub-Department of Animal Behavior at Cambridge University.

A high point during this period was William C. Young's request that Dan write a chapter for the 1961 revision of the classic volumes *Sex and Internal Secretions* that he was editing. Dan reviewed the entire literature of field and laboratory studies on the behavioral neuroendocrinology of parental behavior in birds and mammals in his now classic chapter.

In 1953 Dan published “A Critique of Konrad Lorenz's Theory of Instinctive Behavior,” his famous criticism of ethology that launched him as a major theorist in the field of animal behavior. It was written at the urging of Schneirla and required that Dan translate all of Konrad Lorenz's writings, which had not yet appeared in English. This was a task for which he was prepared by his army service as a cryptanalyst, during which he had become fluent in German. (He often recounted his role in planning the route of the daring American raid, originating in Italy where Dan was stationed, that destroyed the Ploesti oil fields in Romania. It required listening over many months to German air spotters reporting plane sightings and working out a route for the airplanes that would enable them to reach their target without being sighted by the spotters.)

The critique was leveled at the concept of innateness used by Lorenz and presented, as an alternative, a developmental approach to many of the behavior patterns viewed as innate by Lorenz. He introduced to ethnologists many of Schneirla's ideas of the role of stimulus intensity and of approach—withdrawal responses in the ontogeny and phylogeny of behavior—and he emphasized the role of experience, including self-stimulation as a source of experience. These were offered in opposition to Lorenz's concepts of innate releasing mechanism and innately based sign stimuli.

The story behind one controversial aspect of this article is worth recounting. While translating Lorenz's writings, Dan came across articles written during the 1930s in which Lorenz provided what purported to be scientific support taken from animal behavior for the racist policies of the National Socialist Party (Nazi) under Hitler. This was based upon Lorenz's concept of the hereditary nature of innate behavior patterns and the need to maintain their distinctness by preserving their hereditary purity. Dan wrote (1953):

He (Lorenz) states that a major effect (of unrestricted breeding) is the involution or degeneration of species-specific behavior patterns and re leaser mechanisms because of degenerative mutations, which under conditions of domestication or civilization are not eliminated by natural selection. He presents this as a scientific reason for societies to erect social prohibitions to take the place of degenerated releaser mechanisms which originally kept races from interbreeding. This was presented by Lorenz in the context of a discussion of the scientific justification for the then existing (in 1940) German legal restrictions against marriage between Germans and non-Germans.

In an early draft of the "Critique . . ." Dan included a final section presenting this material in Lorenz's own words as indicating the ideological significance of his scientific theory. However, in the final version of the article the section was significantly reduced and inserted earlier in the article where it did not attract as much attention. It was a difficult decision for Dan to make to reduce the prominence of this material, but he was counseled to do this by several of the leading scientists of the day such as Karl Lashley, Hans-Lukas Teuber, and Donald O. Hebb, who had read an early draft of the article containing this final section. They supported Dan's scientific arguments but they advised that the strong negative emotional responses still evoked in audiences to Nazi racial doctrines would obscure and weaken the impact of these scientific arguments.

Although the "Critique . . ." could have divided American comparative psychologists from European ethologists, its actual effect was the opposite. The European ethologists, whose own backgrounds were in evolutionary biology and often in ornithology, soon learned upon meeting Dan that he was not a typical American experimental psychologist who studied animals in contrived laboratory settings. They discovered that he was an evolutionary biologist, a naturalist, and an ornithologist like themselves. Like them he was interested in and knowledgeable about the natural behavior of animals, but unlike most ethologists at that time, he was also knowledgeable about experimental methods in comparative and developmental psychology, endocrinology, and neuroendocrinology. He, therefore, played an important role in the rapprochement between the European and American scientists that culminated in a month-long meeting of these two groups organized by Frank Beach and held at the Center for the Advanced Study of Behavior in Palo Alto in 1957. The meeting was attended by comparative psychologists mostly from North America (Dan, Hebb, Harlow, Beach, and myself) and European and American ethologists (Tinbergen, Hinde, van Iersel, Baerends, Vowles, and Hess). Dan's influence on the relationship between comparative psychologists and ethologists and the relations between them was solidified at this conference and he maintained close personal and scientific relationships with many of them throughout his life.

Dan was a most inspiring teacher and an accomplished speaker whose performances were famous for his imitation of ring dove bow-coo calls, wing flapping movements, and incubating eggs that accompanied his presentations. He spoke extemporaneously and was able to sense the level of understanding of his audience, speaking to them at their level and carrying them along with him as his story unfolded. On only one occasion that I know of did Dan use notes to give a lecture. He substituted for a famous professor at City College who made a show of the fact that notes for his two-hour lectures were scribbled on a single side of one envelope. Dan entered the classroom and, with an exaggerated gesture, produced an envelope, tore off the triangular flap that contained his notes for the two-hour lecture, and threw away the remaining envelope!

Dan was able to describe his research to scientists from other fields so that they understood it and shared his enthusiasm. This ability to convey ideas and information clearly and interestingly was an important reason why he was sought after by multi- and inter-disciplinary scientific groups, such as the group at Salk Institute. He was also quite generous in describing the research of his colleagues at the Institute during his many lectures in this country and abroad. He presented their research with the same enthusiasm and excitement as he presented his own research. He was eminently successful in this as we discovered later, because many scientists in the field who were knowledgeable about our research thought they had heard it from us and knew us personally rather than through Dan's descriptions.

During the last three or four years of his life Dan turned his thoughts and writing to general issues in the study of animal behavior. He was concerned with what motivates scientists in the study of animal behavior. He thought about the kinds of problems that interest them, how they choose the concepts they employ, and how ideology plays a role in theoretical differences among them. Dan perceived that deep emotional and ideological differences, revealed by semantic and conceptual formulations, divided scientists on important issues in the field of animal behavior and these could not be resolved by empirical data alone. Having in mind the then current controversy about the role of experience in species typical (i.e., innate) behavior, he wrote (1970):

When opposing groups of intelligent, highly educated, competent scientists continue over many years to disagree, and even to wrangle bitterly about an issue which they regard as important, it must sooner or later become obvious that the disagreement is not a factual one, and that it cannot be resolved by calling to the attention of the members of one group (or even of the other!) the existence of new data which will make them see the light. Further, it becomes increasingly obvious that there are no possible crucial experiments that would cause one group of antagonists to abandon their point of view in favor of that of the other group. If this is, as I believe, the case we ought to consider the roles played in this disagree ment by semantic difficulties arising from concealed differences in the way different people use the same words at different times; by differences in the concepts used by different workers (i.e., in the ways in which they divide up facts into categories); and by differences in their conception of what is an important problem and what is a trivial one, or rather what is an interesting problem and what is an uninteresting one.

In this regard Dan had a good deal of respect and admiration for Konrad Lorenz despite the deep differences in their theoretical views on the nature of innate behavior and the role of experience (and, of course, Lorenz's more political writing cited above). In fact, referring to his earlier “Critique . . .” directed at Lorenz, he wrote (1970):

When I look over my 1953 critique of his theory I perceive elements of (my) hostility. . . . It does fail to express what, even at that time, I regarded as Lorenz's enormous contribution to the formulation of the problems of evolution and function of behavior, and his accomplishment in creating a school based upon the conception of species-specific behavior as part of the animal's adaptation to its natural environment.

Despite their theoretical differences, Dan felt a kinship with Lorenz because their feelings towards the study of animals and the contemplation of nature were similar. Lorenz, he felt, was also receptive to (and excited by) the myriad phenomena of life and nature when he observed animals. Moreover, as Dan pointed out in comparing Schneirla and Lorenz's similar attitude towards the study of animals (1971):

They share the orienting attitudes that the life of the animal itself poses problems to the investigator, that the units of behavior studied should be natural units evolved through natural selection, and that the contemplation and appreciation of the complexities of nature are valuable human aims, independent of their usefulness in understanding human life (a problem to which both addressed themselves).

He believed his and Schneirla's differences with Lorenz were ideological; they arose from different cultural traditions and could not be settled by empirical evidence. These traditions dictated that for Lorenz the categories of innate and learned were sufficient to deal with how behavior became adapted to natural environments during evolution. But Dan believed these categories were too restrictive and narrow; he required the fluidity allowed by the developmental analysis of animal behavior, not restricted to these concepts, to understand the variety and complexity of adult behavior.

Dan was also concerned about the use of animal data to understand human behavior. On the one hand, he was sympathetic to the efforts of scientists who wanted to deepen their knowledge by seeking relations between phenomena in their own field and a discipline underlying their own. He was aware that those of us who studied animal behavior often evoked concepts from neurophysiology and neuroendocrinology. On the other hand, he was skeptical of this possibility in relating data from subhuman primates to humans and expressed it as follows (1971):

The way in which the sources of aggression in human beings are not only transformed, but arise in the course of social experience, does not leave us with any great hope that simple formulations about the way in which an animal has its hostility turned on and off by signals from other animals (in the way that we describe the behavior of gulls) really would be useful in dealing with human behavior.

The danger in using the data of animal behavior to understand human behavior, he felt, arose from the fact that in all animals the nature of individual and group functioning is embedded in complex frameworks of differences among species. These behavioral characteristics adapt them to different natural ecological and social conditions. Human behavior has its own place in this broad framework but that place cannot be established by finding similarities between human and animal behavior on the basis of seemingly simi lar phenomena. As an example of the misuse of animal behavior to understand human behavior, he argued against the then current use of mother-young relations in the rhesus monkey as a model of human mother-young relations. He pointed to the fact that in monkey species other than the rhesus (e.g., the spider monkey) known at that time, the mother-infant relationship was quite different in part because of differences in the kinds of individual relationships that existed in the social group. As a consequence, separation from the mother in the spider monkey was not as psychologically devastating as separation from the mother was in the rhesus monkey. This was because among spider monkeys other adults took care of the infant and it did not suffer the loss of its mother as much as the rhesus infant, which did not have the benefit of care by other adults.

To provide Institute of Animal Behavior students with the opportunity to learn about human behavior, and in line with the breadth of his own interest in human behavior, Dan organized the Institute of Cognitive Studies as a graduate doctoral program in the Psychology Department in Newark. He recruited to this program the leading Gestalt psychologists in the fields of social psychology, learning, perception, and cognitive psychology such as Solomon Asch, Irvin Rock, Dorothy Dinnerstein, John Ceraso, and Howard Gruber. Their theoretical orientation to human behavior, Dan felt, was compatible with the approach of the Institute of Animal Behavior to animal behavior.

In 1961 Dan married Dorothy Dinnerstein, who did research in perception and wrote on the relationship between men and women in her classic book *The Mermaid and the Minotaur.* Each of them grew through the support their relationship provided.

Almost the last thing Dan wrote expressed the understanding he had arrived at concerning the role animals had played in his own life. He wrote about one's orientation as a scientist to the study of animals (1971):

There is another aspect of the activity and the life of a scientist and another function for science, which is not often enough stressed. In addition to (or instead of) serving a function like that of an engineer, the scientist can also serve a function like that of an artist, or a painter, or poet—that is, he sees things in a way that no one has seen them before and finds a way to describe what he has seen so that other people can see it in the same way. This function is that of widening and enriching the content of human consciousness, and of increasing the depth of the contact that human beings, scientists, and nonscientists as well, can have with the world around them. This function of arousing and satisfying a sense of wonder and curiosity about the riches of the natural world, and of strengthening the civilized human being's weakened feelings of being part of the world around him, is a function which you can see being served in any hall or gallery of the American Museum of Natural History.

Dan died at the threshold of a period of vast expansion of the field of animal behavior and behavioral neuroscience along lines that would have won his enthusiastic approval. The study of the natural behavior of animals in the fields of behavioral ecology, mating preferences, parental behavior, and foraging, which are concerned with the adaptiveness of behavior, has clearly won out over the study of arbitrary, experimenter-oriented animal behavior in laboratories. The renaissance of developmental studies in the flourishing field of developmental psychobiology and the near obscurity of the innate-learned controversy indicates the correctness of his early views. Moreover, Dan would be impressed with the fact that since its inception, due to his efforts, the Institute has trained more than 100 doctoral students and postdoctoral fellows who have taken up research positions at leading universities and other research institutions in this country and abroad. Finally, the research carried on by Mei-Fang Cheng on the ring dove reproductive cycle since his death indicates, first of all, how much is still to be gained by studying the ring dove and, second, how correct he was in his belief that studies should proceed from an understanding of the complexity of social interactions to the analysis of underlying neuroendocrine-neurophysiological mechanisms.

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C. S. Piggot

CHARLES SNOWDEN PIGGOT

June 5, 1892–July 6, 1973

BY GEORGE R. TILTON

HARLES SNOWDEN PIGGOT WAS one of the founding fathers of ocean-bottom marine research. He was, in fact, a pioneer in the study of geologic phenomena in the ocean basins, whose work has revolutionized the way earth scientists view earth evolution. There were no ocean drilling projects when he began his research into radium activity in sediments. Consequently, he not only had to develop laboratory facilities for the measurements, but also had to design and build a coring device capable of obtaining undisturbed cores up to three meters in length. Techniques up to that time had been capable of collecting small surface samples of sediments with a grappling device, but the stratigraphic record was destroyed even in those limited samples. As Piggot stated, such samples give information about present conditions only and divulge nothing of past events. It is these past events that reveal valuable information about geologic processes at work on land as well as in the ocean basins.

His investigations finally produced reliable dates on sediments and sedimentation rates from the North Atlantic Ocean and the Caribbean Basin over a time span of some 300,000 years. Foraminiferal data from the cores, provided mainly by J. A. Cushman, showed that variations in the abundances of species marked changes in water temperatures that could be correlated with glacial and interglacial stages on the continents. This approach established a new and important method for working out the chronology of the glacial epochs. In addition, experiments with W. D. Urry established that the sedimentation rate in a Pacific Ocean red clay was lower by about a factor of five to ten compared to the Caribbean Basin. Although the techniques for measuring the water temperatures and ages of oceanic sediments by radioactive disequilibria have improved greatly since those early experiments, the pioneering work of Piggot and his colleagues placed the science on a firm footing for future studies.

Although Piggot is best known for his ocean sediment work, his scientific career was characterized by remarkable breadth. His doctoral dissertation was in the area of inorganic chemistry and involved oxidation of ammonia, a process for which he later received a U.S. patent. His first position after graduation was in organic chemistry at the United States Industrial Chemical Company, where he worked on processes for large-scale production of ethyl alcohol and acetic acid and their derivative products. Part of the work of his group there led to an improvement in the anesthetic quality of ethyl ether by addition of small quantities of ethylene.

Later in his career he entered the fields of geochemistry and geophysics at the Carnegie Institution of Washington. He provided the lead samples that Aston used to produce the first isotopic composition data for “common” lead and radiogenic lead from uranium decay. He received the Order of the British Empire for work on mine disposal in World War II and the Bronze Star from the U.S. Navy for service at the Bikini atomic test site in the postwar era.

Charles Snowden Piggot was descended from two generations of educators. His grandfather, Aaron Snowden Piggot, was a professor of natural philosophy in Baltimore and taught chemistry, geology, and mining engineering. He also taught chemistry in the School of Dentistry, which later merged with the Maryland Medical School. Finding no suitable text book he wrote *Piggot's Dental Chemistry*, a standard text book for many years. His son, Cameron Piggot, M.D. (1856-1911), studied medicine in Baltimore and was Professor Ira Remsen's scientific assistant at the Johns Hopkins University. He became a professor of chemistry at the University of the South in Sewanee, Tennessee, in 1889, and was later dean of the Academic Department.

Charles was born in Sewanee in 1892. His mother was Anne Olivia Cockey of a Maryland family. The Piggot family was always short of money because his father's salary was small and the university sometimes failed to pay it altogether. It never occurred to him that their financial plight was unusual because all the rest of the university staff was in the same condition. He found the Sewanee environment ideal for a growing boy. In many ways it seems to have been a classical mountain environment of the Old South. Their house was surrounded by forest with unlimited space for hiking, riding, hunting, and camping. Charles was required to carry drinking water from a nearby spring. It was also his duty to cut kindling, which, as the first one up each morning, he used to start a fire in the kitchen stove.

He remembered considerable feuding among the local mountaineers. The only person in the hills with medical training, his father treated all clans who sought aid and did not charge for his services; he considered them too poor to pay. As a result, both Charles and his father were treated kindly by all the local people.

There were also moonshiners as well as hunters and farmers among the population. He recalled that the killing of a federal revenue agent "seemed quite understandable and reasonable at the time!"

After three years of study he graduated from the University of the South in 1914 with B.A. and B.S. degrees. He received a prize for the best paper in economics. Although he had aspirations towards medicine, a lack of funds caused him to concentrate on chemistry, which promised earlier returns. He won a scholarship to the University of Pennsylvania for postgraduate work in chemistry. He also was awarded a scholarship at Johns Hopkins, his first choice, but it was canceled when the dean learned that he had also applied to Pennsylvania. Piggot stated that at Pennsylvania he was "amazed and shocked" by the lack of ethics and courtesies to which he was accustomed in the south. He studied there for two years and then, under a new scholarship, transferred to Johns Hopkins, where he was "once again among gentlemen." There he came into contact with Professor Remsen, who had guided his father in earlier years.

He spent the summer of 1916 at a civilian military training camp, where he received a commission as first lieutenant in the field artillery. In April 1917 his study at Johns Hopkins was interrupted by service in World War I when he served briefly with a field artillery battery, but was then transferred to a team at Johns Hopkins under J. C. W. Frazer, who was working on a means of protection from carbon monoxide gas. "We worked for over a year without a single positive or encouraging result. While working on a form of very finely divided manganese dioxide I had the luck to add a small amount of silver oxide and had the good fortune to obtain a catalyst which completely oxidized carbon monoxide to carbon dioxide whenever the incoming air mixture was thoroughly dry and there was sufficient oxygen to link up with the carbon monoxide," said Piggot in an autobiographical sketch. That catalyst has continued to be used by fire departments and in coal mine rescue devices.

After the war he returned to his graduate studies at Johns Hopkins where he received a Ph.D. degree in 1920, presenting a thesis titled "The Catalytic Oxidation of Ammonia by Manganese Dioxide." Upon graduation he started to work at the U.S. Industrial Chemical Company in a research laboratory devoted to preparing chemical products from ethyl alcohol and acetic acid as the principal starting substances. They developed processes for production of absolute alcohol in tank car lots; also anhydrous ethyl acetate, absolute ethyl ether, and ethylene gas. They found that 100% pure ethyl ether lost its anesthetic properties and that addition of a small amount of ethylene rendered it extraordinarily effective. He also developed a series of methylbenzenes from monobenzene to hexamethyl-benzene, which were useful as solvents. Thus, after completing a thesis in inorganic chemistry, he became active in the field of organic chemistry.

In 1922 the National Academy of Sciences received an invitation from the Ramsey Memorial Fellowship Trust of England to appoint a young chemist for advanced studies in chemistry as a Ramsey Memorial Fellow. Piggot became the first American to receive that appointment and went to University College, London, to study under the physical chemist, Frederick G. Donnan. Donnan introduced him to many stimulating British scientists. Among them was Sir William Bragg, who used some crystals of hexamethylbenzene that Piggot had prepared while working at the United States Industrial Chemical Company for his determination of the crystal structure of the benzene ring. This led to more work in Bragg's laboratory and with the help of Bragg's assistant, Dr. Mueller, Piggot constructed a small X-ray machine, which was used to determine the diameter of the CH_2 chain.

From England he traveled around Europe on vacations. While spending one of them at a winter resort in Switzerland he met a young woman from South Africa, Ruth Blaine, whom he married in 1927 after his return to the United States.

In 1925 he joined the staff of the Geophysical Laboratory of the Carnegie Institution of Washington to conduct research into the significance of radioactivity in geophysical phenomena. This work involved studies of the radium content of the various layers of the earth's crust with resultant heat production and efforts to improve the determination of geologic time by comparing the amounts of lead associated with its radioactive parent, uranium. Much of his early work at Carnegie concerned the geologic time aspect. At that time even such fundamental data as the isotopes of lead and uranium and their abundances were imperfectly known. To answer some of those questions he contacted F. A. Aston, who then operated the most advanced mass spectroscopy laboratory in the world, and arranged to supply lead samples for isotopic analysis. In 1927 he took a sample of pure lead tetramethyl to Cambridge, and, according to Aston, this resulted in the first successful determination of the mass spectrum of ordinary lead "after repeated failures" in earlier experiments.[1] The lead ions, which were collected on photographic plates, indicated isotopes at masses 206, 207, and 208. The isotope at mass 204 was not positively identified due to problems with interference from the isotope of mercury at mass 204. The relative abundances were reported as 206:207:208::4:3:7.

The next step was to study the isotopes of radiogenic lead. The first experiment sought to determine the lead isotopes due to the radioactivity of uranium. Since both thorium and uranium were know to decay to Pb, Piggot selected from Norway a Broggerite sample known to con tain a high concentration of uranium, but very little thorium. The first vial of lead tetramethyl was sent to Aston in 1928, but broke in transit. A second one, sent a few months later, arrived safely. For this sample Aston reported 206:207:208::86.8:9.3:3.9.[2] There was still no data for the isotope of mass 204. The contrast in the ^{207}Pb/^{208}Pb ratios between the ordinary and radiogenic leads left no doubt that uranium possessed a second radioactive isotope postulated to be either mass 239 or 235. Through these activities Piggot's researches played a significant role in unraveling the mysteries of the radioactive decay systems and initiating the science of geochronology.

Upon arrival at the Geophysical Laboratory Piggot constructed an apparatus for determination of the radium content of igneous rocks by measurement of the activity of radon gas that was released by fusion of samples in carbonate fluxes. The resulting publications on the radium content of granites and their constituent minerals and Hawaiian basalts appeared between 1929 and 1932. He also tested for interstitial radium by leaching samples with hot water, finding that radon activity was typically lowered by 10 percent after the process, but that it grew back into equilibrium with its radium parent again.

