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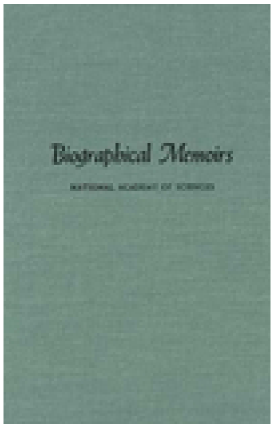

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THE NATIONAL ACADEMIES
*Advisers to the Nation on Science, Engineering, and Medicine*

# Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA

# Biographical Memoirs

## Volume XLVII

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# Contents

# Preface

The *Biographical Memoirs* is a series of volumes containing the biographies of deceased members of the National Academy of Sciences and bibliographies of their published scientific works. Each biographical essay has been written by a fellow member of the Academy familiar with the professional career of the deceased, with only occasional exceptions. These volumes, therefore, provide a record of the lives and work of some of the most distinguished leaders of American science as witnessed and interpreted by their colleagues and peers.

The National Academy of Sciences is a private, honorary organization of scientists and engineers elected on the basis of outstanding contributions to knowledge. Established by a Congressional Act of Incorporation on March 3, 1863, and supported by private and public funds, the Academy works to further science and its use for the general welfare by bringing together the most qualified individuals to deal with scientific and technological problems of broad significance.

# Biographical Memoirs

Volume XLVII

Paul R. Burkholder

# Paul Rufus Burkholder

February 1, 1903-August 11, 1972

By James G. Horsfall

Distinguished scientists don't just happen. Paul Rufus Burkholder wasn't just accidentally elected (1949) to the National Academy of Sciences. He didn't have his greatness thrust upon him; he earned it.

In him was a concatenation of factors that characterize many members of the Academy: (a) He was born into and raised in an intellectual family. His father had a "library" that young Burkholder devoured. (b) He was a bright boy. (c) He was a prodigious worker as witness the extensive bibliography attached. (d) He was an introvert at heart, a loner if you please. This allowed him the time to work. (e) He was stimulated by his early teachers to pursue science. (f) He had a devoted wife who shared his hobbies, his life, and his lab. She worked as hard in biology as he did and raised three sons besides.

I knew Burkholder in the graduate school at Cornell. I knew him again when I was an adjunct professor in his department at Yale. I always admired "Burkie," even though he was difficult to "know" well.

When he was in graduate school, he compensated for some of his introvert tendencies by joining Gamma Alpha, a graduate student fraternity. There he found another "Burkie" (no relative), whom I also knew. This may not have helped Paul too much, for the second Burkie was a very tall man and Paul was a little short. Promptly the two were distinguished as "big Burkie" and "little Burkie." It must be discouraging to be called a "little Burkie." On the other hand, it drives the little Burkies to excell the big Burkies.

At any rate, Paul fought the little-man complex so hard that he was able to put on giant shoes and take giant strides in science.

## HIS INTELLECTUAL BASE

The Burkholder forebears, religious refugees from the German section of Switzerland, came to Pennsylvania during its early colonial period. William Rankin Burkholder, Paul's father, was born in 1857, just before the Civil War, on a farm in central Pennsylvania. As a young man, Paul's father and an older brother operated a successful general store in Middleburg. William was small, quick, and very strong, taking delight in wrestling, hunting, and driving fast horses. To play cards and drink hard liquor was not to be scorned either. Money was plentiful and life was full, strenuous, and satisfying.

In some way, William's older brother became convinced that he should forsake this way of life and become a minister in the church. He brought influence to bear on William to do likewise. This was a difficult problem. Could he give up all those things that he enjoyed? The diary he kept at the time bears witness to his struggle. His brother—positive, eloquent, and persuasive—showed him his duty. Once the decision was made, he went off to college, as very few did a century ago. In time he was ordained as a minister in the United Brethren Church by Bishop Wright, the father of the famous Wright brothers. As his son was to do later, he pursued a solitary, dedicated life of service to the small communities in south-central Pennsylvania. There, when he was thirty-nine years old, he met and married Mary Ellen Schubert, a young girl of twenty-one. Their son, Paul, an only child, was born seven years later, on February 1, 1903, at Orrstown, Pennsylvania.

Mary's father, George Schubert, came from Germany as a young boy. Because the children made fun of his accent, he left school to become a farmer, married a girl of French descent, and raised a family of seven children. Mary, the oldest, like her son later, had an alert mind and a desire to become a teacher. Her mother pressured her to stay safely at home, however, to help with the family and the aged grandparents.

In spite of her bitter disappointment, her sense of responsibility won out and she did not rebel. Thus, her formal schooling came to an end and she turned to church work for an outlet. Here, William found her. Typical of ministers' families, they had very little money, but a house was always provided, along with space for a garden and a farm to keep a cow and a few chickens. Here a small boy could grow in an atmosphere of freedom and with no real sense of hardship. The environment, if somewhat spartan, was intellectual and enriched by visitors to the home and church. Paul read and reread his father's library, consisting mostly of books on religious subjects. Sundays he spent in church, where three times a day he listened to his father's sermons or found the time well spent in meditation.

He understood what his father was trying to do and admired him for his tireless, if often frustrating, efforts to help the people in their individual, everyday problems and to keep the churches growing and active. Paul knew that the basic principles were right, but there must be more than this. Questions were left unanswered, and Paul went out to find the answers.

School and learning were important. Rightly or wrongly, he felt that he was not so bright and must work extra hard to compensate. He took full advantage of the knowledge exhibited by the schoolmasters of the little village schools. Summers, while still very young, he started working for farmers, weeding and picking vegetables and fruit and helping to harvest hay and grain. He came to be known as swift and dependable, so there was no problem in procuring work. Always he saved the money he earned. His spare time was spent roaming in the mountains with his gun, shooting small game for food, and enjoying nature.

Ready for high school, he walked three miles to a train that took him to Chambersburg and three miles home every day. This schedule kept him busy from early until late, **but** he enjoyed the work and did well in his classes.

## COLLEGE DAYS

In 1920 he enrolled in Dickinson College in Carlisle, Pennsylvania. His classes ranged over a great variety of subjects as available in a small liberal arts college. He worked in the library for expense money. He appreciated this easy access to the books and he used them to good advantage. Like many boys, he worked summers as a laborer.

During his four years at Dickinson, he decided to become a botanist. This decision was influenced no doubt by his early and continuing interest in plants. In 1949 Dickinson College recognized his work with an honorary D.Sc.

## GRADUATE SCHOOL

At Cornell, he came under the enthusiastic tutelage of O. F. Curtis and Louis Knudson and almost automatically became a plant physiologist. Of his contemporaries who took plant physiology with him at Cornell, at least four, including himself, were later elected to the National Academy of Sciences.

Cornell was surely the happy hunting ground for biologists. There was the science in the laboratory, but the campus and surrounding hills were laced with gorges through which streams cascaded down to the lake from "far above Cayuga's waters"—to quote the old Cornell song.

What vegetation to study! It was different on the sunny north side from the shady south side of the gorge. Paul's Gamma Alpha house was on the south edge of Cascadilla gorge. He could collect specimens for his classes almost at his doorstep, or he could go across campus to the Fall Creek gorge to collect plants on the north edge or swim in Beebe Lake and be eyed by the Tri Delta girls from their house on the rim above. And besides he helped the brothers make wine during prohibition days. He was surely a chip off his father's block.

Like all graduate students in the twenties, before National Science Foundation student fellowships, he needed money. A job offer introduced him to the excitement of aquatic biology.

The New York State Conservation Department offered him a summer job in the limnological survey of the state watersheds, beginning with the Cayuga Lake basin, with which he was already familiar. He did the phytoplankton studies. He and his colleagues worked hard, had fun, earned money, and published their papers. This work was to influence him throughout the rest of his life.

## THE DEPRESSION

He received his Ph.D. in 1929, the year of Black Friday and the collapse of the stock market, but it did not discourage him. He was able to continue with the limnological survey. It was extended to Lake Erie in cooperation with the Buffalo Museum of Natural Sciences, where he became a curator of biology and continued plankton studies on the lake. The government boat, *Sheerwater,* manned by a discrete crew and carefree young scientists, came to be known to the "rum runners" on Lake Erie as a friend in time of need. There were adventures aplenty on the waters and opportunities for firsthand observa tion of an epic in our history. Buffalo society added to the fun and frolic.

While at the Museum, he planned and installed exhibits and participated in the teaching program. Continuing his plankton studies, he spent one summer working in northern New York waters and another working in some lakes of the upper Hudson watershed. The last summer he did a study of the phytoplankton of Frenchman's and Penobscot bays, in Maine, near Bar Harbor.

In 1930 he married Lillian Miller, a lady who shared his biological enthusiasms for the rest of his life and who survives him. With the acquisition of a wife and a little later a son, Franz, life took on a more serious aspect. He wished to return to a more academic environment. In the meantime the depression had deepened and no jobs were available. A National Research Council Fellowship saved him, however, and he spent two rewarding years, one at Harvard, where a second son, Peter, was born in 1935, and one at Columbia, where he came under the influence of E. W. Sinnott, a member of the Academy. While at Columbia, he took a course in bacteriology. This became another influence on the rest of his career.

## TO CONNECTICUT COLLEGE

Even after two fellowship years, jobs were still scarce, but one appeared in the nick of time. A young Ph.D., George Avery, had recently begun teaching at the Connecticut College for Women, at New London. (This college has now been "liberated" to Connecticut College and men go there.) Being alone among all those women, Avery shouted for help to Sinnott at Columbia. Sinnott sent him Burkholder.

Burkholder promptly made his late-blooming course in bacteriology pay off. He taught the girls about germs, but now his interest in plant physiology returned. Avery started his work on plant hormones at Connecticut College and since it was a timely subject, they collaborated on work in this field and produced a book on it. The next three years yielded an impressive return in papers published.

## BACK TO AQUATIC BIOLOGY

While at New London, Paul purchased a small farm near Falmouth, Massachusetts, on Cape Cod. Falmouth harbor was near, and so Burkholder bought a sail boat and returned to the water. Wood's Hole was near, too, and here was the opportunity to return to the study of the creatures that live in the water.

This lasted only three years, however. In 1938 Dr. Rickett left the post of plant physiologist at the University of Missouri and the boy from the hills of Pennsylvania went to the hills of Missouri. This seemed a logical move. A large well-staffed agricultural school provided many interested students for the botany courses. Despite much teaching to be done, he managed to carry on research work, mostly in plant nutrition. Here the third son, Karl, was born in 1940. When he left, Dr. Tucker, chairman of botany, jokingly said, "I will have to hire two men to take your place, a day man and a night man."

## A YALE MAN

In 1940 Sinnott entered his life again. Dr. Sinnott went to Yale as chairman of the botany department and asked Burkholder to go with him as plant physiologist.

Botany at Yale was at an all-time low ebb, partly because of the depression and partly because of a failure to bring in new faculty members, but mostly because the university had no agriculture program to send the students of botany on to jobs. Zoology was strong at Yale because there was a medical school to beckon the students from that department.

The next few years were spent in building up the faculty of botany. Norman Giles, a new Ph.D. in plant genetics, came from Harvard; Reader came as a taxonomist in charge of the herbarium. Ed Tatum brought his *Neurospora* work and Dave Bonner followed. Burkholder taught a new course in microbiology that became very popular. Galston came to teach plant physiology. Paul Sears set up a program in conservation.

The department was on the way. Graduate students appeared. Joshua Lederberg, who discovered sex in bacteria; Ed Adelberg, now head of microbiology at Yale; Guillard at Wood's Hole; Sy Pomper; Lou Nickell; Ina May Martin from Jamaica; Joyce and Ralph Lewin; and many others received degrees from the department.

The years were good at Yale. Burkholder was made Eaton professor of botany. His research moved apace, especially in microbiology, production of vitamins by yeasts, and production of antibiotics by lower fungi.

## CHLORAMPHENICOL DISCOVERED

Burkholder soon made a dramatic discovery. Fleming's penicillin had arrived during the war. Penicillin came from a fungus. What other antibiotics from microorganisms could be found? Waksman had discovered streptomycin and received the Nobel Prize. Burkholder soon discovered chloramphenicol in an actinomycete, which he isolated from a Venezuelan soil. The Parke-Davis Company developed, purified, and synthesized it.

Chloramphenicol was discovered just in time to save the lives of soldiers in the Korean War from scrub typhus, a classical killer of soldiers in wartime.

Chloramphenicol is a dramatic case in biology for another reason. Chemists like to chlorinate and nitrate compounds. In 1947, when chloramphenicol was discovered, dogma said that no organism could do these chemical tricks. They couldn't attach a chlorine or a $NO_2$ group to carbon as chemists could. Chlorine and nitro were the substituents chemists used to make killers for organisms—note dinitrophenol wood preservative or chlorinated hydrocarbon insecticides.

Burkholder's actinomycete proved the dogma wrong. Not only could it chlorinate a compound, it could nitrate it, too; and besides it could put both substituents in the same molecule.

This only proves that chemists were a million years late in learning these tricks.

## A PALACE REVOLUTION

In the meantime, Yale botany was not all serene. Burkholder became the unfortunate victim of a palace revolution. Sinnott had held a brilliant group of thinkers together, but then he moved up to be dean of the Sheffield Scientific School. Having always supported Burkholder, Sinnott made him chairman of the botany department, but Burkholder had no liking for scientific politics and had not developed administrative skills.

Severe unhappiness showed in the microbiological contingent. And Burkholder didn't really hold a union card in microbiology. He came into it late as a plant physiologist. This problem was tied to the national picture as well. The bacteriologists of the country had arrogated to themselves the term microbiology. People working with other microorganisms like protozoa, fungi, and one-celled algae were low in the pecking order; and Burkholder's specialty was fungi.

Bacteria are mostly dealt with in two university disciplines—medicine and agriculture. Yale had no agriculture. Medicine, therefore, became a magnet for the new microbiology created by Burkholder at Yale in the botany department. And so it was natural for friction to develop within classical botany. Whereupon, botany lost another child that it had begotten.

The explosion was severe. Tatum left, Galston left, Naylor left, Burkholder left, and Bonner deserted the botany department for the medical school and a new department of micro biology. It seems ironical that Bonner was later elected to the National Academy of Sciences through the botany section, not through microbiology.

## GEORGIA BECKONS

Georgia beckoned to Burkholder and Burkholder listened. He labored strenuously there to build up science in general and microbiology in particular, but it went too slowly for him.

The Sloan-Kettering Cancer Institute in New York thought so, too. Cornelius Rhoades, its director, suggested to George Avery, by then director of the Brooklyn Botanic Garden, that Burkholder could be moved. He could be and was.

The next five years at the Garden, with his old friend George Avery and with Sloan-Kettering cooperation, saw an intensive program of screening soil organisms for antibiotics that might have the potential for chemotherapy of cancer.

There is a fascinating sequel to the Sloan-Kettering phase. They kept Burkholder's cultures "on deposit" when he left. Rhoades chanced to mention this to Jasper Kane, then a research executive with the Pfizer Company. Kane procured the cultures for further screening. In one of these was the fungus that produces Terramycin for Pfizer.

## BACK TO COLUMBIA

Since the cancer idea did not pay off, Burkholder returned in 1961 to Columbia and to his old love for the sea and marine biology. Perhaps the inhabitants of the sea could provide the hoped-for antibiotics that fungi from the worked-over soil could no longer supply. He affiliated with the new biology laboratory in the Lamont Geological Observatory at Columbia, situated up the Hudson River from New York. He was no longer landlocked. He had come full circle back to the organisms in the water.

He spent a season in the Antarctic working with Sieburth on his problems. Why do penguins have so few bacteria in their intestines? Sure enough, as expected, an antibiotic produced by a phytoplanktonic organism was passed in the short food chain to krill and on to the penguins, in which it inhibited growth of bacteria in the gut.

Burkholder marvelled at the plankton growing so thick in the sheltered waters of the Antarctic. This was the foodstuff that supported the whalers for so many years.

He isolated bacteria from the waters of the Antarctic Sea and from Atlantic waters, from the Gaspé Peninsula to Puerto Rico. He learned to scuba dive and examined corals, sponges, and seaweeds from the Caribbean to the Great Barrier Reef of Australia and the Philippines for possible sources of chemotherapeutic drugs.

At the age of sixty-five, he retired from his Lamont Geological Observatory post and went to the University of Puerto Rico as a professor of marine biology at its laboratory in La Parguera and lastly to the College of the Virgin Islands. In both locations he taught graduate students and continued to study the beautiful life in the warm seas.

He was still hard at work at marine biology when leukemia, a disease for which he had attempted to find a cure, suddenly struck him down. He died on August 11, 1972, and thus was closed the life of a very productive biologist. He couldn't lick cancer, but cancer licked him. He came close to being struck down with his boots on, as he surely would have preferred.

Dr. Burkholder is survived by his wife of forty-two years, Lillian, and by three sons, all in scientific endeavors. Franz Burkholder, formerly a computer programmer with Minneapolis-Honeywell now has his own company in Boston. Peter Burkholder is a professor and chairman of the department of pathology in the hospital of the University of Wisconsin. Karl Burkholder is a clinical psychologist in the school system of Arlington Heights, Illinois.

## AN OVERVIEW

George Avery has said to me that Burkholder was an imaginative and inspired teacher, always colorful in his lectures. Richard Benoit, one of Burkholder's former students has expressed my thoughts for an overview better than I could. He wrote me, "Dr. Burkholder truly believed that the business of science was discovery and he was essentially an explorer, like the great botanist-explorers of the generations before his own." Even as a boy he explored the mountains of Pennsylvania. As a graduate student, he explored the gorges and forests around Ithaca. He explored Lake Erie for phytoplankton and later the waters of the Atlantic, the Caribbean, the South Pacific, and Antarctica.

In between, he explored soils for antibiotics and discovered chloramphenicol. He explored land and water plants and hog stomachs for vitamins—zeroing in on vitamin $B_{12}$, the therapeutant for pernicious anemia. Bushwhacking his way through a forest of hog and human stomachs, he found out how to administer $B_{12}$ orally instead of intravenously.

Benoit continues, "Others may remember Burkholder for his successes; I will remember him for his seeking, his exploring. He did not always reach his goal, but he carried the light far up the mountain."

I agree with Benoit. Science has lost a great frontiersman!

I would like to thank Mrs. Lillian Burkholder, Dr. George Avery, Dr. Richard Benoit, and Dr. L. G. Nickell for their assistance, so graciously given, in the preparation of this memoir.

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*KEY TO ABBREVIATIONS*

Am. J. Bot. = American Journal of Botany
Ann. N.Y. Acad. Sci. = Annals of the New York Academy of Sciences
Appl. Microbiol. = Applied Microbiology
Arch. Biochem. = Archives of Biochemistry
Bot. Gaz. = Botanical Gazette
Bot. Mar. = Botanica Marina
Bull. Buffalo Soc. Nat. Sci. = Bulletin of the Buffalo Society of Natural Sciences
Bull. Mar. Sci. Gulf Caribb. = Bulletin of Marine Science of the Gulf and Caribbean
Bull. Torrey Bot. Club = Bulletin of the Torrey Botanical Club
J. Antibiot. = Journal of Antibiotics
J. Bacteriol. = Journal of Bacteriology
J. Org. Chem. = Journal of Organic Chemistry
Limnol. Oceanogr. = Limnology and Oceanography
Mar. Biol. = Marine Biology
Plant Physiol. = Plant Physiology
Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences
Spec. Sci. Rep. Fish. = Special Scientific Report, Fisheries

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Arthur L. Day

# Arthur Louis Day

October 30, 1869-March 2, 1960

By Philip H. Abelson

Arthur L. Day usually described himself as a "physicist," but this description is too simple for a man whose scientific achievements spanned the fields of physics, geophysical chemistry, volcanology, seismology, and ceramic research.

Dr. Day began his scientific training at the Sheffield Scientific School of Yale University, from which he received his Ph.D. in 1894. He taught physics at Yale until 1897, when he decided that his career should be in the laboratory, not the classroom. He had worked with Friedrich Kohlrausch during the summers of 1894 and 1895 and was convinced that advancement in the field of physics required a foreign, particularly German, postgraduate experience. In 1897 he went to the Physikalisch—Technische Reichsanstalt in Charlottenburg—Berlin, one of the best physics laboratories in the world, and volunteered his services as an unpaid assistant. His offer was accepted, and he soon became a member of the regular staff. It was there that he became interested in high-temperature thermometry, a field that was to be his primary research interest for the next fifteen years.

In 1900, the U.S. Geological Survey established a physical laboratory as part of the Division of Physical and Chemical Research, headed by George F. Becker. A principal aim of the laboratory was to conduct high-temperature research in silicate equilibria. Dr. Day was offered a temporary appointment as physical geologist in the fall of 1900, and he accepted. The appointment was made permanent in 1901.

Dr. Day undertook two major investigations at the U.S. Geological Survey. The first was the investigation of equilibria in mineral systems at high temperatures. He chose to study some of the most common minerals of igneous rock, the plagioclase feldspars. He began this work with a study of their melting relationships and soon enlisted the help of E. T. Allen of the Survey's chemical laboratory. The work was pioneering and productive. C. D. Walcott, Director of the Survey, described the results of their research as " . . one of the most important contributions to geologic physics ever printed."

The second line of investigation begun by Dr. Day in the physical laboratory of the Survey was an extension of the gas thermometer scale to high temperatures. At that time no reliable gas thermometer measurements had been made at temperatures around 1150° C. Temperatures in that region were usually estimated by extrapolating the temperature-resistance relationship of platinum resistance thermometers or by rather inaccurate radiation methods. Since it was evident that much of the projected work on mineral relationships would lie in the temperature regions above 1150° C, Dr. Day undertook to extend the nitrogen thermometer scale.

While this work was under way at the Survey, in 1902 Andrew Carnegie created the Carnegie Institution of Washington "to encourage, in the broadest and most liberal manner, investigation, research and discovery, and the application of knowledge to the improvement of mankind." As soon as an Executive Committee was formed, an investigation was begun to determine what work should be undertaken in the near future by the new institution. Eighteen advisory committees were appointed to help make the decisions. T. C. Chamberlin, C. R. Van Hise, and Charles D. Walcott formed the Advisory Committee on Geology; they, together with R. S. Woodward, Carl Barus, and A. A. Michelson (the members of the Advisory

Committee on Physics) formed an Advisory Committee on Geophysics. This committee invited advice from many other distinguished scientists and recommended that the Carnegie Institution establish a Geophysical Laboratory.

The Institution did not immediately establish the laboratory as recommended. Instead, it made grants to Becker and to Day to increase and extend the work that they were doing in the physics laboratory of the Survey: to Becker for experiments on the elasticity and plasticity of solids and to Day for his investigation of mineral fusion and solution at high temperatures and pressures. Space was provided by the Survey to conduct these experiments as of July 1904. Each year following, the Carnegie Institution made larger grants to Dr. Day, who was able to assemble a capable staff of workers to vigorously pursue his investigations.

In December 1905, the Carnegie Institution, seeing that the work was bringing results, appropriated money for the creation of a geophysical laboratory in Washington to house the work. Dr. Day, by virtue of his broad experience and practical nature, was the logical choice for the directorship of the laboratory, and he held this position from 1906 until 1936.

At first the Geophysical Laboratory was concerned mainly with phase-equilibrium studies of the oxides and sulfides of the earth's crust. Dr. Day, continuing the work he had begun years before in Germany, extended the standard gas thermometer scale from 1200° C to 1600° C in a series of experiments that he completed in 1911. By extrapolating from this temperature, a value was assigned to the melting point of platinum. This work established a practical temperature scale, defined in terms of closely spaced melting points of pure substances.

After completing this work, Dr. Day turned his attention to the geophysics and geochemistry of volcanoes, which provided accessible "laboratories" to test the theories of high-temperature geological phenomena. A prevailing theory that particularly interested him was that volcanic emanations are completely anhydrous. In 1912 he and E. S. Shepherd went to the active volcano region of Kilauea, Hawaii, to collect gas samples directly from liquid lava. The development of gas-collecting equipment to avoid contamination by air was an important by-product of these studies. Dr. Day not only found water vapor to be the principal volcanic gas, but he also discovered unexpected variation in the nature and volume of the gases; the amount and proportion changed with every escaping bubble. The methodology and results constituted an important advance in volcanological studies. Dr. Day's interest in volcanoes led him to the study of hot springs, and he collaborated with E. T. Allen in a series of monographs on the hot springs of Yellowstone National Park and the Lassen Peak and Geyserville regions of California.

In its early years the Geophysical Laboratory was considered by many to be of little practical significance. Dr. Day often told the story of the Senator's wife who could see no reason for trying to find out how rocks were formed as long as they could be bought more cheaply than they could be made. The usefulness of the laboratory was demonstrated in 1917, when it was called into war service to ease a critical shortage of optical glass.

Optical glass of high quality was urgently needed by the armed forces for use in gunsights, periscopes, rangefinders, field glasses and the like. Before the war optical glass was obtained almost exclusively from Germany, and by 1917 the United States had exhausted its own small supply in filling orders from Great Britain and Canada. To make the situation worse, European methods of manufacture had been kept secret and there were no American glassmakers experienced in optical glass production.

A few days after the declaration of war, the National Research Council delegated Dr. Day to make a personal canvass of the possible resources for the manufacture of satisfactory optical glass in this country. He presented an informal history of his activities in a talk at a joint meeting of the Section of Physics and Chemistry of the Philadelphia Section, American Chemical Society, which was published in the *Journal of the Franklin Institute,* in October 1920. In this report he said that he discovered in the canvass to which he had been assigned that optical glass had been made in small quantities in this country long before the year 1917.

When his canvass was completed, Dr. Day reported to the National Research Council that one firm in the United States was regularly producing glass of fair optical quality at the rate of perhaps 2,000 pounds per month; that there was another plant of much larger capacity that might be deemed available but which had never produced glass of strictly optical quality; and that four others, including the Bureau of Standards Laboratory, were very small and still in the experimental stage.

In 1915, the Bureau of Standards Laboratory in Pittsburgh had erected a small furnace in which a number of pioneer essays were attempted. One type of optical glass (a borosilicate crown) had been produced at the Bureau of Standards Laboratory by March 1917, when the situation was most critical, and their experimental work was continuing.

As forecast by the General Munitions Board in 1917, the estimated requirements of the army and navy amounted to 2,000 pounds of optical glass per day. Following this revelation, there were earnest conferences in the National Research Council before a course of action was determined upon, which was to ask the President of the Carnegie Institution of Washington to allow the resources of the Geophysical Laboratory, both in men and apparatus, to be applied to this overwhelming task, not that optical glass had ever been made there, but that at the Geophysical Laboratory there was available a larger and more experienced group of silicate chemists than perhaps could be found elsewhere.

On April 19, 1917, thirteen days after the United States entered World War I, the Executive Committee of the Carnegie Institution authorized Day, "upon application from the United States Government or any of its agents, to undertake an investigation of the properties and technique in production of optical glass, and to secure cooperation in so far as practicable and essential with governmental and private agencies engaged in the study or production of this material."

About the end of April 1917, members of the Geophysical Laboratory staff were detailed by Dr. Day to the Bausch & Lomb Optical Company at Rochester, New York. The silicate chemists pursued the usual research methods to discover what they could about optical glass. They analyzed existing samples and assembled the indicated raw materials; they calculated the evaporation of alkali during the melting process and the kind and amount of material likely to be dissolved out of the containing vessel; and they estimated what the initial composition would be that would yield the required product. Then they made up two other samples differing from the first by a few percent in the most critical ingredient, melted the three samples under like conditions, and plotted the curve representing their relationship to one another and to the prescribed sample. In almost every case the exact specifications for the glass desired fell within that row of three observations, and it became possible to write the formulas for any of the typical glasses required for war service without the advice from a "glass expert." In the days of rule-of-thumb glassmaking, as many as 150 essays had been necessary before a glass of predetermined optical constants resulted. The knowledge attained by the Geophysical Laboratory scientists commanded for them the immediate respect and confidence of the workmen who had hitherto believed these things to be shrouded in impenetrable mystery, and rapid progress and wholehearted cooperation were obtained.

In May 1917, the General Munitions Board (later replaced by the War Industries Board) appointed Day a member of a committee to give continuous attention to the task of developing an adequate supply of optical glass, and he was designated "In Charge of Optical Glass Production, War Industries Board."

It appeared that all of the sources of optical glass available in May 1917 could together produce only about half the quantity required by the General Munitions Board, assuming that all glass produced was of quality suitable for war equipment. It was estimated that Bausch & Lomb Company, by extending their plant, could carry approximately one-half of the war load. To maintain the other half, it was decided to make the Charleroi plant of the Pittsburgh Plate Glass Company available and to place someone in charge of it who should have sufficient knowledge of the requirements and techniques to raise the quality of glass produced there to the standard that the government required and that they had not hitherto attained alone.

After several conferences, Dr. Day reported, "a plan was agreed upon whereby the Pittsburgh Company should undertake to perfect their glass under the direction of the Bureau of Standards Laboratory, located nearby. It appeared to the committee that such an arrangement might work out advantageously, for the chemists of the Geophysical Laboratory were already in charge at Rochester [Bausch & Lomb] and a gentle rivalry between the two institutions might prove an incentive to each, of a kind which might bring results more rapidly than without such an arrangement."

By the time of the armistice, in 1918, Dr. Day had supervised the production of over 90 percent of the optical glass produced in the United States, and a crisis had been averted.

In 1918, Dr. Day took a leave of absence from the Geophysical Laboratory to become Vice President in Charge of Manufacturing at the Corning Glass Works in New York, for which he had been a research consultant since 1905. He remained there until 1920, when he resumed directorship of the

Geophysical Laboratory in Washington. Although no longer in charge of any research activities at Corning, he continued as consultant until his retirement from Carnegie Institution in 1936.

Dr. Day's ability and experience as a laboratory and field investigator, his widespread and intimate acquaintance with scientific workers, and his outstanding qualities as an organizer made him particularly suited for the next major activity of the Geophysical Laboratory, which was the study of earthquakes. From 1921 until 1936 he served as chairman of the Institution's Advisory Committee in Seismology.

The idea for such a committee arose from H. O. Wood's publication in 1916 of a detailed plan for cooperative seismological research in California. This called for the active guidance and financial support of some organization that could enlist the cooperation of the appropriate organizations and individuals. Shortly after John Merriam was elected president of the Carnegie Institution in 1921, it was proposed that the Institution enter the field of seismology and the Advisory Committee in Seismology was formed. This committee, composed of J. A. Anderson, Ralph Arnold, W. W. Campbell, A. C. Lawson, R. A. Millikan, Harry Fielding Reid, Bailey Willis, and Dr. Day, was asked to investigate the matter and advise the Institution.

After a careful examination the committee recommended that the Institution enter this field, beginning in Southern California. After surveying problems in that area, it pointed out the need for four principal studies: geology along the fault zone, surface displacement, continuous seismological observation at selected stations and the development of suitable instrumentation for such observations, and gravity determinations. These four studies together would constitute a comprehensive approach to a discussion of crustal movement of a magnitude and scope beyond anything previously attempted.

Dr. Day was chosen chairman of the committee and proceeded to organize what was then the largest cooperative effort in the history of American science. Among the agencies involved in the joint endeavor were the Seismological Society of America, the U.S. Coast and Geodetic Survey, the California Institute of Technology, the Hydrographic Office of the Navy, the U.S. Geological Survey, the University of California, Stanford University, the observatories at Mount Hamilton, Ukiah, and Mount Wilson, the U.S. Bureau of Standards, and the Geophysical Laboratory of the Carnegie Institution of Washington.

The Advisory Committee in Seismology continued to guide the work of the Carnegie Institution in this field until the retirement of Dr. Day in the fall of 1936. The various projects of the committee were energetically pursued, and the work resulted in several comprehensive reports on seismological problems in the western United States. Dr. Day can properly be given credit for stimulating seismology in the United States and raising it to a level of sophistication that it had not previously known.

In the later years of his directorship, Dr. Day became involved in still other areas of research. With the advance in knowledge of radioactivity and the realization that radioactive disintegration must supply an enormous amount of heat to the earth, it became evident that more work was needed in this field, and in 1925 the Geophysical Laboratory entered this research. In an investigation of the radioactive content of ocean samples, it became evident that better samples were desirable. The work done at the laboratory under the direction of Dr. Day led to the development of Dr. Charles Piggott's gun for obtaining core samples of the ocean bottom. The Geophysical Laboratory also advanced the study of methods of age determination based on radioactive disintegration.

Dr. Day continued to pursue his many fields of interest even after his formal retirement in 1936. He remained espe cially interested in seismology and hot springs and made extensive studies of the volcanic areas of New Zealand. A severe physical breakdown forced him to give up such activities after 1946, and he died suddenly of a coronary thrombosis on March 2, 1960.

The esteem in which Dr. Day was held by his fellow scientists is evident by the memberships and high offices to which he was elected and the honors that were bestowed upon him. He was elected to membership in the National Academy of Sciences in 1911 and served as Home Secretary from 1913 to 1918 and Vice President from 1933 to 1941. He became a Fellow of the Geological Society of America in 1909, and served as Vice President in 1934 and President in 1938. Dr. Day was also elected President of the Philosophical Society of Washington in 1911 and of the Washington Academy of Sciences in 1924.

His memberships also included the Accademia dei Lincei of Rome, the American Academy of Arts and Sciences, the American Philosophical Society, the American Chemical Society, the American Physical Society, the American Geophysical Union, the Franklin Institute, the Turin Academy, the Geological Society of London, the Society of Glass Technology, the Société Hollandaise des Sciences of Haarlem, and the academies of sciences of Sweden, Norway, and the U.S.S.R.

Dr. Day was the recipient of four honorary degrees in recognition of his scientific achievements: from Groningen (1912), Columbia (1915), Princeton (1918), and the University of Pennsylvania (1938).

Among his scientific honors, he received the John Scott Award of the City of Philadelphia (1923), the Bakhuis Roozeboom Medal of the Royal Academy of Amsterdam (1939), the William Bowie Medal of the American Geophysical Union (1940), the Wollaston Medal of the Geological Society of London (1941), and the Penrose Medal of the Geological Society of America (1947). Dr. Day was chosen as Orton Lecturer of the American Ceramic Society in 1934 and as Edgar Marburg Lecturer of the American Society for Testing Materials in 1936.

In 1948, Dr. Day created a fund for an Arthur L. Day Medal, under the auspices of the Geological Society of America, to be presented annually in recognition of "distinction in the application of physics and chemistry to the solution of geological problems." He hoped that such recognition of achievement in the geophysical sciences would lead to more and better research in the laboratory and in the field.

Dr. Day is eminent for his personal research, for his leadership in the establishment of the Geophysical Laboratory to investigate numerous geophysical and geochemical problems, and for the part he played in promoting cooperative effort in several fields of geophysical research. This eminence was recognized by the dedication to him in 1938 of an entire volume of the *American Journal of Science* (number 35-A in the fifth series of the *Journal*) The "Arthur L. Day Volume" contains twenty-three scientific papers on geophysical and geochemical topics, contributed by twenty-four active or former members of the Geophysical Laboratory staff.

Dr. Day was born in Brookfield, Massachusetts, on October 30, 1869, the son of Daniel P. and Fannie Hobbs Day. In 1900 he married Helene Kohlrausch, daughter of Friedrich Kohlrausch, President of the Reichsanstalt. He had three daughters by that marriage: Margaret, Dorothy, and Helen; and one son, Dr. Ralph K. Day, who pursued a career in glass science and technology at Maumee (Toledo), Ohio. In 1933 Dr. Day married Ruth Sarah Easling of Corning, New York. At the time of his death, in March 1960, he was survived by his wife, by his four children, and by five grandchildren.

The following organizations provided source material for this biographical sketch: The American Ceramic Society, Inc., American Philosophical Society, American Society for Testing Materials, Carnegie Institution of Washington, Corning Glass Works, The Franklin Institute, The Geological Society of America, Geological Society of London, University of Pennsylvania, Princeton University, and the Royal Academy of Sciences of Amsterdam. I am indebted also to a summary of Dr. Day's work prepared by J. W. Greig of the Geophysical Laboratory in 1940 and to research by Richard T. Rook.

# Bibliography

*KEY TO ABBREVIATIONS*

Am. J. Sci. = American Journal of Science
Ann. Phys. = Annalen der Physik
Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington Publication
Carnegie Inst. Wash. Year Book = Carnegie Institution of Washington Year Book
Centralbl. Mineral. = Centralblatt für Mineralogie
Geol. Soc. Am. Bull. = Geological Society of America Bulletin
J. Franklin Inst. = Journal of the Franklin Institute
J. Geol. = Journal of Geology
J. Ind. Eng. Chem. = Journal of Industrial and Engineering Chemistry
J. Wash. Acad. Sci. = Journal of the Washington Academy of Sciences
Proc. Wash. Acad. Sci. = Proceedings of the Washington Academy of Sciences
Sitzungsber. Akad. Wiss. Berlin = Sitzungsberichte der Akademie der Wissenschaften **zu** Berlin
Smithson. Inst. Annu. Rep. = Smithsonian Institution Annual Report

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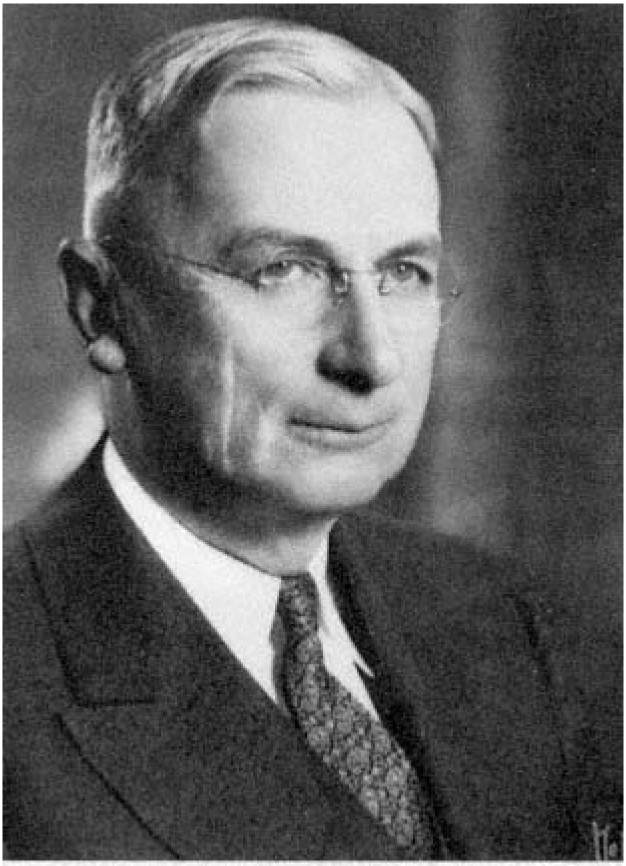

W. D. Harkins

# William Draper Harkins

December 28, 1873-March 7, 1951

By Robert S. Mulliken

William Draper Harkins was a remarkable man. Although he was rather late in beginning his career as professor of physical chemistry, his success was outstanding. He was a leader in nuclear physics at a time when American physicists were paying no attention to nuclei. Besides this, his work in chemistry covers a broad range of physical chemistry, with especial emphasis on surface phenomena. He was a meticulous and resourceful experimenter, as well as an enterprising one, who did not hesitate to enter new fields and use new techniques. He participated broadly, not only in the development of pure science but also in its industrial applications.

Harkins was born December 28, 1873, the son of Nelson Goodrich Harkins and Sarah Eliza (Draper) Harkins, in Titusville, Pennsylvania, then the heart of the new, booming oil industry. At the age of seven, he invested his entire capital of $12 in an oil well that his father had drilled in the Bradford, Pennsylvania, fields. This investment returned his capital several times. Fortunately for science, the returns were not great enough to attract him permanently into oil production.

In 1892, at the age of nineteen, Harkins went to Escondido, California, near San Diego, to study Greek at the Escondido Seminary, a branch of the University of Southern California. The courses of study are described in the University of Southern

California Year-Book for 1891-1892, where Harkins is listed as a student. Apparently, the seminary (now the Escondido High School) served as a preparatory school for the university. Harkins attended the seminary for one year, but had to learn Greek elsewhere because it was discontinued the year he came; he enrolled in a general arts course. It seems that Harkins spent a few more years in Escondido, and made many friends, but there is no other information as to his activities while there.

Harkins entered Stanford University in 1896 at age twenty-three and received an A.B. in chemistry in 1900, at twenty-six. In 1898-1900, he was assistant, then instructor, in chemistry at Stanford. For the next twelve years, Harkins was professor and head of the Department of Chemistry of the University of Montana at Missoula, but he spent a considerable amount of time in postgraduate and postdoctoral work elsewhere. He did postgraduate work at the University of Chicago in 1901 and 1904, and at Stanford University, 1905-1906, culminating in a Ph.D. in chemistry from Stanford on June 10, 1908. He did research in Germany in 1909 and was at the Massachusetts Institute of Technology as research associate in 1909-1910. Harkins left Missoula in 1912 at age thirty-nine for an appointment at the University of Chicago, where he conducted further research the remaining thirty-nine years of his life.

In Missoula, Harkins took part in the life of the city and state: He was President of the Missoula City Board of Health from 1906 to 1912. He was chemist in charge of smelter investigations for the Anaconda Farmers Association (1902-1910), the Montana Copper Company of California (1904), and the U.S. Department of Justice (1910-1912). His first four scientific papers, published during the period 1907-1910, are devoted to arsenical poisoning of animals by smelter smoke, and related matters. In 1911 Harkins did some research for the Carnegie Institution of Washington. On June 9, 1904, he married Anna Louis Hathaway, who was head of the Department of English at the University of Montana. She had also been a graduate student at the University of Chicago.

At the University of Chicago, with the opportunity to conduct pure research and to work with graduate students, Harkins' advancement was rapid. He was first assistant professor of general chemistry (1912-1914), then associate professor (1914-1917), then professor of physical chemistry. In 1916-1917 he was a professorial lecturer at the Mellon Institute for Industrial Research, and he lectured at the University of Illinois (1918-1919). In 1935 he was appointed Andrew MacLeish Distinguished Service Professor at Chicago. He was George Fisher Baker Lecturer at Cornell University in 1936-1937. In 1939 he retired officially at Chicago, but continued research with undiminished vigor until his death in 1951.

During World War I, early in 1915, Harkins began work on explosives for the Allies. Later in the war, he did special work for the army and the Chemical Warfare Service. Throughout his career, while carrying on notable work in pure science, he also contributed to applied science: as a consulting chemist with the U.S. Bureau of Mines, 1920-1922; consulting engineer, U.S. Air Service, 1924-1927; and consulting chemist, Chemical Warfare Service from 1927, Libby-Owens-Ford Glass Company from 1929, Universal Oil Products Company, 1930-1951, and United States Rubber Company, 1939-1941. During World War II, he was a member of the National Defense Research Committee (1941-1945). Civically, he also participated as a member of the Chicago Commission on Ventilation (1916-1928).

Harkins was also active in the affairs of the American Chemical Society. He was editor of the section of General and Physical Chemistry of *Chemical Abstracts* (1939-1951), chairman of the Chicago Section (1915-1916), chairman of the Division of Physical and Inorganic Chemistry (1919-1920), and councillor-at-large for a time. He was the recipient of the Willard Gibbs

Gold Medal of the American Chemical Society on May 28, 1928, in recognition of his work in surface chemistry and on nuclear structure and isotopes. He was also a vice president (chemistry) of the American Association for the Advancement of Science. Harkins was elected a member of the National Academy of Sciences in 1921, and at the annual meetings in Washington in April he took a lively part in the discussions. He was also a member of the American Philosophical Society, to which he was elected in 1925.

In Chicago, Harkins always lived with his family near the university (at 5437 Ellis Avenue). He and his wife had two children, Henry Nelson Harkins and Alice Marion Harkins. Henry Harkins (born in Missoula in 1905) obtained B.S. and M.S. degrees in physical chemistry, a Ph.D. in medicine (1928), and an M.D. in 1931, all at Chicago. He went on to a distinguished career in surgery.[*] His M.S. thesis on surface tension of blood serum was completed under his father's direction in 1926. Marion Harkins won success as a concert singer. The members of the family were devout Episcopalians. The Harkins family had a summer home on Lake Michigan, at Lakeside, across the lake from Chicago. An active mountain climber in his youth in California and Montana, Harkins visited the Rockies annually for many years. At the time of his death, he had been paying daily visits to the hospital after Mrs. Harkins had suffered a stroke.

Harkins' contributions to pure science covered a wide spectrum in the field of physical chemistry, extending also into physics. When I came to Chicago as a graduate student in 1918, it was because I had read about Harkins' pioneering work toward the understanding of nuclear structure, a subject ignored at that time by American physicists. In fact, during the period 1913-1928, Harkins and his students were the only

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[*] G. Egloff, "Fathers and Sons in Chemistry," *Chemical and Engineering News* 22 (1944):804.

Americans engaged in work relating to the structure of the atomic nucleus.

A perusal of the bibliography of Harkins' papers gives a perspective of his scientific interests and of his graduate students and other collaborators. In 1915 the diversity of his interests is already evident. His most extensive work was in surface chemistry (115 papers) and in nuclear and atomic structure and isotope separation (nearly 80 papers).

On the occasion when he received the Willard Gibbs medal, Harkins gave an address that shows something of his personality and the beginnings of his activity in his major fields of research. Following is a quotation from the introductory part of his talk.

"As an undergraduate, research appealed to me as one of life's greatest adventures, and I was attracted both to the very large, in astronomy, and to the extremely minute, in chemistry and physics. While the study of the atom and of radioactivity, then a new subject, had an extreme fascination, there were two subjects of investigation in physical chemistry which seemed to me of such minor importance that I took a firm resolution never to be enticed into working on either of them.

"These two fields of work were surface tension and solubility. To illustrate, let us consider surface tension. I did not realize that the importance of the study of surfaces and surface energy arises from the fact that the surface lies outside every body, particle or cell. To get inside from outside or outside from inside, the surface must be traversed.

"In 1909 I went to Germany to study with Fritz Haber, the chemist whose work on the synthesis of ammonia lengthened the World War by one or two years. On the first day of my stay in Karlsruhe, he invited me to lunch with him and his assistant at the leading hotel of the city.

"Haber insisted that as a visiting professor—I was then professor of chemistry at the University of Montana—only a prob lem of extreme importance should be given to me. He and his assistant rose, drank my health, and Haber said, 'He shall work on surface tension.' Unfortunately, or fortunately, I knew hardly enough German to object, and when much later I found that many of the world's greatest scientists had been interested in surface phenomena, I was thankful for this lack of knowledge."

The work revealed to Harkins the fascinating problems of surface chemistry and initiated his highly original work in that field. When he was able after his establishment in Chicago to resume that work, he began his investigation of the orientation of molecules in surfaces. He was one of the three (the others were W. G. Hardy and Irving Langmuir) who independently suggested the theory of orientation of molecules in surfaces. At Chicago in the winter quarter of 1913-1914, Harkins gave the earliest series of lectures on the theory of this subject. Thus began a long chain of steps, from improved experiments to improved theory to new experiments, which characterized Harkins' work in surface chemistry and related fields for forty years. The sequence was particularly fruitful because Harkins combined meticulous and ingenious experimental techniques with a knack for original interpretation of data. Now continuing the quotation from Harkins,

"After the completion of the experimental work, I returned to America in order to work on physical chemistry with A. A. Noyes and G. N. Lewis, both of whom have been awarded the Gibbs Medal. Here I met my second aversion, for A. A. Noyes stated that, under the grant from the Carnegie Institution which supported the work, it was expected that the general subject of research should be the theory of solutions, but the special subject solubility."

In the last year of his life, Harkins completed a book, published in 1952, *Physical Chemistry of Surface Films,* summarizing his work on the subject. The book contains an introduction by Thomas F. Young, a younger colleague and a great admirer of Harkins. This introduction contains a paragraph that comments interestingly on the fruits of Harkins' work at M.I.T.:

"During the brief period which Dr. Harkins spent at the Massachusetts Institute of Technology in 1909-10, Professor A. A. Noyes was greatly interested in theories of solutions, and inspired an outstanding group of young men to investigate the subject. A remarkable series of papers came from the laboratory describing work done by or under the direction of A. A. Noyes, G. N. Lewis, W. C. Bray, W. D. Harkins, and others. Of course Harkins did not know then how important that work on the thermodynamics of electrolytic solutions would be to his own later investigations of surface phenomena, especially his studies of adsorption. In 1911 he published three papers presenting his researches on solubility carried out at the Massachusetts Institute of Technology. In later years he contributed about ten more papers on ionic interactions. The work of A. A. Noyes and his group aided G. N. Lewis in his discovery of the ionic strength principle. The latter once remarked that the principle was obtained within a few hours after he had picked up notes of a conference held some ten years earlier with Harkins."

Continuing further with the quotation from Harkins' Gibbs Medal address, first about his work on surface chemistry, "Now the greatest of solubility rules is 'similia similibus solvuntur' or 'like dissolves like.' This rule suggested that the experiments on surface tension might have given results more in accord with the theory if more complicated molecules, such as those present in the muscles, had been used. It is advisable, however, in scientific work, to use as simple materials as will give the desired behavior, so substances like butyric acid were considered. . . . a molecule of this substance possesses the interesting characteristic that at one end it is like oil, and at the other like water.

"Thus we may place a thick layer of oil on water and add butyric acid. The water-like ends of the molecules should be soluble in water, and the oil-like ends in the oil, but only at the interface between the two can both ends of the molecule be satisfied at the same time. From this point of view the butyric acid should be very much more soluble at the interface than in either oil or water, which is true. Furthermore, at the interface there should be a certain structure, since the molecules of butyric acid should, in general, be oriented with oil-like ends toward the oil, and water-like ends toward the water . . . .

"A later careful search in the literature showed that Hardy, a noted English biologist, had just suggested (1912) that since a surface is extremely unsymmetrical with reference to the material on its two sides, the molecules in the surface should be oriented. Thus the theory of dissymmetry and that of solubility gave rise in two different minds to the same suggestion."

Especially in his later papers, Harkins deals extensively with emulsion polymerization, soap micelles, and other matters related to the formation of colloids. He also deals with adsorption and with the surfaces of solids and their interaction with liquids. Again, in the Gibbs Medal address, Harkins explains his interest in atomic nuclei as follows:

"In order to understand the action of surfaces, it appeared essential to learn as much as possible about the electrical structure of molecules and of atoms, so, in 1913, I began to study more intensively the current theories of atomic structure. In 1904, Nagaoka had suggested that an atom consists of a central sun or nucleus and a system of negative electrons as satellites. This theory was amplified by Rutherford, who showed that the positively charged atom nucleus appears to be extremely minute in comparison with the space occupied by the atom. For many years the phenomena of radioactivity had been extremely fascinating to me, and this was undoubtedly what caused my attention to be directed more specially to the nucleus, which de termines the stability and even the existence of the atom as a whole."

A series of three papers by Harkins and his student E. D. Wilson in 1915 represents the first of a number of papers published over the years in which Harkins developed ideas on the structure of atomic nuclei. The papers distinguish carefully between chemical elements and atomic species. In general, an element is a mixture of atomic species (isotopes). In 1915 it was already clear that most of the lighter elements have atomic weights very close to a unit that is slightly (about 0.77 percent) less than the mass of the hydrogen atom. The 0.77 percent discrepancy was attributed by Harkins and Wilson (and also independently by Rutherford and others) to what they called a "packing effect," ascribed to a loss of mass predictable from Lorentz' electromagnetic theory if protons and electrons interact at sufficiently close range. They included a speculation that the conversion of hydrogen to helium might be a source of the energy for the sun and stars. Harkins' friend A. C. Lunn, professor of mathematical physics, made the calculations for him. As time went on, it became increasingly clear from mass spectroscopic evidence that those elements whose atomic weights differ from integral multiples of the basic unit are mixtures of isotopes.

Quoting G. N. Lewis [Phys. Rev. 46(1934):897], "It was Harkins who first called attention to the striking connection between the atomic weights of the elements and their abundance, not only in the earth's crust, but [as a much better sample of the solar system], in the meteors." And it was he who first used these abundances as criteria for the relative stabilities of various atomic species. After E. Rutherford's proof of the nuclear atom, and until the experimental proof by J. Chadwick in 1932 of the independent existence of the neutron, it was generally believed that nuclei are built of protons (not so named at first) and electrons. Harkins noticed that the relative abun dances, hence stabilities, of different atomic species are by far greater for nuclei containing an even number of protons and electrons; the next class, in terms of abundance, contains an odd number of protons and an even number of electrons. Two much rarer classes contain an even number of protons but an odd number of electrons, or odd numbers of both protons and electrons.

It was also apparent to Harkins that many of the lighter species (e.g., $^{12}$C, $^{16}$O, $^{20}$Ne) could be thought of as built of *a*-particles; the -particle itself, the helium nucleus, being a very stable composite, formed by close packing of four protons and two electrons, $p_4e_2$. The atomic weights of the composites such as $^{12}$C and $^{16}$O showed little further packing effect. Harkins did not attempt to give a categorical answer to the question of whether such nuclei *consist of* (nearly unchanged) -particles, or whether they merely could be *built from* -particles. It was of course known that -particles can have an independent existence.

In a similar way, Harkins concluded that nuclei such as those of $^{19}$F and $^{23}$Na could be built from, or possibly consist of, -particles plus a hypothetical -particle ($p_3e_2$). Harkins mentioned, but did not emphasize, the possibility of the independent existence of this and other particles (the helion, $p_4e_4$; $p_2e$; the μ particle, $p_2e_2$; and a particle pe). He mentioned that $p_3e_2$ and $p_2e$, since known, would be isotopes of hydrogen (the triton, and the deuteron). Rutherford entertained similar ideas, and independently (and earlier than Harkins) spoke of packing (of H to form He), but it was Harkins who made the major contributions on the stabilities of atomic species and the structure of nuclei. This work culminated in his "new periodic system" of atomic species expressed in a diagram of "isotopic number" versus atomic number. Here, if the structure of any nucleus is written as $(p_2e)Z\ (pe)_n$, *n* is the isotopic number if Z is the atomic number; *n* was later recognized as a neutron number.

It was first shown by Chadwick in 1932 that the grouping (pe) can exist as a free particle, the neutron. Rutherford in his Bakerian lecture in 1920 describes the properties of the neutron as a conceivable free particle. The idea that such a particle might exist kept Rutherford's laboratory "on the lookout" for it and doubtless thereby contributed to Chadwick's discovery. (A little later, it was realized that, in its formation from a proton and an electron, the neutron undergoes a much more radical change than simple association; for example, while the proton and the electron each have a spin 1/2, so does the neutron. Its spin cannot be accounted for by the otherwise expected vector addition to give a spin 0 or 1.) Rutherford in his Bakerian lecture refers to Harkins' 1920 paper, "The Nuclei of Atoms and the New Periodic System," in which Harkins refers to the (pe) grouping. In 1921, in a paper communicated to *Philosophical Magazine* by Rutherford, Harkins first introduced the word *neutron* to describe this particle.[•] Harkins in an article in *Science* in 1946 said "that a neutron exists was assumed independently by Rutherford and Harkins in 1920."

Stimulated by work in Rutherford's laboratory, Harkins undertook work on the disintegration of nitrogen nuclei by the impingement of -particles. Here he used C. T. R. Wilson's cloud chamber method to photograph the tracks of the *a*-particles and the disintegrating nuclei. Although apparently not the first or the most immediately successful in the use of this method, he and his students made valuable contributions. Later, after the neutron had been discovered, he did extensive work on the disintegration of nitrogen and other nuclei by

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[•] N. Feather in "A History of Neutrons and Nuclei. I.," *Contemporary Physics* 1 (1960):191, "A History of Neutrons and Nuclei. II.," *Contemporary Physics* 1 (1960):257, and "Chadwick's Neutron," *Contemporary Physics* 15(1974):565 has reviewed the development of the neutron concept. As he points out, the w*ord* neutron was first used by W. Sutherland in 1899 and in 1903 by W. Nernst, but for different concepts than that of the current neutron. Feather refers to Harkins on pp. 260-61 of his earlier papers.

neutrons. In the course of this work, he developed ideas about the details of the mechanism leading to disintegration. He insisted that the neutron (or -particle) is first captured and held very briefly in the form of a compound nucleus, which then disintegrates. For some years this formulation was frowned on by the theoretical physicists, but eventually it was seen that he was right.

In his later work, Harkins had some ideas about shell structure in nuclei. Harkins was a pioneer, but he was also quick to take up and implement new ideas originating elsewhere. Soon after Lawrence had invented the cyclotron, Harkins built one at the University of Chicago with the cooperation of his students. Planning began in 1935, and the cyclotron (larger than Lawrence's original) was in operation in 1936. Soon it also began to be used by physicists, Enrico Fermi in particular, for neutron-diffusion studies. With the advent of fission, and Harkins' official retirement in 1939, the cyclotron was turned over to the physics department, under S. K. Allison, a former student of Harkins and afterward a professor of physics. The Harkins cyclotron was used thereafter in the Manhattan Project at Chicago during the war. The papers in Harkins' bibliography make no mention of the cyclotron, except for a brief note in *Science* in 1936 by Moon and Harkins, which shows them as thinking about how to improve on Lawrence's cyclotron.

One activity not yet mentioned is Harkins' work on the separation of isotopes. Except for a partial separation of neon isotopes by F. W. Aston using a diffusion method, Harkins was the first to obtain such a separation. This he and his students accomplished in the case of chlorine, using the diffusion of HCl through clay pipe stems. Afterward there was work (not the first but by far the most extensive) on the partial separation of mercury isotopes.

In conclusion, I quote from T. F. Young's introduction to Harkins' book on surface chemistry, mentioned above. "It is of interest to note that many of Harkins' contributions [about one-third] were made after his formal academic retirement in 1939. His productivity, especially after 1940, was greatly enhanced by financial aid from industries and government agencies interested in catalysis, lubrication, the production of rubber, and other applications of surface chemistry. It was his own dominating curiosity, however, which was the principal cause of the productiveness which endured literally to the last day of his life."

