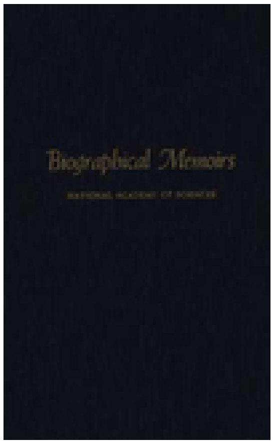

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Preface

The *Biographical Memoirs* is a series of volumes, beginning in 1877, containing the biographies of deceased members of the National Academy of Sciences and bibliographies of their published scientific contributions. The goal of the Academy is to have these memoirs serve as a contribution toward the history of American science. Each biographical essay is written by an individual familiar with the discipline and the scientific career of the deceased. These volumes, therefore, provide a record of the lives and works of some of the most distinguished leaders of American science as witnessed and interpreted by their colleagues and peers. Though the primary concern is the members' professional lives and contributions, these memoirs also include those aspects of their lives in their home, school, college, or later life that led them to their scientific career.

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Biographical Memoirs

Volume 54

Photograph by John Edwards

Jesse Wakefield Beams December

25, 1898-July 25, 1977

by Walter Gordy

Jesse W. Beams ranks among the greatest experimental physicists whom America has produced, a group that includes such men as Joseph Henry, Robert W. Wood, and Ernest 0. Lawrence. Although he carried out many ingenious experiments, he is best known for his development and diverse applications of the centrifuge. His experiments with the centrifuge began in the early thirties and continued until his death. Their impact on science and technology has been enormous.

EARLY LIFE IN KANSAS

Jesse Beams was born on a farm in Sumner County, Kansas on Christmas Day 1898. His parents were frontier people in the true American tradition of the nineteenth century. His father, Jesse Wakefield Beams, senior, while yet a boy, went west from Kentucky, across the Mississippi River. At the age of seventeen he was driving herds of longhorn cattle from Texas to the prairies of the Middle West. Later, he settled on a farm in Sumner County, Kansas. Jesse's mother, Kathryn Wylie, migrated with her parents in a covered wagon from what is now West Virginia to Kansas. After a long and difficult journey, the family settled south of Wichita.

Jesse was a son in his father's second family. His father's first wife died after there were four children in the family, two boys and two girls. Sometime after her death, Jesse's father met Kathryn Wylie, whom he married. They had two children, Jesse and a younger brother, Harold, who grew up to be a distinguished biologist, a professor at the University of Iowa.

Those who seek a genetic or social basis for outstanding achievements and academic excellence may wonder why the two children of the second family of Jesse Beams, Sr., reared on the same farm, grew up to be distinguished scientists and professors whereas none of the children of the first family, so far as I could learn, became known scholars or scientists; apparently, they followed the farm life of their parents. Although Kathryn Wylie's family also lived on a farm, one of her brothers became a physician.

Jesse's outstanding accomplishments could hardly be attributed to early academic opportunity. His first seven years at school were spent in a one-room schoolhouse, several miles from his isolated farm home. He walked to school, or skated when there was ice and snow. Skating on the river, he said, was the easiest way to get to school on cold days. Although the teacher he had must have been excellent, the instruction he received in the first seven grades had to be meager. Anyone familiar, as I am, with the one-room school knows that a single teacher of several grades has little time for teaching any one student or even any one grade. After school there was little time for study because of the heavy assignments of farm "homework"—husking corn, pitching hay, and milking cows. Despite his skimpy grade-school training, Jesse went on to graduate from high school with distinction.

Among Jesse's duties on the farm was the turning of a centrifuge cream separator. Can it be that his lifelong fascination with the centrifuge originated from this hand-cranked separator rather than from something he read in a book? From early childhood he was exposed to spectacular displays of natural phenomena. Many times he must have watched the swirling dust of the whirlwinds that frequently dance over the Kansas plains in summer. He certainly was deeply impressed by the awesome displays of lightning streaking over the wide Kansas skies followed by rumbling thunder. Second in importance to the centrifuge in Jesse's physical experiments were those designed to gain information about electrical discharges, including lightning itself.

While it is easy to connect Jesse Beams's remarkable experiments in physics with his early experiences on the Kansas farm, there were thousands of children brought up on farms of the western plains who undoubtedly participated in the same farm operations, who saw over and over again the manifestations of the same natural phenomena without being so motivated to explore them. There must have been something different in the makeup of the boy Jesse that caused him to see more than the others did, to crave more than they to understand what he saw.

Jesse Beams obtained his undergraduate training at Fairmount College, in Wichita, where he worked at various jobs to pay his expenses. He achieved high honors and was president of his senior class. In consideration of his fascination with physical phenomena, it is not surprising that he chose physics as his major subject. In 1959 his alma mater, which had then become the University of Wichita, conferred upon Jesse the distinguished Alumnus Award.

GRADUATE EDUCATION IN PHYSICS, 1921-1925

After graduation from Fairmount College in 1921, Jesse attended the University of Wisconsin for one year and obtained the M.A. degree in 1922 with a major in physics. In the fall of 1922 he interrupted his graduate education to accept an instructorship in physics offered him by Fred Allison, chairman of the Physics Department of Alabama Polytechnic Institute, now Auburn University. Although he remained at Auburn only one year, he greatly impressed Fred Allison with his exceptional ability as an experimentalist. Much credit must be given to Allison for the future course of Beams's career. At this critical period he urged Jesse to complete his graduate education at the University of Virginia, where he had obtained his own Ph.D. in experimental physics. No doubt Allison was greatly responsible for Jesse's being offered a teaching fellowship at the University of Virginia for 1923 and 1924 and for his decision to accept the offer. It is not surprising that Jesse chose as his thesis director Professor Carroll M. Sparrow, who had directed the thesis research of Fred Allison.

The thesis project that Professor Sparrow assigned to Jesse may have been as exciting to him as lightning over the Kansas farm. Sparrow, proposed that he measure the time interval between the arrival of the quantum and the ejection of the electron in the photoelectric effect. Although Jesse did not achieve this objective for his Ph.D. thesis, his attempts to do so did lead to the development of experimental techniques and instruments that he and others used later for many important experiments. With light from a high-intensity spark source that was reflected from a mirror rotating at high speed, he produced extremely short flashes of light for which the onset and duration were measured with an ingenious light-switching mechanism he developed. The light switch was a Kerr cell that had electrical delay lines differing in length between the activating voltage, which opened the switch, and the spark gap, which shorted out the voltage and thus closed the switch. This system proved capable of measuring time intervals down to a hundred-millionth of a second. By employing liquids of very low viscosity for the isotropic medium in the Kerr cell, he found that the switching time within the cell itself could be made negligible. He used these devices to measure, among other things, the relative interval of time between the excitation and the emission of certain fluorescent spectra and the relative times of the appearance of different lines of a spectrum after excitation.

THE YALE YEARS, 1926-1928

Upon receiving the Ph.D. at Virginia in 1925, Beams was awarded a National Research Fellowship, which he held for two years, the first year at Virginia and the second at Yale. He had the good fortune at Yale to meet and work with Ernest O. Lawrence, a young experimental physicist of considerable imagination and skill, who, like himself, had been reared on an isolated midwestern farm. Their elementary education, or lack of it, was quite similar. Both attended small midwestern colleges, obtained the M.A. degree from a midwestern university, and received the Ph.D. degree in 1925 from an eastern university (Ernest, from Yale). But these two young physicists had something in common that was far more important than their parallel experiences in farm life and education. Both were fired with insatiable curiosity about the physical world, and both possessed exceptional talent for exploring it. They were destined to become leading experimental physicists of the twentieth century.

At Yale, Beams and Lawrence collaborated on several studies, primarily on experiments concerned with measurements of short time intervals, which probably evolved from Jesse's Ph.D. research. After further refinement of the techniques that he developed at Virginia, Beams, with Lawrence, returned to the problem assigned to him by Professor Sparrow for his Ph.D. thesis: measurement of the time interval between the light quantum and the ejection of the electron in the photoelectric effect. By this time, physicists, including

Beams and Lawrence, had become more aware of their limitations with respect to gaining experimental information about the interactions of individual quanta with single electrons. They consequently adopted the more realistic goal of measurement of the time between impending flashes of light and the onset of photoelectric emission. Although this interval of time proved too short for them to measure, they were able to set definitive upper limits for the intervals. They concluded, for example, that photoelectric emission begins in less than 3 x 10^{-9} seconds after the beginning of illumination of a potassium hydride surface.

Probably the most widely known collaborative effort that Beams and Lawrence made was their attempt to chop light quanta into segments by means of an air-driven, high-speed, rotating mirror. In a related experiment, they tried to measure the length of a light quantum. These experiments, though doomed to fail, were bold, suggestive ones at this stage in the development of quantum theory. Evidence that Beams and Lawrence recognized these experiments as far out on the border line of the knowable is revealed in their statement: "There is no definite information on the length of time elapsing during the process of absorption of a quantum of energy photo-electrically by an electron, and [furthermore] the so-called length of a light quantum—if such a concept has meaning—is equally unknown experimentally." [1]

RETURN TO VIRGINIA

After the expiration of his National Research Fellowship and a year spent as an instructor at Yale, Jesse Beams returned to the University of Virginia in the fall of 1928 as an associate professor of physics. This appointment proved to be

[1] J. W. Beams and E. O. Lawrence, "On the Nature of Light," *Proceedings of the National Academy of Sciences of the United States of America,* 13 (1927):207.

fortunate for the university as well as for Jesse Beams. At that time, L. G. Hoxton, chairman of the Physics Department, was concerned about the state of the program of graduate studies and research in physics and was anxious to build them up. As future events proved, he could not have done better than to attract young Beams back to his alma mater, even at a two-rank promotion over his Yale instructorship. In his history of the Physics Department of the University of Virginia, F. L. Brown, professor of physics at the University of Virginia from 1922 to 1961, began the chapter concerning the period from 1928 to 1936 with this statement: "With the return of Dr. J. W. Beams to the University of Virginia as associate professor a new period of growth and development can truly be said to have begun."[2] Increasing numbers of physics students of high quality chose Virginia as their graduate school and Beams as the director of their thesis research. These students came first from the southern states, then later from throughout the nation as Beams's reputation as a clever experimentalist spread. Two students who came early to work with him were Edward P. Ney of the University of Minnesota and J. C. Street of Harvard, both now members of the National Academy of Sciences.

There were no government grants when Jesse returned to Virginia in 1928 and apparently no state funds allocated for research in physics. At that time graduate students supported themselves by teaching the undergraduate laboratories. Fortunately, minimal funds were required for research equipment and supplies. A year later the financial outlook was notably improved; the Du Pont Company established several fellowships at the University, some of which were available for physics. About the same time, a fund for research in the physical sciences was established by the General Education

[2] F. L. Brown, *A Brief History of the Physics Department of the University of Virginia, 1922-1961* (Charlottesville: University of Virginia, 1967), ch. 5, p. 1.

Board, apparently with an agreement that the State of Virginia would contribute enough to maintain the fund at a level of $45,000 a year, of which the physics department was to receive a maximum of $11,670.[3] Although paltry indeed in comparison with present levels of support for physics research, these funds in support of the ingenious experiments of Jesse Beams had an enormous impact on the development of science in this country. What influence Jesse's return had on these encouraging developments in the physics program at Virginia I do not know, but I suspect it was considerable.

Evidence that the administration recognized Beams's worth to the University was his promotion to a full professorship in 1930, only five years after he received his doctorate there. Lest the reader conclude that the administrators of the University of Virginia in the predepression years differed from university administrators today in their rapid, voluntary recognition of the worth of a young staff member, I shall briefly indicate how Jesse's promotion to professorship came about.

According to his wife, Maxine, while Jesse was an associate professor at Virginia he received a "wonderful offer" from another university. Though she did not mention the name of the university, I concluded that it was somewhere in the Midwest, near his native Kansas. The offer was so attractive that he went for an extended visit to consider it. While away he became inclined to accept the offer.

Upon his return, he went to the president of the University of Virginia to resign his position. The president responded, "Young man, you are just causing me much trouble." Then he quickly offered to raise Jesse's salary and to promote him to full professorship.

[3] *Ibid.*

Having concrete evidence that his talents were appreciated by the highest levels of the university administration, Jesse never again came so close to leaving the University of Virginia, despite the many wonderful offers he received through the years. Whenever he received an enticing offer with a considerably higher salary than he was receiving, Jesse would ask Maxine what he should do. Each time she gave him the same answer, "Jesse, you should do what you want to do, what you think is best." Each time the result was the same —he refused the offer and after the decision was made, again to quote Maxine, "He was so happy."

DEVELOPMENT OF THE ULTRACENTRIFUGE

After 1930 Beams's principal research programs were concerned with axially rotating systems from the very, very fast to the very, very slow. This does not mean that his programs lacked breadth and diversity—far from it. Under his continuous cultivation the centrifuge became a family of instruments capable of solving a variety of basic problems in chemistry and biology as well as in physics; it had many important technological or industrial applications, from testing the strength of materials to the separation of uranium isotopes for nuclear energy. He converted the centrifuge, capable of rotating only a few thousand times a minute, to the ultracentrifuge, capable of rotating a hundred million times a minute (~1.5 million rotations per second), with peripheral speeds greater than 2500 miles an hour. At the highest speed, the peripheries of some of the small, spherical rotors experience a force of acceleration a billion times that of the earth's gravitation. The speed is limited only by the strength-to-density ratio of the material composing the rotor. The rotor is magnetically suspended in a highly evacuated container, in which the resistance to rotation is so small that the rotor, once set in motion and allowed to coast, would continue to rotate for many years without a driving force.