The granite experiments paved the way for what was to be his major contribution from the radon work. Around 1933 he expanded his radium abundance survey to include some surficial ocean-bottom sediments. He was struck by the fact that the samples contained several times as much radium as the igneous rocks with which he was then working and wondered whether this high radium abundance was a surface effect or continued below the surface. To answer that question he had to develop a coring technique. The available bottom samples had been obtained with a telegraph snapper, which took a small bite out of the bot tom surface, destroying the stratigraphy in the process. By 1936 he had published a description of a coring device capable of obtaining cores up to about three meters in length, with preservation of the stratigraphy. Its main feature was the use of a powder charge to drive a steel tube into the sediments. The charge could be adjusted according to the depth of water and type of sediment. After retrieval the tube was split into halves to reveal the stratigraphy.

Using the newly developed coring device on board a cable repair ship, he collected his first cores from a traverse across the North Atlantic Ocean from Ireland to Newfoundland and obtained information that aroused renewed interest in oceanography and marine sedimentology. The record of glacial epochs (discussed below) could be recognized in the cores, as well as strata characterized by volcanic ash. The lack of thick sediment layers at the Mid-Atlantic Ridge was noted. The cores became the subject of *U.S. Geological Survey Professional Paper 196-A,* published in 1940, in which M. N. Bramlette and W. H. Bradley described the geological and lithological interpretations and J. A. Cushman and L. G. Henbest discussed foraminiferal studies. In addition, colleagues at the Department of Terrestrial Magnetism used the cores to measure changes in orientation of the earth's magnetic field over the past several hundred thousand years.

Piggot realized that the sediment layers in the ocean bottom would reveal a valuable historical record of the ocean basins, which should provide useful information about processes that have occurred on land. This can best be described in his own words (1938):

These sediments, lying layer upon layer in the bottom, have become the repository of the historical record of the oceans. This record includes the contributions of rivers, reflecting the changing conditions on the continents, as well as those of ice and wind and the myriad of life in the water above. The record of what happened in this water above is filed away in the mud and clay and ooze below. The rocks and pebbles and sand brought by ice, the clay and mud brought by rivers and ocean currents, the skeletons of marine organisms which lived and died and evolved into various forms throughout the ages constitute this record. . . . In addition to these records of life and its many changes there exists a chemical and a physical record. Oxidation and reduction and the nature of the dissolved matter in the water have all left the record of their changes in the bottom, and the nature and size of the minerals and rock fragments bear evidence of the direction and strength of former ocean currents, the movements of ice and the depths of the ocean in the past.

Heretofore, the samples obtained from the deep ocean bottom have been a mere handful of material taken from the very surface of the bottom. These samples give information of present condition only and reveal nothing of past events, so that although the historical record has been known to exist we have been able to see only the top page.

On land the geologist studies the exposed rock strata, but a study of material lying beneath miles of water is enormously more difficult. If, however, we could bring up a vertical section of several feet of this bottom, in its original, undisturbed condition, we might read the history of oceanic events as the geologist deciphers the record in the rocks.

Those remarks sum up quite well the advances that research in marine geology has brought to our knowledge of geological processes upon the earth.

By that time Piggot had become intrigued by the observation that there was not sufficient radium in ocean water in proportion to the uranium content, and that there was too much radium in ocean sediments in comparison with their uranium content. This was the insight that led to the ocean sediment dating experiments in a series of papers that appeared between 1939 and 1942 dealing with the ocean-bottom studies. They were written in collaboration with W. D. Urry, who had constructed an improved apparatus for Rn determination. For their first detailed study they selected a 2.85-meter core from east of Nova Scotia off the

Newfoundland Banks that was a member of the series of cores Piggot had taken in the North Atlantic traverse described above. They chose that particular core because of its very uniform chemical and lithologic composition. The results must have been something of a disappointment because they found only a small variation in radium content with depth that did not permit determination of a sedimentation rate or dating of the core. They concluded that deposition was too rapid to cover enough of an interval to allow detectable radium decay at this site. Piggot speculated that the Labrador Current caused a high sedimentation rate in the area. More positive results were to follow in future studies.

The results of the papers Piggot and Urry published between 1939 and 1941 were summarized and evaluated in 1942 in a landmark paper in *Bulletin of the Geological Society of America*, which reported radium results on cores from the North Atlantic Ocean, the Caribbean Basin, and Pacific Ocean red clay. Depending upon core length and sedimentation rates the cores spanned periods of time reaching back 10,000 to 300,000 years.

A substantial portion of the paper dealt with correlating their Th disequilibrium dates with the geological and foraminiferal observations of Bramlette, Bradley, Cushman, and Henbest on the North Atlantic cores in order to date glacial epochs as known on the continents. They used the foraminifera data of Cushman and Henbest, which gave qualitative measures of water temperatures, or zones of glacial marine deposits from Bramlette and Bradley to define epochs of glaciation. The best results from the North Atlantic Ocean were obtained from a three-meter core collected about halfway between the Mid-Atlantic Ridge and Newfoundland, which contained a record of the past 73,000 years. Progressing down the core, foraminifera abundances indicated cooling of the water occurred 12,800 years ago, with glacial debris deposits occurring at about the same depth. (Piggot and Urry used a value of 82,000 years for the half-life of ^{230}Th, whereas we now use 75,400 years, which reduces the age to about 11,800 years.[3] This value correlates quite well with the ending of the Wisconsin (Wurm) glacial epoch as presently known. The core next recorded a rise in water temperature between about 61,000 and 70,000 years, which correlates approximately with the beginning of the Wisconsin glacial epoch. Two other cores recording only 12,000 and 24,000 years of history, respectively, yielded results generally consistent with the 72,000-year core over the periods of time they spanned. Piggot and Urry showed that the ocean core results compared reasonably well with data from the land masses as then known (e.g., advance and retreat of Alpine glaciers and studies of soils in the midwestern United States), fulfilling the expectations Piggot had expressed when he first started work on ocean sediments.

They further showed that dates from a core in the Cayman Trough of the Caribbean Sea yielded results in close agreement with the North Atlantic data. The core consisted of Globigerina ooze and covered a time span of 300,000 years. The record of the Wisconsin glacial epoch could be recognized in that core with dates that closely matched those found in the North Atlantic core. Their temperature scale, provided by Cushman, for some reason did not serve to distinguish between the Illinois (Riss) and Kansan (Mindel) glacial epochs, but indicated continuous glaciation before about 110,000 years ago.

Given the amount of data and state of knowledge available then, Piggot and Urry could not be very dogmatic about interpretation of those results, but their work outlined the principles and methodology for ocean sediment dating that opened up the field for future investigation. Although we now have much better information due to the ability to measure water temperatures from ^{18}O data and improved techniques for disequilibrium dating, their results still fit quite well with present knowledge of sedimentation rates in the ocean basins and the dating of glacial epochs from those data.

Finally the paper reviewed sedimentation rates for various environments. The rates in the North Atlantic Ocean varied by nearly a factor of ten, depending upon the location of the cores. Off the Newfoundland Bank the sedimentation rate varied between 10 and 60 cm per 1,000 years over the past 12,000 years; in the basin between Newfoundland and the Mid-Atlantic Ridge it varied between 1 and 6 cm per 1,000 years according to their results. The Pacific Ocean red clay yielded lower rates between 0.2 and 1 cm per 1,000 years. They also found that the surficial clay core surface contained ^{226}Ra in excess of the amount required for secular equilibrium with the ^{230}Th parent, whereas calcareous sediments and oozes from the Atlantic Ocean normally contained less ^{226}Ra than required for secular equilibrium.

According to their data sedimentation rates in the Caribbean Basin increased starting 7,000 years ago compared to rates during the Wisconsin glacial epoch. They postulated that the circa 3,000 year delay after the end of the epoch represented the time required to reestablish organic life after a long period of cold surface waters. The present rate was shown to be five to ten times that during Wisconsin glaciation. No such change was noted in their Pacific Ocean clay or in other Pacific and Antarctic red clay data later reported by Urry.[3] The data as a whole, although revealing variable sedimentation rates, generally indicated that depo sition rates for red clays were about five to ten times lower than those for the oozes of the Caribbean Basin.

These results answered the questions Piggot had asked at the start of the experiments—showing that the high concentrations of Ra in ocean sediments were a surface effect and that ^{230}Th abundances in marine sediments were higher than those required for secular equilibrium with ^{238}U. With the aid of the coring device it was possible to use that relationship to date sediment horizons and thereby ascertain sedimentation rates. Furthermore, his belief that information of use in deciphering land-based geologic history could be read in the marine sedimentary record was confirmed.

On a personal note, even though I never met Charles Piggot, my admiration for his radium studies dates back to the days of my doctoral dissertation project around 1949. The goal of that project was to work out methods for accurate analysis of microgram quantities of uranium for application to meteorite studies and geochronology. In reviewing the various available methods, I read the radium papers of Piggot and Urry. Although I finally selected stable isotope dilution (a novel technique at that time!) as the best approach, the sediment disequilibrium dating studies described in their papers made a lasting impression on me.

World War II changed Piggot's activities from radium studies to service-related matters. Shortly before the Pearl Harbor attack the navy bureau of ordnance requested his service from the Carnegie Institution and he was granted a leave of absence. He was assigned to develop procedures for the recovery and disassembly of magnetic and other mines, which were then causing much trouble to Allied shipping. Eventually he worked with 400 trained men who volunteered for that hazardous duty. They worked on the recovery of submarine mines and disarming of bombs, booby traps, and other live ammunition. He established two train ing schools—one for mine disposal and one for research into methods and procedures for stripping dangerous objects. With help from Van de Graaf at MIT they developed a two-million-volt X-ray machine with which they could examine the interior of dangerous objects and decide how best to dissect them. He was awarded the Order of the British Empire for his work in mine disposal.

In March 1946 he was assigned to the staff of Task Force I for Operation Crossroads at Bikini and witnessed two atomic bomb explosions there. His assignment was to devise means for evaluating damage to ordnance equipment and to provide underwater photography using men and equipment from his mine disposal experience. He received a Bronze Star for that service. Upon returning to the United States he became executive director of the Committee for the Geophysical Sciences of the Research and Development Board of the U.S. Navy, where he oversaw ten panels in various fields.

In 1950 he went to London for two years as the first foreign service reserve officer assigned by the State Department to promote cooperation between the scientists and governments of the two countries. While there he served on the Ramsey Fellowship Board as well.

In 1952 he returned to the United States to work at Yale as a consultant and assistant supervisor on a navy project there. In 1955 he surveyed and appraised eighteen scientific institutions in India for the National Academy of Sciences.

Piggot was elected to the National Academy of Sciences in 1946. He was a fellow of the Geological Society of America and the American Geographical Society and a member of the American Chemical Society, the Washington Academy of Sciences, and the Royal Institution of Great Britain. As mentioned above, the Bronze Star and Order of the British Empire were among his awards.

The Piggots had two children, son Deboorne and daughter Anne Marguerite, now Mrs. Robert W. Black.

AN AUTOBIOGRAPHICAL SKETCH prepared for the National Academy of Sciences provided information on Piggot's family, childhood, education, and career activities. Records from the Geophysical Laboratory and an obituary in *The Washington Post*, dated July 9, 1973, yielded additional information covering some of his professional activities, publications, and awards. I am indebted to Gordon L. Davis, former staff member of the Geophysical Laboratory, for locating those sources of information.

NOTES

1. F. W. Aston. *Nature* 120:224 (1927).
2. F. W. Aston. *Nature* 123:313 (1929).
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HENRY PRIMAKOFF

February 12, 1914–July 25, 1983

BY S. P. ROSEN

ENRY PRIMAKOFF, THE FIRST Donner Professor of Physics at the University of Pennsylvania, was a theoretical physicist well known for his contributions to condensed matter physics and to high energy physics. His name is associated with spin waves in ferromagnetism, with the photo-production method for measuring the short lifetimes of neutral mesons, and with an underwater shock wave. He became a leading authority on weak interaction phenomena in nuclei, such as double beta decay, muon capture, and neutrino scattering. He was an outstanding teacher and had a unique influence upon all of his students.

EARLY YEARS

Henry Primakoff was born in Odessa, Russia, on February 12, 1914, and died in Philadelphia on July 25, 1983. In life he had come a long way, from an early childhood in a city beset by war and revolution, through an arduous and often dangerous journey from Russia into Romania and across more than half of Europe, from Bremen to the lower Bronx, and ultimately to the City of Brotherly Love where a long battle with cancer awaited him. In sickness and in health, Henry bore himself with great courage and zest for life and his final years were filled with as much involvement in the world around him as at any other period of his life. He died peacefully in the midst of his family.

Through his mother, Henry was descended from a large, assimilated Jewish family of merchants who had lived in Odessa for several generations. Through his father, Henry came from a Greek-Orthodox family of wealth and prestige. His paternal grandfather married a Jewish woman and was banished from the family estate; years later his father did the same. After the Russian Revolution, one granduncle rose to become a general in the Red Army, but he was executed by Stalin in the famous 1937 purge of the army. Khrushchev subsequently rehabilitated General Primakoff, and a statue in his honor is said to stand in Kiev.

Henry's father was born in Kiev, studied medicine, and graduated as a doctor in 1911. His mother, a strikingly beautiful woman, came to Kiev to study pharmacy after graduating from the gymnasium in Odessa, and it was through the medical connection that his parents met. During the First World War his father became an army doctor and was wounded while operating on soldiers behind the front lines. He joined his wife and young son in Odessa at the end of the war, but died a few months later in 1919. At his funeral the Red flag was flown and the Internationale sung.

About two years later, Henry's mother and her parents decided to leave Russia and join an uncle who had settled in New York. This required escaping across the nearest border, the Prut River, into Romania, trudging for long night hours through woods, and hiding by day in remote farmhouses. Eventually they found a haven on the farm of some relatives about five hours by train from Bucharest. Henry was instructed not to talk to his mother when they went into town, because it was too dangerous to speak Russian in that part of Romania at that time. He and his family received travel documents from the embassy of the Kerensky government in Bucharest and set out on the long train journey through war-torn Europe to Bremen, and thence on the steamship "der Flieger" to New York, where they finally settled in 1922.

Once established in the lower Bronx, Henry made rapid progress in his new language and in school, although he had some startup problems with four-letter words. They were used so frequently by his classmates that he took them to be a normal form of greeting. One day a much bigger kid approached him with a friendly "Hi!," to which Henry gave a four-letter response. The kid recoiled in astonishment and prepared to slug this insolent, foul-mouthed immigrant; but fortunately for Henry, some other kids realized the problem and saved him from a beating in the nick of time. From that time on he was much more circumspect in his usage of four-letter language.

In high school Henry took an active interest in politics and journalism, becoming editor of the school paper one year and president of his class another. He read widely and had an excellent all-around academic record, ending up as second best student in the entire school. He won scholarships to Columbia and Harvard and opted for the former despite advice from his granduncle to the contrary. In the fall of 1931 Henry began his freshman year at Columbia.

His interests in college were sufficiently broad that he did not become really serious about science until the beginning of his junior year. On entering Columbia he was quickly disabused of early ideas for a career in journalism by his experiences on the college paper, and he gave some thought to the study of literature or philosophy. The interest in philosophy stood Henry in good stead several years later when he interviewed for the Harvard Society of Fellows and was able to hold a lively conversation with the famous philosopher A. N. Whitehead for more than two hours. The fellowship, however, went to John Bardeen, a future Nobel laureate.

By the middle of his junior year Henry was concentrating more and more upon physics. He and five or six like-minded students formed an informal club to study special relativity from the short volume by Tolman, which had just been published. The club went on to distinguish itself in the world of physics, its members including Norman Ramsey, Nobel laureate; Herbert Anderson, student of Fermi and co-discoverer of the (3,3) resonance in meson-nucleon scattering; Robert Marshak, co-inventor of the universal (V-A) form of weak interactions and founder of the Rochester conferences in high energy physics; and Arthur Kantrowitz, former director of the Avco Everett Research Laboratory.

Henry spent his senior year at Columbia taking graduate courses and in one of them, a laboratory course, he met Mildred Cohn, who was to become his wife and a distinguished chemist known for the application of nuclear magnetic resonance to biochemistry. During this year the club members became aware of the need for graduate school if they were to become professional physicists; Henry applied to Princeton and was accepted.

In those days there was little financial support for graduate studies. One had to be prepared to pay tuition (about $100) and support oneself for at least the first year. With help from his family and money either saved from his undergraduate scholarships or earned from various odd jobs Henry managed to stay at Princeton for a year. New York University then offered him a fellowship and he went back to New York to complete his Ph.D. He received his degree in 1938 and married Mildred in May of that year.

Despite the bleak economic climate of the times, Henry and Mildred both ended up with jobs in New York, Mildred in the Biochemistry Department of Cornell Medical School and Henry first at Brooklyn Polytechnic Institute and then at Queens College. After Pearl Harbor he began to work on a navy project concerned with sonar and submarines. Oppenheimer approached him to join the Manhattan Project, but he refused on the grounds that he wanted to work on projects for the present war and not the next one. He did not believe that an atomic bomb could be built in a reasonable time, and was greatly surprised by the news of Hiroshima.

When the war ended Henry accepted a joint physics and mathematics appointment at NYU. Richard Courant, founder of the institute that bears his name, wanted to have his mathematicians interact with a physicist and he chose Henry for the job. A year later Arthur Hughes and Eugene Feenberg persuaded Henry to join the physics faculty of Washington University in St. Louis. Mildred eventually arranged to work in Carl Cori's department at the medical school, and so in 1946 they began a new chapter in their lives in St. Louis, Missouri.

PHYSICS RESEARCH

Henry wrote his first paper while still a graduate student on “second and higher order processes in the neutrinoelectron theory.” In it he and co-author M. H. Johnson calculated the forces between neutrons and protons due to the exchange of virtual neutrino-electron pairs in the new Fermi theory of beta decay. It was a prophetic choice of topic because Henry came to devote a great deal of effort in the postwar era to weak interactions in nuclei.

Possibly the best and most significant paper that Henry wrote was also started while he was a graduate student. He and fellow student Ted Holstein were studying the field dependence of the intrinsic magnetization of a ferromagnet at low temperatures when they had the ingenious idea of expressing the spin operators that appear in the Heisen berg exchange interaction model in terms of boson creation and annihilation operators. With appropriate approximations, which turned out to be equivalent to approximations used by Bloch and by Moeller in a very different and less complete treatment of the problem, they were able to diagonalize the Hamiltonian, including magnetic interactions as well as exchange and dipole-dipole interactions.

The essential idea of this approach is that, while most of the atomic magnetic moments in the ferromagnet will line up with the external magnetic field, there will always be a few that, because of temperature agitation, deviate from complete alignment. By means of the boson transformation, Holstein and Primakoff showed that the spin deviations were not localized on a particular atom, but propagated through the crystal in spin waves. Spin waves, originally proposed by F. Bloch, are regarded as the principal modes of excitation of ferromagnets, and in recent years they have even entered nuclear theory.

Although this paper has become a classic and was reprinted as such in a recent Japanese collection, its importance was not recognized until several years after the end of the Second World War. The first reference to it that I have been able to find is by Akhieser in the *Journal of Physics* of the U.S.S.R. in 1946. In this country Van Vleck published a survey of the theory of ferromagnetism based on a lecture given in Paris in 1939, which was "amplified considerably," in *Reviews of Modern Physics* in 1945 without referring to Holstein and Primakoff. By 1958, in a review of spin waves with Van Kranendonk in the same journal, he was referring to their work as "the conventional approach." The story of how this change came about, at least insofar as this author can trace it, is quite interesting.

It began in 1946 with the discovery by J. H. E. Griffiths of Oxford University of ferromagnetic resonance effects of unusually large frequency compared with the Larmor precession of electron spin in a magnetic field. Charles Kittel, then at MIT, gave a classical interpretation of the anomaly in 1947 and a year later D. Polder, of Bristol, derived the Kittel formula using quantum mechanics. In his derivation he used the method of Holstein and Primakoff for describing the quantum mechanical states of a ferromagnet and the corresponding energy eigenvalues. Subsequently, Luttinger and Kittel used an "ingenious but somewhat devious method" directly to calculate the ferromagnetic resonance frequencies from one term in the appropriate Hamiltonian. Once the relevance of quantum mechanics to ferromagnetism became firmly established the paper of Holstein and Primakoff received its due recognition.

It is interesting to note that, even though their work played a seminal role in theories of ferromagnetism and anti-ferromagnetism in the 1950s and even though the Holstein-Primakoff transformation is famous to this day, neither Holstein nor Henry ever worked on this subject again, and as far as I can ascertain, they never applied their methods to other problems.

Although Henry never worked on the Manhattan Project itself, he did do some research relevant to the Bikini underwater test that led to the discovery of what some authors have called the Primakoff wave. The problem with the test was whether it would set off a severe tidal wave that would do serious damage in the Pacific Basin. Applying methods used by G. I. Taylor for shock waves in air, Henry found a simple, exact solution for the shock wave problem in water at high energy and showed that the properties of this wave, including its height, are all determined solely by its energy. The result was never published in the open literature, but it is described and attributed to Henry by Courant and

Freiderichs on page 424 of their book on "Supersonic Flow and Shock Waves."

In his early years at Washington University Henry wrote two papers with Eugene Feenberg, one on collapsed nuclei and the other on the interaction of cosmic-ray primaries with starlight and sunlight. The former anticipated later ideas of T. D. Lee and G. C. Wick on super-dense matter; and the latter showed that cosmic ray primaries should consist mainly of protons because energetic electrons would undergo too much scattering from photons in intergalactic space (through the inverse Compton effect) to reach the vicinity of the Earth. Henry also wrote papers on muon decay, muon capture, and hypernuclei, which were of considerable interest to cosmic ray physicists, the intellectual forerunners of present-day high energy physicists.

In a seminal paper of this era written with S. DeBenedetti, C. E. Cowan, and W. R. Konneker, Henry derived the basic formulae for the angular distribution of photons from positron annihilation in solids; it is still quoted today. He also wrote his initial papers on the Primakoff effect and on double beta decay during this period.