# Bibliography

*KEY TO ABBREVIATIONS*

Chem. Rev. = Chemical Reviews
Colloid Symp. Monogr. = Colloid Symposium Monograph
Ind. Eng. Chem. = Industrial and Engineering Chemistry
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Biol. Chem. = Journal of Biological Chemistry
J. Chem. Phys. = Journal of Chemical Physics
J. Colloid Sci. = Journal of Colloid Science
J. Franklin Inst. = Journal of the Franklin Institute
J. Phys. Chem. = Journal of Physical Chemistry
J. Phys. Colloid Chem. = Journal of Physical and Colloid Chemistry
J. Polym. Sci. = Journal of Polymer Science
Philos. Mag. = Philosophical Magazine
Phys. Rev. = Physical Review
Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences
Publ. Am. Assoc. Adv. Sci. = Publications of the American Association for the Advancement of Science
Sci. Mon. = Scientific Monthly
Z. Phys. = Zeitschrift für Physik

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Vladimir N. Ipatieff

# Vladimir Nikolaevich Ipatieff*

November 21, 1867-November 29, 1952

By Louis Schmerling

Fortunately for both the scientific and the industrial worlds, Vladimir Nikolaevich Ipatieff, who was born in Moscow on November 21, 1867, did not maintain his original intent to have a military career. When he was eleven years old, he was enrolled at the Third Moscow Military Gymnasium after three years in a classical gymnasium. He had no difficulty completing the courses, but his grades were poor until he was promoted to the sixth class at the age of fourteen. His favorite subject was mathematics, which he studied beyond the class requirements. His report card showed steady improvement, particularly in science courses. However, on being graduated at the age of sixteen, his application to the Mikhail Artillery School in St. Petersburg was rejected on the basis of his grades. He entered the Alexander Military School in Moscow, where he received an intense military education. He ranked near the top of his class and, rather than accept rank as sergeant, he decided to transfer in September 1886 to the Mikhail Artillery School, to which he was now admitted, the 450 ruble tuition being waived because he had the highest grades in his class in mechanics, artillery, and chemistry. He became an officer (lieu

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tenant)[*] on August 7, 1887, a day that he considered memorable because of the solar eclipse that occurred and the concurrent scientific flight in a balloon made by a famous chemist, Dimitrii I. Mendeleev. Using part of the money he received from the government and from his father for officer's equipment, he furnished a small chemistry laboratory in his home where he could study qualitative analysis (quantitative analysis being beyond his means because a balance was too expensive).

After a four weeks' vacation, Lt. Ipatieff chose to join the Second Reserve Artillery Brigade and became a teacher of arithmetic and artillery at a battery school in Serpukhov (about 60 miles from Moscow). Since his classes were in the morning, he could devote his afternoons to studying chemistry, largely from two Russian language books: Mendeleev's *The Fundamentals of Chemistry* (3d edition, 1884), and Menshutkin's *Analytical Chemistry,* books that he claimed were his real teachers.

After teaching for two years, he passed competitive entrance examinations and, in September 1889 was admitted to the Mikhail Artillery Academy in St. Petersburg, which had been founded to give technical training to officers who were to serve as engineers in government munitions plants, as inspectors of materials furnished by private concerns, or as members of the Artillery Committee of the Chief Artillery Administration.

Unfortunately (from Ipatieff's viewpoint), the supposedly well-equipped chemical laboratory at the Academy was less useful than it could have been: it had equipment for classes in qualitative and quantitative analysis, but not in organic chemistry.

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[*] Graduates of an institution such as the Mikhail Artillery School were given commissions and assigned( to (or, if their grades were high. permitted to choose) active duty. Then, after serving in the army for a few years and after passing stiff competitive examinations, they could enter academies (such as the Mikhail Artillery Academy) for specialized training that led to high positions in the army or in military educational institutions.

Ipatieff moved his home laboratory to his apartment in order to carry out experiments while studying for examinations. He found it necessary to get approval from the Governor of St. Petersburg because the police were suspicious of home laboratories, which might be used to prepare explosives.

He received industrial experience in his junior year when he spent June and July working at plants and factories, particularly in steel mills. He spent much time learning analytical methods of metallurgy. He commuted to the plants in order to be able to use his own laboratory in the evening. He was criticized by his supervisor (and his final grade was low) for spending more time in his laboratory than at the plants, but he had no regrets because he felt he learned much there that helped him all his life.

Fellow students found Ipatieff's notes quite useful and persuaded him to prepare manuals of qualitative and quantitative analysis; these were subsequently published in 1891 by the Academy.

Much of Ipatieff's time at the Academy was spent studying the properties and analysis of steel, working with the renowned Russian metallurgist, Professor Dimitrii K. Chernov. This resulted in 1892 in his first publication, "The Chemical Investigation of the Structure of Steel." Largely due to this work and to his manuals, Ipatieff was retained by the Academy as an instructor (with the military rank of captain) after he was graduated on May 30, 1892, third highest in his class. He was granted a short leave of absence, which he used to travel to Moscow to marry Varvara Ermakova, whom he had known for ten years.

His first teaching duties involved a junior class course in qualitative analysis. He decided to devote one hour a week to lectures on the laws of chemistry because he realized the deficiency of his chemical education at the Academy. He published a set of notes entitled, *Principal Laws of Chemistry.*

Academy regulations required that in order to continue teaching all instructors present an approved dissertation three years after their appointment as instructors. Ipatieff asked the advice of Professor A. E. Favorsky of St. Petersburg University, who was lecturing on organic chemistry at the Academy. Favorsky suggested that he study organic chemistry and carry out research in that field. "For," said Favorsky, "it is only in organic chemistry that you will learn to think chemically and to experiment rationally." Ipatieff therefore took a course in organic chemistry from N. A. Menshutkin at the University of St. Petersburg, but found that although he profited from the lectures, he did not find them interesting because Menshutkin presented them chiefly from an analytical and physical chemistry viewpoint. Ipatieff devoted himself to studying A. M. Butlerov's textbook of organic chemistry.

His first practical work in organic chemistry was carried out in Favorsky's laboratory, where he started to study the isomerization of allene hydrocarbons to disubstituted acetylenes, as proposed in Favorsky's master's thesis. He spent much time learning how to prepare dimethylpropylcarbinol by the reaction of dimethylzinc with the butyryl chloride. By the end of the spring of 1893, he had prepared two pounds of the tertiary alcohol. He studied the action of bromine on tertiary alcohols (usually using the commercially available *tert*-butyl alcohol) to obtain a dibromide from which an allene could be prepared. This and subsequent work formed his dissertation, "The Action of Bromine on Tertiary Alcohols and of Hydrogen Bromide Upon Acetylene and Allene Hydrocarbons in Acetic Acid Solutions," which was presented and accepted in 1895. Ipatieff became an assistant professor and taught both inorganic and organic chemistry. He was awarded the first of his many (about twenty-five) awards in 1895—the Order of St. Stanislaus, third class.

In 1896, the Academy, which was entitled to send one of its instructors abroad each year, decided to send Ipatieff to study chemistry and the new explosives in use in other countries. Favorsky suggested that he go to Munich to work in the laboratory of Adolf von Baeyer, to whom Favorsky immediately wrote, enclosing a copy of Ipatieff's dissertation, published in German. This letter and one from a Russian classmate of Baeyer resulted in Baeyer's accepting Ipatieff as an assistant for joint work. Baeyer suggested that Ipatieff study the structure of the terpene derivative, carone. Based largely on the determination of the structure of the caronic acids formed by permanganate oxidation of carone, Ipatieff was able to prove the structure of carone in about four or five months. Baeyer was so pleased by his assistant's work that he told Ipatieff to write up the investigation so they might publish it jointly rather than, as was usually the case, publish in Baeyer's name only with an expression of gratitude to the young co-worker at the end of the paper.

The remainder of Ipatieff's research in Munich was independent work, to which he turned at Baeyer's suggestion. He chose to finish work on a problem he had started at the Academy, the action of hydrogen bromide on allenes and other dienes. He found that addition of hydrogen bromide to 1,1-dimethylallene yielded the same dibromide as did its addition to isoprene. Dehydrobromination of the dibromide prepared from dimethylallene yielded isoprene, and Ipatieff was thus the first chemist to synthesize and then prove the structure of isoprene.

While in Baeyer's laboratory, Ipatieff met Dr. Richard Willstaetter of Germany, who later became noted for organic research, particularly the synthesis of chlorophyll, and Dr. Moses Gomberg of the United States, who would later discover stable free radicals. They remained lifelong friends.

Before returning to St. Petersburg in 1897, Ipatieff visited chemists in Germany and France, including Rudolf Fittig, Pierre Berthelot, and Charles Friedel. He inspected military institutions and discussed ballistics. While in France, he spent four months with Paul Vielle, discoverer of smokeless gun powder, studying the combustion of ballastite at various charging densities to check the accuracy of the parallel layer combustion theory.

Ipatieff carried out his usual large quantity of research experiments when he returned to the Artillery Academy. He was appointed a member of the Explosives Commission and of the Fifth Section of the Artillery Committee, which dealt with gunpowder and chemical questions. He also accepted appointment as assistant professor at the Institute of Civil Engineers to teach chemistry and to supervise student experiments. Despite all these interests, he found time to attend the Second International Congress on Pure and Applied Chemistry in Vienna in the spring of 1898 and to write his dissertation on allene hydrocarbons, on the action of nitrosyl chloride and nitrogen oxide on unsaturated compounds, and on the synthesis of isoprene. Acceptance of his dissertation at a public examination resulted in his being given the title Professor of Chemistry and Explosives.

As the first chemistry teacher to hold the rank of professor at the Artillery Academy, Ipatieff redesigned and refurnished the laboratories and wrote textbooks on inorganic (seven revised editions) and organic chemistry (six revised editions).

In 1900 he began to prepare a large quantity of butadiene by the only method then known—the passage of isopentyl alcohol vapors through a heated tube at about 600°C. However, he obtained isovaleraldehyde and hydrogen instead of the expected butadiene, methane, and water. He found that when he used a glass or quartz tube instead of the iron tube he had used in his earlier experiments, there was no reaction unless the temperature was raised to 700°C. Similar experiments showed that passage of other primary alcohols through the hot iron tube (but not the quartz tube) produced aldehyde and hydrogen, secondary alcohols yielded ketones and hydrogen, and tertiary alcohols underwent no reaction other than dehy dration. Ipatieff concluded that the iron wall of the tube caused the dehydrogenation of the alcohols without undergoing any change; in other words, there occurred a new phenomenon, which Russian chemists called a contact reaction and other European chemists called a catalytic reaction. The significance of Ipatieff's work was that he showed that such reactions could occur at high temperatures; it had been assumed that under such conditions there would be complete breakdown of the alcohol and no clean reaction would occur. It had been believed that the conversion of an organic compound could not be directed at temperatures above 250°C, certainly not at 500—600°C. Ipatieff also showed for the first time the influence of reaction vessel walls on a reaction. He became so interested in this subject that he dropped all other investigations and spent all his research time on catalysis, a field in which he made many outstanding contributions during the next fifty-one years.

He showed that easily reducible oxides and the metals (for example, zinc, cupric oxide, and copper) catalyzed the dehydrogenation of alcohols to ketones and aldehydes. On the other hand, when he used a graphite tube to investigate the effect of carbon, he was astonished to find that a different type of reaction occurred and at a lower temperature; ethyl alcohol was dehydrated to ethylene. Further investigation proved that the effect was due not to the graphite but to the clay binder used in the tube. Finally, Ipatieff showed that the difficult-to-reduce alumina in the clay was the dehydration catalyst.

In 1902 he was appointed Professor Ordinary at the Artillery Academy, a considerable promotion both in salary and rank. He also became a lecturer at the University of St. Petersburg, with which he was connected until 1916, taking over a course in general chemistry in 1906.

Because the chief function of the Academy was to train officers, Ipatieff found it difficult to find assistants for research. Nevertheless, he was able to discover new catalytic reactions, such as (in 1903) the isomerization of olefins over alumina or zinc chloride and the conversion of ethyl alcohol to butadiene in the presence of powdered aluminum at 600°C. Moreover, in order to study the effect of high pressure on catalytic reactions, he designed a rotating autoclave (or "bomb") having a closure consisting of a disk gasket of heat-treated copper or other metal between two knife edges, one on the autoclave top and the other on the bottom of the cover. The usefulness and safety of this piece of apparatus was proved by the many tens of thousands of experiments that were, and are still, carried out in it.

During the war between Russia and Japan (1904-1905), Ipatieff and other officers who had been graduated from the Artillery Academy and continued to work in technical institutions received promotions to the same ranks as men who had been graduated at the same time but had gone into active service. He became a colonel.

Despite the war and the political unrest that followed, Ipatieff's scientific research continued with little interruption. He investigated the effect of high pressures on such chemical processes as the addition of hydrogen to unsaturated hydrocarbons (olefins and aromatics), the destructive hydrogenation of organic compounds, and the polymerization of ethylene. These researches were destined to play an important role in the chemical industry. Ipatieff showed that the liquid phase hydrogenation of organic compounds is a more rapid reaction and in many cases proceeds farther than the vapor phase hydrogenation at atmospheric pressure, a reaction then being studied in France by Paul Sabatier and J. B. Senderens.

In 1906 the Russian Academy of Sciences awarded Ipatieff the 4000-ruble Ivanov Prize in recognition of his scientific work. This increased his prestige and resulted in his being permitted to submit a dissertation, "Catalytic Reactions Under High Pressures and Temperatures," to the University of St. Petersburg for the Doctor of Chemistry degree. Such permission was necessary because he had never been graduated from a classical gymnasium. A university regulation of 1884 made it possible to admit to public examination for higher degrees scientists whose achievements had made them famous; permission from the Minister of Education was necessary. Ipatieff received the permission, presented his dissertation, was examined publicly in February 1908, and was named a Doctor of Chemistry.

In 1909 Ipatieff discovered an important phenomenon, the "promoter effect" of additives on catalysts. He noticed that the high-pressure hydrogenation of olefins in the presence of copper oxide was slow when carried out in a bronze-lined autoclave, but rapid and complete when an iron autoclave was employed. He concluded that the iron wall of the autoclave was a promoter for the hydrogenation. Similarly, he found that complete hydrogenation occurred in the bronze-lined autoclave, if the added copper oxide was mixed with iron filings. Incorporation of promoters in catalysts is, of course, now widely used.

Having taught for twenty-five years, as an "ordinary professor" for ten, Ipatieff in 1912 was named emeritus professor, a position that permitted him to continue teaching for ten more years at the Academy and to draw a yearly pension of 1500 rubles.

His scientific life did not interfere with his military life. In 1910 he was promoted to the rank of major general: in 1914, lieutenant general. However, military factors did interrupt his research. During World War I, he was chairman of the Commission for the Preparation of Explosives, which by the end of the war controlled almost the entire chemical industry. In 1916 he was named chairman of the Chemical Committee of the Chief Artillery Administration, formed largely because of the German's use of poison gas. The Committee had five branches: poison gases, gas masks, explosives, incendiaries and flame throwers, and acids. It was concerned with developing the production of these items as well as deciding the best types to manufacture.*

The personnel of the Chemical Committee remained largely unchanged, even after the Russian Revolution in 1917, because most of its members were nonpartisan and worked only for the good of the country while sincerely regretting the mistakes of the old regime. The Chemical Committee was disbanded when it had relatively little to do after the war was almost over. The Bolshevik leaders asked Ipatieff to help convert the chemical industry from a wartime to a peacetime basis. He was appointed chairman of both the Chemical Committee of the Chief Artillery Administration and the Technical Section of the War Council, positions from which he was relieved in June 1918 when he pointed out that he would be more useful if his scientific ability were used. He served as chairman of the Chemical Administration of the Supreme Council of National Economy (S.C.N.E.) during 1921-1926.

He found life in St. Petersburg (now renamed Petrograd) quite unpleasant in 1919-1920. Malnutrition and fuel shortage led to epidemics; typhoid fever raged through the city. Food was rationed. Work in the laboratory ceased in 1918 because water pipes froze and there was no gas supply or heating fuel. About all he did was attend meetings of the Academy of Sciences (to which he had been elected as one of the three chemist members in January 1916) twice each month and give a weekly two-hour lecture at the Artillery Academy to about seven students, who wore overcoats in the unheated classroom.

Ipatieff and his family survived the Revolution largely because some of the leaders realized that the country had to make good use of a man with his scientific ability and because he was friendly with all people, whether revolutionists or peasants.

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* The work and political affairs of the Commission and the Committee are discussed in a most interesting and detailed manner in Ipatieff's memoirs, *The Life of a Chemist,* pp. 190-236.

In 1920 Ipatieff was chosen to direct the Central Chemical Laboratory (previously the Central Laboratory of the Ministry of War) in Petrograd, since he had done so well in reorganizing the chemical industry as chairman of the special commission of the Chemical Committee of the S.C.N.E. Unfortunately, he had difficulty obtaining chemicals and apparatus, particularly because foreign purchases were not approved. One advantage of his connection with the laboratory (renamed the National Institute of Scientific and Technical Investigation) was its food research department, the investigations of which were paid for in food that was distributed to the hungry research workers and their families. Ipatieff was able to resume research in high-pressure catalysis using equipment he moved from the laboratory in the Artillery Academy. His chief areas of study were destructive hydrogenation of polynuclear aromatic hydrocarbons into mononuclear aromatic hydrocarbons and conversion of carbonic acid into formic acid.

However, much of his time was spent making trips to Germany, England, France, and other European countries to negotiate for chemical supplies for his laboratory in Russia. He was also very busy on his return to Russia with committees concerned with the development of the chemical industry. He became chairman of the Scientific Technical Administration, which had jurisdiction over fourteen institutes, ranging from The Institute of Fertilizers to The Aerodynamic Institute and The Chief Bureau of Weights and Measures. The Administration also subsidized many scientists working in other laboratories on problems of interest to industry.

Ipatieff was considered a government official even though he never became a Communist Party member. He gave many personal reports to Lenin and, after 1924, to Trotsky. Early in 1926, Trotsky became the chairman (in name only) of the Scientific Technical Administration, while Ipatieff remained on the board as vice-chairman. In 1926 both were removed from the Administration, chiefly as a result of disputes over development of one or another branch of the chemical industry, particularly nitrogen fixation and dyes, but Ipatieff remained chairman of a special Technical Council of the Chemical Administration.

Ipatieff's interest in scientific chemistry remained high, and in 1926 he resumed annual publication of a large number of papers. In January 1927 he signed a contract, with Soviet approval, to establish research on high pressure and catalysis in the Bayerische Stickstoff Werke in Berlin, agreeing to devote three periods of one and one-half months each in Germany. The government approval was probably granted to "atone" for Ipatieff's incomprehensible removal from the technical committees and because it had been agreed that the USSR would share in any discoveries or inventions that resulted. Ipatieff worked very happily in the new surroundings; the laboratory personnel were friendly and, best of all, he was free to concentrate on purely experimental work.

The Soviet government recognized Ipatieff's scientific ability. In 1927 he was awarded the Lenin Prize for his work on catalysis and high pressure. In May a banquet in his honor celebrated the thirty-fifth anniversary of the publication of his first paper.

A month later he spoke on his latest achievements at a meeting sponsored by German scientific societies, to which were invited about twenty prominent Russian scientists to discuss their researches. At a dinner during this "Scientific Week," he was asked why he did not leave Russia and live in a country where his scientific work would find a more favorable environment. Without hesitation, he replied as he had on other occasions to similar questions; he felt it his patriotic duty to remain in his country for the remainder of his life and to devote all his ability to meeting its needs. Professor Albert Einstein, who overheard the question and answer, remarked that he agreed. However, within six years both men left their respective countries on grounds that fully justified their actions. Nevertheless, Ipatieff, and also probably Einstein, long felt that he had betrayed his beliefs and deserted his country.

In 1927 Ipatieff founded and directed the Institute of High Pressures in the Artillery Academy. Work developed efficiently and smoothly, with his son, Vladimir, one of the twelve men working under his direction. The research included the precipitation of metals and oxides from aqueous solutions by hydrogen and the oxidation of phosphorus by water under pressure. As consultant to Bayerische Stickstoff Werke, he developed the latter reaction into an industrial process for the manufacture of phosphoric acid for use in the manufacture of fertilizers.

Although Ipatieff's research projects were extremely successful and resulted in many publications; although the government sent him as its delegate to many international meetings, including the International Bureau of Pure and Applied Chemistry in The Hague (1928), the Congress on Industrial Chemistry in Strasbourg (1928), and the International Engineering Congress in Tokyo (1929); and although he was appointed chairman of the chemical committee of the Russian Academy of Sciences (1928), he could not keep from worrying about the future. Many chemists were arrested by the G.P.U. (State Political Administration), and rumors, confirmed by friends close to the G.P.U., suggested that Ipatieff's name was fourth on a list of chemists being considered for arrest, largely because of the government's dissatisfaction with his work in Germany (despite its earlier approval). Therefore, when he was appointed to replace a professor of electricity who was to be one of ten delegates to the International Power Congress in Berlin, but who could not go because the G.P.U. had arrested him, Ipatieff was pleased to accept. While wives were usually not permitted to go abroad with their husbands, Ipatieff succeeded in getting his wife's passport in only three days by saying he would be a delegate only if she could accompany him, because she needed medical treatment abroad. They crossed the Russian border at Negorloe on June 12, 1930. Most of their personal possessions were left behind; Ipatieff had not told even his wife (until they had left the country) that he did not expect to return to his beloved Russia, to Leningrad (formerly Petrograd), or to the laboratories.

At the Berlin meeting, Ipatieff met many chemists prominent in the chemical industries of various countries. One of these was Dr. Gustav Egloff of the Universal Oil Products Company (UOP) in Chicago, with whom Ipatieff conversed in German because he did not speak English. He mentioned his interest in visiting laboratories in the United States. Egloff helped him get the necessary visa from the American Consul and in September the Ipatieffs arrived in New York City. Ipatieff met with Hiram Halle, president of UOP; after a visit to the company's research laboratories in Riverside, Illinois, he accepted Halle's invitation to become Director of Chemical Research. It was agreed that he would spend six months a year, for the first three years, in Germany, where he was under contract to the Bayerische Stickstoff Werke. He returned to Berlin, where his work was concerned chiefly with the precipitation of pure aluminum oxide and of various metals and their oxides by the action of hydrogen on solutions of salts.

In May 1931 Ipatieff and his wife returned to the United States, where he was permitted to remain as lecturer on catalysis in organic chemistry at Northwestern University, a position offered to him by Professor Ward V. Evans.[*] For several years Ipatieff gave one lecture a week at the University (a task that made him practice the English he was studying intensively with

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[*] In a speech made at a dinner held in celebration of Ipatieff's seventy-fifth birthday, Evans said, "When I cash in, and they see fit to enumerate the little things I have been able to do, I hope they say, 'He brought Ipatieff to Northwestern University.' This will be glory enough for me."

a private tutor); he spent the remainder of the week supervising research at UOP. Subsequently, he spent Wednesdays and Saturdays at the University and the remainder of the week at UOP. He Americanized his appearance by shaving off his beard. Thus, at sixty-four, the age at which most men are getting ready to retire, Ipatieff began to study a difficult new language and to carry out research with the objective of applying catalysis to petroleum technology. The Professor (the name by which Ipatieff was known at UOP) and his co-workers developed several catalysts and processes, at the same time adding to the fundamental knowledge of hydrocarbon reactions.

It was found that, unlike sulfuric acid, which catalyzed the polymerization of olefins to produce not only olefins but also paraffins and dienes (a reaction the Professor named "conjunct polymerization"), phosphoric acid resulted in only olefinic polymers ("true polymerization"). A solid catalyst (kieselguhr impregnated with phosphoric acid) was developed and was already used industrially by 1935 for the conversion of gaseous olefins (formerly waste matter) to liquid gasoline having a high octane number, especially after hydrogenation. This was the first of many catalysts employed in continuous flow petroleum refining processes. It is still in worldwide use.

Other reactions discovered and applied industrially included the catalytic alkylation of olefins by isoparaffins, previously believed to be the most inert of all organic substances, and the isomerization of saturated hydrocarbons, for example, of *n*-butane to isobutane. Processes based on these reactions produced high-octane aviation gasoline and played an important role in the winning of World War II. The processes are still used in the production of motor fuel.

Many other chemical advances, a number of which found practical application, were made by Ipatieff and his research group during his UOP career. These included the development of hydrogenation and dehydrogenation catalysts, the al kylation of aromatic compounds, the demethylation of paraffins, and other reactions. (See the titles of the almost 160 papers published in 1933-1954, listed in the appended bibliography.) His name appeared as inventor or co-inventor on more than two hundred U.S. patents.

Early in this writer's career at UOP, the Professor mentioned that he wanted each chemist working under his supervision to have two problems, one for the company and one for the chemist's chemical soul. The chemical soul problem, which occupied 10-15 percent of the chemist's time, often became a company problem. A most important example of such a problem was the isomerization of n-butane, studied by Herman Pines despite the fact that the higher boiling n-butane could be sold as a component of gasoline (at least in cold weather) and was more valuable than isobutane; it was not then fully appreciated that only isobutane undergoes catalytic alkylation to yield high-octane gasoline and would be used in an important commercial process.

Another research principle that the Professor emphasized at UOP was that new reactions being investigated, even in an industrial organization, should be studied first with pure compounds and then applied to commercial mixtures. He felt it was easier and quicker to understand the results and reach conclusions when relatively simple products, rather than complex mixtures, were obtained.

Soon after his arrival in the United States, the Professor began work on his chemical autobiography, C*atalytic Reactions at High Temperatures and Pressures.* This was a well-organized review of all the catalytic work he and his collaborators carried out from the time he first showed (in 1901) that inorganic substances induce organic reactions until 1936. He wrote the book because many chemists pointed out the desirability of a work that would coordinate his isolated papers, many of which had been published in various Russian and other foreign journals.

Furthermore, he was extremely irritated by the treatment his work had received by authors of books on catalysis; he was annoyed by the fact that his contributions were ignored and credit for the reactions he had discovered was given to others. His book was published in Russian in April 1936 by the Russian Academy of Sciences, which asked his son, Vladimir, to edit it. Several months later, the English translation appeared.

The writing of this chemical autobiography and of his memoirs, *The Life of a Chemist,* illustrates one of the outstanding characteristics of the Professor—his strict self-discipline, which was probably a result of his military training. He was able to write the books by making sure that he wrote at least three pages each morning before leaving his Chicago residence for the UOP laboratory in suburban Riverside. He took advantage of the time spent on the train (about forty minutes round trip) to study English, by reading novels.

Occasionally, when excited over an idea he was anxious to impart to his assistants, the Professor would burst into the laboratory and start talking rapidly in Russian. The blank look on his assistants' faces quickly let him know what was wrong; with a smile and an "excuse me," he would start over again, just as excited, but now in English.

He had a most gracious personality: considerate, courteous, and charming. He never regarded the people working with him as his subordinates. He often asked about their families and was genuinely sorry to hear of illnesses and misfortunes. He did not reprimand, but suggested and taught in a most unobtrusive manner. It was never necessary for him to assert his authority; he inspired cooperation and encouraged independent thought.

While in the United States, the Professor was repeatedly visited by Troyanovsky, Soviet ambassador to the United States and a former chemistry pupil, who tried to persuade him to return to Russia. He was asked to come back to help solve the many problems of the Russian industry. The Professor ex plained that this was impossible because of his contract with UOP, and suggested that Russia would benefit by licensing processes for which he was responsible, such as polymerization of gaseous olefins. His refusal to return resulted in his being expelled from the Russian Academy of Sciences in January 1937; he was deprived of his Soviet citizenship and forbidden to return to the USSR. The Professor took the expulsions quite philosophically; he was convinced that the Soviet government could not deprive him of honors given by the Tsarist regime for scientific work and not for political beliefs. Furthermore, he became a United States citizen on March 11, 1937; his wife became one a month later. On April 26, 1939, he was elected to membership in the National Academy of Sciences.

Ipatieff's attachment to chemistry was obvious to all who knew him. A UOP chemist recalls being surprised soon after beginning work at the company to find the Professor working at a laboratory bench on which there were chemicals, test tubes, flasks, distilling columns, and other glassware. He asked the Professor (in Russian, the mother tongue of both men) whether his assistant was away, thus causing him to be in the laboratory. The Professor drew himself up and replied, "I am doing some of my own research because I love intimacy with chemistry. I love to carry out experiments with my own hands, to see and smell transformations of matter."

The Professor was deeply religious and completely unprejudiced. When he took the United States citizenship examination, he answered in the affirmative when asked whether he went to church. In answer to the next question, "What church do you attend?", Ipatieff replied, "Any church; this is a free country." This was the final question.

Once, when the Professor and the writer were having a lengthy discussion as to why an unexpected result had been obtained in an experiment, the writer remarked in exasperation, "Only God knows!" The Professor answered, "Yes, but He doesn't care."

The Professor had a favorite phrase that he used to keep the chemists working with him from leaping to unwarranted conclusions based on the unexpected results of an experiment. "Remember," he would warn, *"ein Experiment ist kein Experiment."*

In 1939 he deposited $35,000 with Northwestern University to establish the Ipatieff Prize ($3000) to be awarded triennially by the American Chemical Society for outstanding chemical experimental work performed in the field of catalysis and high pressures by a chemist not yet 40 years old. When asked why he limited the prize to chemists under 40, he replied that honors were for old men; young men needed money.

The Professor also established a fund at Northwestern in 1939 to institute a high-pressure catalytic laboratory, which the University named the Ipatieff High Pressure and Catalytic Laboratory and which UOP insisted on equipping because the company appreciated Ipatieff's scientific and technical work. The Professor not only kept financing the laboratory while he was its director, but also named it principal beneficiary of his will.

Work carried out in the laboratory included catalytic condensation of alcohols with ketones and other reactions of alcohols. Most work was concerned with terpenes, for example their polymerization, alkylation, and isomerization; this was done chiefly because reactions of these hydrocarbons were not being studied in the UOP laboratories.

The Professor's fortitude is illustrated by his preparations for a throat operation he underwent in December 1939. His throat had been inflamed and his voice hoarse for some months. He carefully kept the matter secret from his associates, and told everyone he was taking a month's vacation. He and his assistants laid out a program of research work to be carried out in his absence and made other plans, just as they had done before his other vacations. He said his goodbyes and left with no hint that he might soon undergo a serious operation. On December 2 he had a minor but torturous operation for the removal of tissue for examination purposes. On December 8 he learned it was cancerous. The operation, performed on December 18, was successful; but for the remainder of his life, the Professor spoke in a hoarse whisper. When he received the Willard Gibbs Medal of the Chicago Section of the American Chemical Society on May 24, 1940, his acceptance speech had to be read for him because he was forbidden by his physician to deliver any public speeches. However, it was not long before he was again able to speak at meetings.

In 1951 the Professor flew to The Hague to attend the Third World Petroleum Congress. Though 84, he had never flown before. Dr. Vladimir Haensel, who accompanied him, remembers the event: "I would not say he was apprehensive, but, for reassurance, after he got into his seat and put on the belt, he crossed himself and from there on really enjoyed the trip. He had faith in God and faith in experienced personnel, and felt that this was a pretty good combination. We came back on the *Queen Mary* with Ipatieff strolling the deck while the ship was rolling and pitching violently, and most passengers were staying in their cabins. The flight back was not needed—he *had* done it once."

Still actively engaged in research, Ipatieff died in Chicago on November 29, 1952. His wife died only ten days later. They were survived by two of their four children: their youngest son, Vladimir, a professor of chemistry in Leningrad, and their daughter, Anna. Their other two sons had passed away earlier—his first son, Dimitrii, was killed in action on the Vilna front during World War I, and his next son, Nicolai, died of yellow fever in 1934 in the Belgian Congo, where he was working as a government food inspector.

This biography could not have been written without the aid of Ipatieff's autobiography, *The Life of a Chemist,* which presents in

520 pages a detailed description of the Professor's schooldays, his scientific life, and his relations with the Russian governments, ending with his emigration to the United States in 1930. His shorter memoir, *My Life in the United States* (two hundred pages), covers the succeeding years until February 1941. I also depended on my own memories as well as those of many of my colleagues.

# HONORS AND DISTINCTIONS

## Awards and Medals

| | |
|---|---|
| 1895 | Order of St. Stanislaus, 3d Class |
| 1896 | Minor Butlerov Prize, Russian Physical-Chemical Society |
| 1898 | Order of St. Anna, 3d Class |
| 1902 | Order of St. Stanislaus, 2d Class |
| 1904 | Order of St. Anna, 2d Class |
| 1904 | Order of St. Vladimir, 4th Class |
| 1906 | Ivanov Prize, Russian Academy of Sciences |
| 1907 | Order of St. Vladimir, 3d Class |
| 1913 | Moshnin Prize, University of Moscow |
| 1913 | Order of St. Stanislaus, 1st Class with Star |
| 1913 | Order of St. Alexander (awarded by the King of Bulgaria) |
| 1915 | Order of St. Anna, 1st Class with Star |
| 1916 | Commander of the French Legion of Honor |
| 1916 | Order of St. Vladimir, 2d Class with Star |
| 1920 | Major Butlerov Prize |
| 1927 | Lenin Prize, Soviet Government |
| 1928 | Berthelot Medal |
| 1939 | Lavoisier Medal |
| 1939 | Medal presented by King Boris of Bulgaria |
| 1940 | Willard Gibbs Medal, Chicago Section, American Chemical Society |
| 1940 | Modern Pioneer Award, National Association of Manufacturers |
| 1942 | Honor Scroll, American Institute of Chemists |
| 1943 | Fawcett Aviation Award |
| 1952 | Chevalier of the Cross of Lorraine and Companion of the Resistance |
| 1952 | Order of the French Association of the Knights of Cyprus and Jerusalem |

## Honorary Degrees

| | |
|---|---|
| 1927 | Sc.D., University of Munich |
| 1928 | Sc.D., University of Strasbourg |
| 1938 | Sc.D., Northwestern University |
| 1939 | Sc.D., University of Sofia, Bulgaria |

## Honorary Memberships

| | |
|---|---|
| 1916 | Russian Academy of Sciences |
| 1922 | Goettingen Academy of Sciences |
| 1930 | German Chemical Society |
| 1938 | Russian Institute of Science, Belgrade, Yugoslavia |
| 1939 | National Academy of Sciences, USA |
| 1939 | Officier de l'Academie de la France |

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*KEY TO ABBREVIATIONS*

Artilleriiskii Zh. = Artilleriiskii Zhurnal

Ber. Dtsch. chem. Ges. = Berichte der Deutschen chemischen Gesellschaft

Bull. Acad. Sci. USSR = Bulletin of the Academy of Sciences of the USSR (Izvestiya Akademii Nauk SSSR)

Bull. Soc. chim. France = Bulletin de la Société chimique de France

Chim. Ind. = Chimie et Industrie

C. R. hebd. seances Acad. sci. = Comptes Rendus hebdomadaires des seances de l'Academie des sciences

Div. Pet. Chem., A.C.S. Mtg. = Division of Petroleum Chemistry, American Chemical Society (General papers presented before meetings of the division)

Dokl. Akad. Nauk SSSR = Doklady Akademii Nauk SSSR (Proceedings of the Academy of Sciences of the USSR)

Ind. Eng. Chem. = Industrial and Engineering Chemistry

Ind. Eng. Chem. Anal. Ed. = Industrial and Engineering Chemistry, Analytical Edition

J. Am. Chem. Soc. = Journal of the American Chemical Society

J. Appl. Chem. USSR = Journal of Applied Chemistry of the USSR (Zhurnal Prikladnoi Khimii)

J. Chem. Educ. = Journal of Chemical Education

J. Chem. Ind. USSR = Journal of Chemical Industry, USSR (Zhurnal Khimicheskaya Promyshlennost)

J. Org. Chem. = Journal of Organic Chemistry

J. Phys. Chem. = Journal of Physical Chemistry

J. prakt. Chem. = Journal fuer praktische Chemie

J. Russ. Phys.-Chem. Soc. = Journal of the Russian Physical-Chemical Society (Zhurnal Russkago Fiziko-Khimicheskago Obshchestva)

Khim. Prom. (Berlin) = Khimicheskaya Promyshlennost (Chemical Industry, published in Berlin)

Khim. Tverd. Topl. = Khimiia Tverdogo Topliva (Chemistry of Solid Fuels)

Natl. Pet. News = National Petroleum News

Oil Gas J. = Oil Gas Journal

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| 1950 | |
| 2,502,569 | With Herman Pines. Manufacture of Alkylcyclopentane Hydrocarbons. |
| 2,514,546 | With Herman Pines. Production of Cycloalkylperhydroindan Hydrocarbons. |
| 2,519,576 | With Herman Pines. Production of Arylindans. |
| 2,519,577 | With Herman Pines. Production of Arylindan Hydrocarbons. |
| 2,526,895 | With Herman Pines. Production of Polycyclic Aromatic Hydrocarbons. |
| 2,526,896 | With Herman Pines. Production of Diaryl Alkanes. |
| 2,526,897 | With Herman Pines. Production of Arylindans. |
| 1951 | |
| 2,538,248 | With George S. Monroe. Isomerization of Olefins. |

| | |
|---|---|
| 2,557,505 | With Herman Pines. Production of Diarylalkanes. |
| 2,563,037 | With George S. Monroe. Conversion of Aromatic Amines to Aromatic Hydrocarbons. |
| 2,578,207 | With Herman Pines. Di-(Alkylphenyl)-Alkenes. |
| 1952 | |
| 2,584,103 | With Herman Pines. Alkylation of Aromatic Hydrocarbons. |
| 2,586,535 | With Herman Pines. Catalytic Hydrogenation of Aromatic Hydrocarbons in a Stainless Steel Reactor. |
| 2,587,577 | With Herman Pines. Production of Arylindanes. |
| 2,622,110 | With Herman Pines. Di-(cycloalkyl)-alkanes. |
| 1953 | |
| 2,630,460 | With Herman Pines. 4-Alkylbiphenyls. |
| 2,631,174 | With Herman Pines. Polycyclic Benzene Hydrocarbons. |
| 1954 | |
| 2,671,114 | With Herman Pines. Bis(hydronopyl) Ether. |
| 2,671,120 | With Herman Pines and Bruno Kvetinskas. Noncatalytic Isomerization of Aromatic Compounds. |
| 2,694,730 | With Herman Pines. Bicyclooctene Derivatives. |
| 1956 | |
| 2,750,261 | With George S. Monroe. Hydrogen. |
| 2,751,406 | With Herman Pines. Oxidative Condensation. |
| 2,758,140 | With Herman Pines and Bruno Kvetinskas. Noncatalytic Condensation of Aromatic Compounds with Unsaturated Hydrocarbons. |

H. S. Jennings

# Herbert Spencer Jennings

April 8, 1868-April 14, 1947

By T. M. Sonneborn

Herbert Spencer Jennings was widely recognized and greatly respected not only as a pioneering biological investigator but also as a thinker, philosopher, and educator. He was a master of the art of setting forth simply, clearly, and vividly, in print and in public lectures, the current state of genetics and general biology and of recognizing and pointing out their implications for the general public and for specialists in various disciplines. The development of such an accomplished and extraordinarily humane man from humble origins is a wonder worth exploring. I shall attempt to do that before surveying and assessing the accomplishments of his mature years. Fortunately, much of the story can be reconstructed from diaries, letters, and other documents in the "Jennings Collection" of the library of the American Philosophical Society. These and other sources, my own twenty-two years of association with the man, and the passage of twenty-six years since his death have provided more than the usual opportunity to study the subject and put him in perspective.

## CHILDHOOD AND YOUTH (1868-1886)

The little town of Tonica, population 500, in northern Illinois, boasted three churches and no saloons during the years H. S. Jennings lived there, from his birth, April 8, 1868, to age six, and again from ages eleven to eighteen. Tonica was the center of a small farming district inhabited by people who were on the whole practical, religious, and narrow in scope. The town's high school started just when Herbert was ready for it: he was in its first graduating class, in 1886.

A major, if not the only, center of adult intellectual life of the community was the home in which Herbert was born. His father, Dr. George Nelson Jennings (M.D., Rush Medical College, Chicago, 1864) was one of the founders, in the year Herbert was born, of the local literary society, which met at the Jennings's home. Dr. Jennings was a tremendously excited participant in this society for six years, until he took his family to California in a fruitless effort to improve his station in life.

The physician father had already risen far above the station into which he had been born (1833) in Litchfield County, in northwestern Connecticut. There he had lived until 1853, the faithful son of a poor housepainter, whose lack of drive and confidence held him in Connecticut while nearly all his relatives ventured west to Ohio or south to Georgia. Young George had labored as his father's helper and as a lone hired hand on a farm until spurred by his mother, Cindarilla Morgan, to become a district school teacher. During the years in Connecticut, his mother's family set his standards and molded his character. Uncle Ira, a liberal preacher and astute businessman, was George's model of the perfect gentleman and humane being; and Ira's son, Pliny, inspired him to smooth his rough, awkward country bumpkin ways and to aspire to self-improvement and advancement. Tales of the successes of relatives and friends who had gone to the fertile and prospering Midwest led George at twenty to shake off the bonds of his hard life and try his fortune in northern Illinois. Working at first on farms, clerking, and teaching district school, he soon saved enough to set up his parents in Illinois and, soon after, to marry Olive Taft Jenks.

Olive came from an old Rhode Island family that had settled there in 1643 and produced Joseph Jenks, Governor of the state from 1727 to 1732. Olive's grandfather had emigrated to northern Pennsylvania in 1802, and her father and mother with their eight children pushed on in 1836 to Vermilionville, Illinois. Olive was born a week after their arrival. Her family was sensitive to the main issue of the day—slavery. On the way to Illinois, they had witnessed the brutal treatment of slaves in Cincinnati. Their home in Illinois became a station on the underground railway for slaves fleeing to Canada. When she was a young woman, Olive became a district school teacher; her brother joined the Union Army.

Both Olive and George, who married in 1856, were intensely religious. Olive remained so throughout her life and devotedly supervised the religious education of her children. Even before leaving Connecticut, George had struggled with questions concerning the irrationality of some religious doctrines and of the evils perpetrated in the name of God as recounted in the Bible, but he hoped eventually to be able to recognize their "rightness." Meanwhile, he remained a practicing member of the Congregational Church and maintained religious practices at home. In deference to him, his wife temporarily left the Baptist Church and became a Congregationalist for some years.

With Olive's encouragement, George soon abandoned what to him were distasteful and unrewarding occupations, worked his way through medical school, and built up a good practice as a country doctor. Never in the least tempted to enter actively into the Civil War, he acquired the resources to collect a library and the time to indulge his love of reading and study. Soon his meditations on his readings, especially of Herbert Spencer (after whom he named his first son), Huxley, Tyndall, and Darwin (after whom he named his other son), led him to replace formal religion with science—especially evolution—as his guide to a philosophy of life. Once and for all he broke off all con nection with the church. To his credit and to that of his fellow townsmen, although Dr. Jennings was looked on as the village infidel, he was loved as a person and respected as a physician.

While his father was at the height of his emotional and intellectual revolution and in the midst of his peak enthusiasm for books and the new literary society, his son, Herbert Spencer Jennings, was born and grew to the age of six. It is not difficult to imagine the great influence his father had on Herbert's early development. George Jennings's autobiography records with thinly disguised pride that the child taught himself to read before he was three, read a biology book at four, preferred books on natural history at five (but Shakespeare next), and memorized many of Macaulay's "Lays of Ancient Rome." Herbert took his loved books to bed with him, not to read, but for company. Although clearly a bookish child, he also had many playmates, mostly drawn from the large clan of relatives living in Tonica. Into his play with them, he introduced the characters of the *Iliad.* When not playing with them, he preferred to be alone. In these early years were laid the foundations of the self-sufficiency that marked Herbert Jennings's life, until he found, much later, other contemporaries of his ilk.

The years from six to eleven (1874-1879) developed a very different aspect of H. S. Jennings. During this period, his father sought his fortune in California. These were years of great adventure for young Herbert—he helped to build a rough home in a deserted sandy plain south of Los Angeles; he became intimately familiar with farm animals; he traveled from Sacramento to Upper Lake, north of San Francisco, in a covered wagon; he watched hordes of Chinese working in orchards near Sacramento; he listened to the noisy, strange funeral rites of Digger Indians near Upper Lake. These and many other experiences widened the horizons of the sensitive, observant child. He started school at eight and learned with great difficulty to write. For California, with its brown hills and lofty moun tains, its strange trees, and many beauties, he acquired a love that persisted throughout his life. Many years later, he wrote that his idea of the most desirable life was to go to California and stay there. For his parents, the California interlude was totally different: hardship, penury, and one failure after another in farming, business, and medicine. At the end, the Jennings family was literally penniless. Herbert and the other children were more or less aware of the poverty and failures, but they were too full of adventure and fun to be appreciably affected by it.

Back in Tonica (1879-1886), the physician—father again quickly built up a good practice; but he had lost his ambition. He settled down to the quiet monotony of a country doctor's life, turning again to the world of books and thought, and finding great satisfaction and pride in the progress of his brilliant son. Herbert's mother, extraordinarily devoted to her children and active in social service, took him regularly to the Baptist Church and Sunday School, much to his silent dissatisfaction. He was an excellent student at school and a studious, persistent reader at home; but he led a happy, sociable life with his "set," which consisted mostly of his cousins, entered vigorously into their games, and enjoyed fishing and other country pleasures. Occasionally he did an odd job to earn a bit of money. This chapter of Herbert's life closed with graduation from high school in 1886.

## BETWEEN HIGH SCHOOL AND UNIVERSITY (1886-1890)

Although George Jennings made a good living, it was not good enough to permit him to send his children to a university. So Herbert had to look to making a living, hopefully to save enough to further his education. Too young and inexperienced to try to compete successfully for a teaching post near home, he welcomed the opportunity, provided by the good offices of his brother-in-law, to try for a post near Laurens, in north western Iowa, a place too isolated and undesirable to attract much competition. Passing the two-day qualifying examination under rough and costly circumstances, he proceeded to his post in the spring of 1886 and remained at it until the end of the three-month term. He had only five or six pupils, one totally ineducable and the others little better. They were filthy and odorous as well. For the first two weeks, he boarded in the miserable home of his ineducable student, who dropped out at the end of that time. Then he boarded seven miles away in Laurens with his sister Lily and her husband, walking daily across the swampy, wild, deserted prairie to and from the school. For his $25 a month and effort at independence, Herbert Jennings paid heavily in frustration and homesickness. But he continued to study and read, devouring Gibbon's *Decline and Fall* during the noon recesses, and impressing his brother-in-law—the poorly educated but able founder, editor, and publisher of *The Laurens Sun—as* having the greatest and most wonderful mind he had ever encountered, a profound student of everything he delved into. To others he encountered, Jennings seemed frail, a poor mixer, hard to approach, and unfit for life on the frontier. He himself confessed that he kept to himself and became acquainted with hardly anyone.

As soon as possible, he returned to Tonica and, having no job, went back to high school for a year (1886-1887) of "postgraduate" work. As part of this work, he wrote an essay (a copy of which still exists) describing in detail his teaching experience in Iowa, but most of his work was in science with a new teacher, Thomas Brunk, an M.A. in botany from Cornell. Brunk was so impressed by Jennings's mental abilities and capacity to get at the root of every question that he later called him to a college teaching post in Texas. Sometime in 1886, Herbert's mother died. Curiously there is no mention of this in either his or his father's autobiography.

In the spring of 1887, Herbert again tried teaching district school, this time in the Trout district of Illinois, near his home. Again the experience was unsatisfactory. The people of the district had little interest in education and the students were miserable, many of them in his opinion being "degenerate or on the verge." His opinion that he was not fitted for this kind of work was confirmed; he felt that, except for the money earned, teaching did more harm than good to the development of his mind and character. Again, one term was enough.

In the fall of 1887, Jennings went to the Illinois State Normal School at Normal, near Bloomington. This was a good experience. During his year there he had superior teachers, especially in mathematics, history, and the classics, and he gained much from associations with fellow students in a debating society and other activities.

This additional training, attested to by a top-grade teacher's certificate, enabled him (1888-1889) to get a post as teacher in one of the best district schools in the area, the Quaker District of Putnam County. This third attempt was completely different from his first two. The families in the district were intelligent and ambitious for their children. The children were able and included most of the top students in the county competition. Herbert made many lasting friends in Putnam County, but his work was hard and heavy. He taught everything from primer to Latin and geometry, holding twenty-seven classes a day, including sessions during recesses and noon hour, for fewer than twenty-seven students. This schedule wore him down.

So he seized an unexpected, unimaginable opportunity. His former high school teacher, Thomas Brunk, had become Professor of Botany and Horticulture at Texas A. & M. College, at College Station. In 1889 he recruited Herbert as Assistant Professor at what must have seemed a fantastic salary—$600 for the academic year, twice as high a salary as he had ever before commanded. Absurd though it seemed to hold a college post without ever having been to college, young Herbert acted then as he always would: he grasped the opportunity and made the most of it. His duties were to help in the elementary botany class in compositae, grasses, and other forage plants, stressing their economic value for Texas ("the most disagreeable, repelling part"), and to supervise the making of gardens by students. His time was "almost entirely occupied in making indexes, lists, maps, etc., of orchards and gardens, writing letters and orders, and all kinds of miscellaneous work." Nevertheless, he managed to attend classes on horticulture, fungi, plant diseases, and plant histology and to study inorganic chemistry on his own. He also collected fungi and published his first scientific paper on the parasitic fungi of the region, in which he reported some new species.

Until near the end of the year, Jennings was unaware that Brunk was the instigator and leader of a raging academic battle that split the faculty into two bitterly opposed factions. The technical absurdity of Herbert's position as Assistant Professor, although he had never attended college, and the failure of Brunk to have given the post to another man (to whom it was alleged to have been promised) were among the targets of the anti-Brunk faction. In the end, the President, the Director of the Experiment Station, the Business Manager, Brunk, Jennings, and many others were required to resign or not be rehired. His year at A. & M. was humorously and vividly described in Jennings's last (1946) publication, "Stirring Days at A and M."

During the year, Jennings was on the whole content with his lot and suffered but little from the homesickness that had dominated his first period away from home when he was in Iowa. He kept up a voluminous correspondence with his family and friends, telling much about his work and thoughts and feelings. His plan to study Greek could not be carried out for lack of time, and he regretted slighting the "higher" subjects that were more to his taste: literature, philosophy, the Bible (in which Ecclesiastes was his favorite book), and the ancient classics of Greece and Rome. Although he had become interested in botany (not in horticulture), Jennings wrote, nevertheless, "I would be glad to drop this all any time—be most joyful and light-hearted over it—and return again to studies which are more to my taste, more naturally. I half think I will do this yet, sometime, when I have earned enough to support me for a while. If I could think that my abilities would warrant it, that I could ever take a good place in those lines—such a place even as it seems as if I *may* be able to take in the scientific line—I certainly should do it. But it certainly would be throwing away a chance such as few men have, and I might regret it in poverty and failure all my life. It is a hard problem and one I have a great many wrestlings over."

Not until a year later would the final decision be taken, a result largely of economic opportunity, but never would Jennings's conflict of interest between science and the humanities be fully resolved.

## LEHRJAHRE (1890-1897):

### Michigan, Harvard, Jena, and Naples

Back in Tonica during the summer of 1890, Jennings read and studied in preparation for entrance into college, hoping that some day it might become possible. His father's help made it possible that fall. Herbert passed the entrance examination for the University of Michigan, receiving a year's credit toward graduation. "I was rather strongly set against scientific study and toward study of a philological nature. I had much trouble to choose my work, and could not decide definitely . . . so I made a sort of compromise and made up my mind to see a little further before decision. . . . At the end of the first semester my interest had become greatly taken up with scientific work—now for the first time properly carried on in my experience . . . . I decided to continue with it, although my interest in language studies was unabated and I hated to give them up. . . . My interest in scientific studies continued to increase to the end of the year so that they overshadowed everything else."

It was the biology course given by Jacob Reighard, then an Assistant Professor but to be promoted to Professor the next year, that excited Jennings most. John Dewey's *Introduction to Philosophy* also had great impact on him. "Professor Dewey's attacks on Herbert Spencer's Philosophy and on Materialism showed that they had no monopoly on rigid logical thinking and partially at least set one free from my heretofore compelled adherence to such doctrines, a change which though the process was painful, as all upheavals of established principles must be, was very welcome. I was left again in the condition of suspense of judgment; the great questions were entirely reopened."

After this first year at Michigan, Jennings's financial resources were exhausted. Very tired, he went home to Tonica for the summer to recoup his energy, playing croquet and not unmindful of feminine charms, especially those of Lulu Plant, who was then being courted by his brother George Darwin. Nearly half a century later, Lulu and Herbert were to marry. As the summer drifted by, Jennings's plans for the next year failed to crystallize. He and his father lacked the resources necessary for another year at Michigan. Then, shortly before the start of the fall 1891 term, he received an offer of an assistantship in zoology. Reighard had sensed Jennings's ability and promise. His offer of an assistantship was thought by Jennings to be the turning point in his career; he returned to Michigan clearly destined to become a biologist.

He threw everything he had into his job, which proved to be very demanding. He collected the organisms needed for the class, ordered the supplies, taught the laboratory work, went over the papers and notebooks, kept the business records, and served in general as a factotum. Reighard not only set very high standards for the students, but also expected a great deal of hard work from his assistant. Together with his own course work, including much appreciated further work with Dewey in ethics and transcendental philosophy, Jennings's duties and studies drove him to put in fourteen to fifteen hours a day, seven days a week. He wondered whether this work grind wasn't too high a price to pay for his success, especially as his poverty added the shame of shoddy clothes, shoes, ties and hats. Unlike those who claimed not to care about other people's opinions of their appearance, Jennings admitted to caring a great deal and being painfully sensitive about his own appearance. He endured his miserable state because of what he believed it could bring him in the not-too-distant future: a decent living in an intellectual occupation. He recognized that in order to reach that happy state he should perform the task in hand—no matter how hard or distasteful—with the same total commitment as if it were the ultimate goal itself.

These and other revelations of his innermost thoughts and feelings were poured out to his cousin Eva Curtis (later Page), for whom he had lifelong admiration and affection. His own sensitive nature sought and was responsive to the sensitivity of fine women. The letters of this period to Eva reveal Jennings as far more mature than the five-year-younger writer of the high school essay. His style is much more like that of his later years, and his thoughts are deep. He wrote of the responsibility of teaching, of his disbelief in individual immortality, of evolution as a relative truth, of the likelihood of free will, of the superficiality of scientific knowledge, and of the essential lack of understanding of anything in the universe, especially one's self. But of everything, he wrote with humility and uncertainty and with care to avoid upsetting the beliefs of his correspondent, who had been brought up in orthodox religion.

During the summer of 1892, Jennings became one of a small crew of rough, adventurous, uneducated men who worked for the Michigan Fish Commission. His pay of $100 plus room and board was an important part of his family's plans, making it possible for his father to divert enough support from Herbert to enable his other son, George, to go to Michigan. Jennings's job was to preserve the fish brought in by the nets so that their stomach contents could be examined later, in order to find out how to increase the fish productivity of the inland lakes. His use of his spare time was the important part of the experience for Jennings, as it had been earlier in Texas. He used this time to study the Rotifera of the lakes. This study, continued for several summers, led directly to several publications and indirectly to his thesis research on rotifer embryology at Harvard and later to studies on the behavior, fecundity, longevity, and genetics of rotifers. The initiation of the rotifer studies was suggested by Reighard, who had proposed to Jennings that he join him in a biological survey of these lakes, taking the Rotifera as his assignment. Reighard failed to obtain funds for the enterprise, so it was temporarily abandoned; but Jennings characteristically carried out his assigned part of the plan during time not engaged in preserving fish catches.

During the year 1892-1893, Jennings continued as an assistant and graduated at the end of the college year. This was the one year that H. B. Ward was instructor at Michigan, taking the place of Reighard, who had moved up the academic ladder. There seems to have been a marked contrast in the attitudes of the new instructor and his assistant towards the students. Ward was rather high-handed with the girls in the class, while Jennings was gentle and understanding, without relaxing his insistence on high-quality performance. Naturally, Jennings's position was much appreciated.

During this year, Jennings again felt overworked and limited in scope. He yearned to break through the straightjacket of almost exclusive confinement to scientific matters. "My interest in human life, in the world as a whole—in the entire frame-—was never so intense, indeed so almost consuming, when I give it a chance to come to consciousness, as it is now."

"I seem often to have good thoughts, to see occasions for action, openings for higher development; these must be dropped to take up the daily routine." "I do not know what my opinions are on matters religious or philosophical. I have thoughts on these subjects, but I have not compared them or grouped them together to see what the whole is; it is all fragmentary." "Often in conversation or otherwise I see in the character of some other person something which shows a weakness in my own thought or character: the view thus given should be definitely incorporated into my action, as it cannot be if accident be depended upon to bring it to mind." "I feel as if I need a counteraction for the harshly repressive tendencies of science." "I must try to make calls on my friends to be a more social being."

But he was less discontented with the narrowness of his concentrated study than he had been previously, and concluded that he was as content as he would be with any other single line of work. He felt as if his thinking were developing, moving forward. He began to think through his own philosophy of science, concluding tentatively that science told only *how* things worked, leaving untouched the mystery of whence, whither, and why, and that its ultimate justification was utilitarian. This movement of thought was to go much further with continued basic changes.