To appreciate the difficulties Beams and his group had to overcome to produce the ultracentrifuges that rotate up to 1.5 million times a second, let us review briefly the history of the development of the centrifuge to the time he began working with it. The simplest centrifuge is one mounted on a shaft and rotated by some external system attached to the shaft, such as the motor-driven wheels of an auto or the rotating blades of an electric fan. Alternately, a moving fluid may be used to drive the shaft-mounted rotor, as was done for centuries in waterwheels and windmills. Serious difficulties are encountered when one attempts to spin the shaft-mounted rotors at speeds up to a few hundred rotations a second. These difficulties come from inability to make the inertial axis of the rotor coincide exactly with the axis of the shaft about which it is forced to turn. Anyone driving a car at high speeds knows the problems caused by wheel imbalance, but the wheels of a car driven at the national speed limit make only a dozen turns a second.

In 1883 a Swedish engineer, Carl G. P. de Laval, overcame some of the difficulties by mounting a steam-driven turbine rotor on a long, flexible shaft that could shift under the force of an imbalance to the inertial axis of the turbine wheel. With this innovation, de Laval constructed a small steam turbine capable of turning at seven hundred rotations a second. Between 1920 and 1925, Theodor Svedberg, at the University of Uppsala, with meticulous design and exceptional workmanship, constructed small centrifuges mounted on non-flexible shafts, which achieved rotational speeds of the order of a thousand rotations a second. When the rotor was mounted under hydrogen gas at subatmospheric pressures to reduce frictional heating, Svedberg succeeded in separating out and weighing large biological molecules through the molecular sedimentation produced by centrifugal fields up to approximately a million times the gravitational field. His well-known experiments won for him the Nobel Prize in 1926.

The early design of the centrifuge from which Beams learned most appears to be that made by two Belgian scientists, E. Henriot and E. Huguenard, who produced a shaftless, air-driven rotor and suspended it in space by a jet of air. The unattached rotor was free to spin in stable equilibrium about its own inertial axis of rotation. The suspension of the rotor in space is an application of Bernoulli's principle, which will be familiar to those who have had a first course in physics. With this type of centrifuge, rotors an inch in diameter can be spun up to four thousand rotations a second. The principal deterrent is the frictional resistance of the air.

This brief summary brings the history of the centrifuge to the time when Jesse Beams became involved with its development and applications. In his article, "Ultrahigh-speed Rotation,"[4] he wrote:

> It was this system [referring to that of Henriot and Huguenard] that came to our attention in the late 1920's when Ernest O. Lawrence and I were looking for a way to make high-speed photographs of the breakdown of electric sparks and of other phenomena of very brief duration. By mounting a mirror on an air-driven rotor we were able to build a high-speed camera that met our needs. This was my introduction to high-speed rotation.[5]

Back at Virginia in the early thirties, Jesse had begun to dream of the many important new applications that would be possible if the rotational speed of the centrifuge could be increased from the few thousand rotations a second then available to a million or more rotations a second. Conse

[4] J. W. Beams, "Ultrahigh-speed Rotation," *Scientific American,* 204(1961): 135-47. *Ibid.,* pp. 138, 140.

[5] *Ibid*, pp. 138,140

quently, he concentrated on the factors that restricted the speed of previously designed rotors and began his protracted efforts to overcome them. It is interesting that his close friend and coworker, E. O. Lawrence, whom he had left at Yale, was at the same time concentrating his inventive talents on making electrons whirl in circles, faster and faster, about a common axis. At Virginia I was told that a friendly competition existed between Beams and Lawrence, who was then at Berkeley, to see which one could increase the rotational speeds of their respective systems at a faster rate. I do not know the final score, but history seems to indicate that they both won. Jesse succeeded in increasing the speed of centrifuge rotations a thousandfold, from a few thousand rotations a second to more than a million rotations a second.

Beams realized that the rotor must be enclosed in a relatively high vacuum if his model was to achieve higher rotational speeds than the previous "ultra" centrifuges. The high vacuum would also eliminate the frictional heating of the liquid solutions, which seriously interfered with the sedimentation experiments. In his first designs the rotor was suspended in an evacuated container by a flexible shaft that passed through a heavy oil seal to the outside, where it was attached to an air-driven turbine. The flexible shaft could shift its position slightly, thus allowing the rotor to spin about its own inertial axis, as in the system of de Laval. Because of the externally rotating parts, this model was far from frictionless, but it did eliminate the troublesome problem of frictional heating of the samples in the rotor, and it did permit rotors as much as a foot in diameter to be spun thousands of rotations a second. Beams stated that one of his most difficult problems was the development of a practical, vacuum-tight oil gland through which the rotating shaft would pass. Once this problem was solved, the design became a model for many commercial centrifuges for separation of molecules in solu tion. In 1961 Beams stated that ultracentrifuges of this general type had been the "workhorses" of molecular sedimentation experiments in this country for twenty-five years.[6]

Although this evacuated, shaft-supported ultracentrifuge proved to be enormously useful, it was not the ultimate one that Jesse was seeking. His desired ultracentrifuge was one in which the spin rate would be limited only by the tensile strength of the rotor itself. To reach this ultimate limit, Jesse knew that the rotor must spin in a very high vacuum and that it must not be impeded by a supporting shaft. About 1934 he and his associates began to experiment with magnetic field support of a rotor that was constructed of, or implanted with, a ferromagnetic material. The field of an electromagnet, located outside and directly above the evacuated container, could penetrate the walls of the container and lift the rotor. This ferromagnetic rotor would seek the region of strongest field, that in line with the magnet's core, and, when spinning freely, would also seek to rotate about its own inertial axis of symmetry. Consequently, Jesse cleverly hung the cylindrical core of the external electromagnet by a flexible wire in a loose-fitting oil container so that the spinning ferromagnetic rotor could pull the axis of the supporting magnetic field exactly into line with its own axis of rotation. This feature in the design solved the troublesome problem of stabilization of the spin axis at very high rotational speeds—but other problems remained to be solved.

A symmetrical rotor completely stabilized along a vertical axis could still shift up or down along this axis if the critical balance between the lifting magnetic field and the gravitational pull was not maintained exactly. Beams and his group first solved this problem by focusing a horizontal light beam across the rotor onto a photoelectric cell. If the rotor moved

[6] *Ibid*., p. 140.

slightly upward or downward, the light intensity on the photoelectric cell would increase or decrease in such a way as to produce a correcting current in the electromagnet that would restore the original position. In later models they achieved stabilization with a conducting loop placed above the rotor. If the rotor should move upward toward the loop, the current would increase; if it should move downward, the loop current would decrease. A servomechanism connected to the loop sent a correcting signal to the electromagnet.

With the rotor thus stably suspended entirely by externally applied fields in its closed, evacuated container, the only remaining problem, that of finding a satisfactory method of spinning the rotor without introducing the mechanical driving shaft, was solved elegantly when Beams and his associates constructed the rotor in such a way that it could be driven by electromagnetic induction fields produced by "field" coils outside the container. In effect, the rotor became the turning armature of a synchronized induction motor.

This was the ultimate ultracentrifuge of which Jesse had dreamed. It would spin rotors ranging in diameter from less than a thousandth of an inch to more than a foot, and ranging in weight from a billionth of a pound to more than a hundred pounds. The rotors could be spun without detectable instability ("sleeping tops") to speeds of more than a million rotations a second, speeds at which they would explode under the enormous centrifugal fields of more than a billion G that could be easily produced. The resistance to spin was due almost entirely to residual air in the container. With the vacuums easily obtainable, this amount was so small that a freely coasting rotor would lose only one revolution per second of speed in an entire day. So little was the resistance, that by painting a spherical rotor with one side dark (absorbing) and one side light (reflecting), Jesse was able to increase the speed simply by shining a light beam on the spinning rotor. He thus achieved a new and sensitive measure of light pressure.

This completely stabilized, almost resistanceless rotor developed at Virginia under Jesse Beams's guidance made possible many new experiments. Although the instrument was used in other laboratories, some of the more significant applications were carried out by Beams and his group at Virginia. For example, Beams was the first to succeed in separating atomic isotopes with a centrifuge. I shall give further details about this later. By driving the rotors to explosive speeds, he and his group used the new ultracentrifuge for extensive measurements of the strength of materials. Of particular importance was their finding that thin metallic films (with thickness of the order of atomic dimensions) were proportionally much stronger than the corresponding bulk metals. They found, for example, that the tensile strength of a silver film thinner than 0.000025 cm is thirty times that of the bulk silver.

Extensive application of the ultracentrifuge is made in the purification of materials in solution by the sedimentation process and in the separation of organic and biological molecules and measurement of their molecular weight. Such measurements as these had been made with earlier centrifuges, but the new Beams ultracentrifuge made the separations more complete and the measurements more precise. The centrifugal fields of the Beams ultracentrifuge proved to be sufficiently large to produce sedimentation in all known substances in either the gaseous phase or in liquid solution. It was thus able to purify almost any known substance that can exist in a liquid or a gaseous phase at a temperature ranging from that of liquid helium to well above room temperature. Molecular weights can be measured to a precision of much better than one percent in a range from fifty to more than a million molecular weight units.[7] It requires little imagination to visualize the widespread chemical and biological applications of such a tool.

GAS CENTRIFUGE CONCENTRATION OF ATOMIC ISOTOPES, ESPECIALLY THOSE OF URANIUM

The Beams contribution that is likely to have an enormous eventual impact on the industry and the economy of this and other nations is his pioneering use of the ultracentrifuge for separation of atomic isotopes, especially those of uranium. Sir J. J. Thomson invented the first atomic-beam mass spectrometer in 1907 and five years later used it to show that neon consists of two stable isotopes, ^{20}Ne and ^{22}Ne. Then F. W. Aston, one of his students, greatly improved this type of mass spectrometer and used it to measure the masses of most of the stable isotopes. Other scientists—among them A. J. Dempster, K. T. Bainbridge, and A. O. Nier—further refined the beam-deflection type of mass spectrometer for precise measurements of all known stable isotopes and for concentration of certain isotopes in very small quantities for important tracer studies. This method was recognized as inadequate, however, for the large-scale concentration of the heavier isotopes needed for industrial uses.

The possibility of using the centrifuge for isotopic separation was proposed by F. A. Lindemann and F. W. Aston as early as 1919. Several physicists, including Aston, followed their proposal with theoretical papers and experimental efforts to separate isotopes by centrifugal methods. All the attempts failed until 1937, when Beams and his students succeeded with his newly developed ultracentrifuge in separating ^{35}Cl and ^{37}Cl in chlorine gas. To justify his use of the

[7] J. W. Beams, "High Centrifugal Fields," *The Physics Teacher,* 1(1963): 103-7.

centrifuge for isotopic separation after others had met with failure and abandoned it, Jesse said: "This seemed worthwhile because according to theory the separation factor should depend principally upon the differences in the masses of the isotopes rather than upon their absolute values so that the method, if successful, could separate the isotopes of the heavier as well as the lighter [elements]."[8]

In his early history of isotopic separation with the gas centrifuge, Beams further wrote: "Soon after the announcement of uranium fission by neutrons in March 1939, the writer and L. B. Snoddy, at the University of Virginia, like many other workers, became interested in the separation of ^{235}U and ^{238}U isotopes."[9] For their initial work they obtained a small grant-in-aid (March 1940) from the Carnegie Institution of Washington and later, in 1940 and 1941, grants totaling $6,353.57 from the Naval Research Laboratory. With this modest support, in 1941 Beams and his group succeeded in making the first separation of uranium isotopes with the gas centrifuge. After the formation of the Manhattan Project, governmental support of experimental work on centrifugal separation of uranium isotopes increased, as did the restrictions for security of the projects. Throughout the war, the project under Beams's direction was maintained at Virginia, although work was started at other places.

I shall outline briefly the methods that evolved from these early efforts at ^{235}U concentration. Rapidly spinning cylindrical tubes were used to centrifuge circulating columns of UF_6 gas. These tubes were vertical, and the temperature was maintained somewhat higher at the lower ends than at the upper ends. Convection currents circulated up the center of the tubes and down along the outside walls. The centrifugal

[8] J. W. Beams, *Early History of the Gas Centrifuge Work in the U.S.A.* (Charlottesville: University of Virginia, 1975), p. 2.

[9] *Ibid.,* p. 15.

forces increased the $^{235}UF_6$ concentration along the axis and the $^{238}UF_6$ along the outer walls of the tubes. The concentrated samples of $^{235}UF_6$ were drawn off from the axial center of the tubes and passed on to other tubes where the concentration was increased further. This process was repeated in a series of tubes until the $^{235}UF_6$ had reached the desired concentration. To provide the desired capacity, parallel systems of tubes were arranged. Details of the system may be found elsewhere."[10]

Near the end of World War II, the U.S. Army decided to adopt gaseous diffusion as the principal method of separation of uranium isotopes. Consequently, support of the gas centrifuge project was terminated in January 1944. During the following decade, work on the project was dormant, according to Beams, primarily because of strict security classification. Work on the method proceeded, however, in Germany and in Russia. A team of Germans and Russians, working in Russia, apparently made substantial progress in simplification of the technique. Dr. G. Zippe, a leading member of the team, an Austrian who had been allowed to return to Germany, described the work in an interview with M. Shutte, who reported it to K. Brewer of the Naval Research Laboratory. Possibly because of reported progress in other countries, the centrifuge method was reappraised in this country in the late 1940s, and funds were made available to reactivate the project on a small scale at the University of Virginia. A. R. Kuhlthau, who had worked on the project during the war, was given responsibility for obtaining personnel and getting the work started. He was instrumental in bringing Zippe to Virginia in August 1958 to work with the project until June 1960, when he returned to Germany. This

[10] J. W. Beams, A. C. Hagg, and E. V. Murphree, *Development in Centrifuge Separation,* Report 5230, ABC, Washington, D.C., 1951.

association allowed the Virginia group to become familiar with the Russian experiments made during the period when gas centrifuge work was inactive in this country. To summarize I quote from Beams's account:

> While Zippe was still at Virginia, Dr. Ralph Lowry, who was soon to follow Kuhlthau as director when the latter became associate provost of the University, and Dr. Alwyn Lapsley joined the Virginia group and together they set about to assemble and utilize all of the advantages of their own, the Zippe and all other known techniques. As a result it soon became clear (to a number of optimists) that the gas centrifuge might possibly eventually become a competitor with the diffusion method. The progress made at Virginia soon persuaded the AEC to add a group at Oak Ridge and one at the AIR Research Company in California to the project also to shift the responsibility for the project from the Division of Research to the Production Division. The wholehearted cooperation of the three contractors together with the amazing developments in the method since that time is striking testimony not only to the wisdom of this action but to the administrative skill and devotion to excellence on the part of the directors and staffs of the three projects as well as the AEC staff that has had the AEC administrative responsibility.[11]

After his formal retirement at the University of Virginia in 1969, Beams continued to work with the gas centrifuge program as a consultant to the overall program of the AEC, as well as to the project at Virginia. He had the satisfaction of seeing the process brought to the point of acceptance by our government as a major source of ^{235}U concentration for our nation's nuclear energy requirements. In April 1977, three months before Jesse's death, President Carter authorized the conversion to the gas centrifuge process of a large-scale plant at Portsmouth, Ohio, originally planned in the mid 1970s as an expansion of the gaseous diffusion facility. This first large-scale gas centrifuge separation plant in the United States is under construction at the time of this writing (1980).