While at Washington University Henry would receive offers from other institutions from time to time. In almost all cases, the letters offering him a position would end with words to the effect, "We can also find a job for your wife." There was one case, however, that involved a reversal of roles between Henry and Mildred. In 1948 Johns Hopkins made an offer to Mildred and closed it with the statement, "We can also find a job for your husband." She did not accept it, and they remained in St. Louis until 1959.

PERSONAL RECOLLECTIONS

I first met Henry Primakoff in the fall of 1955, when he was spending a sabbatical leave at the Clarendon Labora tory in Oxford, and I was a graduate student in search of a thesis topic. Henry had given a seminar on double beta decay and Roger Blin-Stoyle, my supervisor, suggested that I investigate some of the problems raised by Henry. This proved to be the beginning of an association that was to last for twenty-eight years.

Double beta decay is an extremely slow process, being in a certain sense a succession of two ordinary nuclear beta decays, but it is closely tied to important questions regarding the neutrino. If the neutrino has a mass, and if it is its own anti-particle, then it is possible for double beta decay to take place without the emission of neutrinos in the final state; the neutrino emitted in the first ordinary beta decay is reabsorbed in the second. With the advent of the standard model for particle physics, the study of neutrino properties has become an important avenue of exploration for the new physics.

In 1957 I became a research associate at Washington University in St. Louis and after crossing the Atlantic on the Queen Mary, I took the train from New York to St. Louis. Henry picked me up at the station, but locked his car with the keys inside. While we were waiting for the man from Triple A to do the old coat-hanger trick, Henry invited me to join him in writing a review article on double beta decay. I accepted without hesitation.

He had been asked to write the article the previous year and had already missed one deadline; he needed a collaborator to help him meet the next one. As it happened, we missed that deadline, too, by a few weeks and our article was not published until 1959. It formed a bridge between the original work of Maria Goeppert-Mayer, Majorana, Racah, and Furry in the 1930s and the important developments brought about by the discovery of parity nonconservation and the two-component neutrino in the second half of the

1950s, and it remained a standard reference for many years. It also provided a useful starting point for the modern developments in double beta decay of the 1980s.

My appointment at Washington University ended in 1959, and I went to work for the Midwestern Universities Research Association in Madison, Wisconsin. A year later Henry accepted an appointment as Donner Professor of Physics at the University of Pennsylvania; it was to be the final move of his career.

For the next ten years Henry and I kept in close touch with one another, but our interests went in different directions. Henry investigated the fundamental properties of the weak interaction in various settings, while I began work on flavor symmetries of elementary particles. With the introduction of the universal V-A interaction for all four-fermion weak interactions in 1958, Henry had realized that the rate for muon capture in nuclei, the second leg in the Puppi triangle, would be sensitive to the hyperfine splitting of the parent atom. Jeremy Bernstein, T. D. Lee, and C. N. Yang had independently discovered the same effect and they invited Henry to join them in publishing the result. He went on to develop an extensive theory of muon capture in nuclei, which culminated in the elementary particle treatment put forward by C. W. Kim and himself and relied upon the use of the Goldberger-Treiman relation in complex nuclei. At the same time Henry examined the behavior of elementary particles themselves; with P. Dennery and J. Dreitlein he studied rare decays of the muon, the relationship between the decays of charged and neutral Sigma hyperons, and semi-leptonic decays of K-mesons. With Ephraim Fischbach and other students he investigated parity-violating nuclear forces, a topic that reached back to his very first paper.

In the overlap between Washington University and Penn sylvania Henry wrote two seminal papers with Arden Sher on the approach to equilibrium in quantal systems. Max Dresden has described these papers as a "brilliant exposition" that clarified a field in which much confusion had existed.

Another idea that Henry extended in this era had its origins in a paper he wrote in 1951 on the photo-production of the neutral pion in the electric field of the nucleus. The essential point of this paper, which has come to be known as the Primakoff effect, is that, under certain welldefined kinematic conditions, the photo-production of neutral pions is controlled by exactly the same interaction as the decay of the pion into two photons. This means that the lifetime of the meson, which is difficult to measure directly, can be extracted from measurements of photo-production, an easier task. In collaboration with C. M. Andersen and A. Halprin he extended this approach to the lifetime of the newly discovered eta meson, a pseudoscalar meson like the neutral pion, and to the production of vector mesons. The Primakoff effect has proved to be a most effective method of measuring neutral meson lifetimes and it is now the standard one.

In 1969 Henry invited me to work once more with him on double beta decay. He had been intrigued by a clever argument by Pontecorvo indicating some tentative evidence for the occurrence of no-neutrino double beta decay. One motivation for searching for the decay mode without neutrinos is the identity of the neutrino and its anti-particle, or equivalently the question of a conservation law for leptons analogous to that for electric charge. There is considerable empirical evidence for such a law, but the most sensitive test occurs in double beta decay, partly because of the energies available to the exchanged virtual neutrino and partly because of the helicity suppression imposed by the two-component neutrino. The lack of complete helicity suppression can be parameterized either by a neutrino mass or by a small admixture of a leptonic current with opposite helicity from the dominant left-handed current. We chose the opposite helicity current on the grounds that the then existing limit on the neutrino mass was much too small to yield an effect of the magnitude anticipated by Pontecorvo, and by ourselves. In recent years the nuclear physics argument proposed by Pontecorvo, simple and elegant though it is, has fallen out of favor, and the modern particle physics point of view emphasizes neutrino mass rather than right-handed currents as the basic helicity parameter.

We did eventually write a paper about neutrino mass and double beta decay, but we dealt with heavy neutrinos instead of light ones. In 1976 Gell-Mann, Ramond, and Slansky proposed the seesaw mechanism for neutrino masses; they argued that the very light, left-handed neutrinos participating in weak interactions were Majorana particles and should have very heavy, right-handed partners. Arthur Halprin realized that heavy Majorana neutrinos could also give rise to no-neutrino double beta decay, and in collaboration with him and P. Minkowski, we showed that the existing limits on the process gave rise to a lower bound on the masses of such heavy neutrinos of several times the proton mass. This is entirely consistent with the seesaw model.

Our last paper together, like the first, was a review article. Henry had been asked to review the subject of baryon number and lepton number conservation laws for the *Annual Reviews of Nuclear and Particle Science,* and again he invited me to join him. “You realize that you may have to finish it by yourself,” he said. When I put down the phone I cried; I knew of his illness, but the stark confrontation with its severity overwhelmed me.

Fortunately the worst did not happen, and we finished the paper together. We met frequently for the next year, worked hard on the paper, and this time we beat the deadline. Most of our meetings were in Philadelphia, but the last one was in West Lafayette. Henry spent a week with me putting the finishing touches on the paper and he seemed to thrive on a demanding regimen of early mornings and late nights even though one leg was impaired by his illness.

Our review summarized all that we had learned over the years about particle number conservation laws and how any breakdowns of these laws would become physically manifest. Up to now none of these manifestations has been detected, but there is one that does have a chance of being definitively observed in the near future, namely neutrino oscillations. Pontecorvo proposed oscillations as the solution to the solar neutrino problem in 1968 and Henry was excited by the idea almost as soon as he read about it. He told us all about oscillations and encouraged us to pay serious attention to the possibility that they might actually play an important role in the physical world. It is sad that Henry did not live to see the most recent developments in the field, both the matter enhancement theory of Mikheyev, Smirnov, and Wolfenstein, and the beautiful experiments on solar neutrino-electron scattering, which were originated by his colleague A. K. Mann, and on the gallium reaction. He would have been delighted.

Henry was a master of the old school of theoretical physics. Not only did he make major contributions to several diverse fields of physics, but his interests and attitude towards physics brought him into close contact with experiment. He did not like to stray too far from experiment and he rarely speculated about things that were beyond experimental reach. Nevertheless he had his own share of clever ideas, and he greatly admired the ingenious ideas of other people. Bruno Pontecorvo was one such person. Henry im mensely admired him for his inventiveness and thoroughly enjoyed the clever ideas he put forward. I do not know whether the two men ever met, but I am certain that any meeting between them would have sparkled with wit and warm admiration.

Given Henry's liking for scintillating ideas, which might be categorized as belonging to the elegance of Paris and Rome, it is somewhat surprising that his writing style, at least in physics, tended toward the Germanic. He seemed compelled to pile on top of one another all the qualifications required for a particular word or phrase; he could not bear to postpone a qualification for one or two sentences for fear that his statement might appear ambiguous or inaccurate. Likewise with his notation. Henry never omitted a single subscript, superscript, or any other qualification. He put down everything! His notes are so unusual that they have been used to decorate the covers of conference proceedings.

Discussing physics with Henry was always a pleasure. He was so full of knowledge and ideas that we would go on for hours without noticing it. But conversations with him were never completely serious. He had a lively and whimsical sense of humor, and he always enjoyed a fanciful diversion. One always came away from them encouraged and happy to go on.

I would like to close this memoir with the remarks I made at a dinner in honor of Henry nineteen months before he died.

REMARKS IN HONOR OF HENRY PRIMAKOFF DECEMBER 18, 1981 AT PHILADELPHIA

I have been asked to speak tonight on the topic of “working with Henry.” It will be a great pleasure to do so, but I would like to begin by drawing upon the vast reservoir of “Henry stories.” Everyone has his own particular favorites, and so here are a few of mine.

The first one, which may be apocryphal, is about a seminar at Queens College years ago. When the talk was over, Henry walked out of the lecture and took a coat from the racks by the door. He put it on, thrust his hands in the pockets, and wandered off. Within a few minutes Henry returned, the skirts of the coat trailing on the floor. A rather tall chap was now poking around the racks, obviously looking for a missing coat. Henry came up to him and, drawing his hands out of the pockets, said "Excuse me please, but are these your gloves?"

Another story is told of a dinner at Washington University, much more formal than this one, honoring some eminent man of academe. Henry, seated at the head table, must have found the after-dinner speeches a trifle tedious, because he soon fell asleep. Now in those days Henry was a smoker, and on this particular occasion he had neglected to put out his cigarette. Thus, when the eminent speaker suddenly sensed in the middle of his speech that he had completely lost the attention of the audience, he looked around to see Henry asleep with his hair on fire!

Sleep has always been important to Henry. For many years he has practiced the fine art of falling asleep during the body of a seminar, but waking up just in time to ask the most penetrating question. Even when his question revealed his previously dormant state, he rarely failed to put the poor speaker on the defensive. Perhaps Henry's greatest tribute to this high art is his definition of insomnia: "When you can't fall asleep in a seminar!"

Now let me turn to the principal subject of these remarks, "working with Henry." You have all seen this "everywhere dense" page of Henry's notes reproduced on the cover of the program. Well, I can summarize the job of a collaborator, and I speak from more than twenty years experience, by saying that he has to translate every mark on this page into words and sentences that others can comprehend. Bob Marshak observed in his talk this afternoon that, whereas most people tend to put down the first factors in an expression explicitly and indicate the rest by dots, Henry never likes to use dots. He puts down everything. His symbols are always fully clothed with all their superscripts and subscripts, parentheses and sundry other brackets. As a result, his papers make a unique visual impact upon the reader and you can readily spot one of them as you flip through the pages of the *Physical Review.*

Henry's writing is really no different from his symbols. Whenever a word or a phrase requires several qualifications, he feels impelled to get them in all at once; and so he piles them in before, during, and after the phrase or the word. He seems not to want to break up one complicated sentence into several simpler ones for fear that the reader may lose track of what qualifies what.

I have been fortunate to work with Henry on very rare and long-lived processes, on whose time scale twenty years is but the blinking of an eye. So we have been able to come back to them every few years and still find new and interesting things to say. When our first article appeared, in 1959, a reviewer wrote in the English *Journal of Physics* that he could not understand how two people could write so much about a process that no one had ever seen! Well, rather than be deterred by that remark, we have gone on from slower to even slower processes, and in our latest work we write about one process, proton decay, which would make our original process, double beta decay, seem like a horse race.

An occasion like this brings back memories of my earliest meeting with Henry. He had come to Oxford on a sabbatical leave, and I was a young graduate student just finished with prelims. How fortunate I have been that a series of happy accidents brought us together! That Henry would come when he did, and Roger Blin-Stoyle should suggest to me, instead of any one of four or five students, that I do a thesis on a subject of interest to Henry. That Henry should have a job when I needed one! So many turnings could have been missed, so many roads not taken! But, happily for me, nothing did go amiss.

I remember being fascinated by Henry's voice and gestures when he lectured. He has that wonderful, melodic accent, the likes of which I had never heard before, and I decided that it had to come from the South—the home, for many Englishmen like myself, of all things gracious and exotic. It was not until several years later that I learned that the accent had really come from Russia by way of New York.

Henry's gestures were to his lectures and to his conversation as punctuation marks are to the written word. His right hand would reach out and carefully place a full stop in the air; his whole body would reach out as though it were an exclamation mark or curl around to form a question mark. Whenever you had a conversation with Henry you would find by the end of it that he was either wrapped around the chair or wrapped around you.

As his students have already testified tonight Henry is a very kind and considerate man. Moreover, he does his good deeds in a quiet and unobtrusive way. In my own case he was very helpful at a time when I thought seriously of leaving physics as a career. He led me gently back into the field, not by exhortation, but by discussing the problems in physics that had brought us together a decade earlier. I shall always be grateful for his friendship and his wisdom.

We all know that, in the last few years, Henry has been afflicted with a grave illness. Indeed, all of us have suffered with him during this period of his life; but we cannot fail to admire the fortitude with which he has borne his affliction, the grace and nobility with which he has faced it. If there is amongst us a true prince of the spirit, then surely that man must be Henry Efromovitch Primakoff.

Thank you, Henry.

I am very glad that I had the chance to say these things directly to Henry. The pain of his passing, though it will never cease, does diminish with time and those who knew him well are left with many happy memories to enjoy.

I THANK MILDRED COHN Primakoff for encouraging me to write this memoir and for allowing me to view the transcript of a memoir dictated by Henry in his last years. I am grateful to Norman Ramsey and Robert Marshak for sharing their recollections of the junior year club at Columbia with me, and to J. B. Keller and Cathleen Morawetz for telling me about the Primakoff wave. Max Dresden emphasized the seminal character of the work on statistical mechanics. P. W. Anderson and Charles Kittel provided me with valuable clues on the early history of the Holstein-Primakoff transformation. However, I take full responsibility for the interpretation of history presented here.

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Photo by Harris & Ewing, Washington, D.C.

J. FRANK SCHAIRER

April 13, 1904–September 26, 1970

BY H. S. YODER, JR.

FEW PEOPLE HAVE GIVEN so generously of their kind help and good cheer to so many as J. Frank Schairer. His multitude of friends reflect on his passing with sadness, yet they cannot help being warmed by the memories of his vigorous and happy life and grateful for his prodigious contributions to experimental mineralogy and petrology. Schairer's researches yielded an immense number of accurate determinations of the melting relations of the common rock-forming minerals at one atmosphere and in vacuo. To many colleagues the greater contribution was his simple and contagious philosophy of life that added zest and joy to all whose lives he touched.

John Frank Schairer was born in Rochester, New York, on April 13, 1904. His father, John George Schairer (1876-1965), was a master lithographer and later a farmer. His mother, Josephine Marie (née Frank) Schairer (1874-1939), taught school for eight years before her marriage in 1903. Frank, the name his parents preferred, was the first born and he was followed by six girls.

EXCEPTIONAL SCHOLASTIC RECORD

When Frank was five years old he entered the kindergarten of Rochester Public School No. 32 with his sister Helen.

They also entered first grade together at Immaculate Conception Parochial School. “He was a bright student, but not bookish,” and skipped third grade, according to Helen in 1971. His exceptional scholastic record in grammar school (eight-year program) led to the winning of a Gold Medal. As was the custom in families of modest means, Frank had a magazine route and delivered the *Ladies Home Journal* and the *Saturday Evening Post* in the neighborhood. He fished with his chums in the tributaries of the Genesee River, which flows through Rochester.

In 1917 Frank entered Rochester Cathedral High School (four-year program) where his potential was immediately recognized. Sister Pauline (Smyth) and Father J. E. Grady especially influenced his subsequent development in the field of science. During his first year in high school the family moved from Rochester to a farm near the town of Greece, some 5 miles northwest of Rochester, because of his father's occupational health problem and economic straits resulting from a long strike. The new farm life demanded much of Frank's spare time, yet he maintained honor grades and was a member of the debating team. His mother, always the teacher, imparted her own interest in nature, especially botany, to the family during hikes through the local woods. A small chemistry laboratory was built by Frank in the attic where flares were produced for the neighborhood fireworks display on the Fourth of July. The parental concern and control of the use of the laboratory are well documented by his sisters. There was a great sense of family unity and love, and most of the activities centered on the home. Instillment of good moral values coupled with discipline wisely administered and a sound basic education appeared to be the predominant goals of his parents. All the children were afforded some musical training and Frank learned to play the piano. For those who have heard him sing at church, campfires, or at occasions when the National Anthem was sung, the musical training would seem to have been of no avail.

At the suggestion of his mentors at Rochester Cathedral High School, Frank entered competitive examination for a scholarship at Yale University offered by the alumni association of Rochester. A tie resulted and, after due consideration, the alumni decided to offer both men a scholarship. A loan from an aunt, Miss Mary Schairer, provided some supplementary funds for him to accept the opportunity. Summer employment and waiting tables at Johnson's Eating House provided the remaining money for his support.

HOOKED FOR LIFE

His first interest in mineralogy grew out of a field trip to Old Gillette quarry at Haddam Neck, Connecticut, during a weekend outing spent with a classmate's father, George N. Norton, the local country doctor who had taken geology under James Dwight Dana. Greatly fascinated by the beauty of the well-crystallized minerals in the pegmatite with its highly varied and exotic chemistry, Schairer was hooked on the chemistry of rocks and minerals for life.

At the end of his freshman year in 1922 at the Sheffield Scientific School, Frank won the New York Yale Club prizes in chemistry II and German I. In his sophomore year he won the Samuel Lewis Penfield prize for excellence in mineralogy. At that time the laboratory assistants in the mineralogy course were William W. Rubey (*NAS Biographical Memoirs*, volume 49) and James Gilluly (*NAS Biographical Memoirs,* volume 56), both of whom became prominent members of the U.S. Geological Survey and leaders in the National Academy of Sciences. On the basis of that single course in mineralogy and many field excursions, Schairer helped or ganize the Yale Mineralogical Society and was elected its first president on October 5, 1923.

With his principal focus on chemistry he found summer employment in the relatively new research laboratories of the Eastman Kodak Company in the synthetic chemicals department under the direction of the highly respected Hans Clarke. Frank's assignment was the synthesis of sodium thioglycolate, a technique that was tricky because of the required purity of chemicals (Shepler, 1971). He was later dismayed to learn that all his efforts were spent merely to produce a chemical for modifying wool fibers and especially for use in the straightening or cold waving of human hair. His sisters recall his returning home at the end of the summer with hands deeply stained chestnut brown!

Schairer's senior year was predominantly devoted to chemistry. He saved his lunch money to buy the three-volume work by H. W. Bakhuis Roozeboom on the principles of heterogeneous phase equilibria to supplement his interests in thermodynamics and phase chemistry. He graduated magna cum laude in the class of 1925 after election to membership in Alpha Chi Sigma (chemistry) and Sigma Xi (scientific research). Time was also found to pursue his interests in organic evolution and advanced petrology. On graduation he had five papers in mineralogy published or in press!

STRADDLING THE FENCE

Although registered for a doctorate in chemistry, Schairer made the unusual request to take a master of science degree in mineralogy. The chairman of the Department of Geological Sciences at Yale was then Charles H. Warren, the same man who had earlier, while at the Massachusetts Institute of Technology, launched Norman L. Bowen on a career of experimental petrology involving phase equilibria at the Geophysical Laboratory in Washington, D.C. With the strong support of the faculty of the Department of Geological Sciences, the dean of the graduate school agreed to the request. During the 1925-26 period, Schairer served as laboratory assistant in the Sterling Chemical Laboratory and as president of the Chi chapter of Alpha Chi Sigma. His essay for the M.S. degree was on "The mineralogy and paragenesis of the pegmatite at Collins Hill, Portland, Connecticut." It was given a superior rating and was described as representing "an exceptional amount of original work for an M.S. degree." It is of note that his principal professors included John Johnston, formerly a chemist specializing in high-pressure phenomena in minerals at the Geophysical Laboratory; W. E. Ford, who was revising E. S. Dana's *Textbook of Mineralogy* (fourth edition); H. W. Foote, who made substantial contributions to the chemistry of minerals and was a physical chemist trained by S. L. Penfield; and Adolph Knopf, internationally renown igneous petrologist.

THE ENTHUSIAST

In April 1926 Professor Charles H. Warren wrote to A. L. Day, director of the Geophysical Laboratory in Washington, D.C., recalling that Norman L. Bowen had been a predoctoral fellow there and suggesting a "somewhat similar program" for another graduate student, J. F Schairer. He is "not only a man of unusual ability, but of really tremendous energy, and his enthusiasm for research work exceeds that of almost anyone I have known for a long time. We have never found it possible here to give him so much work to do that he was not habitually branching off into some piece of research work." The interview that followed shortly was successful, but apparently Warren had difficulty in obtaining the necessary financial support. In the meantime, Schairer worked on the Na_2SO_4-NaCl-NaF-H_2O system under Harry

W. Foote in chemistry. The temperature range was 10-35°C so no special equipment was required. At the end of January 1927 another interview was arranged with Day; Schairer (1964) records his recollections of the discussion:

At that time Arthur L. Day, director of the Geophysical Laboratory, did not want any young men on fellowships. He said that they were only there for a year, took up time and space, and in one year only found out what they should have done if they only knew how. However, he agreed to see me and talk it over. He found that I had a tremendous interest in phase equilibrium and had been working at Yale on the quaternary system Na_2SO_4-NaF-NaCl-H_2O. He said he wouldn't take me on a fellowship but would give me a job. I was surprised and pointed out that I did not yet have my doctor's degree. He said, “You know that a doctor's degree is merely a certificate that you have had the proper training and I can see that you have had that.” So many students fail to recognize this truth and believe that if you have this magic degree you do not have to think or work the rest of your life and that the world owes you a living.