During the summer of 1893, Reighard's plan for the preceding summer came to fruition. Jennings and a few others (including H. B. Ward and Frank Smith) joined him in starting a biological survey of the lakes. Jennings again concentrated on the rotifers, but used his leisure to read and think about social theory, politics, and comparative religion. At this period, T. H. Huxley was his paragon of the many-sided neo-Renaissance man and greatly cheered his hopes of retaining and fostering his own bent towards many-sidedness while maintaining his enthusiasm for rotifers and their values for biological research.

Returning to Michigan for graduate work, in 1893-1894, Jennings served as a graduate assistant, but not without some private grumbling. He felt that he was underpaid for what he was obliged to do and that his role was misrepresented. The catalogue listed Professor Reighard and Mr. Jennings as giving the course in mammalian anatomy (the cat course), but Jennings gave it virtually alone. At the time he thought this a shady procedure, and unjust. He was sorry he took on the job. He recognized Reighard as intellectually strong, an excellent lecturer, and a superb teacher of methods of scientific work, but also thought he had his own interest too much at heart and valued too little the interests of his associates. Jennings thought it had been a mistake to put his career in Reighard's hands, that for his own good he had to get away, hopefully to Harvard, to take his Ph.D. and to prepare himself "to take any kind of a place or go as high as my natural abilities would allow me." After he had arrived at Harvard, he looked back on Michigan and Reighard in a new light. At prestigious Harvard, he found none of the biologists to be "so all-around able" as Professor Reighard: "The more I see and hear of other people, the more I believe in Professor Reighard as an intellectual man." When the time came, as it would some years later, he was quite prepared to accept a position on the zoological faculty at Michigan with Reighard, who in the interim had become Director of the Laboratory (1895-1925).

It was not easy for Jennings to leave Ann Arbor after one year of graduate work. He felt very much at home there with good friends. He knew and liked the students. Classes were small (fewer than ten in the class he taught), the students serious and able. They had jolly times together, students, assistants and junior instructors intermingling freely. And Jennings was in love with one of the students, Mary Louise Burridge; by the time he was ready to leave, they were engaged. She had fascinated him from first sight with her fresh and unpredictable approach to things and continued to do so as they read, talked, and studied together. He was happily spending most of his free time with her. It was not pleasant to look forward to separation, a separation he foresaw as lasting at least two years before he could be in a position to marry. It turned out to be four years, with very few opportunities for the couple to be together. But they did not let their intention to marry interfere with getting the best preparation for a career.

At Harvard, that was what he got. He entered as a graduate assistant in 1894 and began at once to work on a thesis under the supervision of Professor Mark. He knew what he wanted to do for a thesis before he arrived there and had been assiduously collecting material, the rotifer *Asplanchna,* hoping that Mark would let him do a thesis on its early embryology, which occurs inside the mother. Mark was agreeable. Jennings proceeded rapidly to an M.A. (1895) and Ph.D. (1896) in zoology, with minors in botany and geology.

The two years at Harvard were rich and important years in Jennings's scholarly and personal development, far more so than he had anticipated. The library, the greatest university library in America, was quickly appreciated and steadily used. The presence of able and committed graduate students with similar interests was another highly appreciated resource; the eight students (including Castle, Neal, Mayr, and Goto) in zoology seemed to Jennings a large number compared with the number at Michigan! During the second year, he and some of them (Neal, Mayr, and Goto) met informally once a week to discuss some of the great problems of biology, such as those of heredity, development, and evolution. Jennings found preparation for these discussions to be more beneficial than the meetings themselves. From his teachers he received unequal benefits. His supervisor, Professor Mark, gave him freedom and support, but not ideas. He got most benefit from two young instructors, Parker and Davenport, both only a few years his senior.

G. H. Parker's course on the nervous system and physiology of sense organs was solid, like Parker himself; and Jennings appreciated both the man and the course. Whether it had any influence in directing him to the study of behavior is not evident from available documents. These documents do tell much about the influence of Davenport and his course, Factors in Development—Morphogenesis. Jennings rented a third-floor room in the Davenport home during both his years at Cambridge. During his first semester at Harvard, he reacted strongly to Davenport's course, which gave him more ideas than any other course and set a model he was to follow in reviewing the primary sources and giving full references to them. Davenport was then a Lamarckian and an anti-Weismannian; as is well-known, he became a classical geneticist after the rediscovery of Mendelism in 1900. In the 1890s, Davenport held that heredity and development could be accounted for by action of the physico-chemical milieu. Jennings was tremendously stimulated by Davenport's point of view and by the wealth of his ideas. At first dazzled by Davenport's reductionist—Lamarckian point of view, he soon rejected it and concluded that biological discoveries merely push back a step the wonder and mystery of life without removing or explaining them.

During the summer of 1895, between his two years at Harvard, Jennings worked at the Agassiz Laboratory in Newport, Rhode Island. He credited this experience with contributing as much to his biological knowledge and understanding as did his studies at Harvard. He spent the summer observing the fantastic variety and wonder of metamorphic development in marine invertebrates. There too he shared biological exploration, living quarters, and discussion with Castle, Davenport, Montgomery, Mayr, Goto, and an earnest. bright, fine Harvard undergraduate from the Midwest, Walter Cannon. Jennings's one venture into the high society of Newport found him, as he was long to remain in such circum stances, ill at ease. Before returning to Harvard, he spent two weeks with his fiancée in Tecumseh, Michigan, and visited Ann Arbor, where Reighard welcomed him "as a chum," and Tonica, where the harmony of the home in which he had been reared had given way to the discord of his father's second marriage. Returning to Cambridge, he appreciated all the more its advantages and its values in remedying his rustic, midwestern, underdeveloped aspects.

Life during the two years at Harvard was a vast improvement over his previous life. The first year, he had an assistantship (remission of the $150 tuition plus $225), which permitted only stringent living conditions and not nearly enough exposure to the cultural life of Boston; but the duties were light—he taught three days a week from 9 to 4:30 and in only one semester. So he had time for courses, research, reading, thinking, and letter-writing. His qualities of mind, character, and research were quickly and greatly appreciated. He became the candidate of the Zoology Department for a coveted Morgan Fellowship for his second year and won it. The fellowship, with its stipend of $500 and no duties, freed him entirely for study, research, reflection, and as much as he could afford of Boston's cultural life. Although he complained of the exorbitant minimal admission fees ($1), Jennings indulged in opera, symphony concerts, chorals, Shakespearean theatre, and even the cheaper (25-cent admission) Gilbert and Sullivan performances.

The tangible recognition represented by the fellowship raised his spirits and self-confidence. He admitted to a few intimates his feeling that he was beginning to be a master of biology and that he would be prepared to deserve and handle any available college position. The world seemed much brighter, and he was rarely depressed. He developed an appreciation of the wisdom of the relaxed pace at Harvard—the attitude that an educated, cultured person sets aside time from work to enjoy many facets of life. He abjured his previous unduly long workday (which in truth he had never expected to maintain after it had won him the opportunities he believed it would); set aside some time daily for recreation and the "second life" of culture, people, and nature; and learned to heed the first signs of overwork, convinced that he could accomplish as much in less time when fit. Applying these principles, his health improved, except for an attack of mumps, and for the first time in his adult life he began to exceed his previous 125-pound limit.

Most of what is known about Jennings at Harvard comes from letters. Those to his fiancée have disappeared, but S. W. Geiser preserved copies of many letters written to his father, his brother George, his sisters Aldie and Kate, a friend, Joseph Brennemann (his younger roommate at Michigan, who came from the countryside near Tonica and became a pediatrician), and his cousin, Eva Curtis Page. The letters to Eva reveal his thoughts on religion, biology, and philosophy, and show that he was preparing his character and conduct for marriage as well as for a professional career. He envisioned a modest home, as happy as the home of his fiancée's parents, as artistic as his fiancée's taste and talents; and a couple, free of monetary worry, with time to enjoy life together with mutual attention and consideration, and to share things large and small.

The letters to his father and sisters provide the first available evidence of his talent for popular exposition of scientific observation and thought. At this he was already a master, like his idol Huxley. Davenport's ideas were beautifully explained by his pupil, with homely, telling similes and were related to the Lamarckian—Weismannian antithesis; Jennings's own embryological observations at Newport were spun like a yarn of the Arabian Nights. His thesis, as an example of basic research, was likened to artistic endeavor in the sense of being an ideal. spiritual, nonpractical attempt to discover and represent truth, seeking the general in a concrete, isolated example.

Jennings's thesis on the early embryology of the rotifer, *Asplanchna,* was not only a meticulously carried out (he claimed, "more minutely than has ever been done") and thoroughly documented study of cell division and cleavage, but also, in typical Jennings fashion, a searching discussion of the significance of his findings in relation to current "laws" and theories of cell division and early development. He submitted his thesis to compete for the Walker Prizes of the Boston Society of Natural History and it won their first prize ($60). He competed for Harvard's Parker Travelling Fellowship ($500) and won that. This enabled him to spend a year in Europe, the last of his *Lehrjahre* (1896-1897), instead of accepting a one-year appointment as instructor at Indiana University or waiting for a vacancy in the instructorship at Michigan, which didn't materialize anyway. Before setting forth for Europe, he made a long visit to his ailing fiancée in Tecumseh.

Plans for work in Europe were not fully settled until after he went abroad. An important part of Jennings's plan was to learn German, which he began at once in a Harz Mountain pension recommended by Parker, and to absorb German culture, especially its music. He had been attracted to music from his earliest youth and had been surprised to note in Boston what a large part music played in the life of cultured people. There he became a Wagner enthusiast. Little wonder then that music ranked high in his plans for life in Germany. So he tramped through the mountains and woods, visited the sights, and took in the music.

The scientific plans, still tentative, were contingent on being located in a cultural center. He knew he did not want to continue in purely descriptive biology, which he thought had landed "biology in a sort of Dismal Swamp." Experimental physiology and the *Entwicklungsmechanik* of Roux and Driesch, which he had learned and thought about in Davenport's course, seemed "a great change for the better." Working with Roux was ruled out because of the cultural limitations of Halle, the town in which he was located. He wrote to several other people exploring possibilities and settled on Verworn, at Jena, after receiving from him a warm response.

This proved to be one of the most felicitous decisions of Jennings's life, far more so than he imagined at the time it was made. He looked on his forthcoming studies as a new and original way of extending the descriptive embryology of his thesis into the experimental domain. To obtain understanding of the factors governing the movement of cells during early embryology, he imagined much might be learned by studying the factors governing the behavior of isolated cells, *i.e.,* their reactions to stimuli. Verworn had been successfully studying the reactions of individual cells to such stimuli as the electric current; so Jennings proposed to extend these studies to other stimuli. As events were to show, this rationale for experimental embryology was more appealing in imagination than fruitful in actuality. Jennings did not learn anything significant about the behavior of embryonic cells from this approach. Instead, he became fascinated by cell reactions to stimuli as a subject in itself and as a basis for comparative invertebrate psychology. The work done in Verworn's laboratory was in fact the beginning of his most successful and important work as an experimental biologist.

Verworn, only five years older than Jennings, called his attention to the fact that the cell he worked on—the ciliated protozoan, *Paramecium*— oriented and swam toward the cathode in an electric current when it was already swimming, but failed to respond in this way, and seemed not to respond at all, if it was quietly in contact with a solid object when the current was turned on. This was the starting point of Jennings's investigation. He resolved to examine the responses of *Paramecium* to solid objects, gravity, chemicals, and to combinations of these stimuli with each other and with the electric current. A few months after he started work, he wrote: "I've been discovering some queer Paramecium tricks in the last few days. I'm beginning to believe that one might as well stand off and watch a city full of men, with a telescope, and make theories about the forces which compel them to move in such and such a direction or stop moving at certain times. However, all I want to find out is what there is in all this 'reaction to stimuli,' etc.; I don't care how it turns out." The results of his work during the one semester at Jena, as sole experimentalist in the laboratory, were written up and published as the first of a series of papers on "Studies of reactions to stimuli in unicellular organisms." He reported in this first paper the aggregation of paramecia in weak acids and the mechanism of this aggregation, their negative reaction to alkali, their nonreaction to certain chemicals and to osmotic pressure, the responses to certain combinations of stimuli, and many other basic observations.

While in Jena, Jennings attended lectures by Verworn on general physiology, by Biedermann on human physiology, by Liebmann on psychology, and a few by Haeckel. (He also attended a club of American students devoted to pedagogy and philosophy.) Haeckel seemed to him "too popular and commonplace" to be worth continuing; Liebmann, a Kantian, he found to be the most impressive lecturer he had ever heard; Verworn was largely a repetition of Davenport, less deep and broad, but brilliant and solid. Verworn seemed an all-around good man of uncommon powers with a strong philosophical bent, looking on science as the investigation of the laws governing the only reality—man's mental phenomena. Verworn thus denied materialism—the existence of an objective material world—but also denied vitalism. On all this, Jennings formed his own opinions and continued to develop them for many years thereafter.

From Jena, he went to Italy, again being first an avid tourist, ecstatic over the art and music, before settling down to work at the Naples Zoological Laboratory. There he made some unsuccessful attempts to do embryological work of the sort made famous by Roux and Driesch and their schools. At the same time he gave much thought to the great problems of biology and their current status, these reflections serving as a basic frame of reference for all of his subsequent work and thought. And he made his first contact with some of the leading European biologists. Driesch, impressed with Jennings's thesis on *Asplanchna,* had written a laudatory review of it for the *Archiv für Entwicklungsmechanik* and called on Jennings to show him the manuscript. Thus began most cordially a relationship that was to turn much later into one of profound disagreement on vitalism and "psychic research." Jennings also became acquainted with Herbst, Zur Strassen, Haeckel, Ziegler, Richard Hertwig, Waldeyer, His, and many others.

As Jennings's year in Europe drew to an end, plans for the next year had to be made. Reighard and Mark urged him strongly to stay for a second year. He applied for a renewal of the Parker Fellowship and doubtless would have obtained it. But he decided against this and withdrew his application because he was miserable at being so long delayed from marriage and kept so far away from his fiancée, whose life was then shadowed with the sorrow and stress of her father's long illness and death. The decision having been made, Jennings began to worry about his chances of finding the kind of position he desired. He feared that his failure to follow the advice of Mark and Reighard might lessen their support, but this fear was totally unfounded. There was more substance to his worry about a glutted job market. He wrote to his friend, Frank Smith, who had been a colleague in the work of the biological survey and had settled into a good secure position at the University of Illinois, "Zoologists are getting terribly frequent nowadays and it makes me speculate about the future." The competition was getting rough. Visible men on the spot had a great advantage over the invisible man thousands of miles away in Italy. In spite of excellent credentials and favorable reactions, Jennings could not get a bid sight unseen. So he returned to the States early in the summer of 1897, but too late to secure any of the better opportunities. The best he could get was an instructorship in botany and bacteriology at Montana State College, in Bozeman, the lesser of the two offers that had been made to his Harvard fellow student, Neal, who had been his comrade in Italy and had wisely come home earlier.

## DESCENT AND RISE (1897-1907): MONTANA, DARTMOUTH, MICHIGAN, NAPLES, PENNSYLVANIA, JOHNS HOPKINS

The salary ($900) at Montana was not bad, being on a par with what Michigan was paying for a new instructor; but the institution was remote, its physical condition was pitiful, the assignment was outside Jennings's field of special competence, and he had virtually no opportunity to continue his researches. In every realistic sense he had fallen from the crest of the wave to the bottom of the trough and again had to face the task of working his way up.

Naturally, he was discouraged. His fiancée's illness continued to prevent her return to college. Her spirits (and his) were further depressed by the illness and death of her father, followed quickly by her mother's emergency operation for an advanced cancer, detected almost too late. She nursed her mother during her complete recovery, keeping house without help for the family of four. The prospect of leaving home to marry was not bright; and, even if it had been possible, Jennings was three days away in a job far beneath his expectations and capacities. But these were two courageous souls not to be long diverted or subdued by adversity.

Not yet thirty, Jennings enjoyed excellent health and the character to make the most of his opportunities, however short of his desserts they might be. The assignment in botany and bacteriology was seized as an opportunity to learn, as well as to make a living. The lack of office or lab space, the poor buildings and apparatus, were not used as an excuse, but as a reason to work and plan for better conditions. Part of his teaching was done in a twelve-foot-square room in the high school, part in an old skating rink "metamorphosed by partitions into an 'Academy'." Both rooms housed other classes as well, so Jennings had no place to call his own where he might work between classes. But, he said, "we are struggling on, all of us in the same fix, and all cheered by the possibility and hope that *some* time we shall have a college building and every professor have a room of his own."

A new building was in fact under slow and much delayed construction. In anticipation of the move, Jennings had been authorized to order apparatus and reagents; he did so to the tune of $600. Meanwhile, he made do with little more than microscopes, by finding favorable organisms and stages of their life cycle for his classes. He had one student in economic botany, six interested and hard working students in cryptogamic botany, and sixteen in phanerogamic botany. He realized, more than ever before, "how problems of the two sciences (botany and zoology) are exactly the same." He described bacteriology, to which he came almost completely unprepared, as "a very interesting field and I'm glad to have a chance to work it up to some extent. . . . The whole year will be valuable to me—if only I can switch over into something else later." Indeed, time was to prove its value; it provided the foundation for Jennings's capacity to take the then unusual and important step of including unicellular plants and bacteria in his broad and comprehensive review, "Genetics of the Protozoa," which would be published thirty years later.

With characteristic energy and thoroughness, he threw himself also into his additional assignment as botanist at the Agricultural Experiment Station, attending various agricultural and horticultural meetings, work for which he nevertheless did not feel fitted. He also had full responsibility for the chaotic herbarium, which he put into good shape, classifying the specimens and making them usable. His ambition was to leave the herbarium as a memento of his presence in Bozeman. He gave talks to the Faculty Club, a Domestic Science Club, and elsewhere in the state on experimental biology, bacteriology, and heredity. And on top of it all, he tried desperately to find some time to continue his behavior work on *Paramecium:* "My only chance now is not to let myself be entirely forgotten. Having gotten completely outside of real scientific circles and even out of zoology, I'm afraid it will be difficult to get back." If he did any research, it did not come to publication that year. Withal, he found time to read Kant and Schopenhauer, critically garnering the wheat from the chaff, and entertained himself in the evenings with the stories and novels of Stevenson, Thackeray, and the like.

During the Christmas holidays, he made the long trek to Michigan to be with his fiancée for a few days. "It seemed like a sort of crazy thing to do, from such a distance—but we live only once, and these things are measured by a very different standard on the inside from on the outside."

Soon he began to look to possibilities in the East (Ohio State, Michigan, Dartmouth) for the next year. By late March, Jennings had settled on a fine one-year position ($1400) at Dartmouth, where he would take the place of Gerould, who was to be in Europe. This he considered "a good place to get a position *from."* He also arranged with Reighard to join in the summer survey of Lake Erie for the U.S. Fish Commission.

That he left behind him at Montana a tremendous impression on colleagues and students is abundantly attested to in letters collected in 1934 by his biographer, S. W. Geiser. He did not lower his standards for students and made only one concession to their weak background preparation: He gave them perhaps more sympathetic attention and inspiration than otherwise, if that were possible. In doing so he was not in the least supercilious or condescending; that was utterly foreign to his character. As always, Jennings made the best of his situation.

The year at Montana ended and he headed for Michigan, where at last he married his long-time fiancée, the artist Mary Louise Burridge of Tecumseh, on June 18, 1898. The happy couple went to Put-in-Bay, Ohio, an island serving as the summer survey headquarters. Arriving well before the survey party, they had the place alone for their honeymoon. Three weeks after the great event so long looked forward to, Jennings wrote his closest friend, "There isn't any disenchantment nor the slightest indication of one, in marriage, and I feel now that I am sure there's to be none." The Jenningses spent their time boating, walking, sitting on the rocks, reading aloud the *Iliad* and the *Odyssey,* and making a map of the distribution of the twenty-five water plants in one arm of the lake, fascinated as earlier by survey work.

The survey had already been delayed two weeks from the proposed starting time; as Jennings wrote, the remaining six weeks could hardly permit much accomplishment. Besides, he was worried about his brother, George, who was in the thick of the Spanish-American War in Cuba. But the work had to be done. As he wrote, "Competition is fierce, and if I don't keep moving I am going to be left." He did move; no less than three research papers based on the work of those six weeks appeared in 1899. They were papers II, III, and IV in his series, "Studies on the reactions to stimuli in unicellular organisms." His work on *Paramecium* in Verworn's laboratory had shifted his focus from rotifers to Protozoa, from morphology and distribution to behavior; so during this summer at Put-in-Bay he concentrated on the Protozoa, which, in earlier years of the survey, had been assigned to others, first Frank Smith and later C. A. Kofoid.

After this happy and successful summer, Jennings went on to his post at Dartmouth light of heart and full of hope. He was indeed back in the swim of things again. He prepared for publication papers based on the work of the summer and a more general summary paper, "The Psychology of a Protozoan," for a psychological journal. Viewing the reactions of Protozoa as "the beginnings of mind," he continued for years to call the attention of psychologists to his findings. While at Dartmouth, he also completed a monograph on the rotifers of the United States (1900), the drawings for which were made by his wife. On the march again, he "walked with breathless haste," according to the laboratory director, Professor Patten. The following year, Dartmouth offered him a position in botany, and Michigan an instructorship in zoology. He accepted the offer from Michigan, his first opportunity to stay on in a position in his own field of work.

An important event of Jennings's year at Dartmouth was the contact made with Raymond Pearl, with whose life he was to be intertwined in various ways for more than fifty years. Pearl was then a senior undergraduate. Earlier, he had switched from classics to biology after one week in the required course on elementary biology given by Professor Gerould. When Jennings arrived to give Gerould's course, Pearl served as his assistant. The two men had much in common: zest for biology, intense interest in the classics and literature, devotion to music, and a broad inclusive interest in the whole universe. Pearl was strongly attracted to Jennings. He went with him to Michigan as his graduate student, doing his Ph.D. thesis on the behavior of planarians, and participating during each of his three summers at Michigan in the survey work on the lakes. When the Jenningses left Michigan, the Pearls took their house. When Jennings left the University of Pennsylvania for Johns Hopkins, Pearl took his place.

The relative positions of the two men then reversed. Pearl took the lead in developing statistical and mathematical biology; he worked closely for a while with Karl Pearson in London. Jennings later followed Pearl's lead in these fields and made important contributions to them. Pearl also took the lead in applying statistical techniques to a study of conjugating paramecia; he thereby raised the question of assortative mating, *i.e.,* a tendency of like to mate with like. Jennings then made comparable and decisive studies of the subject. During World War I, Pearl was head of the Statistical Division of the Food Administration in Washington, D.C., under Herbert Hoover: and Jennings was a member of Pearl's staff, assigned to the statistics of sugar supply, needs, and distribution.

In 1918, Pearl followed Jennings to Johns Hopkins, but to a part of the university five miles distant—the new School of Hygiene and Public Health. Seven years later, when Jennings was trying to get support from the Rockefeller Foundation for work of his department, Pearl succeeded in obtaining from them magnificent support for five years to set up for himself at Johns Hopkins an independent Institute for Biological Research. Jennings's hopes were dashed. However, at the end of this five years, the Rockefeller Foundation ceased to support Pearl's institute and it came to an end. In the same year, Rockefeller began a modest continuing grant to Jennings in support of his research.

Like as they were in the nature and breadth of their interests, Pearl and Jennings were at opposite poles in personality. Totally unlike Jennings, Pearl was aggressive, positive, partisan, highly (and it seems happily) controversial, exuding self-confidence and authority. To close the book on the relations between these two giants, after Pearl died in 1941, Jennings wrote (1943) a long, fully appreciative, generous, and sympathetic biographical memoir of him for the National Academy of Sciences.

Pearl was not the only person to be involved in changing relations with Jennings. Another was H. B. Ward. During one of his student years at Michigan, Jennings was Ward's laboratory assistant. In 1902, Jennings was Director of the Survey of the Great Lakes, while Ward was a member of his staff. Less dramatic was Jennings's shift with Mark. Mark was the supervisor of Jennings's Ph.D. thesis work at Harvard. In the summer of 1906, Mark and Jennings served as equals in a trio (the third being E. B. Wilson of Columbia) of top-level advisors to the University of California in regard to the establishment of an oceanographic branch at La Jolla. Of this, Jennings wrote: "It seems remarkable to be made one of a trio with these two."

Part of Jennings's commitment at Michigan was to write a book on the anatomy of the cat, begun as a set of laboratory directions by Reighard. This he completed during his first year at Michigan (1899-1900). Again his work included drawings by his wife, for which she received $400. The Reighard and Jennings *Anatomy of the Cat* remains to this day the standard text on the subject; for many years, it yielded Jennings modest royalties.

The first year at Michigan was a good year. Although he didn't receive a grand salary, Jennings had the precious gold of time to do research. Both of the graduate students in zoology elected to work with him, and his scientific output again began to zoom. He had to give only one lecture a week for the hundred students in the introductory biology course and an hour or two a day in the class laboratory, which was looked after by five assistants. Jennings himself went over the students' notebooks and had the responsibility for all purchases and for keeping the accounts of the whole department. A few months after arriving at Michigan, Jennings was already being sounded out by "a good university" for an assistant professorship to start a Department of General Physiology, but this came to naught. Financially strapped, he thought of trying to add to his income by writing a textbook on introductory biology and started work on it.

At the end of the first year at Michigan, feeling relatively secure, the Jenningses made the decision to splurge on a delayed "wedding trip" to Europe. Landing in Holland, they traveled up the Rhine and on to Switzerland and Italy. Although their activities were limited by the recurring illness of Mrs. Jennings, the summer was a great antidote to the hard work of the preceding three years. Jennings's sense of time and values kept his balance of interests and activities, if not his bank balance, during these years of stringent finances. He never forgot that he had only one life to live and should make the most of it; and he always strove not to let his scientific interests and ambitions deprive him of broader interests and time to share them with his wife. Only when fighting for survival and a push ahead did he let himself temporarily neglect broader interests.

When he returned to Michigan in the fall (1900), after the delights of Europe, Jennings's funds were so low that for a long time he had to draw his salary a month in advance to pay current expenses. Seeing no chance for advancement in salary or rank unless an offer came from elsewhere, he kept looking. His failure to rise at the time was not due to lack of effort on Reighard's part. He considered Jennings to be "without an intellectual superior" in American biology and encouraged and supported him in every way he could —allocating him everything he wanted in the biology building then being planned, assigning him minimal teaching duties, and giving him all the apparatus he requested.

At this time, Jennings encountered, and was long to be plagued by, opposition to his research claims, since his behavior work ran afoul of the views and reports of one of the foremost biologists in America—Jacques Loeb, and his student, Garrey. This conflict is worth examining as an example of a basic schizophrenia that has plagued biology for centuries: the split between those who see life as solely physico-chemical, *i.e.,* the reductionists, and those who see life as a new level of complexity far above the simply or solely physico-chemical. Jennings had begun to crystallize his position as a student at Harvard when he recoiled from the simplistic reductionist teachings of Davenport. In his first experiments on the behavior of *Paramecium,* while still at Jena, he had been almost overwhelmed by the complexity of the behavior of this "simple" cell. As his studies continued in subsequent years, he showed that the responses of the cell were a function of its *gross* structure, important aspects of which were the cell's asymmetry and correlated spiral movement. Similar results were obtained on various unicellular and multicellular organisms. They could not be understood as simple physico-chemical materials, but only as complex arrangements of such materials into a higher level of organic structure that had new properties and modes of functioning.

For centuries, as again today, there have been recurrent efforts by the most "advanced" biologists to make biology a "hard" science, *i.e.,* physico-chemical. In the first decades of the twentieth century, Loeb was greatly admired as a leader who pushed biology in that direction. Trained primarily in physics and chemistry and, in important respects, innocent of biology— like the current generation of most "advanced" biologists—the Loeb school believed it could be shown that the behavior of cells was in fact simply a system of physico-chemical reactions; and, what was worse for Jennings, they reported their failures to confirm his results in their attempts to repeat his experiments. So they attacked Jennings viciously in their papers and thought of him as a "vitalist," *i.e.,* one who resorted to a nonmaterial vital force of an essentially nonscientific nature. This was a total misunderstanding of his position.

Jennings seized opportunities to meet with Loeb and to try to clarify his position, to explain how he had been misunderstood, and to pinpoint reasons for their disagreements in experimental results. He appreciated the importance and value of Loeb's efforts to stress physico-chemical aspects of biology, but found him surprisingly ignorant of some critical aspects of the cells, especially their asymmetry. The personal encounters between the two were cordial and pleasant, but the publications of Jennings's opponents, especially Garrey, remained unaltered in tone. Finally, at the Christmas meeting of the AAAS, Jennings performed under a microscope the experiment that Garrey had claimed couldn't be repeated, the whole experiment being projected through the microscope onto a large screen for all to see. He also repeated the whole show privately for Loeb, who said he was now convinced that Jennings had been right all along and that he would tell Garrey to correct his statements in his next paper. Interestingly, in his presentation at the meeting Jennings did not mention the names of those who had been denying the validity of his experiments. To his friend Neal he wrote: "I . . didn't give Garrey the general blowing up that I had come loaded for. It would have been the easiest thing in the world, as his paper is a fearful thing,—full of errors of the most fundamental nature, that positively vitiate the whole thing and I was in a position to demonstrate this with the stereopticon, but refrained. It's a good thing anyway to have this in reserve, for Loeb they say isn't to be depended on, and may later go back to his old attitude even after admitting what he did. But he certainly treated me finely, and I enjoyed being with him very much. He showed though that he doesn't know anything about this particular matter and isn't competent to work on it or talk about it at all: many of his ideas were positively comical, in view of the facts." So Jennings went about his business, giving similar demonstrations and lectures to Davenport's class at the University of Chicago and elsewhere. He was justifiably confident of the correctness of his work and chose to show the evidence instead of using merely argument or invective to make his points.

Meanwhile, things got worse for Jennings at Michigan. Early in 1901, Reighard had a recurrence of the breakdown he had experienced some years before, and had to enter a sani tarium. Jennings, in addition to his regular duties—a new course in general physiology and supervising four research students—gave the lectures for Reighard's comparative anatomy and cat courses. He had no time for research. And he was still at the bottom of the academic ladder, an Instructor at $1200, still borrowing his salary a month in advance. He was discouraged and tried to get some inspiration by reading biographies of successful scientists—Huxley, Kölliker, and Pasteur. To Neal he wrote: "I have been getting somewhat discouraged over science, living along thus, drawing my salary in advance with no prospect of advancement nor doing the things that it is nearest my heart to do. I sometimes speculate on whether it is too late to turn to something else, where a man would have a chance of becoming more nearly free: I'm afraid it is." He would never have believed that six years later he would be a full Professor at a top university with most of his time free for research.

Happily, an outside offer came in the spring of 1901. A Detroit newspaper recorded the threatened exodus of one of Michigan's best men, saved only at the last moment by promotion to Assistant Professor and an increase of salary to $1600. Jennings also was temporarily given Reighard's place as Acting Director of the survey of the lakes for the summer of 1901. This he didn't enjoy, because it left him no time for research and because, as he wrote to Neal, "Management of men and affairs, official correspondence, and the like, is unaccustomed work for me, and not that for which I was especially designed." Yet he was to do just that for nearly thirty years. After another busy and fruitful year at Michigan (1901-1902), he was appointed Director of the lake survey for the summer of 1902, his last stint with the survey.

Still mired in money problems, Jennings pushed ahead during the academic year 1902-1903 on the proposed potboiler, the textbook of introductory biology. He finished the writing and

Mrs. Jennings was working on the illustrations. They were still not finished four months later. Whether the illustrations were ever finished or, indeed, whether the book was ever submitted for publication, I have been unable to discover. In any case, the Jenningses' energies were soon directed to a new adventure. one of the happiest of their lives.

It came about in this way. The Carnegie Institution of Washington had sought Jennings's advice on how they might best spend institution money. He advised them to use it to free good men (not mentioning himself) of other duties so they could have time for research. Carnegie responded with a grant of $250 to Jennings to aid his investigations. (He needed the money for apparatus, reagents, typing, and drawing.) Soon thereafter, the institution made him a grant of $1000 toward a research table and expenses for a year at the Naples Zoological Laboratory. Then, in June 1903, Jennings's father died, leaving financial problems that put some temporary strain on the son. To make a go of the Naples venture, he earned an extra $100 teaching for Reighard a new course on experimental embryology and $240 teaching summer school at Michigan. His total wealth at this time was about $600. With the Carnegie grant, he thought he could manage a year in Italy.

During the spring and summer of 1903, while preparing for the year in Naples as Research Assistant of the Carnegie Institution, the long wished for outside offer was being negotiated. Professor Conklin, then Head of Zoology at the University of Pennsylvania, offered him an instructorship at $1750. Jennings, already Assistant Professor at Michigan, held out for the same rank and for a leave of absence the first year so that he could go to Naples. Conklin agreed. Reighard tried hard to keep Jennings at Michigan and personally pled for this before the regents, but the university had sunk its funds for biology in a new building and the regents could not allocate anything for a salary increase. So in August, the Jenningses left Ann Arbor for good and looked over Philadelphia on their way to the steamboat.

The year at Naples was both delightful and exceedingly productive. Their joint diary records the couple's daily life, their work and play, their ills and joys, their impressions of people and sights and events. They made many new and lasting friends at the Naples Laboratory, including Hans Spemann and the protozoologist Penard. Their capacities for broad enjoyment of the world now had the chance for exercise that had heretofore been largely stifled by hard, narrow work except for the summer of 1900 in Europe. Jennings not only pushed his research on behavior considerably further, but also wrote seven research papers summarizing in logical order his work and thought on the topic. These papers were published as a book, *Contributions to the Study of the Behavior of Lower Organisms.* (Publication #16 of the Carnegie Institution of Washington, 1904). While still in Italy, Jennings was approached by Whitman of the University of Chicago in regard to filling the post left vacant by Davenport's departure. Matters moved so slowly during the summer of 1904 that Whitman decided it would be unethical to bring them to a head so close to the beginning of Jennings's engagement at Pennsylvania. This decision relieved Jennings, especially since Whitman promised to renew negotiations later.

At the end of this great year, the Jenningses returned to the United States and set up shop in Philadelphia. A few months after he started, Chicago made him the promised offer of $2500 for the next academic year, but the Jenningses agreed to stay at Pennsylvania for $2250. They loved Philadelphia, their association with the Conklins, and the cultural opportunities of the city. Mrs. Jennings studied drawing and painting at the Academy of Art. Jennings taught only two advanced courses, on general physiology and animal behavior. The buildings and equipment were poor; quarters were cramped; "but after all, those aren't the chief things in life. I think associations with pleasant and interesting colleagues is the best thing, and that I have here."

The years at Pennsylvania (1904-1906) were years of intense activity for Jennings. His behavior work had become widely appreciated and he was in demand to give outside lectures. Several, given once per week, on "The Beginnings of Mind," at the Brooklyn Institute of the Museum of Arts and Sciences, were particularly wearing—he started back to Philadelphia at midnight after each lecture. During the spring of 1905, his health began to decline. He lost considerable weight, which he could ill afford; his digestion was badly upset; he had a succession of severe colds; he developed a mild case of albumenuria; and his appendix began to cause trouble. On examination for life insurance, he was refused. So he was shocked into taking it easy during the summer of 1905. Although only thirty-even, he wrote, "I can't work so steadily as I used to, and it looks as if I can't expect to do any more than my college work and keep well."

He spent June at the Tortugas Laboratory which was run by his old friend, Mayr, from the summer at Newport, and there met Professor Brooks, famous researcher and biological philosopher and Director of the Zoological Laboratory of the Johns Hopkins University, who was soon to play a major role in his life. Brooks had been a referee of Jennings's manuscript for the Carnegie book on behavior and was much impressed by it; but of this Jennings knew nothing. The rest of that summer was spent at Woods Hole, Massachusetts. This was Jennings's first visit to this great summer gathering of biologists. There he gave several lectures and continued work on another book, *The Behavior of Lower Organisms,* but at a pace that permitted him rapidly to regain lost weight and recover his health. Of this health episode he wrote: "I am thoroughly convinced that continued immurement in a brick house in a city, along with a good quantity of college work, would finish me up within a limited period. There has seemed some prospect that I might get out of college work, into a pure research position with the Carnegie Institution. That would allow us to arrange life on a proper basis—live long and be happy, and get some good work done, too!" Such an offer never came; Jennings went on till his retirement "with a good quantity of college work." From this time on, he was to fight repeatedly the battle of trying to regulate his life so as to keep well and still do scholarly work.

The recuperation during that outdoor summer at Woods Hole, where he lived in a tent, encouraged him to commit himself heavily for the coming year. Professor E. B. Wilson invited him to give the lectures in the Visiting Biological Lecturer Series at Columbia and to have his new book published by the Columbia University Press. He accepted, raised about $1000 for the publication of the book, hopefully to be recouped by receipt of two-thirds of the sale price of each copy sold, and gave a series of five lectures at Columbia in February 1906.

These lectures were repeated in the same month at the Women's College of Baltimore (later Goucher College) at the invitation of Professor Maynard M. Metcalf. A particularly interested member of the audience was Professor Brooks, who, as mentioned earlier, had his eye on Jennings as one of the up-and-coming young biologists. He invited Jennings to lecture to a joint meeting of his department with the Department of Psychology and Philosophy, presided over by the distinguished Professor J. Mark Baldwin. The lecture garnered Jennings an offer to come to Johns Hopkins as Associate Professor of Experimental Zoology, salary $3000, with the promise of succeeding Brooks as Director when Brooks retired. In his diary, Jennings wrote: "This astonished me extremely; it seemed impossible. But following the maxim that one should accept his opportunities, I accepted." But not without hesitation at leaving his beloved Philadelphia.

Before going to Baltimore, Jennings used the excuse of the visit to La Jolla with Mark and Wilson to spend the summer in California. He and his wife were entertained by the wealthy Scripps family and had joyous times with some of their own relatives, especially his sister Aldie and his brother George. In his diary he wrote of George's wife, the former Lulu Plant, whom he had admired as a youth in Tonica, that she was "as charming as ever." The California visit brought back memories of his childhood. He visited Artesia, where thirty-two years earlier his father and uncle had tried to farm until the water failed them, and the nearby schoolhouse, where he had begun his schooling exactly thirty years earlier. He and his sister relived their early years in going over the papers, including the remarkable autobiography of their physician father, who had died in California just three years before. Jennings also found time to carry out an investigation of the modifiability of the behavior of the starfish, his last research on behavior before changing fields. On this and his other behavior work, he gave greatly admired lectures at La Jolla, San Diego, Berkeley, and Chicago before settling in Baltimore just before the fall term of 1906.

Jennings's experimental research, until the call to Johns Hopkins, was all conducted on the reactions of lower organisms to stimuli. From the start at Jena ten years earlier, he had had the insight to recognize the bearing of his work on psychology and he took and made occasions to drive this important point home. Previously, the two fields were largely pursued independently without much mutual awareness. His efforts to overcome this isolation were outstandingly successful. The leading behaviorist of the day, John Watson, attended Jennings's lectures at Johns Hopkins and was clearly influenced by him. Jennings's book, *The Behavior of Lower Organisms,* was repeatedly reprinted, most recently by Indiana University Press in 1962, fifty-six years after the first printing. In the preface to this reprinting, another psychologist, Donald D. Jensen, wrote that the book remains "a basic text for the student of animal behavior . . . a work . . . important in the history of experimental psychology." No subsequent researches by Jennings exceeded in importance and lasting effect on science those he carried out on behavior in the first decade of his research career.

Starting work at Johns Hopkins in the fall of 1906, Jennings was promoted in 1907 to Professor of Experimental Zoology. He wrote to his old friend Neal: "And so I have finally come to man's estate in my profession! Eight hours of teaching per week for four months: the rest of the time for research! I never expected to find such a place: indeed, I wouldn't have believed such a one to exist in American universities! I had a chance to go back to the University of Pennsylvania on this same footing of work, rank and salary and was much tempted to do it . . . . The men in this department here are geniuses, but eccentric, which doesn't make the social side as attractive as at Philadelphia." But he stayed at Johns Hopkins for thirty-two years, until he retired in 1938.

A few years after Jennings's arrival at Johns Hopkins, Brooks, the Director of the Laboratory, died. President Gilman appointed a committee to recommend a successor. The committee recommended T. H. Morgan of Columbia, then at the start of the great period of his work that established the chromosomal and gene theories of heredity. No one could find fault with this choice—except Jennings. When he got wind of the committee's decision, he confronted President Gilman with the letter that set forth the terms on which he had come to Johns Hopkins. One of the terms was that he was to succeed Brooks on the latter's retirement. Gilman had no recollection of the agreement, but he stood by his word. So Jennings became Henry Walters Professor and Director of the Zoological Laboratory in 1910. He had indeed reached the top at the age of forty two. And he learned a lesson: have all agreements in writing. That lesson he never forgot. He put all of his own agreements in writing, spelling them out in full. I know, because when I became his research assistant in 1930, he wrote me a letter stipulating every aspect of my duties. This hurt my feelings and I told him that there was no need for such a letter, that he should know he could count on me to do whatever he wished. It was then that he told me the story of the letter from Gilman and of his resolve and practice based on it.

## AT THE, SUMMIT: JOHNS HOPKINS (1906-1938)

Formerly of great importance and influence, the Department of Zoology at Johns Hopkins had to a considerable degree lost its eminence and was attracting but few graduate students when Jennings arrived on the scene. Brooks, who had earlier turned out a large number of foremost biologists, had grown old and ill and had ceased to be a leader in research. Naturally, he did not represent the currently exciting new lines of work. Jennings had doubtless been brought to Johns Hopkins in the hope that he would reverse the trend. He began by teaching and directing research in areas of experimental zoology, behavior, and general physiology.

Before the first year at Johns Hopkins came to an end, Jennings's own research had shifted to the exciting field of heredity, now revitalized by the rediscovery (1900) of Mendel's papers. He had intended to start work along this line in the summer of 1906, before going to Johns Hopkins, but put it aside to spend the summer in California. After his teaching duties for the first year ended early in 1907, he started what was to prove his major research effort for the next decade: investigation of heredity in Protozoa.

I shall not attempt to recount here the course of Jennings's life at Johns Hopkins in the detail given to his career up to this point. My intention has been to try to expose as fully as the available evidence permits what went into the making of this man, how he developed into what he became. I do not mean to imply that he did not continue to develop after reaching the summit of his academic success, for he did. He recognized that people come as novices to each stage of their life and that one's experiences continue to modify one throughout life. Nevertheless, by 1906, aged thirty-eight, Jennings's character and capabilities were pretty well determined. What further changes occurred during the next seventeen years can only be guessed, for he either lacked the time or the inclination to keep a diary during this period, and very few of his letters have come to my attention. On the contrary, many large volumes of diaries record his life, work, and thought from 1923 to 1945. These I have only sampled and so am not yet prepared to use to full advantage. For the most part, therefore, the remainder of this biography will be mainly an account of events and accomplishments. In those respects, the years at Johns Hopkins consist chiefly of two widely different periods: one of intensive investigation (1907-1916) and one of varied activities (1917-1937).

Jennings's work on genetics followed the same general pattern as his work on behavior. In both fields, he began by investigating as thoroughly as was then possible a single, relatively simple organism. He then made a comparative study of other organisms at about the same level of complexity, other ciliates and other Protozoa, following this with studies of multicellular organisms. At each step in the sequence of studies, he noted the special features exhibited by each organism and the common features or generalities shown by organisms at similar or different levels of complexity, ending with the fundamentals and their implications for biology and for man. Finally, he synthesized this approach to the two fields of behavior and genetics into a beautiful, integrated view of science, philosophy, and the practice of human living.

During the years at Johns Hopkins, especially during the first decade when no longer engaged in experimental work on behavior, he carried on lively discussions of the significance of the behavior work in a number of book reviews and general papers. He identified the behavioral properties of cells and organisms with the properties of their supramolecular levels of organization of matter. His radically experimental analyses led him thus to a monistic view in which mind could not be separated as distinct from matter. The human mind and human behavior were as determined in their operation as those of lower organisms, and they operated on the same basic principle of the interaction of outer factors with inner structural factors. In the lower and higher organisms, and in man, behavior was viewed as determined, though at increasing levels of complexity, by the properties of organismic structure and its responses to previous experience. Behavior was shown to be modifiable by experience, the modification having a material basis in altered physiological states. In man, these experiences included perception of outer events and such inner events as sensations, feelings, emotion, and thought, all themselves properties of the highly complex material organization of man. This view, obviously far removed from the views of the simplistic mechanists, likewise left no place for the vitalists' "entelechy" or "élan vital."

Towards the end of this career, Jennings shifted his focus from the level of the cell and the individual organism to the social level of groups. Again he began with paramecia, seeing in their mating behavior the beginnings of the development of social systems and social behavior. He also gave some attention to social behavior and organization in higher organisms and would doubtless have carried this much further had he been able to remain active longer.

His genetics studies, begun in 1907, came at a time when leaders in biological research were attempting to test the generality of Mendel's laws. Initially discovered in work on higher plants, they were soon extended to higher animals by Bateson in England and by Cuénot in France. This work settled the question of generality for all higher organisms. It left open, however, the questions of how early in evolution these laws began to operate, whether they evolved independently in plants and animals, being somehow connected with multicellularity, and whether some more primitive and simpler mechanism of heredity existed in unicellular organisms. There was also a need to find organisms that would be more favorable for pushing genetic analysis still further. All of these considerations were probably in Jennings's mind when he made the decision to shift from work on behavior to work in the new field of genetics. He probably also foresaw the ultimate desirability of examining the possibility of hereditary individual differences in behavior. For reasons that will become evident, he did not succeed in establishing unicellular organisms as the most favorable material for genetic analysis. T. H. Morgan's work, begun two years after Jennings started work on *Paramecium,* established the fruit fly, *Drosophila,* as the choice organism at that time and for decades to come; but eventually—more than thirty years later—Jennings's decision to turn to the simplest organisms was vindicated by a great period in which others used them for the deepest and most fundamental penetration into genetics. Jennings barely lived to see the beginnings of this movement; he died before it reached its greatest achievements. His choice of paramecia was premature and he did not go far enough in his choice of simple organisms. The bacteria and their viruses proved in the 1940s to be the rewarding materials. Nevertheless, Jennings laid broad and deep foundations for all later work on the genetics of the Protozoa.

In this new field, his earliest studies (1908) were made on the processes of reproduction in paramecia. They showed—contrary to prevalent opinion, including his own—that they were fundamentally the same as in higher, multicellular or ganisms. This was an essential step toward the ultimate unification of genetics. He then proceeded to establish, with characteristic abundance of quantitative data, that the asexual progeny of a single cell—a clone—of *Paramecium* exhibited genetic constancy. Variations among the cells of a clone were due to diverse environmental conditions or to stages of growth and development. In other words, they were phenotypic, not genotypic, diversities. The principle of the genetic uniformity and constancy of the clone, established first for *Paramecium* and by his students for other ciliates, was later extended by other students to asexually reproducing multicellular organisms (*Hydra,* rotifers). This has remained a basic principle of genetics for all organisms that form clones.

Jennings then turned (1910-1913) to the study of heredity in the sexual reproduction, conjugation, of *Paramecium.* This was to be the test of whether Mendelian laws, or some simpler or different laws, held for unicellular organisms. But the test required making crosses between the members of genetically different clones. To his great disappointment, he was unable to make the necessary crosses. There was no lack of paramecia differing in visible hereditary characters, but the different types refused to mate with one another. In fact, he found that the conditions required for mating to occur were different for the visibly different clones, so that when two kinds were grown together, each mated only with its own kind, even when both kinds were mating at the same time in the same culture vessel. The explanation of this annoying limitation on analysis and the discovery of the means of circumventing it did not come until a quarter of a century later.

Meanwhile, Jennings proceeded to accomplish what he could within the technical limitations imposed at that time. He showed that conjugation between paramecia of the same kind produced many hereditarily diverse clones. This, of course, agreed in a general way with expectations based on Mendelian laws, if the initial paramecia were heterozygous, *i.e.,* genetically impure. However, he could not test this directly, because he was unable to make crosses among the diverse clones thus produced.

So, he approached the matter indirectly. If the variations were due to genic recombination, such variations should not arise at conjugation in genetically pure (homozygous) paramecia and homozygosity should be producible by successive inbreedings. In the same way, Johannsen had obtained pure-breeding lines of beans by successive inbreedings. To his astonishment, he continued to obtain hereditary diversities in abundance in spite of successive self-fertilizations. His contemporary, Victor Jollos, obtained comparable results and concluded that the hereditary variations that both he and Jennings observed were not due to Mendelian phenomena, but to temporarily persistent variations (*Dauermodifkationen*) in expression of a constant, pure genotype. Jennings, however, was unwilling to accept this interpretation without exploring the possibility that the variations might be due to an initial very high degree of genetic impurity (heterozygosity) that would still be high after a limited number of successive self-fertilizations.

This led him to calculate the mathematical expectations on Mendelian theory. His calculations made this explanation unlikely and he cautiously concluded that Mendelian shuffling of the genes might not be the whole of the matter. Nevertheless, the erratic nature of Jollos's results prevented him from accepting Jollos's interpretation. Twenty years later, Jennings's students showed that pure-breeding lines of paramecia could in fact be selected and that some of the variability was due to mechanisms other than Mendelian recombination. Only later, very near the end of Jennings's life and too late for him to assimilate it, did the beginnings of a full understanding of the matter emerge from observations of his former student, Sonneborn, on "nuclear differentiation." After his death, discoveries of the transient and persistent regulations of genetic activity via repression and derepression mechanisms further clarified the situation.

Jennings's excursion into the mathematics of heredity (1911-1924) was a delightful exercise of his talent for mathematics. During some periods of his life, he would start his day with pure mathematical studies for the sheer love of them. His late but joyous encounter with calculus called forth the conviction that its study should come at the beginning of a biologist's training. With such proclivities, his long and varied pursuit of mathematical applications to biological problems was inevitable. The earliest (1911-1917) applications, those directed to the analysis of successive inbreedings in paramecia, led him to pursue the analysis for various systems of breeding in a series of contributions that were among the first in what was to develop, in the hands of Fisher, Wright, Haldane, and their followers, into the important discipline of mathematical or population genetics. Later (1918-1924), Jennings applied these talents to the analysis of the theory of the linear arrangement of the genes in the chromosomes, using the data of the Morgan School on crossing-over and interference in *Drosophila.* In the midst of these studies, he gave one of the evening lectures at the Woods Hole Marine Biological Laboratory on the subject of the theory of the linear arrangement of genes in the chromosome.

I have heard that the events were somewhat as follows. The lecture was announced as a critique of the theory, without any indication of which side of the current controversy Jennings would attack or defend. Morgan and his co-workers were at Woods Hole at the time and Morgan fully appreciated Jennings's mental power and influence. So before the lecture he made it his business to seek out Jennings, give him the latest data, and convey to him the confidence his group had in their interpretations. Perhaps, in his affection for Jennings, Morgan didn't want him to make a fool of himself, as if that were possible; but in this case, the data were rather abstruse and not widely understood or appreciated. Jennings gave Morgan no clue of what he would say in his lecture.

When the time came, Morgan and his cohort were sitting in the front row prepared to defend themselves from the anticipated attack. Instead of attacking, however, Jennings showed that the complex set of data fully justified the interpretation made and could hardly be reconciled with any theory that was not essentially the same. After the lecture, Morgan rushed to the speaker's platform and threw his arms around Jennings.

Meanwhile, Jennings was struggling with the problem of evolution. He was unwilling to accept as final his demonstration of the genetic constancy of the clone, but sought "evolution in progress." Surprisingly, he sought it in change of heredity within a clone. For this sort of work, involving selections, paramecia were not favorable; it was impossible to select for small hereditary differences in characteristics that were so phenotypically variable during growth and development and so responsive to environmental variables. So he hit upon the much more favorable shelled rhizopod, *Difflugia.* Its shell (or "test") was formed at the time of cell division and then persisted unchanged. Selection for variations in shell characteristics eventually yielded (1916) within a clone minute but statistically significant differences that persisted for some generations in the absence of further selection. This welcome result was held by Jennings to be "evolution in progress." Although this work was important for the thinking of his contemporaries, the basis of the diversities selected and their durability remained obscure and has not to this day been clarified—except for one special case of outstanding significance.

This case, returned to in the 1930s and reported in full in 1937, concerned the so-called mouth and teeth of the shell.

The mouth is merely a circular aperture in the shell and the teeth are a circlet of small projections from the rim of the mouth. During reproduction, half of the cell mass extends through the mouth and is at first naked, a new shell with mouth and teeth forming on it before the cell divides into two. Remarkably, the mouth and teeth of the new shell are formed in juxtaposition to those of the existing shell, each tooth of the new shell forming in the space between two of the teeth of the old shell by what we would now call a "negative template" mechanism. Jennings's experiments included removing some or all of the teeth and some of the adjacent shell, with demonstration of their correlated effects on the shell of the daughter cell. Many years later, in the 1950s (after Jennings had died), the discovery of "template replication" in the reproduction of genic DNA prepared biologists to appreciate the significance of Jennings's discovery of a comparable process at the supramolecular level of structure.

After his basic studies of 1907-1916, which culminated with the conclusion that he had demonstrated evolution in progress, Jennings became interested in the causes and nature of these "evolutionary" variations. In his day, Lamarckism was still defended in some quarters on the basis of purported observations and experiments, especially by Kammerer. Jennings repeatedly reviewed the evidence critically and stimulated students to undertake studies of the possible inheritance of characteristics acquired as a result of environmental action. He was well aware of the alternative of selection of spontaneous or undirected mutations, but he kept an open mind while demanding critical evidence.

I was one of the students he stimulated to undertake such experiments. I recall vividly his questions and comments on this topic during my oral examination for the Ph.D. degree in 1928. After asking me about the work in a number of other laboratories on this topic, he then asked what had become of the investigators. Fortunately, I knew that most of them had committed suicide or gone out of their minds. He concluded with: "Let that be a lesson to you!"

From 1916 until the early 1930s, Jennings published only one laboratory investigation, but he directed the researches of many students. Some of them (including Stocking and Middleton) carried on studies of *Paramecium* and other ciliates; others (including Root, Hegner, and Taliaferro) extended to other rhizopods the kind of study he had made on *Difflugia;* and some extended work to multicellular organisms. Karl Lashley, later famous as a psychologist, who was probably attracted to Jennings because of his work on behavior, found Jennings steeped in genetics and, entering into the spirit of the laboratory, was the first student to extend the genetic work to multicellular organisms. He did his thesis research (1911-1915) on inheritance of tentacle number in *Hydra.* Soon thereafter (1917-1920) Jennings inspired Bessie Noyes to carry on genetic work with rotifers, the organisms on which he had made his first extensive studies, in the 1890s. Beginning in 1920, he also turned to rotifers, along with his assistant, Ruth Stocking Lynch. The pressure of other duties and commitments prevented him from concentrating on his work. It was done intermittently, largely in summers, and the statistical analyses and writing dragged on for years. Finally, the results appeared in 1928 in two papers on the life cycle of *Proales* during parthenogenetic reproduction and on factors, particularly maternal age, affecting fecundity and length of life. Among other students participating in the program on multicellular organisms were J. Finesinger and Helen Miller (Costello) working on a rotifer, Emily Emmart on rotifers and G*ammarus,* and Sonneborn on the flatworm *Stenostomum.*

Jennings meanwhile was developing his views of the evolutionary process, which he published repeatedly. They were based solidly on the *Drosophila* work of Morgan's laboratory, especially on the series of multiple alleles and specific modifying factors affecting eye color. These provided ample evidence for the minutest hereditary variations; hence they supplied the materials for the operation of Darwinian natural selection of almost imperceptible gradual changes. The demonstrated genetic complexity of an apparently simple character led Jennings to stress strongly the basic error in the notion that each of the kinds of unit characters under study was determined by only one gene.

In 1930, with support of a grant from the Rockefeller Foundation, Jennings returned for a few years to a reinvestigation of the genetics of paramecia. He soon lost heart in it, however, because of his inability to cross genetically diverse clones, so he went back to *Difflugia.*

At this time, I was his research assistant. I asked, and he generously gave, permission for me to continue to work with *Paramecium* with the objective of putting it in condition for standard Mendelian analysis. His generosity paid off in 1937 with my discovery of mating types that made crossbreeding and Mendelian analysis at least as easy as in higher organisms and that added certain additional advantages for classical genetic analysis. He was overjoyed with this new turn of events and came back himself to a renewed investigation of the genetics of paramecia, choosing the species *P. burisaria,* while I continued to work with *P. aurelia.* At this time, Jennings was sixty-nine, a year from retirement; but he was still able to carry on exhausting and exhaustive laboratory work with remarkable vigor and intellectual power. The story of this work, done mainly after his retirement, will be told presently. First, however, to complete the picture of his thirty-two years at Johns Hopkins, let me try to portray the life he led at Johns Hopkins while the researches and writings mentioned above were in progress.

Jennings's early years (1906-1910) at Johns Hopkins were golden: exceptionally light teaching duties, ample opportunities for research, no administrative responsibilities. He threw himself with astounding intensity into genetic research and publication, while maintaining interest in behavior through teaching, outside lectures, and publications. His work almost cut him off from the prized luxury of correspondence with family and friends. This was one price he had to pay for what he called the "inhuman" concentrated pursuit of scientific investigation. On the other hand, there were rewards, both in the joy of research and in high recognition. The first of several honorary degrees was conferred in 1909 (by Clark University); he was elected President of the American Society of Zoologists in 1908 and to membership in the National Academy of Sciences at the relatively young age of forty-six (1914).

His elevation to the position of Director of the Zoological Laboratory in 1910 naturally brought about great changes in his life. One of his first moves (1911) was to fill the position left vacant by his own promotion. He brought in S. O. Mast, whose research he had supervised and found impressive during the summer of 1903 at Michigan. Now that Jennings had turned to genetics, Mast took over the work in general physiology. Jennings had driven himself so hard in research that by 1911 it began to tell on his health; he had to curtail greatly his working hours. He tried to recover his strength during the summer of 1911 by a trip to France and England, which he greatly enjoyed, for it satisfied his recurrent longing to expand his horizons beyond the confines of the laboratory to the whole range of the great works of man.