[11] J. W. Beams, *Early History of the Gas Centrifuge,* p. 39.

Gas centrifuge plants for ^{235}U enrichment are already in operation or under construction in Europe.

The primary considerations that led to the decision by our government to construct its first centrifuge plant for 235U enrichment was the significantly lower energy consumption of the centrifuge method as compared with the gaseous diffusion process. According to information given me by P. R. Vanstrom, vice-president for engineering and development of Union Carbide Corporation, the gas centrifuge plant being constructed at Portsmouth will require about 145 MW of power, whereas the same capacity provided by the gaseous diffusion process would require about 2700 MW, almost twenty times that required for the gas centrifuge process. At the time of the original choice of the diffusion process and the cessation of work on the centrifuge process, we were an energy-rich nation working under the urgency of a world war. Now when this country and the entire world face a serious energy crisis, the pioneering work of Beams and his group at Virginia offers great hope for efficient production of our most promising form of energy.

PRECISE MEASUREMENT OF THE GRAVITATIONAL CONSTANT

With the developmental work on the gaseous centrifuge safely in other hands, Beams again concentrated his thinking on basic new problems. That he was approaching, or past, the normal age for retirement seemed to make no difference to him nor in the results he achieved. Indeed, at this advanced age he may have conceived the most important experiment of his career—one with the potential for increasing the accuracy of measurement of the gravitational constant *G* a thousandfold.

The first laboratory measurement of the gravitational constant G was made in 1798 by Henry Cavendish, of Cambridge. His beautifully simple experiment is known to all physicists. Two equal spherical masses connected by a rigid, symmetrical bar were suspended at the center of the bar by a fiber to make a torsional balance. Two much heavier spherical masses were then placed on opposite sides of the two suspended balls so that the gravitational attraction between the fixed and suspended masses produced a twisting torque on the fiber. With the measured angle of twist, the torsional constant of the fiber, and the separation of the centers of the spheres, the gravitational constant could be calculated from Newton's gravitational formula. Since that time, the Cavendish experiment has been repeated many times by many physicists with some variations and some improvement of equipment but with little improvement in the accuracy of the constant. The best of these values is considered to be 6.670 ± 0.015 dyn $cm^2 gm^{-2}$, obtained by P. Heyl and coworkers at the National Bureau of Standards in 1942. This was the accepted value of G at the time Beams began his experiments; Cavendish's value is 6.674.

It is astonishing that in the space age, when many new tests of Einstein's general relativity theory were being planned, the basic cosmic constant, *G* (if it is a constant!), was known to only three significant figures. The space-age need for a better *G* must have challenged Jesse as much as had the need to find a way to produce nuclear energy without wasting so much energy in the process.

The method that Beams designed represents the greatest advance in the technique for measurement of the gravitational constant since the Cavendish experiment in 1798. Superficially, his apparatus appears to be similar to that of Cavendish. There are the two very heavy spheres on opposite sides of a smaller, suspended-mass system. In the Beams experiment, the smaller system is in an airtight jar. The gravitational attraction tends to align the suspended bar between the centers of the two large spheres outside the jar. Unlike those of the Cavendish system, these spheres are mounted on a table that can be rotated with the smaller-mass system. The rotation of the table is controlled by the suspended-mass system through a servomechanism. A light beam that comes from a source mounted on this table is reflected from a mirror attached to the suspended cylinder and falls on a photocell mounted on the same table. When the suspended mass system starts to rotate toward the heavier mass system, the reflected light beam begins to move off the photocell, thus sending a signal through the servomechanism to the motor that turns the table. In response to the signal, the motor rotates the table so as to maintain the beam of light on the photocell. The spherical-mass system, mounted on the table, is then rotated so that a constant angle is maintained between the two attracting systems. As the suspended bar is accelerated to align with the massive spheres, the latter system is given the same angular acceleration by rotation of the table. It is just as though the earth, which accelerates a falling apple, were to accelerate away from the apple at the same rate. To a person on the earth, the apple would not appear to fall, but to an "outside" observer, the apple would appear to be unsuccessfully chasing the earth at an ever increasing speed. Likewise, an observer off the rotating table sees the two inertial systems on the table as turning together at a slowly increasing velocity, the rate of increase of which is determined by gravitational attraction between the two systems.

The Beams method has two important advantages that make it potentially orders of magnitude more accurate than previous methods for measurement of *G*. The first results from the fact that one can obtain *G* from measurement of a relatively large angular velocity accumulated from a very small gravitational acceleration continuously applied over a long period of time. Within a few days, the system achieves a visible rotation and a velocity measurable with high accuracy.

From the measured time for acquiring a given angular velocity, the gravitational acceleration is easily obtained. With this acceleration and the effective separation of the two mass systems, *G* can be calculated. Although in the first experiments the smaller mass system was suspended by a quartz fiber to damp out possible oscillation, the torsional constant of the fiber does not enter into the calculations. The second important advantage is that effects of surrounding masses in the laboratory and elsewhere in the universe can be averaged out by a known, constant rotation imposed on that caused by the gravitational acceleration.

While the Beams method for measurement of *G* probably will be refined eventually to achieve its potential accuracy, estimated to be of the order of one part in a million, only one part in 4000 was achieved by Beams and his associates before his death. In 1975 they reported the value 6.6699 ± 0.0014 dyn $cm^2 gm^{-2}$, with an order of magnitude greater accuracy than that achieved with other methods.[12]

Efforts are continuing at the University of Virginia and at the National Bureau of Standards to realize more fully the potentialities of the Beams method. Some theorists, including P. A. M. Dirac, have proposed that *G* may not be exactly constant but decreasing perhaps by one part in 1010 per year because of expansion of the universe. The Beams method appears inherently capable of measuring variations in *G* with greater accuracy than its absolute value. At the University of Virginia, R. C. Ritter is leading attempts to adapt the method for detection of the predicted changes in *G* with time.

An ingenious method for testing the assumption of continuous creation of matter was designed by Beams and his associates: R. C. Ritter, G. T. Gillies, and R. T. Rood. Two

[12] G. G. Luther et al., "Initial Results from a New Measurement of the Newtonian Gravitational Constant," in *Atomic Masses and Fundamental Constants,* vol. 5 (1976), pp. 629-35.

cylinders are concentrically rotated in an evacuated chamber that is acoustically and magnetically shielded. The outer cylinder is rotated with a precise, constant angular velocity, ? The inner cylinder is magnetically suspended like the rotor in the Beams ultracentrifuge and is given a rotational velocity ? by phonons of a laser beam. Creation of matter within the inner cylinder would increase its moment of inertia and decrease its angular velocity, however slightly, relative to that, ?, of the outer cylinder. In normal operation, ? is maintained equal to ? by means of a laser pulse sensor and phonon driver with a feedback correcting signal. The amount of correcting signal to maintain ? equal to ? gives evidence for matter creation. This proposed experiment, under construction at the time of Jesse's death, is being continued by R. C. Ritter.

A NEW INSTRUMENT FOR BIOPHYSICAL STUDIES

The many applications of the Beams ultracentrifuge for isolation and molecular weight measurement of large molecules of biological significance are widely known and have been mentioned earlier in this biography. Less known is the powerful new instrument for studies of the interactions of such molecules that Beams invented in the later years of his life. This new instrument, a magnetic-suspension densimeter-viscometer, described by Hodgins and Beams,[13] measures simultaneously and with quickness and exceptional precision the density and viscosity of a fluid system. The density is measured to one part in a million and the viscosity to one part in ten thousand.

The idea for this new instrument must have come to Jesse from his magnetically suspended ultracentrifuge. A small

13 M. G. Hodgins and J. W. Beams, "Magnetic Densimeter-Viscometer," *Review of Scientific Instruments,* 42(1971): 1455-57.

cylindrical buoy is magnetically suspended in the fluid. The calibrated electromagnet required to support it gives the fluid density. The buoy is rotated slowly by an induction field externally applied, as in the ultracentrifuge. The period of rotation at a constant power input gives the viscosity. In one design the buoy is held fixed and the fluid container slowly rotated to measure changes in viscosity. The device is capable of measuring viscosities without introducing significant shearing stresses in the liquid. Among other things, measurements with it have revealed that dilute solutions of viruses, when under extremely small shearing stresses, exhibit solid-like behavior.

Jesse worked on the refinement and application of the densimeter-viscometer up to the time of his death. In fact, on the day he died, his longtime friend and collaborator, D. W. Kupke, a professor of biochemistry in the Virginia Medical School, came at Jesse's request to his bedside to complete their latest collaborative paper on the application of this instrument. This paper reported modifications of the magnetic suspension densimeter-viscometer that made possible continuous and accurate recording of the variations in viscosity and density of solutions undergoing change. The results obtained revealed conformation of changes of ribonuclease in the presence of guanidinium chloride and a disulfide cleaving agent. Kupke relates that Jesse was excited and elated over the results. They completed the paper, and evidently Jesse signed the accompanying letter contributing it to the Academy *Proceedings,* for it appears in the October issue for 1977 with the statement, "Contributed by Jesse W. Beams."

PROFESSIONAL ACTIVITIES AND PERSONAL ATTITUDES

Jesse Beams was a respected leader in professional societies devoted to the advancement of science. He held the highest office to which his fellow physicists could elect him, the presidency of the American Physical Society. A listing of the many offices he held, the many councils and boards on which he served, is given at the end of this memoir. He received numerous awards, prizes, and medals, including the National Medal of Science, and honorary degrees from several universities, the last from Yale, where he and E. O. Lawrence worked together as young postdoctoral fellows. These various honors are also listed at the end.

How did Jesse feel about his various decorations and awards? I think he felt humbly grateful for the evidence they gave him that his friends and fellow scientists held him in high esteem. He craved their approval and good will, but he was troubled about being singled out and rated, so to speak, above his friends. Perhaps the Thomas Jefferson influence at Virginia had something to do with his attitude, but I think that humility was a part of Jesse's basic nature. It seems most appropriate that one of the honors he received was the Thomas Jefferson Award. I asked Mrs. Jesse Beams (Maxine) how Jesse felt about his many honorary degrees, medals, awards, and citations. She told me, "Jess was very modest about these things. He never would let me have these framed. They were always tucked away. I often couldn't tell his mother about them. She'd feel proud and put it in the local paper in Kansas. Naturally [for Jesse], this was just too much!"

Although Jesse Beams's contributions to discoveries in physics belong to the world and are known and used throughout the world, the influence of his educational and professional leadership is national. Probably no other physicist had so great an impact on the development of physics in the southeastern states as Jesse Beams had. He was one of the organizers of the Southeastern Section of the American Physical Society and served as its first chairman (1937). In 1973 the Southeastern Section established the Jesse Wakefield

Beams Award, to be given each year for significant research in physics. For sixteen years Beams served on the Board of Directors of the Oak Ridge Institute of Nuclear Studies. One can hardly visit a university in the southeastern states without encountering a professor who was a Beams student, or the student of a Beams student. It is understandable that his impact was greatest on the University of Virginia, where it was indeed abnormally great. In the spring of 1980 when I went to Charlottesville to learn all I could about Jesse's life and work there, I encountered Beams Ph.D. students all over the place. Frank Hereford, president of the University, took an hour of his time to talk with me about Jesse even though he was preparing for commencement ceremonies to be held the next day. Dexter Whitehead, dean of the Graduate School, did the same. This was not surprising; both were Beams's students. I met other students of his who are now professors of physics or engineering there.

It is evident that the University of Virginia recognized Jesse Wakefield Beams as one of the greatest professors in the long history of the University. He was elected to their most select societies—the Raven Society, the Thomas Jefferson Society, and the Colonnade Club. He was given the Distinguished Virginian Award by the State of Virginia.

How was Beams's laboratory regarded by scientists abroad? I answer this by relating an incident that occurred in the late sixties. Sir Harold Thompson, then Foreign Secretary of the Royal Society, when on a tour of scientific institutions in America, stopped for a visit with us at Duke. During the course of our conversation I asked him which laboratory that he had seen during his visit in the States had impressed him most. Of course I expected him to name one of the large laboratories of an institution such as Berkeley, Cal Tech, or MIT; to my surprise, he said that he was most impressed by the laboratory of Jesse Beams at the University of Virginia. He went on to say that the floors of the Rouss Laboratory were rotting through in places and the walls were cracked and unpainted—but that the instruments for Beams's ingenious experiments were firmly mounted on concrete piers and that their vital working parts were cleverly designed, made from materials of the highest quality, and constructed with the greatest care and precision. I couldn't resist adding "in the true Oxford—Cambridge manner?"