Schairer reported to work on September 1, 1927. His thesis was written at the Geophysical Laboratory and his comprehensive examinations in organic and physical chemistry were mailed to the laboratory and administered by the director. The Ph.D. degree was awarded in June 1928, and the thesis was published in two parts by the American Chemical Society (Foote and Schairer, 1930 a,b). The system investigated was pertinent to the mineral deposits of Searles Lake, California. One of the compounds discovered by Schairer in the synthetic system was $Na_2SO_4 \cdot Na(F,Cl)$. It was later discovered by W. F. Foshag (1931) in the sediments of Searles Lake and named schairerite. The brine that permeates the salt deposit is uniformly 23°C throughout the year, a temperature close to one of Schairer's isotherms. [The ratio of F to Cl in the natural compound has been determined by Brown and Pabst (1971) to be 6:1. Another similar compound with F:Cl equal to 4:1 is known as galeite, and the fluorine endmember, also synthesized by Schairer, has been found in the Kola Peninsula [Kogarko, 1961).]

Because of crowded conditions at the Geophysical Laboratory, Schairer had the good fortune to be assigned a desk in the office of N. L. Bowen, who no doubt had a very sympathetic understanding of Schairer's predoctoral status because of his own similar experience. At that moment Bowen was probably enjoying the most intellectually exciting time of his life. He was preparing his lecture series at Princeton University, given in the spring of 1927, for publication as “The Evolution of the Igneous Rocks,” copies of which were not received until December 1928 (Bowen, 1928). Bowen showed Schairer the quenching technique of Shepherd, Rankin, and Wright (1909), now the preferred method for studying silicate phase relations. Together they investigated leucite-diopside, which turned out to be a simple binary system (Bowen and Schairer, 1929). Nevertheless, true to the rule that no system is really simple, they had to devise a method for making compositions on the join in spite of the volatilization of potassium at temperatures ranging from 1200-1740°C, just within the melting point of the pure platinum crucible (1755° C on the Geophysical Laboratory temperature scale).

During his intensive training period at the Geophysical Laboratory, he managed to write his thesis and complete a major work on “The Minerals of Connecticut.” He had collected the data while he was at Yale by hiking to the various mineral localities, occasionally using a streetcar or train to reach the more distant parts. His *Bulletin of the Connecticut Geology and Natural History Survey* records many reports of the first discovery of the rarer minerals and corrects several misidentifications. In Schairer's view some of the quarries were a “mineralogist's paradise.”

According to Schairer (1964) the only directive he ever received from the director of the Geophysical Laboratory was that he could do anything he "chose to do but he hoped it would have something to do with iron oxides." With this slight nudge Bowen and Schairer investigated the iron-oxide systems with great vigor. From the incongruent fusion relations of acmite ($NaFe^{3}+Si_2O_6$) on the $Na_2O{\cdot}4SiO_2$-Fe_2O_3 join (Bowen and Schairer, 1929) they investigated the ternary Na_2SiO_3-Fe_2O_3-SiO_2 (Bowen, Schairer, and Willems, 1930). They clearly stated that the system was not really ternary because small amounts of iron were in the ferrous state. Obviously, the next system to do was FeO-SiO_2, but the charges oxidized in air and attacked the platinum crucible and all ceramic containers. Bowen and Schairer solved this problem by using pure iron crucibles in an inert (oxygen-free nitrogen) atmosphere. In this way the Fe_2O_3 content of the liquid was controlled reproducibly in the presence of native iron and subsequently determined by chemical analysis. Development of this important buffering technique, now used for other metal-oxide systems, led to the successful investigation, with various colleagues, of the following systems relevant to rocks and rock-forming minerals.

Ca_2SiO_4-Fe_2SiO_4	1933
CaO-FeO-SiO_2	1933
MgO-FeO-SiO_2	1935
Albite-Fayalite	1936
Nepheline-Fayalite-SiO_2	1938
CaO-FeO-Al_2O_3-SiO_2	1942
CaO-MgO-FeO-SiO_2	1950
FeO-Al_2O_3-SiO_2	1952

These systems contain practically all the important end members and many of the simple solid solutions of the rock-forming minerals: olivines, pyroxenes, pyroxenoids, melilites, feldspars, feldspathoids, cordierite, iron oxides (e.g., wüstite, magnetite, and hematite), and the SiO_2 minerals.

The complexity of the two quaternary systems led Schairer to express the major courses of fractionation of liquids as a flow sheet. The boundary curves within the tetrahedral models could thereby be laid out in two dimensions and the relationship of the various invariant points exhibited with clarity. The flow sheet concept has since been applied to the even more complex relationships of natural rocks that tend to concentrate at or near invariant points in multicomponent space.

The applications of these systems to the steel and ceramic industries were greatly appreciated, but Schairer was true to his major goals in spite of the accolades from those industries (Schairer, 1942):

As you probably know, my major interest has always been the application of physical chemistry to geological problems, particularly the problem of the origin of igneous rocks and their minerals. My attitude has always been that my major studies should be on rock-forming minerals and that I should confine my attention to systems of direct or very close interest in solving mineralogical or geological problems. If these studies had ceramic, metallurgical, or other scientific interest it was all to the good, but I have never instituted any research project because it was of special interest to ceramics or metallurgy. Thus, I do not wish to pose as a bona fide ceramist, although I have always had a kindly and sympathetic interest in problems in the general chemistry of ceramics.

He was not a bona fide metallurgist either, but it did not prevent him from making a major contribution in that field during World War II as described below.

The motivation for investigating the iron-bearing systems was primarily the pursuit of knowledge, but another driving force was probably the escalating argument between colleagues N. L. Bowen and C. N. Fenner regarding the con centration of FeO and SiO_2 in the residual liquids undergoing crystallization differentiation. Bowen argued that fractional crystallization leads to the enrichment of residual liquids in iron relative to magnesium, but is overshadowed more commonly by an overwhelming increase in the alkali and silica content. Fenner, on the other hand, advocated the more rarely observed trend of absolute enrichment of residual liquids in iron. Fortunately the argument was resolved later when due regard was given the state of iron oxidation (Osborn, 1959), and both the Bowen trend and the Fenner trend could be demonstrated in the laboratory.

FELDSPARS AND FELDSPATHOIDS

The first system to be investigated at the Geophysical Laboratory was the plagioclase solid solution series (Day, Allen, and Iddings, 1905). The high viscosity of the feldspar melts was the chief difficulty; albite glass also resisted crystallization even in subsequent experiments by Bowen (1913). Seeding was of no avail. For this reason, Bowen and Schairer (1936, 1938) tried to use fayalite as a flux in promoting the crystallization of albite, and some success was achieved. Even in preliminary studies (Schairer and Bowen, 1947) on the more general systems Na_2O-Al_2O_3-SiO_2 (Schairer and Bowen, 1956) and K_2O-Al_2O_3-SiO_2 (Schairer and Bowen, 1955), containing albite and sanidine respectively, considerable difficulty was found in growing the feldspar crystals. Schairer (1951) finally arrived at a solution to the kinetics problem. By a process of acclimation involving successively lower treatment temperatures of the liquid with intermediate crushings of the glass, he was able to produce the appropriate structure in the liquid so that crystallization took place promptly below the true melting point. From then on it was possible to study the alkali feldspar join (Schairer, 1950) and the ternary feldspars (Franco and Schairer, 1951).

EARLY AND LATE CRYSTALLIZING MINERALS

In 1935 Schairer and Bowen produced a preliminary phase diagram for $NaAlSiO_4$-$KAlSiO_4$-SiO_2, which Bowen (1937) later described as "petrogeny's residua system." The system is of great petrologic interest because it contains compositions like those of the final liquids yielded at the end stages of the fractionation of rock magmas. The diagram was revised in 1950 by Schairer (1957), but proof of the concept came after an incredibly large number of detailed experiments, mainly by Schairer and his colleagues.

Forsterite-Nepheline-Silica	Schairer and Yoder (1961)
Fayalite-Nepheline-Silica	Bowen and Schairer (1938)
Diopside-Nepheline-Silica	Schairer and Yoder (1960)
Anorthite-Nepheline-Silica	Schairer (1954)
Spinel-Nepheline-Silica	Schairer and Yoder (1958)
Forsterite-Leucite-Silica	Schairer (1954)
Fayalite-Leucite-Silica	Roedder (1951)
Diopside-Leucite-Silica	Schairer and Bowen (1938)
Anorthite-Leucite-Silica	Schairer and Bowen (1947)
Spinel-Leucite-Silica	Schairer (1954)

The removal of the early-formed crystals, in each case the first phase named in the left-hand column above, did indeed result in the fractionation of liquids toward the residua system. The deductions from field evidence overwhelmingly support this concept documented quantitatively in the laboratory principally by Schairer.

Progress on the grand plan to verify the concept that the residual liquids generated by the fractionation of basic magmas were enriched in alkali-aluminosilicates was slowed by Bowen's move to the University of Chicago in the fall of 1937, and brought to an abrupt halt by the onset of World

War II. No one since has had the courage, time, or support to undertake the quaternary systems involving both the potassium- and sodium-bearing phases.

THE EXPEDITER

The trustees of the Carnegie Institution of Washington and its president, Vannevar Bush, then director of the Office of Scientific Research and Development, made the Geophysical Laboratory staff available to the government without charge to aid in solving defense problems. It was recognized early in 1941 that the solution to the hypervelocity (>3,500 feet per second) gun problem depended on an understanding of the fundamental causes of gun erosion. The investigation of gun erosion was undertaken by the Geophysical Laboratory because of its special capabilities with the techniques of high-temperature and high-pressure research. Techniques developed for the study of minerals and rocks under conditions believed to exist deep in the earth proved to be directly applicable to the study of reactions between powder gases and gun barrel metals. The Geophysical Laboratory devoted full time to the problem and specific tasks were assigned to the staff. Schairer was a consultant to Division 1 from November 1941 to December 1942 and July 1943 to September 1944; and a special assistant from October 1944 to June 1946.

The successful development of the stellite-lined machine gun barrel was expedited through the close cooperation of fifteen of the division's contractors. Schairer's role is described by Burchard (1948):

The “spark plug” who fired the enthusiasm of these contractors—even when their efforts seemed to be of no avail in making refractory metals behave — was J. F. Schairer. Through his tireless efforts, which involved spending half his time traveling from one laboratory to another, each one of this group of contractors was kept fully informed of the progress being made by the others. Through his grasp of the ramifications of this exceedingly complex metallurgical program, he was able to pick up information at one laboratory and relay it to another one which could use it.

The new caliber 0.50 machine gun barrel could withstand the firing of “thirty times as many rounds as would ruin ordinary steel barrels fired on the same schedule . . .” By the beginning of 1944 the modified barrels were adapted as standard by the War Department and used in the Pacific campaigns. The same alloy was greatly instrumental in expanding the practical experimental range of pressure vessels used in hydrothermal research at the Geophysical Laboratory after the war.

For his exceptional services Schairer received in 1948 the President's Certificate of Merit and His Majesty's Medal for Service in the Cause of Freedom (Great Britain).

BACK TO FUNDAMENTAL SCIENCE

The Geophysical Laboratory was able to conclude most of its war work by June 1946, and the objectives of future research were focussed and defined (Adams, 1946). Of the five major fields of interest outlined, the first identified was the study of fusion relations of minerals and of related equilibria. An essential part of the laboratory's proposed program was, therefore, to expand the past studies on the anhydrous combinations of the principal rock-forming oxides to complete the existing information on the compositions and mutual stability relations of the rock-forming minerals. With this mandate Schairer renewed his research with characteristic vigor and the help of an array of staff members, fellows and visiting investigators:

$$\left.\begin{array}{l} Na_2O\text{-}MgO\text{-}SiO_2 \\ Na_2O\text{-}FeO\text{-}SiO_2 \end{array}\right\} \quad \text{Schairer, Yoder, and Keene (unpub.)}$$

$MgO\text{-}Al_2O_3\text{-}SiO_2$	Keith and Schairer (1952)
$K_2O\text{-}MgO\text{-}Al_2O_3\text{-}SiO_2$	Schreyer and Schairer (1961)
$K_2O\text{-}MgO\text{-}Al_2O_3\text{-}SiO_2$	Schairer (1954)
$Na_2O\text{-}MgO\text{-}Al_2O_3\text{-}SiO_2$	Schairer and Yoder (unpub.)
$CaO\text{-}MgO\text{-}Al_2O_3\text{-}SiO_2$	Chinner and Schairer (1962)
$Na_2O\text{-}Al_2O_3\text{-}Fe_2O_3\text{-}SiO_2$	Bailey and Schairer (1966)
$CaO\text{-}MgO\text{-}Fe_2O_3\text{-}SiO_2$	Huckenholz, Schairer, and Yoder (1968)

(Brief accounts and the preliminary phase diagrams for those systems not described in detail will be found in the *Annual Reports of the Director of the Geophysical Laboratory.*) In addition, work on the ferrous iron systems continued and some of the investigations incomplete at the beginning of the war were prepared for publication.

In describing the complex quaternary systems to audiences unfamiliar with phase equilibria, Schairer would use his "magic cheese knife" with which he would cut the tetrahedral model in various directions. Each cut would display the results of a join representing real or assumed end members that he had investigated. His clarity of presentation, punctuated with humor and wit, brought an understanding nonspecialists rarely achieved. In his humility he usually failed to inform the listener of the years of effort, multitude of time-consuming experiments, and the great care taken to achieve accuracy that had been required for each of the works described graphically as "cuts." He was indeed a perfectionist who remained productive through great efficiency and industry.

THE BASALT SYSTEMS

With the formulation in the fall of 1958 of the simplified basalt tetrahedron by Yoder and Tilley (1962), based on the principal normative components of basalts, Schairer laid aside the oxide-system approach to the investigation of the major rock-forming minerals. The tetrahedral model of basalts, the most abundant rock type in the earth's crust, was later expanded by Schairer and Yoder (1964) to include the melilites. It was possible thereby to represent a wide variety of both alkaline and tholeiitic basalts and determine their melting behavior and interrelationships.

He now tackled the basalt system with incredible vigor, coming to work early and leaving late. The halls of the laboratory still ring with Schairer's evening departure as he loudly announced that he was "saturated with all solid phases." The critical planes investigated included:

Forsterite-Nepheline-Silica	Schairer and Yoder (1961)
Forsterite-Diopside-Silica	Schairer and Yoder (1962) Kushiro and Schairer (1963)
Forsterite-Diopside-Albite	Schairer and Morimoto (1958)
Diopside-Enstatite-Albite	Schairer and Morimoto (1959)
Diopside-Nepheline-Silica	Schairer and Yoder (1960)
Forsterite-Diopside-Nepheline	Schairer and Yoder (1960)
Nepheline-Diopside-Akermanite	
Nepheline-Akermanite-Wollastonite	
Akermanite-Wollastonite-Albite	Schairer and Yoder (1964)
Akermanite-Diopside-Albite	
Akermanite-Nepheline-Albite	
Nepheline-Diopside-Wollastonite	

The major conclusion from this vast amount of data was that major igneous rock types clustered on or near critical univariant curves and the principal invariant points of the natural multicomponent system.

The basalt tetrahedron consists of five oxide components (SiO_2,Al_2O_3,CaO,MgO,Na_2O), yet after public presentation someone in the audience would invariably ask about the effects of Fe, Ti, Mn, or some other element. About 99 percent of most rocks can be represented by ten elements, but it was clear that the systematic investigation of the ten-component system was some years ahead. A short cut was initiated by Yoder and Tilley (1962) in which the melting relations of natural basalts themselves were investigated with the same care and precision as the pure synthetic systems. By means of a sequence of studies on a wide variety of natural basalts (Tilley, Yoder, and Schairer, 1963, 1964, 1965, 1967, 1968), it was demonstrated that the simple, pure systems appropriately displayed the melting behavior of complex natural rocks. It was evident that a full understanding of the behavior of most igneous and metamorphic rocks would be achieved by the systematic study of the simple systems, including the various volatile constituents, rather than the haphazard study of selected rock varieties. The advantages of the simple systems approach include control of the entire range of compositional variables, the effects of a single variable can be evaluated, and principles can be demonstrated unambiguously. On the other hand, there are several disadvantages with the use of natural rocks as starting materials. The results apply only to a single bulk composition and are not applicable to related rock types. The rock may have endured other processes subsequent to consolidation; the rock was not necessarily all liquid in the early stages of formation; the rocks do not necessarily rep resent equilibrium assemblages; and the large number of degrees of freedom result in multiple interpretations that do not lead to establishing fundamental principles.

ENDMEMBER MINERAL SYSTEMS

Concurrent with the above-described petrological programs Schairer had a continuing interest in the detailed study of the major mineral groups. The phase relations of the olivines and the ternary feldspars have already been mentioned. Other groups include the pyroxenes, melilites, and the garnets. In almost every group his studies were the pioneering efforts to elucidate the melting behavior and limits of solid solution between endmembers.

Building on the work of Allen et al. (1909), Bowen (1914), and Atlas (1952), Boyd and Schairer (1964) outlined the solvus on the join diopside-enstatite. Bowen, Schairer and Posnjak (1933) displayed the complexity of ferrosilite-hedenbergite. The join enstatite-ferrosilite was investigated in great detail by Bowen and Schairer in 1935. The feature of major petrologic interest was the nature of the orthopyroxene-clinopyroxene inversion, a potential geothermometer. Even though the results have been reinterpreted and reinvestigated many times since, the inversion characteristics of those pyroxenes continue to hold the attention of petrologists. Because of the difficulty of preparing iron-rich compositions exactly on the plane, it was evident that the investigation of the pyroxene quadrilateral, diopside-hedenbergite-enstatite-ferrosilite, was to be an enormous undertaking. To get a preliminary view of that system, natural analyzed pyroxenes were used (Yoder, Tilley, and Schairer, 1963). The general relations have been verified by others by considering compositions off the plane of the pyroxene quadrilateral; however, the melting relations for the pure system have not yet been determined. Pyroxene solid solu tions that included components rich in Al, Fe^{3+}, Cr, and Na were also investigated, usually with Fellows, who could maintain the fast pace kept by Schairer.

The study of the melilites began with akermanite-gehlenite-pseudowollastonite by Osborn and Schairer (1941), now a classic both in the theory of fractionation involving solid solution and in experimental methodology. They also prepared a preliminary diagram for akermanite-iron-akermanite. The ternary melilites, akermanite-gehlenite-sodium melilite, posed problems of a different order. The sodium melilite endmember was stable only at high pressures, but the limits of solid solution determined by Schairer, Yoder, and Tilley (1965) at 1 atmosphere were close to those found in melilite-bearing igneous rocks. That work involved the investigation of the melilite joins in gehlenite-nepheline-wollastonite and akermanite-nepheline-wollastonite; the ternary liquidus; four isothermal sections in the crystal + liquid regions of the ternary; and some portions below the ternary solidus where kinetic problems were evident. The results from the pure system were tested with analyzed natural melilites.

The *Annual Reports of the Director of the Geophysical Laboratory* record many other results on important mineral systems that, unfortunately, were never described in detail. Each investigation opened the door to a new array of problems, and for Schairer there was a compulsion to get on with the exciting chase. His meticulous notes on each experiment, calibration, and material preparation contain a wealth of information on the behavior of silicates. It is difficult to imagine that the determination of any silicate phase diagram in the future will be investigated with the same care and accuracy that were the hallmark of Schairer's work. New analytical tools (e.g., electron microprobe and scanning electron microscope) will indeed reduce the work, but it will still take another person who, in Schairer's words, has “learned to think like a silicate.”

In recognition of his precise phase equilibria determinations and contributions to experimental mineralogy and petrology, Schairer received the Hildebrand Award of the Washington Chemical Society in 1942, the Arthur L. Day Medal of the Geological Society of America in 1953, and the Roebling Medal of the Mineralogical Society of America in 1963. He was elected to the National Academy of Sciences in 1953.

THE STORY TELLER

To keep abreast of the key problems of interest to geologists, Schairer traveled extensively in the United States and in Europe and Japan, particularly to visit field parties. For more than twenty summers he organized month-long excursions to key outcrops mainly in the western states usually with several of the young Fellows from the Laboratory. The excursions were run with great efficiency and always in good humor, with Schairer managing the cooking. He only balked at cooking a steak well done. The fellows not only learned considerable geology but also the names, both common and botanical, of the wild flowers, which he could identify by the hundreds. The pH ranges of tolerance of the plants and shrubs were often clues to the underlying rock types. Living in the rough and spending long, difficult days hiking have always been part of a geologist's life, but few have enjoyed the discussions around the campfire at night with so many talented investigators.

Schairer was a masterful story teller and each excursion— sometimes referred to as his endowed vacation—generated humorous events that he would turn into a captivating yarn. A favorite was the technique of cracking an egg that had been transported on a pack mule in the desert for weeks.

How to eat cod fish cheeks that were "too good for city folks" or the preparation of smoked eels, steamed clams, and slumgullion, and why the Pope never tasted turtle soup were part of the menu. Other stories included the burning of the underwear ceremony, how to ride a rented horse, and endless bear and skunk encounters. He was an expert on varieties of beer, but some doubted his qualifications when he announced that, "There is no such thing as bad beer." In his stories he usually bore the brunt of the joke. His acumen for storytelling developed early in his life and was aided and abetted by his sisters in spontaneous fun sessions. He never talked about other people; his rule was, "If you can't say something nice, keep your mouth shut." The story he particularly liked to tell arose from his encounters with the mountain people during the blazing of the Appalachian Trail. One short version goes like this.

We started up the hollow and passed the time with everybody. Everybody was friendly but distant. After all, we were foreigners—anybody who lives more than 2 miles away was a foreigner. Then we started out on an old road. It got narrower and more and more gullied. Finally it was just a mountain trail.