Jennings's level head, keen mind, sound judgment, and objectivity were soon recognized and exploited by his university colleagues. The Academic Council had been appointed by the President prior to 1912; thereafter it was elected by the faculty. At the first election, Jennings was overwhelming first choice; he was elected continuously thereafter until 1934. In the affairs of the university, he was a foremost spokesman for the faculty and was increasingly called upon to chair or serve on the most important university committees. When the university administration made what he dubbed "half-hearted" attempts to discontinue the undergraduate college and concentrate on graduate work, Jennings led the faculty in an unsuccessful effort to bring this about. As the years wore on and he became increasingly taken up with university affairs, he commented sadly that the decline of scholarly work by professors as they grew older was not due to failing powers or interest but to their being put on committees.

Within his own department, the story was similar. Administrative work absorbed increasing amounts of his time and energy, leaving less and less for laboratory research. For a few years he managed to keep at it pretty vigorously along with a group of students, especially Lashley, Root, Ruth Stocking, and Middleton. Together they presented a symposium on the genetics of lower organisms at the December 1914 meeting of the American Association for the Advancement of Science. The importance and influence of the Johns Hopkins laboratory was again on the rise, fulfilling the design of Brooks. When the School of Hygiene and Public Health was created (1918) at Johns Hopkins, several of Jennings's students, including Hegner and Taliaferro, were chosen to be on its faculty.

After World War I, the number of students in Jennings's department increased to a point that made Jennings deplore the diffusion and dilution of attention to them. He wrote, "I am not good at keeping contact with so many lines of work. Concentration is my successful method." A stream of visitors flowed to the department, and this led Jennings to write: "There is nothing that utterly destroys all chance for scientific investigation or any continuous work, like having a Distinguished Guest on your hands": but, nevertheless, he enjoyed these contacts. He summed up his situation in these words: "There is a strong push toward forcing the head of a department into the position of a factotum, a servant that attends to all sorts of things to further the work of others, but with no opportunity to do serious work himself." Some relief came in 1919 with the appointment of his former student, Ruth Stocking Lynch, as his assistant in both research and office work, but this hardly offset the increase in demands on him.

Nevertheless, Jennings came to realize that he was much appreciated. In 1921, his students, colleagues, and friends presented him with a volume of appreciative letters and joined together in a celebration of the twenty-fifth anniversary of his doctorate. They had Linton paint his portrait, which was eloquently presented to the university by his distinguished colleague, the philosopher Lovejoy. At a festive dinner, his former Harvard teacher, Davenport, summarized his contributions to science and Jennings responded with one of his most charming essays, "On the Advantages of Growing Old." This recognition was of course well deserved and heartily bestowed. Jennings had thrilled many students with his scholarly and penetrating courses on behavior, development, genetics, and evolution. He had opened their minds to the broader aspects of biology and its philosophical overtones in seminars on the method and nature of science, on the history of biological theories, on the body—mind problem, on vitalism and mechanism, on the implications of physical relativity and indeterminism for biology, on eugenics and the race problem, on the relation of biology to human affairs, and on creative and emergent evolution. For some years these seminars were held in the evenings at his home, until they became too much for his wife and eventually for him also. The combination of uncompromising respect for significant facts down to the minutest detail with the broad scope of his encompassing mind was fully appreciated by his students, who considered working with him a treasured privilege.

And, reciprocally, he appreciated his students, perhaps more than they deserved. The year (1928) that I received my Ph.D., he and Mrs. Jennings invited all those receiving their degrees to dinner at their home. After dinner, he made a little speech. He told a bit about each of a number of previous Ph.D.s from the department, showing their pictures and telling of their successes after leaving Johns Hopkins. He concluded with the statement that in his opinion those who were now receiving their Ph.D.s were in no way inferior to those he had been speaking of. No wonder his students adored him!

Jennings's eminence naturally led to participation in many off-campus affairs. He held high office and took part in the work of many national and international biological, psychological, and philosophical societies and meetings (see appended list). He served during World War I with the Food Administration in Washington, D.C. He testified before Congress on immigration policy. After serving five years (1920-1925) with the National Research Council, he was happy to terminate that committee work, for he thought he was not suited to it. Widely sought as a speaker on a great variety of topics by many professional and lay groups, he found it difficult not to accept.

He wore down periodically from sustained tenseness and overwork. When in this condition he used to say: "I am H. S. Jennings only a few hours per day." In order to put his physiology back in working order, he not only cut down working hours but turned to other interests, such as music and literature. Most evenings he played chamber music records and read aloud with his wife and son. He even tried to learn to play the clarinet so he could join local chamber music groups, but he never made much of a success of this and regretted that he was too deliberate to be freely rhythmic. His favorite reading was Pepys's *Diary,* biographies, histories (returning periodically to Gibbon), Elizabethan drama, and some novels, like *Gil Bias.* Such recreation, freedom from evening engagements, and going early to bed restored him so completely that he never felt better—until he was down again, and again became worried and discouraged.

Two events of the mid-1920s particularly lifted his spirits: the Leidy Award of the Philadelphia Academy of Natural Sciences, which prompted him to write, "A bit of recognition of this sort quite cheers one up when he wonders if he isn't dropping behind," and an offer from Stanford University, which he did not accept but which prompted his comment that there was "a certain satisfaction in letting authorities here know that they cannot keep men on the salaries they pay." The President of Johns Hopkins took a different view; he thought it worth so much to be at Johns Hopkins that there was no need to match salary offers from elsewhere.

Nothing gave Jennings more pleasure or sent his spirits soaring higher than opportunities to get away for an extended period from the press of duties at Johns Hopkins. For many years he escaped from the Baltimore heat as soon as possible after the spring semester, spending the summer at the Marine Biological Laboratory in Woods Hole, often with assistants and students, researching and enjoying contacts with the biologists there. Two joyous summers (1925 and 1926) were spent at the Pacific Grove Station of Stanford, where he gave lectures based on those already prepared for courses at Johns Hopkins, so that he was relatively free. And he spent two of the happiest and most wonderful years of his life as Visiting Professor at Keio University, Tokyo, in 1931-1932, and as George Eastman Visiting Professor at Oxford (1935-1936). His diaries tell the day-today tale of these richly varied experiences he shared with his wife.

In one very important respect the increasing complexity of Jennings's life had a most valuable effect. Unable to find the long continuous periods needed for laboratory research, he could in shorter periods prepare and give outside lectures or write and publish essays and books about the matters he dealt with at length in his courses and seminars. Using his gifts for clear thinking and for abstracting main ideas, together with his extraordinary talent for exposition in lucid, engaging language, he became an eminently successful educator of biologists, people in related fields, and the reading public. In the early decades of genetic studies, he was a leader in publicizing its main concepts and their significance for man. He expounded the import of genetics and general biology for the philosopher, the sociologist, the psychologist, the psychiatrist, public health workers, and the ordinary layman. He preached caution and sense to the eugenicists, recognizing the desirability but slowness of feasible eugenic action and the speed with which man's urgent, immediate needs could be met by improving his conditions of life. He saw no threat of genetic deterioration from medical cures or public health measures, which to him were but a continuation of man's evolved gift of the power to overcome his deficiencies.

He published many articles on these and other themes and gathered his ideas together in the popular book, *The Biological Basis of Human Nature* (1930). The ground had been prepared by his little booklet, *Prometheus or Biology and the Advancement of Man* (1925). His textbook, *Genetics* (1935), was focused on principles; it was not rich enough in detail to comply with the notions of college professors as to what a textbook should be. As one reads through these three books some forty years after their publication, one is surprised and impressed to find how little they would have to be changed to conform to the present state of knowledge and understanding. Of course, much more has been discovered, and genetics has been molecularized; but Jennings recognized, from what was then known, what would be the enduring main principles and their significance for biology and man. Stands taken later by his most knowledgeable and imaginative successors are clearly and prophetically set forth in those three books.

In my opinion, however, the greatest and perhaps the most enduring of Jennings's books is *The Universe and Life* (1933). This small book, originally presented as three Terry Lectures at Yale, sets forth beautifully his view of the world and man's place in it. It synthesizes harmoniously what his studies of behavior and genetics had taught him. Each human being is initially unique in his genetic endowment and develops uniquely. The exception to genetic uniqueness—identical twins—is no exception to uniqueness of development. To that there is absolutely no exception. The brain, with all its complexity of structure and capacity for functioning, is genetically determined to develop. Development, of course, includes alterations in mental structure as a result of experiences, and each such change in the material structure of the mind affects its later responses in the form of behavior and of further material change of mental structure. Each step in this continual interplay between experience and the mind is completely determined, in the sense that the effect of each experience is determined by the current state of the brain, itself a resultant of the effects of the whole sequence of previous experiences on the brain—mind structure and functioning. The development of mind in each individual, although determined at each step, is nevertheless unique and unpredictable, because, if for no other reason, of the individual's unique and unpredictable sequence of experiences. In the sense of being free from the step-by-step determinisms operating in any other individual, each individual is free and capable of making choices unique to himself. If each individual is free, unpredictable, and unique (though determined), then so is each generation of man. (If this is not demonstrated by the average man, it surely is by the great men, who put their stamp on their times.) In this Jennings saw man's greatest hope:

What has failed in the past need not fail in the future. The unpredictable future of man will be what man chooses to make it, and that no one can foresee.

## THE FINAL YEARS (1937-1917)

Jennings was sixty-nine, a year shy of mandatory retirement, when he began the series of investigations on the genetics of *Paramecium bursaria.* A gay seventieth birthday party was given for him by his students—gay because it was not an end but a beginning. He would now be free for the concentrated research that he had foregone for so many years. The research he wanted to do looked promising, and he was still strong and active enough to make the most of his freedom. So, in 1938 he became Emeritus Professor and a full time researcher.

Before long his happiness was darkened by his wife's illness. She died in November, 1938. His diaries tell touchingly of his sadness at the loss of the woman with whom he had forty years of beautiful married life. His son and daughter-in-law, Francina Snyder Jennings, lived with him. She was devoted to him and did much to help him through this difficult period.

Fortunately, he was already committed to organize a symposium on the new mating type work for the Christmas meeting of the AAAS at Richmond, Virginia. He threw his energies into this task. He had to give a large part of the symposium himself, not only the general introduction and his own paper on *P. bursaria,* but also Sonneborn's paper on *P. aurelia,* for Sonneborn fell ill with measles just a few days before the meeting. At the symposium, Jennings was a tremendous success. He projected live through the microscope onto a big screen the spectacular agglutinative mating reaction that occurs immediately when cultures of *Paramecium* of complementary mating type are brought together. More than thirty years earlier he had used the same method in Chicago to demonstrate how paramecia aggregate in a region of weak acid.

Immediately after the Richmond meeting, Jennings packed up nearly 2000 of his experimental cultures and set off with his assistant, Elizabeth Kirkwood, for Los Angeles, where he was to be Visiting Professor at UCLA for the spring semester. Unable to get a train compartment from Chicago on, he took an upper berth for himself and a lower for his satchels of culture vials, which he nursed along with tender care. In spite of this, most of the cultures were dead on arrival. But duplicates mailed from Baltimore arrived safely, and Jennings was soon set up to continue his researches while giving a much appreciated course and seminar on genetics of the protozoa. Again teaching proved a severe strain, and he rejoiced when his last class ended, to hearty applause, in May 1939. During the following summer, arrangements were made for him to stay on as Research Associate at UCLA. With support for research assistance obtained from the Carnegie Corporation and from the Committee on Sex Research of the National Research Council, he was able to engage T. T. Chen as cytologist. Chen had worked with him at Johns Hopkins. He also engaged a number of junior assistants, including especially Elizabeth Hegglund, and eventually Ruth Stocking Lynch, who had long been his associate at Johns Hopkins.

His diary of the first year at UCLA records day after day his fearful loneliness, in spite of the presence of co-workers, friends and relatives. His niece, Carolyn Jennings, who taught biology at Los Angeles City College, had long been a great admirer of her uncle; she brought him days of joy at parties and on expeditions. Her mother, Lulu, the widow of his only brother, frequently had him for meals. She always had fascinated him, from the time he first saw her when he was a youth in Tonica, Illinois. His interest in Lulu had been kept alive over the years at family gatherings in California and during her visit when the Jenningses were at Oxford. His visits to her home excited him immensely. She was still beautiful. Judging by oblique entries in his diary, the thought of marriage probably came into both their minds soon after his arrival in Los Angeles. On October 21, 1939, they were married and the fearful loneliness came to an end.

Jennings was still much in demand as a lecturer and continued to comply with invitations through 1943. In 1939, he took part in a symposium at Stanford; he returned during Stanford's Fiftieth Anniversary Celebration in 1941 to give a major lecture, receive an honorary degree, and be made an Honorary Fellow of Stanford University. In 1940, he attended the Bicentennial Celebration of the University of Pennsylvania, again giving a major lecture (the Leidy Lecture) and receiving an honorary degree. In 1941, he gave lectures to a number of California biological societies. For six weeks in 1943, he gave the public Patten Lectures at Indiana University on "Life, Age and Death in Single-celled Organisms in Relation to General Theories of Life and Death." These, I believe, were his last lectures. In the same year came his last honors, an honorary degree from the University of California and election as Honorary Fellow of the Royal Society of Edinburgh.

Then he began to cut down his outside activities, resigning from the Council of the National Academy of Sciences in 1940, after many years of service, and from all other committee work. He confined his work life to research and publication. By 1945 he had brought to publication his whole series of investigation on *P. bursaria* and failed only to finish the book he was writing about them and their general significance, as he had summarized this in his Patten Lectures. The unfinished manuscript of the book based on these lectures is in the Library of the American Philosophical Society.

These last researches, carried on between his sixty-ninth and seventy-fifth years, were unsurpassed in lasting value by any other major investigation of his life, except for his first experimental researches on cell behavior, which had been carried on between his twenty-eighth and thirty-eighth years. He discovered the existence of systems composed of more than two interbreeding mating types, thus rendering unlikely the possibility that ciliate mating types were early stages in the evolution of male and female. He discovered that *P. bursaria* consists of reproductively isolated subdivisions (varieties, syngens, or biological species) at about the same time Sonneborn was finding this true of *P. aurelia.* In the quarter century since this discovery, all well-studied ciliates and many other unicellular organisms as well have been found to conform to the same principle —the species of the taxonomist is a group of biological species.

Concentrating most of his attention on one of these biological species of *P. bursaria,* Jennings attempted to work out the genetics of its four mating types. Most of his data fit the fairly simple hypothesis of determination by two pairs of unlinked genes, but he had some data that did not fit and therefore remained cautious. Many years after Jennings died, Siegel and collaborators verified the hypothesis and explained most of the exceptions. Others have carried on the genetic work on *P. bursaria* in other exciting directions, all solidly based on Jennings's pioneer work.

Finally, Jennings showed that clones of *P. bursaria* have a life cycle; most of them, at least, pass through periods of immaturity (inability to mate), adolescence, maturity (ability to mate), and senescence, ending in death. The whole cycle lasted several years in some clones under the cultural conditions employed by Jennings, and some did not die by the time he was forced by ill health to stop laboratory work. There was great variability in the length of life and vigor of different clones, some dying immediately after their origin. Two major causes of this variation were identified—one was the age of the parent clones; the other was the relationship between the parent clones. Inbreeding and old parents yielded few or no vigorous daughter clones; outbreeding and young parents yielded the highest proportions of vigorous long-lived daughter clones. The decisive factors were thus both genetic and physiological. Jennings concluded that natural death did not arise first, as some held, with the evolution of multicellular organisms; but he did not exclude the possibility that some clones might be immortal; some did not die during several years of culture. This study of aging and death was too heroic a task for even younger men to follow up in so long-lived a species; but other investigators, following the same general plan with more favorable species of *Paramecium,* succeeded in confirming the essential features of the life cycle and showed that in these species all clones eventually die, only sexual progeny surviving by initiating new life cycles.

The demonstration by Jennings of the existence of a clonal life cycle and of the role of conjugation in initiating new cycles ran counter to opinions he had previously maintained. Thirty years earlier, under the influence of the then new genetics and of his own overwhelming evidence for the production at conjugation of hereditary variations, including variations in vigor, he interpreted "rejuvenescence" after conjugation as merely one class of the genetic variations. He was also unwilling to exclude the possibility, to a certain degree supported by available evidence, that the apparent aging and death of clones was a reflection of the cumulative effect of unfavorable cultural conditions. Only after he had himself exhaustively investigated the matter during his last years of research activity did he change his views and bring all of the facts into a coherent interpretation. This was typical of the man. In his earliest behavior work he had declared that he didn't care how the experiments turned out, he wanted only to discover the facts and their meaning. And so it was until his last work. It mattered not what he had published thirty years before; all that mattered were the facts and their meaning.

Jennings was genuinely amazed throughout his life at his own success. He bore his eminence and honors modestly. Always high-strung, jerky, energetic, he threw himself completely into every task he undertook, whether it was to his liking or not. He was a man immensely capable both of concentration and of enjoyment. Although a keen observer of people, including himself and his ills, he took people as he found them. He shared his physician—father's skepticism about drugs and medical care; during most of his life he largely ministered to himself on the basis of observations and experiments on himself. He even used glasses bought at the five-and-ten-cent store, for he apparently needed only magnification and that only for fine print. He was decidedly an intellectual, quick but deliberate of mind. His class and public lectures never called attention to himself as a person. With no trace of theatricality, he depended on fully or largely written out, clear, logical, vivid formulations of what he wanted to say. On the whole, his incalculable influence on those who made contact with him was achieved mainly by the example he set of freedom from pettiness, recognition of and concentration on fundamentals, profound respect for both objective investigation and the search for meaning, and an exquisite just balance in dealing with fact and thought.

Only during his last year or two did his health fail to the point at which serious productive work became impossible for even his extraordinary drive. During the last two months he gradually sank while in the Santa Monica Hospital; he died April 14, 1947, a week after his seventy-ninth birthday.

# HONORS AND DISTINCTIONS

## Academic Career

| | |
|---|---|
| 1888-1889 | Assistant Professor of Botany, Texas State Agricultural and Mechanical College |
| 1892-1894 | Assistant in Zoology, University of Michigan |
| 1893 | B.S., University of Michigan |
| 1894-1895 | Assistant in Zoology, Harvard University |
| 1895 | M.A., Harvard University |
| 1895-1896 | Morgan Fellow, Harvard University |
| 1896 | Ph.D., Harvard University |
| 1896-1897 | Parker Travelling Fellow (Harvard University), Jena, Germany, and Naples, Italy |
| 1897-1898 | Professor of Botany and Bacteriology, Montana State Agricultural and Mechanical College |
| 1898-1899 | Instructor in Zoology, Dartmouth College |
| 1899-1901 | Instructor in Zoology, University of Michigan |
| 1901 | Acting Director, U.S. Fish Commission, Biological Survey of the Great Lakes |
| 1901-1903 | Assistant Professor of Zoology, University of Michigan |
| 1902 | Director, U.S. Fish Commission, Biological Survey of the Great Lakes |
| 1903-1904 | Research Assistant, Carnegie Institution of Washington, Naples Zoological Station |
| 1903-1906 | Assistant Professor of Zoology, University of Pennsylvania |
| 1906-1907 | Associate Professor of Physiological Zoology, Johns Hopkins University |
| 1907-1910 | Professor of Experimental Zoology, Johns Hopkins University |
| 1910-1938 | Henry Walters Professor and Director of the Zoological Laboratory, Johns Hopkins University |
| 1938-1947 | Emeritus Professor, Johns Hopkins University |
| 1939-1947 | Research Associate, University of California |

## Honorary Degrees

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|---|---|
| 1909 | Ll.D., Clark University |
| 1918 | Sc.D., University of Michigan |
| 1933 | Sc.D., University of Pennsylvania |

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| 1933 | Ll.D., Oberlin College |
| 1935 | A.M., Oxford University |
| 1940 | Ll.D., University of Pennsylvania |
| 1941 | Ll.D., University of Chicago |
| 1943 | Ll.D., University of California |

## Visiting Lectureships and Professorships

| | |
|---|---|
| 1925 | Stanford University |
| 1931 | Keio University, Tokyo, Japan |
| 1933 | Terry Lecturer, Yale University |
| 1934 | Vanuxem Lecturer, Princeton University |
| 1935-1936 | Eastman Visiting Professor, Oxford University |
| 1939 | University of California at Los Angeles |
| 1940 | Leidy Lecturer, University of Pennsylvania |
| 1943 | Patten Lecturer, Indiana University |

## Awards

| | |
|---|---|
| 1896 and 1908 | Walker Prize, Boston Society of Natural History |
| 1925 | Leidy Award, Philadelphia Academy of Natural Sciences |
| 1931 | One of 14 scientists to have name inscribed in Buhl Hall of Science, Pennsylvania State College for Women |

## Memberships

| | |
|---|---|
| 1918-1924 and 1928-1936 | Member of the Council of the American Philosophical Society |
| 1920-1925 | National Research Council, Division of Biology and Agriculture |
| 1928-1931 | Chairman of the Bache Fund, National Academy of Sciences |
| 1934-1940 | Member of the Council, National Academy of Sciences |

## Corresponding Memberships

Academy of Natural Sciences, Philadelphia
Russian Academy of Sciences
Société de Biologie de France

## Honorary Fellow

Royal Microscopical Society, London
Royal Society, Edinburgh

## Editorial Boards

*Biological Bulletin, Genetics, Human Biology, Journal of Experimental Zoology, Journal of Comparative Psychology*

## Professional Societies and Offices

American Society of Zoologists (President, 1909)

American Society of Naturalists (President, 1910)

Fellow of the American Association for the Advancement of Science (Vice-President, Section F, Zoology, 1925, and member of Committee of 100 on Scientific Research)

Genetics Society of America (First Chairman, 1922)

Society of Experimental Biology and Medicine

Eugenics Research Association

## Miscellaneous

| | |
|---|---|
| 1905-1938 | Trustee, Marine Biological Laboratory, Woods Hole, Massachusetts (Trustee emeritus, 1938-1947) |
| 1912-1933 | Academic Council, Johns Hopkins University (except for years away from Baltimore) |
| 1917-1918 | Statistician, Sugar Division of U.S. Food Administration |
| 1932-1940 | Educational Advisory Board of the Guggenheim Foundation |

## SOURCE MATERIALS

The Jennings collection in the Library of the American Philosophical Society contains all of the source material: diaries, autobiographical sketch, correspondence, commonplace books, and many other documents, including his father's autobiography.

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*KEY TO ABBREVIATIONS*

Am. Nat. = American Naturalist
Am. J. Physiol. = American Journal of Physiology
Am. J. Psychol. = American Journal of Psychology
Anat. Rec. = Anatomical Record
Biol. Bull. = Biological Bulletin
Biol. Symp. = Biological Symposia
Bull. Mich. Fish Comm. = Bulletin of the Michigan Fish Commission
J. Comp. Neurol. Psychol. = Journal of Comparative Neurology and Psychology
Johns Hopkins Univ. Circ. = Johns Hopkins University Circular
Johns Hopkins Alumni Mag. = Johns Hopkins Alumni Magazine
J. Exp. Zool. = Journal of Experimental Zoology
J. Philos. Psychol. Sci. Methods = Journal of Philosophy, Psychology and Scientific Methods
Proc. Am. Philos. Soc. = Proceedings of the American Philosophical Society
Proc. ——— Int. Congr. Eugen. = Proceedings of the ——— International Congress of Eugenics
Proc. ——— Int. Congr. Genet. = Proceedings of the ——— International Congress of Genetics
Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences of the United States of America
Psychol. Bull. = Psychological Bulletin
Sci. Mon. = Scientific Monthly
Surv. Graphic = Survey Graphic
U.S. Fish Comm. Bull. = United States Fish Commission Bulletin

1890 Some parasitic Fungi of Texas. Texas Agricultural Experiment Station. Bulletin no. 9.

1894 A list of the Rotatoria of the Great Lakes and of some of the inland lakes of Michigan. Bull. Mich. Fish Comm. no. 3, pp. 1-34.

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Alfred H. Joy

# Alfred Harrison Joy

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By O. C. Wilson

The Mount Wilson Observatory was founded in 1904 by George Ellery Hale who at that time succeeded in enlisting the support of the Carnegie Institution of Washington. The completion of the 60-inch telescope in 1908 marked the beginning of a lengthy period of pre-eminence in stellar spectroscopy, which was further enhanced when the 100-inch telescope began operation in 1918. This era lasted roughly until midcentury when the completion of other large, more modern, telescopes and technological advances provided serious competition.

During the period noted above a considerable part of the success of the Mount Wilson Observatory was, of course, due to its excellent equipment and its advantageous location. But at least an equal part must be credited to the able, enthusiastic, staff of stellar spectroscopists who were responsible for the proper use of its facilities. This group, of whom Alfred Joy was the last survivor, included Walter S. Adams, P. W. Merrill, R. F. Sanford, M. L. Humason, and G. Stromberg. In varying degrees these men contributed enormously to virtually all areas of stellar spectroscopy and, as a glance at the appended bibliography will testify, Joy was at the forefront of productivity.

Alfred H. Joy was born in Greenville, Illinois, and was educated locally, obtaining the degree of Ph.D. from Green ville College in 1903. He then moved to Oberlin and studied physics for a year, receiving an M.A. in 1904. His career then deviated considerably from that of most professional researchers since, after leaving Oberlin, he became a teacher in 1904 at the Syrian Protestant College in Beirut (now the American University of Beirut) where he remained, with the exception of one year, through 1914. The year of 1910-1911 he spent as a Thaw Fellow studying astronomy at Princeton under H. N. Russell. His interest in astronomy had been stimulated by working at the observatory at Beirut and by being a member of the Lick Observatory Eclipse Expedition to Aswan, Egypt, in 1905. Joy's enthusiasm for astronomy led him to be a volunteer summer assistant at Oxford and Cambridge in 1909, at Yerkes Observatory in 1910 and 1911, and at Potsdam Astrophysical Observatory in 1914. In this way he acquired a variety of practical experience, met many of the leading astronomers of the time, and filled in some of the gaps in his astronomical education.

In 1914 Joy returned to Yerkes Observatory to spend a year as an instructor and to take part in several research programs. He had planned to return to Beirut in 1915 but, since this was rendered difficult by World War I, he accepted an offer by George E. Hale to come to the Mount Wilson Observatory as an assistant in solar researches being carried on by Hale and Charles E. St. John, and he continued this work for three years. During this period, in 1916, because of his interest in stellar distances, he became associated with Walter S. Adams in a study of spectroscopic parallaxes of stars. This was a method that had recently been worked out by Adams and A. Kohlschütter to determine absolute magnitudes of stars by noting the relative intensities of certain absorption lines in their spectra. Thus it was not until the age of thirty-four that Joy finally began work in the field that was to occupy him for the remainder of his active life, and upon which his well-deserved reputation was to rest.

At this time stellar spectroscopy was still in its early stages. The Henry Draper Catalogue (published 1918-1924), which provided spectral classifications of well over 200,000 stars derived from objective prism spectrograms, was still in preparation. But the study of stars with slit spectrographs, which would provide radial velocities and detailed information of many kinds, had been hampered by the relatively small telescopes, slow spectrographs, and relatively slow photographic emulsions then available. In fact, very little information of this kind had been obtained for stars much below fifth magnitude in apparent brightness. The installation of the large Mount Wilson reflectors, together with improved spectrographs and photographic plates, now began to open up for exploration a vast region of fainter objects containing a great variety of fascinating stars of widely divergent properties. Joy and the other members of the spectroscopic group at Mount Wilson lost no time in taking advantage of these opportunities.

The spectroscopic absolute magnitude program mentioned above, and the routine collection of stellar radial velocities (these could both be found from the same spectrograms), were group undertakings involving several staff members, including Joy, and extended over many years. But numerous other investigations were carried on simultaneously, especially spectroscopic studies of many types of variable stars that had hitherto been mostly inaccessible. Several of the staff also participated in these investigations, though not in the same way as for the large "observatory" programs. To a considerable degree, Joy, Merrill, and Sanford divided up the general variable star field, although there was some overlap, and a number of joint papers were published. Merrill did most of the work on the long period M-type variables, while Sanford tended to specialize on those of spectral types R and N. This left an extensive list for Joy: Cepheids, novae, irregular variables, flare stars, the brighter variables in globular clusters, and those variables named for their prototypes, U Geminorum, T Tauri, RR Lyrae, W Vir ginis, and RV Tauri. I do not know how this division of labor among the variable stars came about, but in practice it seemed to work very satisfactorily. In fact, the Mount Wilson spectroscopic group was outstanding, in my opinion, for a feeling of mutual respect, good fellowship, and cooperation and for a total lack of the jealousies, frictions, and internal bickerings that blot the records of so many research organizations. To this smooth and pleasant operation Joy's own personality automatically made him a major contributor.

Joy's extensive bibliography has already been mentioned. A great many of the items in it are brief notes recording an interesting fact concerning the spectrum of a single star or of a group of stars. Others give more or less extensive lists of stellar radial velocities or spectroscopic parallaxes. Still others relate to the derivation of the orbits of spectroscopic binaries or to lists of spectroscopic binaries newly discovered at Mount Wilson. Much of this work originated in the large observatory observing programs previously mentioned. But while this work was in progress Joy was also busy collecting information on a wide variety of variable stars. A great many of these objects were quite faint and the collection of the necessary information consumed a number of years of often difficult and trying observation. As a consequence, most of Joy's major papers were not published until the decade preceding his retirement and during a period of several years thereafter. In the following paragraphs I shall try to give the highlights of Joy's major contributions to astronomy; for convenience, they are arranged in order of the absolute magnitudes of the various objects, beginning with the intrinsically brightest.

Joy published several notes and papers on the spectra of novae. Perhaps the most interesting of these was the recurrent nova RS Ophiuchi, which had outbursts in 1933 and 1958. This object is noteworthy for the appearance in its spectrum of forbidden lines of very highly ionized atoms such as [Fe XIV], [Ca XV], [Ca XIII], and [A XIV], of which Joy measured and identified a considerable number.

Over many years Joy collected spectrographic observations of nearly 160 Cepheid variables. These stars are intrinsically bright, have small individual motions, and are, therefore, well suited to studies of galactic rotation. Joy used his radial velocities to derive the parameters of galactic rotation, getting good agreement at the time (1938) with other current determinations. Since then new and improved values of the parameters have been derived, although Joy's radial velocities remain of great value. They were used, unchanged, in the extensive work of R. P. Kraft and M. Schmidt (1963) in which improved knowledge of the absolute magnitudes of Cepheids and better photometry enabled these authors to make a step forward.

Another group of intrinsically bright stars whose spectra show superficial similarities to those of the Cepheids are the semiregular variables of RV Tauri type, and similar objects that do not fit accurately the RV Tauri criteria. Joy made a spectroscopic study of thirty-eight of these stars. He found that they could be separated into two groups of low and high velocity, a division supported by certain spectroscopic features. Although these stars appear to have luminosities similar to those of the Cepheids, there are decided kinematic and spectroscopic differences. Even today, the proper relationship of these stars to other variable or nonvariable objects does not appear to be certain.

In connection with his studies of the intrinsically bright variable stars, Joy obtained spectrograms of W Vir as early as 1925 that showed that this type of Cepheid differed in spectral behavior from the standard ones as well as having different spatial distribution in the galaxy. He found that W Vir showed hydrogen emission on the rise to maximum and that its radial velocity was larger than is usual for Cepheids. This was an anticipation of the general division of stars into populations I and II proposed much later by W. Baade. Pursuing this matter further, Joy obtained spectrograms of a number of variables in globular clusters. He found that these objects are virtually all W Vir-type Cepheids, or RV Tauri and semiregular variables, and that classical Cepheids and Mira stars are essentially absent from the clusters. Of particular interest was his study of Barnard's variable in M3, which proved to be similar to W Vir. Joy also made studies of a considerable number of RR Lyrae variables, determining their radial velocities and showing them to be a high velocity group, also a member of Population II.

As mentioned previously, P. W. Merrill was the specialist in long period M-type variables. Joy did some important work in this field, however, especially on Mira itself, first in a study of the peculiar close, early type, companion of Mira, and, later, an extensive investigation of the spectrum of the variable, some of it done at the highest available dispersion of 2.3 Å/mm. This work revealed many detailed differences between the lines of various elements during a cycle and even some differences between lines of the same multiplets. One of the most significant results was that Joy was able to identify most of the absorption features that appear in the strong hydrogen emission lines as due to metallic or molecular lines, thus demonstrating that the region where the hydrogen emission is produced lies below that responsible for the normal absorption line spectrum.

To this point we have dealt with Joy's work on intrinsically bright variables. But there were many others, known or surmised to be intrinsically faint, which Joy studied with his customary intelligence and thoroughness, and for which he uncovered much hitherto unknown information.

One such group is that named for its prototype T Tauri. Joy made a spectroscopic study of a number of these objects and found numerous strong emission lines of hydrogen and metals in their spectra. The hydrogen lines are several angstroms in width, vary in an irregular manner, as is also true of the overall brightness of the stars, and are displaced toward the violet, indicating ejection of matter. The absorption lines are characteristic of a spectral type near G5 and yield absolute magnitudes close to those of corresponding normal main sequence stars. Joy showed also that these variables are associated with the dark absorbing clouds of the Milky Way and that some are involved in faint reflection nebulosity. More recent work by G. H. Herbig and others indicates that the T Tauri stars are probably very young and in the process of settling into a stable state on the main sequence.

Joy worked also on a number of spectroscopic binaries, some of which have proved to be of outstanding interest. One of these was RW Tauri, whose components are of spectral types B9 and Ko, the latter probably a subgiant. As the eclipse of the B9 star progressed, Joy found that the hydrogen lines showed first a widely red displaced emission component, then no emission at all near the center of eclipse, and lastly a widely violet displaced emission component. These observations indicate that the B9 star is surrounded by a rapidly rotating ring of matter in which the emission lines are produced. The eclipse of this ring by the K-type star explains the spectroscopic phenomena.

Two other important binaries investigated by Joy are the U Geminorum stars SS Cygni and AE Aquarii. His observations showed that the late type components are main sequence stars while their companions are peculiar hot B-type subdwarfs. The periods are short: 0.27 d for SS Cygni and 0.70 d for AE Aquarii. These systems are noteworthy for occasional outbursts of light that originate in the hot companions. Later investigation by R. P. Kraft has shown that all U Geminorum stars are short period binaries of this kind.

Over many years Joy maintained a great interest in the dwarf M-type stars that populate the faint end of the main sequence. He produced a number of lists of such stars with spectral classifications and estimates of H and K emission strengths. He was particularly concerned with the stars in this region that show sudden increases in brightness from time to time, the so-called flare stars. He was fortunate to obtain spectrograms of one of them, UV Ceti, during flares, and found that the hydrogen emission lines widen and strengthen, and that a strong continuum appears, which extends into the violet and tends to veil the absorption spectrum. It is believed that these flares are analogous to the well-known solar flares, and in recent years radio emission has been observed from some of these objects during flaring.

Alfred Joy was secretary of the Mount Wilson Observatory from 1920 until his retirement in 1948 and thus had a number of administrative chores in addition to his research. He served as president of the Astronomical Society of the Pacific several times and as president of the American Astronomical Society in 1950-1952. For many years he edited the Astronomical Society of the Pacific leaflets, which provided both professionals and amateurs with short authoritative expositions of current astronomical research. In 1944 he was elected to the National Academy of Sciences and in 1945 was awarded an honorary Sc.D. by Greenville College. He was the recipient in 1950 of the Bruce Medal of the Astronomical Society of the Pacific in recognition of his outstanding achievements.

Joy was married in 1919 to Margherita O. Burns and is survived by her, by two children, Richard and Edith, and by several grandchildren.

Alfred Joy was always a kind, considerate, and helpful colleague. He was fortunate, both as to time and place, in having a great opportunity in his chosen field, and his skill and intelligence enabled him to make excellent use of it. I think it is no exaggeration to say that all who knew him regarded both the man and his work with the greatest admiration and respect.

# Bibliography

*KEY TO ABBREVIATIONS*

Astron. J. = Astronomical Journal

Astron. Soc. Pac. Leafl. = Astronomical Society of the Pacific Leaflet

Astrophys. J. = Astrophysical Journal

Pop. Astron. = Popular Astronomy

Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences

Publ. Am. Astron. Soc. = Publications of the American Astronomical Society

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Edward C. Kendall.

# Edward C. Kendall

March 8, 1886-May 4, 1972

By Dwight J. Ingle

Edward Calvin Kendall isolated thyroxine from the thyroid gland; he and associates crystallized glutathione and established its chemical structure; and he and associates isolated a series of steroid compounds from the adrenal cortex and contributed importantly to the determination of the structure and synthesis of several of them. With Philip S. Hench, he conceived the idea that cortisone might be useful in treating rheumatoid arthritis, and they planned clinical studies that confirmed the hypothesis. Kendall also initiated and participated in a number of related studies.

During his professional life he was called "Nick" by close friends and by his wife. He was referred to as "The Chief" by some of his laboratory associates, but commonly he was addressed with deference, as "Doctor Kendall."

Edward C. Kendall, the third child of George S. and Eva F. Kendall, was born March 8, 1886, at South Norwalk, Connecticut. The home was a citadel for religious teachings. The father, a dentist by profession, took an active interest in community affairs. Edward attended the Franklin Elementary School and, for two years, South Norwalk High School. He spent a year at Stamford High School preparing for college. During these years he excelled in mathematics and became interested in the work of a foundry and a machine shop. In his teens, he set up a shop in the attic of his home; there he built electrical apparatus and did machine work.

At Stamford High School Kendall developed an interest in chemistry. It was enhanced by his brother-in-law's stories of an amateur chemist who developed a secret process for making high-quality writing paper. This much admired brother-in-law graduated from Columbia University in 1900, and this influenced Edward to enter there four years later.

Edward concentrated his attention on chemistry and, as a college senior, he wrote a thesis under the guidance of Professor H. C. Sherman. During the summer of 1908, he served as a laboratory instructor in the department of biochemistry. He was awarded a scholarship for post-graduate work in biochemistry and received an M.S. degree in June 1909.

He then became the first recipient of the Goldschmidt Fellowship and began research on amylase, an enzyme of the pancreas. Kendall observed that the amount of reducing sugar produced by given amounts of amylase varied considerably, and he identified sodium chloride as the factor causing the variability; the presence of the salt enhanced the activity of amylase severalfold. His first paper reported this research in the *Journal of the American Chemical Society;* Professor Sherman was co-author. He received the Ph.D. from Columbia in June 1910. (Hereafter, I shall refer to my subject as Dr. Kendall, for I addressed him thus for forty years.)

In his memoirs, Dr. Kendall tells of departing from the sheltered, restricted life of his boyhood; but he cites specifically only that he played cards on Sundays and that he once tested the consequences of saying "God damn" out loud. As an adult, he was not overly religious, but he was more puritanical than many who practice religion loudly. These early years must have been important in the development of his quiet, scholarly demeanor and self-discipline. He continued to keep physically fit throughout his adult life. He had participated in high school sports and, in college, he was a bow oar in a four-man shell.

On September 1, 1910, Dr. Kendall began working in the chemical laboratory of Parke Davis and Company; his assignment was to isolate the hormone of the thyroid gland. He stayed five months. He found that punching a time clock was annoying, and he was disappointed by the intellectual isolation. There were no seminars, and he found himself working in competition with another chemist who was assigned the same problem.

After returning to New York City, he accepted an invitation to occupy and equip a new laboratory in St. Luke's Hospital. In the beginning, he worked without salary but was given funds for supplies and equipment. Eventually a salary of $1200 a year was provided, but it was never increased.

Dr. Kendall continued research on the thyroid gland. Near the end of the nineteenth century, Professor Eugen Baumann, a physiological chemist at the University of Freiburg, had prepared iodine-containing extracts of thyroid glands that were useful in treating clinical hypothyroidism. Baumann's partially purified principle was named iodothyrin. The findings of Baumann served as a starting point for Dr. Kendall. By 1913 he had purified the active principle about a hundredfold. The method of bioassay was to measure changes in the urinary nitrogen of dogs. The biologic activity of the partially purified preparations was also demonstrated in hypothyroid patients. The research was not appreciated by the clinical staff of the hospital, whose attitude toward the partial purification of the iodine-containing compound seems to have been "So what?"

At about this time the hospital administrator sent Dr. Kendall a box of cereal with a letter directing him to analyze the contents. The letter and the cereal were thrown summarily into the wastebasket. Not then or ever would the young chemist take orders of this sort or accept distraction from his own goals. This and similar incidents formed the basis of his determination to move to a research-oriented institution.

It was Professor Clarence M. Jackson, soon to become a great teacher of anatomy at the University of Minnesota, who told Dr. Kendall of developments at the Mayo Clinic and suggested that he apply to Dr. Louis B. Wilson, Director of Laboratories, for a position. Dr. Henry S. Plummer, a many-sided genius, was involved in the treatment of diseases of the thyroid and in studies of its pathologic physiology. Drs. Will and Charlie Mayo were interested in diseases of the thyroid. Dr. Kendall was invited to join the staff of the Mayo Clinic and he began his research there on February 1, 1914. He was concerned with two projects: first, the isolation of the hormone of the thyroid gland, and second, the determination of the amount of the acid-insoluble fraction of thyroid glands removed surgically from patients so these data could be correlated with other clinical and laboratory findings.

Baumann had prepared iodothyrin by boiling thyroid tissue with 10 percent sulfuric acid to hydrolyze the proteins; Dr. Kendall came to use repeated treatment with hot dilute sodium and barium hydroxides followed by separation of the acid-insoluble material. Near the end of 1914, an acid-insoluble fraction that contained 47 percent iodine had been prepared. At this point, ethanol was used as a solvent. On December 23, a sample was dissolved in a small amount of ethanol and evaporation started. The young chemist was tired and fell asleep. When he awakened, the ethanol had evaporated, leaving on the bottom of the beaker a white crust surrounded by a ring of yellow waxy material. When more ethanol was added, the latter material dissolved but the white crust did not. When the residue was analyzed the following morning, it was found to contain 60 percent iodine. During the day, more of the crust was prepared. On Christmas morn ing, some of the white crust was dissolved in ethanol that contained a small amount of sodium hydroxide. The addition of a few drops of acetic acid precipitated crystals. This pure compound was later named "thyroxin" and, still later, when it was found to be an amino acid with an amine group, an "e" was added to make the name "thyroxine" (The ending "ine" indicates the chemical class to which the compound belongs.) Some hypothyroid patients were treated with the crystalline hormone; it was fully active in relieving the symptoms of thyroid deficiency.

A year later, Edward C. Kendall married Rebecca Kennedy of Buffalo, New York. To Dr. Kendall and "Becky," four children were born—Hugh, Roy, Norman, and Elizabeth.

Before coming to the Mayo Clinic, Dr. Kendall had applied for a position at the Rockefeller Institute and was bluntly turned down by its director, Dr. Simon Flexner. This rankled the younger man and, in 1916, he took special satisfaction in reading a paper, "Isolation in Crystalline Form of the Iodine-Containing Compound of the Thyroid Gland," at a session of the Federation of American Societies for Experimental Biology, chaired by Dr. Flexner.

Efforts to identify the structure of thyroxine and to synthesize the compound extended over the next ten years; they resulted in failure. Dr. Kendall described thyroxine incorrectly as triiodo-hexahydro-oxindolepropionic acid. In 1926, Dr. C. R. Harington of University College, London, identified the nucleus of thyroxine as the tetra-iodo derivative of thyronine and he synthesized thyroxine. At that point, the Mayo Clinic closed its research on the chemistry of the thyroid hormone.

Dr. Kendall was already an important scientist and was to accomplish goals more significant than the isolation of thyroxine; but he was not then, nor was he to become, a great chemist. His formal training in chemistry had been brief, and from the time he had received his Ph.D., he no longer worked with a master. A stubborn man, throughout his life he held that his intuitive beliefs were valid until the evidence against them became overwhelming. Other chemists had advised him over and over that his proposed structural formula for thyroxine was incorrect. Usually, when confronted with proof that a belief was incorrect, he would accept it with good grace; but undue faith in his own ideas and resistance to the suggestions of others characterized his whole life as a scientist. Yet, in another sense, these foibles may have been necessary for his noble aims, his tenacity, and, hence, his great achievements. As Albert Szent-Györgyi said, "Discovery consists of seeing what everybody has seen, and thinking what nobody has thought."

The research interests of Dr. Kendall shifted to studying the specific compounds involved in the effect of thyroxine on oxidation in the body. Attention was focused on cysteine and glutathione. Since the latter compound could not be purchased, the Mayo group became involved in a program to crystallize it and prepare it by synthesis. The compound was first isolated, analyzed, and named by Professor F. Gowland Hopkins of Cambridge University in 1921. Bernard F. McKenzie and Dr. Harold L. Mason were collaborators of Dr. Kendall in isolating glutathione in crystalline form and in identifying it as a tripeptide of glutamic acid, cysteine, and glycine. This was accomplished independently of the isolation of crystals of glutathione and determination of structure by Professor Hopkins. The two groups agreed that the compound is glutamyl-cysteinyl-glycine. It was first synthesized by Dr. C. R. Harington.

The Section of Biochemistry at the Mayo Clinic was involved in basic research, graduate education, and performing clinical biochemistry. The last-named function was directed by Dr. Arnold E. Osterberg, first research associate of Dr. Kendall at the Mayo Clinic. It was Osterberg who suggested the name "thyroxin." Although Dr. Kendall participated in graduate education to a small extent—he held the rank of professor since 1921—he would permit few distractions to his research. He was never to become a sitting scientist; he was almost always at the bench.

In the fall of 1929, Albert Szent-Györgyi, who, throughout his life made discoveries and stimulated the research of others, became a visiting scientist in Biochemistry at the Mayo Clinic. Szent-Györgyi had isolated small amounts of a substance that he first named "hexuronic acid." It is widely distributed in plants and animals and relatively large amounts are in the adrenal cortex. Fresh beef adrenal glands were made available to Dr. Szent-Györgyi, and he isolated substantial amounts of the compound during the eight months he spent in Dr. Kendall's laboratories. Hexuronic acid was later identified as vitamin C and given the name "ascorbic acid."

The initiation of research on adrenal glands in Dr. Kendall's laboratories coincided with the publication of convincing evidence that an extract of beef adrenal glands would sustain life in adrenalectomized animals and would reverse the symptoms of Addison's disease in human patients. Several investigators claimed to have achieved this during the 1920s, but the first to publish statistically reliable evidence (1927) for the prolongation of life in adrenalectomized animals was Professor Frank A. Hartman at the University of Buffalo. In 1930, Hartman and Katherine A. Brownell at Buffalo and J. J. Pfiffner and W. W. Swingle at Princeton University prepared extracts of the adrenal cortex that would sustain adrenalectomized animals indefinitely, would revive them from a state of adrenal crisis, and would relieve the symptoms of patients with Addison's disease. The efficacy of the Pfiffner-Swingle extract was demonstrated on patients with Addison's disease by Dr. Leonard G. Rowntree of the Mayo Clinic. Dr. Rowntree came to Dr. Kendall with a plea to prepare adrenal cortical extract. The challenge was accepted, but Dr. Kendall looked beyond the immediate clinical need toward the isolation and chemical identification of the hormone of the adrenal cortex.

During the early 1930s, Dr. Giles A. Koelsche, a Fellow in Biochemistry, carried out an important study of the effects of thyroxine and of adrenal cortical hormones on nitrogen balance in dogs. It is generally believed that the hormones of each gland are catabolic. This is true when they are given in excess, but Koelsche demonstrated experimental conditions in which physiological doses of adrenal cortical hormones favor a positive nitrogen balance and anabolism. Dr. Joseph L. Svirbely came to the Mayo Clinic for one year, did sophisticated biological studies, then returned for several summers. Svirbely had contributed importantly to the identification of hexuronic acid as vitamin C during his association with Dr. Szent-Györgyi at Szeged, Hungary.

Dr. Frank C. Mann supported the research of Dr. Kendall in two important respects. First, he was director of the Institute of Experimental Medicine of the Mayo Clinic, which carried out all animal experimentation. Dr. Mann performed all of the adrenalectomies on dogs used by the Kendall group in bioassay procedures and research. Second, Dr. Mann was a member of Mayo's Board of Governors for a number of years. He was one of the effective spokesmen for the Clinic's laboratory investigations. He was a great experimental surgeon and physiologist, and a pathologist of broad interests, who had important insights into the complexities of life and disease. Dr. Kendall and Dr. Mann were, in unique ways, strong personalities. Each had a warm personal regard for the other, but Dr. Mann was well aware of Dr. Kendall's foibles as a scientist. Dr. Kendall's knowledge of physiology was shallow; he did not appreciate the complexity of cause-and-effect relationships, and he did not fully appreciate the extent of biological variability. He was also given to making premature announcements of laboratory results. All of this exasperated Dr. Mann. On one occasion, a brash young biochemist who came to the Institute to do research and bioassays on adrenalectomized dogs was questioned by Dr. Mann about his knowledge of physiology. The new Fellow replied that, since he was trained in biochemistry, he had good basic knowledge of physiological processes. Dr. Mann replied, "I know more than two hundred biochemists and not a damn one of them knows any physiology." When the remark was repeated to Dr. Kendall, he said, "I know more than two hundred physiologists and not one of them knows any biochemistry."

Some members of the Clinic staff and some members of the Board of Governors questioned the wisdom of supporting basic research in the Section of Biochemistry. So long as Drs. Will and Charlie Mayo ran the clinic, they supported Dr. Kendall's programs. Dr. Charlie especially would come frequently to Dr. Kendall's laboratory bench to chat and keep in touch with progress. When the Mayo brothers began to turn over more and more administrative responsibilities to committees, the research program of Dr. Kendall was in some danger. He had to appear before the Board each year and resell the program. These were depression days and the Mayo Clinic did not accept any outside support for any of its functions. Dr. Kendall could plead a cause with quiet optimism, always promising early progress. But there were years in which there was little progress to report. Dr. Mann was in a position to scuttle Dr. Kendall's program but did not. He would express misgivings, then support the continuation of the research. There was no true inconsistency in this, for Dr. Mann understood better than most physicians the necessity for basic research, that it is errant, and that years of effort may go by without discovery.

In 1928, when I was an undergraduate student, a physician friend gave me a publication containing "before and after" pictures of a ten-year-old girl in whom treatment with thyroxine at the Mayo Clinic had corrected cretinism within a few months. There was a remarkable spurt in growth. I wrote a letter to Dr. Kendall. I received a reply to each of my questions about this patient. In 1932, I heard him speak on thyroxine at the University of Minnesota School of Medicine. After the lecture I wrote to him and was again treated with kind consideration. I was told that he now aimed to isolate the hormone of the adrenal cortex. I was studying the work performance of adrenalectomized rats: The general technique was to anesthetize the rat with sodium phenobarbital, weight the gastrocnemius muscle with 100 grams, and stimulate it electrically to lift the weight three times per second. Normal rats could continue work of the stimulated muscle for more than fourteen days. When the adrenal glands were removed, the amount of work accomplished began to fall below that of sham-operated animals within two hours, and muscular responsiveness was lost within a day. I had shown that ability to work was lost because of circulatory failure and that the state of shock was due to absence of the adrenal cortex rather than of the adrenal medulla.

When I asked Dr. Kendall for a sample of adrenal cortical extract, he suggested that I first test lactyl epinephrine. He had an intuitive guess that since the adrenal glands contain both lactic acid and epinephrine, the two compounds might be linked together to form the hormone of the adrenal cortex. He believed that he had extracted this compound from adrenal glands, but the evidence was tenuous. A final ether extract contained lactic acid and gave a positive test for the catechol grouping. He believed that synthetic lactyl epinephrine was prepared in his laboratory, again basing this conclusion on the finding that an ether-soluble product contained lactic acid and gave a positive test for the catechol grouping. There was no proof that lactic acid and epinephrine were chemically bonded. Dr. Kendall had injected this latter product into dying adrenalectomized dogs and had observed temporary improvement in some of them; I found the product to have an epinephrine-like effect on my "fatigued" adrenalectomized rats and to cause a small temporary recovery in muscle work. The benefit to both the dogs and rats was due to the presence of free catecholamine and represented only a pharmacologic effect.

When I treated "fatigued" adrenalectomized rats with adrenal cortical extract supplied by Dr. Kendall, there was an almost complete recovery of work output. It was simpler to treat the animals with extract from the time of adrenalectomy and the beginning of muscle stimulation. I developed a twenty-four-hour assay test of adrenal cortical extract that was sensitive, fast, and reliable. Bioassays done on adrenalectomized cats and dogs required several days and large amounts of extract, and were not very sensitive. I was invited by Dr. Kendall to join his group as a Mayo Foundation Fellow, and I did so in September of 1934.

In December, 1933, a crystalline organic substance was isolated from adrenal cortical extract. Dr. Kendall announced the isolation in crystalline form of the hormone of the adrenal cortex. It was then assumed that the adrenal cortex secretes but one hormone and that any crystals formed during fractionation of the extract were likely to represent that hormone. The crystalline material was tested in adrenalectomized dogs, and it was concluded that it sustained the lives of these animals. After I established the muscle-work test at the Mayo Institute, my first assignment was to test this crystalline material. It did not have demonstrable activity. It was then retested in adrenalectomized dogs with negative results. During the first series of tests on adrenalectomized dogs, a high dietary intake of sodium chloride was used; this prevented symptoms of adrenal cortical insufficiency. It is possible that the crystalline material contained some biologically active compounds, but the data did not prove the presence of a hormone, nor were the crystals shown to be a single compound. Dr. Kendall never fully explained these facts in any subsequent publication, so his claim to have isolated the hormone of the adrenal cortex in 1933 has been regarded by some reviewers as a report of the first isolation of cortisone.

At about the same time, Dr. Arthur Grollman at Johns Hopkins Medical School reported the isolation of crystalline material from adrenal cortical extracts and that the crystals sustained the life of adrenalectomized rats. These claims were not independently confirmed and the chemical nature of his crystalline material was not fully determined.

Dr. Mann was deeply concerned, first, when Dr. Kendall reported verbally on the apparent activity of lactyl epinephrine —he did not publish a claim for activity—and, again, when his published claim (1934) to have isolated the hormone of the adrenal cortex was not confirmed. At this time, Dr. Kendall was asked by the Board of Governors to give Dr. Harold L. Mason increased responsibility for the purification of crystalline material and chemical characterization. Mason was well trained in organic chemistry and had learned the then new methods of microanalysis. He was a perfectionist, who did elegant work at the bench. Dr. Kendall was not a master of the principles of research nor of all the methods needed in this program; his intuitive judgments as to what should be done were a source of frustration to his young colleagues. He did not follow the advice of Dr. Mann to give Mason greater responsibility. About this time, Mason and Charlie Myers went to Dr. Kendall to air their complaints. At their suggestion, a weekly conference was set up and this proved an important means of advancing the project.

Happily, the muscle-work test was of use in following biological activity during fractionation and purification. By the fall of 1935, Dr. Hugo W. Nilson and Mr. Donald Krockow joined the group to conduct bioassays and research on adrenalectomized dogs. Each did his work superbly.

The Kendall group was at this time processing large quantities of beef adrenal glands supplied by Parke Davis and Com pany. Dr. Kendall was at his best as an extractionist. In exchange for the glands, epinephrine was separated, purified, and returned to Parke Davis. The Wilson Laboratories also supplied beef adrenal glands in exchange for bioassays on the adrenal cortical extract prepared for clinical use by this company. For at least five years, the laboratory processed 900 pounds of beef adrenal glands every week. Bernard McKenzie had an important part in the success of this program.

Dr. Kendall supplied adrenal cortical extract to Mayo Clinic physicians to treat patients with Addison's disease. It was now generally available from commercial sources, but large amounts were required to restore a patient with Addison's disease to normal vigor; no patient could afford to purchase all of the cortical extract needed. Dr. Robert F. Loeb of Columbia University demonstrated that animals and patients with insufficient amounts of adrenal cortical hormones will survive for a long time under nonstress conditions if given a high sodium chloride diet or saline to drink. Dr. George Harrop of Johns Hopkins Hospital called attention to the elevated blood serum potassium of animals and patients during adrenal cortical insufficiency. Under the direction of Dr. Kendall, William D. Allers, a Mayo Foundation Fellow, prepared high sodium—low potassium diets and demonstrated that adrenalectomized dogs could survive nonstress conditions for indefinite periods and could even reproduce—but did not lactate—when fed these diets. These findings were promptly applied in the treatment of patients with Addison's disease at the Mayo Clinic; some patients were stabilized for months on the high sodium—low potassium diet. Adrenal cortical extract was used only if the patient got into trouble when subjected to some form of stress or if he failed to continue the special diet. Dr. Hugo Nilson continued and advanced the studies on adrenalectomized dogs.

All phases of the program were significantly advanced during 1935-1936. The research led to the isolation of five crystalline compounds. It was an uneasy time, for two rival groups were in the field. Dr. J. J. Pfiffner had moved from the Princeton University laboratories of Professor W. W. Swingle to collaborate with Dr. Oskar Wintersteiner at Columbia University. Wintersteiner was a superb organic chemist, who had mastered the methods of microanalysis: The Wintersteiner group began to isolate crystalline compounds at about the same time as the Kendall group. In Switzerland, Professor Tadeus Reichstein and his students were progressing rapidly toward the same objective.

Compound E of the Kendall group was found to be active in the muscle-work test in late 1935. Wintersteiner and Pfiffner were the first to report the isolation of a few milligrams of this compound, but they did not recognize it as being biologically active. The same substance, later to be called cortisone, was isolated by the Reichstein group; they too failed to recognize it as biologically active. The Kendall group converted their Compound E (cortisone) into a diketone, which had androgenic activity, as demonstrated by Professor F. C. Koch of the University of Chicago. It was correctly deduced from this finding that Compound E is a steroid. Independently, the competing groups recognized that the compounds isolated from the adrenal cortex are steroids.