In his personal relationships Jesse Beams maintained the same high standards that he did in his laboratory experiments. He spoke freely, but softly, and always in a kindly manner. In my many years of association with him I never once heard him make an unkind remark about anyone. He expected his students and associates to work hard, very hard—and they usually did—but Jesse never coerced them into doing so. Rather, he enticed them by his enthusiasm and encouragement, by his exciting projects and ideas, and, most of all, by his own example of persistence and hard work.

For fourteen years, from 1948 to 1962, Jesse served as chairman of the Department of Physics at the University of Virginia. This was a period of rapid growth and development of the department, and I was puzzled that Jesse could manage all the business of the department and continue working for long hours in the laboratory with his students and associates, as he is reported to have done. Consequently, I asked John Mitchell, a professor in the department during this period, how Jesse, with all his other duties, managed the department. He immediately replied, "With benevolent laissez faire!" This confirmed opinions others had given me. President Frank Hereford remarked that he was a good chairman who kept the departmental meetings short and saw to it that nothing distracted the staff from physics. From Hereford, and also from Dexter Whitehead, dean of the Graduate School, I heard the following example of how Jesse handled difficult departmental problems. Sometime earlier, Jesse had persuaded C. J. Davisson (of the Davisson-Germer experiment) to come to Charlottesville after his retirement from the Bell Telephone Laboratories. Davisson was given an office in the Physics Department, which he used less and less as he grew older. Meanwhile the physics staff grew, and office space became scarce. There was increasing pressure on Jesse to ask Davisson to give up his office. Instead of doing this, he called a meeting of all the physics faculty members. When they were assembled, Jesse quietly asked "Will all of you who are in favor of throwing old Dr. Davisson out of his office, please hold up your right hands." None did, and the meeting was promptly adjourned.

Donald W. Kupke, one of Jesse's colleagues with whom he collaborated for sixteen years on biophysical problems, best expressed in his tribute to Jesse the sentiments of those with whom I talked at Virginia. These are his words:

> Anyone who knew Jesse Beams even slightly would agree that his first concern was for others. This concern was genuine; invariably, he would stop his work, listen attentively without interruption or haste, and be supportive to any who came to him—whether they were of high rank or of no rank at all. He was a gentle, guileless person who sought to be helpful in whatever matter—large, small or even nonsense—which was brought to him. He displayed a remarkably constant good humor, sick or well, troubled or elated. He was also a quiet man who thought deep thoughts about the universe and the role of mankind, but he did no preaching; his lifestyle and deeds preached his scriptural convictions most eloquently.[14]

It is sometimes said that beside every great man of achievement there is an equally great woman. Although this statement probably does not apply for every great man, it certainly seems to have been so for Jesse. Upon her marriage to Jesse in 1931, Maxine Sutherland Beams resigned the

[14] D. L. Kupke, "Obituary, Jesse W. Beams," *Trends in Biochemical Sciences,* 2 (1977):N284.

teaching position she enjoyed and devoted her entire time to assisting Jesse in any way she could. She soon found that he wanted to be free of the business matters of living so that he could more freely devote his time and thought to his experiments. To give him this freedom, she took care of business matters, the household, and transportation. When they built their house, it was she who dealt with the architect and the contractor. She kept the records and paid the bills, even those for Jesse's dues in professional societies. Statements of professional dues and other bills that came to him at the laboratory he simply brought home and dumped on a table or sometimes in the middle of the bed. The purchase of clothes that required fitting often necessitated prior arrangement with the clothier, some selections by Maxine, and considerable maneuvering and coaxing before she was able to get Jesse to leave the laboratory to visit the clothier. He said that he simply did not have time to do it. Once there, he wanted to buy two or three suits so that he would not have to come again soon.

Even more difficult for Maxine than buying Jesse's clothes or taking care of business matters was inducing him to stop work long enough to get adequate relaxation. In efforts to do this Maxine tried many approaches, one of which I shall describe. His students wanted to attend the home football games but felt guilty about doing so while their professor continued to work in the laboratory. Maxine detected this situation and concluded that by attending the games Jesse could improve his relationships with his students and at the same time get much needed recreation for himself. She secretly purchased two season tickets and confronted him with pleas to take her to the games. Somewhat to her surprise, he agreed, but at the half-time intermission he insisted on returning to the laboratory to check on the experiments. Maxine also encouraged Jesse to participate in social activi ties, and she accompanied him to the social events of the many scientific organizations of which he was a member. One of the joys my wife and I anticipated in attending such events was our association with this delightful, kindly mannered couple.

Maxine devoted forty-six years of her life to being a good wife to Jesse; these years were evidently rewarding and happy for her as well as for him. When I asked for her comments about her life with Jesse, she said: "Jess was the most delightful, kind, devoted person in the world, and I was so lucky to have been given the wonderful privilege of sharing his fascinating, interesting life for forty-six years. And those two years of waiting around to decide, them too, I count in the total for forty-eight—forty-eight wonderful, calm, peaceful, devoted years, filled with excitement and the unexpected but always with love and devotion."

A single-sentence remark made to me by President Hereford summarizes this memoir, "Jesse Beams was the ultimate gentleman scholar."

Many individuals have provided information used in this memoir. Those whom I asked for help were enthusiastically cooperative. Mrs. Jesse Beams (Maxine) graciously gave me information about Jesse's life and personality that I could not have learned from anyone else. His former students, Frank Hereford, Jr., president of the University of Virginia, and Dexter Whitehead, dean of the Graduate School, took time during a busy commencement weekend to talk at length with me.

For essential information about the Beams research programs in physics and nuclear engineering, I am indebted to several of Beams's former students or associates, particularly to John W. Mitchell, Ralph A. Lowry, A. Robert Kuhlthau, John W. Stewart, and D. R. Carpenter, Jr. Information about the biophysical research was obtained from Donald W. Kupke, a professor in the Virginia Medical School. I am grateful to Professor Mitchell also for acting as our host and arranging interviews with other staff members at Virginia. On more than one occasion I have had the opportunity of discussing the life and accomplishments of Jesse Beams with Howard Carr, one of his students, who served for many years as chairman of the Physics Department of Auburn University. Paul R. Vanstrum, vice-president for engineering and development of the Nuclear Division of Union Carbide, gave me much information about Jesse's role in the development of the gas centrifuge process for concentration of uranium isotopes. He also provided the excellent photograph preceding this article.

Finally, I want to thank my wife, Vida Miller Gordy, who helped me in every phase of this memoir.

PROFESSIONAL CHRONOLOGY HONORS AND DISTINCTIONS

Earned Degrees

1921	A.B., Fairmount College (now Wichita State University)
1922	M.A., University of Wisconsin
1925	Ph.D., University of Virginia

Positions

1922-1923	Instructor in Physics and Mathematics, Alabama Polytechnic Institute
1925-1926	National Research Fellow in Physics, University of Virginia
1926-1927	National Research Fellow in Physics, Yale University
1927-1928	Instructor in Physics, Yale University
1928-1930	Associate Professor of Physics, University of Virginia
1930-1969	Professor of Physics, University of Virginia
1948-1962	Chairman, Department of Physics, University of Virginia
1953-1969	Francis H. Smith Professor of Physics, University of Virginia
1969-1977	Professor Emeritus and Senior Research Scholar, University of Virginia

Professional and Honorary Societies

American Academy of Arts and Sciences (fellow, elected 1949)

American Association for the Advancement of Science (Chairman, Section B, 1942; Vice-President, 1943)

American Association of Physics Teachers

American Association of University Professors

American Philosophical Society (elected 1939; Councilor, 1951-1954; Vice-President, 1960-1963)

American Optical Society

American Physical Society (fellow; President, 1958)

American Physical Society, Southeastern Section (first Chairman, 1937)

National Academy of Sciences (elected 1943)

Virginia Academy of Sciences (fellow; President, 1947)

The Honor Five (University of Wichita)
Phi Beta Kappa
Sigma Pi Sigma
Sigma Xi
Colonnade (University of Virginia)
Raven Society (University of Virginia)
Thomas Jefferson Society (fifty years at the University of Virginia)

Boards and Committees

1942-1960	Science Advisory Committee of the Ballistic Research Laboratory, Aberdeen Proving Ground
1933-1940; 1951-1955	National Research Council (Division of Physical Sciences, NAS Council, NRC Governing Board)
1952-1954	National Science Foundation, Physics Division
1948-1954	Board of Directors, Oak Ridge Institute of Nuclear
1960-1970	Studies (which became Oak Ridge Associated Universities)
1954-1960	General Advisory Board of the U.S. Atomic Energy Commission
1948-1969	Board of Directors, Virginia Institute for Scientific Research

Awards

1942	Potts Medal, The Franklin Institute
1946	U.S. Naval Ordnance Development Award
1956	John Scott Award, given by the City of Philadelphia
1958	Lewis Award, American Philosophical Society
1959	Alumni Achievement Award, Wichita State University
1963	Meritorious Award, Virginia Academy of Sciences
1967	National Medal of Science
1971	Life Fellow, The Franklin Institute
1972	Atomic Energy Committee Citation
1972	Distinguished Virginian Award
1972	Jesse W. Beams Lectureship in Biophysics initiated at the University of Virginia
1973	Jesse W. Beams Award for Research established by the Southeastern Section of the American Physical Society

Honorary Degrees

1941	Sc.D., College of William and Mary
1946	Sc.D., University of North Carolina
1949	Sc.D., Washington and Lee University
1969	Sc.D., Florida Institute of Technology
1976	Sc.D., Yale University

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Photograhy by Hans Knopf

Elmer Keiser Bolton

June 23, 1886-July 30, 1968

by Robert M. Joyce

Elmer Keiser Bolton was one of the outstanding leaders of industrial research. He became an industrial research director at a time when research was only beginning to be a significant factor in the chemical industry. There were no models for this new role, and Bolton's concepts of directing industrial research were in large measure those that he formulated himself, reflecting his vision and drive to achieve important commercial goals.

The record of industrial products developed by Du Pont research organizations that he directed is impressive; it includes synthetic dyes and intermediates, flotation chemicals, rubber chemicals, neoprene synthetic rubber, nylon synthetic fiber, and Teflon® polytetrafluoroethylene resin. His leadership in bringing these developments to fruition was always apparent to management and to those he directed, but because of his characteristic self-effacement his name is not widely associated with these accomplishments. Throughout his career he supported and encouraged his technical personnel. Most important, he had the knack of picking the right time and direction to take in moving a research lead into development. Many of his decisions proved crucial to the ultimate success of these ventures.

Elmer Bolton's paternal grandfather emigrated from Bolton-le-Moors, England about 1833 and settled near Frankford, Pennsylvania. Bolton's father, George, was born in 1861 and grew up and settled in that area; he ran a men's furnishings store on Main Street in Frankford. He married Jane E. Holt, and the couple had two children: Elmer Keiser, born 23 June 1886, and Thomas Coulston, born 17 December 1887. Both attended the local high school in Frankford and then went on to college. The younger brother, Thomas, became a professor of insurance and finance in the College of Business Administration at Syracuse University.

Elmer went to Bucknell University and took "The Classical Course," which led to a B.A. degree in 1908. The Classical Course included elective studies in chemistry during the second, third, and fourth years, and it must have been at this time that his interest in chemistry was kindled. Bolton entered Harvard in 1908 to pursue graduate study in organic chemistry. He received his A.M. from Harvard in 1910 and his Ph.D. in organic chemistry in 1913. His thesis, directed by Professor Charles L. Jackson, concerned the chemistry of periodoquinones.

At Harvard, Bolton formed what were to be lifelong friendships with two students who were to become outstanding professors of organic chemistry: Frank C. Whitmore, who would teach at Northwestern and Penn State, and Roger Adams, who had an illustrious career at Illinois. Adams in particular was later to have an important influence on Bolton's ideas about industrial research. Adams had received his A.B. from Harvard in 1909 and continued there to his Ph.D. in 1912. He then spent the academic year 1912-1913 with Professor Richard Willstätter at the Kaiser Wilhelm Institut in Berlin, which may have been a factor in Bolton's decision to do the same thing for the succeeding two years.

Bolton and Adams shared many traits. Both were extremely devoted to organic chemistry but also had a variety of other interests. They had the happy faculty of "being interested." Both had an intense drive for concrete accomplishment and an instinct for inspiring those who worked with them. Their choices of different careers may have reflected, in part, differences in family background: Bolton's forebears followed commercial pursuits, whereas there were teachers, including his mother, in Adams' family. In later years, Adams unquestionably influenced Bolton's ideas on industrial support of chemical research and chemistry students in universities. Adams always referred to Bolton as "Keis," whereas at Du Pont he was "Dr. Bolton" to all but a few close friends, to whom he was "Elmer."

When Bolton received his Ph.D. from Harvard in 1913, the university awarded him the Sheldon Fellowship, which he used to spend two years of postdoctoral research with Willstätter at the Kaiser Wilhelm Institut. Here he became involved in Willstätter's major program on anthocyanins, and he published three papers with Willstätter on isolation and structures of some anthocyanin pigments.