All of a sudden around a sharp bend in the trail came two mountaineers. One was an older man with a white beard, and the other was a younger man carrying a gunnysack in which it was obvious there were four two-quart jars of corn liquor.

So we just sat there. And there was an awkward pause. And then the conversation got going as they do in the great circle of the mountains—it was a hell of a fine day, or damned if it wasn't. You start with the weather and you end with the weather.

And the next thing you talk about is the crops, which are important to the mountain people, for if the crops are bad they might starve.

And then the talk was about illness, the miseries, as they called it. And about that time everybody was sick, with inadequate food and inadequate housing, and so forth.

And then another adequate topic of conversation was this proposed Shenandoah National Park, was that all nonsense or was it going through.

And we said, Yes, they are going through, and they might make their plans accordingly.

And we got back to the weather, if it was a good day it was a good day for a drink, or if it was a bad day, we need a drink.

And the fellow says, “Do you fellows ever drink?”

And I said, “I don't mind if I do.”

And he brought out a two-quart fruit jar.

Charlie is a nice guy, but he doesn't drink. It was the most embarrassing thing in the world. I rushed up to Charlie and grabbed the two-quart fruit jar. I nearly knocked him down. I swung the fruit jar up, took a good swig, and swung it down again, and I said, “Charlie doesn't drink, but I drink for him.” And I took another good swig.

So they thought it was so cute I got Charlie's drink.

And there was an awkward pause. And it suddenly dawned on me that I had a drink in my pack. We never drank on the trail, because climbing mountains and drinking liquor didn't go too well together, but Sunday night when we got in Mrs. Meig's before dinner a right good snort was always appreciated.

And so we weren't carrying a two-quart fruit jar, but I had a pint thermos bottle filled with liquor in my pack. And I said, “Won't you have a drink of my liquor?” And I went over to get it out of my pack.

The mountaineers can't figure what anybody would put in those packs.

So I pulled out my raincoat and my flashlight and what was left of a sandwich, and the liquor wasn't there, it was in the side pocket. So I pulled out everything trying to find it. And finally I pulled out this pint thermos bottle and handed it to the fellow.

And he took a drink and he looked very startled. He took another little drink, and he handed it back. I put it back in the pack and tied up the pack and we set down and there was an awkward pause.

Then the old fellow says, “I can tell you where you-all got that liquor.” And I said, “You can?” And he said, “Yes, that is Hazel Hollow liquor.”

And Hazel Hollow was about 30 or 40 miles to the north.

And I said, “Yes?”

And he said, “I can tell you who made that liquor.”

And I said, “Can you?”

And he said, “That is Jack Dodson's liquor. And I can tell you when you got that liquor.”

“How can you tell me that? When I got it, it was in a two-quart jar.”

But he was right all the time. And he looks at me and said, "You must be Mr. Frank."

Schairer was too much for them; they all called me Mr. Frank.

Here I give a guy a drink of liquor, and he tells me my name. And I said, "Would you mind telling me how you do it?"

He said, "Each hollow has its own formula. There is only one make of liquor in Hazel Hollow, and this is Jack's. And Jack has only made three batches this year. The first batch he was terribly thirsty, so he let the batch burn. It couldn't have been that, because it was burned. And having burned the first batch he was terribly cautious, and the second batch was perfect."

That was the batch I had. In fact, it was so good that word got around and it only lasted three days. So he knew within three days of when I bought it.

It couldn't have been that third batch, because he had stored the third batch.

The first batch was burned, the second was that good batch, and it only lasted three days and I had it.

"And Jack never sells any of his liquor to anybody outside the mountains but this fellow Frank, and so you must be Mr. Frank."

Here was a down-to-earth man who was at home with sincere and simple people. The so-called trappings of success—fancy car, big house, and designer clothes—were not of interest to him. He had a disdain for politicians and lawyers, but in spite of this fact his son was successful in both fields. Some colleagues wondered how he managed to be elected to such high offices in the professional societies. In one case he was not even present at the meeting at which he was nominated! His answer was that he just tried to be honest and straightforward in his dealings with others.

He was a deeply religious man who usually said his nightly prayers together with his wife Ruth. Although he was devoted to the Catholic faith and received communion with great humility, he was not impressed with (nor did he participate in) its more elaborate ceremonies. Even while working in the field, however, he rarely missed Sunday Mass, rising early to avoid delaying the others and returning in time to cook breakfast. The Bible he carried was in French. One of his favorite stories included a description of his confession with the Bishop of Quimper. The hour-long discussion in the confessional about the best fishing spots in France was totally misinterpreted by the other parishioners waiting in line.

THE ORGANIZER

Shortly after arriving in Washington, Schairer joined the Wild Flower Society, but discovered they were a “bunch of piddling old ladies” who “took an hour to walk a mile.” On November 22, 1927, he joined with friends holding similar views and formed the Potomac Appalachian Trail Club. He served first as treasurer and then as supervisor of trails. They started blazing trail near Harper's Ferry, West Virginia, and worked south through Virginia. Under Schairer's supervision the club constructed and blazed about 260 miles of the Appalachian Trail from 1928 to 1932. Their work contributed to the formation of the Shenandoah National Park that had been authorized in 1926 and was established on December 26, 1935. From his contacts with the mountaineers Schairer learned to square dance. With his usual enthusiasm he became a charter member of the Allemande Lefters, a square dance group in Palisades, Maryland. As the group matured it was occasionally referred to as the Allemande Leftovers.

Another organization he helped form, in March 1947, was the National Capital Orchid Society. He served as its president for two terms, in 1949 and again in 1963. For twenty years he was editor of its bulletin. In 1957 Schairer was president of the Eastern Orchid Congress. With untiring energy Schairer kept alive the sparkle, fun, and fascination of growing orchids. He was an inspiration to many growers because he was able to maintain over seventy-five varieties in a windowsill environment that was the envy of many commercial growers. As he would say in his many lectures to clubs and societies, “You've got to think like an orchid.” He had very simple ways of tailoring the humidity, sun, and temperature to the needs of each variety on his sun porch. His successes influenced many commercial growers who, with elaborate greenhouses, expanded the number of varieties for public sale. It was his custom to give the back part of his best plants to club members with the recommendation that they follow suit. Except for the bridal bouquet for his daughter Jeanne, the spectacular blossoms were plucked only to enhance the health of the plant. In addition to his interest in orchids he led an annual tour in May to see the trilliums in bloom on Skyline Drive in the Shenandoah National Park.

Schairer would organize a fishing trip when there was a lull in his many activities. He learned the hard way that the opening of the fishing season was not a national holiday. The only reprimand in the files of Dr. Day, director of the Geophysical Laboratory, was a letter to Schairer in regard to unauthorized absences for long fishing weekends. It seemed thoroughly incongruous that such a highly energized person would stand for hours in cold water patiently waiting for a fish to bite. It took considerable persuasion during geological field trips to prevent him from dropping his line in every tempting stream.

During his professional career Schairer served as president of the Mineralogical Society of America (1943); president of the Geochemical Society (1967); president of the Volcanology, Geochemistry and Petrology Section of the American Geophysical Union (1956-59); vice-president of the Geological Society of America (1944); and vice-president of the International Association of Volcanology. In ad dition, he was elected to the Petrologist's Club on December 20, 1927, and served as its secretary-treasurer (the presiding officer) for five years.

THE FAMILY TIES

The Schairers were a close and loving family. Frank's frequent trips home during college and after his move to Washington continued until the death of his mother on November 13, 1939. He married Ruth Naylor, who shared his fondness for the outdoors, on July 20, 1940. Frank had followed his own advice to the young Fellows: Take your bride-to-be on the trail. After rain, cold, blisters and hunger, you—or she—can make sure that love is not blind. They honeymooned on Upper Kintla Lake in the remote northern section of Glacier National Park, Montana.

Twins John (Jack) Everett and Jeanne Evelyn, born on February 17, 1943, were a constant joy for all the family. Frank's father made long visits to Washington and took great pride in wheeling the twins down the avenue.

His six sisters are Helen and Marion Schairer of Rochester, New York; Mrs. Emily Callahan of Bridgeport, New York; Rosemary Schairer and Mrs. Virginia Meagher of Anchor-age, Alaska; and Margaret Turner of Victor, New York. Jack Schairer now resides in Madison, Wisconsin, and Mrs. Jeanne Rzeszut lives in Anchorage, Alaska. Mrs. Ruth Schairer continues to enjoy their retirement home in Chevy Chase, Maryland.

REST AT LAST

Mandatory retirement occurred on June 30, 1969, but as Schairer put it, “They retired me one day and rehired me the next day.” His part-time employment, with supplemental remuneration, did not diminish his full-time contribution. At a banquet in his honor on April 21, 1969, over 100 friends saw a pictorial review of his life. He was presented with "The Schairer Volume," a special volume of the *American Journal of Science* (vol. 267A), consisting of twenty-eight papers on new phase equilibria studies by his colleagues, former fellows, and a few other experimentalists. Hardcover copies of the volume were bound in blue and gold, the Yale University colors.

The end of this happy and productive life came on September 26, 1970, when Schairer died suddenly while splashing about in the waters of the Chesapeake Bay near the summer home of his brother-in-law at Point-no-Point, Maryland. All his furnaces were loaded with runs, a manuscript on a quaternary system was in preparation on his desk, and Fellows were awaiting his help on their projects.

A large number of his friends came personally to pay tribute to the man for his integrity, extensive knowledge, contagious enthusiasm, and for the great pleasure he gave to all. Most fittingly, his casket was covered with orchids, *Cattleya chevy chase.* After services at the Shrine of the Most Blessed Sacrament at Chevy Chase Circle he was laid to rest in section 17, lot 4, in Washington's Rock Creek Cemetery.

Twenty-five years have slipped by since Schairer's death; yet the personal memories are still vivid and the Schairer stories are frequently recalled by the Geophysical Laboratory staff. Schairer's phase diagrams remain the firm foundation on which igneous rocks are discussed today.

IT IS A PLEASURE to acknowledge the kindness of Frank's six sisters in preparing their recollections in 1971 of his early days at home and school. Professor Brian J. Skinner arranged to have copies of files sent from the undergraduate school, graduate school, and alumni office of Yale University. James H. Shipler provided confirmation of Schairer's employment at the Eastman Kodak Company and some background on his synthesis of organic chemicals. Free access to the files of the Geophysical Laboratory was made available through the courtesy of its director, Charles T. Prewitt. The officers of the various societies with which Schairer was associated are thanked for providing confirmation of his election, offices, and awards. The manuscript was reviewed at various stages by Gordon Davis, F. R. Boyd, F. Chayes, B. Mysen, D. K. Bailey, M. L. Keith, S. A. Morse, and E. F. Osborn. The special help of Mrs. Ruth Schairer in providing the more personal details of their family life is greatly appreciated. No one would agree more than Frank on how an understanding and loving wife can provide the necessary environment for a happy and successful life.

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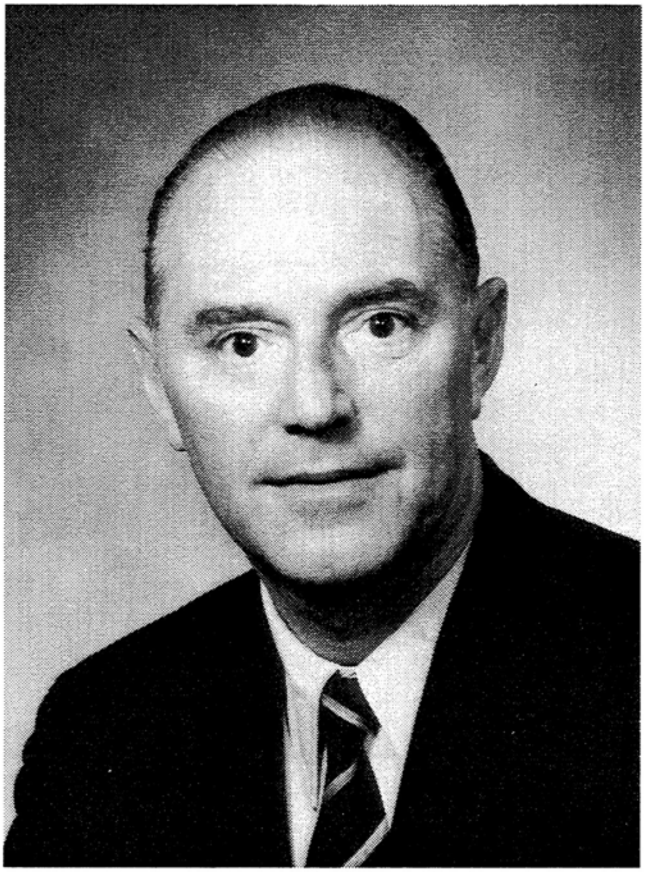

H. Bolton Seed

HARRY BOLTON SEED

August 19, 1922–April 23, 1989

BY JAMES K. MITCHELL

ROFESSOR HARRY BOLTON SEED was the father of the important new area of geotechnical earthquake engineering. Having established himself as one of the country's brightest and most productive young civil engineers by the time of the great Alaska earthquake in 1964, he immediately began research that led to an understanding of soil behavior and ground response during earthquakes that is the basis for present-day seismic design around the world. Concurrently he built the program in geotechnical engineering at the University of California, Berkeley, into one of the best in the world. He will be remembered as one of the foremost civil engineers of any generation, and the hundreds of students who learned from him during his forty years at Berkeley provide eloquent testimony to his superb teaching skills, brilliant mind, and compassion for all of those around him.

Harry Bolton Seed was born in Bolton, England, on August 19, 1922. The son of a cotton mill manager, he spent his childhood in the industrial center of Lancashire, where he attended Farnworth Grammar School. At Farnworth he excelled both at sports and academics, and thus were sown the seeds of what would become a most remarkable career.

Two influences from Farnworth were to change his life forever. The first of these was sport. As captain of the high school's soccer, cricket, and tennis teams, he developed a love for competition and tremendous physical stamina, both of which were to stand him in good stead throughout his subsequent collegiate and professional careers. Sport also presented him with his first serious dilemma with regard to career selection, as at the age of eighteen he was faced with the need to choose between a career as a professional soccer player or accepting England's highest scholarship award to attend the university of his choice. After making the difficult decision to accept the scholarship, he satisfied his desire for high-level soccer competition by captaining the University of London's soccer team, and in his senior year was selected as captain of the All-England team.

The second major influence on Harry's life at Farnworth was the school's headmaster, Mr. Wilson. In addition to being headmaster Wilson taught mathematics. Harry developed a lifelong love of mathematics in the Farnworth classroom. He also developed a heartfelt appreciation for the elegance of excellent instruction and would later credit Wilson with having instilled in him a desire to teach.

Harry Seed served as a lieutenant in the army during the Second World War and completed his undergraduate studies at the University of London, where he received a B.Sc. in civil engineering in 1944 and a Ph.D. in structural engineering in 1947. Following two years as assistant lecturer at Kings College, he came to the United States to study soil mechanics at Harvard University under the tutelage of Karl Terzaghi and Arthur Casagrande. He received his S.M. degree from Harvard in 1948 and spent the next year at Harvard as an instructor. This was followed by a year as a foundation engineer for Thomas Worcester, Inc., in Boston.

In 1950 Professor Seed joined the civil engineering fac ulty at the University of California, where he spent the remainder of his career as an engineering educator, researcher in geotechnical engineering, and consultant to numerous companies and government agencies. He built the program in geotechnical engineering at Berkeley into one of the largest and best in the world. A major factor in this development was his bringing colleagues together from different areas of geotechnical engineering, including geological engineering and rock mechanics, as well as soil mechanics and foundation engineering. He served as chairman of the Civil Engineering Department from 1965 to 1971, a period during which it rose under his direction to number one ranking in the United States for the quality of its graduate programs.

Professor Seed had an enormous impact on every area of research activity in which he worked. His early studies of the mechanics of pile foundations still form the basis of modern methods of pile-soil interaction analysis. His research on soil compaction and the influences of methods of compaction on soil structure and mechanical properties provides the foundation for current understanding. His contributions to analytical methods of pavement design were of the first rank.

In the 1960s he introduced the new field of geotechnical earthquake engineering, and he is recognized as the father of this important field. His pioneering studies included the development of methods for seismic site response analysis, for the analysis of soil-structure interaction, for seismicity evaluation, and for assessment and mitigation of liquefaction potential. The results of his research have led to a total revision of concepts and methods for earthquake resistant design of earth dams, nuclear power plants, coastal facilities, and building foundations, as well as revision of codes of practice, design procedures, and regulations. His meth ods and recommendations, founded on sound scientific principles, have been adopted throughout the world.

Through his research Professor Seed developed design methods that revolutionized many aspects of engineering practice and thinking. They have had enormous influence on the safety of critical structures such as major dams, nuclear power plants, and highrise buildings. His investigations of major disasters, such as the 1964 great Alaska earthquake, the 1971 San Fernando earthquake in California, the 1976 failure of the Teton dam, the 1979 slide at the port of Nice in France, and the 1985 Mexico City earthquake, have, with the aid of modern methods of analysis and experimental techniques, led to a basic understanding of their causes and to the measures that must be taken to prevent similar occurrences in the future.

In addition to his many research contributions, Professor Seed was also unusually active as an international consultant. He served as a consultant on projects all over the world, on behalf of both domestic and foreign government agencies and engineering firms, including virtually every major civil engineering organization in the United States. Of the hundreds of projects in which he participated, most were critical facilities, including more than 100 major dams, more than 20 nuclear power plants, and innumerable major buildings and transportation facilities. His selection by the government of Egypt, under Agency for International Development sponsorship, to make a seismic safety evaluation of the Aswan High Dam placed the safety of virtually millions of people in his hands. His work in all these areas will have an impact on the world for generations to come.

Harry Seed's work as an engineering educator, scholar, and servant of his profession was unsurpassed. He was the epitome of a model scholar devoted to the advancement of engineering science and practice. He gave large amounts of time to public service activities. He was always brilliant as a public speaker and was recognized for years as the best lecturer and teacher in his department. He guided fifty Ph.D. degree candidates to the successful completion of their dissertation research, and they have gone on to distinguished careers of their own in the geotechnical engineering field. Many have elected to follow his example and pursue university careers. Harry took great pride in the success of his doctoral students and was tremendously pleased to see three of them inducted into the National Academy of Engineering.

His writings—nearly 300 papers and reports—are exceptionally lucid and insightful and provide eloquent testimony, as well as a lasting record of his work. These writings were intended to teach, and have had an unprecedented impact on the geotechnical profession. In keeping with his firm conviction that teaching, research, and professional practice ideally should complement each other, his writings were aimed at a very broad audience and led to numerous important advances at all levels of the profession. Harry loved every aspect of his work, and he was fully active, maintaining a full schedule of teaching, research, and professional activity until very shortly before his death in April 1989.

Professor Seed was the recipient of many awards and honors for his contributions. The American Society of Civil Engineers accorded him more awards than any other engineer in the history of the society. These include the Norman Medal twice, the James J. R. Croes Medal three times, the Thomas A. Middlebrooks Award four times, the Thomas Fitch Rowland Prize, the Wellington Prize, the Walter A. Huber Research Prize, and the Karl Terzaghi Award. For his excellence as an educator he received the Distinguished Teaching Award from the University of California and the

Vincent Bendix Research Award and the Lamme Award from the American Society for Engineering Education.

Other awards include election as Fellow of Kings College, London University; the T. K. Hsieh Award of the British Royal Society and Institution of Civil Engineers, Great Britain; the Distinguished Engineering Achievement Award of the Institute for the Advance of Engineering; and the first Kevin Nash Gold Medal of the International Society for Soil Mechanics and Foundation Engineering.

He was selected as Faculty Research Lecturer at the University of California in 1986, the highest honor that the faculty can bestow on one of its own. Other distinguished lectureships that Professor Seed was awarded include the Horace A. McCrary Lecture at the Massachusetts Institute of Technology; the Terzaghi Memorial Lecturer at Boğazici University, Turkey; the Rankine Lecture of the Institution of Civil Engineers, Great Britain; Northern Testing Services Distinguished Lecturer; Martin S. Kapp Memorial Lecturer of the American Society of Civil Engineers; James H. Haley Memorial Lecture, Boston Society of Civil Engineers; Distinguished Civil Engineering Lecturer, University of Nevada; Charles Schwab Memorial Lecturer, American Iron and Steel Institute; and the Nabor Carillo Lecture, Mexican Society for Soil Mechanics.

Harry Seed was elected to the National Academy of Engineering in 1970, to honorary membership in the American Society of Civil Engineers in 1985, to the National Academy of Sciences in 1986, and to honorary membership in the Earthquake Engineering Research Institute in 1988. In 1987 he was awarded the first honorary doctoral degree awarded by the Ecole Nationale des Ponts et Chaussees in Paris.

Harry Seed was the devoted husband of Muriel Johnson Seed and father of Raymond Bolton Seed and Jacqueline Carol Seed. It was a source of great pleasure that his son chose to enter the field of geotechnical engineering and is now a member of the civil engineering faculty at Berkeley.

Harry Bolton Seed was truly a giant of his generation, and all of us are the richer for having had him among us. His life's work of teaching, research, and professional practice have had a profound influence on the field of geotechnical engineering. For those who knew him well, however, he will be most remembered as a generous and compassionate gentleman, with wit, incisive insights, and a wide-ranging interest in the world around him. No problem was too small to be analyzed and solved; every person was given his time and consideration. He was truly a teacher in the highest sense of the word, and he is sorely missed.