Prior to the chemical identification of compounds, each of the three competing groups referred to a compound by letter, as A, B, C, or the like. Each group happened to assign a different letter to its compound. This led to some confusion among those who read the original research reports and among interested persons who had only secondhand information about the researches.

Noncrystalline preparations from the adrenal cortex were far more potent than was Kendall's Compound E in sustaining the life of adrenalectomized animals. This was first shown by Wintersteiner and Pfiffner. The search for the hormone of the adrenal cortex continued. In 1936 we heard a rumor that

Reichstein had isolated the life maintenance hormone and had named it corticosterone. There was some dismay in the Mayo group; but, when Reichstein published (1937) the chemical characteristics of the new compound, it was recognized as being identical with Kendall's Compound B, which had been isolated earlier. It, too, was much less potent than noncrystalline fractions in sustaining life in adrenalectomized animals.

As the structure of these compounds was worked out, it became possible to name them. In the Kendall series, Compound B was corticosterone, Compound A was 11-dehydrocorticosterone, Compound E was 17-hydroxy-11-dehydrocorticosterone (cortisone), and the soon-to-be-isolated Compound F was 17-hydroxy-corticosterone (cortisol or hydrocortisone). Each of these compounds was active in the muscle-work test, cortisol being the most potent. By 1938 Professor C. N. H. Long and his students at Yale Medical School had shown that each of these compounds affects carbohydrate metabolism and can be bioassayed by measuring the level of liver glycogen in fasting young adrenalectomized rats or mice.

In 1937 M. Steiger and T. Reichstein prepared 11-desoxycorticosterone by partial synthesis. Dr. George W. Thorn of Johns Hopkins Hospital found this steroid to be highly potent in sustaining the life of adrenalectomized animals and of patients with Addison's disease. Only minute amounts were found in adrenal cortical extracts. It seemed unlikely that this steroid was the naturally occurring life-maintaining hormone of the adrenal cortex, but the urgency to isolate an adrenal cortical hormone of clinical importance seemed to be over.

I interrupt the account of research to describe more of Dr. Kendall's personal life. He was a quiet man but did not conceal his enthusiasms. His treatment of others was usually kind, but he would sometimes become sharply impatient and sarcastic. I once caused him to lose his temper. I refused to do an experi ment according to his research plan and proceeded to tell him of his weaknesses in biologic research. This was well intentioned, but unwise, and I soon regretted it. He did not raise his voice but his face flushed, then paled, and his lips and hands trembled. I lost the argument and did the experiment as he directed, but I added controls. When my data showed the controls to be necessary, he accepted the findings in good humor. I learned to do my own experiments first and to bring the data to Dr. Kendall after the studies were completed. To be more precise, I reported results only if they seemed significant. Dr. Kendall neither tested ideas in private nor concealed his mistakes, as many of us do. He would announce what he expected to find "just around the corner."

During the holidays Dr. and Mrs. Kendall hosted members of the laboratory group. Mrs. Kendall was an unassuming, gracious, and generous lady. Summers, the whole family lived at their cottage on Lake Zumbro where each child became a fine swimmer. On weekends, a nap after lunch was routine. Dr. Kendall would plan research strategy then and some nights. One of his few diversions was playing chess, and he kept a few games going by mail. Each Fourth of July, the members of his laboratory group would gather at the Kendall cottage for a picnic. For years, Dr. Kendall offered a prize to any guest who could cross Lake Zumbro in a tub. There were some who made the attempt, but I am not aware that anyone won the prize. Several summers, Dr. Kendall took his three sons on long canoe trips.

Dr. Kendall was a man of scholarly demeanor, yet he was not a scholar. He excluded from his life many activities that others enjoy. He focused attention on long-range research objectives and relaxed his attention and effort only to the extent needed to maintain physical and mental fitness.

I have mentioned that Dr. Arnold E. Osterberg directed the laboratories of clinical biochemistry. Dr. Marschelle H. Power was an important member of this group and was to become head of the section when Dr. Kendall retired.

In 1936 Dr. Willard Hoehn, a well-trained organic chemist, came to Mayo's to replace Dr. Charles S. Myers. Warren F. McGuckin and Bernard F. McKenzie were productive members of the team doing research on steroids. Miss Eva Hartzler joined the group to work with Dr. Hugo Nilson in studies of changes in electrolyte metabolism caused by adrenal cortical insufficiency in dogs.

Before the end of the decade, Drs. Pfiffner and Wintersteiner dropped research on the adrenal hormones. Both Hugo Nilson and I left the Kendall group. Harold Mason and Willard Hoehn were soon to drop out of the program. Dr. Frank Stodola came to work with Dr. Kendall for a time. Before shifting his interests to other problems, Mason prepared a noncrystalline residue of adrenal cortical extract that was far more potent than 11-desoxycorticosterone in sustaining the life of adrenalectomized animals. There was something important in the adrenal cortex that remained to be isolated in pure form and chemically identified.

In 1934 Dr. Kendall had suggested that the adrenal cortex may secrete more than one hormone; other investigators advanced the same hypothesis, but I believe that Dr. Kendall was the first. He then dropped the idea for several years. By 1939 Dr. George W. Thorn and I, working with steroids supplied by the Mayo group, demonstrated a qualitative dissociation of the biological properties of 11-desoxycorticosterone and 17-hydroxy-11-dehydrocorticosterone (cortisone). Our findings were supported by the studies of Professor C. N. H. Long and his associates at Yale, with whom we regularly exchanged data. Our general conclusions were confirmed independently by Dr. Benjamin B. Wells and Dr. Kendall. Dr. Roger Reinecke joined the Kendall group and did important studies of the glycogenic effects of the adrenal steroids. It became clear that Kendall's compounds A, B, E, and F, each of which is oxygenated at posi tion 11 of the steroid nucleus, affect organic metabolism and that 11-desoxycorticosterone, and an as yet unidentified principle in the amorphous residue, affected the metabolism of electrolytes and water. Professor Hans Selye called the 11-oxy compounds "glucocorticoids" and the latter "mineralcorticoids." These characterizations were too simple and not entirely accurate, but nevertheless useful. Animal experimentation by the Mayo group ended in 1942.

Meanwhile, events in other parts of the world had begun to affect the lives of all of us: Europe was at war. Prior to the direct involvement of the United States, our armed services and the National Research Council began to organize and set priorities for research to support military medicine. Attention focused on a rumor that Germany was buying beef adrenal glands in South America for the purpose of making adrenal cortical extract. It was said that the extract was being used to counteract the hypoxia of Luftwaffe pilots to permit them to fly at higher altitudes. It was being claimed, especially by American pharmaceutical houses, that adrenal cortical extract would counteract traumatic shock and surgical shock. There were other claims that it would raise resistance of laboratory animals to hypoxia. It was well established that adrenally insufficient animals and patients are abnormally sensitive to all forms of stress. It seemed reasonable to expect that the cortical steroids would raise the resistance of combatants to the kinds of stressors encountered in war. The medical research division of the Office of Scientific Research and Development gave top priority to the synthesis of Kendall's Compound A. In late 1941 a committee of outstanding chemists, most of them experienced in steroid chemistry, was appointed to plan and direct the research. Dr. Kendall was one of them.

The committee decided to first prepare Kendall's Compound A as a step toward the more difficult job of preparing Compound E. Research toward this objective was carried out in a number of laboratories including those of Merck and Company. In the fall of 1943, Compound A was synthesized in very small yields by Professor T. Reichstein.

Research continued, especially in Dr. Kendall's laboratories. To this laboratory came a series of brilliant young chemists, several of them from Merck and Company, including Lewis H. Sarett. Dr. Vernon Mattox joined the Kendall group to stay; he has played an important role in the Section of Biochemistry since that time. The steps toward the synthesis of Compound A were improved. A small amount of the substance was prepared in Dr. Kendall's laboratories.

By the middle of 1944 all members of the collaborating groups, except those of Mayo's and Merck, stopped research on the synthesis of Compound A. By then the biological studies on the usefulness of adrenal cortical extract and adrenal steroids in raising resistance to hypoxia, surgical and traumatic shock, and other stressors had yielded largely negative results.

Collaboration between the Mayo group and the Merck group—the exchange of scientists was continued—plus information from Professor Reichstein, made it possible for Merck and Company to prepare nearly 100 grams of Compound A by the end of 1945.

Tests of Compound A were begun with high hopes by several groups of clinical scientists. All the results were in agreement: The compound had little therapeutic value in patients with Addison's disease.

Most of the once interested chemists dropped research on the synthesis of Compound E; Dr. Kendall wanted to keep at the original objective and the Merck group agreed to continue the collaboration. The officers and scientists of this company deserve great credit for farsighted policies of research in this and other areas.

The early years of the 1940s brought personal sadness to Dr. Kendall. Near the beginning of the decade, Mrs. Kendall experienced the first of a series of periodic mental illnesses. Also, their son, Roy, developed a malignancy during his medical internship, and he died after about eighteen months. Hugo Nilson and I were with Dr. Kendall in Atlantic City when this happened. I had just left the two of them when Dr. Kendall received word that his son was dead. Hugo was stunned and unable to find words to express his feelings; Dr. Kendall was a father figure to each of us, and we had affection for each member of the family. It was the older man who placed his hand on Hugo's shoulder and spoke quietly of the cruelty of disease and that man must work toward the reduction of it. Nothing short of his own death would prevent his striving toward that goal. Dr. Kendall's son, Norman, took his own life soon after he was discharged from military service, adding to the tragedies of these years.

In December of 1944, Dr. Lewis H. Sarett of Merck and Company prepared a few milligrams of Compound E. During the collaboration aimed to improve the yields, several important contributions came from the Kendall group, most of them from the young men. Dr. Kendall had learned a great deal of steroid chemistry by this time and was the source of some fruitful ideas. Still, his major role was to keep the program going and to focus the efforts of a number of gifted collaborators on the problem.

The first large-scale synthesis of Compound E was completed at Merck and Company in 1948. Most clinical endocrinologists had lost interest in the possible clinical usefulness of this and other glucocorticoids. Another phase of research on the hormones of the adrenal cortex seemed to be over.

Dr. Philip S. Hench of the Mayo Clinic had observed that patients with rheumatoid arthritis sometimes go into remission when they become jaundiced and that some women have relief from arthritis when they become pregnant. He postulated that some humoral substance formed during jaundice and during pregnancy is responsible for the remission. I heard him talk about this theory and the relevant evidence in the middle 1930s. One of the young physicians working with him came to me to learn how to ligate the common bile duct of the rat so as to induce jaundice; he then studied the joints of the jaundiced animals, but there were no significant changes. Drs. Hench and Kendall discussed the problem on a number of occasions. It was decided in 1941 to test Compound E for a possible effect on rheumatoid arthritis, when a sufficient amount became available for clinical investigation.

In September of 1948 some synthetic Compound E was injected into a female patient. It came from a supply that had been prepared at the Mayo Clinic by Vernon Mattox from a precursor (4,5-*ß*-dihydrocortisone acetate) supplied by Merck and Co. There was dramatic improvement in the patient. The clinical study was continued and expanded by the use of Compound E supplied by Merck; almost all arthritic patients treated with the steroid went into a remission that lasted as long as the hormone was given. There was great excitement at the Mayo Clinic, but it was tempered with caution against making a premature announcement. The results were kept confidential for several months.

At that time I was a research scientist at the Upjohn Company. In February of 1949, Dr. Gifford Upjohn told me of a rumor that a great medical discovery had been made at the Mayo Clinic. I made a trip to the Mayo Clinic and was taken by Dr. Hench to see arthritic patients in remission; I was shown the recently completed film of the effects of treatment. This was quite exciting, and I was especially pleased to hear the story from Dr. Kendall. He had never lost faith that Compound E would be of use in clinical medicine.

Dr. Randall G. Sprague talked with me about the possible overdosage effects of Compound E: doses required to suppress the symptoms of rheumatoid arthritis are large. Was it not likely that continued high dosage would induce Cushing's syndrome in the patients? I knew that normal animals could be brought to death by overdosage with Compound E; it seemed plausible that unwanted effects would occur in humans as well. Would the adrenal cortices of these patients undergo compensatory atrophy? It seemed probable. Would continued high dosage of the steroid damage the capacity of the anterior pituitary to again secrete corticotropin when the administration of steroid was stopped? I was almost certain this would not occur, but I was wrong. When Dr. Sprague and I met in Atlantic City in April of the same year, he told me that Cushing's syndrome had begun to appear in some arthritic patients treated with Compound E.

Did Dr. Kendall have some special foresight that Compound E and related steroids would suppress inflammation and therefore the symptoms of arthritis? He never claimed to have. Dr. Kendall had suggested the testing of Compound E in mental diseases, in cancer, and in other diseases. It is my belief that he and Dr. Hench were playing a hunch without much evidence or logic to support it. In 1941 a strange, brilliant man named Valey Menkin demonstrated that adrenal cortical extract and Compound E would suppress inflammation in laboratory animals. Most of us ignored the research of Menkin. There is no evidence that Dr. Kendall attached special significance to it, although he supplied the Compound E for the studies.

Compound E could have been tested in arthritic patients years before it actually was, for several grams that had been isolated from natural sources were at hand, and more could have been extracted and purified at any time. The first test came about as follows: In the latter half of 1948, Dr. Hench had a female arthritic patient who had not responded to treatment; she begged him to try any possible sort of new therapy. Dr. Hench came to Dr. Vernon Mattox—Dr. Kendall was on a trip—and inquired into the possibility of obtaining some Com pound E to test. Dr. Mattox replied that he could not supply the steroid unless instructed to do so by Dr. Kendall. When Dr. Kendall returned, Dr. Hench raised the question with him and the two agreed to the clinical trial.

In May of 1949 Drs. Kendall and Hench discussed the naming of Compound E; they coined the word "cortisone."

In the days, weeks, and months that followed announcement of the therapeutic value of cortisone in inflammatory diseases, there was wide acclaim, which surely brought satisfaction to each of the two men. On October 25, 1950, a newspaper correspondent with an inkling that Dr. Kendall would receive the Nobel Prize called the scientist's daughter, Elizabeth, in New York State, and she passed the news on to her father. In the early afternoon on the following day, it was officially announced that Edward C. Kendall, Philip S. Hench, and Tadeus Reichstein would receive the Nobel Prize in Medicine and Physiology for their investigations of the adrenal cortex. Replying to my telegram of congratulations, Dr. Kendall thanked me and added, "That was the day!"

Dr. Kendall shared his financial award with several of his associates who had contributed importantly to the isolation, identification, and synthesis of cortisone; Harold Mason and Vernon Mattox were among them.

Other honors and awards followed, including election to the National Academy of Sciences in 1950. Dr. Kendall retired from the Mayo Clinic in 1951, but he did not retire from the bench. He moved to Princeton, New Jersey, became a visiting professor of chemistry at the university, and set up a research program at the James Forrestal Research Center just north of the city.

By then it had become apparent that cortisone does not cure rheumatoid arthritis or other inflammatory diseases. These diseases are not caused by a deficiency of adrenal cortical hormones and prolonged treatment, with the amounts of corticoids required to suppress inflammation causes unwanted side effects in a significant number of patients. A better form of treatment was needed and Dr. Kendall set out to find it. He postulated, first in 1945, that an unidentified steroid is secreted by the adrenal cortex and that this steroid is linked with ascorbic acid. His quest included the extracting of adrenal glands and the attempted synthesis of postulated formulae. He worked toward this general objective for almost all of his remaining life. He focused on the aim to synthesize a ketal representing a conjugate of ascorbic acid and hydrocortisone. As a step in this direction, he prepared furandiones—somewhat simplified analogs of ascorbic acid. He published two communications to the editor of the *Journal of the American Chemical Society,* concerning the preparation and properties of tetrahydro-3,4-furandione and its dioxolane and dioxane derivatives. He did not achieve his objective of creating a new therapeutic agent, although each letter and each Christmas message implied that success was near.

On one occasion when I visited Dr. Kendall at his Princeton laboratories, he talked with me about a fable that I had written for reading at a dinner meeting of the Endocrine Society. It was about a great scientist who was destroyed by administrative duties. Dr. Kendall guessed correctly that I was frustrated by distractions from research; he proposed that we take a walk. Within the woods adjoining his laboratory was an abandoned circular building almost hidden by vines, brush, and untrimmed trees. Dr. Kendall called it "The Witches Nest." "I need a biologist with me," said The Chief. "If you want isolation, I will find salary money, research funds, and fix up 'The Witches Nest' for you. And you can have it with no telephone." (I dated the beginning of my troubles from the day I first acquired a telephone.) It was a high compliment from Dr. Kendall: He wanted me to work with him again. But I could not.

Dr. Kendall called our home a few weeks before he died.

I was away and he talked with my wife, Geneva. She said that he seemed tired and lonely and she was sad because this was completely unlike him. But he kept working. While having lunch with several scientists on a consulting trip to the Merck Company, May 1, 1972, he went to the blackboard to write a formula. He was suddenly taken ill, was hospitalized, and died from a coronary failure three days later. Mrs. Kendall died February 14, 1973.

I have argued that a man should be remembered for his best personal qualities and his achievements rather than for his foibles and failures, but I cannot write of Dr. Kendall without describing his weaknesses as well as his strengths. To do so would create an image of a person who never existed. His greatness lay in his ability to select important goals that were achievable, to persevere toward them during periods of adversity and disappointment, and to select gifted associates. Once when a younger associate asked permission to spend some time at another problem, Dr. Kendall replied, "I want to grow a great big oak tree; I am not interested in a bunch of blackberry bushes."

In his autobiography he writes of the driving force that keeps a scientist at an important goal, and he adds, "But two components of the drive can be understood and are appreciated by almost everyone. These are a love of whatever things are true and a desire to create something."

There was, I think, another quality of the man, which is suggested in this closing quotation by the French anthropologist C. Levi-Strauss:

A grain of slightly mad recklessness,
Might, in this domain as in others,
Be the price you have to pay for great and noble findings.

# HONORS AND AWARDS

## Awards

| | |
|---|---|
| 1921 | John Scott Prize, City of Philadelphia |
| 1925 | Chandler Medal, Columbia University |
| 1945 | Squibb Award, Endocrine Society |
| 1949 | Lasker Award (with P. S. Hench), Lasker Foundation |
| 1949 | Research Corporation Award, Research Corporation of New York |
| 1950 | Page One Award (with P. S. Hench), New York Newspaper Guild |
| 1950 | John Phillips Memorial Award, American College of Physicians |
| 1950 | Remsen Award, American Chemical Society, Maryland Section |
| 1950 | Edgar F. Smith Award, American Chemical Society, Philadelphia Section |
| 1950 | Research Award, American Pharmaceutical Manufacturers Association |
| 1950 | Passano Award (with P. S. Hench), Passano Foundation |
| 1950 | Medal of Honour, Canadian Pharmaceutical Manufacturers Association |
| 1950 | Nobel Prize (with P. S. Hench and T. Reichstein), Nobel Foundation |
| 1951 | Dr. C. C. Criss Award (with P. S. Hench), Omaha Mutual Insurance Association |
| 1951 | Award of Merit (with P. S. Hench), Masonic Foundation |
| 1951 | Cameron Award (with T. Reichstein), University of Edinburgh |
| 1951 | Heberden Award, Heberden Society of London |
| 1952 | Kober Award, Association of American Physicians |
| 1961 | Alexander Hamilton Medal, Alumni of Columbia College |
| 1965 | Scientific Achievement Award, American Medical Association |

## Honorary Degrees (Doctor of Science)

| | |
|---|---|
| 1922 | University of Cincinnati |
| 1950 | Western Reserve University |
| 1950 | Williams College |
| 1950 | Yale University |
| 1951 | Columbia University |

| | |
|---|---|
| 1951 | National University of Ireland |
| 1964 | Gustavus Adolphus College |

## Member

American Academy of Arts and Sciences
American Chemical Society
American Philosophical Society
American Physiological Society
American Society of Biological Chemists (President, 1925-1926)
American Society of Experimental Biology and Medicine
American Society of Experimental Pathology
Association of American Physicians
Endocrine Society (President, 1930-1931)
National Academy of Sciences
Sigma Xi

## Honorary Member

Columbian Society of Endocrinology
Heberden Society, London
Royal Society of Medicine of England
Swedish Society of Endocrinology

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*KEY TO ABBREVIATIONS*

Am. J. Physiol. = American Journal of Physiology
Arch. Intern. Med. = Archives of Internal Medicine
Biochem. J. = Biochemical Journal
Collect. Pap. Mayo Clin. = Collected Papers of the Mayo Clinic and the Mayo Foundation
Fed. Proc. = Federation Proceedings
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Biol. Chem. = Journal of Biological Chemistry
J.-Lancet = Journal-Lancet
J. Org. Chem. = Journal of Organic Chemistry
Med. Clin. North Am. = Medical Clinics of North America
Minn. Med. = Minnesota Medicine
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Proc. Staff Meet. Mayo Clin. = Proceedings of the Staff Meetings of the Mayo Clinic
Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians

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Eugene L. Opie

# Eugene Lindsay Opie

July 5, 1873-March 12, 1971

By Esmond R. Long

Eugene Lindsay Opie, pathologist, distinguished for pioneer and basic studies in many fields of medicine, died in his ninety-eighth year after more than seventy years of research and service in his chosen field. As a medical student at The Johns Hopkins University in the 1890s, he made an important discovery that alone would have made him eminent; namely the close relation of abnormality of the islands of Langerhans of the pancreas to diabetes mellitus. This relationship was his principal subject of investigation for ten more years. He later went on to studies that had wide influence in malaria, tuberculosis, the fundamentals of immunology, the general principles of inflammation, medical education and leukemia and cancer. In the late years of his life—his last research paper was published in 1970—he was concerned with factors maintaining the proper water balance in animal tissues. He was almost blind at that time, but retained an astonishing memory of factors that might apply, and a keen sense of touch that gave him knowledge of what he could not see. In the last weeks of his life, marred as they were by blindness and serious loss of hearing, he continued to listen with pleasure to technical discussions by his visitors, even when he had passed the point of understanding them.

Impressive as these details are, they still provide but an incomplete list of Opie's services to human health and welfare.

He held many important administrative positions, which he filled with faithful devotion to detail. He served his country, not only through his influence in teaching and research, but actually in the military activities of two wars—the Spanish-American War and World War I.

The Spanish-American War merits more than passing interest because Opie's role in it was indicative of his character and receptive mind. In 1898, with William G. MacCallum, an associate at the Johns Hopkins Medical School who also became a distinguished pathologist, he volunteered for medical service. Opie and MacCallum accompanied seriously ill and wounded patients from military institutions in the South to hospitals for continued care in the North. Typhoid fever and malaria were rampant in these camps, and the two young Hopkins interns learned much that they used later in understanding the ravages of these diseases.

This is an appropriate place to mention another pioneer investigation in which Opie and MacCallum were engaged as medical students. The eminent pathologist William H. Welch had turned Opie's attention to diabetes. Another Hopkins luminary, internist William Sidney Thayer, drew both men into important studies of lasting value on malaria. In correlated but independent researches, Opie and MacCallum worked out, in meticulous detail, the life cycles of the hematozoon parasite of bird malaria in the sparrows and blackbirds of Baltimore and—in MacCallum's case—in Ontario, Canada. These studies, despite the youth of the investigators, were never considered immature. At the end of their lives, the reports were still considered among the authors' best.

Opie was born in Staunton, Virginia, on July 5, 1873. His parents were native Virginians of distinguished ancestry. His father was a surgeon who became one of the founders of the College of Physicians and Surgeons of Maryland (later the Medical College of the University of Maryland). Son Eugene, who had determined even in childhood to become a physician, matriculated in the school of which his father was dean shortly after he had been graduated with the A.B. degree from The Johns Hopkins University, in 1893. After a year at the medical school of the University of Maryland—already steeped in Johns Hopkins traditions—Opie decided to return to the new medical school at that university. He joined the first class to graduate, completing the medical course in 1897. The unusually active part he took in medical research in his student days is indicated above.

Opie was never robust, nor given to strenuous exercise. An anecdote of his Hopkins school days is worth telling. A contemporary at Hopkins, Lawrason Brown, who became one of America's most noted phthisiologists, took Opie in hand to ensure a pleasanter life for him in the midst of a grinding curriculum. Brown was husky and strong and a leader in sports; Opie was seemingly frail and left out. Brown made the older and bigger boys include Opie in their groups and give him a chance in baseball and other sports. Ironically, after leading immensely useful lives, Brown died of advanced pulmonary tuberculosis at sixty-six, but Opie lived on despite the many infirmities of old age until ninety-seven.

Opie remained at Hopkins for seven years after his graduation in medicine, as fellow, assistant, and instructor in bacteriology under the department head, the celebrated William H. Welch, who had started the younger man on his way to eminence. During the Hopkins years, Opie made a thorough study of lesions of the pancreas. This led, first, to a new outlook on diabetes and eventually to an understanding of the role of the pancreas in furnishing an internally secreted hormone regulating carbohydrate metabolism in the body. Other investigations brought out the effect of gallstones impacted at the point of union of the bile and pancreatic ducts—diverting bile to the pancreas and causing the grave lesion hemorrhagic pan creatitis. This study integrated gross and microscopic study, and produced successful experiments reproducing the disease in animals. These investigations were formative in developing the lasting skill of Opie as an experimental pathologist.

In 1904 Opie developed a relationship that lasted all his life, with the newly created Rockefeller Institute for Medical Research (now the Rockefeller University). The Rockefeller Institute was then largely dependent for its leaders on the Johns Hopkins Medical School. The Rockefeller Institute and the Henry Phipps Institute in Philadelphia, as Opie later wrote, ensured the privilege of pursuing research under conditions that permitted the results of one study to suggest the next, year after year, with few unwelcome interruptions. One of the most favorable influences at the Rockefeller Institute was daily close association with a score of dedicated investigators who helped make up the backbone of America's early laboratory medical research. Opie's Rockefeller years, from 1904 to 1910, were marked by illuminating studies on proteolytic enzymes and their relation to immunological processes.

One of the principal printed sources on Opie's life and work in his forty most active years of research and teaching was his own article, "Peripatetic Education of a Pathologist" (in *Medical Clinics of North America,* July 1957, pp. 935-952). Opie always regretted that he did not have a chance to study under the great German leaders in pathology in his formative years, whose printed works he read assiduously. He did travel frequently in later years, and made personal contact with these men and their successors, but he never profited, as he had wished, by their elementary instruction. In his early years, Opie was himself already a leader in the mainstream of medical research and teaching and too much in demand in the developing medical schools in his own country to take the necessary time off.

In 1910 he accepted appointment as professor of pathology at the School of Medicine at Washington University in St.

Louis, then being reorganized in the light of a now famous report by Abraham Flexner. This report, on medical education in America, was based on a survey by Flexner, sponsored by the Carnegie Foundation for the Advancement of Teaching. One of its first effects was the selection of a few medical schools, not very well off at the time, for development as superior institutions for medical teaching and research. These schools were to be developed along the lines followed by the great schools of continental Europe and The Johns Hopkins University in the United States. Pathology was one of the most basic of several fundamental sciences of medicine, and was accorded a high place in the proposed revolution in medical schools. Opie was a promising candidate for a role in the forthcoming development of a superior department of pathology. In "Peripatetic Education of a Pathologist," he noted a widespread tendency of medical schools to choose pathologists as deans. The medical school of Washington University proved no exception, and Opie soon found himself in that responsible, but not altogether desirable, position. He had been in the top rank as a full member of the Rockefeller Institute; now he found himself a full professor of pathology in an institution favored for expansion, and its dean as well. The development plan called for integration of the medical school with a hospital—in this case the Barnes Hospital—in a medical center for the promotion of realistic clinical teaching.

Opie was instrumental in effecting this union; at the same time, he turned out a wealth of research on many topics. In this period, he developed his influential program of studies on tuberculosis, noting the immunologic relations of primary infection in childhood to the character of the well-known chronic tuberculosis, or phthisis, in adults. A series of publications, commencing in 1914 and never totally dropped, crystallized his concepts. In simplified form, these held that the pulmonary tuberculosis of adult type is not the continuation of tuberculosis of childhood, but rather a new exogenous infection ac quired in adolescence or adult life—a view that contrasted sharply with that held by many other noted students of the disease. A corollary of Opie's thesis was that the first infection modified the anatomical character of the tuberculosis acquired from contact in later life. Additional studies in the immunological field, particularly on enzymes, leukocytic and other phagocytic cells, and antigens and antibodies, related immunological sequences to the pathology of inflammation. These studies were continued on a wider scale at the Henry Phipps Institute in subsequent years.

During Opie's tenure at Washington University (1910-1923), he served as a medical corps officer in World War I, in grades from captain to colonel in the Hospital Unit formed at Washington University and in special commissions for the study of infectious disease in the Army. Out of his military experience came important studies on pneumonia, influenza, trench fever, and tuberculosis.

In 1923 Opie left Washington University to accept appointment as professor of pathology, director of the department of pathology, and director of the laboratories of the Henry Phipps Institute at the University of Pennsylvania. His initial program of research there is best described in his own words taken from "The Peripatetic Education of a Pathologist":

"At the Rockefeller Institute I had studied the enzymes concerned with tubercle formation and observed the beneficial effect of injected leukocytes upon the course of experimental tuberculous pleurisy. In the pathological laboratory of Washington University and during my service in the Army I had come to realize how accurately the lesions of tuberculosis are defined by X-ray films of the affected organs after their removal at autopsy . . . . At the Henry Phipps Institute of the University of Pennsylvania opportunity was offered to continue, under very favorable conditions, study of the spread of tuberculosis and to obtain insight into the pathogenesis and transmission of the disease observed in patients and their families. It was pos sible to organize the outpatient department of the Institute so that its routine operation by clinical staff, X-ray department, visiting nurses and laboratory gave accurate and continuous information about the spread of the disease through several generations in whole families, white and colored, living in the district around the institute. . . . The revealing chest films and ingenious procedures introduced by Maurice McPhedran, my associate in these studies, had an important part in establishing the value of X-ray diagnosis. The frequent occurrence of recognizable pulmonary lesions unaccompanied by symptoms became evident for the first time, and the emergence of active disease from those latent, that is asymptomatic lesions, was repeatedly observed . . . . These studies were guided by parallel investigation of the pathological anatomy, immunology and pathogenesis of the disease . . . . The relative importance of heredity, nutrition and transmission of infection was actively discussed. Studies at the Phipps Institute emphasized the dominant importance of the latter. Transmission of the disease by intimate family contact to both children and adults was clearly shown and marital transmission definitely established. Tuberculosis was found to occur in large part as long-drawn-out family or household epidemics in which it was transmitted from one generation to the next."

Opie's studies in Philadelphia were by no means limited to the clinical and visiting nurse services at the Phipps Institute itself. By arrangement with the Philadelphia school system, he carried out careful surveys of similar character on thousands of schoolchildren. At the University of Pennsylvania, he conducted investigations of the same character in medical students, whose work with patients in the autopsy room, and at the Philadelphia General Hospital, inevitably involved far more than average exposure to tuberculosis.

But this was not enough to satisfy Opie in his examination of every facet of his developing concepts. The relative susceptibility of the white and colored races was always an important consideration. He and his Phipps staff collaborated with the International Health Division of the Rockefeller Foundation in a study of the dissemination, transmission and character of tuberculosis in the predominantly black population of Jamaica. For ten years Opie visited the island every year. He found the disease more rapidly progressive in the black population of Jamaica than in either the whites or blacks of Philadelphia. This was particularly true among previously rural families who had moved from the less crowded countryside of Jamaica to the massive contagion of the towns and cities.

In all of Opie's studies, the tuberculin test as an indication of the onset of infection was invaluable. The starting dates of childhood infection, in particular, could be determined precisely, and by family X-ray examination the source of the new infection could generally be identified promptly, and protective measures instituted immediately.

Long after he left the Phipps Institute, Opie continued with this pattern of study on tuberculosis. In 1939, while on leave of absence from his position at Cornell University (see p. 301), he served as visiting professor of pathology at the Peiping Union Medical College. There he compared the anatomical character of first infection and superinfection in Chinese adults. The crowded urban and less crowded rural environments gave him an unrivalled opportunity to pursue his studies. In effect, these confirmed his previous views on the contagion of the disease. His paper in collaboration with McPhedran on "The Contagion of Tuberculosis" in 1926 had already set forth in detail many of the factors concerned in exposure and infection, including their quantitative and time relations, first infection as compared with reinfection, and such considerations as age, sex, and race. There was still controversy regarding the two contrasting theories of the origin of the common ulcerative pulmonary tuberculosis of adults, i.e., the long-held concept of *exogenous* spread from old and latent childhood infections and

Opie's concept of new, heavy and continuous infection from another person. In the long run, the latter has prevailed in public health practice.

Concurrently with his studies of tuberculosis at the Phipps Institute, Opie engaged in two other lines of highly productive research: investigations of the phenomena of immunity and leukemia. The work of Opie and his associate at the Institute, Jacob Furth, cleared up many of the mysteries of the phenomenon of anaphylaxis and the allied Arthus phenomenon. With other associates—Stuart Mudd, Joseph Hughes and Valey Menkin—he related immunological factors to surface properties of bacteria and mammalian cells, determined the role of dead tubercle bacilli as an immunizing factor in promoting phagocytosis, and described the fixation of inflammatory irritants at the site of inflammation. Opie's studies in these several fields, carried out in the 1920s, remain of practical working value today.

During Opie's tenure at the Phipps Institute, a generous anonymous donor provided substantial funds for an experimental study of leukemia in animals. A program of laboratory investigation outlined by Opie was assigned to his associate, Jacob Furth. Large laboratory space was provided, where productive studies related the leukemias in special strains of laboratory mice and chickens to heredity and the action of viruses. Opie continued these studies on a larger scale when he left the Phipps Institute in 1931 to assume the position of professor of pathology at the medical college of Cornell University and pathologist of the New York Hospital. The financial grant that had been provided in Philadelphia was transferred to Cornell, and Furth's studies with Opie were expanded on a large scale to include isolation in relatively pure form of leukemia virus and various factors in the induction of leukemia.

Opie's ten years at Cornell (1931-1941) were marked by notable contributions to the principles of medical education.

Not that this was a new field for him; he had, as noted before, taken an influential part in the modernization of medical teaching as professor and dean at Washington University. At Cornell he was intimately associated with the development of the College of Medicine and the New York Hospital as a medical center, and was indeed an important member of the hospital's Medical Board. One of his biographers, John Kidd, testified to the value of Opie's immense erudition, wisdom, rich experience, long perspective, and uncommon good sense in solving the multiple problems met by the Board. During this period influential immunological studies were also conducted, particularly with Jules Freund, on sensitization and antibody formation, tuberculosis in a variety of aspects, leukemia, and inflammation. Opie's work was not so much a return to these studies as a continuation of researches that had never been dropped.

In 1941, at the statutory retirement age of sixty-eight, Opie left the New York Hospital-Cornell Medical Center to move a few hundred yards away and continue research as "guest investigator" at the Rockefeller University, where he had been one of the original staff, thirty-seven years earlier. Retirement was a technicality. He was indeed relieved of administrative duties and the multiple harassments and demands on his time that are inevitable in an academic position of daily responsibilities. He could not stop scientific research, however. It had been a part of his life since boyhood and Opie regarded it as enjoyment rather than a series of tasks to be completed. In his own words, "It is a fortunate circumstance that most of those who follow academic careers derive so much satisfaction from the doing of their work that they are unwilling to give it up." He could also reflect quietly on the changing outlook on old age. He recalled, with his gentle and ever present sense of humor, a time when he quizzed a medical student on the time of life when a certain abnormality was likely to occur. "In the later period of life," the student replied, "between forty and fifty years of age."

For nearly thirty years after his technical retirement at sixty-eight, Opie continued scientific research at the Rockefeller Institute. There, incidentally, he occupied laboratory rooms directly across the hall from those of Peyton Rous. The two octogenarians had much in common, and must have enjoyed many reminiscences. Each had been chief editor of the *Journal of Experimental Medicine* and had watched the march of medicine as reflected in the pages of that distinguished publication.

In his period of retirement, Opie, with a few associates, published more than forty medical papers. Careful study of these reveals no decline in their significance and lucidity. The majority of the papers were devoted to carcinogenesis, water exchange, and osmotic pressures in living mammalian tissues. The latter group of studies were physiologic in essence, but had an important bearing on pathologic phenomena, particularly those of inflammation. The cancer studies were unique in their exposition of certain dietary factors in relation to the development of experimentally induced tumors. Interest in the problems of previous papers had not abated. Opie brought up to date certain aspects of studies on tuberculosis, leukemia, and bile-induced injury of the pancreas. His final papers (1970) were on arteriosclerosis in the mesenteric arteries of rats, and adoption of standards of the best medical schools of Western Europe by schools in the United States. It must not be forgotten that the later studies were carried out in the face of gathering frailty of age and loss of sight and hearing. Opie retained a good memory, however, and had an extraordinary sense of location. He negotiated the short distance from his apartment to his office each day on foot and through traffic, and he could tell a visitor exactly where to find a piece of apparatus or a particular book on his laboratory shelves.

He emphasized the fact that it was never too late to learn. He looked back frequently to his experience in China in his late sixties, when he observed the unique combination of modern medicine with ancient philosophy, which allowed two systems of the practice of medicine to exist side by side. Speaking of his China experience, he said, "Some of my education has come late in life, unfortunately, because I might have profited more if it had come earlier."

He reflected frequently on what constituted good teaching in medicine, recalling as formative the close association he had with great men at Johns Hopkins in the correlation of clinical medicine with pathology. He considered the so-called clinical pathological conferences one of the most fruitful practices in developing an intimate understanding of medicine. Also, as an experienced experimental pathologist himself, he believed that students who might well be engaged in research in later years should not wait until they were faced with investigative problems that might require experimentation, but should learn experimental techniques in their formative days. Accordingly, in his years at Cornell, he offered elective courses in experimental pathology, and learned to his joy that the majority of students wished to take them. His method was to assign a problem to groups of four students in various fields in which guidance was at hand in the persons of various members of the faculty of the department of pathology. The method proved remarkably successful. Qualified students went on to research in their postgraduate years, much better prepared than they otherwise would have been. Many undertook fulltime careers in medical teaching and research. Opie discussed the full-time versus the part-time system of teaching and research in medical schools, but without taking a partisan stand. It seemed obvious to him that each system had advantages.

Opie always gave great credit to the philanthropic institutions and persons who provided financial support for his work. Prominent among these was the International Health Division of the Rockefeller Foundation, which was extremely helpful in his tuberculosis and immunological studies. The National

Tuberculosis Association also aided in Opie's tuberculosis research. The leukemia researches were supported by an anonymous donor that he never identified publicly, and by a trust fund in India established by Sir Dorabji Tata in honor of his wife. A study of arthritis, which Opie largely turned over to his younger associates, was promoted by the Markle Foundation. In general, Opie used this support to secure stipends for the numerous assistants and associates he needed in his many-faceted investigations.

Opie served many scientific societies in his long life. He was elected president of the American Society for Experimental Pathology (1923), the Society for Experimental Biology (1934), the Harvey Society (1936), the American Association of Pathologists and Bacteriologists (1918), the National Tuberculosis Association (1929), and the American Association of Immunologists (1929). He was a member of the Board of Scientific Directors of the. International Health Division of the Rockefeller Foundation (1934-1938), and of the Executive Committee of the International Union against Tuberculosis. He was honored many times for his work in special fields. Medals conferred upon him included the Trudeau Medal of the National Tuberculosis Association, the medal of the Puerto Rico Society of Phthisiologists, the Gerhard Medal of the Pathological Society of Philadelphia, the Gold Headed Cane of the American Association of Pathologists and Bacteriologists, the Banting Medal of the American Diabetes Association, the gold medal of the New York Academy of Medicine, the T. Duckett Jones award of the Helen Hay Whitney Foundation, and the Weber-Parkes prize of the Royal College of Physicians of London. He became a member of the National Academy of Sciences in 1923, and was awarded its Kovalenko Medal in 1959. He took all of these distinctions in stride and never capitalized on any of them.

Opie was twice married, first to Gertrude Lovat Simpson, on August 6, 1902., and, after her death, to her sister Margaret

Lovat Simpson, on September 16, 1916. There were four children by the first marriage, Thomas Lindsay, Anne Lovat, Helen Lovat and Gertrude Eugenie. The four survived their father.

Opie was a gentleman in every sense of the word. He was not expansive in social relations, but was always friendly and always helpful when his advice or assistance was sought. He had a quiet sense of humor that must have helped him many a time in his busy life. He could be firm in defending his opinions in controversy, but always temperately.

That wise commentator and warm friend of many years, Peyton Rous, writing of Opie's accomplishments under the intriguing title, "An inquiry into certain aspects of Eugene L. Opie," at the time of Opie's academic retirement (*Archives of Pathology,* 1942, 34:1-6) described him as a humanitarian in a telling sense of the word, gifted as a medical teacher, organizer, advisor, and administrator, adventurous in imagination, direct, simple and devout. These were indeed his attributes.

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*KEY TO ABBREVIATIONS*

Am. J. Hyg. = American Journal of Hygiene

Am. J. Hyg. Monogr. Ser. = American Journal of Hygiene Monograph Series

Am. J. Med. Sci. = American Journal of Medical Sciences

Am. J. Pathol. = American Journal of Pathology

Am. Rev. Tuberc. = American Review of Tuberculosis

Arch. Intern. Med. = Archives of Internal Medicine

Arch. Pathol. = Archives of Pathology

Arch. Pathol. Lab. Med. = Archives of Pathology and Laboratory Medicine

Chin. Med. J. = Chinese Medical Journal

Bull. Johns Hopkins Hosp. = Bulletin of the Johns Hopkins Hospital

J. Am. Med. Assoc. = Journal of the American Medical Association

J. Exp. Med. = Journal of Experimental Medicine

J. Immunol. = Journal of Immunology

J. Med. Res. = Journal of Medical Research

J. Tech. Methods = Journal of Technical Methods and Bulletin of the International Association of Medical Museums

Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences of the United States of America

Trans. Natl. Tuberc. Assoc. = Transactions of the National Tuberculosis Association

Twentieth Century Pract. = Twentieth Century Practice

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Jn R. Paul.

# John Rodman Paul

April 18, 1893-May 6, 1971

By Dorothy M. Horstmann And Paul B. Beeson

John Paul was born in Philadelphia, Pennsylvania, the third child in a family of eight. His father was Henry Neill Paul, and his mother had been Margaret Crosby Butler of Yonkers, New York. The Pauls trace their ancestry to Joseph Paull of Illminster, England, who emigrated to this country in 1685, settling with William Penn's Quakers in Philadelphia. On his mother's side, John was a descendant of Theophilus Eaton, the first governor of Connecticut, and of Benjamin F. Butler, Andrew Jackson's attorney general. His maternal grandfather was William Allen Butler, an eminent lawyer and poet.

John's father was also a lawyer and a man of broad scholarly interests. He loved Shakespeare and Elizabethan drama, was Dean of the Philadelphia Shakespeare Society for many years, and published a number of Shakespeare commentaries as well as *The Royal Play of Macbeth,* a scholarly analysis of the origins of the play. His interests were also in natural history. In the 1880s, while at Princeton, he had taken part in expeditions to Montana for geological and paleontological explorations. His accounts of these trips fascinated John, who later counted them as a major factor in awakening his interest in science at a very early age.

This interest was further encouraged when the Paul family settled in Chestnut Hill, then an open suburb of Philadelphia, with woods, streams, and fields where the Paul children could roam and bring home to their family museum minerals, butterflies, birds eggs, and even Indian arrowheads. Summers were spent at Beach Haven on the New Jersey shore, and shells and sea specimens were also added to their collections. A love of the out-of-doors was a deep and sustaining influence throughout John Paul's life, as well as the source of many of his hobbies—bird watching, archeology, wood carving, building stone walls, watercoloring, and photography, etc. It was also responsible for his being an active and articulate conservationist as early as the 1930s, long before the need for preservation of the environment became a popular cause.

As a child, John is said to have been reserved, rather shy, but with a strong humorous streak. He had stamina and energy but was not robust, so to improve his health he was sent to New Hampshire at age eleven, where he was tutored and spent much time out-of-doors during the winter before entering St. George's School, in Newport, Rhode Island. The six years at St. George's were important and happy ones: he was an outstanding student and won many scholastic prizes in Latin, Greek, and history. He became editor of the school magazine, manager of the football team, and coxswain of the school crew.

In 1911 John entered Princeton. At first he led a relaxed and carefree life there without any particular scholarly focus—until he came under the influence of Edwin Grant Conklin, professor of biology. Conklin was the kind of professor who lit fires under his students; from then on John took off scientifically and spent as much time as possible in the laboratory. Still he lived a full life at Princeton: he was very popular in his class, was an editor of the Princeton *Tiger,* a member of the Ivy Club, and manager of the crew and sometimes its coxswain.

The decision to study medicine came about through association with Cecil Drinker, one of John's heroes at the time and subsequently Dean of the Harvard School of Public Health.

The Paul and Drinker families had been neighbors and close friends during summers at Beach Haven, where Cecil had taught John to sail the Drinker's yawl. Cecil's younger brother, Philip, and John were the same age and became fast friends. Later they roomed together all through St. George's School and Princeton. During the Princeton years, Cecil, Philip, and John sailed together in the summers and it was on these pleasant cruises that Cecil persuaded John that he should go to medical school. The advice was accepted, and after graduation from Princeton in 1915, he enrolled at the Johns Hopkins University School of Medicine. His goal, he said in later years, was "to be a medical scientist just like Cecil," rather than a practicing physician, the role his family had in mind for him. He never swerved from his commitment to a career as an investigator.

In 1917, when he was in his second year of medical school, the United States entered World War I. In June, along with thirty-one other medical students, Paul joined the Hopkins unit as an enlisted man and sailed from New York in the first U.S. convoy of World War I to head directly for France, carrying combat and other troops—the vanguard of Pershing's army. The major part of the army transport on which he found himself was occupied by seasoned soldiers, and (as he wrote later) the medical contingent "consisting of the Johns Hopkins Hospital Unit, a hastily assembled and motley group of raw recruits, occupied a place befitting their military experience—far astern and deep in the bowels of the ship." After eighteen long days at sea, often in submarine-infested waters, the overcrowded ships finally reached St. Nazaire, where the men received a warm welcome from the French. The Hopkins unit went on by train to Bazoille sur Meuse, where Base Hospital #18 was set up, well back of the front line. John's assignments were as bacteriological technician and substitute ambulance driver. The bacteriology laboratory was a valuable experience that apparently had considerable influence in shaping his subsequent career. His first duty was to determine the effectiveness of a new method of treating wound infections by continuous irrigation with a sodium hypochlorite solution devised by Dakin, an English chemist. The mountainous task of doing daily smears and bacterial counts on swabs from the wounds of the many patients who had infections with gas-forming bacilli eventually proved dull work; and since the results did not prove to be helpful as indicators of the course of infection, the study was finally abandoned. Driving an ambulance turned out to have greater rewards, which involved a month at St. Nazaire assembling Ford ambulances (they arrived two in a box) and proudly driving the finished products back to Bazoille.

After a year in France, Paul, his great friend John M. T. Finney, Jr., and others of the students at Base Hospital-#18 were persuaded that they should return to Hopkins to complete their medical education. By a fortunate stroke of fate, Paul missed the ship on which he was scheduled to return—one that was torpedoed and lost—and came through on another, which sailed successfully from Brest to New York. Shortly after his arrival in Baltimore, the 1918 influenza epidemic erupted and the wards of the Johns Hopkins Hospital were filled with desperately ill patients, many of them professors and staff members. Apparently the wave of relatively mild respiratory illness that had swept France in 1917 was the forerunner of the more severe 1918 epidemic, and infection with the agent in France conferred immunity on those who had had experience with it, including John Paul. At that time he had had no clinical training but in the desperate situation was put to work on the wards as a substitute intern, to care for the patients as best he could. It was a harrowing ordeal, since the mortality rate was extremely high. The helplessness of the physicians made a deep impression on the young Paul. As he wrote years later:

"Many were the nights I passed, making do with a few hours of uneasy sleep grabbed as best I could from a 20-hour day, tossing and twisting and deciding that it would have been better if the influenza patients had never come to the hospital. The lessons we learned in those days were not how to treat patients who had postinfluenza pneumonia with drugs, but rather how to save their lives by preventing exposure on the isolation wards by mixing them up with cases of tonsillitis and scarlet fever. If the patients with influenza were kept by themselves they had a far better chance of avoiding cross-infection with pathogenic bacteria, especially the hemolytic streptococcus. This was an *epidemiological* principle reminiscent of the days of Semmelweis and puerperal fever."[*]

Although his military experience caused him to miss his third year of medical school, Paul graduated with the class of 1919. He immediately joined W. G. McCallum, professor of pathology at Hopkins on a trip to Lima, Peru, where the summer was spent working on bartonellosis. The routine consisted of doing autopsies at the hospital in the mornings and exploring the city and its archeologic treasures in the afternoons.

On his return to Baltimore, John joined McCallum at Hopkins as an assistant in pathology. Two of his classmates, Arnold Rich and Leslie Webster, did the same, and the three had a lively and productive year in the laboratory. Paul's first contribution to medicine was made during that year—a paper on the histopathology of measles conjunctivitis. Thus his first work dealt with a virus infection—a prophetic note since he devoted most of the rest of his professional life to investigations in that field.

The next two years were spent as an intern at the Pennsylvania Hospital in Philadelphia. In 1922 he was appointed Director of the Ayer Clinical Laboratory of the Pennsylvania

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[*] J. R. Paul, "A Clinician's Place in Academic Preventive Medicine: My Favorite Hobby," *Bulletin of the New York Academy of Medicine* 47(1971): 1264 (hereafter cited as "Clinician's Place").

Hospital, a post that had previously been held by Dr. Warfield T. Longcope. Activities during the six-year period at the Ayer Laboratory resulted in papers on a variety of subjects dealing with bacteriology and pathology, including the first ones on rheumatic fever, a disease that was to engage much of his attention in the ensuing years. In fact, it was when he presented a paper on the pleural and pulmonary lesions in rheumatic fever at the clinical meetings in Atlantic City in the spring of 1928 that he was invited by Francis Blake, professor of medicine at Yale, to join his department as an assistant professor. Thus began Paul's long association with Yale—one that continued until his death forty-three years later.

In the 1920s, the Yale Medical School was in the midst of a renaissance under the dynamic leadership of Dean Milton C. Winternitz. Winternitz had gathered together for the recently created full-time faculty a stellar group of young clinician scientists, including among others Francis Blake, John P. Peters, Grover Powers, and James Trask. According to Dr. Paul, they were all "young, eager, and well trained men, imbued with the idea of making the fulltime system work." He himself fitted into this setting perfectly and within several years of his arrival in New Haven had launched into several major pieces of work that proved to be important landmarks in clinical investigation. Among these were the studies of rheumatic families, the discovery of the heterophile antibody (Paul-Bunnell) test for infectious mononucleosis, and the demonstration that the commonest clinical expression of infection with polioviruses is not paralysis, but the "minor illness" or "abortive" form of the disease that is often so mild as to go unrecognized.

The seeds of the scientific philosophy that characterized his later work at Yale, and in fact ran throughout his entire professional life, were planted early in John Paul's mind—when he was a second-year medical student. At that time he attended a meeting of the Federation of Biological Sciences in New York

City at which there was a lively discussion of the disastrous epidemic of poliomyelitis that had raged in New York and New England during the summer of 1916. In looking back on that occasion, he wrote:

"I must have been a singularly impressionable young man at the time, and I was certainly engrossed in watching and hearing the words of these great men, who were engaged in recounting their efforts to attempt to solve the problem of epidemic poliomyelitis, applying the very weapons which we had been taught to use in our first years at medical school. Dr. Simon Flexner was in the chair; Dr. Peyton Rous was at his side as secretary; and Hideyo Noguchi, who was there as a speaker, was introduced as a man of mystery, one who could almost turn lead into pure gold, or at least turn the virus of poliomyelitis into 'globoid bodies.'

"As a rapt listener, the idea first dawned on me that the religion of the true physician was incomplete without having the concepts of prevention thoroughly ingrained in him. This was particularly true when it came to the prevention of such a colossal tragedy as the 1916 epidemic. My immature reasoning, which I never lost, was that, together with attempts to cure this pestilence, there should be attempts to control it, and this should be done by clinicians who knew the disease best. In other words, this concept should radiate from the top physicians and pediatricians."[*]

Once these ideas had taken root, they were nourished by Paul's experience during the 1918 influenza epidemic and gradually flowered in the 1920s and 1930s as his concept of "Clinical Epidemiology" developed and took form. In his presidential address to the American Society for Clinical Investigation in 1938, he said:

"The term, Clinical Investigation in Preventive Medicine,

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[*] Paul, "Clinician's Place," p. 1263.

is cumbersome and so I will not use it. . . . It presupposes the existence of a so-called sister science, Curative Medicine. . . . Clinical Investigation in Epidemiology is better for the purposes at hand; Clinical Epidemiology is best, and really what I mean. . . . It is a science concerned with circumstances, whether they are 'functional' or 'organic' under which human disease is prone to develop. It is a science concerned with the ecology of human disease. It must face the question of 'why' as well as 'how'. Clinical Epidemiology differs, therefore, from the orthodox science of Epidemiology, both in its aim, and its locale, as it were. The orthodox epidemiologist must of necessity deal dispassionately with large groups of people. It is the multiplication of observations which give him his results. The clinical epidemiologist, on the other hand, must of necessity deal with small groups of people; people whom he knows well and groups no larger than a family, or small community. The restriction of the size of the group rests on the fact that clinical judgment cannot be applied wholesale, without the risk of its being spread too thinly to be effective. . . . The clinical epidemiologist, . . . starts with a sick individual and cautiously branches out into the setting where that individual became sick, the home, the family, and the workshop. He is anxious to analyze the intimate details under which his patient became ill. He is also anxious to search for other members of the patient's family, or community group who are actually, or potentially, ill. It is his aim to thus place his patients in the pattern in which he belongs, rather than to regard him as a lone sick man who was suddenly popped out of a health setting; and it is also his aim to bring his judgment to bear upon the *situation,* as well as on the patient.

"Obviously there is nothing new to the family doctor about this concept of Medicine. It is the heart and soul of family practice and probably has been as long as family practice has existed. But now that the emphasis has shifted away from the home and into the Hospital or Dispensary, clinical epidemiology will be practiced only if we take thought about it."[*]

Dr. Paul never cared for the term "Preventive Medicine," although eventually his professorship and his section at Yale used this terminology. He regarded it as "too boastful, too suggestive that great things might be just around the corner."[†] His belief was that the focus should be on the teaching of the underlying principles of prevention, i.e., epidemiology. In championing these concepts he was perhaps ahead of his time, but in the past decade his pioneering efforts have begun to bear fruit. The best possible support is provided by his own achievements in which he combined so successfully the study of certain diseases: at the bedside, in the laboratory, and in the natural setting in which they developed.

In the New Haven Hospital in 1928 rheumatic fever was a common disease. Paul took advantage of the opportunities this situation provided and turned his attention to unraveling the epidemiology of the disease. His focus was on rheumatic fever in families and the factors involved in its spread. Many of the social and environmental aspects were explored through intimate, long-term studies over an eight-year period of all members of 122 rheumatic families and suitable control families. When he began his studies, the role of the hemolytic streptococcus was not yet appreciated, but based on his observations Paul concluded that respiratory infection of some kind precipitated the acute attack. While not the first to suggest a relationship between the hemolytic streptococcus and rheumatic fever, it is fair to say that the book he published in 1930, *The Epidemiology of Rheumatic Fever,* and particularly the second edition in 1943, set forth the evidence for a causal relationship in such a way that there was never any further question about it.

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[*] J. R. Paul, "Clinical Epidemiology," *Journal of Clinical Investigation* 17 (1938):539-41.

[†] Paul, "Clinician's Place," p. 1267.

It was in the course of serologic investigations of patients with rheumatic fever that the heterophile antibody test for infectious mononucleosis was discovered. This came about when, having confirmed the observation made by Davidson in 1929 that agglutinins to sheep cells are present in the sera of patients with serum sickness, Paul raised the question whether such heterophile antibodies might not also be present in rheumatic fever since there were similarities between the symptoms of the two diseases. The results with sera of rheumatic patients were negative, but quite by accident, among the control specimens from patients with serum sickness and various other acute illnesses, there was one with an extraordinarily high titer—higher than had ever been described in serum sickness or any other clinical condition. The patient from whom the specimen came was a medical student with infectious mononucleosis. Gradually over the ensuing months several other patients with this disease were also found to have high heterophile antibody titers, while tests on some 275 controls gave consistently negative results. In 1932 Paul and Bunnell, a medical resident who collaborated on the project, published their findings. The test, which is still sometimes referred to as the Paul-Bunnell test, remains today as the chief laboratory method in the diagnosis of infectious mononucleosis.