Willstätter was sufficiently impressed with Bolton to comment in his autobiography on Bolton's research, as well as on another trait. He wrote, "One of my capable American colleagues had the minor weakness of easily making mistakes in calculations. One day in exasperation I burst out, 'You must have been a bank teller.' The answer: 'That I was; I earned my way through school by being a bank teller in Philadelphia.'" [1]

Several consequences of his stay in Germany were to have a significant influence on Bolton's subsequent career. First,

[1] R. Willstätter, *Aus Meinem Leben* (Weinheim: Verlag Chemie, 1949), p. 221.

he carried away a lasting impression of Willstätter's careful and logical approach to tackling a research problem and of his rapport with his collaborators. Second, Bolton learned about the German system for training chemists, the German dye industry, and the relationship between the two. In 1920, when he had become responsible for Du Pont dye research, he wrote:

> The greatest contribution which the German universities have made to their dye industry is not in the number of research chemists who have entered the industry with previous knowledge of operating processes, nor the development of new fields of research by the professors of the universities, but in the large number of research chemists who have been taught the correct methods for attacking a problem by men prominent in their line of work and if the American dye industry is to succeed in future years, it is necessary to receive from our universities chemists who have had a thorough training in the fundamental principles of research work.[2]

The third important consequence of Bolton's stay in Germany was his exposure to and interest in German efforts to make a synthetic rubber. Harries, at the University of Kiel, was particularly active on this problem, studying the polymerization of isoprene and dimethylbutadiene, and Bolton became familiar with the research through Harries' publications and, quite probably, by attending seminars. Although Harries never made a practical rubber, Bolton was impressed, and his confidence that the objective of a synthetic rubber could be attained was later to be the key to the development of neoprene.

One other German import was the phrase "eine gute Nase," which Bolton often urged upon his men: smell out the significant goal, the critical experiment, and the proper turn in the road.

When Bolton returned from Europe in 1915, he found the American chemical industry frantically trying to develop

[2] E. K. Bolton, memorandum, 16 July 1920.

methods for making organic chemicals, practically all of which had previously been imported. The Du Pont Company was seeking well-trained chemists, and employed Bolton in August of 1915.

At that time much of Du Pont's research was conducted by the Chemical Department at the Experimental Station just outside Wilmington. Bolton went to the Station, where he worked on the synthesis of glycerol. The following year he was offered an instructorship in organic chemistry at Harvard—quite possibly to replace his friend Roger Adams, who was leaving Harvard to go to the University of Illinois. Bolton chose to stick with industry and declined the offer.

In 1916 the Du Pont Company decided to embark on the manufacture of synthetic dyes, and Bolton was selected leader of the Dye Group that was set up at the Station to develop manufacturing processes. The development of dye intermediates was carried out at Jackson Laboratory, across the Delaware River from Wilmington, under the direction of Dr. C. M. A. Stine. One can imagine the furor that would ensue today from an incident that Bolton later described:

> I was in charge of a small group of chemists studying dye processes, as the Company had decided to enter the dye industry. One of the processes which I personally explored was the preparation of methyl violet, a dye of high tinctorial strength, and this work had progressed to the point of a small semiworks plant. When the first chemical exhibition opened in New York, I was assigned to go to it. Thinking that the day I would be absent would be a suitable time to clean the entire laboratory, I requested the helper to give the place a thorough cleaning and to get rid of the dye scattered around the building on the shelves, tables, and windowsills. The helper took my instructions too literally and not only cleaned the laboratory from top to bottom but also emptied the boxes containing well over a hundred pounds of methyl violet, accumulated from the semiworks operations, into the historic Brandywine Creek that ran close to the laboratory. For a number of hours the Brandywine was a beautiful violet-colored stream as it flowed through the City of Wilmington. There appeared to be no comment expressing appreciation of the improvement in the aesthetic

> appearance of the stream but there were comments from my supervisor the next morning when he told me that a large cotton finishing plant, located farther down the stream, had to close at noon because of the methyl violet. For a short time I was positive that my tenure with Du Pont was about to end abruptly, but fortunately this event never transpired.[3]

There was little knowledge of dye manufacture in the United States, and Bolton was sent to England in December 1916 to become familiar with British technology for the manufacture of dyes, particularly indigo.

When Bolton returned from England, he was assigned to the Wilmington office as advisor on dyes and intermediates. In 1918 he was transferred to the Dyestuffs Department as assistant general manager of the Lodi Works, where silk colorants were made. In 1919 he returned to the Chemical Department as manager of its Organic Division, a capacity in which he supervised the research of the Dyestuffs Department at Jackson Laboratory.

Two principles that Bolton was to follow and insist on throughout his career were firmly in his mind by 1920. Thus, he wrote: "A very important problem in the development of the Chemical Industry is to determine whether the methods for research work for developing new manufacturing processes lead to results in the shortest time with the minimum expenditure of money."[4]

This philosophy was later to be implemented many times in his decisions to move laboratory findings quickly into the development phase.

Again in 1920, he wrote: ". . . the method of manufacture should then be developed with the use of pure materials for the purpose of eliminating the confusing effect of byproducts introduced by impurities. . . . After the most favor

[3] E. K. Bolton. "Fundamental Research in the Chemical Industry" (Willard Gibbs Medal Address, 21 May 1954).

[4] *Ibid.*

able conditions for the manufacture of the intermediate or the dye have been worked out with pure materials, the process should then be adapted to the use of available plant materials."[5]

This emphasis on the use of pure materials in research was stressed to the chemists in Bolton's organizations almost as much as the historic Du Pont emphasis on safety, and it was to be a critical factor in the discovery and development of neoprene and nylon. One wonders whether this insistence on purity might have been picked up during his experience with Willstätter; Roger Adams, who also worked in Willstätter's laboratory, was equally insistent on this point.

In 1922 the Du Pont Company established research divisions for each of its four production departments. These divisions were located at plant sites, and the Chemical Department was maintained at the Experimental Station to carry out exploratory research aimed at developing new areas of chemical business.

Bolton was appointed director of research for the Dyestuffs Department, and it was here that the vision that characterized his career became apparent. Although the principal business of his department was dyestuffs, Bolton decided to explore other types of products that might be made from the great variety of dye intermediates then at hand. By 1923 he had in progress research on rubber accelerators, and soon thereafter extended the research to encompass antioxidants for rubber and gasoline, flotation agents, insecticides, seed disinfectants, and large-scale manufacture of tetraethyllead. Several commercial products soon emerged from these programs, and the business of the Dyestuffs Department was broadened substantially.

In the early 1920s the notorious Stevenson Plan, a British colonial natural rubber monopoly effort, was promulgated to

[5] *Ibid.*

control the price and supply of natural rubber at a time when U.S. demand was increasing rapidly because of the burgeoning automobile industry. Bolton had not forgotten the German work on synthetic rubber with which he had become acquainted during his postdoctoral years in Germany, and he persuaded his management (Vice-President Willis Harrington) that a synthetic rubber would be an excellent research objective.

Work was started in September 1925 on polymerization of butadiene, which was obtained by hydrogenation of diacetylene, but not much progress was made. From December 29 through 31, 1925, Bolton attended the first National Symposium on Organic Chemistry at Rochester, New York, where he heard a paper by Father Nieuwland of Notre Dame entitled "Acetylene Reactions, Mostly Catalytic." Nieuwland described a new catalyst based on cuprous chloride that polymerized acetylene to higher unsaturated hydrocarbons, primarily divinylacetylene (DVA). He reported that DVA polymerized rapidly at room temperature to a hard resin. This resin had the remarkable and embarrassing property of sometimes exploding when struck. Bolton had the ideas that DVA might be the basis for a synthetic rubber, and that Nieuwland's chemistry might be modified to yield monovinylacetylene (MVA), an alternative raw material for butadiene. He approached Nieuwland, found that Nieuwland had stopped work on DVA because he felt it too treacherous, and then proposed that Nieuwland work with Du Pont as a consultant on the company's efforts to develop this chemistry. Nieuwland agreed. On 3 May 1926, at the invitation of Nieuwland, Bolton sent W. S. Calcott to Notre Dame to consummate the consulting agreement and to get technical details for the laboratory preparation of DVA.

Over the next three years, the Du Pont group, working with Nieuwland, made little progress toward a synthetic rubber. They did learn to carry out Nieuwland's reaction in high yield in a continuous-flow reactor, and they were able to modify it to get MVA as the principal product in good yield. In 1929 modification of the conditions for polymerizing DVA gave a lead to a new finish that had excellent chemical resistance, but that discolored on exposure to light.

Despite the discouragements of these three years, Bolton persisted in maintaining the synthetic rubber program, and events were taking place that would enable him to give it new impetus. In 1927 the company Chemical Director, Dr. C. M. A. Stine, persuaded company management to undertake a program of fundamental research with no specific commercial objectives and received authorization of $250,000 for the purpose. In 1928 Wallace Carothers, then an instructor at Harvard, was hired as group leader to head this program. It is probable that Bolton's friend Roger Adams influenced this selection: Adams had trained Carothers and was by then department head at Illinois and a consultant for Du Pont.

In 1929 Stine was promoted to the company Executive Committee, and Bolton was transferred back to the Chemical Department as assistant chemical director; his responsibilities included the work in Carothers' group. Bolton asked Carothers to start a project on reactions of MVA and another on purification of DVA, the latter because of his belief that pure DVA was essential to a study of its polymerization. Dr. Arnold Collins, assigned to the latter project, arranged for construction of a laboratory still capable of distilling DVA at low pressure in a nitrogen atmosphere. With this instrument he isolated from impure DVA a low-boiling, chlorine-containing fraction that, on standing under nitrogen over the weekend, polymerized to a rubbery solid. The road was now open to Bolton's dream.

The chlorine-containing compound was soon found to be 2-chlorobutadiene (chloroprene) and to be easily made by copper-catalyzed addition of hydrogen chloride to MVA.

Bolton moved quickly. He and Harold Elley, who had succeeded him as director of research for the Dyestuffs Department, laid out a development program. In nineteen months Elley's group had done the process and product development and had in operation a pilot plant with a monthly capacity of 1500 pounds. The new product, Duprene (now called neoprene), was announced on 2 November 1931 at a meeting of the Rubber Division of the American Chemical Society in Akron.

Duprene was not a replacement for natural rubber: initially it would cost twenty times as much as the natural product. Nevertheless, it was far superior to natural rubber in resistance to oils and to outdoor degradation, and Bolton insisted that it would find commercial uses; he was right.

The critical technical discoveries in neoprene development were made by several scientists, but it was Bolton who recognized the import of Nieuwland's work, gave his organization the direction to capitalize on it, and persisted for six years in the face of many technical discouragements to achieve his objective of the first commercial synthetic rubber.

Carothers' arrival at Du Pont in 1928 to head the fundamental research group had preceded Bolton's return to the Chemical Department by a year; one year later, in 1930, Bolton was appointed director of the department. Thus was effected the confluence of two remarkable careers. Carothers was a chemical genius: Bolton recognized that and gave him and his programs full support. After Carothers' death, Bolton wrote of him:

> In our association with Dr. Carothers, we were always impressed by the breadth and depth of his knowledge. He not only provided inspiration and guidance to men under his immediate direction, but gave freely of his knowledge to the chemists of the department engaged in applied research. In addition, he was a brilliant experimentalist. Regarding his personal characteristics, he was modest, unassuming to a fault, most uncomplaining,

a tireless worker—deeply absorbed in his work, and was greatly respected by his associates.[6]

One of Carothers' projects from the time of his arrival had been a study of ways to convert small molecules to high polymers by using bifunctional analogs of known coupling reactions. Application of this concept to the Wurtz synthesis of hydrocarbons and to the Williamson synthesis of ethers did give multiunit products but not high polymers. The first break was the finding that bifunctional esterification could be made to give high molecular weight, aliphatic polyesters (at the time, called superpolymers) by carrying out the ester exchange between the dimethyl ester of an aliphatic dicarboxylic acid and an aliphatic glycol in a "molecular still." This was a high-vacuum still with a condensing surface very close to the reaction mixture, so that not only the methanol by-product but also low molecular weight polyesters were removed from the reaction zone, thus driving the equilibrium toward high molecular weight products. Then came the key observation: in April 1930 Dr. Julian W. Hill touched a stirring rod to the surface of a molten polyester that had been made in the molecular still and withdrew it, pulling out a long filament of polymer. Two weeks later it was found that such filaments could be irreversibly stretched (cold drawn) to several times their original length, necking down to a smaller diameter, and becoming clear and much stronger than the undrawn fibers.

Here was the exciting prospect of a synthetic fiber—but that goal soon became evanescent. Study of many aliphatic polyesters failed to turn up any that had the properties desired in a textile fiber. Their melting points were too low,

[6] R. Adams, "Wallace Hume Carothers," in *Biographical Memoirs* (New York Columbia University for the National Academy of Sciences, 1939), p. 298.

and they were soluble in or sensitive to many common organic solvents. A polyamide was made from e-aminocaproic* acid but was deemed too high-melting and insoluble to he spun by the techniques then in use. A compromise between these properties and those of the polyesters was sought in polyesteramides, but again these were low-melting and solvent-sensitive. The possibility of laying the foundation for commercial fiber development appeared remote, and the entire research program in this field was discontinued in late 1932.

Bolton refused to give up. In early 1934 he urged Carothers to reexamine his superpolymer work to see if some basis could be found for synthetic fiber development. Carothers decided to take a further look at polyamides.

He surmised that the apparent intractability of the polyamide from e-aminocaproic acid that had been made earlier was due to cyclization reactions of the 6-carbon segment, and he therefore began study of 9-aminononanoic acid, which should not cyclize. After treatment in the molecular still, the polyamide from this acid was spun into fibers that melted at 195° C and, after cold drawing, resembled silk in strength and pliability. These observations renewed the hope of making a new type of textile fiber from a synthetic polymer.

Carothers' group prepared polyamides from a variety of amino acids and also from dibasic acids and diamines. The leading candidate for development became 5/10 polyamide (the numbers signified the numbers of carbons in the diamine and the dibasic acid, respectively; thus, 5/10 was made from pentamethylenediamine and sebacic acid). It melted at 190° C and had the desired combination of properties in fiber form. Moreover, it could be spun without gel formation, presumably because the 10-carbon dibasic acid segment was not prone to cyclization reactions.