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KENNETH WARTINBEE SPENCE

May 6, 1907–January 12, 1967

BY ABRAM AMSEL

N 1964 WHEN KENNETH SPENCE moved from the University of Iowa to the University of Texas he must have thought he was embarking on a long, new phase of his career. His parents were both long-lived and he was then only in his middle fifties. Three years later, on January 12, 1967, at the age of 59 he died of cancer, ending a distinguished career as a theorist, experimenter, and teacher, and toward the end of his life, as an editor in collaboration with his wife, Janet Taylor Spence.[1]

PERSONAL HISTORY

Spence was born on May 6, 1907, in Chicago, where his father was an electrical engineer. The family moved to Montreal when he was a young child and Kenneth spent his youth and adolescence there. At West Hill High School in an area of Montreal called Notre Dame de Grace he was active in basketball, track, and tennis. Later at McGill University he injured his back during track competition and, as part of his therapy and convalescence, he went to live with his grandmother in LaCross, Wisconsin. He attended LaCross Teachers College and majored in physical education. There he met and married Isabel Temte. The couple had two children, Shirley Ann Spence Pumroy and William James Spence.

He returned to McGill, switched his major to psychology, and took his B.A. in 1929 and a master's degree in 1930. (As a personal aside, sixteen years later I completed a master's degree at McGill under the supervision of Robert B. Malmo, a Yale Ph.D. who knew about Spence's work and his association with Clark L. Hull. I took a seminar with Chester E. Kellogg, who had been Spence's graduate advisor and was very proud of it. For these reasons I found myself heading to Iowa City to study and work with Spence.)

From McGill Spence went to Yale University, where he was a research assistant in the laboratory of Robert M. Yerkes. Under Yerkes' direction he completed a dissertation on visual acuity in the chimpanzee and received the Ph.D. degree in 1933. As Hilgard reports,[2] during his years at Yale Spence began an intellectual association with Clark L. Hull that was, in part at least, a product of a graduate course in experimental psychology that Hilgard was then teaching. With Walter Shipley he performed an experimental test of one of Hull's deductions concerning the difficulty of blind alleys in maze learning in the rat. This led to other papers on maze learning which, as Hilgard writes, Spence published on the side while doing his dissertation on visual acuity in the chimpanzee. These papers revealed Spence's great promise at designing experiments relative to theory, and this feature of Spence's style became the hallmark of his theoretical-experimental work. Indeed, students who worked with Spence at Iowa roughly from 1940 to 1964 usually referred to their Ph.D. degrees as being in theoretical-experimental psychology.

From Yale Spence went on a National Research Council fellowship to the Yale Laboratories of Primate Biology at Orange Park, Florida, where he spent four years and did his seminal work on discrimination learning in the chimpanzee, about which more later. In 1937 he was offered a one-year assistant professorship at the University of Virginia to fill in for someone on leave. The next year he went to the State University of Iowa, where he spent twenty-six years, twenty-two as department head, before moving to the University of Texas in 1964.

PROFESSIONAL HISTORY

Kenneth Spence was one of the major learning theorists of his time. Although his name and Hull's appeared together on a paper just once, in a methodological article in 1938 dealing with the differences between correction and non-correction procedures in maze learning, their names are usually linked to identify the most influential neobehavioristic theory of the 1940s and 1950s that encompassed conditioning, learning, and motivation. Spence's contribution to this theory was explicitly acknowledged by Hull in the preface to *Principles of Behavior*,[3] but it can also be inferred from the level of correspondence maintained by the two men. The volume, the time span, and the theoretical content of this correspondence make it, from an historical point of view, perhaps the most extensive and important in the history of the psychology of learning.[4] One can, however, begin to appreciate Spence's independent contribution to learning theory simply by reviewing the thirteen papers he published in the *Psychological Review* between 1936 and 1966.

Spence's contributions fall into three major categories: (1) learning and motivation theory, (2) the experimental psychology of learning and motivation, and (3) methodology and philosophy of science. (In some of the writings on methodology and philosophy of science Gustav Bergmann was a major collaborator.) In this latter area one of Spence's contributions was to help clarify for all of us the role in psychology of operationism and the nature of theory construction, and to point out the difficulties that exist in the formulation of psychological theories. Among his insights was that psychologists, unlike physical scientists, are faced with the necessity of constructing theories even at the level of trying to establish the basic laws of behavior; because of the nature of their observations and the fact that they do not work in closed systems, psychologists cannot in most cases begin with simple empirically derived generalizations.

In the introductory portion of his Silliman lectures, Spence (1956) made clear his position on psychology as a scientific discipline, including other than methodological factors that impeded its too-slow progress. This point of view, offered in classrooms and privately on many occasions, was that these impediments lay within the discipline of psychology itself— in the holists and the humanists, particularly, who ranted against artificial laboratory situations, and in the practitioners (the clinicians, mainly) who were beginning to dominate the American Psychological Association and were generally disdainful of theoretical-experimental psychology and paid little if any attention to its findings.

Spence's contributions to learning theory, apart from his collaboration in the Hull-Spence system, were of two kinds. His first contribution was as a systematist, as a commentator on and interpreter of the characteristics of the theories and systems of others. His chapter in the edited volume of Stone (1951) is an example of this skill, as is his contribution to the Stevens *Handbook of Experimental Psychology* (1951). Edward Tolman, whose theorizing in animal learning and motivation provided at the time the major alternative to the Hull-Spence position, is reported to have said he never fully comprehended the structure of his theory until he saw Spence's analysis of it.

The second, and Spence's main contribution, was to the body of theory itself, beginning with the famous early papers on discrimination learning. These papers included the derivation of transposition in discrimination learning from stimulus-response gradients of excitation and inhibition, and the derivation of seemingly sudden solutions to discrimination problems from principles of continuity in learning. As is the case in the work of so many distinguished scientists, this early work of Spence's, a product of his time at the Orange Park Primate Laboratories, was, as we shall see, the focus of much of the research in the Iowa laboratory in the 1940s, and it will remain perhaps his most influential.

Spence's more formal, theoretical contributions to the study of learning and motivation are summarized in his Silliman lectures at Yale University, published as *Behavior Theory and Conditioning* (1956). They reveal a substantial difference between himself and Hull in theoretical style. As Kendler[5] points out:

In essence . . . Spence's formulation, as compared to Hull's, shifted in the direction of paying more attention to the behavior of the animal in interpreting the theoretical consequence of a given experimental variable. This difference seems inevitable if it is remembered that Hull was resolute in his determination to present his theory in a formal manner. No doubt this methodological commitment encouraged him to select postulates that could be stated simply and nearly. Spence, in contrast, more sensitive to the fine nuances of experimental data and more aware of the provisional nature of psychological theorizing, did not feel any compulsion to offer anything resembling a final solution. His aspirations were in touch with the realities of his subject matter and within these constraints he worked to interpret available data and predict new findings. His pragmatic approach to theorizing is brilliantly revealed in the concluding chapter of *Behavior Theory and Conditioning* in which he demonstrated how fundamental principles of conditioning can be applied profitably to the analysis of complex learning tasks.

This difference between Spence and Hull in pragmatism of approach was revealed in another way. Spence was not, after Hull's death, vigorous in pursuit of Hull's later interests in the quantification of reaction potential; like Hull, however, he did continue to try to reduce learning phenomena to mathematical equations (1952, 1954). In these attempts he was, in substance if not in exact form, in tune with developments in mathematical psychology which, from about 1950, were given new impetus by Estes at Indiana and by Bush and Mosteller at Harvard. A quarter century after Spence's death a genuine mathematical psychology of learning of any generality seems still in the (perhaps distant) future.

Like so many scientists of his caliber and standing, Spence's published work does not reflect all of his scientific interests. Many of the unpublished ones were covered in his seminars, and in many cases they were the source of Ph.D. dissertation topics for his students. One of Spence's interests that at first surprised some of his students was his attempt at a neobehavioristic interpretation of perception. As we thought about it, however, we saw that this was a topic he carried over from his early work on vision and on theories of discrimination learning in the chimpanzee. It reemerged at Iowa in the 1940s in the work surrounding the two major theoretical issues, to which I have already alluded, that Spence brought to Iowa from his work at the Orange Park laboratories. (Indeed, Spence and his students at Iowa, and not Hull and his students at Yale, were the protagonists on the S-R-behaviorist side against Tolman and his followers at the University of California, Berkeley, on the cognitive-behaviorist side in these and other issues, for example, the controversy surrounding latent learning.)

The first issue was whether discrimination learning was relational or specific. This addressed the role of transposition raised by a number of American psychologists in the first two decades of this century, but usually attributed to the Gestalt psychologists, particularly Kohler,[6] who showed that in discriminating between stimuli on a dimension, the hen, chimpanzee, and human child appear to respond to the relational aspect of the stimuli. The animal learns, according to this view, to respond not to one specific stimulus and not to another (large versus small circle, dark versus light shade of gray), but to the relation between them (to the larger or the darker of two stimuli). Spence's (1937) famous nondirectional S-R analysis of transposition was a tour de force whose power continues to this day to be recognized in psychological theories of discrimination learning.

The second issue was whether discrimination learning was a gradual process or a sudden event. This issue divided the insight theorists at Berkeley and the Hull-Spence view that differences in habit strength accrued gradually through successive reinforcement and nonreinforcement of responses. To argue this point Spence (1940) invented the presolution phase of discrimination learning, a phase during which the subject was exposed to both of the discriminative stimuli, but only for a number of trials too small for learning to be apparent behaviorally. The presolution phase was followed by a phase of reversal of the positive (reinforced) and negative (nonreinforced) stimuli and this phase was carried to the point of clear-cut discrimination learning. These experiments showed that, even without any apparent learning, presolution discrimination training retarded solution in the reversal phase, proving that excitatory and inhibitory potentials had been building up to the two stimuli in the presolution phase even though these were subthreshold for response evocation and were not reflected in discriminative behavior. According to Spence the insight pro ponents, Krechevsky[7] in particular, would not make this prediction.

In addressing both these issues Spence emphasized what he called the receptor-exposure act. This emphasis was an example of Spence's attention to the non-obvious specifics of the experimental arrangements that were employed (a feature of Spence's style that, as Kendler[8] pointed out, differed greatly from Hull's greater interest in the more formal, abstract aspects of theorizing). Spence's argument was that the apparent rapidity with which rats learn a discrimination on a Lashley jumping stand will depend on where the stimuli are placed, as they tend to look at where they are jumping. Because they jump to land on a platform the two stimuli between which they must choose should be placed near the bottom of the stimulus panels they face rather than higher up—a small point, but critical to how quickly the discrimination is learned and how sudden the learning seems to be. The receptor-exposure idea was an element in Spence's never-published theory of perception.

In light of this interest in perception and its relation to discrimination in animals, Spence always insisted that his theory of discrimination learning was a theory about inarticulate organisms and should not be applied directly to humans (sometimes with an aside that perhaps college freshmen, frequently the subjects in psychological research, might be an exception). He was explicit in stating that, as children gained symbolic skills and language, new factors arose. Spence was pragmatic and cautious and did not make the claim that the Hull-Spence (in this case, the Spence) theory could with minor additions be extended to explain these skills and behaviors. A dissertation by Margaret Kuenne,[9] directed by Spence, relating language to transposition in young children, addressed these particular concerns, as did a body of later work by Tracy and Howard Kendler.

If we think of Spence's research career as spanning about a thirty-year period (apart from his early work in maze learning as a graduate student at Yale), it can be divided into two major phases. The first phase, beginning in the middle 1930s and ending about 1950, is marked by the work described above on discrimination learning in the chimpanzee and later in the rat and by some preoccupation with philosophical-methodological matters. From about 1950 on, almost all of Spence's own research papers involved human subjects and involved classical (Pavlovian) eyeblink conditioning. During this period much of the other research from the Iowa laboratory was on instrumental learning in the rat and consisted of master's theses and doctoral dissertations that Spence supervised, much of it on interactions between motivation and reinforcement. (To my knowledge Spence's name never appeared as a co-author on a journal article based on a student's Ph.D. dissertation, and I believe this was also generally true of articles based on master's theses. The student was frequently given the problem to work on or it was suggested by Spence in his classes and seminars. He gave advice and helped with the writing, but the publications belonged to the student.)

The eyeblink conditioning experiment employed by Spence in much of his own later work was for him the closest he could come to a “psychological vacuum” for teasing out the most fundamental principles of association and the relative roles of habit and drive in simple learning. While I don't remember his ever having said this in just these terms, some of the very last work he did with this procedure supports this assertion. Spence demonstrated with great clarity that human eyeblink conditioning data could be “contaminated” by cognitive factors (a little air creeping into the vacuum) and that such factors accounted for the greater extinction rates in Pavlovian conditioning in humans than in animals.

He and his students showed that if human subjects were told a cover story to mask the true purpose of the conditioning procedure, the rate of extinction, the decline in responding when the unconditioned stimulus was omitted, was very much slower than when the subject was aware of the experimental sequence and could detect the transition from reinforced acquisition to nonreinforced extinction. The vacuum under these masked conditions was restored and one presumably got closer to revealing the most fundamental laws of association.

Kenneth Spence did not live to see the full flowering of the cognitive revolution in psychology, which can be dated from about 1960, and his stance vis-à-vis the cognitivists is not well understood. Influenced by Pavlov and by the early (1913-19) brand of Watsonian behaviorism, Spence was not a thoroughgoing behaviorist in the mold of the later, more doctrinaire Watson of 1924[10] or of the post-1950 B. F. Skinner.[11] Spence's position, like Hull's and Tolman's before him, is now characterized as a form of neobehaviorism. (He was nevertheless a behaviorist in every methodological sense.) Like other neobehaviorists he did not take the more extreme positivistic stance of the later Skinner—of avoiding the use of empirical constructs defined operationally. This is particularly clear in the fact that, as we have seen, a substantial part of his work, particularly in the 1950s, had as its major purpose the separation of habit and motivational or drive factors in the eyeblink conditioning experiment. Some of his work involved the concept of level of anxiety, defined by a subset of items taken from the Minnesota Multiphasic Personality Inventory that became known as the Manifest Anxiety Scale.[12] This work at the University of Iowa was in collaboration with I. E. Farber, Janet A. Taylor (later Janet Taylor Spence), and others. Anxiety defined in this way was shown on the one hand to have generalized drive properties to facilitate simple (eyeblink) conditioning, but on the other hand to have disruptive properties to retard or interfere with more complex (e.g., paired-associate, multiple-unit maze) learning, and a neat theory was developed to account for this apparent paradox.

Spence's work is still among the best of its kind, and is frequently cited, though not as often as in the six-year period from 1962 to 1967 (the year he died), when he was the most cited psychologist in a survey of fourteen journals judged to be the most prestigious in the field.[13]

In any account of his intellectual history one must not overlook, and cannot overestimate, another facet of Kenneth Spence's contribution—the seventy-five doctoral students who came out of his laboratories, a large number of whom have gone on to make significant contributions of their own.

TEACHING

As head of the Department of Psychology at Iowa, which he became in 1942 following the untimely death of John A. McGeoch, Spence inherited a relatively small group of colleagues with diverse interests. Carl Seashore, who had been dean of the graduate school, maintained an office in the department, and one of each of several specialties in psychology were represented: history and systems, social psychology, psychoacoustics, statistics and measurement, clinical psychology, and conditioning and learning. However, after a few years, at least by 1946 when I was there as a student, Spence's interests in the theoretical-experimental psychology of conditioning and learning and motivation dominated the department, particularly the graduate curriculum.

Spence took his own teaching very seriously. His lecture notes were meticulously prepared and were updated from year to year. In the years I was at Iowa he taught a two-semester course in learning that was taken by every first-year student, regardless of major area of interest. During each spring semester he offered a graduate seminar on special topics in learning that reflected his major interest of the moment. And in the summer sessions he alternated courses in theories of learning and theories of motivation. Although he was regarded by outsiders as very doctrinaire, a vigorous proponent of the Hull-Spence position, his students knew that, particularly in his seminars and in his summer courses, he covered the various theories of learning and motivation other than Hull's and his own in great detail and with great insight. He took fierce pride in the graduate education provided at Iowa. I have often told the following story to illustrate how Spence felt about the Iowa education.

At one of the first meetings of the newly formed Psychonomic Society (I think in Chicago in 1961), Kenneth said to me, “I hear you have reviewed Mowrer's book.” (Spence had some theoretical differences with O. H. Mowrer.) When I admitted I had done such a thing, Spence added accusingly, “And I hear you gave it a favorable review.” I thought my review had on balance been favorable, so feeling trapped and fighting for time, I asked him if he would actually read the review. He said he would and, breathing relief, I said I would send him a copy. Scene two is some months later at a spring meeting, and I asked Kenneth, “Did you read my review of Mowrer's book?” Yes, he had. “And did you think it was a favorable review?” He gave me one of his penetrating looks and said, “No, I didn't, but who but an Iowa graduate would have known it was not favorable?”

HONORS

Kenneth Spence was the recipient of many honors starting in his years as a graduate student at McGill University when he was awarded the Prince of Wales Gold Medal in Mental Sciences and the Governor General's Medal for Research. Later he was elected to the Society of Experimental Psychologists and received its Howard Crosby Warren Medal for outstanding research in psychology and was elected to the National Academy of Sciences. He received the Distinguished Scientific Contribution Award of the American Psychological Association the first year it was awarded. (The story goes that this APA award was created, in part at least, to honor Spence after he had been urged to run for its presidency four or five times and, not having been elected, refused to run again.) But perhaps the honor Spence cherished most was his invitation to deliver the Silliman lectures at Yale University. He is the only psychologist ever selected for this honor.

NOTES

1. This memoir owes much to two obituaries. One by E. R. Hilgard in *Amer. J. Psych.* 80:314-18 (1967) and one by H. H. Kendler in *Psych. Rev.* 74:335-41 (1967).

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MAX TISHLER

October 30, 1906–March 18, 1989

BY LEWIS H. SARETT AND CLYDE ROCHE

AX TISHLER WAS AN ILLUSTRIOUS SCIENTIST, a chemist, who— unlike most of his Harvard associates—elected to join the ranks of industry. After a thirty-two-year career of remarkable scientific productivity and leadership at Merck & Co., Inc., the world's largest prescription drug company, he returned to academe. As professor of chemistry at Wesleyan University in Connecticut he carved out yet another distinguished career.

At Merck he led research teams whose work was of enormous importance for human health, resulting in practical processes for synthesizing ascorbic acid, riboflavin, cortisone, miamin, pyridoxin, pantothenic acid, nicotinamide, methionine, threonine, and tryptophan. He also led a microbiological group that developed fermentation processes for actinomycin D, vitamin B_{12}, streptomycin, and penicillin. In addition, his invention of the animal-health drug sulfaquinoxaline, the first coccidiostat, made possible a great expansion of the poultry industry and created overnight a new field for research—an event of great magnitude for agriculture.

As a result of such leadership Tishler in 1957 became the first president of the Merck Sharp & Dohme Research Laboratories Division of Merck & Co., Inc. In 1987 when Presi dent Reagan presented Tishler with the National Medal of Science, the citation described him as "a giant on the chemical scene these past fifty years. . . . The importance of Dr. Tishler's specific contributions to the nation's health can scarcely be exaggerated."

Max Tishler embodied the classic American ideal of success. Born in Boston in 1906, he was the fifth of six children of European immigrants. His father, a cobbler, left the family when Max was only five years old. As Max grew up he worked to help support his family. He held jobs as a baker's delivery boy, a newspaper seller, and a telephone answerer.

After all this his career took a crucial turn when he got a job as a pharmacist's assistant with duties that included tending the soda fountain as well as—what is more important— packaging and delivering drugs. The influenza epidemic of 1918 found Max delivering drugs in his native Boston. The ill and dying were everywhere. Deeply touched, he resolved to make a career in some place where he could contribute to health care.

An outstanding scholastic record in high school earned Max a scholarship to Tufts College (now Tufts University), where he was accorded a B.S. magna cum laude in chemistry in 1928. There he met Elizabeth M. Verveer, a freshman in his chemistry laboratory, who became a talented pianist and sculptor. They married in 1934 and their union was a source of joy and stability for Max all his life. Their two sons—Peter V., a physician and genetics researcher, and Carl L., a clinical psychologist—added to his happiness.

From Tufts Max went directly to graduate school at Harvard, where he came under the stimulating influence of Elmer P. Kohler and James Bryant Conant, later president of Harvard. Max earned his M.A. in chemistry in 1933 and his Ph.D. in organic chemistry under Kohler in 1934. For his doctoral dissertation he accomplished the first-ever resolution of an allene, a landmark confirmation of organic chemical theory. He later described Kohler as "a great experimentalist from whom I learned a lot of laboratory techniques." Conant was "a great teacher, very stimulating," whom Max got to know much better in 1939, when he helped Conant revise his textbook *Chemistry of Organic Compounds.*

Both these distinguished educators helped Max at another critical turning point in his career when, with academic appointments scarce, he sought an opportunity in industry. Kohler spoke on his behalf to Randolph T. Major, director of the budding research program at Merck. Conant recommended Max as the outstanding chemist to go through Harvard in a generation.

So, in 1937, Max received an offer to join Merck, which was then just a small company making fine chemicals in Rahway, New Jersey. It was a good time to join that company and that industry. George W. Merck, president of the firm and son of its founder, was a gentleman whose resources matched his high aspirations. Thus, he was able to use the Merck sales base—chemical commodities such as iodine, silver nitrate, ether, and chloroform—as a platform for building a more innovative organization.

George Merck's ambition was to convert a company making fine chemicals into one creating new therapeutic agents for humanity. To this end he had launched in 1933 a program to greatly expand the company's scientific research organization and laboratories and to broaden the scope of its effort to include basic research.

This ambitious expansion project required a variety of capabilities new to Merck. It called for the building of communications with the world's medical research community, where findings in basic research were laying the groundwork for new drugs. Further, expertise in isolation of active principles from natural products was needed. To lead this effort Karl Folkers was brought in from Yale. With success in isolating natural products that proved medically useful— vitamins and hormones—there arose a need for process development: the conversion of laboratory procedures for preparing mere milligrams of a therapeutic substance into full-scale manufacturing processes. This was the basis for Max Tishler's joining Merck as a process development chemist.

The task that challenged Max was worthy of all his skills. The chemistry of the vitamins and hormones was far more intricate than anything the pharmaceutical industry had ever worked with before. His first assignment at Merck was to develop a new, practical synthesis for riboflavin (vitamin B_2), which is essential for growth and normal health. He worked out an economical, large-scale production process that greatly increased the yield and thus permitted the first use of riboflavin to enrich white bread.