The first investigations of poliomyelitis, the disease on which Paul's main work was subsequently concentrated, also began early in the 1930s. In Middletown, Connecticut, twenty-six miles from New Haven, a small epidemic occurred in 1930. Paul and his colleague James Trask were struck by the wide range in severity of the disease. Some suspected cases not only did not have paralysis, but had little or no neck stiffness. Were these also infections with the virus of poliomyelitis? The following year, New Haven experienced a sharp epidemic and the opportunity to answer this question by attempting to isolate the virus presented itself. Characteristically, Paul and Trask went into the homes of patients hospitalized with paralysis and visited others in the neighborhoods where the cases were coming from. They took histories, did physical examinations, and collected throat washings from siblings and contacts who had minor illnesses—brief episodes of headache, vomiting, and sore throat, commonly labelled "summer grippe." Family after family, through which waves of such illnesses passed, were studied. The throat washings were duly tested by intracerebral inoculation of monkeys, and from two children, polioviruses were recovered. These were the first isolations of the virus from living patients in over thirteen years, and they added significantly to the handful of such successful isolations reported in the world literature up to that time. In describing these important results, Dr. Paul later pointed out that the work had been undertaken against the advice of several well-known senior investigators in the field who considered the approach "one which was expensive and would not yield valuable results. . . . Monkeys cost from 6 to 8 dollars each. This was regarded as an expensive laboratory animal, and it was deemed unwise for amateurs to spend so much money on this type of research, particularly when it was their initial piece of work in this field and there were many more orthodox things to be done."[*] The successful outcome in the face of such discouraging pronouncements bolstered the confidence of the two young investigators, and Paul and Trask continued their "unorthodox" studies. Using recently isolated human strains of the virus, they soon made another major contribution by demonstrating that polioviruses exist in at least two and possibly three serologic types. A preliminary report of similar findings by Burnet and McNamara in Australia had appeared in 1931, but it had been greeted with incredulity by most U.S. Workers.

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[*] J. R. Paul, "From the Notebook of John Rodman Paul," *Yale Journal of Biology and Medicine* 34(1961-62):164.

These early and highly productive investigations by the Yale Poliomyelitis Study Unit, founded in 1931 and led for many years by Paul and Trask, were supported by small grants of several thousand dollars from various sources; but in 1936, the unit received the first research grant ever given by the President's Birthday Ball Commission, which eventually became the National Foundation for Infantile Paralysis and finally the National Foundation. The Foundation continued its support of the unit through Grant # 1 until 1968.

In the mid-1930s, Paul's work was interrupted by illness. He developed severe pulmonary tuberculosis, and after a brief hospitalization in New Haven went to The Desert Sanitorium in Tucson, Arizona, where he remained for two years. The time spent there, far from being a fallow period, was full of activity of quite a different sort. Leita Paul accompanied her husband and took advantage of the opportunity to study the archeology of the Indians of the Southwest at the University of Arizona. She reported to John on lectures and seminars, and he took an equal interest in the subject. The experience was a source of lasting fascination with Indian culture for the Pauls. As he improved, John joined Leita in digging expeditions that yielded many fine pieces of pottery. During the summers—when Tucson was unbearably hot—they stayed in Tyrone, New Mexico. There they became particularly interested in the Mimbreño tribe, whose pottery designs were unusual in that they were zoomorphic. Paul collected and photographed many rare specimens: his interest culminated in a fine exhibition of Mimbreño pottery, which he displayed in 1956 at the Yale Art Gallery, working with art students in gathering materials and preparing and illustrating the catalogue with his own drawings.

Several other major hobbies also flourished in Arizona: photography, ornithology, and painting. During the first year, largely spent in bed on an open-air porch, a camera rigged to the foot of the bed allowed photography of western birds—a whole world of new species to learn about. Later, many of these were portrayed in delicate watercolors of great precision and charm.

Throughout the Arizona period there was regular communication with the laboratory in New Haven. In fact, a letter went out to Dr. Paul from Dr. Trask every single day during the two years. Plans for experiments were discussed, results were reported and analyzed, and at one point Dr. Trask came out for a visit. As convalescence progressed, Paul was able to undertake some work in the laboratory at The Desert Sanitorium and also to serve as a consultant on rheumatic fever cases. His interest in this disease was as lively as ever, so he decided to take advantage of his situation by investigating the effects of climate on its occurrence by determining the prevalence of rheumatic heart disease among Indian children living in reservations from the edge of Mexico to the Canadian border. Since the social and environmental aspects of life on the various reservations were similar, any differences in rates might be attributed to climate. With Dr. George Dixon, Dr. Paul started off in the family Ford and systematically examined approximately 1000 Indian school children in Montana, Wyoming, New Mexico, and Arizona, representing latitudes of 45°, 43°, 37°, and 33°. The results indicated that rheumatic heart disease was ten times more prevalent in those living close to Canada than in children living on the Mexican border.

Back in New Haven in 1937, work on poliomyelitis went forward rapidly. The thrust was still toward exploring the clinical epidemiology of the infection as a clue to how the virus was spread and how the disease might be prevented. Having previously demonstrated the presence of the agent in the throats of individuals with the minor illness, or the "abortive" form of poliomyelitis, Paul and Trask again went into the homes, this time to collect fecal material from siblings of paralytic cases. Previous tests of such specimens had been unsuccessful, largely because of the problem of bacterial contamination and resulting brain abscesses in intracerebrally inoculated animals. From a fecal specimen collected in 1937, however, poliovirus was isolated, by switching to intraperitoneal inoculation of the monkeys. The recovery of virus from feces had actually been reported some twenty years earlier by Swedish workers, but their results had been challenged by Dr. Flexner of the Rockefeller Institute and that was enough to cause the subject to be dropped.

Paul and Trask's confirmation of the presence of virus in the intestinal tract opened a whole new era in research on poliomyelitis: it turned investigators away from the unfruitful experiments with laboratory-adapted strains that had led to false conclusions and an incorrect characterization of poliovirus as a strictly neurotropic agent. The swing of the pendulum back to clinical investigation and field epidemiology led rapidly to the demonstration by the Yale group that during epidemics the virus is shed not only from the throat and intestinal tract, but is also present in sewage and in flies that feed on feces.

During the 1930s, every summer brought large epidemics of poliomyelitis somewhere in the United States, and Paul and Trask were called upon as consultants to aid the harassed and frustrated health officers who were helpless in the face of mounting numbers of cases. At least the presence of "experts" gave the appearance that *something* was being done. Thus opportunities were provided to engage in a series of clinical epidemiological studies. Investigations were carried out in Philadelphia; Los Angeles; Charleston, South Carolina; Detroit; Buffalo, New York; Florida; Toronto, Ontario; and Winnipeg, Manitoba; to name but a few.

In the early 1940s, with the outbreak of World War II, new problems and new responsibilities arose. Dr. Paul was appointed Director of the Commission on Neurotropic Virus Diseases of the Army Epidemiological Board. The Commission was set up to study arthropod-borne virus diseases and poliomyelitis in members of the U.S. Armed Forces. In 1943, Dr. Paul headed a small group known as the Virus Commission, which included besides himself, Maj. Albert Sabin and Lt. Col. Cornelius Philip. The team was dispatched to North Africa to study hepatitis, sandfly fever, and poliomyelitis. Dr. Paul's diaries vividly describe Cairo at the time as unbelievably crowded, teeming with British and American military personnel and allied and axis civilians. The laboratory was finally set up outside the city in the desert, close to the 38th General Hospital, which was staffed by the Jefferson Medical College Unit. One of the first problems to be dealt with was the occurrence of increasing numbers of cases of suspected poliomyelitis among U.S. troops. The British army had reported cases of myelitis and encephalitis some months earlier, and a strain of poliovirus had been recovered at the Rockefeller Institute in New York from nervous tissue of a fatal case. But the U.S. medical officers, particularly the neurologists, were very skeptical about the diagnosis of poliomyelitis in U.S. troops in Egypt and accepted it only after the virus was repeatedly isolated from various patients. Obtaining monkeys to carry out the tests was a problem, and Paul had to visit Ethiopia to trap his own animals and bring them back to Cairo.

These results confirming the occurrence of poliomyelitis among adult "immigrants" to the area were of particular importance to Paul because of their implications concerning the epidemiology of the infection. Paralytic poliomyelitis was considered a rare disease in Egypt—not more than five or six cases were reported yearly in a population of 16 million, and all were in young children, none in adults. Paul concluded that the paucity of cases among the local inhabitants did not denote absence of the virus, but quite the opposite. The presence of *infantile* paralysis denoted probable wide dissemination of the agent in a population living in a poor sanitary environment:

circulation of the virus was confined to susceptible young children who acquired their infections in the first few years of life and subsequently remained immune for the rest of their lives. However, when susceptible adults, such as soldiers from England and the United States who had never met the virus before, were introduced into such an area of high endemicity, they were exposed, infected, and contracted paralytic poliomyelitis at a surprising rate. Several years later Paul provided further confirmation of this hypothesis in studies that he conducted among U.S. troops in other parts of the world, including the Pacific and the Far East. Shortly after the war he obtained the final proof in the course of serologic investigations conducted in Egypt. By means of these he showed that a high percent of children in the area had experienced poliovirus infections and had acquired specific antibodies by two years of age, and by four virtually all were immune. It was in connection with these efforts that he coined the term "serological epidemiology" to describe an approach that in his hands had yielded discoveries of fundamental importance.

The other two diseases that the Virus Commission was assigned to investigate, hepatitis and sandfly fever, were both continuous sources of enormous morbidity in the U.S. Army, seriously interfering with the conduct of military operations. An amicable arrangement was worked out in which Sabin and Philip took on sandfly fever, and Paul chose to study hepatitis. In these studies he was joined by Maj. W. P. Havens of the 38th General (Jefferson) Hospital Unit. Epidemiologic features of cases in British and American troops in North Africa and in Sicily during the epidemic of 1943 were investigated, and sera and stool specimens were obtained from patients in the acute phase of the infection. These materials were kept frozen until some months later, when, having recovered from a severe bout of hepatitis, Paul returned to New Haven, where he was joined by Major Havens. Together they set about testing the speci mens collected in Egypt in volunteers in order to identify virus positive pools that might be used in attempts to transmit the infection to some experimental animal. These studies in volunteers constituted the first demonstration in the United States of the enteric—oral transmission of infectious hepatitis; similar results were obtained simultaneously in England by MacCallum and Bradley. Paul and Havens later went on to define the times at which hepatitis virus was present in blood and feces both before and after the appearance of jaundice. They also conducted cross-protection studies in volunteers and showed a lack of cross immunity between serum and infectious hepatitis. This work, along with studies by others, formed the basis of the differentiation of the two hepatitis viruses and the diseases they cause, a classification that still holds today.

After World War II, with the return to peacetime activities, Paul turned his attention to building the Section of Preventive Medicine into an active teaching, training, and research unit. The Section had been established in the Department of Internal Medicine at Yale in 1941, with Dr. Paul as professor and chairman. He preferred this arrangement to having a separate department because he viewed his territory as an integral part of medicine and pediatrics rather than a separate discipline allied to public health. The focus was to be on "the epidemiology of disease in families, in the local community, . . . clinical and environmental virology, including the role of insects and animals as vectors of human disease, etc."[*]

Paul struggled in the not too favorable climate of the 1940s and 1950s to bring his concepts of clinical epidemiology to medical students, house staff, fellows, and faculty. He championed the home visit as a teaching exercise in which a student, accompanied by a faculty member, could gain some idea of the role played by family and environmental and social deter

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[*] Paul, "Clinician's Place," p. 1268.

minants in influencing the patient's disease, whether it was an acute infection or a chronic or recurrent malady. He strongly supported J. N. Morris's contention that "epidemiology . . . helps to complete the clinical picture and to clarify the natural history of disease."[*] His efforts to bring his philosophy to students and colleagues also took form in his book, *Clinical Epidemiology,* published in 1958 "to introduce this subject to doctors or students of medicine, biology, or sociology in non-technical language and with examples they might use."[**] The book went through several editions and has been the text for students in courses given in many departments of preventive medicine in the United States.

To the Section of Preventive Medicine—later Epidemiology and Preventive Medicine—over the years came a stream of postdoctoral fellows from the United States and other parts of the world. Many of them subsequently went on to distinguish careers in virology and epidemiology in Europe, South America, and the United States. At Yale they absorbed the philosophy of clinical epidemiology that permeated Paul's laboratory, while participating in ongoing studies on rheumatic fever, arthritis, streptococcal infections, measles, infectious mononucleosis, hepatitis, and particularly poliomyelitis.

Work on poliomyelitis was in full swing in the Yale laboratory in the 1940s and 1950s, and some of Dr. Paul's most telling contributions were made during this period. One came about in the course of a serological survey of poliovirus antibodies among north Alaskan Eskimos that he conducted in 1949. There was no evidence that poliomyelitis had occurred in the Point Barrow area since 1930, when illnesses suggestive of the paralytic form of the disease had been recorded. In those

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[*] J. N. Morris, *Uses of Epidemiology,* 2d ed. (Baltimore: Williams & Wilkins, 1964), p. 277.

[**] J. R. Paul, *Clinical Epidemiology* (Chicago: Univ. of Chicago Press, 1958; rev. ed., 1966), p. xiii.

pre-tissue-culture days the only feasible technique for large-scale serum surveys was the mouse neutralization test, which could be used to detect only Type II poliovirus antibodies. Remarkably, Paul and his colleagues found that virtually none of the Eskimos under the age of twenty years had Type II antibodies, whereas these were almost universally present in persons over that age, i.e., those who had been alive in 1930 when poliomyelitis was known to have visited the area. Clearly the 1930 epidemic had been due to Type II—an extraordinary piece of luck for the investigators. Some of the Eskimo sera were also tested for antibodies to Types I and III polioviruses by the more cumbersome and restricted technique of neutralization tests in monkeys. The results were unexpected and extremely provocative: antibodies to these two types were present only in persons over thirty-five and forty-five years, respectively, indicating that Types I and III had been absent for very long periods. The import of these findings, the "moment of truth" as Paul put it, was that a single exposure to a poliovirus type could convey lifelong immunity. From that time on, he held the belief that the way to provide permanent immunity against poliomyelitis was to induce inapparent infection with attenuated virus strains. He therefore aligned himself early with the approach championed by Sabin, and by Koprowski, which eventually led to the development of the currently used oral poliovirus vaccines.

In the 1950s Paul and his colleagues conducted small and large field trials of the Sabin strains in the United States and in Central America, with special emphasis on the unique opportunity that the live virus vaccine afforded to do experimental epidemiology in humans. These studies contributed not only to establishing the safety and effectiveness of the vaccines, but they also provided precise information about the incubation period of the infection, the capacity of polioviruses to spread among susceptible contacts, the prevalence and interfering ef fect of infection with other enteroviruses on oral vaccine "takes," and the dissemination of vaccine strains in the environment—in flies and in sewage.

By the 1950s, Paul had become a recognized international figure in virology and epidemiology. Inevitably, with his great knowledge and almost unique perspective, he was constantly being asked to serve on committees for the United States Armed Forces, the National Institutes of Health, the World Health Organization (WHO), and others. For fifteen years he was Director of the Commission on Viral and Rickettsial Infections of the Armed Forces Epidemiologic Board, and for twenty years, a member of several of the WHO Expert Committees on Viral Diseases. In 1956 he also served as the leader of the first medical mission under governmental auspices to visit the Soviet Union. As a member of numerous U.S. Public Health Service Committees during the years 1955-1962, he was in the thick of controversies over the licensing of the Salk and Sabin-type poliovirus vaccines and the subsequent problems associated with them. In this period he served as a senior statesman, helping to guide a national policy.

Many honors came to Paul over the years. Among these were the Medal of Freedom of the U.S. Government (1946), the Howard T. Ricketts Award of the University of Chicago (1954), and an Honorary D.Sc. from the same university (1956). He was elected to the National Academy of Sciences in 1945. In 1950 he was elected an honorary member of the Royal Society of Medicine, London, and was also a Fellow of the Royal College of Physicians and of the Royal Society of Health of Great Britain. The list of professional societies in which he was active is a long one, and he served as president of several: the American Society for Clinical Investigation (1938), the American Epidemiological Society (1950), and the Association of American Physicians (1956). In 1963 he was awarded the Kober Medal of the latter—an honor that he particularly cherished.

He was also a member of the Society of American Archeologists, the American Geographical Society, the American Academy of Arts and Sciences, and the Century Association of New York.

Dr. Paul retired from the chair of Epidemiology and Preventive Medicine in 1961. His research activities continued undiminished, however, for he immediately became the first Director of the World Health Organization Reference Serum Bank at Yale, one of three established by WHO in different countries. He led this laboratory through its early development and undertook among other projects a nationwide serologic survey of U.S. military recruits, measuring their immune status with respect to a variety of viral and bacterial diseases.

Infectious mononucleosis had been one of Paul's continuing interests since the chance discovery of the heterophile antibody test in 1931. Over the years unsuccessful attempts were made in his laboratory to isolate the etiologic agent, using materials collected from students at Yale, Smith, and the University of Connecticut. While the search was a frustrating one, the prospective serological study of the disease at Yale, which was undertaken by Niederman and Paul beginning in the late 1950s, eventually paid off handsomely. Thus when a technician in the laboratories of Werner and Gertrude Henle in Philadelphia developed infectious mononucleosis and was found to have acquired antibodies to the recently discovered Epstein-Barr virus (EBV) in the course of her illness, the Henles and their colleagues were quick to explore a possible etiologic role for EBV in infectious mononucleosis. They turned to the Yale Laboratory, and serologic tests on the collection of sera obtained from Yale students on entry to college, together with serial specimens from those who developed the disease during the ensuing four years, provided strong confirmatory evidence of the role of EBV in infectious mononucleosis. Although at this time he was not immediately involved in the work, Dr. Paul took great satisfaction in seeing his foresight rewarded and the potential of sero logical epidemiology as a research tool so productively exploited by his younger colleagues.

An episode in connection with the first publication of serological studies on the Yale students, which represented a collaboration between Niederman and McCollum at Yale and the Henles at the University of Pennsylvania, illustrates one strong aspect of John Paul's character—his uncompromising honesty. A reporter for the *New York Times* had published an article in the Sunday science section hailing the Yale authors and the Henles for the remarkable discovery that EBV is probably the etiologic agent of infectious mononucleosis. Dr. Paul was aghast at this misstatement and immediately sent a letter to the editor (published the following Sunday) pointing out that it was the Henles and their colleague Diehl who were responsible for the discovery, not the Yale group, whose contribution was merely confirmatory. The editor was so astonished that he telephoned Dr. Paul to tell him that never before in his long experience had he received such a letter denying credit for an accomplishment.

Chronic heart disease imposed the necessity for less and less physical activity, and in 1966 Dr. Paul retired from the directorship of the WHO Serum Bank. The increasing discomfort and restrictions due to his cardiac problems never elicited a complaint from him, however. He suffered no decline in intellectual vigor, and with typical determination and fortitude he spent the last five years of his life in a concentrated period of writing. He accepted an appointment of the Department of History of Science and Medicine and set about preparing three books and a number of papers on historical subjects. His most important contribution during this period was his magnificent book, *A History of Poliomyelitis,* published by the Yale University Press in 1971—a few months before his death. This has been widely acclaimed as a classic in medical history: it not only tells the story of a dramatic disease, but also reveals the way scientific discoveries are actually made—how backward steps as well as forward ones are part of the slow process and how the personalities and foibles of scientists inevitably affect the course of events.

Published after his death was "An Account of The American Epidemiological Society" and *Serological Epidemiology,* which he edited with Dr. Colin White. He also wrote histories of several Commissions for the Archives of the Armed Forces Epidemiologic Board (AFEB). These included *A History of the Virus and Rickettsial* (*V & R*) *Commission, A History of the Early Years of the Virus Infections Commission,* and *Addendum to the History of the Neurotropic Virus Commission,* which completed his earlier account written shortly after World War II, when military censorship was still in effect. In addition he left several incomplete manuscripts, including one on influenza in World War I.

Throughout his seventy-eight years, Paul was a person of enormous energy, imagination, and inner strength. These traits were concealed in a gentle exterior that often belied the toughness beneath. He had the essential qualities of a leader: all who came in contact with him respected him and recognized his integrity, judgment, and wisdom. Despite his eminence and many honors, he retained a certain shyness; modesty and humility were strong in his character. His professional manner was rather formal and reserved, and it was not easy to get to know him well. But with friends he was a wonderful companion—lively, warm hearted, and possessed of a delightful sense of humor, understated humor. He was a stimulating conversationalist and took immense pleasure in good talk in the company of friends over dinner and a bottle of his favorite Mosel wine, of which he was a connoisseur. He had a particular gift for friendship and kept up with a host of devoted friends around the world who had entered his life beginning in his school days.

Paul travelled widely over the years; an unquenchable wanderlust was one of his main characteristics. His foreign travels began early, when as a medical student he spent a summer working at the Grenfell Mission in Labrador. Curiosity about the behavior of disease in different geographic areas, cultures, macro-, and microclimates took him to many parts of the globe. Besides his early expedition to Peru with W. G. McCallum and assignment during World War II in North Africa and the Middle East, his later research activities involved fieldwork in Costa Rica, Cuba, Puerto Rico, Iceland, Germany, Czechoslovakia, Morocco, Israel, Korea, and Japan. Whatever the country, his interest in the culture and art of the people—and always the birdlife—led him to devote as much time as possible to exploring these aspects. He kept a diary during most of his life, and his war journals and various travel journals are filled with perceptive and witty observations on people and places. Not surprisingly, descriptions of birds and many skillful sketches of them dot the pages.

John Paul's life has been described by John Enders as a "splendid edifice."[*] It was indeed rich in all of its facets—in family, friends, career, and by no means least in the devoted companionship of his wife, Leita, during their forty-nine years of marriage. In summing up, one of his Yale colleagues has said: "Armed with a quiet zest and a pocketful of hobbies that stretched from Aves to Zunis, he was courteous and reserved on the surface, delightful and gracious beneath and with a generosity of spirit and adventurousness of mind few have commanded. There is an old pre-Socratic saying that 'Character is Man's Destiny.' Dr. Paul richly fulfilled that vision—as a physician and teacher, as a scientist, and most of all, as a person."[†]

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[*] J. F. Enders, "Book Review: A History of Poliomyelitis: Yale Studies in the History of Science and Medicine—6. By John R. Paul," *New England Journal of Medicine* 285 (1971):359.

[†] E. Atkins, "John Rodman Paul, M.D.," *Yale Medicine* (Fall/Winter 1971), p. 9.

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*KEY TO ABBREVIATIONS*

Am. J. Hyg. = American Journal of Hygiene
Am. J. Med. = American Journal of Medicine
Am. J. Med. Sci. = American Journal of the Medical Sciences
Am. J. Public Health = American Journal of Public Health
Ann. Intern. Med. = Annals of Internal Medicine
Arch. Pathol. = Archives of Pathology
Bull. Ayer Clin. Lab. = Bulletin of the Ayer Clinical Laboratory of the Pennsylvania Hospital
Bull. N.Y. Acad. Med. = Bulletin of the New York Academy of Medicine
Bull. U.S. Army Med. Dep. = Bulletin of the United States Army Medical Department
Bull. WHO = Bulletin of the World Health Organization
Conn. Med. = Connecticut Medicine
Conn. State Med. J. = Connecticut State Medical Journal
Int. Assoc. Med. Mus. Bull. = International Association of Medical Museums Bulletin
Johns Hopkins Hosp. Bull. = Johns Hopkins Hospital Bulletin
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Bacteriol. = Journal of Bacteriology
J. Biol. Chem. = Journal of Biological Chemistry
J. Clin. Invest. = Journal of Clinical Investigation
J. Exp. Med. = Journal of Experimental Medicine
Med. Clin. North Am. = Medical Clinics of North America
Milbank Mem. Fund Q. = Milbank Memorial Fund Quarterly
Newsl. Assoc. Teach. Prev. Med. = Newsletter of the Association of Teachers of Preventive Medicine
Pan Am. Health Organ. Sci. Publ. = Pan American Health Organization Scientific Publication
Proc. Pathol. Soc. Phila. = Proceedings of the Pathological Society of Philadelphia
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
R.I. Med. J. = Rhode Island Medical Journal
Trans. Am. Clin. Climatol. Assoc. = Transactions of the American Clinical and Climatological Association
Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians
Yale J. Biol. Med. = Yale Journal of Biology and Medicine

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WTPecora.

# William Thomas Pecora

February 1, 1913-July 19, 1972

By Charles A. Anderson

William Thomas Pecora was a stimulating and enthusiastic geologist, known as "Bill" to countless friends. He had an exciting career with the U.S. Geological Survey that culminated in his appointment by President Richard Nixon on April 20, 1971, to serve as Undersecretary of the Department of the Interior. In June of 1972, Pecora was hospitalized because of diverticulitis and was unable to survive postoperative complications leading to his death on July 19 of that year. Interior Secretary Rogers C. B. Morton paid high tribute to Bill Pecora, stating, "Our department—and the nation—has lost a singularly talented and energetic scientist and administrator. Few men possess the leadership qualities which Dr. Pecora showed in his quest for balance and harmony in resource development and conservation. Dr. Pecora understood conservation in the true sense. As a scientist, he recognized that Nature's forces are neither angry nor benign; they operate on laws and principles of matter, motion, physics, and chemistry. He felt strongly that man must not seek to subdue these forces, but understand them, work with them, and live in harmony with them."[*]

Bill Pecora was born in Belleville, New Jersey, the son of Cono and Anna (Amabile) Pecora. Both parents were born in southern Italy in the village of San Arsenio, near Paestrum, an

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[*] Eulogy at St. Patrick's Episcopal Church, July 21, 1972.

early Greek settlement, and the recorded Pecora lineage dates from about 1600 A.D. His parents were married in Newark, New Jersey, and lived in Belleville for a time until they moved back to Newark, where they lived until their deaths. There were ten children in Bill's family—four boys and six girls. Bill was number nine, and in his boyhood he was the unhappy "water boy," assigned to filling the twelve water glasses for the family. The Pecoras were an unusually devoted and close group, and family loyalty seemed to increase with the passage of time. Christmas was a "must" gathering, and one year Bill traveled from South America for thirty—six hours without sleep in order to be present for the Yuletide celebration.

Bill's two older brothers, Louis and Charles, and his eldest sister, Jean, carried on the family business of wholesale imports. His brother Sam, a physician, loved to discuss poetry and science with Bill. Surviving children of the Pecora family are five sisters: Mrs. Ellis Blackman, Mrs. Vincent J. Casale, and Mrs. Frank Immersi of Newark; Mrs. Gerard A. Riccardi, Juno, Florida; and Mrs. James J. Vasselli, Tequesta, Florida.

Bill attended the Abington Elementary School and Barringer High School in Newark, and was graduated from the latter in 1929. He was very active in extracurricular affairs in high school, serving as editor of the monthly school paper, "Acropolis," and of the yearbook, "Taps." He contributed an editorial to "Taps" that summarized the four years of student life at Barringer High School, with appreciable emphasis on military jargon in keeping with the title of the yearbook. Baseball, soccer, and fencing used up some of his energy, and he participated in the HiY Club, Latin Club, Italian Club, Thalians (acting), Forum (debating), and Science Club. In his spare time, he wrote the words for the song of the Class of 1929. Louise I. Capen (civics and history), Roger B. Saylor (math and science), and V. L. Sibilia (Italian) were teachers who took much personal interest in Bill and entertained him at their homes. As might be expected, he was valedictorian of his graduating class.

In 1929, Bill was awarded a Charles H. K. Halsey Scholarship that provided funds for a four-year scholarship of $1000 annually, to be used for tuition and living expenses at Princeton University. Three scholarships were awarded that year to candidates who were residents of New Jersey and had received their preparatory education at one of the public high schools in that state and who, in the opinion of university authorities, would be unable to obtain an education at Princeton without this financial assistance. An additional requirement was that each winner be of a different religious faith—one Protestant, one Jewish, and one Roman Catholic. Bill Pecora was the Roman Catholic selected.

At Princeton, Bill majored in geology and geological engineering, with an emphasis on hard rocks. He received the B.S.E. degree in 1933, and was graduated with honors. His senior thesis was, "The problem of the Susquehanna Complex with special attention to specific gravity variations," with A. F. Buddington, H. H. Hess, and Edward Sampson as advisors. During his undergraduate days, he found time to serve on the editorial board of the campus humor magazine, "Tiger," and was associate editor of two other campus publications, "Nassau Herald" and "Bric-a-Brac." He was a member of the Pistol Club and Rifle Club and was active in varsity fencing, serving as captain of the team in his senior year when he was Intercollegiate Fencing Champion. After graduation, he stayed on for two years at Princeton as a tutor in geology. In the summer of 1934, he was a field assistant to Erling Dorf, who was working in Montana on Paleozoic stratigraphy and on the Beartooth Butte Formation, as well as collecting ostracoderms and fossil plants.

Scholarship aid made it possible for Bill Pecora to start graduate studies at Harvard in 1935, where he concentrated largely in optical mineralogy and petrography under Esper S. Larsen, Jr. Bill became an assistant to Professor Larsen, which was considered an honor by the graduate students because of the legend that Esper Larsen's absentmindedness required an assistant who was very bright. Later Bill became an instructor in petrography. He was always regarded as one of "the Professor's boys," and Larsen encouraged Bill to select a thesis area in the Bearpaw Mountains, where he had a group of students working on igneous rocks. Bill received a grant from the Holden Fund to finance his fieldwork in 1937-1939 in the western fringe of the Bearpaw Mountains.

For his doctoral thesis, Bill made a petrologic study of the Boxelder laccolith and concluded that it differed structurally and petrologically from all the described laccoliths in the Highwood Mountains and that the syenitic rocks differentiated from shonkinite after the emplacement of shonkinitic magma into the laccolithic horizon. The progression of differentiation was from plagioclase shonkinite to plagioclase syenite to sanidine syenite. In addition, Bill studied the unusual nephelinebearing pegmatites in the Rocky Boy stock, located about twenty miles southeast of the Boxelder laccolith, concluding that these rocks resemble the unusual pegmatites of the Kola Peninsula in Russia. He also described drusy vugs in a monzonite dike that contained albite tablets associated with prehnite and smaller masses of calcite. Bill was awarded the Ph.D. degree by Harvard in 1940.

During the summer of 1936, Bill was in Germany as a member of the United States Olympic fencing team, and during his later student days at Harvard he continued this activity. In one demonstration, Bill and his partner fenced on roller skates. He was very gregarious and had another social life completely outside the Harvard geology department. To the envy of his friends, Bill liked to brag about the great Italian meals he had enjoyed in South Boston.

Foster Hewett, Chief of the Metals Section of the U.S. Geological Survey, was a frequent visitor to the geology department at Harvard before the outbreak of World War II, and because of the glowing recommendations from the department, he hired Bill Pecora to participate in the Strategic Minerals Program. Bill's first assignment in 1940 was the study of nickeliferous deposits in the western states; the assignment was later expanded to include similar deposits in Brazil. Bill found that the richer deposits of nickel were the result of long weathering of pyroxenite or peridotite during a complex physiographic history and that serpentinite was not a favorable rock for the residual accumulation of nickel. Garnierite in the nickel-silicate deposit near Riddle, Oregon, had three modes of occurrence, reflecting an orderly variation in color, specific gravity, and nickel content, which serve as useful guides for economic geologists.

Mica was an important strategic mineral during World War II, and in 1943 Bill was assigned to southeastern Brazil to participate in and direct investigations of the mica-bearing pegmatites. He and his associates were able to unravel the complexities of the structural controls, which led to the discovery of important mica deposits needed for the war program. Bulletin reports were published by the Brazilian government and by the U.S. Geological Survey. In addition, Bill spent some time in Colombia, Venezuela, and Brazil investigating quartz crystals needed for radio oscillators during World War II.

Bill had a sharp eye and an affection for minerals, and the pegmatites in Brazil were excellent collecting sites for choice specimens. In 1946 he returned to Washington, D.C., with a number of semiprecious gems that he enjoyed displaying to his numerous friends. Phosphate minerals are common in the Brazilian pegmatites, and Bill collaborated with several of his colleagues in mineralogical studies of his specimens that resulted in a series of papers describing new minerals. Later in Montana, he found two new carbonate minerals, which he named after his friends, Wilbur Burbank and Frank Calkins. Bill described whewellite, a calcium oxalate monohydrate from Montana, the mineral's first known locality in North America. As a result of considerable laboratory work, Bill and his colleagues described nine new mineral species from his mineral collections.

In 1949, he started his large-scale geologic mapping program of eight fifteen-minute quadrangles in the Bearpaw Mountains in Montana. He became a very effective recruiting officer at universities and colleges and selected many promising students as field assistants. Ten of these are named as co-authors on the published maps; seven remained with the U.S. Geological Survey and now have responsible research assignments. The first four geologic maps were published in 1957 on a planimetric base as Miscellaneous Geologic Investigation Maps and the other four were published in bulletins in multicolor on topographic base maps, starting in 1960 and ending in 1963. Bill was very proud of the accomplishments of this mapping project of the Cenozoic alkalic igneous rocks.

In 1956, Bill Pecora published an outstanding review paper on carbonatites, which are essentially carbonate-silicate rocks containing a great variety of minerals. He concluded that the carbonatites were deposited by solutions ranging widely in temperature, pressure, and concentrations that were derived from alkalic magmas during silicate crystallization. He also concluded that a carbonate magma in the normal sense is less likely to exist than carbonate-rich solutions, which at elevated temperatures and pressures can contain higher concentrations of dissolved ingredients than are normally in hydrothermal solutions. At many localities, the carbonatites occur as veinlike or dikelike bodies or as cores in volcanic plugs of alkalic rocks. Demonstrating his awareness of the importance of mineral resources, he emphasized that the carbonatites and related alkalic rocks contain an impressive reserve of rare commodities—nio bium, titanium, zirconium, rare earths, and uranium—as well as the common commodities—barium, strontium, magnetite, phosphate, and vermiculite.

In a later paper (1962), Bill concentrated on the carbonatite problem in the Bearpaw Mountains, where a composite alkalic stock contains a volcanic neck of subsilicic porphyritic cancrinite syenite, partly altered to sericite, calcite, sulfide minerals, barite, and zeolites. Subsequent fracturing was followed by introduction of pegmatites and veins, composed of K-feldspar, biotite, calcite, pyrrhotite, pyrite, uranium-rich pyrochlore, rare-earth carbonates, barite, and ilmenite, a suite of minerals that is typical of carbonatites, having considerable economic potential. These differing mineral assemblages pose some problems in geochemical history, but the evidence indicates that early magmatic crystallization of silicates yielded an aqueous residual fluid that became progressively enriched in $CO_2$, S, F, P, Ca, Fe, and Mg.

Geochemistry has long been one of the fields of major interest in the Geological Survey, and during the mid-1950s there was a growing recognition of its increased importance in the Survey program. In 1957, Bill was selected to be Chief of the Branch of Geochemistry and Petrology because of his background in field geology, mineralogy, petrology, and geochemistry. He effectively recruited talented youngsters to appreciably strengthen the research capability in these joint disciplines. Many of these early recruits are still with the Survey, whereas others were attracted to university positions that they now occupy with distinction. Bill strengthened activities he believed were undernourished, establishing strong programs in geochronology, experimental petrology, and mineralogy. As Branch Chief, he played an important catalytic role when natural coesite was discovered and its significance as an indicator of meteorite impact on both the earth and the moon was pointed out. In 1961, Bill returned to his former status of Re search Geologist, and during this interval he wrote an important paper on the carbonatite problem in the Bearpaw Mountains.

Bill Pecora was named Chief Geologist in 1964, and a year later was appointed Director of the Survey by President Lyndon B. Johnson. As Director, he pressed vigorously for programs that would be responsive to current and emerging national problems, such as the accelerated investigations of gold resources when national stocks were being depleted at an alarming rate and investigations for offshore development of oil and gas in anticipation of the impending energy dilemma. In response to problems revealed by the Alaska earthquake of 1964, he obtained approval to establish the National Center of Earthquake Research in Menlo Park, California, now an important center for earthquake studies.

The Santa Barbara oil spill on January 28, 1969, was the first major challenge to face Bill Pecora as Director, and he moved forcefully into action with the firm conviction that the fundamental facts must be determined in order to reach a rational decision. Many of these facts are available to the public in Geological Survey Professional Paper 679, published promptly under his direction in 1969. An account of Bill Pecora and his activities as Director at that time is given below.

"One old timer among the newcomers in Washington was William Pecora, Director of the U.S. Geological Survey. Pecora had 30 years of distinguished service with the U.S.G.S. to his credit, and was held in high esteem by his colleagues in science and government. For the recriminations associated with Santa Barbara, Pecora was the most senior scapegoat. Secretary Udall was no longer around to take the rap; and Secretary Hickel . . . could scarcely be blamed for the original blowout. But Pecora—Pecora was in command of the agency that was most intimately connected to the original leasing in the Santa Barbara Channel and the subsequent management of operations there. When appointed as director in 1965, he had taken over a staid and scientifically respectable agency with a history of conservatism. . . . Pecora, a former Olympic athlete, liked action and the personal engagement of Washington politics. His activity in policy matters and in defense of U.S.G.S. programs brought his agency into the rough political world. . . . True, his biography does lean to degrees, scientific societies, research honors, and membership in national academies, but, good poker player that he is, he took bad deals well, and his appointment in 1971 as Undersecretary of the Interior was helped more than hurt by his performance in Santa Barbara."*

The discovery of large reserves of oil and gas in 1968 on the north coast of Alaska led to Bill's second major challenge. A large part of Alaska is underlain by permafrost, which would melt beneath the pipeline proposed to transport hot oil to southern Alaska, where harbors are available for tankers in an area of major earthquake faults. Again, Bill insisted that all of the geological factors involved should be assembled and analyzed before making decisions, and the Geological Survey made a careful study of the geological aspects of the proposed pipeline route. Bill arranged for the mechanism for discussions between industry and government agencies that led to the preparation of environmental and technical stipulations for the construction of the trans-Alaska pipeline. These are now a part of the permit and, if properly monitored, will provide acceptable protection of Alaska's unique environment and will also provide access to the area's vast resources of petroleum.

Bill recognized that conventional methods of gathering resources and environmental data were not providing information as rapidly as our expanding population and economy required. He believed that aircraft and spacecraft might gather important data quickly, effectively, and economically; thus, he actively

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* From Carol E. and John S. Steinhart, *Blowout: A Case Study of the Santa Barbara Oil Spill* (Belmont, Calif.: Wadsworth Publishing Co., Inc., 1972), p. 12; reprinted by permission of the publisher, Duxbury Press, North Scituate, Mass.

supported research in photogeology and remote sensing. As a result of this research, new aerial methods were developed for finding fresh water, surveying volcanoes, and assessing the environment. In 1964, he formed an association with the National Aeronautics and Space Administration (NASA) that enabled the Geological Survey to accelerate its remote sensing research and extend it to include analyses of the potential values of surveying the earth from space. Results of these spacecraft studies led Bill to recommend that the Secretary of the Interior establish a departmental program to survey the Earth from space and to extend the research in remote sensing to meet needs of all bureaus of the department. This recommendation led to the establishment of the Earth Resources Observation Systems (EROS) Program, which was announced on September 21, 1966. Bill directed that performance specifications be developed to provide "remote sensing data of maximum usefulness to the maximum number of scientists and technicians throughout the world."

The performance specifications for the space survey, coordinated and refined in cooperation with the Department of Agriculture, were delivered to NASA on October 21, 1966, for implementation; and the first Earth Resources Technology Satellite (ERTS-1) was launched on July 23, 1972, just three days after Bill's death. Without the vision and drive of Pecora, there might never have been an EROS Program.

Although the announcement of the EROS Program produced varied reactions, Bill strongly emphasized the need and soundness of the program, and on December 10, 1974, the *New York Times* quoted Dr. James C. Fletcher, Administrator of NASA, as saying, "If I had to pick one spacecraft, one space age development, to help the world, I would pick ERTS and the operational satellites which I believe will be evolved from it, later in this decade." Bill placed Earth in a new perspective.

Directors of federal organizations make annual pilgrimages to Congress to defend their requests for funds, and Bill Pecora was superb in his meetings with the appropriation committees, always presenting his requests in an articulate and friendly manner. Because of his excellent memory for detail, he could answer pertinent questions from the members of the committees clearly and forcefully and with a delightful sense of humor if the situation was appropriate. He always left the members of the committees well aware of the importance of the USGS program and its benefits to the taxpayer.

During the period Bill Pecora served as Director of the Geological Survey and Undersecretary of the Department of Interior, he became increasingly involved with the geological aspects of problems concerning our environment. In his Horace M. Albright conservation lecture, given at Berkeley on January 18, 1972, he emphasized "inadequate data persist in the arena of assessment and decision making. Consumers demand a continuing supply of energy and resource products on one hand, and demand maximum pollution protection on the other. . . . Geologic science demonstrates that nature is a massive polluter of the environment. In comparison, man's activity is of little consequence on a planetary scale in some issues, but may be of serious consequence in a local context. . . . Science and research are needed more than ever to provide guidance to courses of national action aimed at fulfilling human needs. As the most intelligent species on earth, man can certainly provide for himself and yet prudently protect the total ecosystem from unnecessary and unacceptable degradation."

A Committee on Geological Sciences of the National Research Council sponsored a study, "The Earth and Human Affairs," and Bill Pecora met informally with this group on several occasions, inspiring the members with his enthusiasm for the job at hand. In appreciation, the published report[*]

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[*] National Academy of Sciences, *The Earth and Human Affairs* (San Francisco: Canfield Press, 1972).

was dedicated "To the memory of William T. Pecora, an early. effective, and enthusiastic advocate of 'the geologic perspective'."

Bill Pecora made many contributions to the official and nonofficial activities of the employees of the Geological Survey. During the period from 1947 to 1967, he was an effective member of the U.S. Civil Service Commission's Board of Examiners for Geology, concerned with the development and maintenance of high standards in the selection of geologists for federal employment. He was always a live wire at the various scientific meetings of geologists held in Washington, D.C., where he could be counted upon to probe the strengths and weaknesses of the speakers' presentations and stories. For many years, he was an active participant in the Survey's Pick and Hammer shows, which were presented primarily to poke fun at the Survey brass. Some of his particularly hilarious performances are still vividly remembered by the cast and audience. Shortly after becoming Director, lie was a speaker at a conference including two eminent science advisors and the President of the National Academy of Sciences. When Bill was introduced, he began by addressing these gentlemen as "Your Highness," "Your Excellency," and "Your Worship"; the character of the meeting was somewhat modified after that greeting.

Bill Pecora was the recipient of many honors during his professional career. Among these that deserve mention are the presidencies of the Geological Society of Washington (1964) and of the Cosmos Club (Washington, D.C., 1968). He was a Fellow and Councilor of the Geological Society of America and of the Mineralogical Society of America. He was elected Honorary Member of the Rocky Mountain Association of Geologists. He received the Doctor of Science degree from Franklin and Marshall College (1969) and Doctor of Engineering degree from the Colorado School of Mines (1970). The American Association of Petroleum Geologists presented him with its Public

Service Award (1972). He received the Distinguished Service Award of the Department of the Interior (1968) and the Rockefeller Public Service Award (1969). He was a Foreign Member of the Brazilian Academy of Sciences. The American Philosophical Society elected him to membership (1970) and he was a Fellow of the American Academy of Arts and Sciences (1965). He was elected to the National Academy of Sciences in 1965.

On April 7, 1947, William T. Pecora married Ethelwyn Elizabeth Carter from Franklin County, Kentucky. They had two children, William Carter, born in 1949, and Ann Stewart, born in 1953. The Pecora family lived in a house near the Potomac River that was designed by an architect friend. After becoming a homeowner, Bill developed into an enthusiastic gardener, which helped him to forget temporarily troublesome problems left in the office. He was particularly enthusiastic about C*amellia sasanqua* and he planted many of the shrubs in his garden. Bill and Ethelwyn enjoyed entertaining guests in their lovely home, where Bill presided over the carving of the roast and extolled the virtues of the red wine that he had just discovered in one of the many liquor stores in Washington, D.C.

In 1973, a 6000-foot ridge in the Bearpaw Mountains was named Pecora Ridge in honor of Bill. The ridge extends southwest about two miles from the 6916-foot top of Baldy Mountain and forms the divide between Eagle Creek and the headwaters of Birch and Little Birch creeks. In the summer of 1973, William Carter Pecora carried the ashes of his father and those of Esper S. Larsen 3d, Bill's close friend since Harvard days, to the top of Baldy Mountain. This was an appropriate mission as Bill dearly loved the Bearpaw Mountains.

On July 21, 1972, at the auditorium of the Department of the Interior, U.S. Geological Survey Director V. E. McKelvey delivered a eulogy for Bill: "It is hard for any of us here to believe that Bill Pecora—our loved one, friend, colleague, leader—is no longer with us. We have suffered a loss that we are only beginning to fathom but it is a loss we know we cannot replace. There will be no replacement for Bill's smile and hearty chuckle, his intellect, his uncanny ability to perceive the solution to seemingly unsolvable problems, his cool in the midst of crisis, his verve in approaching a difficult situation, his contagious excitement over a new idea, his ability to inspire, to bring out our best efforts, to lead us to accomplishments that we never dreamed were possible. We know too that our loss as loved ones, friends, and colleagues is not ours alone. It is the Nation's loss and the world's, for Bill Pecora's talents were being effectively applied to the solution of problems of great national and international significance."

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*KEY TO ABBREVIATIONS*

Am. Assoc. Pet. Geol. Bull. = American Association of Petroleum Geologists Bulletin

Am. Mineral. = American Mineralogist

Braz. Div. Fom. Prod. Miner. Avulso = Brazil, Divisão de Fomento da Producção Mineral Avulso

Geol. Soc. Am. Bull. = Geological Society of America Bulletin

Min. Congr. J. = Mining Congress Journal

U.S. Geol. Surv. Bull. = U.S. Geological Survey Bulletin

U.S. Geol. Surv. Misc. Geol. Invest. Map = U.S. Geological Survey Miscellaneous Geologic Investigations Map

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Jack Schultz

# Jack Schultz

May 7, 1904-April 29, 1971

By Thomas F. Anderson

Jack Schultz was one of the last graduate students to get his degree in classical *Drosophila* genetics under Thomas Hunt Morgan. This was in 1929. For the rest of his life, Jack's goal was to understand, in molecular terms, how a set of genes could direct and control the development of an organism; and he lived to see the results of his pioneering research become the foundation on which such an understanding could be based.

Jack Schultz was born in Astoria, Long Island, New York, on May 7, 1904, the eldest of three sons of Morris and Bessie (Krones) Schultz. Both parents had been members of large Russian-Jewish families of rabbis, farmers, and tradesmen living in two small villages in the area of Minsk, near the Polish border. During the political difficulties of the 1880s and early 1890s in Russia, the young adults of both the Schultz and Krones families tended to be ardent socialists and became involved in revolutionary activities. Some perished in the abortive revolution; others were forced to flee the country. Thus, around 1896, Morris Schultz cut short his training at trade school and came to New York. Bessie Krones also had attended a socialist worker's school and, in addition, helped her family make ends meet by running a knitting machine at home. About 1897 she, too, left her village in Russia to come to New York. Somewhat later Morris Schultz was able to help several members of their families join them. Other members stayed in Russia until World War II, when a few managed to evade death at the hands of the Nazis by escaping to Argentina.

Jack's parents met soon after they arrived in New York and were married seven years later. Morris Schultz was a gentle soul and a great reader who had liberal, but not extreme, views. Besides being associated with the group that founded the Jewish daily newspaper, *Forward,* he soon became engaged in various businesses, mostly in establishing and operating neighborhood grocery stores in various industrial communities in the New York City vicinity: west New York, Astoria, Passaic, Clifton, and finally, Long Beach. He was always closely and sympathetically involved with the factory workers to whom he supplied food.

Immediately after arriving in this country, Jack's mother found work in a knitwear factory and presently became forelady there. She was an energetic and vivacious, kind, yet demanding woman, with a strong influence over the activities of those around her. With a reputation as an excellent cook of traditional dishes, she was devoted to her home and to a large circle of relatives.

Jack had two younger brothers, Mortimer, who was born in 1907, and Charles, born in 1912. Their parents were ambitious for all three children and were determined to give them cultural advantages that they themselves had been denied in Russia. The children attended public schools and received, in addition, some religious training in the Jewish schools of their communities. Although their father found it difficult, he did manage to send his sons to college and to help with the financing of their graduate studies. Mortimer became a high school teacher of science in Long Beach, New York, and served as chairman of the science department there for many years before he retired. Charles, who has literary interests, has been on the advertising staff of *Women's Wear Daily* for many years.

Jack was a studious child, an omnivorous reader at a very early age, as indeed he was throughout his life. He was rapidly promoted through grade school, and was therefore much younger and smaller than his classmates. This, no doubt, accounted at least partially for his not being at all interested in athletic activities and for his not being entirely accepted socially by his fellow students. The partial isolation that he experienced served only to reinforce his intellectual interests. His teachers and older relatives considered him a brilliant and independent, but highly impractical, boy. During these years Jack took violin lessons and frequently accompanied his mother on her Saturday afternoon expeditions to the Metropolitan Opera House. Later he took up the flute, which he played occasionally, with great pleasure, for many years. At Clifton High School he became deeply interested in the humanities; this interest was stimulated in large part by his teacher of French, Miss Mary Smith. She discovered Jack's unusual facility with languages and encouraged him to explore the best in the literature of all the languages he had the opportunity to learn. Later in life this proficiency was to help him cement close friendships with people of many nationalities.

During his first two years at Columbia University, he concentrated on the humanities. He entered eagerly into the stimulating atmosphere of undergraduate life in New York City in the early 1920s, when there was a sudden surge of excitement in the theater, painting, literature, social philosophy, and music. This was an important stage in his development, for Jack retained an active interest in the humanities throughout the rest of his life.

It was only in his junior year that Jack first became seriously interested in making plans for a definite career. He had decided to take a premedical course when he was suddenly, and happily, diverted. Like most young men of those times, he needed extra money for books, concert tickets, and the like; so he answered a bulletin board ad for someone to wash bottles and make fly food in the laboratory of Professor Thomas Hunt Morgan. Thus, as luck would have it, he was drawn to the celebrated "Fly Room" in the department of zoology. Morgan soon recognized him as a promising young man, and Jack responded by rapidly learning the revolutionary genetic principles that were being established by Morgan and his group. Calvin B. Bridges and Alfred H. Sturtevant were especially kind in introducing him to the new genetic theories and techniques. Their experiments and thinking, more perhaps than those of anyone else, influenced Schultz's later scientific work. Even as a novice, Jack not only listened to what was going on in the Fly Room but was soon tentatively contributing some small points of his own to the discussions. In the course of a few years, he was named a University Scholar, a Teaching Assistant in Zoology, and a National Research Council Fellow. More important, he enrolled as a graduate student under the guidance of T. H. Morgan, the world's foremost geneticist, and of Edmund B. Wilson, the most outstanding cytologist in the United States.

Jack received his bachelor's degree in 1924, his master's in 1925, and his doctorate in 1929. His Ph.D. thesis showed that the large *"Minute"* class of mutations in *Drosophila* all produced nearly identical somatic effects, and yet occurred at many different loci. Evidently a large number of independent mutations could lead to similar phenotypic effects on development. Although many theories have been advanced to explain them, the molecular mechanism for these effects remains a mystery to this day.

In his thesis Jack acknowledged the many kindnesses of Morgan, Bridges, and Sturtevant. He also thanked two visitors to the laboratory: Dr. Curt Stern and Dr. Helen Redfield. Stern was a postdoctoral student from the Kaiser Wilhelm Institute in Berlin, who had come to Columbia as a Fellow of the International Education Board. Stern himself had been concerned with the mosaic, normal-appearing patches in Minute flies that occurred when somatic crossing-over eliminated the Minute region of a chromosome. Jack and Curt both enjoyed and profited from the close scientific relationship that was established at Columbia and that lasted throughout the rest of Jack's life.

Helen Redfield had, as an undergraduate at Rice Institute, been introduced to the new developments in genetics by Hermann J. Muller and Edgar Altenburg. Although she was at that time acting as Assistant in Mathematics, she was fascinated by the *Drosophila* work and was encouraged to carry out simple experiments using the sex-linked lethal genes then being extensively studied by Muller and Altenburg. For graduate work she went to the University of California at Berkeley, where in the department of zoology she served as Teaching Fellow and received her Ph.D. under the direction of Samuel J. Holmes. She came to Morgan's laboratory at Columbia in 1925 as a Fellow of the National Research Council. Here, in addition to completing her study of the maternal inheritance of a sex-limited lethal effect, she embarked on new studies on crossing-over in triploids. There soon developed among the younger members of the group at the Columbia laboratory an unusually rich and rewarding comradeship; and, since Jack's and Helen's interests were so closely related, they became special allies in the discussions and activities. Presently they began the collaborative work on interchromosomal effects on crossing-over that was to continue for many years. They were married in 1926.

At Columbia Jack was also closely associated with Selig Hecht and his newly formed group studying the biophysics of vision. Like Hecht, Jack was convinced of the then revolutionary, but now almost axiomatic, idea that biological problems could be understood in chemical or physical terms only if appropriate physico-chemical methods were used to study them.

This no doubt led to his early analyses of the absorption spectra of the eye pigments of various mutant stocks of *Drosophila.* This work suggested that the pigments were metabolically related and anticipated the hypothesis by Boris Ephrussi and George W. Beadle (1935), and by Beadle and Edward L. Tatum (1941), that each enzyme in a metabolic pathway is the product of a specific gene acting on a specific substrate. Thus, if a mutant gene fails to make an effective enzyme, the substrate of that enzyme (an intermediate pigment in the *Drosophila* case) might accumulate in the tissue involved (to give the eye its mutant color, for example).

In 1928 Jack and Helen moved to Pasadena where Morgan and his group were setting up a new laboratory at the California Institute of Technology, under the auspices of the Carnegie Institution of Washington. The two Schultz children, Peter and Jill, were born in Pasadena and spent most of their early childhood there. At this stage Helen preferred to devote only part of her time working at the laboratory on interchromosomal effects. The results of her experiments may be briefly summarized: In the first place, there were demonstrated, in structural homozygotes with normal sequence, hitherto unsuspected positive and negative correlations of crossing-over in given regions of nonhomologous chromosomes. Some of the positive correlations were believed to be the result of response to common environmental factors, such as the persistence of the polarized pattern of pairing of chromosomes seen at meiosis. However, other correlations, and the negative correlations in particular, gave evidence of a real interchromosomal influence. Extensive data were gathered on the facilitation of crossing-over by heterologous inversions (a phenomenon previously noted by C. B. Bridges and others) with special attention to the effects of combined inversions on interference and to differential responses of different regions to different inversions. The heterochromatic regions were shown to be important in these inter chromosomal effects. Also, the regional differences found between triploids and diploids, both in structurally homozygous and in structurally heterozygous types, indicated that the effects of triploids on crossing-over are to be considered an example of the phenomenon of interchromosomal influence.

At Cal Tech Jack attacked many problems in *Drosophila* genetics, collaborating with Sturtevant, Bridges, Theodosius Dobzhansky, David G. Catcheside, and others in showing how genes control development and how their effects are modified by suppressor genes. With Morgan and Bridges (and after Bridges' death, with Viola Curry) he co-authored eleven of the group's annual reports to the Carnegie Institution under the title, "Constitution of the germinal material in relation to heredity." Today such a title would imply that the group was reporting results in molecular biology, but the reports actually concerned the classical genetics of *Drosophila:* descriptions of new mutants, dose effects in sex determination, position effects, the elaboration of salivary gland chromosome maps, effects of X rays on crossing-over, etc. Jack contributed his share to the papers that resulted from this work and even participated in a study with Albert Tyler on the reversibility of fertilization of *Urechis* eggs. There is a legend that it took only two weeks for Jack to teach J. B. S. Haldane the basic genetics of *Drosophila.*

But Jack was primarily interested in the molecular biology implied by the impressive title of the Carnegie reports: he wanted to know the *chemical* constitution of the genetic material and how it functioned not only in heredity, but how it functioned to produce the phenotype. It had become obvious that chromosomes contained the genes, but their chemical composition and chemical role in directing development of the phenotype were not amenable to study by the classical staining methods available in Morgan's laboratory.

Fortunately, at that time Torbjörn O. Caspersson in Stockholm was developing methods for the microspectrophotometry of cells in the ultraviolet end of the spectrum; Jack, with his training in both genetics and biophysics, was the ideal person to use these methods in following the metabolism of the nucleic acids in cells. So in 1937, under the auspices of the Rockefeller Foundation, he went to work with Caspersson. Jack and his family spent two very pleasant and stimulating years with their new Swedish friends in Stockholm; those with whom they formed especially warm and long-lasting friendships included the Casperssons, the Gert Bonniers, and the John Runnströms.

In the laboratory, Jack and Caspersson soon showed that there is indeed a relation between the metabolism of the two kinds of nucleic acids: The nucleolus was found to contain large amounts of pentose nucleic acid, whereas the chromosomes themselves largely contain deoxypentose nucleic acid. Moreover, the observation that the cytoplasm of rapidly dividing cells is rich in pentose nucleic acid as compared to resting cells gave them a glimmer of current thought as to the mechanism of gene action: mRNA synthesis, ribosome synthesis, tRNA synthesis, and protein synthesis. Jack's review of this and of other work, published as early as 1941, "The evidence of the nucleoprotein nature of the gene," concludes, "At the present time the properties of the genes and of nucleoprotein metabolism are evidently parallel: specificity, self reproduction, relations to synthesis and distribution of nucleoproteins in the cell, all are what they should be were the genes nucleoproteins. It would seem therefore, that our present task is to develop the physiology of the nucleoproteins into an effective physiology of the genes."

When one also recalls that in 1932 Jack had been involved with Bridges in the discovery of specific suppressor genes, one can appreciate his reaction to current concepts of how gene action is controlled by interactions among operons, repressors, and inducers: It was one of *déjà vu.* As an explorer, Jack had long before sketched that intellectual territory, but its scientific relevance became generally accepted only years later after other molecular biologists had settled into it and painstakingly harvested specific chemical evidence for its reality.

When World War II broke out, Helen and the children sailed for the United States, but Jack tarried in Edinburgh long enough to attend the International Genetics Congress and barely managed to catch one of the last civilian boats home.