At this point Bolton made a characteristically bold and visionary decision. He took the position that a synthetic fiber would be too large and important a commercial development to be based on the raw material castor oil, which was then the only practical source of sebacic acid. He reasoned that there were six carbons in benzene, that both adipic acid and hexamethylenediamine could be made from benzene, that there would always be plenty of benzene, and that the polyamide to be developed should be 6/6.

This polyamide was first made on 28 February 1935. Owing to advances that had been made in the technology of handling and spinning polyamides, 6/6 could be spun into fibers. These had high strength and elasticity, were not sensitive to common solvents, and melted at 263° C, thus providing a good margin above commonly used ironing temperatures.

On 27 October 1938 Du Pont announced that it had authorized construction of a plant to be built at Seaford, Delaware to make the world's first completely synthetic fiber—6/6 nylon. The announcement was made in a radio broadcast by Dr. C. M. A. Stine, whose initiation of the fundamental research program in the Chemical Department eleven years earlier had begun the chain of events that had led to nylon. Research came to a halt that day at the Experimental Station as we clustered around radios that had been brought in for the occasion.

It is worth noting that Bolton, with his firm ideas about process development, had insisted that every aspect of the nylon process be thoroughly worked out in a pilot plant at the Experimental Station. Here again, his insistence on pure raw materials came to the fore. The pilot plant began suddenly to produce poor quality fiber. Bolton surmised that inferior purity of one or both of the nylon raw materials was the source of the problem, and he insisted on shutting down the pilot plant. The raw materials were carefully checked and found to be below the specified purity; spinning was resumed only after they had been brought up to standard. The Seaford plant was essentially an enlarged carbon copy of this pilot plant, and it had a remarkably trouble-free startup.

One can only speculate on the degree to which Bolton and his colleagues realized at the time the magnitude of what they had wrought. It is certain that Bolton's persistence and vision kept the development alive during many discouraging times. He recognized a fact of history—that pioneering inventions represent dramatic change and are almost invariably brought to fruition only by the dogged persistence of a few people who have faith.

When Bolton became director of the Chemical Department in 1930, the technical staff numbered 121; when he retired in 1951, it had grown to 203. The stature of a manager in industry, however, is measured not by the size of the organization he creates, but by its accomplishments. The development of neoprene and nylon paved the way to two new industries and must be considered his greatest achievements as an industrial research director. Considerable space has been devoted to these developments because they illustrate the role that Bolton played as director. The following paragraphs summarize significant contributions made to the Du Pont Company by the Chemical Department during his tenure as director of that department.

Neoprene put Du Pont in the synthetic rubber business and thus in position to commercialize another synthetic elastomer that emerged from Chemical Department research—Hypalon® chlorosulfonated polyethylene—in 1935.

Nylon fiber grew beyond all expectations. It also spawned a number of related products, including monofilaments and molding resins. It can be considered the progenitor of the many new products based on condensation polymer technology that were developed by Du Pont and other companies.

In 1937 Bolton returned from a visit to Imperial Chemical Industries (ICI) in England, with whom Du Pont then had an agreement for exchange of technical information, with a few grams of the first sample of high molecular weight polyethylene ever made. Although the ICI polymerization method required a pressure of 3000 at, well beyond the reach of commercial compressors then available, Bolton immediately launched programs to investigate the properties and potential uses of this novel polymer, using samples supplied by ICI. His foresight was rewarded almost two years later when Chemical Department scientists discovered that ethylene could be converted to high molecular weight polymer in an emulsion system at a pressure of only 1000 atm, which could be attained with commercial compressors. Less than three years later, in 1943, this discovery was commercialized as Alathon® polyethylene.

In the course of the research on ethylene polymerization, it was found that carrying out the polymerization in carbon tetrachloride at moderate pressure gave a mixture of low molecular weight products that could be separated by distillation and that were shown to have the structure $Cl(CH_2 CH_2)_n CCl_3$. It was found that this reaction, which was named "telomerization," occurred in addition to polymerizations of many monomers and with a wide variety of endgrouping compounds, called "telogens," which covered a broad spectrum of capability for terminating the growing polymer chain. Although the low molecular weight products of telomerization reactions did not achieve commercial significance, the reaction became widely used to control the molecular weight range of many commercial addition polymers.

The adventitious discovery of the spontaneous polymerization of tetrafluoroethylene by Roy J. Plunkett of the Or ganic Chemicals Department in 1940 was taken up by the Chemical Department. They found a way to make the polymer under controlled conditions and how to fabricate the polymer, which represented a new order of intractability in plastics. Teflon® tetrafluoroethylene resin was commercialized in 1944, in time to make a contribution in World War II. A number of other products based on polymers and copolymers of tetrafluoroethylene have followed.

Research in the Chemical Department in the 1940s led to discovery of the urea herbicides; the first two were marketed in 1953 and 1954. This discovery evolved into a family of herbicides with uses ranging from nonselective weed control in tank farms and railroad rights-of-way to selective control of seedling weeds in cotton and cereal crops.

Also in the 1940s, the Chemical Department first made polyvinyl fluoride and observed the outstanding durability of its films to outdoor exposure—far superior to polyethylene or polyvinyl chloride. This observation eventually led to the commercialization in 1961 of Tedlar® polyvinyl fluoride film, which is used as a pigmented, weather-resistant, laminated overlay for architectural metal siding and paneling.

In 1948 the Chemical Department began research on the concept of a photosensitive, etchable plastic printing medium as a replacement for metal printing plates. Practical realization of this idea proved exceedingly difficult, and success was not achieved until 1958 with the introduction of Dycril® photopolymer printing plate.

These developments are testimonial to an effective research organization, but organizations tend to reflect the character of their leaders, and Bolton's influence was always felt. He met with his staff managers every Tuesday and Thursday morning. The purpose of the Tuesday meetings was to allow two first-line supervisors to discuss the research of their groups, and each brought one of his chemists to give a brief talk on his own work. This was Bolton's way of maintaining personal contact with the technical staff. A young chemist approaching his first such appearance felt rather like a pitcher who had been called up from the minor leagues to start a World Series game, yet the appearance itself belied that impression. Discussion after the chemist's talk involved only pertinent technical questions or suggestions. Bolton's supportive approach to his research scientists flowed to all of his managers. I recall thinking after my first such meeting that those men seemed genuinely interested in what I was doing, and were pulling for me.

Bolton was not an outgoing person, yet he was an easy and interesting conversationalist at the occasional social gatherings that involved members of the Chemical Department. He had detailed knowledge of all of the research programs in the department and, to a surprising extent, of many of the programs being carried out in research organizations of other departments of the company. He had a deep personal interest in research personnel as individuals. In his Perkin Medal Address, he said:

> Since the most valuable research asset is good men, it is the policy of the Company to staff our laboratories with the best-qualified men available. As stated recently by Dr. James B. Conant, "Ten second-rate men are no substitute for one first-rate man." This has certainly been the experience of Du Pont's research organization [p. 112].

His interest in his scientists did not stop with their acquisition; he knew them all by name and by accomplishment. He was responsible for transferring a number of them to more responsible positions in other departments.

He was adamant about giving credit to the scientists who made the discoveries, and this was an important factor in the success of his organizations. It is revealing that he was exempt from the verbal caricatures that the troops in most organizations make among themselves about their superiors; he simply engendered respect.

Bolton received honorary D.Sc. degrees from his alma mater, Bucknell University (1932), and from the University of Delaware (1942). He was a member of the Board of Trustees of Bucknell (1937–1967) and was Trustee Emeritus (1967–1968). He served on the visiting committees of MIT (1938–1939) and Harvard (1940–1941).

He was a regional director of the American Chemical Society (1936–1938) and director-at-large (1940–1943). He served on the Advisory Board to *Industrial and Engineering Chemistry* and *Chemical and Engineering News* (1948–1949).

He was honored with: The Chemical Industry Medal, 1941; The Perkin Medal, 1945; Election to the National Academy of Sciences, 1946; and The Willard Gibbs Medal, 1954.

None of the twenty-one U.S. patents on which Bolton was named inventor or coinventor led to a significant commercial achievement; his technical contributions were those of a manager and were made through the organizations he directed. He had a strong belief in establishing good industry–university relations and was instrumental in hiring top university professors as consultants at a time when the practice was not prevalent. His first such venture turned into a fortuitous and fortunate double: he engaged Roger Adams as a consultant, but Adams balked at Bolton's proposal that he visit Du Pont every month and suggested that Bolton also hire his Illinois colleague C. S. (Speed) Marvel so that the two could visit on alternate months. Bolton did so, beginning one of the longest and most fruitful consulting arrangements in the history of industry-university relationships. Bolton served for many years as chairman of the Du Pont Committee on Aid to Education and was in considerable measure re sponsible for a substantial increase in the amount and scope of grants to universities in support of technical education and research.

Bolton married Marguerite L. Duncan in 1916, and they had three children, Marjorie Louise (Mrs. Robert A. Orr), Elmer K., and Duncan A.

Bolton retired from Du Pont in 1951. He continued to follow the scientific literature and, at his request, Du Pont abstracts of research reports. He died 30 July 1968 at the age of eighty-two.

He was a great leader, endowed with the trait that he often urged on his associates: he had "eine gute Nase."

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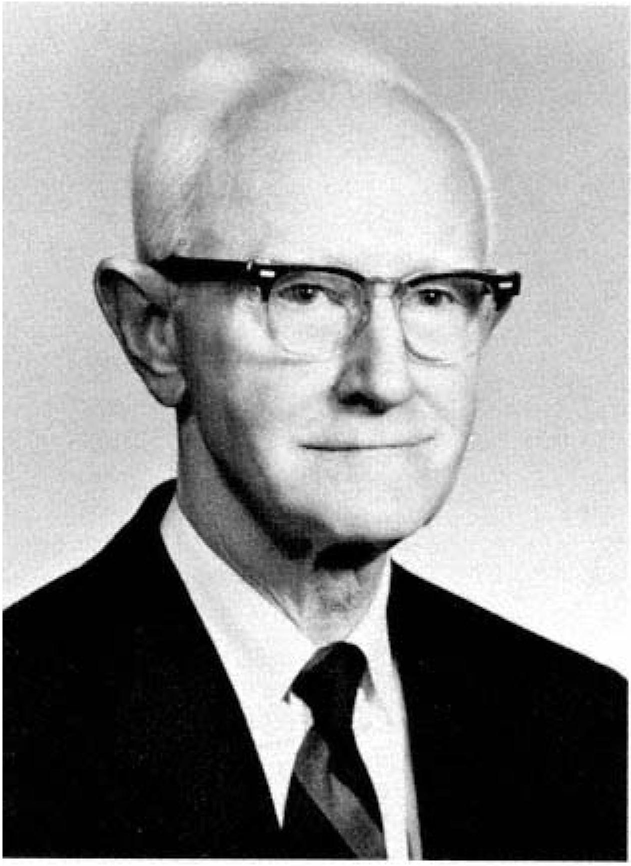

W H Bradley

Wilmot Hyde Bradley

April 4, 1899-April 12, 1979

by V. E. McKelvey

Wilmot Hyde Bradley was both an ordinary and an extraordinary man. He was ordinary in the sense that he was of average build, had plain tastes, was unpretentious, and considered himself to be no better than anyone else. He was, in fact, a superb geologist with extraordinarily broad interests. He was a generalist, not in the sense of one who hasn't specialized in anything or who knows a little bit about a lot of things, but in the sense of one who has demonstrated the ability to probe deeply into diverse subjects and to contribute new and illuminating knowledge about them. He was extraordinary also in his leadership capabilities, exercised first as chief of the Branch of Military Geology, which he helped to found in 1943, and then as chief geologist of the U.S. Geological Survey from 1944 to 1959. In the eyes of his associates he was extraordinary because of his exceptional warmth and selfless attitude toward others, the personal interest he showed in their work and problems, his good humor and wit, and his ability to make almost any conversation an interesting, stimulating exchange among all the participants.

Bill, the son of Anna Miner Hyde and John Lucius Bradley, was born April 4, 1899 in Westville, Connecticut, a suburb of New Haven. He attended grammar school in Westville, high school in New Haven, and college at the Sheffield

Scientific School of Yale University. He enlisted in the U.S. Naval Reserves in 1918, and his junior year at Yale was chiefly given to naval-officer training on a two-masted schooner, supplemented with courses in navigation, spherical trigonometry, and related subjects. Although he had majored in engineering in his first years at Yale, he switched to chemistry in his senior year. He soon had doubts about his choice, however, and his friend and later USGS colleague, Arthur A. Baker, then a first-year graduate student, suggested that geology might be more to his liking. After nine weeks of exposure to an introductory course taught by Alan Bateman, he changed his major to geology and graduated from Yale in 1920 with a Ph.B. That summer he served as field assistant to Frank C. Calkins of the U.S. Geological Survey in the Cottonwood District in the Wasatch Mountains of Utah.

The following two years were spent in graduate studies at Yale, with summers as a geologic aide to Julian D. Sears of the Survey on the north flank of the Uinta Mountains. During the second of these field seasons with "J.D.," in which James G. Gilluly also served as an assistant, Bill first saw and became fascinated with the Eocene Green River Formation. He learned that David White, then chief geologist, was looking for someone to work full time on the Green River because of its oil shale potential; he volunteered for the assignment and was taken on full time by the Survey in the fall of 1922 to work on the Green River.

The fall of 1922 thus began Bill's union with two of his lifelong loves-the Green River Formation and the Survey. A third was joined during the same period when, on November 4, he married Catrina van Benschoten, also of New Haven and a friend since childhood. Their marriage—blessed with two daughters, Anne and Penny—was a devoted one, lasting until Bill's death.