I (Lewis Sarett) doubt whether Max Tishler was surprised by this first of many successes, because he was nothing if not confident. I vividly recall how, when he was in charge of development, he used to tell the basic research staff, "Don't worry about the complexity of the compounds you synthesize. If something is medically promising, we'll find a way to manufacture it."

No substance offered more of a challenge to these bold words than cortisone. The synthesis of the first 16 milligrams of this hormone in 1944 opened a slender path through which, in principle, practical quantities could be made. One hundred grams were eventually synthesized—enough to distribute to various clinicians for experimental studies. In 1948 Philip Hench at the Mayo Clinic discovered that cortisone had a unique effect on inflamed joints in an arthritic patient. Suddenly, a pressing public demand for thousands of kilograms exploded within the tranquil setting of the little company in New Jersey.

An awesome challenge confronted Max Tishler. On the one hand the existing process could yield only small amounts of cortisone—and those at a high cost. On the other hand, concerns voiced in the U.S. Congress carried the implicit threat that the government might move to take over cortisone as a national project. George Merck had responded by assuring the Congress that his company was fully capable of meeting its obligation to the countless patients who might benefit.

The burden of fulfilling that pledge fell squarely on Max's shoulders. The yield from deoxycholic acid as starting material was minuscule—a fraction of 1 percent. The supply of this starting material, a component of cattle bile, was insufficient for the projected demand. Another of the many problems was that osmium, which was used in the original synthesis, would have been required in quantities exceeding the available supply in the United States and perhaps even the world.

Under intense pressure from the medical community— not to mention the U.S. government—Max Tishler put together, first, a team of capable and strongly motivated synthetic chemists; and second, within an astonishingly short time, a practical process for large-scale production of the desperately needed hormone. As a result, the black-bordered insert expressing regret at the limited availability of cortisone that accompanied Merck's first announcements of the compound's medical utility, gradually disappeared. Max Tishler and his team came through with the most complex manufacturing process ever undertaken in the pharmaceutical industry.

Watching all this from up close, I (Lewis Sarett) learned more about pharmaceutical research from Max than from anybody else. I've never known anyone like him. He was born with an energy level that was like an avalanche and a brain that was incandescent, just scintillating. The combination of energy and ability was extraordinary. Partly because of that, and partly because Max worked really incessantly—from very early in the morning fill late at night—he was able to do things that other people couldn't do. He had a fertile imagination and whatever he was interested in, he managed to do.

Even so, Max sometimes had to deal with the frustration and adversity that is part of any sustained research project. A longtime associate has told how he responded to such challenges:

Max was driven to do things well, and he could not tolerate problems not being solved. That made it all exciting. He was utterly fearless in the face of trouble and actually *impatient* to hear *all* the bad news—all the failures of good ideas, or setbacks from whatever source. Unlike most of us, who seem to need a little time to face up to reversals, he never even blinked. To sweep the bad news under the rug, even briefly, simply was not in him. This wholly admirable trait caused not a little grief to those of us with enough pride to want to clean up our own disasters, but it sure taught us to do it quickly!

But Max's successes far outweighed the reverses and brought him broad scientific recognition. This was symbolized by his election in 1953 to the National Academy of Sciences, an unusual distinction for a scientist in industry.

Also, inevitably, his achievements brought him a series of promotions. In 1957 he was chosen to head Merck's entire research and development effort. John T. Connor, then president of Merck, recalls that event:

As I saw it, what we needed was a research director who would manage the whole research program, not doing much research work directly himself, but setting up projects and putting people in charge of those projects in a great variety of fields, and giving general guidance and supervision and ideas for their work as it went along. Our choice was Max Tishler. . . . Max turned out to be the consummate leader. He was inspirational, he was aggressive, he was brilliant, he was helpful to his research associates—altogether unbelievable.

So, Max had to adjust to becoming primarily an administrator, not an easy transition for most hands-on scientists. He knew—and each of the chemists in his organization knew—that when he delegated a task, he could carry out that task better and faster himself. I (Lewis Sarett) do believe Max tried to delegate as far as he could, but it came very hard to him. He thought so fast about so many things that he could work out the answer almost as soon as he assigned a problem to somebody.

That made him a tough man to work for—not patient. But though he might blow up at a colleague's shortcomings, then in the afternoon or evening he would call up and apologize. Anyway, the criterion I had for doing a job for Max was to ask myself, "If Max were doing this, would he do it better than I'm doing it?" and I always had to answer, "Yes." So that kept me from boiling over.

But Max generally resisted any temptation to micromanage. Looking back over the years, one of his associates had this to say about his management style:

Max made very fundamental decisions about what to do and what not to do, but I don't think he ever dictated any of the details on what to do. He just might say, "Now, you are going to work on streptomycin. You're going to get it out. You are going to make 1,000 kilograms and you're going to have it by November 1."

To offset his demanding style Max had a personal relationship with almost everyone he dealt with. He was interested in children, domestic problems, and so on in a way that very few people were. Even when he had as many as

1,800 people under his direction, he used to say, "I have to think about 1,800 families."

A top Merck executive, who knew him for many years, remembers: "Max Tishler had more one-on-one relationships with people at all levels of the research laboratories than would have seemed believable to an outsider."

It truly might be said that Max's main interest was not chemistry, but chemists. Thus, he was able to bring out the best in his fellow scientists—now by a word of encouragement, now by suggesting a change of course, now by his compassionate concern for personal problems, and always by listening.

If scientists were growing discouraged over repeated research failures, Max would help them—not necessarily by finding the solution to the problem, but perhaps by causing them to think of a new way to look for it. If a parent wanted help for a son or daughter seeking admission to college, Max did what he could. When a Merck scientist's spouse or other relative had a serious medical problem, Max would learn about it somehow. Then he gave wise counsel, made phone calls, and took whatever other action might be necessary to provide support and hope.

While his disregard for textbook administrative practices endeared Max to the scientists concerned, it put severe demands on his time. It also put a premium on concise presentations. Visitors to his office would find that unless they covered the ground quickly, Max would glance pointedly—and disconcertingly—at his wristwatch, knowing that several consultees were awaiting their turns.

Professor Donald Cram, who spent a short period in the Merck research laboratories working on the penicillin project, has quoted from his first interview with Max (*Chemtech,* December 1986, p. 712):

Tishler: So you're interested in doing research? What can you do?

Cram: In my master's work at Nebraska, I worked on rearrangements of. . . .

Tishler: What is the base-catalyzed condensation of benzaldehyde and acetophenone?

Cram: Benzalacetophenone—I made a ton. . . .

Tishler: Why are you here without your Ph.D.?

Cram: My draft board told me to leave school and get a job to aid the war effort. I fully intend to return to. . . .

Tishler: As far as I am concerned, you are hired.

This vignette, presented by Cram as part of Max's eightieth birthday celebration, was absolutely typical. He seemed to anticipate what chemists were going to say before they could get the words out.

With other, less familiar disciplines, however, he was simply filled with intense curiosity. One evening a Merck executive was seated at a ceremonial dinner next to an eminent professor of pharmacology. As the meal continued, the executive found himself falling behind the other guests. The reason was that as he lifted his fork to his mouth his neighbor kept asking question after question. Eventually the professor relented and explained. He was simply doing to this Merck executive what Max Tishler had done to him earlier that year.

As the reader may have gathered, Max Tishler was uniquely curious, gifted, and impatient. As such, he was sympathetic to kindred souls. He did not care for the usual round of polite interviews, striving to determine how well a candidate scientist might fit into the organization. The recommendation most compelling to Max would seem to be: “Although this man gets along with almost no one, he is the brightest scientist we've seen in some years.”

Max had intense, one might say puritanical, views on right and wrong. He preferred black and white to shades of gray. Thus, more than thirty years ago, when the scientific and therapeutic achievements of the pharmaceutical industry were called in question by a few witnesses who appeared before the Kefauver Committee, Max—like many other scientists—was outraged by what he felt was unfair criticism based on distortion of the facts. Feeling that objective and unimpeachably authoritative observers would agree with him, he conceived the idea of asking such observers to recognize publicly the contributions the industry had made to saving life and protecting health.

Max had a proposed statement lettered on a scroll and hand-carried by a personal courier to fourteen Nobel Prize winners in medicine and chemistry. The scroll, which all of them signed, resides to this day in the Merck archives. It says, in part:

The scientists in the laboratories of the pharmaceutical industry have in fact become partners in the total research effort, frequently initiating fundamental research, still more frequently associating with scientists in universities and elsewhere in a joint endeavor. We find in these men true collaborators. . . . We believe it is important to record publicly our recognition of the many significant contributions made by the research laboratories and scientists of this industry to the progress of medicine.

It was remarkable for such eminent scientists to become involved in the highly politicized healthcare debates of those days. Their willingness to do so is a striking example of the scientific community's respect for Max Tishler.

The same moral commitment to defend scientific truth as he saw it gave birth to a 1973 book co-edited by Max and his friend and fellow chemist Milton Harris, *Chemistry in the Economy.* Here again, by setting forth the benefits that chemistry confers, he sought to counterbalance the intense criti cism that chemistry was receiving for various sorts of toxicity.

Max's career kept rising, and in 1962 he was elected to the Merck board of directors. His research budgets continued to rise as valuable new products emerged from the laboratories. Under Max Tishler's overall leadership, Merck chemists, biologists, and clinical investigators discovered, developed, and obtained regulatory approval for a series of drugs and vaccines, which in many respects revolutionized the practice of medicine and healthcare throughout the world. Among these were many vitamins essential to life and growth; cortisone and other steroids; drugs effective against high blood pressure and congestive heart failure sure as chlorothiazide, hydrochlorothiazide, and later methyldopa; indomethacin, the first clinically important non-steroidal anti-inflammatory agent; antidepressants; vaccines against measles, mumps, and rubella; and animal health drugs such as the coccidiostat sulfaquinoxaline and the anthelmintic thiabendazole.

Max's career took a new turn in 1969 when he was promoted from the research division to the newly created corporate position of senior vice-president for science and technology. But, isolated from his many personal research projects and the scientists who headed them, he began to feel out of his element and restless.

Thus, in 1970, eighteen months before mandatory retirement, he accepted an invitation to become professor of chemistry at Wesleyan University in Middletown, Connecticut. There, Max found himself again in the midst of scientists, students, and research ideas. He played a leading role in developing a Ph.D. program in chemistry, which added a new dimension to the Chemistry Department. He took on graduate students and was a mentor—in the best sense of the word—to them and countless other younger scientists.

In addition, he created and organized the annual Peter A. Leermakers Symposium in Chemistry. This has become a major event in the American chemical community, bringing internationally renowned chemists and an audience of hundreds of scientists to the Wesleyan campus each spring. In his spare time he continued his lifelong hobby of growing many species of cacti, orchids, and other exotic plants.

Before long he became University Professor of the Sciences and chairperson of the Chemistry Department. Even after reaching emeritus status in 1975 he taught courses in medicinal chemistry and remained extraordinarily active in research. Until only a few weeks before his death he was involved in all phases of departmental activities and continued to advise and encourage graduate and undergraduate students, with whom he was enormously popular.

Remarkably, in the midst of all his university activity, he found a way to contribute another pharmaceutical product to Merck. One of Max's students had been Satoshi Omura, now professor and executive director of the Kitasato Institute in Tokyo. The institute's microbiologists produced certain fermentation broths and, at Max's suggestion, these were screened at Merck for possible antiparasitic activity. Activity was indeed detected, and this quickly led to the avermectin family of compounds, which have proved effective not only against a wide variety of internal and external parasites of animals but also against the fly-borne parasite that causes onchocerciasis (river blindness) in people in many tropical countries, primarily in Africa.

Many honors came to Max. Besides those previously mentioned, he received the Priestley Medal (the American Chemical Society's highest honor) and the Eli Whitney Award for Inventions. He was elected president of the American Chemical Society in 1972, during a critical period in the organization's history.

Introducing him at his induction into the Inventor's Hall of Fame in 1982, I (Lewis Sarett) pointed out:

One might say that Max Tishler invented the term “developmental research.” Early in his career at Merck, he recognized that there was a need for basic chemical studies in process development. He put “research” into “development.” Although this is common practice today, it was a new concept at the time and had a profound impact on biomedical and pharmaceutical research.

This memoir has described how Max Tishler earned his secure, honored, and enduring place as a true pioneer in the history of chemistry. But, it was typical of the man that he preferred to measure his accomplishments by their impact on people. Interviewed some years ago, he answered a question about what he considered the most important contributions that Merck—and Max personally—had made to society:

I think we saved the lives of a lot of people, contributed to the control of disease, and made life more pleasant. That has given me the greatest pleasure. I can't say which development was the greatest thrill for me: cortisone development, streptomycin development, or penicillin development. That would be like choosing which of your twelve children you like best. Each one has had an impact on me.

Consider, for example, a commercially unimportant drug that I helped to develop—namely actinomycin, an organism that Dr. Selman A. Waksman discovered. This turned out to be an important compound useful for treating a very rare form of cancer, called Wilms' tumor, which afflicts children. The number of cases that occur each year is not large, but for the individual children and their families the drug is vitally important. The late Dr. Sidney Farber, a great pathologist who set up the Dana Farber Institute in Boston, once invited me to come up and see some of the children who had been getting actinomycin. He introduced me to half a dozen who had been treated with actinomycin five years earlier. They looked robust, and were considered to be permanently cured. . . . It makes everything worthwhile when you see things like that.

Even with so many good things to reminisce about, Max was never one to dwell in the past. In the same interview he went on to say, "I wish I were twenty-five years younger. I think there's great excitement ahead." That is the questing, ever-curious Max whom I will remember best.

Max Tishler was eighty-two when he died, of complications of emphysema, in Middletown, Connecticut. One of his friends at Merck summed up the feeling of many who had known him: "I think about Max frequently. He was such a nice combination of very gifted, very conscientious, and very human."

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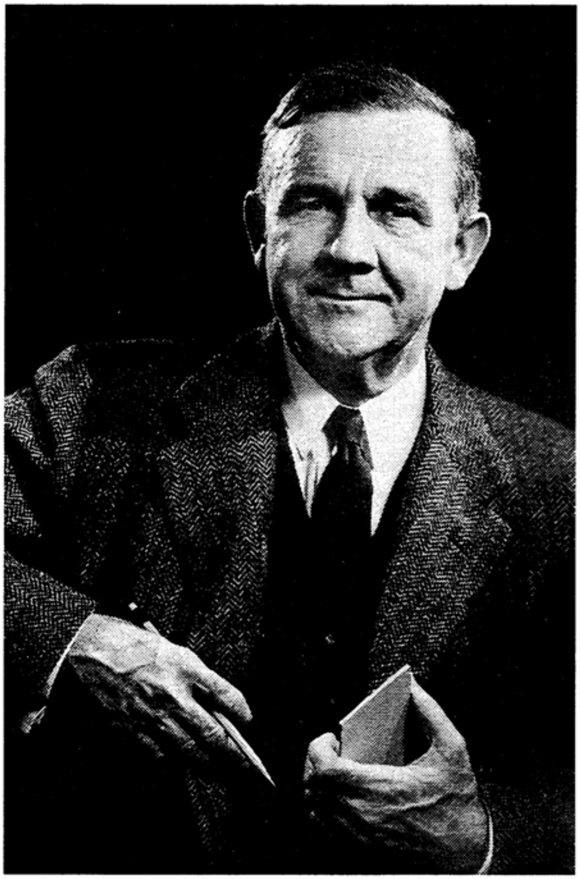

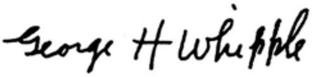

GEORGE HOYT WHIPPLE

August 28, 1878–February 2, 1976

BY LEON L. MILLER

I FIRST MET GEORGE H. WHIPPLE in December 1938 when he interviewed me for the position of research fellow biochemist to work with him and other members of the Department of Pathology. I was impressed by his soft-spoken, taciturn manner and by his air of friendly reserve. Although I spent more than eight years in Whipple's laboratory working with him and others in many of the ongoing research problems, I came to realize that I had learned very little about Whipple personally. During department conferences or in small research meetings he stuck closely to the case discussions at hand or the research data being presented. He did not encourage wide-ranging discussion or sharp differences of opinion that might provoke controversy. However, he never discouraged a young worker from doing an experiment to test his own ideas.

To learn some little about Whipple's personal life and feelings one must read the definitive biography, *George Hoyt Whipple and his Friends*, published in 1963 and written by George W. Corner after more than fifty years of close association and friendship. Whipple's autobiography in *Perspectives in Biology and Medicine* (1959) leaves one with the feeling that he did not enjoy talking or writing about himself. I cannot recall a single instance where Whipple spoke in praise of his own work or ideas.

Whipple invited familiarity neither from junior colleagues nor research collaborators; nor was he given to small talk unless it touched on hunting, fishing, or baseball. Corner said Whipple saw no fun or value in talking about something he could not expertly comprehend.

George Hoyt Whipple was a pathologist who managed to combine in one lifetime the activities of three careers of distinction—one as founder and longtime dean of the School of Medicine and Dentistry at the University of Rochester, another as a devoted and inspiring teacher of medical students and as mentor of young pathologists, and a third as an internationally recognized medical researcher who made substantial contributions to several areas of research in experimental medicine. Best known perhaps for his studies in experimental anemia, he was honored with George Minot and William Murphy as co-winner of the Nobel Prize in Medicine or Physiology in 1934. In spite of these and many other honors, George Whipple in his brief autobiography said, “I would be remembered as a teacher.”

THE EARLY YEARS (1878-1900)

Born in 1878 in the village of Ashland, New Hampshire, George Hoyt Whipple was the only direct male descendent of two New Hampshire country doctors, Solomon Mason Whipple, his grandfather, and Ashley Cooper Whipple, his father. When George was only two years old, his father suffered an untimely death from pneumonia; George and one small sister were left to be raised by his mother Frances Anna Hoyt Whipple and his maternal grandmother Frances Moody Hoyt. They indelibly impressed upon him the virtues of thrift, frugality, modesty, and work. They were largely responsible for insuring that George received a sound early education and preparation for college at Phillips Andover Academy. There he showed an aptitude for the sciences and mathematics, but came to regard languages, including Greek and Latin, as necessary but uninspiring drudgery.

As a boy, George learned to love the great outdoors and developed a fondness for hunting and fishing, which remained with him for the rest of his life. During the summer vacations of his prep school and college years he worked at a variety of jobs, mostly providing help and service to summer tourists and campers on Squam Lake and Lake Winnepesaukee in New Hampshire. That work afforded him opportunities for dealing responsibly with people and yielded earnings, which he carefully husbanded for his college and medical school expenses.

As far back as he could remember George Whipple tacitly anticipated that, like his paternal forebears, he would become a physician. Apparently his mother encouraged him in this ambition, adding some financial support from a small inheritance and influencing his choice of Yale as the college for his premedical studies.

As an undergraduate at Yale, George distinguished himself not only as an outstanding student of the sciences, but also as a prize-winning gymnast and oarsman. In his senior year Whipple fell under the influence of the nutritionist Russel H. Chittenden[1] and the physiological chemist Lafayette B. Mendel.

In his autobiography Whipple refers to Mendel as “an unusual man who exerted a strong influence on me. Work with him was exciting and never to be forgotten.” Whipple's potential as a researcher was recognized by his election while at Yale to membership in Sigma Xi and by his graduation in 1904 with senior honors.

Realizing that he did not have enough money to pay for his planned medical education, Whipple took a year off to work at Dr. Holbrook's Military School in Ossining, New York, where he taught mathematics and science and served as athletics coach.

In choosing Johns Hopkins as the school for his medical education George Whipple was significantly influenced by his mother. She had learned about the university's outstanding teachers, who were interested in research, and how unique Hopkins was in that respect. It had an excellent library and an associated hospital organized to foster clinical teaching. Furthermore, Hopkins was the only medical school in the United States that required a bachelor's degree and a knowledge of Greek, Latin, French, and German for admission.

JOHNS HOPKINS YEARS (1901-14)

In his first year at Hopkins, Whipple's comparatively extensive training in physiological chemistry at Yale qualified him to apply for a student teaching assistantship in John J. Abel's Department of Physiological Chemistry. This not only afforded him some much needed financial support, but gave him an opportunity to savor the excitement and general spirit of zealous interest in the new developments in medicine in which Johns Hopkins had become the leader in America.

As a medical student at Hopkins, Whipple was typical in being self-motivated to study and work hard and to follow Osler's advice “to keep their hearts on ice” and avoid amorous distractions. His performance in his first year anatomy course was outstanding enough to win him a second-year appointment as a student instructor in anatomy. During that year Whipple's training was dominated by his introduction to pathology, which fascinated him in all its aspects. The exemplary leadership of William Welch, Eugene Opie, and William McCallum—inspiring teachers and scientists who correlated clinical illness and disease and the findings at autopsy and under the microscope—all engaged Whipple's lasting interest. At the same time he was deeply impressed by the array of unsolved problems, some of which would later incite him to firsthand research.

Standing fourth in his class of fifty-four students, Whipple was eminently qualified for one of the choice internships in medicine, which included pediatrics; however, fate intervened when a junior member of the Pathology Department was about to leave Hopkins, and McCallum, acting as Welch's agent, offered the post to Whipple, presumably to concentrate on pediatric pathology. That year in pathology was supposed to prepare Whipple for an anticipated career in clinical pediatrics, however Whipple was so thoroughly entranced by his experience in all aspects of the work in pathology that he sought a second year's appointment from Welch. Welch gladly accepted him and ventured to predict that a second year in pathology would inevitably lead him to a career in pathology.