Back in Pasadena, he wanted to continue the work he and Caspersson had begun. But, as George Rudkin tells us, "The years following [Jack's] return from Sweden were marked by the search for financial support for the acquisition and operation of the complicated, expensive instrumentation of the type developed in Stockholm, this in an era when grants were rare and in a laboratory where simple equipment was stressed. The period (1939-1943) ended with a harried year spent partly as visiting professor with Lewis John Stadler, at Missouri, working on variegation in corn, partly at Cal Tech and partly at Woods Hole, not far from the Marine Laboratory of the Philadelphia based Lankenau Hospital Research Institute at North Truro, Massachusetts. That same period saw the completion of a new review, this time from a chemical point of view, 'The Gene as a Chemical Unit,' much of which is still illuminating thirty years later. In 1943, he joined the Lankenau organization, then under the directorship of Dr. Stanley P. Reimann.

"Reimann was in the process of building up The Institute for Cancer Research (as it was later called), which was dedicated to the proposition that logical solution(s) to the cancer problem would come from an understanding of the basic mechanisms underlying development and growth. Jack enthusiastically brought his expertise in genetics, embryology, and physiology to this new enterprise. He became immersed in trying to understand the many facets of cancer and, as one of the few geneticists in the field, wrote a number of classical reviews on the subject."

Jack and Helen were warmly welcomed to the Philadelphia scene by members of the faculty of the University of Pennsylvania, particularly by Charles W. Metz and his associates. The Schultz family lived first in the village of Ithan, on the Main Line west of Philadelphia. In 1949, when the Institute moved from its crowded and makeshift quarters at the old Lankenau Hospital on Girard Avenue, to its modern new building in Fox Chase, they moved to Elkins Park and finally settled in Huntingdon Valley, a suburb in the northeast, where they could happily do some gardening. Jack and Helen also spent many delightful summers at Cold Spring Harbor with such friends as Milislav Demerec, Theodosius Dobzhansky, Curt Stern, Ernst W. Caspari, Berwind P. Kaufmann, Alfred E. Mirsky, and Barbara McClintock.

In his own laboratory, Jack and his colleagues worked on projects in many areas. With human material easily available from the Lankenau Hospital, Dr. Reimann soon induced Jack to study human chromosomes. As a result, in 1946 he and Patricia St. Lawrence were able to map two of the chromosomes associated with the nucleolus in preparations of human pachytene chromosomes and to show that, like the pachytene chromosomes of other species, they have distinctive chromomere patterns. This area of research has been continued at The Institute for Cancer Research by Jack's former student, David Hungerford.

Another project involved devising minimal media for growing *Drosophila* cultures as a preparation for studying the one-gene-one-enzyme hypothesis in a higher organism. Although it was found that different stocks indeed had different requirements for such substances as tryptophan, the project was eventually dropped because of the difficulty in getting clear-cut nonleaky, nutritional mutants. In another, somewhat related project, Jack and Elizabeth K. Patterson followed the activities of such enzymes as the peptidases during the development of specific organs in *Drosophila.* Measurements of the nucleic acid contents of the eggs and larvae of various *Drosophila* stocks and species were made first with Leo Levenbook and then for many years with Elizabeth Travaglini. It was found that the eggs of different species contained remarkably different extrachromosomal DNA's.

Meanwhile, Jack had been accumulating sufficient funds to acquire the expensive microspectrophotometers necessary to make quantitative measurements of the nucleic acid contents of the bands in salivary gland chromosomes. Eventually he and George Rudkin were able to estimate the extent of polyteny of these chromosomes by comparing their DNA contents with those of chromosomes in other tissues. But the biggest harvest came in studies of giant chromosomes in *Rhynchosciara,* in which they discovered that certain bands increased their DNA content in the process of "puffing"—a stage in which the genes in these bands become active and synthesize messenger RNA. This was an important observation, for it dispelled the old dogma that each cell in a multicellular organism contains a set of genetic material that is identical to that contained in the diploid cells of the germ line. This paved the way for the discovery of disproportionate replication of other genes, particularly those involved in the production of ribosomal RNA. The work also showed that the compacted, or "heterochromatic," chromosome regions, with which Jack's name had been associated since his days at Cal Tech, are relatively inactive, whereas the extended or "euchromatic" regions are available for transcription into the RNA messages that direct protein synthesis.

The use of *Drosophila* for testing the activities of various carcinogens was also studied, and it was found that some of these agents produced interesting developmental abnormalities; however, the results were never published *in toto.* In fact, whereas Jack was willing and even eager to talk about his own work in private or public, he was most reluctant to sit down and write up the details for publication. But although he seemed to lose interest in writing what he had already done, an important element in this apparent neglect was his conviction that a scientific paper should be of real theoretical relevance and must not represent merely the stockpiling of data in yet another publication. Thus, in his own work, which was largely of a pioneering nature and therefore theoretically risky, he was apt to make new plans and to wait for the completion of critical experiments—those that would bring the stubborn data into satisfactory focus. The result was that many of the data were not published *in extenso.* It should be remembered also that during the years at the Institute, he was devoting much time and energy to the general development and coordination of basic research there. Much more time and energy was spent, for example, on meetings elsewhere dealing with grant applications or to the consideration and reviewing of such material as papers for publication (those by many outsiders as well as by Institute staff), and so on. His nature was such that he practically never refused a request; thus he was unable to protect himself, even when failing health demanded just that. And so it unfortunately happened that much of Jack's experimental work was brought to the attention of colleagues only through lectures and discussion or through abstracts in the proceedings of meetings he attended.

Jack exerted a profound influence on the Institute by wandering through its laboratories and engaging his colleagues in relaxed, stochastic discussions of science, music, literature, or the theater. These peripatetic discussions were extremely valuable. Not only was Jack active and perceptive in selecting and recruiting personnel, but it was his friendly, inquiring, and enthusiastic nature to delve into the biological projects of all his colleagues and to offer helpful advice and guidance freely. Jack's effectiveness in these endeavors was greatly facilitated by his astonishingly broad comprehension and competence, as well as by his charm and appreciative wit.

Sometimes he would suggest a definitive experiment that others would perform. For example, a basic problem in embryological differentiation had long been to determine whether embryonic nuclei become irreversibly differentiated as the cells themselves diversify. Jack suggested to Robert Briggs, who was then at the Institute, that one way to test this would be to provide enucleated eggs with nuclei obtained from embryos at progressive stages of development. If the egg then developed into a mature individual, this would mean that the donor nucleus had not been irreversibly differentiated at the stage in question; on the other hand, if the development of the egg stopped at an earlier stage, the nucleus it received presumably had been irreversibly changed. With Jack's encouragement, Briggs and Thomas J. King then developed methods for successfully transplanting nuclei and showed that frog eggs implanted with nuclei from a very early stage (blastula) develop more successfully than those provided with nuclei from later stages (gastrula and neurula).

A second productive project at the Institute was spurred on by Jack's resolve that one should thoroughly investigate the genetics of tumor cells and compare them with the genetics of the cells in the tissues from which they had originated. He recognized that the material of choice for such studies would be the free-living ascites forms of transplantable solid tumors; because individual ascites cells multiply rapidly, it should be feasible to study the chromosomes of dividing cells of these tumors by the standard squash and staining techniques with which he was so familiar. Furthermore, if means could be found to make cells of different types fuse with each other, one should be able to develop techniques for studying their genetics in much the same way that the genetics of bacteria had been so successfully pursued. Jack was thus led to invite George Klein to come to the Institute from Sweden to instruct him and Theodore S. Hauschka in working with ascites tumors. Shortly thereafter, the Swedish cytologist, Albert Levan, joined the group. They soon made the startling discovery that some tumor cells were like diploid normal cells in having two chromosomes of each kind, whereas others had four or more chromosomes of each kind. Moreover, they found that the higher the ploidy of a tumor, the greater the range of host strains of mice to which the tumor could be successfully transplanted. They concluded that increases in the number of chromosomes are somehow associated with alterations in the tumor's transplantation antigens in such a way that the tumor is less susceptible to rejection by an otherwise incompatible host.

Jack also realized that one could not develop a genetics of somatic cells unless one had a large number of well-defined mutations. It was well known that such mutations are difficult to induce in the readily available diploid cultures in which each cell contains two chromosomes of each kind. Jack suggested that the difficulty might be overcome by working with haploid cells, *i.e.,* cells that contain only one chromosome of each kind. For in such a haploid cell, one might expect that a mutation might be able to express itself phenotypically without having to overcome the influence of its wild type allele that would normally be present in a diploid cell. Jack therefore encouraged Jerome J. Freed to isolate haploid cell lines from parthenogenic frog embryos. Freed was later joined in this work by his wife, Iiselotte Mezger-Freed, and together they did indeed isolate such haploid lines. However, when they tested Jack's hypothesis, they found to their, as to any geneticist's, chagrin that haploid and diploid lines had nearly the same apparent mutation rates. Lisa favored an explanation that involved inherited units in the cell membrane. Needless to say, the search for an explanation of the Freeds's unexpected result is still going on.

Jack's mere presence at the Institute often led members of the staff into cooperative research with other scientists in the Philadelphia area. For example, when Peter Nowell at the

University of Pennsylvania recognized that the cultures of human leukemic cells he was developing could provide a convenient source for human chromosomes, his department head, Dr. Dale R. Coman, suggested that he contact Jack because of Jack's experience in studying human chromosomes. Jack, in turn, referred Nowell to his student, David A. Hungerford. The ensuing collaboration between Nowell and Hungerford was most fruitful; among other things, they discovered the so-called Philadelphia chromosome that is associated with chronic granulocytic leukemia.

One of the most congenial and productive people whom Jack helped to bring to the Institute was Beatrice Mintz. Jack had created an atmosphere in the Institute in which she felt free to undertake a major project—that of producing animals with cellular genetic markers chosen at will. The plan was to combine cells of two early mouse embryos of different genetic origins and then implant the composite into a foster mother to develop into a single mosaic or allophenic animal. Obviously, such a project was risky; even if the experiment worked, it might take many years of discouraging trial and error before a result could be obtained. And indeed, several years were required to develop the necessary techniques. Finally, in 1965, when the first allophenic mice with visible coat color markers were born, Bea could hardly wait to show them to Jack and Helen. When Jack saw the striped baby mice he exploded, "Wow!", with wonder and delight. It was immediately clear that he perceived the significance of the coat patterns on the young mice duplicated from one individual to another and the promise they held for unravelling the developmental order in any other tissue in which appropriate markers might be available.

Jack was very generous in giving of his time, especially to younger people. He never presumed to direct their work, yet often when one of them had a new idea or new data he would find himself intuitively knocking at Jack's door to share his discovery and benefit from Jack's broad knowledge and incisive scientific perceptions. Jack encouraged the student to develop an appreciation of, rather than a disparagement for, the complexity of biological systems. Whenever a manuscript came to him for review, as was often the case, he would praise what was valid in it and criticize the spurious parts without giving offense. He would insist that the style be clear and the meaning unambiguous. "Why don't you say exactly what you mean?" He had the ability to give most of his colleagues the feeling that they were special individuals, and he seemed to be a second father to many.

Jack asserted that he never in his whole life played a sport such as tennis, a card game such as bridge, or a table game such as chess. This is a slight exaggeration, for although he never seriously participated in such games, still he was known to have played tennis and chess with Helen and the children—albeit somewhat unenthusiastically. More to his taste were noncompetitive activities: gardening, swimming, tramping in many quiet countrysides and along isolated mountain trails, trips to exotic desert regions with the Dobzhanskys and others at Cal Tech or motorboating with the Casperssons in Sweden. He and Helen especially enjoyed canoeing among the Elizabeth Islands during those summers spent by Morgan's group at Woods Hole and, during all of their life together, exploring at leisure villages and cities encountered both in this country and abroad, in a serious attempt to arrive at some real understanding of the life of all kinds of people. On the other hand, Jack was interested in the publicized personalities of competitive sports and talked to his friends about them—not because he cared for the popular sporting events, but rather because the participants (both the active and the passive) remained to him psychological curiosities.

Jack was unusually fond of intellectual play with words, ideas, and attitudes. He was an expert at some of Eric Berne's nondestructive "games people play." Somewhat related to this, perhaps, was his great love for seminars, which for him took on the aspects of an anticipated fete or celebration. Listening to a good lecture, he would soak up the information as it was presented, and after the talk could usually be counted on to lead a lively discussion in which, without any apparent preparation, he would bring up a set of recondite facts and views that would broaden the audience's perspective and nicely wrap up the topic at hand.

Reimann's retirement as director in 1956 produced a crisis, for it appeared that the Institute's Board might appoint a nonscientist to run the affairs of the Institute and that basic research might be jeopardized. Some of the senior staff left the Institute, but Jack, together with the embryologist Tom King, the crystallographer A. Lindo Patterson, and the three biochemists Gerrit Toennies, Hugh J. Creech, and Sidney Weinhouse, stayed on to form a nucleus of scientists that held the remaining staff together. In 1957 the Board wisely appointed Timothy R. Talbot, Jr., a physician and hematologist, to be Director of the Institute. Jack was of great aid to the new Director in helping the Institute to recover and gain strength. As Talbot himself has written, "[Jack's] motive was to assure that the Institute would once again develop a growing diversity and excellence. There was literally no limit to the number of hours that Jack devoted to this process. No meeting with him was ever without some new input, some lucid presentation of biological phenomena, some new concept that we explored and debated, some warm perception of a colleague or commentary on the world in which we lived. He was always cutting through to the heart of the matter, and with exquisite graciousness. . . .

"While he was exerting his influence through and upon me, he was also in intimate contact with all of the Institute. There were few fields of science to which he could not direct his attention intelligently, and much of his time was devoted to the scientific and personal encouragement of the people around him. Jack's intellectual capabilities were awe-inspiring. He could read at a glance what most of us had to struggle through. He had almost total recall, but unlike many encyclopedic minds, his analytical digestion of all that he absorbed and his ability to synthesize new ideas were truly phenomenal. The good fortune for all of us is that these great abilities were housed in a framework of love and compassion and concern for society. In addition, he had a foundation in philosophy and art almost equal to that which he had in science."

In the spring of 1961, Jack accepted an invitation from the Department of Genetics and his old friend, Curt Stern, to spend a term as visiting professor at the University of California at Berkeley. His lecture, "New Ideas," given at the Third Annual Graduate Students' Genetics Colloquium, serves as a fitting memento of that visit, and it revealed to scientists Jack's talent as an essayist.

Besides his many activities at the Institute, Jack served on the editorial boards of the *Journal of Heredity, The American Naturalist, and Genetics.* He was elected Vice President (Zoology) of the American Association for the Advancement of Science in 1961, President of the Genetics Society of America in 1963, and President of the American Society of Naturalists in 1968. In his presidential addresses and posthumously published essays, we see a fusion of scientific and literary erudition that reveals Jack's understanding of, and sympathy for, the human condition. He was elected to the National Academy of Sciences in 1969, a few weeks before his sixty-fifth birthday and his mandatory retirement.

As Senior Member Emeritus, Jack felt himself partially released from his unofficial stewardship of biological affairs at the Institute and thus more free to devote his enthusiasm and energies to his own research. With Kenneth Tartof, he worked on gene amplification in bobbed stocks of *Drosophila;* and with

Francis Ashton, he continued his electron microscopic studies of the fine structure of *Drosophila* chromosomes.

But he had little time left. Some years earlier he had begun to have attacks of angina. A heart attack on April 29, 1971, proved fatal.

Jack Schultz is survived by his widow, Dr. Helen Redfield Schultz, by his son, Dr. Peter R. Schultz, by his daughter, Judith Jillian (Mrs. Richard Frisch); and by five grandchildren.

As one of his foreign collaborators has said, "To discuss problems with Jack was a unique occasion. You got more through personal talk with him than through his publications, for Jack had a lot to say that he hadn't published." Many colleagues throughout the world, as well as those at the Institute, can attest to their indebtedness to his scientific insight as well as to his warmth and humanity. He is sorely missed by all of us who knew him.

The early history of Jack Schultz's parents and the story of Jack's early life were kindly made available by Mrs. Jack Schultz.

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*KEY TO ABBREVIATIONS*

Am. Nat. = American Naturalist

Anat. Rec. = Anatomical Record

Cancer Res. = Cancer Research

Carnegie Inst. Wash. Year Book = Carnegie Institution of Washington Year Book

Cold Spring Harbor Symp. Quant. Biol. = Cold Spring Harbor Symposium on Quantitative Biology

Exp. Cell Res. = Experimental Cell Research

Fed. Proc. = Federation Proceedings

J. Cell. Biol. = Journal of Cellular Biology

J. Exp. Zool. = Journal of Experimental Zoology

J. Genet. = Journal of Genetics

J. Gen. Physiol. = Journal of General Physiology

J. Histochem. Cytochem. = Journal of Histochemistry and Cytochemistry

Proc. Am. Assoc. Cancer Res. = Proceedings of the American Association for Cancer Research

Proc. ———— Int. Congr. Genet. = Proceedings of the ———— International Congress of Genetics

Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences of the United States of America

Rec. Genet. Soc. Am. = Records of the Genetics Society of America

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Smith Stevens

# Stanley Smith Stevens

November 4, 1906-January 18, 1973

By George A. Miller

Stanley Smith Stevens was born in Ogden, Utah, to Stanley and Adeline (Smith) Stevens. He attended Mormon schools in Salt Lake City and after being graduated from high school in 1924 was sent on a three-year mission to Belgium and Switzerland for the Mormon Church. He returned in 1927 to enroll in the University of Utah and in 1929 transferred to Stanford University, where he received the A.B. degree in 1931. After two years of graduate study, he received his Ph.D. degree in psychology from the Department of Philosophy, Harvard University, where he served under E. G. Boring as assistant in psychology from 1932 to 1934. The following year he spent studying physiology under Hallowell Davis at the Harvard Medical School, on a National Research Council Fellowship; in 1935-1936 a fellowship from the Rockefeller Foundation enabled him to become a Research Fellow in physics at Harvard.

Psychology had achieved departmental status at Harvard in 1934, and in 1936 Stevens accepted a position as instructor in experimental psychology. He was promoted to assistant professor of psychology in 1938, gained academic tenure as associate professor of psychology in 1944, and became professor of psychology in 1946. In 1962, at his own request, his title was changed—he became "the world's first Professor of Psychophysics."

Stevens spent much of his boyhood in the polygamous household of his grandfather, Orson Smith, in Logan, Utah, surrounded by cousins of all ages. It was a hard, frontier style of life, but he later wrote that "the hardships of the adults were mostly lost on us children." It ended in 1924 with the deaths of both parents and his subsequent departure on the mission to Belgium. In 1930 Stevens married Maxine Leonard, and in 1936 they had a son, Peter Smith Stevens. Shortly afterward Maxine was overwhelmed by a postpartum depression that devastated their lives; she returned to Utah to live with her parents and died two decades later. In 1963 Stevens married Geraldine Stone.

In 1940, at the request of the U.S. Air Force, Stevens and L. L. Beranek created joint laboratories at Harvard to study the effects of intense noise in military aircraft and the possibilities of reducing it. Stevens was director of the Psycho-Acoustic Laboratory; Beranek, of the Electro-Acoustic Laboratory. The Psycho-Acoustic Laboratory was housed in the basement of Memorial Hall, a monstrous Victorian-Gothic building erected in 1875 as a dining hall. The laboratory began in the old furnace room; its rapid expansion into the abandoned kitchens was a project that occupied much of the director's attention —much of the work was done with his own hands. During the first year, young adults were exposed to 115 decibels of noise for periods of seven hours, during which a battery of psychomotor tests was conducted. Their performance was not impaired by noise, although they suffered temporary hearing losses. The major effect of noise was to make voice communication impossible, so the program of the laboratory shifted to testing and redesigning components of intercom and radio systems. To carry on this work, Stevens assembled a large and distinguished staff and welded them into a highly effective team: by the end of the war, the Psycho-Acoustic Laboratory had expanded to some fifty people.

The laboratory continued after the war with a reduced staff, and in 1947 Stevens brought Georg von Békésy to the United States to become a member of it. The remaining space in the basement of Memorial Hall was remodeled under Stevens's close supervision in order to accommodate the Department of Psychology in 1946, and from 1949 to 1962 Stevens served as director of the Psychological Laboratories as well as of the Psycho-Acoustic Laboratory. Stevens rechristened his own laboratory in 1962 as the Laboratory of Psychophysics. In 1965, over Stevens's strong objections, the laboratories and Department of Psychology were moved again, this time to William James Hall, which had been built for the Department of Psychology and Social Relations.

The accomplishments of the Psycho-Acoustic Laboratory during the war brought well-deserved credit to its director, and during the years immediately following the war Stevens was active in the bureaucratic affairs of science at the national level. He was consultant to the Research and Development Board from 1946 to 1952, Chairman of the National Research Council Division of Anthropology and Psychology for three years, and recipient of a Presidential Certificate of Merit. His interest in these activities declined after 1952, however, as he increasingly preferred to devote his major efforts to his own research.

Stevens was a member of the American Philosophical Society, the Society of Experimental Psychologists, the Acoustical Society of America, the Optical Society of America, the American Psychological Association, the Eastern Psychological Association, the American Physiological Society, the Psychonomic Society, the American Association for the Advancement of Science, the Philosophy of Science Association, the Society for Neuroscience, the American Academy of Arts and Sciences, Phi Beta Kappa, and Sigma Xi. His awards included the Warren Medal of the Society of Experimental Psychologists in 1943, the Distinguished Scientific Contribution Award of the Amer ican Psychological Association in 1960, the Beltone Institute Award for distinguished accomplishments as an educator in 1966, the Rayleigh Gold Medal Award of the British Acoustical Society in 1972. He was elected a member of the National Academy of Sciences in 1946.

He died quietly but unexpectedly in his sleep on January 18, 1973, while attending a meeting of the Winter Conference on Brain Research in Vail, Colorado. He is survived by his wife, his son, and three grandchildren.

Such are the facts. It is probably worthwhile to summarize them for reference purposes. But such facts are little more than the skeleton of a man's life. Like most skeletons, they give barely a hint of the man himself or what he suffered and accomplished.

In some "Notes for a Life Story" written in 1970, Stevens commented that his career "exhibits no plan or purpose, no over-reaching strategy, only tactical maneuvers brought on when circumstance has confronted desire. A series of accidents, in fact. Any man's life builds on a succession of accidents. That explains only part of it, however, for among the chance encounters there are some that take effect, whereas against other exposures a person stands as though inoculated with some natural antibody." As chance would have it, those encounters that took effect on Stevens thrust him into at least four separate careers. There was Stevens the administrator of laboratories. There was Stevens the professor and educator. There was Stevens the philosopher of science. And there was Stevens the scientist. The overreaching design that his friends can see in his life grew out of his art in blending these careers, using each in the service of the others.

## ADMINISTRATOR

My introduction to Dr. Stevens occurred in August 1942. I was a new graduate student, interested in speech and hearing; the teacher who had sent me to Harvard recommended me for employment in the Psycho-Acoustic Laboratory. I was directed to Dr. Stevens's office and found him in what I came later to recognize as a characteristic posture —legs extended, ankles crossed, feet on corner of desk. As he sat up and turned to greet me I saw a handsome man in his mid-thirties—tall and muscular, round-shouldered with long arms and large hands, a 4-4-4 on the somatotype scales; a long face with a high forehead and excellent features; wavy black hair and a natty moustache; an open, level gaze and an expression that in repose seemed sad, even disapproving, but could break into an irresistibly winning smile. When he wished, he could be one of the most affable people I have ever met. I remember leaving that brief meeting completely charmed and excited by the prospect of being paid for what I wanted to do anyhow.

In appearance he could have been a matinee idol, but the idea of S. S. Stevens as an actor would strike anyone who knew him as absurd. He could never have spoken lines from another's script. He was his own man, if ever anyone was. I did not actually join the laboratory until eighteen months later; by then I had learned that my first impression was only one side of a very complex personality.

Stevens was a primitive—he had in him the force of Nature. When the clouds gathered and thunder rolled forth, he was as little concerned as Nature for who might be caught in the storm. When the skies cleared and you found to your surprise that the landscape was still where it had been before, the day could be filled with sunshine. Those who could not weather the storms disliked him, and even those who admired him often found him difficult to work with. When he was seriously interested in a problem, he could move forward only at full speed—sometimes he ran over you. But those willing to stick it out were greatly rewarded.

He was not really as difficult to get along with as many seemed to think. It was a matter of understanding his ways. Sometimes he would appear at the door of your room and bark, "Know what you're doing?" Once you recognized this as his way of saying "good morning," the what-have-I-done panic subsided. Stevens's gruffness protected a basically shy person.

Other insights into his mannerisms took longer to come by, however. For years I thought him inordinately secretive, often carrying his reluctance to give out information so far as to withhold his decisions from those directly affected by them. I eventually learned that his natural retentiveness was only part of the reason. An equally important part was that often he had not yet made the decision one thought he was withholding. As he said of himself, "Decision never comes easy to me, and trying to decide to do something often tears me apart more than doing it." He had a great interest in the stock market, and all his friends with any capital sought his advice on investments; but he himself did little trading. "In order to be successful," he said, "you have to average two correct decisions on each trade. I am congenitally unsuited to the making of even one decision—correct or not."

Administrators are decision makers. A man who is "congenitally unsuited" to making decisions obviously cannot be a good administrator. Stevens knew that. Indeed, he often used it as proof of his incompetence when he wanted to avoid administrative responsibilities. He had an intense dislike for administration—for making decisions, for accommodating superiors, for compromising his own opinions, for interrupting his work to cope with the crisis of the day.

In truth, however, he was a superb administrator. His methods might not work elsewhere, but for the head of a wartime laboratory they were remarkably effective. If success is to be measured in terms of assembling a good team of scientists, using them wisely and keeping them happy in their work until they do better than they know how, then Stevens was a very successful administrator. He was an astute judge of intellectual horseflesh. He was a wise man with broad experience, so when he did make a decision, however painful the process may have been, it was usually the right decision. And because decisions did not come easy, he was never tempted to over-control. He lavished his concern on good equipment and an optimal arrangement of the laboratory environment. He painstakingly edited or rewrote reports. He set an example of dedication, working fourteen hours a day. These may not be practices recommended in manuals on how to become a successful administrator, but they worked for him. His was not the strategy of an executive, but of a patriarch.

The laboratory was his family, and members were given the duties and privileges of siblings, nephews, or cousins. The head of this extended family was concerned for the welfare of his kindred, and he rewarded them or disciplined them for their own good and the good of the group. This family provided not only for work, but also for the social life of its members—dinner at the Faculty Club; in the early days, a group foray to Boston's Chinatown or three carloads of incompetent but enthusiastic beginners invading the Fresh Pond Municipal Golf Course; later, weekends at "the farm" in New Hampshire, with maintenance work in the summers and skiing in the winters. At the time it seemed perfectly natural and fulfilling.

As in any family, everyone was on first-name terms. It never occurred to us to call him "S. S. Stevens"—he was "Smitty" to everyone. Anyone who tried calling him "Stanley" was lucky to be merely ignored.

Smitty was a close man with a dollar, and he spent his laboratory budget as if it were his personal checking account. Younger staff members, frustrated in their hopes of receiving what they regarded as deserved raises in pay, could be heard to call him miserly or worse. When confronted on the subject, he would explain that if a staff member's salary were too high. he would be priced out of the market when the time came to leave Harvard. Certainly a frugal childhood and the lean depression years had left their mark on Smitty, as on most of his peers. But in his case, it went deeper than a mere respect for money. Retentiveness was a personality trait. He disliked discarding or replacing personal possessions. He disliked lending books. He liked documentation and record-keeping. His memory was excellent and detailed. He held to his opinions regardless of their popularity. He was intensely loyal to his students and collaborators. He retained his identity as a Mormon of frontier stock. He saw variability as noise, masking the central invariances of both life and science. Even his contempt for "the seductive myth that experience writes on an empty slate" was consistent; genetic endowment is something you can hold onto. He was instinctively conservative, in the true sense of that much-abused term.

Smitty expected a full day's work, and to insure that he got it, he often would wait near the door of the laboratory in the morning to intercept late arrivals. One rainy morning during the early days of the Psycho-Acoustic Laboratory, a staff member who had arrived late hurriedly hung his hat where water dripped from it onto the Webster's dictionary below. In a rage, Smitty threw the offending hat to the floor and stamped on it. loudly berating its tardy owner. For many months, the scene was recounted in whispers by the awed onlookers.

Such episodes were exceptional, but no one ever doubted that the director was intensely concerned about every detail of the work that went on in his laboratory. Usually the battles were intellectual. Smitty would not tolerate fuzzy thinking, and his blunt, honest criticism wounded many tender egos. The fact that he was usually right didn't make it easier to take. Anyone willing to play the paternal role is bound to inspire ambivalence, but at least you knew he really cared about you and your work. His combination of wisdom, shrewdness, and intelligence, coupled with his training as a debater in school and college, made him almost invincible in arguments; but if you ever convinced him that you had a better idea, Smitty respected you for it. He could be as severe and critical of himself as of others.

His recipe for administrative success cannot be generally recommended. Even he would not have been so successful with his methods had it not been for the organizational gifts of his secretary and his administrative assistant, Didi Stone.

The Psycho-Acoustic Laboratory continued on a reduced scale after the war. In the early fifties, when Smitty lost interest in administrative matters, he probably would have been willing to reduce it to a one-man show in support of his own research. But Georg von Békésy's beautiful research depended on support from Smitty's grants, so he continued. Although Harvard could never find a way to give Békésy a faculty appointment, Smitty believed he was a great scientist and made every effort to provide space, facilities, assistants, and money for his work. When his judgment was vindicated by the awarding of a Nobel Prize in medicine to Békésy in 1961, Smitty seemed more elated than the recipient.

But the laboratory continued to shrink. Smitty's career as an administrator had ended even before 1962, when a stubborn president of Harvard forced him to step down from the post of director of the Psychological Laboratories and then in 1965 compelled him to leave his beloved basement, shaped for over a quarter of a century to meet Smitty's every need. It was disgracefully ungenerous treatment of a senior professor who had contributed so much for so long to make Harvard's Department of Psychology one of the world's best. Thus, this facet of his life ended on an unfortunately bitter note.

## TEACHER

It may seem anomalous that a man could base a distinguished career as an administrator on his dislike of making decisions, but that pales into insignificance beside the anomaly of Stevens's accomplishments as an educator.

As a young instructor at Harvard, he taught the laboratory course in experimental psychology and sections of Boring's introductory course. Later he added a course in mathematics for psychologists. But he disliked lecturing and was a mediocre classroom teacher. Gordon Allport, chairman of the psychology department, opposed Stevens's promotion in a long letter that detailed his shortcomings, citing particularly his open disparagement of teaching. Another of Allport's objections was the aversion shown him by some of the students. President Conant discussed the letter with Stevens, who later wrote, "Allport was right, of course, for in neither temperament nor appearance am I the outgoing teacher . . . I told Conant, as I had already told Allport, that I would teach my courses faithfully, but to enjoy standing before classes was beyond my power."

Smitty summarized his educational philosophy in one sentence: "Anyone worth teaching doesn't need to be taught." As with all his strong opinions, there was a well-developed network of arguments linking this sentence to his more general views of life and people. He applauded Boring's proposal that the department should abandon undergraduate teaching entirely. Only the most outstanding students should be admitted to the department, for graduate study; any who did not fulfill their promise should be asked to leave at the end of the first year. Graduate education should be based on seminars and research apprenticeships. This is very close to the system followed at the Rockefeller University, and I know how well it works; but it was totally unacceptable at Harvard, both to the administration and to Stevens's colleagues in psychology.

He predicted that if his colleagues persisted in giving undergraduate lectures on popular subjects, psychology would attract students who would change it from a science into socially relevant but intellectually empty do-goodism. When in his opinion that prediction had been fulfilled, his reaction was to refuse to call himself a psychologist. Stevens tried to coin a new name for the old-time science and helped found the Psychonomic Society; but he decided that he preferred the title "psychophysicist" for himself. The chairman of the department used an increase in undergraduate enrollment in negotiating for increased support from the dean, but such arguments held no appeal for Smitty. His unsuccessful efforts to reform Harvard were a continuing source of frustration to him.

Stevens believed firmly in the primacy of nature over nurture, in the inheritance of intelligence, in the dependence of personality on body type, in the genetic basis of schizophrenia. These opinions contrasted sharply with those of B. F. Skinner, who was invited to return to Harvard, very much on Smitty's initiative, in 1948. Neither Skinner the environmentalist nor Stevens the nativist could carry the day, so graduate education in the department continued much as it had in the days of Boring and Allport.

Smitty's responsibilities as a lecturer were interrupted by the war; they never resumed. He offered seminars in mathematical psychology and sensory psychology and until 1965 focused his efforts on one semester of the proseminar that was required of all first-year students. That semester concentrated on the history of psychology and on sensation and perception. It was a punishing course, with 150 pages of technical reading to be covered each week, and highly competitive examinations. Students gave weekly reports, but the professor was never reluctant to come down into the heat of debate and straighten them out. It was like an initiation ritual-no one enjoyed it at the time, but afterwards most seemed glad they had gone through it. And some students discovered that their formidable professor was not as dangerous as he sounded, that the brand of science he practiced was really fascinating once you got the hang of it.

But the classroom was merely a recruiting area. The real teaching went on elsewhere. "Actually," Smitty wrote, "two forms of teaching give me great joy: the joint endeavor of laboratory apprenticeship, and editorial give and take. In those two ways I seem always to be teaching. But deans count you at work only when you stand before a group with your mouth moving." Working with Smitty on an experiment—setting up the equipment, running each other as judges, pulling people out of the halls to serve as subjects, plotting the data, and arguing what they meant—was a rich experience for a young student. When it came to writing up the results, the interaction intensified. Stevens was a master of clear, expository prose; his own was marred only a by tendency, usually curbed, to become slightly more flowery than necessary. He liked the sound of a well-rounded sentence.

The amount of time he was willing to devote to a word-byword review of his students' writing was extraordinary. He had a gift for acting dense when it served his pedagogical purpose. How often I remember this exchange:

SSS: "What does this mean?"
GAM: "Oh, Smitty, you know what it means."
SSS: "Of course I know what it means. But look at what it *says!"*

And then would come the rephrasings. Behind Smitty as a critic and editor stood E. G. Boring, the consummate stylist. Both knew that the job of a scientist was not complete until the results of his experiment had been communicated. Both believed that an author should do all in his power to save the reader's time. And if they judged a student worthy of tutoring, both felt that the most valuable skill they could give him was an ability to phrase his thoughts clearly and briefly. Boring worked alone in his study and sent you five pages of single-spaced commentary. Stevens called you into his office and made you work through it with him. Both had their most direct effects through their editing. You cannot write clearly unless you think clearly, and their lessons in clear thinking were more valuable than all the psychological facts they taught you.

Although Smitty did not include it on his list of enjoyable forms of teaching, he was constantly instructing you about something. Whether he was guiding you down an advanced slope on his short skis, taking you to see a Harvard commencement, unpacking a newly arrived piece of equipment, or driving a new wellpoint at the farm—whatever you did with him was accompanied by a steady but unobtrusive sharing of knowledge and opinion. He loved to teach; it gave him a way to overcome his shyness and to reach out to people with gifts in hand.

His teaching was not limited to graduate students. Some of his best pupils were the postdoctorals who came year after year. He spent three years editing the *Handbook of Experimental Psychology* when he could easily have done it in one if he had not felt compelled to educate even the most distinguished contributors. In his own way, and for the limited audience he commanded, he was one of the most effective teachers of his generation.

A strong personality invites analysis, and those who had to find ways to coexist with Smitty found him a fascinating topic for speculation. His style of life, his independence of fashion, his tastes and outspoken opinions made him a thing apart, even in a community of individualists like Harvard. We sometimes debated whether he was an intellectual—he was more a man of action and argument than the intellectual stereotype seemed to allow. Consistent and well-established opinions guided him where others preferred to chat about current best-sellers or the latest intellectual fads. We knew that E. G. Boring, with his great historical knowledge, was an intellectual. G. W. Allport was a paradigm intellectual, so graceful and flexible in style and mind as to seem positively slippery at times. He was always prepared to shift the subject when irreconcilable differences loomed ahead: Stevens sought out the differences and tried to overpower them. Two such men could not long cooperate in one small department, and in 1946 Allport joined with Clyde Kluckhohn, Talcott Parsons, and Samuel Stouffer to create the Laboratory and the Department of Social Relations.

There were many benefits to Harvard from the new department, but some of us felt it was as much an accommodation to personality differences as an innovation in teaching and research. Smitty felt that the fission of the department gave the real scientists a chance to concentrate on the serious business of psychology. He never really forgave those of us who worked to reunite the interesting problems of social psychology with the scientific methods of experimental psychology.

In spite of his uncompromising opinions of the ways his colleagues undertook to teach psychology, Smitty was not really opposed to teaching. He was merely opposed to being asked to do it in any style but his own.

## PHILOSOPHER

Smitty once told me that someone had discouraged him from studying philosophy because he did not write well enough. The remark stuck in my mind because it conflicted so sharply with my view of him as an excellent expository writer. Whether or not the evaluation was correct, Smitty abandoned any aspirations to become a professor of philosophy. The surge of interest in the philosophy of science in the 1930s, however, did provide him an opportunity to become a philosopher, a role that suited him far better than the role of professor of philosophy.

Oddly enough, the initial impetus seems to have come from a physicist, P. W. Bridgman. It is odd because Smitty's exposure to philosophy and philosophers had been considerably more than incidental. As an undergraduate at Stanford he had shunned courses in science and mathematics in favor of "the windy subjects," as he later called them; at that time, "the philosopher image seemed most congenial." As a graduate student at Harvard, he had begun to discover his eventual vocation; but psychology was still administratively a part of the Department of Philosophy, so there was more philosophy to be studied. And from 1936 to 1940 he had to pass each day through the offices of the philosophy department on the first floor of Emerson Hall in order to reach the psychological laboratories on the third floor—it was often convenient to take a seat at lectures there. But it was Bridgman's solipsistic operationism in *The Logic of Modern Physics* that stimulated Stevens to write three philosophical essays on operationism in psychology in 1935-1936.

The problem he attacked in those papers had been set for him by E. G. Boring, who in 1932 was struggling to escape the traditional cleavage between mind and body that he had inherited from Titchener. He asked Smitty, his laboratory assistant, to read the manuscript of *The Physical Dimensions of Consciousness.* Smitty commented later that "an operational restatement of psychology's basic concepts was Boring's real aim," but at the time neither of them was able to do it. Bridgman's operationism showed Smitty the way, and, with Boring's considerable help, the three papers were written in 1935.

The argument, briefly stated, was that scientific concepts are defined by the operations scientists perform; that discrimination is the basic operation of all scientists; that psychology is the science whose responsibility it is to test and measure discrimination; and that psychology can accomplish this by analyzing mentalistic concepts such as experience, sensation, and sensory attributes in terms of the operations available to study them. Thus, discrimination was to replace immediate experience as the basis of all science, and discrimination is defined as a concrete, physical, differential response on the part of a living organism.

Toward the end of the 1930s, the logical positivism of Vienna was transplanted to America, where it produced enormous ferment. These ideas enriched and reinforced Smitty's philosophical leanings toward physicalism, and in 1939 he published a tutorial paper for his colleagues in psychology that reviewed operationism, logical positivism, physicalism, the unity of science, semiotics, the hypothetico-deductive method, and their relevance to the theoretical foundations of scientific psychology. In one way or another, he wrote, "they all assert essentially that science seeks to generate confirmable propositions by fitting a formal system of symbols (language, mathematics, logic) to empirical observations, and that the propositions of science have empirical significance only when their truth can be demonstrated by a set of concrete operations."

Smitty's interest in these ideas was not confined to any armchair; they were directly pertinent to his work as a practicing scientist. What was really troubling him was measurement. In 1936 he published a scale for the measurement of the psychological magnitude, loudness, as a function of the acoustic amplitude of the stimulus; and in 1937 (with Volkmann and Newman), a similar scale for the measurement of the psychological magnitude, pitch, as a function of the frequency of vibration. Loudness and pitch are subjective experiences, but if one rejects on philosophical grounds the cleavage between the physical and mental, then what were these measurements measuring? When a critic charged that they did not measure anything, that they were meaningless, how was one to reply?

Between 1932 and 1940 a committee of the British Association for the Advancement of Science had debated the question: Is it possible to measure human sensation? In its final report, the committee chose Stevens's scale of loudness as a concrete example, one which was said by its author to have all the formal properties of other basic scales, such as those used to measure length and weight. The members of the committee could not agree among themselves. For those who rejected the possibility of measurement, the critical argument seemed to be that there was no possible operation for adding two sensations together comparable to the operations of placing two lengths end-to-end or two weights in the same scale pan.

The only way to meet this objection was to demonstrate that psychologists have other operations, just as objectively describable as those for length and weight, that endow subjective scales with all the desirable properties of the basic scales of measurement in physics. In order to sustain such a claim, however, it was necessary to understand precisely what the relations were in physics between the measurement operations and the properties of the resulting scales. Bridgman gave him part of the answer. Another part was to be found in physicist N. R. Campbell's broad definition of measurement as the assignment of numerals to objects or events according to rules. But Stevens's problem was to make explicit the various rules for assigning numerals, the group structure of the resulting scales, and the statistical operations applicable to measurements made with each type of scale.

At a Congress for the Unity of Science in 1939, Stevens made a preliminary attempt to classify types of scales and illustrate them by examples from sensory psychophysics. "It was a botch," he said later, but he felt he was on the right track. "I began to tabulate the various kinds of scales and the kinds of operations needed to create them. Then it became clear that each kind of scale permitted a different mathematical transformation, and suddenly one evening in Emerson Hall the picture snapped into focus—there exists a hierarchy of scales defined by the mathematical transformations that leave the scale form invariant." Consultation with G. D. Birkhoff sent Stevens to the library to learn more about mathematical groups. His final classification was revealed to the Psychological Round Table in December 1940 and published in 1941 at the next Congress for the Unity of Science. The name he proposed "nominal" for the permutation group, "ordinal" for the isotonic group, "interval" for the linear or affine group, and "ratio" for the similarity group—have since become so standard that many authors who use them are unaware of their origin.

The answer to his critics was now complete. Scales of measurement are to be evaluated not in terms of the tangibility of the objects or events that are measured, but in terms of the operations of measurement that are used.

Having reached this operational resolution of his original problem, Smitty's interest in the broader issues of philosophy seemed to recede. In 1940 he and Rudolf Carnap organized a monthly discussion group at Harvard on the Unity of Science, but his growing responsibilities for the Psycho-Acoustic Laboratory reduced his participation in such discussions to a sometime thing. After the war he published several articles expanding on his classification of scales, but his philosophical ideas dwindled into odd paragraphs tucked away here and there in the more popular summarizations of his scientific work.

The closest he came again to explicit philosophical pronouncement seems to have been in an article in *Science,* in which he dubbed his views "schemapiric"—a hybrid of the formally schematic and the empirically substantive. In his schemapiric view of science, words and symbols serve only the neutral purpose of implementing a schematic structure, which may be related by operational rules to an empirical structure. But it was just a new name for views he had hammered out thirty years earlier. Whatever he called it, Stevens had found a philosophy he could live by.

## SCIENTIST

It is a long way from a poor Mormon household in Logan, Utah, to a professorship at one of the world's leading universities. Others strong and talented enough to pursue parallel courses to Harvard generally preferred, when they got there, to take on protective coloration from their new environment. Stevens may have envied them at times, but he could never have imitated them. The lessons he had learned along the way were too much a part of him, too important for what he wanted to do as a scientist.

In his "Notes for a Life Story," he describes how he worked to pay his way through college. "Summers I worked for the Idaho Power Company, starting as a grunt (hole digger and lineman's helper), living in a tent in a construction camp beside a mountain stream where you almost froze to death at night. I later worked up to summer utility man. That meant that I had a new job almost every two weeks as I replaced the man on vacation, whether meter reader, waterheater installer, or night troubleshooter. Forty years later, it becomes clear that my education for science took place more in the summers than in the winters." At Harvard, when he built a laboratory, he worked side by side with the carpenters and electricians. When he set up an experiment, he worked alongside the shop man–usually Ralph Gerbrands. Skilled hands and a knack for coaxing experimental equipment to perform were valuable tools in Stevens's scientist's kit. He never forgot the skills learned at the Idaho Power Company. The tough and skeptical view this experience had given him of his fellow man wasn't wasted either.

Although at various times and in various ways he contributed to a wide variety of psychological problems, Stevens's central concern throughout his life was psychophysical measurement. This has always been a particularly seminal area of research in psychology; techniques of measurement worked out under the well-controlled conditions of the laboratory have been repeatedly generalized to the measurement of attitudes, abilities, and other topics of greater personal and social importance than the sensory magnitudes they were designed to measure. As a result, many psychologists who have worked in this field have had more interest in the methodology they used than in the results they obtained. Such colleagues were a constant irritation to Stevens, who never viewed measurement as an end in itself. He was not averse to generalizing his measure ment techniques beyond their application to sensory magnitudes—to the measurement of physique and temperament, the prestige of occupations, socioeconomic status, the value of money, perceptions of national power, the seriousness of offenses and the severity of punishments—but he always evaluated such work in terms of the meaning of the results, not the technical versatility of the measurement operations used to obtain them. In a subject rampant with methodolatry, Stevens's contributions were always refreshingly sensible. It was the tendency to elevate the means over the ends that eventually gave operationism a bad name among scientists, but Stevens was never guilty of this.

One of the many schisms that divide psychologists into warring camps is that between the nomothetic and the idiopathic, between the search for universal laws and the concern for individual differences. With respect to problems of measurement, it becomes a question of whether one is more interested in the first or the second moment of the distribution. There is always a distribution, of course, and psychologists have performed valuable services by informing people where they stand in it with respect to their peers. Great statistical sophistication has supported such studies of individual variability. Stevens's interest, however, was not in the variance, but in the invariances of the measurements. Elaborate statistical analysis never impressed him. "What scientific discoveries," he once asked, "owe their existence to the techniques of statistical analysis or inference?" In his *Handbook* he urged his colleagues to "cultivate a love for invariance" and to "seek uniformities in heterogeneity." He believed that "the delineation of the conditions of invariance for any phenomenon would tell us all we want to know about the matter," and that a scientist's responsibility is to provide "measures that will stay put while his back is turned." It was good advice, but difficult for many psychologists to take.

In the measurement of sensory magnitudes, Stevens's own area of central concern, tradition was against him. In the nineteenth century, G. T. Fechner had based psychophysical measurement on the counting of just-noticeable differences (jnd's). If, for example, you wished to measure the brightness of a light, you were supposed to count the number of jnd's a person could detect between complete darkness and the light to be measured. Since the magnitude of a jnd was proportional to the magnitude of the stimulus to which it was added, this argument led Fechner to determine a logarithmic relation between the stimulus intensity and the sensory magnitude—each time the intensity is doubled, there should be a constant increment in sensation. L. L. Thurstone later provided the statistical rationale: The size of the jnd depends on inherent variability in the sensory system. The variability of any measurement is generally a function of the magnitude being measured. Hence, by observing the variability, one could infer the magnitude.

Although the argument seemed somewhat backwards, it was plausible enough to persuade psychologists for at least a century that sensory magnitudes are a logarithmic function of stimulus intensity. What bothered Stevens was that it wasn't true. The facts about differential sensitivity were true enough, but the relation between jnd's and sensation did not hold. If you ask people to adjust the intensity of one tone until it sounds twice as loud as another, for example, you find that loudness grows much more rapidly than the number of jnd's. Stevens pointed this out in 1936 when he proposed his first sone scale for loudness, based on a review of such direct estimations by B. G. Churcher. The vast disparity between the subjective magnitudes of different jnd's "is astonishing in view of the original assumption by which they were considered equal. Their integration for the purpose of obtaining a reasonable numerical scale for the measurement of the magnitude of 'sensation' is obviously not valid."

When this work was interrupted by the war, that is where the matter had to be left—as a puzzling disparity. The puzzle was sharpened by the fact that there was no comparable dis parity for pitch; jnd's for pitch are subjectively equal. In 1940 Stevens and Volkmann suggested that this difference should be explained by the difference in the discriminatory mechanisms that mediate pitch and loudness. When the frequency of a tone is changed, new excitation is substituted for old; when the intensity of a tone is increased, new excitation is added to old. Stevens later generalized this distinction to other sensory modalities, calling the additive attributes "prothetic" and the substitutive attributes "metathetic."

After the war he did not return immediately to this puzzle. In his autobiography Smitty speaks of the years from 1945 to 1952 as "seven lean years." He was busy planning and supervising the renovation of the basement in Memorial Hall, directing the Psycho-Acoustic Laboratory, commuting to the high councils of science in Washington, enjoying the honors and recognition he received, rebuilding the farmhouse in New Hampshire into a ski lodge and experimenting with skis, and for three of those years his major preoccupation, with the skilled editorial assistance of Didi Stone, was the 1400-page *Handbook of Experimental Psychology*. But his scientific publications during those years were accounts of prewar work on the theory of measurement or experiments conducted during the war. He felt increasingly defeated by success, unable to get back to the detailed work of research while his desk bloomed with important papers pressing for attention. He spoke of the "gnawing fear that the fire was spent, that science, the jealous mistress I had abandoned for war research, had now abandoned me."

He credited an argument with W. R. Garner about the scaling of loudness with providing the impetus that finally sent him back into the laboratory for the final twenty years of his life. As any operationist would have expected, the loudness scale that is measured depends on the experimental operations performed. You could count jnd's, ask people to adjust one magni tude until it was halfway between two others or to make one magnitude twice another, compare two ears with one, ask people to rate magnitudes or ratios between magnitudes, or use other clever schemes that psychologists had invented. And no two methods seemed to give exactly the same scale. On operational grounds, there seemed no better way to choose among methods than to flip a coin.

A more consistent operationist might have let it go at that, but Smitty simply could not accept the idea that there was not an underlying invariance in all that heterogeneity. And so he set out to find it.

He began by adopting the method of magnitude estimation—which he invented as the simplest and most direct procedure he could think of for getting at a person's impressions—as his basic experimental operation. A series of stimuli were presented in an irregular order, and the person was asked to assign numbers to them; he could assign any number he liked to the first stimulus, but thereafter the ratios of the numbers he assigned should correspond to the ratios of his subjective impressions. When Smitty plotted the data obtained in this manner against the stimulus intensity, he repeatedly found not a logarithmic function, but a power function. When loudness was the attribute to be judged, for example, every time the intensity was increased by 10 dB, the number assigned to it doubled. Whereas Fechner would have predicted that logarithmic increments in intensity would be judged as constant increments in loudness, Stevens found that they yielded constant ratios in loudness. The loudness, $L$, was proportional to the energy to the 0.3 power: $\mathrm{L} = kI^{0.3}$. From this it follows that the underlying invariance is the simple principle that equal stimulus ratios produce equal subjective ratios.

There followed an intense period of work extending this insight to other sensory modalities. Many collaborators, one of the most important of whom was J. C. Stevens (no relation).

assisted in this work. In every case they found power functions, with exponents ranging from 0.3 for brightness up to 3.5 for electric shock. Discrepant results obtained by other, more complex judgmental operations were rationalized away —at least to Stevens's satisfaction—and the whole psychophysical structure built on variability by Fechner and Thurstone was replaced by the power law, or, as many now call it, "Stevens' Law."

Science has been likened to a vessel that the crew must continually rebuild during the voyage. This particular bit of reconstruction concerned a vital part of the craft, and it was not accomplished without considerable complaint from the other passengers. One repeated objection was that the yardstick Stevens was using was the number scale that people carry around in their heads—perhaps the invariance was attributable to their arithmetic habits rather than to their sensory transducers. In order to meet that objection, Stevens generalized the method to what he called "cross-modality matching." Magnitudes on sensory continuum, A, are matched to magnitudes of two other continua, B and C. The ratio of the exponents of the function matching A and B to the function matching A and C predicts the exponent of the function matching B and C. The numbers used in magnitude estimation can be regarded as simply another perceptual modality like the others; anyone who regards them with suspicion can dispense with them. Thus, Stevens came to regard all measurement as a matching procedure; numerical matching is merely a special case.

In 1965 Gösta Ekman, a distinguished psychophysicist in Stockholm, reviewed the subject and rendered this verdict on Stevens's accomplishment: "After a hundred years of almost general acceptance and practically no experimentation, Fechner's logarithmic law was replaced by the power law. The amount of experimental work performed in the 1950s on this problem by Stevens and other research workers was enormous, and the outcome was an outstanding success. The power law was verified again and again, in literally hundreds of experiments. As an experimental fact, the power law is established beyond any reasonable doubt, possibly more firmly established than anything else in psychology.

Stevens continued active work on these problems until he died. The premature deaths of both Ekman and Stevens were terrible blows to psychophysics. Fortunately, however, in the weeks before his death, Stevens completed the manuscript of a book that, when published, will summarize psychophysics and preserve his contributions to this old but still vital branch of scientific psychology.

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*KEY TO ABBREVIATIONS*

Am. J. Psychol. = American Journal of Psychology
Am. Psychol. = American Psychologist
Am. Sci. = American Scientist
J. Acoust. Soc. Am. = Journal of the Acoustical Society of America
J. Exp. Psychol. = Journal of Experimental Psychology
J. Sound Vib. = Journal of Sound and Vibration
Percept. Psychophys. = Perception and Psychophysics
Proc. ———— Int. Congr. Acoust. = Proceedings of the ———— International Congress of Acoustics
Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences
Psychol. Bull. = Psychological Bulletin
Psychol. Rev. = Psychological Review
Q. J. Exp. Psychol. = Quarterly Journal of Experimental Psychology
Vision Res. = Vision Research

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John T. Tate

# John Torrence Tate

July 28, 1889-May 27, 1950

By Alfred O. C. Nier and John H. Van Vleck

On June 21, 1966, the physics building of the University of Minnesota, which previously had only a functional designation, was named the "John T. Tate Laboratory of Physics," a fitting tribute to a man who had helped so much in bringing America to the forefront in physics and in making Minnesota one of its leading centers.

## EARLY LIFE

Tate was born in Adams County, Iowa, on July 28, 1889, the son of Samuel A. and Minnie Ralston Tate. The area was a rural one and the nearest sizable town, of about 1,000 population, was Lenox, slightly over the line in an adjacent county. This Tate consequently listed as his town of birth, and the fact that it was not in Adams County caused considerable confusion in his clearance papers and other documents during World War II.

Tate's father was a country doctor of Scottish descent, whose ancestors had come to America before the Revolutionary War. Several generations of them were Presbyterian ministers in this country. His mother was of Irish descent.

Tate's mother died when he was about ten years old. After that he was sent to live in New York City with the family of his father's brother. Two important considerations led to this decision. Educational facilities at that time obviously were better in New York than in rural Iowa. Also, the nomadic life of a widowed country doctor could not offer much in the way of a home atmosphere for a boy.

In New York, young Tate attended the Horace Mann School. By the time he was in high school, he exhibited a fondness for science. He used to experiment at home with a small chemistry set, and he created one or two small explosions in the house. His high school yearbook had a rhyme for each member of the graduating class, and the one for Tate was "terribly taciturn Tate, with HCl on his pate."

## UNIVERSITY EDUCATION

After finishing high school, Tate entered the University of Nebraska as an electrical engineering major. He presumably chose this institution so he might see something of his father. About this time, the latter had accepted an assignment as physician on the Rosebud Indian Reservation, located in South Dakota, just north of the Nebraska line. The young Tate helped support himself in college by taking a summer position involving maintaining the power plant of the reservation.

After being graduated from the university in 1910, he completed two years of graduate work there, and received an M.A. in 1912. He had shifted from engineering to physics, and the paper he published in the *Physical Review* in 1912, "The Theoretical and Experimental Determination of Reflection Coefficients of Absorbing Media," was essentially his M.A. dissertation.

It was something of a tradition for physics students at Nebraska to continue their graduate work at the University of Berlin, as many of its senior staff (Almy, Brace, Skinner, Tuckerman, and others) had done so. This Tate was able to do in 1912, with the aid of a loan from his brother, and perhaps a legacy from his father who had died in 1911. He took his Ph.D. only two years later under James Franck, who had not yet left Berlin for the University of Göttingen. His dissertation was "The Heat of Vaporization of Metals," a title that implies chemistry. There is no reference to this piece in *Science Abstracts,* so apparently it was not published in a scientific journal even in abridged form. This may be because of the onset of World War I, which probably forced Tate to return to America earlier than he otherwise would have done. Dr. Paul Foote informs us that he thinks Tate published his dissertation as a pamphlet.

## THE NEBRASKA AND EARLY MINNESOTA YEARS, 1914-1917

It is not surprising that, after receiving his doctorate, Tate was offered an instructorship at the University of Nebraska, which he accepted. A year later he was made an assistant professor. During the academic year 1915-1916, Professor Anthony Zeleny of the University of Minnesota, on sabbatical leave at Princeton University, was instructed to look for promising young men while he was in the East. How he became acquainted with Tate is not known, but on June 3, 1916, he wrote to Professor Henry Erikson, chairman of the Minnesota physics department, strongly urging that Tate be considered. By June 26 Tate had visited Minnesota, created a most favorable impression, and received an offer of a position. By June 29 he had accepted the offer. Initially his position was only as instructor at $1500, but he was promised that he would be considered for promotion to assistant professor at the end of the year if he made good. The promotion possibility was tendered with some reluctance, but was rationalized on the grounds that it was so late in the year and that Tate had created such a favorable impression. That Tate made good cannot be doubted. He received this promotion as well as two others in the next few years, with the result that by 1920 he was a full professor at the age of only thirty-one! Except for the interruptions occasioned by World Wars I and II, he served on the Minnesota faculty continuously for thirty-four years.