Bill's Geological Survey Professional Paper 140, "Shore Phases of the Green River Formation in Northern Sweetwater County, Wyoming," served also as his doctoral dissertation, and he received his Ph.D. from Yale in 1927. His sound academic training laid a solid foundation for the career that followed. He was appreciative of his teachers' stimulus—particularly Adolph Knopf at Yale who inspired him to search for causes and dependent relationships among natural phenomena and processes.

Bill's broad interests had been stimulated before he reached the college level by his father, a dentist, who was interested in all things mechanical and electrical, and who taught Bill how to wire motors and to make and experiment with various kinds of wet batteries, Leyden jars, and other electrical devices. His mother and her maiden sister, Carolyn, also played strong parts in arousing Bill's curiosities, for they were intensely interested in birds, flowers, butterflies, and moths and took him on numerous trips to nearby woods and meadows, and to Yale University's Peabody Museum as well.

With this background, it is easy to understand how Bill's interest in the Green River Formation encompassed almost every aspect of its composition and geologic origin. His first scientific paper described "Fossil Caddice Fly Cases from the Green River Formation of Wyoming," and subsequent papers dealt with its mineralogy, plant and animal fossils, physical structures such as varves and mud cracks, stratigraphy and areal geology, and geochemistry, as well as the climate of the Green River Epoch and the paleolimnology of the Green River lakes. In his later years, Bill broadened his study of oil shale to its formation in the modern environment. Motoaki Sato, Bill's colleague in some of these studies, says of them:

> One of Bradley's main scientific concerns was to find a modern lake that was producing rich organic ooze with very little clastic material. He was

excited when he heard R. S. A. Beauchamp, an English limnologist, describe a remarkable organic ooze that was forming in the northern part of Lake Victoria. The organic ooze consisted almost wholly of algal matter which would not decay in a warm, wet, and oxidizing environment. Bradley immediately began his search for such lakes and found that only one more lake in equatorial East Africa and two lakes in Florida, one of which is Mud Lake, are known to be accumulating this kind of pure algal ooze. His tireless and all-out effort in understanding the limnology, microbiology, and geochemistry of Mud Lake, Marion County, Florida, began soon afterwards. Bradley's approach was characteristically multidisciplinary. Not only did he examine the algal ooze microscopically to identify microorganisms and the evidence for their activities, he mobilized experts in the nation to identify various organic compounds and microbes existing in the organic sediment, and to develop tools for sampling the *ooze* and conducting *in situ* measurements of geochemical parameters. His main effort was directed to unravelling the secret of *how* the algal matter resisted decay in a subtropical to tropical environment and how relatively oxygen-rich algal matter changed with time to the hydrocarbon-rich organic matter of oil shale in an ordinary diagenetic environment. His pioneering efforts in this realm of science have given impetus to many students of organic sedimentation. One of the most thorough documentations of geochemical parameters existing in organic sediments, which Bradley worked on even after his retirement from the U.S. Geological Survey, is expected to be published shortly.[1]

With respect to the significance of Bill's overall work on the Green River, Erle Kauffman of the U.S. National Museum writes:

Bill Bradley was the true "father" of non-marine aquatic paleoecology and paleolimnology. He pioneered modern technique by crossing the line between recent and ancient ecosystems to become a respected limnologist and aquatic biologist, and then to apply that knowledge with great precision to the interpretation of ancient ecosystems. His early work was twenty to thirty years ahead of its time, and stands today as one of the best examples of fresh water paleoecology on record. A measure of Bradley's perception as a student of paleoecology and paleoenvironments has come to light in the recent "Green River controversy," which seemed to pit

[1] Motoaki Sato 1979: personal communication.

Bradley's older stratified fresh water lake model against a newer playa lake model for the origin of Green River oil shale. Bradley reasoned that varved oil shales with their high organic residues and beautifully preserved biota could only have formed in a permanently stratified lake, characterized by a thick, poorly circulated, O_2 depleted and H_2S enriched hypolimnion and a metalimnion and epilimnion with a diverse fresh water biota, high productivity, and seasonal fluctuation producing well defined varves.

From the outset, Bill recognized a significant stratigraphic interruption in the middle of the Green River formation in which the stratified lake model broke down. He termed this the "middle saline facies," or the Wilkins Peak Member of the Green River Formation. He noted paleogeographic restriction of the Wilkins Peak and determined that the lake had shrunk considerably and was without outlet at this time. He reviewed the geochemistry of the unit, and especially the unique suite of authigenic saline minerals. From this he concluded that evaporation greatly exceeded fresh water input, and that a shallow, clear saline lake persisted, with broad episodically exposed brine flats. Bradley was describing a playa; he simply never called it this. Subsequent filling of the Green River lake basins resulted in the redevelopment of a large stratified freshwater lake system, possibly with a saline hypolimnion. Subsequent detailed stratigraphic analyses of the Wilkins Peak Member by others have revealed extensive new evidence in support of the playa lake model for the Wilkins Peak phase of the Green River. Whereas this started out as a sober analysis of a specific unit, it mushroomed into a quiet controversy in which some workers insisted on a playa origin for most of the Green River System. From the outset, Bradley agreed that the Wilkins Peak evidence fit the playa model, and even presented new evidence in support of it. But at the same time he quietly warned of extrapolating to make one depositional system (the playa model) fit the whole Green River. His prediction was correct, the controversy has run its course, and now in the aftermath Bradley's perception and the breadth of his observations have re-emerged. His original stratified lake model, from the standpoint of integrated geochemical, sedimentological, paleogeographic, and paleoecological evidence, is still supported for most of Green River time, and most of the varved oil shale.[2]

Although the Green River Formation was the stimulus for much of Bill's work, his field studies included geologic map

[2] Erle Kauffman 1979: personal communication.

ping and evaluation of oil and gas possibilities in southwestern Wyoming and adjacent parts of Utah and Colorado, as well as in southcentral New York. One of the most rewarding of his other assignments was his pioneering work on the C. S. Piggott cores from the deep ocean floor of the North Atlantic between the Grand Banks off Newfoundland and the continental shelf of Ireland. Eleven in all, and averaging nearly 8 feet in length, they were the first obtained from the abyssal depths. Bill's careful studies of these cores, made in collaboration with several others, showed for the first time the possibilities of unravelling geologic, oceanographic, and climatic history from analysis of the sedimentary record on and beneath the deep ocean floor.

Another series of investigations demonstrating Bill's multidisciplinary interests had to do with the dynamics and history of tidal flats in Maine. It is noteworthy that his principal report on these studies was published by the U.S. Fish and Wildlife Service because of their bearing on commercial clamming.

Although the Geologic Division of the Geological Survey had been blessed with fine leadership beginning with its first chief geologist, Grove Karl Gilbert, none gave it better or more dedicated service than Bill Bradley. Wise in the ways in which a scientific organization can be guided to do its best work, Bill led not by command or directive; through inspiration, the contagiousness of his own enthusiasm, and the stimulating effect of his interest in other people and their work and problems, he brought forth their best efforts. Bill's sixteen-year period of service as a branch chief and chief geologist spanned a period of great change in the Geologic Division, including a several-fold expansion in professional personnel and the beginning or expansion of important new programs, such as military geology, airborne geophysics, the geology of radioactive minerals, and engineering geology.

Not only was he able to find solutions to the many difficult problems that he had to face during this expansion, but he found them without affronting or offending those concerned. He won and held the deep respect and affection of his associates.

Through his membership, Bill contributed to the activities of a diverse array of organizations: National Academy of Sciences, American Philosophical Society, American Academy of Arts and Sciences, Geological Society of America, American Society of Limnology and Oceanography, International Limnological Association, American Association for the Advancement of Science, American Association of Petroleum Geologists, Botanical Society of America, Sigma Xi, Society of Economic Paleontologists and Mineralogists, Geological Society of Washington, and the Cosmos Club of Washington. Several of these organizations honored him with awards and high office: the National Academy with its Award of Merit in 1940, the Philadelphia Academy of Science with the F. V. Hayden Medal and Award in 1971, the Geological Society of America with its presidency in 1965 and its Penrose Medal in 1972, the Geological Society of Washington with its presidency in 1946, and the Society of Economic Paleontologists and Mineralogists with honorary membership. Bill also received the Department of the Interior's Distinguished Service Award in 1958 and an honorary Doctor of Science degree from Yale in 1947. His colleagues honored him with *the Bradley Volume Festschrift,* published by the *American Journal of Science* in 1960.

At the conclusion of his forty-eight-year career with the Geological Survey in 1970, Bill and Catrina moved to the west shore of Pigeon Hill Bay, Maine. There he continued writing up his results on the Green River and Mud Lake while enjoying the physical stimulation of outdoor work on their farm. During a visit with him in the fall of 1978, when illness had already begun to overtake him, he was rhapsodic about the enjoyment and satisfaction his life had given him—the love of his family, the excitement of his research, his stimulating and rewarding friendships, and his life with the Geological Survey. His life was a joyous and satisfying one to him and an enriching one for his family, his friends, his scientific organizations, his country and its Geological Survey, and his science. He died of a stroke on April 12, 1979, eight days after his eightieth birthday.

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Photograph courtesy of Josef Karsh

James Bryant Conant

March 26, 1893-February 11, 1978

by Paul D. Bartlett

The career of James Bryant Conant covered a remarkably wide range of human concerns. He was a vigorous and prolific organic chemist, devoted to interpreting chemical reactions on a physical level and applying such knowledge to the structures of important natural products, especially chlorophyll. After fourteen years on the Harvard faculty, he served as Harvard's president from 1933 to 1953 and took an important part in organizing the United States scientific effort in World War II. He then served four years as the chief U.S. representative in Germany, first as high commissioner and then as ambassador. In the ten years from 1957 to 1967, he conducted an influential, in-depth study of American secondary education, resulting in a number of books and many important policy recommendations. He provided an account of this many-faceted career in the autobiography, *My Several Lives.*[1]

Conant was born in Dorchester, Massachusetts on March 26, 1893, the third child and only son of James Scott Conant and Jennett Orr Bryant Conant. His father was a man of few words, but with a lively interest in mechanical arts and drawing, who began as a draftsman and then became the owner

[1] James Bryant Conant, *My Several Lives* (New York: Harper & Row, 1970), 791 pp.

of a successful photoengraving company. Visits by young Conant to this establishment and a small shop-laboratory, provided for him at home by his father, were the only links to chemistry in his early environment. His sisters, Esther and Marjorie, eleven and eight years older than he, respectively, were both artistically inclined. His mother had a warm interest in people, in reform, and in transcendental religious movements. Politically she was basically a dissenter.[2]

After six years in public elementary school, Conant was enrolled in the Roxbury Latin School, which was highly rated for its college preparatory courses, including physics and chemistry. The school's greatest asset, for Conant's purpose, was the science teacher Newton Henry Black, who not only gave a stimulating course but helped and encouraged the boy chemist at every turn. He often joined a few students, including Conant, in sandwich lunches at the physics laboratory. Black provided unknowns for Conant to analyze in his home laboratory, suggested outside reading, allowed Conant to use his own laboratory and sensitive equipment, gave career advice, and later coauthored an elementary text, *Practical Chemistry,* with Conant. Black was instrumental in finding a way for Conant to get effective credit for some of his extra work by anticipating the freshman chemistry course at Harvard. Black also provided Conant with a long-range plan including eventual graduate research with T. W. Richards at Harvard. Thus Conant's lifelong interest in secondary education had a background of personal experience of how important this stage can be in the life of a student.

Although his plan called for graduate research with Richards, another contact made during Conant's third and last undergraduate year at Harvard resulted in an important modification. Having a little time (despite being on the editorial board of the Harvard *Crimson!*) and much zeal to get

[2] *Ibid*., p. 11.

started in research, he arranged to do a special piece of research with Professor E. P. Kohler, newly arrived on the Harvard faculty that year. This essentially extracurricular activity gave such mutual satisfaction that Conant became the assistant in Kohler's advanced organic chemistry course during his first two years in graduate school. He reconciled his newly found enthusiasm for organic chemistry with Black's blueprint for his education by arranging to do a double thesis—two years at "half time" (discounted by the assistantship) with Kohler and one year at full time with Richards. Conant felt that Black never forgave Kohler for this intrusion into a carefully laid long-range plan.[3]

Kohler was a product of Ira Remsen's prolific school of organic chemistry at Johns Hopkins. More than was common in the classical tradition, Kohler was always searching for the rational explanations of organic chemical phenomena. He saw in the developing electronic theory on the one hand, and quantitative experimentation on the other, an escape from dependence on "schools of thought" in the interpretation of chemical phenomena. To all aspects of academic life Kohler brought a rare wisdom and total integrity for which he was respected throughout the Harvard faculty, and which was surely included in Conant's comment "I worked with Kohler so closely as a research student, a teaching assistant, and later as a junior colleague, that I am sure that many of my attitudes and opinions are a consequence of his views."[4]

Conant received his Ph.D. degree from Harvard in 1916, six years after leaving preparatory school. The entry of the United States into World War I brought about rapid changes in the lives of chemists. Conant began the academic year of 1916–1917 in a teaching position at Harvard, which he left for national service, ending at the close of the war as a major

[3] *My Several Lives,* p. 33.
[4] *Ibid.*

in the Chemical Warfare Service. In 1919 Conant became an assistant professor at Harvard. Two years later he married Grace Thayer Richards, daughter of T. W. Richards. This outstanding union was an important source of strength in the shifting scenes of the following half-century. Their honeymoon in continental Europe and Britain was also the occasion for making important scientific and university contacts.