Corner characterized Whipple in his youth as “a tall, handsome young man, respected by all his acquaintances. With his reserve and plain speaking went fair dealing and courtesy. He held his own views without vanity or arrogance, but with tenacity, and was not to be jostled from his place by authority.” Corner adds that Whipple “did not make friends easily, but the friendships he made lasted for life.” Corner quotes Peyton Rous, who, while both were at Hopkins, said in comparing Whipple with himself, “George had a granitic aspect and repose, whereas I was overactive and a volatile talker.”

In 1907 after two years of assistantship Whipple embarked on his career as a pathologist and was encouraged to undertake research in descriptive or experimental pathology. In those first two years he proved in two papers that he had developed his skills as a keen and careful observer. In the first paper he compared the apparent role of the lungs and the lymphatics with that of the gastrointestinal tract in the spread of the tubercle bacillus, as observed in a long series of autopsies of humans dead of tuberculosis. In the second paper he described in detail the results of an autopsy on a thirty-seven-year-old physician dead of a previously undescribed condition that Whipple designated lipodystrophia intestinalis, characterized by the accumulation of granular material (staining for fatty acids) in the walls of the small intestine and lymph nodes. Later observers recognized the novelty of Whipple's observations and named it Whipple's disease. They characterized his original description as “a classic of clarity, objectivity, and completeness.”

In 1908 with the encouragement of Welch, Whipple went to the Gorgas Hospital in Panama to work for a year with Samuel Darling, the resident pathologist. This broadened Whipple's experience to include the ravages of tropical disease and provided him an opportunity to study and report some of his observations on the massive hemolysis of black-water fever. The salary for the work in Panama allowed him to travel to Europe before returning to Hopkins. He spent several months at Heidelberg in the laboratories of Krehl and Morawitz, where he saw a first-class European laboratory in action, and participated briefly in some studies involving the experimental production of anemia in rabbits.

Upon his return to Hopkins in 1909, Whipple focused his efforts on the pathologic disturbance of function such as that associated with acute chloroform poisoning and liver injury in the dog, as described by Howland and Richards. Although Whipple and King failed to produce experimental cirrhosis in the dog by inflicting repeated episodes of chloroform liver injury and necrosis, they observed and recorded the increased bleeding tendency and jaundice and measured decreases in fibrinogen levels in their dogs. Fur thermore, they correlated these changes with the severity of histologically demonstrable liver injury and necrosis and they were quick to suggest that the liver was the site of fibrinogen synthesis.

Whipple's early interest in the pathogenesis of jaundice led him to submit *An Essay on the Pathogenesis of Icterus* in the blind competition for the Warren Triennial Prize of the Massachusetts General Hospital; Whipple was declared the winner of the prize in April 1910. This immensely pleased his department chairman Welch and added considerably to Whipple's growing renown. Soon thereafter he was offered professorships at the University of Pennsylvania and the University of California schools of medicine. He chose to turn them down and in 1911 was appointed an associate professor of pathology at Hopkins.

Whipple spent the spring and summer of 1911 in Vienna in the laboratory of Professor Hans Meyer; there he learned how to produce the experimental porto-caval shunt in the dog known as the Eck fistula. Using this technique in later years Whipple was able to study the effects of totally diverting the hepatic portal vein blood flow on a number of hepatic functions in the dog.

During the period 1907-14 of his professional maturation as a pathologist, Whipple's research interests clearly shifted from studies primarily concerned with histopathologic anatomy to problems in which altered functions could be studied with the tools of biochemistry and physiology. Thus, during the last several years at Hopkins with the collaboration of Charles W. Hooper,[2] Whipple started a long series of studies in dogs on the origin and excretion of bile pigment and on icterus as one manifestation of impaired hepatic function. These studies were briefly interrupted in the spring of 1914 when Whipple, at age thirty-four, married Katherine Waring and accepted an offer to become professor of experimental medicine and head of the newly established Hooper Foundation for Medical Research at the University of California School of Medicine in San Francisco.

THE SAN FRANCISCO YEARS (1914-21)

In spite of the many difficulties of establishing a totally new laboratory Whipple with the continuing collaboration of C. W. Hooper, who accompanied him in the move from Hopkins, managed to continue his research on bile pigment metabolism, culminating in a series of twelve publications between 1915 and 1917. Altogether these studies established the following important facts concerning the origin and excretion of bile pigments:

a) That the bile pigment bilirubin was derived not only from the breakdown of red cell hemoglobin, but also from muscle hemoglobin.
b) That neither the bilirubin in fed bile nor the bilirubin derivable from the heme of fed hemoglobin gave rise to a measurable increase in the bile pigment secreted in bile fistula in dogs. These studies finally refuted any speculation that bile pigment might be reabsorbed and reutilized in the production of new red cells.
c) That the heme moiety of hemoglobin could be converted to bilirubin in both the pleural and peritoneal cavities as well as in the liver.
d) Reemphasized that normal liver function was essential for the excretion of bilirubin.

In the course of studying bile pigment metabolism and recognizing that blood red cells were the major normal source of bilirubin, Whipple and Hooper studied the effects of acute hemorrhagic anemia and diet composition on bilirubin excretion and shifted the emphasis of their research to the study of the regeneration of red blood cells in simple anemia. In 1918 they published the first of a long series of papers in which the curve of red blood cell regeneration was described as influenced by dietary factors. This was followed by a second report on the curve of regeneration as influenced by starvation, sugar, amino acids, and other dietary factors. Concurrently, with the collaboration of W. H. Kerr and S. H. Hurwitz, Whipple was making the first of what would become a long series of studies on the regeneration of the blood serum proteins and they recorded the effects of fasting on the curve of protein regeneration. This was followed by studies on the influence of diet on the curve of regeneration after plasma depletion.

Although these early studies on blood and plasma protein regeneration pointed to dietary factors having important influences on quantitative changes in blood and plasma protein regeneration, it became apparent that accurate reproducible measures of blood and plasma volumes would be essential before accurate estimates of the total circulating mass of blood and plasma proteins could be made. This led to the development by Hooper, Smith, Belt, and Whipple of reproducible dye dilution methods for the quantitative estimation of plasma volume.

In 1921, two years after Whipple had been appointed dean at the University of California School of Medicine in San Francisco, he received an offer from the president of the University of Rochester to come to Rochester, New York, to plan, organize, and lead a new school of medicine as dean and as chairman and professor of pathology. With his research program in full swing, Whipple was reluctant to leave California, but Rush Rhees was not to be deterred. He went to San Francisco personally to encourage Whipple to reconsider. As dean at Berkeley, Whipple was not happy with the physical separation of the faculty and students during the first two preclinical years in Berkeley from the clinical staff and hospital in San Francisco. Whipple was finally won over by recognizing that Rhees' offer was a rare opportunity to create a medical school from the ground up, with a full-time faculty in a physical setting conducive to easy exchange between clinical and preclinical disciplines. In addition, Rhees reassured him that in Rochester he would not be expected to participate actively in the social life or community service, which might detract from his total commitment to the teaching and research functions of the medical school.

Whipple's ongoing research programs at the Hooper Institute continued, with the friendly cooperation of all concerned at Berkeley, without interruption until late in 1922, when the first building was completed at the new medical school in Rochester.

THE ROCHESTER YEARS (1921-77)

When Whipple's research technician Frieda Robscheit-Robbins arrived in Rochester in December 1922 with forty of her special strain of dogs, their research studies were soon resumed and between 1921 and 1925 there were published several series of papers dealing with:

a) Determination of circulating plasma and hemoglobin volumes;
b) Dietary and other factors affecting bile salt production and secretion;
c) Measurement of blood fibrinogen and the effects of diet, hemorrhage, liver injury, and other factors on plasma fibrinogen levels;
d) Roentgen ray intoxication in dogs (these papers with Stafford Warren were regarded twenty-five years later as clas

sical descriptions of the anatomic and functional effects of radiation injury);

e) Blood regeneration following simple anemia (a series of six papers sought to evaluate the effects of varying diet composition on hemoglobin regeneration in simple hemorrhagic anemia and demonstrated that blood hemoglobin levels in the dog could be completely controlled by diet).

Because there were large differences in responses of different dogs Whipple and Robscheit-Robbins were able to obtain quantitatively more consistent results by establishing more rigorously standardized control conditions. These involved the use of:

a) Their specially bred strain of Dalmatian-English bull dogs, which, though not genetically pure, were closely similar in appearance;
b) Prior bleeding to produce a standard anemia of about 40-45 percent of normal, which could be maintained for weeks at this level with minimal further bleeding while the dogs were fed a basal diet, adequate to maintain their weight and health, but affording only a variably small hemoglobin regeneration of 1-3 grams per week;
c) Systematic and accurate measurements of hemoglobin levels and circulating volume in response to diet supplements of specific foods, inorganic salts, or drugs.

Thus, in 1925 Whipple and Robscheit-Robbins published the first of what was to become a series of eighteen papers on “Blood Regeneration in Severe Anemia.” G. W. Corner, Whipple's biographer, commented on the second of these papers covering the favorable influence of liver, heart, and skeletal muscle in diet: “This report with its unequivocal emphasis on liver feeding is the most important single pa per as regards George H. Whipple's world reputation as a scientist, in the whole of his immense lifetime list of more than 300 publications." That report, clearly establishing the superior potency of fed liver in promoting the regeneration of hemoglobin in the anemic dog, caught the attention of George Minot in Boston; he and William Murphy were preoccupied with the treatment of humans afflicted with pernicious anemia for which there was at the time no known cure. In a relatively short time they were able to demonstrate conclusively that a diet containing large amounts of raw or cooked beef liver produced phenomenal sustained remissions of pernicious anemia. The effectiveness of liver feeding in the successful treatment of pernicious anemia was soon widely confirmed and recognized internationally.

Between 1925 and 1930 Whipple and Robscheit-Robbins published a total of twenty-one papers describing the use of the standard anemic dog to test a lengthy array of foods of animal and vegetable origin. In general, foods derived from animal tissues as a group were much more potent than foods of plant origin, with one notable exception; cooked apricots were found to be the most potent food of plant origin, surpassing beef heart and beef skeletal muscle in stimulating hemoglobin regeneration. Much effort was also devoted to comparing the effects of fed whole foods with the effects of feeding the corresponding inorganic ash derived from the combustion of the foods. It became clear that the potency of various food supplements was roughly paralleled by their iron content; however, the effects of feeding iron salts alone, or the iron-containing ash of foods, resulted in at most 40-50 percent of the hemoglobin regeneration seen after feeding the whole food supplement.

Several of the twenty-one papers described attempts to fractionate liver chemically by the relatively crude methods then available, with limited success; with the collaboration of the laboratories of the Eli Lilly Company, a liver fraction corresponding to 3 percent of the liver weight showed 65-75 percent of the potency of whole beef liver in the standard anemic dog. Another liver fraction (Eli Lilly Company fraction 343 NNR) tested and found by Minot and Murphy to be potent in treating human pernicious anemia had only 10-20 percent of the potency of whole liver in the response of the standard anemic dog. These observations clearly indicated that the liver factors effective in treating human pernicious anemia were not the same as those responsible for the large hemoglobin response in the standard anemic dog. Thus, there remained the troubling, unanswered question of whether whole liver contained a factor other than iron (in some unusually assimilable form), copper, and other trace metals, which, when fed, may stimulate hemoglobin production in the standard anemic dog.

THE ROCHESTER YEARS (1930-40)

The news of the award in 1934 of the Nobel Prize for Medicine or Physiology to George Minot, William P. Murphy, and George H. Whipple came as a stunning surprise to Whipple, who accepted it with quiet modesty. In explaining the basis for the honor, Professor I. Holmgren speaking for the award committee said, “Of the three prize winners, it was Whipple who first occupied himself with the investigations for which the prize is now awarded. . . . Whipple's experiments were planned exceedingly well, and carried out very accurately, and consequently their results can lay claim to absolute reliability. These investigations and results of Whipple's gave Minor and Murphy the idea that an experiment could be made to see whether favorable results might also be obtained in the case of pernicious anemia, an anemia of quite different type, by making use of the foods of the kind that Whipple had found to yield favor able results in his experiments regarding anemia from loss of blood."

George H. Whipple's life in Rochester was dominated by total commitment and devotion to his work as dean, as department head, as teacher of pathology, and as medical researcher. During the work week he was always available and he routinely appeared in the pathology laboratories on Saturday and Sunday mornings. After he became well known he frequently declined invitations to travel and talk at other institutions or meetings. However, he found time to serve as a member of the board of trustees of the Rockefeller Foundation from 1997 to 1943. There his presence was described as quiet and unobtrusive, but his quietly voiced opinions were highly regarded and respected. His contributions to the board were so highly valued that he was invited to join the trustees of the general education board of the Rockefeller Foundation, on which he served from 1936 to 1943. In 1936 after declining the directorship of the Rockefeller Institute for Medical Research, he was elected a trustee and member of the board of scientific directors of that institute (1936-43).

During this decade Whipple continued to use the standard anemic dog to explore several questions bearing on the metabolism and synthesis of hemoglobin. By taking advantage of the infection-free bile fistula dog, William B. Hawkins and Whipple were able to resolve previous inconclusive observations about the reutilization of the pigment moiety of parenterally administered dog hemoglobin in the production of new red blood cells. It became clear that even in the standard anemic dog, with its maximal stimulus for hemoglobin production, the pigment moiety of parenterally given hemoglobin was not reutilized; rather it was excreted in the bile as completely in the form of bilirubin as in the comparable nonanemic state.

In 1937 Hawkins and Whipple used the bile fistula dog to determine the average life span of the red blood cell in the dog by timing the large increase in bile pigment that occurred at about 124 days after the acute massive regeneration of red cells in response to the massive hemolysis induced by the administration of phenylhydrazine. Interestingly, Whipple in 1926 was aware of the estimate of 110 to 140 days for the life span of the red cell made by A. Lichtenstein and A. J. L. Verwer, which was based on quantitative measurements of urobilin excretion; however, Whipple categorically stated, “This accuracy (in measuring urobilin) was praiseworthy but they contributed nothing to our knowledge of the life cycle of red cells. . . .”

In another study with F S. Daft and Robscheit-Robbins published in 1935 Whipple compared hemoglobin production in the fasting state with production during feeding of sugar only. These studies were documented by measurements of urinary nitrogen partition and nitrogen balance. The results supported the view that, even during total starvation, small but significant amounts of hemoglobin were produced and that hemoglobin production was substantially improved by sugar feeding associated with a marked reduction in urinary nitrogen excretion. In discussing their results the authors repeated an idea expressed by Davis and Whipple fifteen years earlier, when they sought to explain the occurrence of extensive liver cell regeneration during the starvation or sugar feeding after chloroform-induced liver necrosis, viz. that those results pointed “to intriguing possibilities of exchange between various body proteins and hemoglobin or other proteins according to the physiological needs of the moment.”

In the early 1930s Whipple also returned to the object of his work with Kerr and Hurwitz described in 1918, viz. the regeneration of the plasma proteins, and initiated a long series of studies utilizing a standardized hypoproteinemic dog (with total plasma protein level of 4.0 grams) maintained on a standard low protein diet of known composition with plasma protein levels controlled by routine plasmapheresis (the bleeding of known measured volumes of blood followed by the intravenous return of the removed red cells after their separation by centrifugation and washing). Measurement of the plasma volume and total plasma protein removed was used to determine the effectiveness of a particular protein food supplement over the course of a week's test interval.

While the plasmapheresis studies were demonstrating the importance of the qualitative and quantitative character of dietary protein on production of plasma proteins, Whipple and his colleagues were finding that dog plasma proteins given intravenously or intraperitoneally along with a nonprotein diet could maintain dogs in weight and nitrogen balance. The results of these studies, along with those obtained earlier on liver cell regeneration after chloroform liver injury and necrosis and those on hemoglobin regeneration in the fasted anemic dog, all led Whipple to propose his hypothesis of "the dynamic equilibrium between blood and tissue proteins." I believe this was conceptually his most important contribution to our understanding of the fundamental character of mammalian protein metabolism. His astute inferences were soon extended and definitively documented in detail after the advent of the isotopic era in the late 1930s. The successful preparation of useful quantities of deuterium (^{2}H) and heavy nitrogen (^{15}N) after Harold Urey's Nobel Prize-winning discoveries led to the classical work of Rudolf Schoenheimer, Sarah Ratner, and David Rittenberg, on the basis of which Schoenheimer wrote the concise classic *The Dynamic State of the Body Con* *stituents* and ushered in the modern era of biochemistry and biology.

The discoveries of artificial radioactivity by Joliot Curie and Fermi and the invention of the cyclotron by Ernest O. Lawrence with its capability of producing useful amounts of radioactive iron (^{59}Fe) permitted Whipple, Paul F. Hahn, and William F. Bale to begin in 1937 a critical examination of the nature of iron absorption and utilization. In a short time they were able to define the unique character of iron as the one essential food factor where the amount absorbed from the small intestine was rigorously controlled and reflected the state of (iron) stores in the body. It became clear that only insignificant amounts of iron were found normally to be excreted or lost in the urine, feces, or bile. Iron absorption from the gastrointestinal tract of the nonanemic dog was found to be correspondingly minimal, however, with severe hemorrhagic anemia and iron deficiency; iron absorption was found to be substantially increased until bodily iron stores were replenished.

THE WAR YEARS (1940-50)

Between 1939 and 1943 Leon L. Miller and Whipple examined the effects of protein depletion on the susceptibility of dogs to the hepato-toxic effects of chloroform anesthesia. They found that protein depletion was associated with severe or lethal liver injury after as little as fifteen minutes of anesthesia, while the dog normally nourished with protein could sustain one hour of anesthesia with little or no injury. Feeding a protein-depleted dog a single large protein meal or its equivalent content of L-methionine or L-cystine shortly before anesthesia completely prevented liver injury after fifteen minutes of chloroform anesthesia. Shortly thereafter William Hawkins, Phyllis Hanson, and Whipple were able to demonstrate an analogous increase in sensitiv ity to arsphenamine liver injury in protein-depleted dogs that could be prevented by prior feeding of protein or the sulfur-containing amino acids. As with chloroform liver injury, arsphenamine liver injury was not prevented by nonsulfur-containing amino acids. This led Whipple to emphasize the importance of body protein stores and their content of S-containing amino acids in protecting the liver against toxic agents, but the exact mechanism of the protective action was not established.

In 1940 Sydney C. Madden and Whipple reviewed eight years of their work and that of others on the plasma proteins; they emphasized the presumptive role of the liver as the site of plasma protein synthesis and were able to confirm for plasma protein synthesis in the dog the dietary essentiality of those amino acids found by William C. Rose to be indispensable for growth in the rat.

During World War II, with the collaboration of Merck & Co. Inc. in supplying pure amino acids, Madden et al. formulated a number of pure amino acid mixtures and tested them for their effectiveness in promoting synthesis of plasma proteins when given orally or parenterally. When given either orally or parenterally with an adequate intake of nonprotein calories several of the amino acid mixtures could completely satisfy all the metabolic requirements for maintenance of weight and nitrogen balance in the dog and at the same time support ample plasma protein and hemoglobin regeneration. This work was also important because it led to the demonstration at Rochester in a few human subjects that a mixture of the essential amino acids, or an enzymatic digest of casein containing all of the amino acids, when given parenterally along with adequate nonprotein calories, could maintain positive nitrogen balance for several days. These results were independently confirmed by Robert Elman et al. in St. Louis and were important be cause they showed the feasibility of total parenteral alimentation and emphasized the critical requirement for nonprotein calories at the same time. Total parenteral alimentation is now routinely carried out in many hospitals and on an ambulant basis to provide lifesaving nutrition to patients who are unable to be nourished by the normal gastrointestinal route for long periods of time.

Between 1943 and 1955 Whipple and his group also carried out studies of plasma protein and hemoglobin production, using first ^{15}N-labeled lysine, and later ^{14}C-labeled lysine; these studies led to the following important conclusions:

a) There was more direct confirmation of the labile exchange between reserve protein stores and circulating plasma proteins and a reaffirmation of Whipple's view: “This adds up to a dynamic equilibrium in body protein production, storage, and utilization or exchange.”
b) The intramedullary interval for the synthesis and release of labeled hemoglobin containing red cells was three to five days in the dog. Once released into the circulation red cells have a normal life span of 110-130 days and neither the iron nor protein moieties of their contained hemoglobin undergoes metabolic exchange during the life span of the red cell.
c) Isotopically labeled red blood cells or plasma proteins given intraperitoneally to dogs rapidly appeared intact in the circulating blood.
d) After creating experimental ascites in the dog secondary to constriction of the vena cava, Frank McKee, Charles Yuile, and Whipple demonstrated a very rapid turnover of isotopically labeled plasma proteins in what appeared to be a large static accumulation of ascitic fluid in the peritoneal cavity.

RETIREMENT YEARS (1952-75)

At age seventy-five in 1953, after more than thirty years as a professor of pathology and dean of the school of medicine, Whipple relinquished the deanship to Donald G. Anderson and two years later retired. In retirement he continued to keep in touch with the activities in the Pathology Department and medical school, but allotted time to enjoy pheasant hunting, salmon fishing in Nova Scotia on the Margaree River, and fishing for tarpon off the Florida coast. In spite of his honors, prizes, medals, and international recognition, George H. Whipple said in the closing words of his modest autobiography, "I would be remembered as a teacher."

NOTES

1. In major respects George H. Whipple's research on the dog over fifty years amply substantiated Chittenden's largely intuitive generalization to the effect that: "It is one of the axioms of physiology that the majority of the diseases of mankind are due to or connected with perversions of nutrition. . . . Broadly speaking, the extent and character of the metabolic processes of the body are dependent in large measure on the amount and character of the diet. Furthermore, it is equally certain that the chemical composition of the blood and lymph is quickly affected by the amount and character of the food materials absorbed from the alimentary canal." Russel H. Chittenden. *Physiological Economy of Nutrition.* New York: Frederick A. Stokes Company (1904).

2. Charles W. Hooper, who received his M.D. degree at Johns Hopkins in 1914, was not related to the George Williams Hooper family of San Francisco. Mrs. George W. Hooper established the Hooper Foundation for Medical Research as a memorial to her late husband who had amassed a fortune in the lumber business.

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