In his first year at the University of Minnesota, Tate col laborated extensively with Paul D. Foote, then in his last year of graduate work there. The two men had already been fellow students and laboratory assistants at Nebraska in 1909-1911. The year 1916-1917 saw a quite remarkable group of young men in the Minnesota physics department. Dr. Foote writes us, "Tate was one of my teachers. In fact all of the younger staff took courses under each other. Tate taught me statistical mechanics, and the group, including Arthur Compton, Tate, McKeehan, Klopsteg and others, were in my class on radiation theory."

## WORLD WAR I AND RESEARCH AT THE BUREAU OF STANDARDS, 1917-1918

During World War I Tate served as a lieutenant in the Signal Corps, and at the close of the war he was stationed in Washington, D.C., where Foote had already moved to a position at the Bureau of Standards. Prior to entering the Army, Tate himself may have had a temporary summer position at the Bureau, as its roster for 1917 lists him as an employee. The two men continued collaborating on some of the problems they had studied at Minnesota. They published two papers connected with the latent heat of evaporation of metals, thereby showing continued interest in the area in which Tate had worked for his Ph.D. Something of particular importance is revealed when Foote writes that "by working evenings and Sundays at my laboratory at the Bureau of Standards, we were able to publish several papers on critical potentials."

The general subject of electron impact and critical potentials is probably the research area in which the most notable work of Tate and his research students was performed over the years. His first paper on this subject in 1917 was most timely in its appearance. Bohr's theory of the atom, announced only shortly before, predicted that electrons in atoms should be found only in discrete energy levels, and transitions between levels would result in absorption or emission of radiation according to the relation $Ve = h\nu$, connecting critical potentials and spectral frequencies. Tate's early work indeed verified the existence of energy levels and showed the distinction between the critical energy required to excite radiation and that required to produce ionization. This was of considerable importance at the time, as it furnished unmistakable evidence that the quantum concept was inevitable, though it was then still in embryonic form since the true quantum mechanics was not evolved by Heisenberg, Schrödinger, and others until about a decade later. Whether having been with Franck earlier in Berlin had stimulated Tate's interest in electron impact phenomena, we cannot say for sure. Franck himself did outstanding work in this field after Tate left Germany, but as far as we know, the two men never collaborated on this subject, and Tate may have been attracted independently into the then new field.

## TEACHING AND RESEARCH AT MINNESOTA, 1919-1940

When Tate returned to the University of Minnesota, in January 1919, he shared the teaching of graduate courses with W. F. G. Swann, who was the principal adviser of graduate students during the early 1920s. It was during this period that Tate developed a comprehensive course in classical physics, "Introduction to Theoretical Physics," which he taught every year but two until 1937. The course was taken by all beginning graduate students in physics and an occasional undergraduate bold enough to enroll. It was also taken by many graduate students in mathematics, chemistry, and engineering, so that over the years a great many students were exposed to and inspired by Tate's elegant lectures. Students agreed that he was one of the best, if not the best, teacher they had ever had.

During the early 1920s, he also developed a course entitled, "Seminar in Contemporary Experimental Physics." In it were discussed the latest developments in physics, experimental or theoretical. Occasionally students presented papers, but more commonly Tate did the talking. Graduate students took this yearlong course once for credit, then attended on a noncredit basis for most of the remainder of their student days. Other staff members were frequent visitors.

During this period Tate attended many meetings of the American Physical Society, in fact virtually all of them after he became Editor-in-Chief of the *Physical Review.* Upon his return from a meeting, he invariably reviewed for the class the important papers he had heard. The receipt of an exciting manuscript at the *Physical Review* office almost certainly resulted in its presentation to the class, often without advance preparation, since the paper might have arrived only the hour before the class met. Tate had the almost uncanny ability to extract the essential information from a long paper and, without preparation, present it in a way that everyone understood. His lectures were filled with ideas for possible research problems. In his classes one literally lived on the forefront of knowledge.

When Swann left Minnesota in 1923, the main responsibility for advising graduate students in experimental physics fell on Tate's shoulders, and during the twenty years prior to World War II he was the adviser for twenty-seven of the forty-eight students who obtained their Ph.D.s in physics. John T. Tate did not, however, operate a diploma mill. Life as a graduate student under his direction was not an easy lot. Candidates for the Ph.D. degree were expected to stand on their own feet and to persist until they overcame the inevitable stumbling blocks faced in research. It is no wonder that, with such training, so many of his students later distinguished themselves in positions of leadership and accomplishment. One of them, Walter Brattain, shared the Nobel Prize in Physics in 1956 for his contribution to the invention and development of the transistor.

As the *Physical Review* grew and Tate became active in the establishment and guidance of the American Institute of Physics, he had less and less free time. Travel alone occupied considerable time, as this was before the day of regular air travel, and a trip to New York from Minneapolis required a day and a half in each direction. Throughout his busiest years he taught his two courses, giving eight lectures per week, except for absent days when a substitute filled in. He graded all of his examinations himself.

In spite of the pressure, his office door was always open, and students and colleagues wandered in and out. Nevertheless, for the new graduate student a trip to Tate's office was a traumatic experience. Tate was basically a shy man with an air of aloofness about him. This, coupled with the great respect in which he was held and the knowledge that he was a very busy person, meant that he was regarded with considerable awe by all but his closest friends. One did not go to his office to idle away the time! More relaxed were his sessions with the advanced graduate students when, in the late afternoon after clearing up his *Physical Review* work, he wandered down the research alley and went from room to room to smoke and chat.

He had little time to perform research himself and preferred to help others develop programs. His name often did not appear on papers he had helped initiate. His own name usually appeared last on joint publications with paid assistants, and in at least one case when his name had to appear to justify the expenditure of funds, he apologized to a postdoctoral assistant for the circumstance.

Tate's advice, counsel, and interest were a great stimulus and comfort not only to his students, but also to his colleagues on the faculty of the Minnesota physics department. To this, both writers can testify from personal experience. (Alfred O. C. Nier has been a member of that faculty since 1938, and John H. Van Vleck was a member from 1923 to 1928.)

During the 1920s, an era of atomic physics, Tate's students worked mostly on problems relating to the impact of electrons on gases. Particularly noteworthy was some of the work in the 1929-1931 period. Walker Bleakney had built an instrument for studying ionization processes in gases. It had a collimated monoenergetic electron beam whose energy could be adjusted. In addition, an m/e analysis could be made of the ions produced. When mercury vapor was introduced, it was possible to observe the various multiply charged ions of mercury formed by electron impact, as well as the onset potential for each ion and the ionization cross section. This introduced a new era in mass spectroscopy and led to subsequent developments and applications that have had an enormous impact on other areas of science and technology. To cite but one example, we can quote from a cryptic entry in the handwritten ledger kept by Henry Erikson, for many years chairman of the physics department. "In March and April 1940 Nier established U 235 as responsible for the slow fission in uranium. This gave rise to a considerable interest." In referring to these words, James Gray says in his history of the University of Minnesota, "The reticence of Professor Erikson's comments cannot be duplicated in the literature of science or in all the literature of human affairs. . . ."

In 1929-1930, E. U. Condon was on the Minnesota faculty working on the theory of molecular binding, employing the new quantum mechanics just then coming into use. One prediction of the theory was that in a diatomic molecule or molecular ion there could be a repulsive potential energy curve as well as an attractive one. If so, a molecule excited by electrons of sufficient energy should reach the repulsive state and subsequently dissociate into an atom and an ion, the particles having measurable kinetic energy. At the dedication of the John T. Tate Laboratory of Physics in 1966, E. U. Condon related in his characteristically entertaining manner the circumstances at the time, and how he, Tate, and Bleakney discussed the feasibility of an ex periment to test the theory. It turned out that Bleakney's mercury apparatus was ideally suited for a crucial experiment, and indeed a few days later Bleakney observed the energetic atomic hydrogen ions formed in the dissociation process.

Tate not only expected his students to be self-reliant, but he also expected experiments to be done correctly. After all, he was editor of the *Physical Review* and felt a special obligation to see that results reported from *his* laboratory were not in error. As a former student, Alfred O. C. Nier can testify to this attitude. Gray, again in his history of the University of Minnesota, writes, "A teacher with the hardihood to insist that all his students must make a decent attempt at being geniuses is likely to produce many of outstanding talent. Tate's success may be measured by the fact that in the period when the volume *Men of Science* made a practice of starring those names that seemed particularly bright, Minnesota graduates in physics were nearly as numerous among the elite as were graduates of the Massachusetts Institute of Technology."

Eloquent testimony to the fact that experiments conducted under Tate's direction were performed with care can be found in references on ionization of molecules by electron impact. New workers in the field, even today, proudly establish their credibility by announcing that they were able to confirm the measurements made by Tate and his graduate student, P. T. Smith, some forty-five years earlier!

The 1930s saw the emergence of nuclear physics as a new frontier awaiting exploration, and Tate was determined that Minnesota be a participant in the action. In 1933 the late John H. Williams accepted the position of research assistant to Tate. He was employed to carry on the work on ionization of gases, but before a year had passed he was encouraged by Tate to start a program in nuclear physics. The department owned a 275-keV transformer—kenetron—condenser, a source of high voltage that when supplemented by an ion source, an ion accelerating tube, and other accessories made possible the study of nuclear disintegration processes in light elements.

It was soon realized that to perform significant work in the expanding field of nuclear physics, higher energies would be required. Work was started on a Van de Graaff generator to give energies of one million electron volts, but it was never finished because it soon became apparent that a more ambitious program was in order. In 1936 it was decided to construct a pressure Van de Graaff generator that would provide energies of at least three million electron volts. Because the cost was beyond the resources of the institution, outside help was sought. It was a time when there was a growing appreciation of the potential for applying the fruits of nuclear physics research to medicine and biology. Tate headed a distinguished committee of University researchers representing the several areas of concern, who approached the Rockefeller Foundation for a grant of $36,000 to build a Van de Graaff generator and to finance a program of interdisciplinary research using the facility. On April 7, 1937, the trustees of the Rockefeller Foundation approved the request and the program was launched, the design of the machine being put under the direction of Tate's young protege, John Williams.

Although Tate was primarily an experimental physicist, he had a keen appreciation and understanding of what was going on in theory and this quality was a great help in his research. Many physicists educated early in the present century, when classical physics was well entrenched and quantum theory was a parvenu, were never able adequately to assimilate, or in many cases even to accept, the basic ideas of quantum mechanics. Tate was not a man of this type. When Heisenberg, Schrödinger, and others developed the true quantum mechanics around 1926, he quickly realized its importance. When a lecture course on this subject was given by one of the writers (incidentally one of the earliest, if not the earliest, such course in the United

States), he attended conscientiously, remarking, "I must learn this stuff."

## TATE'S CAREER AS EDITOR

In 1926 Tate was named Managing Editor of the American Physical Society, a position he held until his untimely death in 1950. As Editor-in-Chief of the *Physical Review,* he saw the annual journal pages more than quadruple and the number of subscribers triple. In fact, the *Physical Review* became the leading research journal of physics in the world. In 1929 Tate started the very successful quarterly journal, *Reviews of Modern Physics,* and in 1931 a second new journal, *Physics,* later renamed the *Journal of Applied Physics.*

In his history of the University of Minnesota, Gray comments on the choice of Tate as editor: "His reputation in the profession was so great that the editorship of the *Physical Review* went to him when he was still a young man, and this was the first time that it had been allowed to pass beyond the Allegheny Mountains. His extraordinary ability to digest every experience that came to him made the most of what might have been, to another man, a dull routine assignment."

It is fortunate that Tate's initial appointment as the chief editor of the *Physical Review* practically coincided with the "quantum-mechanical revolution." It would have been a calamity had the post been filled by someone not appreciative of or sympathetic to the great developments that suddenly burgeoned in theoretical physics. But Tate showed rare judgment and common sense in not delaying by much refereeing noteworthy papers dealing with various applications of quantum mechanics; this was important, for America was somewhat at a disadvantage compared to the centers of Europe, where the revolution had germinated. One of the authors remembers how the refereeing of a paper submitted by D. M. Dennison—"The Rotation of Molecules" (a calculation made at

Copenhagen with matrix mechanics)—consisted of Tate's showing him the manuscript in the *Physical Review* office. It was the author's perusal of this article that perhaps triggered his lifelong interest in electric and magnetic susceptibilities, and he recalls receiving a wire from Dennison giving him permission to utilize the results of the manuscript in advance of formal publication.

In the late 1920s, Tate, along with other prominent physicists, saw the need for bringing together the several societies representing different branches of physics; in 1931 he was one of the founders of the American Institute of Physics. It was natural that he be chosen one of the initial members of the governing board of the Institute, and as a result of successive renominations by the American Physical Society, he remained a member until his death. From 1936 to 1939 he served as Chairman of the Board.

## TATE'S BRIEF ROLE AS DEAN

That Tate's interests went beyond physics was well known at the University of Minnesota. In 1930 the University established a new college for students of high ability whose educational objectives crossed traditional college lines. It was only natural that Tate, known for his interest in liberal education, should be chosen to head the select college. Because the college was only experimental, he was not given the title of "dean," but he enjoyed the privileges of the post. In 1937 he was named dean of the University's largest unit, the College of Science, Literature, and the Arts. While he retained his editorships and supervised a few graduate students and postdoctoral assistants, he no longer found time to teach the courses for which he had become so well known. His tenure as dean was destined to be short. He had hardly become established in the position before World War II broke out, and again he was called to the service of his country. In 1943, when it became apparent that he could not soon return to the University, he resigned his deanship to make it possible for others to plan for the future.

## THE WAR PERIOD, 1941-1945

As the war progressed and the Low Countries fell, it became clear that the submarine had come into this conflict a more dangerous weapon than it had been in World War I. Improved means had to be found for detecting and destroying submarines. When the National Defense Research Committee (NDRC) was created in 1940, the Navy asked its aid in the defeat of the submarine. In response, the NDRC set up in early 1941 an organization later known as Division 6. It had, in the words of Frank B. Jewett, President of the National Academy of Sciences at the time, the following objectives: "(1) The most complete investigation possible of all the factors and phenomena involved in the accurate detection of submerged or partially submerged submarines and in anti-submarine devices, and (2) the development of equipment and methods for use of promising means for detection to the point where their final embodiment in form satisfactory for Naval operations can be undertaken by the regular Bureaus of the Navy."

John Tate was chosen to lead this awesome effort. The success of Division 6 in carrying out its assignments is now recorded history. Tate's effective leadership of this complex organization is best summarized in the words of Gaylord Harnwell, who wrote in a foreword to the *History of Division 6* in 1951, "That this contribution was effective is due in major part to the deep and sympathetic understanding of the problems of nature and men which identified the wise leadership of the Division's chief, John T. Tate." In addition to serving as Chief of NDRC Division of Subsurface Warfare, Tate was scientific adviser to the Commander-in-Chief, United States Fleet; Assistant Chief of the Office of Field Services of the Office of Scientific Research and Development in charge of Operations Research;

and Chief and Member of the Rocket Ordnance Division of the NDRC.

## RETURN TO MINNESOTA

On January 1, 1946, Tate returned to the Minnesota campus as Research Professor of Physics, picking up where he had left off nine years before. He taught one course and began a modest research program involving a few students. Now, the *Physical Review* began its rapid postwar expansion, and more and more of Tate's time was devoted to it. From 1946 to 1949 he served as Chairman of the Board of Governors of the Argonne National Laboratory. He continued in his role as an adviser to the government, and at the time of his death in 1950 was Chairman of the Committee on Undersea Warfare of the National Research Council.

During the war virtually all research in physics at Minnesota stopped as almost all of the regular faculty were away, working on defense-related research. When they returned and were joined by other young colleagues, Tate was a central figure, a pillar of strength, helping them get support for their research and counseling them as needed.

## TATE'S PERSONALITY

Many facets of Tate's personality deserve comment. Among them are his modesty and unselfishness. For this reason, as we have already indicated, his contributions to research cannot be judged by the number of papers in his bibliography alone. Several times in the 1930s, he drove his car all the way from Minnesota to meetings of the American Physical Society in Washington, D.C., or Chicago. Although he rather enjoyed driving, his main motivation was that he could thus transport several of his students to the meeting in question. Certainly, they could not have gone there otherwise, for those were the days of the Depression rather than of government-sponsored research contracts.

Tate's characteristic modesty and consideration for others are well illustrated in exchanges of correspondence between himself and J. W. Buchta, Chairman of the Department of Physics at Minnesota. Upon Tate's resignation as dean, the regents of the University suggested that his very special status be recognized by naming him Research Professor of Physics, but they left it to him to decide if he wanted such a title. On February 21, 1944, he wrote to Buchta:

"President Coffey has just informed me of the regents' action. From my point of view I question a title which would, in any way, set me apart from others. On the other hand, as I told President Coffey when I saw him in January, if in his judgment the title Research Professor would be of value to the University or the Physics Department, I would not be adverse to having it.

"I see ways in which having such a title in the Department might be of value to it in that it would give concrete evidence that research in physics is given emphasis by the Board of Regents. . . ."

In another letter dated October 26, 1943, he said:

"For some time I have had it in mind to suggest that you recommend to McConnell that the salaries of the men in the Physics Department who are on leave (in service to the U.S.) be raised in much the same way as you would anticipate they would have been raised had they remained on duty. To do this would give them assurance that the University wants them to return and intends to treat them properly. I recall this was done in my case during the last war and still recall the pleasure it gave me and the feeling that I was still regarded as a permanent member of the University staff."

Tate had unusual patience. We will cite two incidents that reflect this quality. One was at an open business meeting of the American Physical Society a year or two after he had taken over as Editor-in-Chief of the *Physical Review.* His predecessor in that capacity arose to criticize him publicly on the ground that the average length of a paper published was longer than in the previous administration. Tate might well have rejoined that the greater length was only a reasonable manifestation of the rapidly improving quality of American physics at the time, to say nothing of the added complexity of theoretical papers occasioned by the burgeoning quantum mechanics. However, he bit his lip and said nothing. Another time one of us found him painstakingly copying out the many equations in a paper of a distinguished theoretical physicist because the author's handwriting, though beautiful in appearance, was not sufficiently legible for the printer. Many editors would have sent the paper back to the author for rewriting, but this would have caused delay at a time when theoretical developments were moving fast.

Tate was not by temperament a fighter. Although the dictums of the *Physical Review* forbade the use of radical signs, he decided it was easier to make an exception rather than to struggle perennially with one distinguished chemical physicist who was particularly recalcitrant about having radicals replaced by fractional exponents. Tate won victories through his tact rather than by "slugging it out." In 1926 the new library of the University of Minnesota was completed. Grants from the legislature for construction of the building had been obtained by the regents on the ground that there would be a central library facility. To the great irritation of the members of the physics department, the physics books were all moved from their building to the central library. The resulting inconvenience was a reason one prominent physicist gave for leaving after only one year at Minnesota. Tate did not resist the moving of the books, but when one of us visited Minneapolis only a few years later, the physics library had somehow been returned to the physics building where it belonged.

Tate's loyalty to Minnesota was great and unfailing over the years. We know of at least two offers he declined from prestigi ous institutions: one a research professorship, and the other an influential administrative post. Both offers were of the type he would have accepted had he been interested in getting his name in the limelight as often as possible.

Tate did not go out of his way to seek public speaking engagements of a general character. However, when he was enticed to the lecture platform, his speeches were outstanding. He impressed his audiences with his sincerity, dignity, and substantial thoughts. He felt that scientists had an obligation to disclose some of their philosophy and the meaning of their profession to those in other disciplines. Unfortunately, his addresses on such occasions as a joint meeting of Phi Beta Kappa and Sigma Xi or the centennial celebration of Rockford College were not published, as far as we know, although his son has some of the manuscripts. A favorite topic of his was "Science and Human Values," a manifestation of his belief that science is not a separate entity but rather a necessary ingredient in a liberal education. The manuscript of an unpublished commencement address has so many literary and historical references that an uninformed reader would deem it the work of a professor of English or history.

Tate's "outside," or recreational, interests were many and varied. He was fond of golf. At one time he was the champion billiard player of the University of Minnesota faculty, and during his undergraduate days he ranked as a collegiate tennis champion. He was an enthusiastic and skilled photographer. He returned from his studies in Berlin with hundreds of photographs, including one of the last meeting of the Kaiser and two other crowned heads of Europe in a procession before the war. He was also a talented drawer. His son recalls some quite professional-looking sketches of "Gibson Girls" he made in his youth. Since Tate was an experimental physicist, it is not surprising that in the early days of radio he assembled his own radio receiver with a crystal detector. He enjoyed attending most of the home games of the University of Minnesota football team. He liked to play a game or two of billiards or a rubber of bridge after lunch at the Campus Club, at least until the time that his life was excessively burdened by the *Physical Review* and administrative work.

## HONORS OF VARIED NATURE

In 1941 Tate was elected to the American Philosophical Society and in 1942 to the National Academy of Sciences. In his student days he was named to Phi Beta Kappa and Sigma Xi. He was, naturally, a member or fellow of the five constituent societies of the American Institute of Physics, which he helped found (viz., the American Physical Society, the Optical Society of America, the Acoustical Society of America, the American Association of Physics Teachers, and the Society of Rheology). The American Physical Society chose Jack Tate, as he was known to his friends, as its President in 1939.

He received honorary doctorates from the University of Nebraska (1938) and from the Case School of Applied Science (1945). In recognition of his services in World War II, Tate was awarded the Presidential Medal for Merit by the U.S. Government and the King George's Medal for Service in the Cause of Freedom by the British Government. The citation accompanying the Presidential Medal included the statement "With never-ceasing energy and patience, he brought to his task great technical knowledge and analytic ability which he combined with sound and dispassionate judgment. Dr. Tate's selflessness of purpose, steadfast devotion to duty and his telling contributions to the vital cause of our country cannot be measured."

The American Institute of Physics established the John Torrence Tate International Gold Medal in his honor. Appropriately, services that further international understanding and exchange are considered to be of primary importance in selecting the medal's recipient. The Tate Medal was presented to

Paul Rosbaud in 1961, to Sir Harold W. Thomson in 1966, and to Gilberto Bernardini in 1972.

## FAMILY LIFE

Tate married Lois Beatrice Fossler on December 28, 1917, in Lincoln, Nebraska. He had already moved to Minnesota a year earlier, and his bride was someone he had known while he was still on the Nebraska faculty. She soon became active in campus life, especially the activities of the Faculty Women's Club. She was very understanding of the demands of his profession, which included frequent trips away from home. Lois and Jack were avid bridge players and spent many delightful evenings with friends who shared this interest. Lois's death in 1939 made the war years, strenuous ones for all physicists, particularly trying and lonely ones for Tate.

On June 30, 1945, he married Madeline Margarite Mitchell. She had been the entire office force (other than Tate himself) of the *Physical Review* when its headquarters were first moved to Minneapolis. When the American Institute of Physics was created in 1931, she was made its Publication Manager, heading a sizable staff in New York.

He had one son, John T. Tate, Jr., by his first marriage. The younger Tate is a distinguished mathematician and a professor at Harvard University. Like his father, he is a member of the National Academy of Sciences. He is the only American-born mathematician of the celebrated "Bourbaki," the *nom de plume* of a group of mathematicians, mainly French, who have set about rewriting all the foundations of mathematics in modern terms. Thus, for two generations the name of John Torrence Tate has made its impact on the world of science.

## FINALE

Tate suffered a stroke in December 1949, but he recovered sufficiently to be able to work at a reduced rate and to attend the meetings of the National Academy of Sciences and the

American Philosophical Society the following spring. Then on May 27, 1950, he succumbed to a cerebral hemorrhage. It is tragic that America had to lose one of its leading figures in science when he was but sixty. Had he lived a year longer, he would have served as Managing Editor of the American Physical Society for a quarter of a century. Prior to his death, some of his friends were already secretly planning a special issue of the *Reviews of Modern Physics* to appear in 1951, dedicated to him in commemoration of this milestone and with articles by former students and colleagues. This issue did appear, but alas, not as a jubilee edition, but as a memorial.

The authors have benefitted from reading the biographical memoir by K. K. Darrow on pp. 325-28 of the Year Book of the American Philosophical Society for 1951. Tate's role in the Minnesota faculty is described on pp. 416-24 of The University of Minnesota, 1851-1951 by James Gray (University of Minnesota Press, 1951). The article written by Roger Stuewer for The Dictionary of American Biography contains detailed references to obituaries and archival material relating to Tate (unpublished manuscripts, tape recordings of speeches about him, etc.)

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*KEY TO ABBREVIATIONS*

Phys. Rev. = Physical Review

Rev. Sci. Instrum. = Review of Scientific Instruments

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# Melville Lawrence Wolfrom

April 2, 1900-June 20, 1969

By Derek Horton and W. Z. Hassid

Melville Wolfrom was born in Bellevue, Ohio, on April 2, 1900. He was the youngest of nine children in the family of Frederick Wolfrom and Maria Louisa (Sutter) Wolfrom. Originally, Melville's father's name was Friedrich Wolfrum, but some time before his marriage he anglicized it to Frederick Wolfrom. Melville's grandfather, Johann Lorentz Wolfrum, brought his family to America from the Sudeten German border town of Asch (now in Czechoslovakia) in 1854 and settled in a log cabin near Weaver's Corners, Sherman Township, Huron County, Ohio.

Friedrich Wolfrum attended the county schools and worked from an early age to help support the family. For many years he was in the dry goods business in a store in nearby Bellevue, Ohio, and later worked as secretary—treasurer of a local telephone company that was the forerunner of the Ohio Northern Telephone Company. He died when Melville was only seven years old; as a result, from an early age Melville was instilled with the need for self-reliance. When still quite young, he worked on odd jobs, especially during the summers. His mother had a great respect for cultural pursuits, such as music and good literature, and stimulated in him an interest in serious reading. Melville was brought up in a very strict orthodox Lutheran tradition; and, although in later years he did not adhere to this strict religious background, he consistently advocated some type of formal religious training for his children during their formative years.

During his early teens, Melville became involved in a small manufacturing business maintained in the family home. His three oldest brothers bought the patent on a type of horse harness snap that was used successfully by several fire departments. After school each day, on Saturdays and holidays, and throughout the summer vacations, Melville worked on the production of these harness snaps and was paid ten to fifteen cents an hour for his labor. During this time, he often tried to improve the devices and, as a result of this experience, determined to become a college graduate engineer with a view to a career in manufacturing.

Melville attended Bellevue High School and graduated second in the class of 1917. Stimulating teachers helped him develop an early interest in nature study, fine arts, mathematics, and German. His first encounter with science was in high school, where he learned physical geography and botany from a Mr. S. A. Kurtz. Later, he was much influenced by Mr. W. A. Hammond, with whom he studied chemistry and, later, physics. Hammond's influence was primarily responsible for Melville's decision to become a chemist, or, more specifically, a chemical engineer; the more "practical" aspect of the latter field was appealing as a result of his earlier experience in the workshop.

Being without family financial support, Melville was unable to enter college upon graduation from high school. Instead, he obtained a position with the National Carbon Company, a firm manufacturing wet and dry batteries in the nearby town of Fremont. There, in the works laboratory, he tested the quality of the daily products. Within six months he was placed, at the age of seventeen, in charge of the laboratory, with about six persons under him. Here he conducted his first research project, an evaluation of the physical properties of carbon drycell electrodes as a function of the conditions used in baking the electrodes. During the winter, he took an evening course in qualitative inorganic analysis, given at the plant. Early the following summer, he resigned his post to go to Cleveland, with the idea of entering Western Reserve University in the autumn and earning his board by waiting on tables. He worked during the summer at a boarding house and also at a variety of jobs in factories and laboratories in Cleveland that were busy at that time with war production. That autumn, the government established the Students' Army Training Corps, and Melville entered the naval unit at Western Reserve. The prescribed course of study included physics, but no chemistry. The courses were uninspiring, much disorganization resulted from the influenza epidemic, and he disliked the barracks life and the snobbish fraternity system of the school. When the armistice came in November 1918, he returned home to Bellevue feeling frustrated.

After working for a brief period as an advertising representative for a trade paper, in the autumn of 1919 he entered Washington Square College of New York University. The college unit was new and was not functioning well; no chemistry was offered. Again, he gave up his studies and returned to Bellevue, where he felt regarded as a disgrace and misfit.

The following year he worked at odd jobs, as a laborer and then as a bookkeeper. Finally, in the autumn of 1920 he entered The Ohio State University in Columbus and embarked on a course in chemical engineering, an endeavor that at last held his attention and interest and that he enjoyed greatly. For his board, he worked at boarding houses, restaurants, and cafeterias-as waiter, counter man, and dishwasher; he preferred the last kind of work. In general, he always preferred working with material things rather than with people; this preference was evident throughout his life, although he had an unexpectedly perceptive insight into the character of those people he got to know.

Young Wolfrom's first encounter with the chemistry of carbohydrates came at the end of his sophomore year, when Professor C. W. Foulk recommended him for a post as student research assistant to Professor William Lloyd Evans of the Department of Chemistry. The stipend was $250 per year, and Wolfrom put in all of his extra time on the work. During his junior year, he carried out quantitative oxidations of maltose with permanganate at various temperatures and concentrations of alkali. In his senior year, he attempted unsuccessfully to synthesize amino acid esters of glycerol. None of this work was published, but it was a good introduction to chemical research. Professor Evans, a student of J. U. Nef's, was very researchminded and inspirational. Wolfrom continued his work with Professor Evans and received the A.B. degree (*cum laude*) in 1924. The influence of Professor Evans and the other inspirational teachers of his undergraduate days was to endure throughout his career; the broad interdisciplinary approach that he took to research and the insistence upon careful observation, clear expression, and historical accuracy can all be traced to the early roots of his undergraduate training.

During every summer of his college career, Wolfrom worked at Gypsum, Ohio, with his high school chemistry teacher, W. A. Hammond, who was plant chemist for that installation of the United States Gypsum Company. Melville's well-to-do uncle, Frank A. Knapp, impressed by his nephew's progress, offered to loan him the funds to complete his college work, but Melville's mother sternly forbade him to take advantage of this offer, as it did not fit into her scheme of Spartan training for him.

Following graduation from The Ohio State University, Wolfrom moved to Northwestern University, in Evanston, Illinois, to carry out his graduate work under Professor W. Lee Lewis, a student of Nef's. He began research immediately and worked on it night and day, completing the M.Sc. degree in 1925 and the Ph.D. in 1927. His problem was to provide experimental evidence for the enediol theory advanced by Wohl and Neuberg to explain the Lobry de Bruyn—Alberda van Ekenstein interconversion of sugars in alkaline media. He observed that 2,3,4,6-tetra-*O*-methyl-D-glucose could be equilibrated with the D-*manno* epimeric in aqueous alkali and that there was no loss of the 2-0-methyl group and no formation of keto sugars. The result pointed to an enediol intermediate common to the two methylated sugars and showed that the mechanism of enol formation was not one of selective hydration and dehydration, as had been suggested by Nef, but rather was consistent with a simple keto-enol tautomerism. This work, published with Lewis in 1928, was the first in what was to become Wolfrom's remarkably prolific output of research papers on the sugars, extending over more than four decades and numbering more than five hundred individual reports. He had a phenomenal memory for detail from his early work; forty years after his paper with Lewis was published, he could still describe it in exact detail without preparation, even to remembering the values of some of the physical constants.

While at Northwestern, Wolfrom held an unusual teaching post provided by the fire insurance underwriters' association that sponsored a technical course to selected scholarship holders at the University. The program included a course in chemistry, and Wolfrom taught this course in the laboratories of the nearby College of Dentistry, holding the rank of assistant instructor.

Wolfrom was married to Agnes Louise Thompson, of Auburn, Indiana, in 1926. She had been trained at Depauw University in Greencastle, Indiana, as a public school music teacher and later did advanced work at Northwestern Univer sity, where she met her future husband. Throughout their married life, she continued to be involved in music teaching and in musical activities in the community and was ever a sympathetic and stimulating helpmate to her husband.

After receiving his Ph.D. degree from Northwestern, Wolfrom was awarded a National Research Council Fellowship that enabled him to undertake a period of postdoctoral study with some of the leading investigators in his field of interest. First, he went to study with Claude S. Hudson, then at the National Bureau of Standards, in Washington, D.C., and the undisputed leader on the American scene in research on carbohydrates. Hudson, a student of Van't Hoff's, had a strong background in physical chemistry; and his individualistic philosophy of research impressed Wolfrom greatly: his continuity of purpose, his exacting standards in experimental work, and his conservatism in theorizing until a thorough basis of facts had been recorded. All of these attributes, together with Hudson's concise and lucid style of writing, were to serve as models to Wolfrom throughout his career. It is doubtful that two such strong personalities could have long coexisted in the same institution, but Wolfrom ever after regarded Hudson as an inspiring teacher and colleague, to whom he owed a great deal. Years later, they were to be closely associated in editorial and nomenclatural work, and they much enjoyed each other's company.

After a few months in Washington, Wolfrom moved in September 1927 to New York City in order to work in the laboratory of P. A. Levene at the Rockefeller Institute for Medical Research. Levene, the outstanding master on the North American continent in the young discipline of biochemistry, was an ardent genius with a remarkable capacity for hard work and an urbane and cosmopolitan personality; he had a warm interest in all of those who worked with him. In his contact with Levene, Wolfrom was able to assimilate at first hand some of the valuable aspects of the European traditions in science that Levene was able to convey to his co-workers at the Rockefeller Institute, and he was simultaneously exposed to the enormous challenge to the structural chemist offered by the seemingly hopeless slimes and mucins that were components of animal tissues. Levene had realized that more needed to be known about the structures of the simple sugars, especially the linkage positions in the disaccharides and the ring size in cyclic monosaccharide derivatives, before he could ever hope to achieve his goal of structural elucidation in the nucleic acids. Wolfrom worked with him on these aspects, and, within a few months, a paper resulted on the Wohl degradation of cellobiose and its use in determining interglycosidic linkage-position. In quick succession thereafter were published two more papers on the ring structures of methyl D-lyxosides. In the summer of 1928 Wolfrom returned to The Ohio State University to finish up his two-year fellowship. There he worked independently on the synthesis of stable derivatives of the acyclic forms of the sugars, as such acyclic intermediates had been so often proposed as transient species in reactions of the sugars. By removing the thioacetal groups from the pentaacetate of D-glucose diethyl dithioacetal, he was able to obtain and characterize the acetate of the free aldehyde form of D-glucose; similar work in the D-galactose series followed later.

In the autumn of 1929, Wolfrom was appointed Instructor in Chemistry at The Ohio State University and one year later was raised to the rank of Assistant Professor. He remained on the faculty of the Department of Chemistry at Ohio State for the whole of his career, becoming Associate Professor in 1936 and Professor in 1940. In 1939 he was awarded a fellowship by the John Simon Guggenheim Memorial Foundation; and, in February of that year, he traveled to Switzerland to work in the laboratory of Professor P. Karrer of the University of Zürich but returned to the United States at the outbreak of hostilities in Western Europe.

With the aid of the successive generations of students who came to carry out their graduate research under his direction, Professor Wolfrom was able to launch a wide-ranging program of research, with problems of structure and reactivity in the carbohydrate field constituting the principal theme. The procedures used for obtaining acetylated *aldehydo*-D-*glucose* were systematically extended through the sugar series, and new types of aldose derivatives containing substituents on the hydrated carbonyl group were obtained; these showed the predictable behavior in being isolable in two isomeric forms, epimeric at C-1. The new well-established fact that acyclic structures can exist as reactive sugar-intermediates, sometimes having considerable stability, rests largely on his pioneering work. His first Ph.D. student, Alva Thompson, showed that the acetylated oxime of D-glucose undergoes conversion from a cyclic to an acyclic form during the Wohl degradation, and for this work Thompson received the Ph.D. degree in 1931.

Professor Wolfrom often appeared rather formidable and awesome to the new graduate student, even though he was physically only of medium height and build. He expected of his colleagues the standards of work that he set for himself. It was often difficult for lesser people to live up to his standards. His own experimental research was always done with precision, and he was proud to show that the samples he had prepared himself in the thirties were undecomposed several decades later and that their purity was unimpeachable, even by the chromatographic techniques later developed. He expected that all melting points and optical rotations recorded by his students should be as authoritative as his own experimental values.

Not every student or colleague who came into contact with Wolfrom could accept his uncompromising standards. Professor

Wolfrom chose to expend his energies in those areas where the problems could be clearly defined; and, once he had decided what was the right course, he held steadfast to that position, regardless of outside pressures. He tended to avoid becoming involved in situations where negotiations and compromises had to be made, or where the issues could not be stated in precise terms.

A hard taskmaster, Wolfrom earned the greater respect of many of his students after they had completed their work with him. Despite his rather retiring and diffident manner toward groups of people not known to him, and despite the apparent gruffness and terseness that so often characterized his day-to-day contacts with his co-workers, he actually took a deep interest in the welfare of every colleague and student who had a genuine interest in, and aptitude for, science. He went to considerable lengths to help each of his students become established in a suitable post after graduation and kept in touch with a surprisingly large proportion of them long after their departure. He had a deep insight into human personality and found it intriguing to delve into the background and motivations of each of the persons with whom he worked. This interest is reflected in the number of biographical memoirs that he chose to write, especially of his early mentors; these were done with characteristic thoroughness and show his perceptive qualities in understanding human nature.

Although he never regarded lightly any of the work he undertook, Wolfrom had a very strong sense of humor, not always recognized by those who did not know him well. He had an endless store of anecdotes concerning the personalities of science, based on his own contacts with other scientists and on his wide reading of the history of science; the humor of his dry remarks would once in a while be betrayed by a fleeting smile. This side of his personality was most in evidence when he was with small groups of people he knew well and with small classes of advanced students who were perceptive enough to appreciate the subtleties of his comments.

His approach to teaching was always based on a solid, historical foundation that traced the development of science through the major milestones of factual knowledge, rather than through rationalizations and correlations that involved extrapolation of existing information.

At the graduate level, where he supervised almost a hundred Ph.D. students and numerous M.S. candidates, Professor Wolfrom made his major educational contribution. With these students he was able to pursue research on several broad fronts in the field of the carbohydrates. (A list of Wolfrom's published articles and the participating co-workers is given at the end of this memoir.) In the early days, most of the research students were employed as part-time teaching assistants in chemistry at Ohio State. Later, and especially after World War II, outside funding through grants and contracts from government and industry became available, and Wolfrom was able to expand his research program further. The research group was enriched by a regular succession of postdoctoral associates who came from other institutions for one or two years of experience in Professor Wolfrom's laboratory. The group became very cosmopolitan, always containing members from Europe and Asia, and Wolfrom particularly appreciated the new ideas and techniques brought in by these colleagues who had received their doctoral training in other laboratories.

Throughout his career, Wolfrom's early theme of research on the acyclic forms of the sugars continued; in fact, one of his posthumous articles is a book chapter on the subject. Extending the route developed for *aldehydo*-D-glucose pentaacetate, he devised general methods for obtaining crystalline acetates of those sugars in which the carboyl group, aldehydic or ketonic, was present in the free form, uncombined with any hydroxyl group of the sugar chain; and the general chemistry of the hydrated carbonyl group was explored. A synthesis of highercarbon ketoses by the action of diazomethane on acetylated aldonyl chlorides was established that led to the preparation of acyclic *keto*-acetates that, on deacetylation, gave larger chain ketoses. It was shown that the keto-acetates could be used for the synthesis of branched-chain structures. In cooperation with T. M. Lowry of Cambridge University, Wolfrom conducted pioneer work on the optical rotatory dispersion of the acyclic sugar acetates and demonstrated the Cotton effects attributable to the asymmetrically perturbed absorption of the carbonyl group. It was demonstrated that many of the hydrazones and osazones of the sugars were either totally acyclic or contained such a structure as a significant tautomeric form. He recognized at an early stage the potential of nuclear magnetic resonance spectroscopy in structural chemistry and applied it in 1962 to show that an "anhydro-phenylosazone" that had been prepared in his laboratory in 1946 possessed an unexpected, unsaturated phenylazo structure.

The chemistry of the dithioacetals of the sugars was explored in detail, and many useful synthetic transformations were demonstrated. A notable development was the reductive desulfurization of the dithioacetals to the hydrocarbon stage. This reaction was used to establish a major milestone in the chemistry of natural products, the unambiguous correlation between the configurational standards of D-glyceraldehyde for the sugars and L-serine for the amino acids; the correlation was achieved by way of the diethyl dithioacetal of 2-amino-2-deoxy-D-glucose, which was transformed into a derivative of L-alanine without disturbing the configuration of the asymmetric center at carbon atom two.

In the chemistry of dithioacetals Wolfrom also prepared the first dithioacetal of a ketose (D-fructose), and established the technique of mercaptolysis for the fragmentation of polysaccharides. In other hands, the technique of mercaptolysis has been applied successfully for determination of structure, notably with the seaweed polysaccharides agar and carrageenan. The acetylated dithioacetals were shown to be useful characterizing derivatives for the sugars, and subsequent workers have utilized these derivatives extensively for determination of the gross structures of sugars by mass spectrometry. Dithioacetal derivatives were also significant in Wolfrom's work on the structure of the antitubercular antibiotic streptomycin, playing a role in the elucidation of structure of the streptose component. The configuration of the streptidine entity was established by its synthesis from 2-amino-2-deoxy-D-glucose, and further contributions were made on the structure and configuration of the entire streptomycin molecule.

Synthetic methods were developed for amino sugars by displacement of sulfonyloxy groups by nitrogen nucleophiles and applied especially for the synthesis of 2-amino-2-deoxypentoses, until the complete series of eight stereoisomers had been elaborated. Procedures for protecting the amino group were established that led to the successful synthesis of nucleosides containing 2-amino-2-deoxy sugars in the furanosyl form. This work formed part of an extensive program of synthesis of nucleoside analogs having structural variation in the carbohydrate moiety, as potential anticancer agents.

Professor Wolfrom was always concerned with planning research in a logical, orderly way, and he undertook to fill in some of the gaps left by Emil Fischer in the systematic elaboration of the simple sugars. These included the crystalline forms of racemic glucose, racemic glucitol, D-glucose dimethyl acetal, L-fructose, racemic talitol, L-talitol, and xylitol. For key crystalline compounds, he made a special point of recording the data from an X-ray powder diagram as an unequivocal fingerprint of the compound in the particular crystalline modification: he had little faith in syrups unless a suitable crystalline derivative could be prepared. He would accept chromatographic evidence as a tool for monitoring reactions, but only as a preliminary guide to characterization by a definitive method, preferably through a crystalline compound.

Wolfrom investigated a number of problems of technological interest. The formation of color in sugar solutions as present in food products and during sugar refining was investigated. Products of the dehydrating reactions favored by acidity were identified, and the mechanism of the nonenzymic browning (Maillard) reaction between sugars and amino acids was examined; a reactive 3-deoxyhexosulose intermediate was established for the latter reaction, a finding that provided the basis for extensive work in other laboratories. It was demonstrated that the action of alkali on reducing sugars leads to stepwise enolization down the sugar chain. The composition of cane sugar molasses was examined in detail. In a study on the alkaline electroreduction of D-glucose, it was shown that carbonyl groups are reduced completely to the hydrocarbon stage and that a side-product is a twelve-carbon atom derivative formed by way of an aldol reaction.

Electron paramagnetic resonance studies were conducted by Professor Wolfrom and his group on the remarkably stable, free radicals formed when sugars in the solid state are irradiated. The chemical transformations taking place during the controlled ignition of cellulose nitrate were investigated extensively in a project for the armed services. Other nitrated polyhydroxy compounds were also investigated for potential uses as explosive polymers.

The biosynthesis of cotton bolls was investigated by preparing photosynthetically $^{14}$C-labeled cotton celluloses. *β*-D-Glucopyranosyl phosphate, the anomer of the common -D-glucopyranosyl phosphate, was synthesized; and a synthetic route to L-iduronic acid was devised. Both of these products were subsequently found by others to occur naturally.

New techniques for working with carbohydrates and their derivatives, notably in separation methods, were devised in Professor Wolfrom's laboratory. Extrusive column chromatography was developed as a valuable tool for the separation of mixtures of acylated sugars and was utilized particularly in studies on oligosaccharides, including the characterization of several polymer-homologous series of oligosaccharides. Procedures involving ion-exchange resins were introduced into the carbohydrate field, and the use of microcrystalline cellulose for thin-layer chromatography of unsubstituted sugars was developed. The use of sodium borohydride to reduce free sugars to alditols was first reported by Wolfrom's group, as were the first examples of arsenate, benzeneboronate, and urethan derivatives of the sugars, and the use of absolute hydrogen sulfate as a solvent. Also developed were reliable analytical procedures for the determination of acetyl and methoxyl groups in carbohydrates containing them.

Professor Wolfrom devoted many years to the determination of the structure of various polysaccharides. The most challenging of these was heparin, the natural blood anticoagulant. Methods were found for modifying the intractable "backbone" chain of this polymer, notably by use of diborane to reduce the uronic acid moieties, to give a derivative amenable to structural characterization by the method of fragmentation analysis. By means of crystalline disaccharide fragments that were unequivocally characterized, it was shown that *N*- and *O*-sulfated 2-amino-2-deoxy-D-glucopyranose and D-glucopyranuronic acid residues, connected by -D-(1 • 4) linkages, are present in the polymer, and that L-iduronic acid residues also occur. Other animal polysaccharides investigated by Wolfrom were chondroitinsulfuric acid and the galactan of beef lung.

Molecular structures of starch and glycogen were extensively investigated; evidence for the branch points at carbon atom six was placed on a firm, crystalline basis by the method of fragmentation analysis. Incidental to this work, the nature of reversion of sugars by acids was interpreted. Synthetic confirmation of the structure of the branch-point disaccharide, namely isomaltose, involved the difficult step of introducing the -D-linkage in the interglycosidic position; this was achieved by a modification of the Koenigs-Knorr synthesis, with the use of a nonparticipating protecting group at carbon atom two in the glycosyl halide derivative. A similar approach was subsequently used for the synthesis of panose, a trisaccharide fragment involved at the branch points in the polysaccharide. Structures were also established for the mannan and arabinogalactan of the green coffee bean, and the presence of these polysaccharides in commercial coffee extracts was established.

Detailed structural investigations were made on British gums, produced commercially by heat treatment of starch. In the quest for novel derivatives of starch having potential utility in industry, various acetal and unsaturated ether derivatives were studied, and routes were developed for the synthesis of amino derivatives of starch having the hydroxyl group at carbon two replaced by an amino group; the latter were used for preparing polymers having structures related to that of heparin. In the cellulose field, comparative studies were made on various series of cello-oligosaccharide derivatives as models for the parent polymer; these investigations included oxidation with alkaline hypochlorite as related to the industrial extraction and bleaching of cellulose fibers.

Professor Wolfrom had a long-standing interest in the pigments occurring in the osage orange (*Maclura pomifera* Raf.), a common hedge-tree found in Ohio. The chemical nature of two complex phenolic pigments present in the fruit of this plant was elucidated, and a synthesis of their skeletal components was effected. These compounds were the first for which it was established that isoprenoid units were condensed on the nucleus of a common plant-pigment, in this case, an isoflavone; other examples have since been found in plants. Three phenolic pigments containing isoprene units condensed on a xanthone nucleus were discovered in the root bark of the same plant, and it was found possible to elucidate their structures, mainly by use of spectroscopic techniques; one of the pigments was synthesized. Two of them were found to contain an isoprenoid unit in the form of a 1,1-dimethylallene group; they were the first examples discovered of natural phenolic compounds so constituted.

In 1948 Wolfrom assumed the duties of Head of the Organic Division in the Department of Chemistry at The Ohio State University, the department then being under the chairmanship of a physical chemist, Edward Mack, Jr., who, in 1941, had succeeded William Lloyd Evans as departmental chairman. Wolfrom's responsibilities included coordination of the courses and of the requirements for graduate degrees in organic chemistry. He also served for many years on the departmental Library Committee. Thorough as always in the tasks he undertook, he played an important part in developing an excellent chemistry library, both as regards the extent of coverage and the completeness of the collection of early books and periodicals. In 1960, Wolfrom was named Research Professor, and the responsibilities of the Organic Division were passed on to M. S. Newman. In his new position, Professor Wolfrom was able to concentrate more on his individual teaching effort at the graduate level, although he continued to present courses in the chemistry of carbohydrates. His office was a very modest one, in a long corridor of small research laboratories affectionately known to successive generations of occupants as "Sugar Alley." He spurned the opportunity to move into more spacious and modern quarters when the new Evans Laboratory was added to the department in 1960; he felt a sentimental attachment to the antiquated laboratories whose dust was, as legend had it, rich in the seeds of myriad crystal species. Fact or fiction, there is little doubt that the Wolfrom group had an impressive record of success in bringing recalcitrant syrups of sugars to crystallization.

Professor Wolfrom influenced the carbohydrate field in many ways far beyond even his own exceedingly prolific contributions to its literature. He provided the motivation for many others to pursue work in the area. A surprising number of the persons who, at one time or another, have worked in Wolfrom's laboratory have continued independent research on the carbohydrates; those in academic positions include H. El Khadem (Houghton), A. B. Foster (London), S. Hanessian (Montreal), R. U. Lemieux (Edmonton), G. E. McCasland (San Francisco), R. Montgomery (Iowa), K. Onodera (Kyoto), A. Rosenthal (Vancouver), F. Shafizadeh (Montana), the late J. C. Sowden (St. Louis), W. A. Szarek (Kingston), J. R. Vercellotti (Virginia), R. L. Whistler (Purdue), and D. Horton (co-author of this memoir).

Exceptionally well organized, Professor Wolfrom hated wasting time. He handled much of his business by telephone, even with persons in the room next to his office. These calls would be brief and often blunt, as were his letters and notes; he always made his point with maximum impact in as few words as possible. Although he derived great satisfaction from his work, chemistry was by no means Professor Wolfrom's sole preoccupation. He read widely in the classics and history and enjoyed building a fine library in his home. Not an instrumentalist himself, he nevertheless shared his wife's love of music and worked with her in helping the development of the Columbus Symphony Orchestra and in other musical and cultural activities in the community. Together, they also enjoyed the theater, ballet, and the fine arts, both in the local community and during their travels. The Wolfroms frequently received groups of colleagues, students, and visiting scientists in their spacious home on the north side of Columbus. Mrs. Wolfrom, a most gracious hostess, would often entertain the guests with a musical recital at the piano, and Professor Wolfrom was happy to show guests the pleasant garden, the cultivation of which was a source of great enjoyment to him. On other occasions, he would lead the members of his research group and their families on picnic trips into the surrounding Ohio countryside; these expeditions were strictly recreational, although it must be said that, should Wolfrom spot some osage-orange trees along the way, a "work gang" might rapidly be delegated to collect samples.

Five children were born to the Wolfroms: Frederick (who died shortly after birth), Eva Magdalena, Anne Marie and Betty Jane (who were twins), and Carl Thompson.

Because of Wolfrom's precise style of writing and his concern for an accurate historical record, he felt the need for a periodic series of authoritative articles on various aspects of research on the carbohydrates, to be written by qualified specialists and supervised editorially through a rigorous policy in order to ensure extremely high standards of consistency and accuracy. As a result of this idea, near the end of World War II the *Advances in Carbohydrate Chemistry* series was developed. The policies were formulated by an Executive Committee consisting of W. L. Evans, H. O. L. Fischer, R. Maximilian Goepp, Jr., W. N. Haworth, and C. S. Hudson, together with Wolfrom and his co-editor, W. Ward Pigman. With the enthusiastic help and collaboration of publisher Kurt Jacoby and the then-fledgling Academic Press, the first issue of the series was launched in 1945. Wolfrom remained a prime mover in this annual series for the rest of his life and was editor or co-editor of all volumes through Volume 24, except for those of 1950 and 1951. Under his guidance, the series has reflected all of the major significant developments in carbohydrate chemistry and biochemistry, through timely contributions written by authorities in the field. His broad knowledge and critical ability, his attention to editorial detail, and his insistence on "getting it right," have given the series an excellent reputation for quality and reliability. Not even the most eminent authors were immune from his pungent remarks if their manuscripts failed in any way to meet the standards demanded. His respected colleague Claude S. Hudson worked closely with Professor Wolfrom on the early volumes of *Advances;* following the death of Hudson, in 1952, Wolfrom invited R. S. Tipson to join in editing the series. The Wolfrom-Tipson editorial partnership for *Advances* continued for eighteen years thereafter. With the addition of representation from the British Isles on the board of *Advances,* starting with the second volume in the series, a close link was established between British and American carbohydrate chemists that in subsequent years led to much fruitful cooperation, especially in the field of carbohydrate nomenclature.

In collaboration with R. L. Whistler, Wolfrom served as co-editor or consulting editor for the series *Methods in Carbohydrate Chemistry.* These collections of experimental procedures in the carbohydrate field have proved an invaluable standby for research workers. The international journal, *Carbohydrate Research,* inaugurated in 1965, also received strong support from Professor Wolfrom, who served on its Editorial Advisory Board.

For a quarter of a century, Professor Wolfrom worked on the systematization and codification of carbohydrate nomenclature. He was a member of an American Chemical Society committee chaired by R. C. Hockett that developed, during the period 1945-1948, the rules of carbohydrate nomenclature that the society's council approved in 1948. The committee continued to consolidate and extend the rules, this time in cooperation with chemists in Great Britain, and in 1951 Professor Wolfrom became chairman of the committee. The joint study of carbohydrate nomenclature by British and American chemists furnished an excellent example of effective cooperation to improve the language of science for clear and exact reporting of scientific information. A set of rules under joint British-American sponsorship was published in 1953, and as a result of continued cooperative work the rules were extended and further clarified; a revised set of jointly approved rules was issued in 1963.

Wolfrom's committee then continued to work in the development of nomenclature systems for the carbohydrates, in order to accommodate the special requirements of newly developing research areas and to encompass areas, such as the polysaccharides and conformational terminology, not covered in the published rules. At the same time, he sought to develop full international acceptance of the rules by working actively with the Special Committee on Carbohydrate Nomenclature of the Organic Commission of the International Union of Pure and Applied Chemistry. He met several times with the international committee and laid much of the important groundwork for the set of international rules drafted by that body.

In 1959 Professor Wolfrom assumed the duties of Section Editor for the carbohydrates section of *Chemical Abstracts,* a task that he undertook with characteristic thoroughness. All abstracts for that section were carefully checked to ensure that the names used conformed to the approved terminology, and the scientific content of the abstracts was also checked, usually against the original article. Not infrequently, he himself rewrote those abstracts he found unsatisfactory. In 1964 he was made a member of the Board of Advisors for *Chemical Abstracts.*

For his outstanding services in the field of chemical documentation, Professor Wolfrom received in 1967 the Austin Patterson Award, sponsored by the Dayton section of the American Chemical Society. At the award ceremony, many of his old friends and colleagues were present, including Dr. W. A. Hammond, his high school chemistry teacher.

Besides his contributions in the field of chemical documentation, Professor Wolfrom served on numerous committees and held a number of offices in professional societies. From 1940 until 1945, he was an Official Investigator of the National Defense Research Committee. A member of the American Chemical Society, he served as Chairman of the Columbus section and also of the Cellulose Division in 1940, and of the Division of Sugar Chemistry in 1948. In 1958 he was Chairman of Symposium I of the International Union of Biochemistry, in Vienna, and for this he was honored by a citation from the Austrian government. He was a member of the American Society of Biological Chemists, Phi Beta Kappa, Sigma Xi, Phi Lambda Upsilon, Pi Mu Epsilon, and Alpha Chi Sigma. He was a member of the National Committee of the Phi Beta Kappa Book Award in Science from 1961 to 1963 and served as its Chairman in 1963. He was a Fellow of the American Academy of Arts and Sciences, the New York Academy of Science, the Ohio Academy of Science, the American Association for the Advancement of Science, and The Chemical Society (London). In 1959 he was an invited lecturer in the Biochemistry Department at Tufts University Medical School in Boston, Massachusetts.

Numerous other honors and recognitions for his work were bestowed on Professor Wolfrom. In 1950, he was elected to the National Academy of Sciences, and, in 1952, was presented the Honor Award (now the Hudson Award) of the Division of Carbohydrate Chemistry of the American Chemical Society. In 1965, he was honored by The Ohio State University by being named Regents' Professor, a title created at that time to recognize exceptional distinction in scholarly activity at the university. In 1967, he was honored by the Kansas City section of the American Chemical Society with the Kenneth A. Spencer Award for his contributions to agricultural chemistry.

Still at the height of his effectiveness as a scientist, Professor Wolfrom was, in mid-1969, actively planning new research programs to take effect beyond the nominal age for retirement at seventy years. Tragically, an aortic aneurysm, found during a routine physical examination, ruptured several days after its discovery, just hours before a proposed surgical repair could be effected; Professor Wolfrom died in Columbus on June 20, 1969. It is particularly indicative of his methodical and organized personality that, on the day before he was to enter the hospital, he visited each of his research students to plan work for the following few weeks, answered all of the correspondence on his desk, and left instructions for handling the various items of business that were expected to arise. He was survived by his widow, four children, three sisters, and seven grandchildren.

A special issue of the journal *Carbohydrate Research,* comprised of research contributions by former students of Professor Wolfrom's, was published as the Wolfrom Memorial Issue in April 1970, on the seventieth anniversary of his birth. Also dedicated to his memory was the program of papers presented in Toronto, Ontario, in May 1970, at the joint meeting of the Carbohydrate Division of the American Chemical Society and the Canadian Institute of Chemistry. In addition, a special tribute to him was paid at the Fifth International Conference on Carbohydrates held in Paris in August 1970.

Professor Wolfrom's life was a noble example of devotion to the pursuit and advancement of science. His dedicated and sincere personality won the response and admiration of those who had the privilege of knowing him. His important contributions to carbohydrate chemistry will remain a permanent record in the annals of chemistry. He has erected to his name an enduring memorial and has left impressed in the pages of science a monolithic achievement that can serve as an inspiration and a challenge to others. Even more important, his spirit and ideas made a lasting influence on a whole generation of new scholars, so that the qualities for which he stood can continue to flourish.

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