Three of Conant's early papers arose from his summer work in analytical chemistry at the Midvale Steel Company. Beginning in 1919 he turned to research on the mechanisms of some of the reaction types he had encountered during the war. As one thing led to another in his wide-ranging chemical explorations, reaction mechanisms were always a unifying interest. There were many examples of Conant's growing respect for the complexity of reaction mechanisms.

A type of investigation much relied upon by later workers in physical organic chemistry was developed in the studies by Conant, Kirner, and Hussey (1924, 1925) of the reactivity of a series of organic chlorides toward potassium iodide. This study established some reactivity phenomena that had to wait a number of years for final elucidation.

A recurring theme in Conant's approach to reaction mechanisms was the relation between the thermodynamic, or equilibrium, properties of reactions and the reaction rates. He was one of the early organic investigators to face the fact that in some reactions the relations between equilibrium and rate are general and obvious, while in others they are obscure and may even appear nonexistent. Among various equilibrium-rate studies were extensive investigations, mostly with L. F. Fieser (1922–1924), of the reduction potentials of quinones in relation to other reactions. In some of the earlier free radical papers (with L. F. Small and A. W. Sloan, 1926; with N. M. Bigelow, 1928; with R. F. Schultz, 1933) it was shown that bulky aliphatic groups, not in themselves capable of making a free radical stable, could as α-substituents enhance the stability of the already stabilized xanthyl and other diarylmethyl radicals. In partial analogy to metals, free triarylmethyl radicals were found capable of adding to the ends of unsaturated organic systems (with H. W. Scherp, 1931; with B. F. Chow, 1933), a forerunner of a reaction that became important in later polymer technology.

Conant's interests in structure, reaction mechanisms, and electrochemistry, and his feeling for the important problems of biochemistry, all converged upon the respiratory pigments as a major research challenge in the late twenties and early thirties. The heme structure proposed by Küster in 1910 had survived with some revision in the positions of the sidechains by Hans Fischer. It was still being debated whether methemoglobin, the oxidized form that could be reduced back to hemoglobin but could not carry oxygen, was itself an iron hydroxide or oxide. Conant provided definitive evidence in 1923 (from experiments done with his own hands) that oxyhemoglobin contained ferrous iron, while the prosthetic group of methemoglobin was a ferric compound containing no oxygen on the iron. He continued to be fascinated by the unique properties of the oxyhemoglobin system. He probed the details of the absorption-dissociation curves with oxygen and with carbon monoxide, and the oxidation-reduction potentials of related systems; with searching logic he went about as far as he could go in interpreting the interactions of the subunits of hemoglobin and the ligands involved. Further progress would have to await detailed structures by X-ray spectroscopy and a more refined molecular orbital theory, which later interpreted the geometric changes at iron associated with the attachment of molecular oxygen. One of his last chemical accomplishments was the first separation (with W. G. Humphrey, 1930; with F. Dersch and W. E. Mydans, 1934) of a characteristic chemical prosthetic group from the nonheme copper respiratory protein hemocyanin, whose role as an oxygen carrier is its only feature in common with hemoglobin.

In collaboration with Norris F. Hall (1927), Conant pioneered the study of "superacid" solutions, in which the absence of bases comparable in strength to water allowed the differentiation of a wider range of acid strengths than was possible in the usual media for acid-base titration. This interest continued and provided a major method of characterizing the different basic centers in the porphyrin ring. Applied to chlorophyll, such titrations (with B. F. Chow and E. M. Dietz, 1934) revealed three distinguishable basicities at different sites. By electrochemical methods, Conant was able to show (with E. M. Dietz, C. F. Bailey, and S. E. Kamerling, 1931; with E. M. Dietz and T. H. Werner, 1931; with E. M. Dietz, 1933) that chlorophyll was a dihydroporphyrin. Just as he was opening out some of the great complexities of this system and its rearrangement products, he made the momentous decision to quit the field of chemistry to become president of Harvard University.

Other chemical research problems that engaged Conant's attention less comprehensively included the pinacol reduction, the effect of steric hindrance on the reaction of Grignard reagents with carbonyl compounds, diazo coupling, special cases of acid-base catalysis, and the effect of high pressure on organic reactions. In three papers on this subject, initially in collaboration with P. W. Bridgeman (1929), and subsequently with C. O. Tongberg (1930) and W. R. Peterson (1932), the room temperature polymerization of isoprene to a synthetic rubber at 9,000 and 12,000 atmospheres was found to be strongly catalyzed by traces of peroxides and inhibited by hydroquinone but capable of proceeding slowly (despite the presence of hydroquinone) even in the most oxygen-free and peroxide-free samples that could be prepared. Later, the use of high pressure by others led to the important new material, polyethylene. A similarly peroxideinitiated polymerization of *n*-butyraldehyde, analogous to formaldehyde polymerization, was also observed. The polyn-butyraldehyde reverted to monomer at ordinary pressure.

The depth and intensity of Conant's interest in physical, organic, and biochemical research gave little warning to the chemical world of his impending move to the presidency of Harvard University in 1933. For several years before the retirement of President Abbott Lawrence Lowell, there had been general speculation as to his probable successor. A list of the forty candidates considered most probable in Harvard circles did not include the name of Conant. His rise to the top of the list began early in 1933 with a visit from a member of the Harvard Corporation who was much impressed with Conant's clear perception of important educational and administrative issues in the university and his far-sighted views about needed reforms.

The presidency of the university brought an end to Conant's own research and his supervision of graduate students and postdoctoral fellows. But his role as instigator and consultant in some research with G. B. Kistiakowsky and A. B. Hastings kept him involved for several years in weekend conferences. Conant was convinced of the importance of labeling organic compounds with radioactive isotopes. In 1937 the only available carbon isotope for this purpose was carbon-11, with a half-life of about 20 minutes. Despite this limitation, Kistiakowsky and Cramer in 1941 accomplished the labeling of lactic acid at either end with C-11, available for whatever biochemical experiments could be performed in the necessarily short time. Radioactive labeling came into its own a few years later with the availability of carbon-14, a by-product of the atomic energy program.

Conant immediately became as deeply involved in the concerns of the Harvard presidency as he had been in chemistry. In addition, he was drawn rapidly into national affairs by the force of contemporary world events: the rise of the Nazi movement and the looming threat of World War II.

At Harvard, Conant is remembered for a number of important innovations. In the pursuit of excellence in selection of the faculty, he insisted on the sharp definition of tenure so that an assistant professor who was not promoted at the end of his stated term was automatically terminated as a member of the faculty. The adoption of this practice by other universities has been slow but steady. The National Scholarships, instituted early in Conant's presidency, guaranteed that, for a small number of students selected competitively for their scholastic excellence, lack of money was not a barrier to a Harvard education. A small number of University Professorships were established to recognize exceptional scholars whose contributions transcended the usual limitations of departments and of organized teaching.

These administrative steps were taken early in Conant's presidency. Further progress came after World War II with respect to the educational process itself. After long consideration and faculty debate, new emphasis was placed on "general education" in the major areas of scholarship. Conant himself took part for three years in the teaching of such a course, based on case histories in experimental science. During the postwar period there was an extensive reevaluation of the professional schools; the School of Education, for example, was reoriented toward the training of school administrators rather than teachers. Also under Conant's leadership, Harvard abandoned the anachronistic practice of teaching every undergraduate course twice, once for Harvard men and in a second section for Radcliffe women. With somewhat less unanimity in the Harvard community, women were subsequently admitted to the Medical School—and even to the Law School. Conant also set a pattern for deprofessionalizing intercollegiate athletics by placing it, and its budget, under a committee of the faculty, abolishing athletic scholarships, and upgrading the status of intramural sports.

There was no escapism in Conant's nature. He was convinced that the rise of Hitler to power was the start of an inexorable chain of events threatening the United States no less than the nations of Western Europe. The seriousness with which he viewed the Nazi threat was illustrated in the first year of his presidency (1933), when Ernst F. S. Hanfstaengl of the class of 1909 offered Harvard a scholarship for a student to spend a year in Germany. To this close friend of Hitler, Conant replied in an open letter: "We are unwilling to accept a gift from one who has been so closely associated with the leadership of a political party which has inflicted damage on the universities of Germany through measures which have struck at principles we believe to be fundamental to universities throughout the world." Conant's long-held conviction of the seriousness of the Nazi threat led, after the invasion of Norway, to an activist position as he became one of the charter members of the Committee to Defend America by Aiding the Allies. He devoted himself to overcoming the isolationism of the day, testifying in favor of the Lend-Lease Bill and promoting an innovative civilian organization for military preparedness, the National Defense Research Committee (NDRC).

The purpose of this organization, set up in 1940 by President Roosevelt under the chairmanship of Vannevar Bush, was to mobilize civilian scientists and engineers for the development of new instrumentalities of war. Financed by the government, the NDRC let contracts for military research and development in academic and industrial laboratories, each one under a principal investigator chosen for his relevant scientific background. This had the effect, a year and a half before the entry of the United States into the war, of bringing to bear a large amount of scientific talent on new and old problems of war. Problems were chosen in consultation with the military, but in their exploration the great variety of thinking and methodology in the scientific community was free to make its contribution. Conant headed Division B, dealing with chemical warfare, explosives, and many chemical aspects of munitions. Through the NDRC, for the first time, the considered views of civilian scientists on military matters could be heard directly by the government—even when they disagreed with the prevailing military doctrine.

During this period of preparedness—in early March 1941—Conant made a fruitful trip to England, establishing many scientific contacts as well as being received by the king, by Prime Minister Churchill, and by members of the cabinet. This timely initiative led to a rapidly expanding exchange of technical information between the soon-to-be allies.

In the same year a further organizational change created the Office of Scientific Research and Development within the Executive Office of the President, with Bush as chairman. Conant became chairman of the NDRC, which remained the larger part of the new organization, and he acquired direct responsibility for the NDRC work on uranium fission; Conant and Bush became the two technical members of the cabinet-level top policy group supervising the atomic bomb project. On Conant's recommendation in the spring of 1942, this project was expedited by direct, industrial-scale plant construction carried forth simultaneously on four different ways of preparing fissionable material for atomic weapons. Three of the four methods were successful, and all contributed to the successful bomb of 1945.

Also in 1942 Conant served on a committee chaired by Bernard Baruch to review the synthetic rubber program, which was making inadequate progress. After a two-month intensive study, the committee prepared a report that reoriented this program. Before the end of the war the United

States was producing synthetic rubber at the rate of a million tons each year.

While Conant's energies were preempted by these urgent matters of national policy, Harvard was essentially in a holding pattern educationally, while doing as much as possible in the way of research and other services for the government. Conant had felt that giving priority to the war effort was a matter of survival, but when the war was over he reminded the university that its mission of increasing the world's knowledge was incompatible with any continuance of secret or classified research for governmental sponsors. It became firm Harvard policy that all research done at the university must be freely publishable. The real innovation had been the great participation of universities in the war effort; the new policy was a matter of holding that innovation to its historical setting and not letting it get out of hand. Not all universities adopted this position.

In 1946 Conant was invited by President Truman to be chairman of the newly established Atomic Energy Commission. Though declining this appointment, he served actively for the next six years on the AEC's part-time General Advisory Committee under the chairmanship of Robert Oppenheimer. When President Truman in 1950 decided to proceed with development of the hydrogen bomb, it was contrary to a unanimous recommendation of the AEC General Advisory Committee. In the same year, however, the president appointed Conant chairman of the new National Science Board, the policymaking body of the National Science Foundation. Conant was involved in appointing the first director of the NSF, Alan T. Waterman, as well as in guiding the operational policies of the Foundation. These wise policies have undergone only a slow evolution in the intervening decades, although the budget of the Foundation has grown by nearly three orders of magnitude.

In 1950 Conant was the choice of the nominating committee as president of the National Academy of Sciences, of which he had been a member since 1929. The presidency of the Academy had generally been regarded as an honor for which one was chosen and elected without a contest, and Conant accepted the nomination in that spirit. In the meantime, there was a growing opinion among Academy members, spearheaded by the Chemistry Section, that the Academy required a full-time president to meet the challenges of the postwar era. It was felt that Conant—with his many obligations as president of Harvard—would be unable to make such a commitment. After Conant's name was placed before the annual business meeting, members of the Chemistry Section offered the name of Detlev W. Bronk as an alternative. Reached by phone during the meeting by Vannevar Bush in an attempt to resolve the conflict, Conant, unwilling to run against his friend, withdrew his name and Bronk was elected.

During the first term of President Eisenhower, 1953 through 1957, Conant was asked to serve as U.S. high commissioner to Germany and to assume the post of ambassador when the establishment of the German Federal Republic should be ratified. This prospect was so attractive to him that he made it the occasion of his retirement as president of Harvard, obtaining a leave of absence for the second semester of 1952-53, his twentieth year as president. With his long acquaintance with Germany and his appreciation of German science and universities, he was admirably suited for this role in a period of reconstruction. In the course of this assignment he developed warm relations with Chancellor Adenauer and accompanied him on two trips to the United States.

Both the character and the mechanics of this mission to Germany contrasted sharply with Conant's intensive national service during the war. In that grave emergency, American democracy rallied to the need for expediting important tasks by many new methods. In the diplomatic mill, the democratic system was equally proficient at obstructing uncontroversial undertakings with rules of procedure, checks, and balances. For him to become ambassador it was necessary not only that the Allied Powers ratify the treaty setting up the Federal Republic (which required, in the case of the French, over two years after the signing of the treaty), but also that the United States Senate confirm his appointment as ambassador. This finally occurred more than a week after the ceremony at which the three Allied high commissioners had been scheduled to present their credentials as ambassadors to the new German president. On hearing the reason for Conant's special interim status when he attended this ceremony, the French ambassador remarked graciously that he would have thought such a thing could happen only in France.[5]

As U.S. high commissioner, Conant had the major duty of trimming down an organization larger than Harvard University, preparing it for sudden liquidation at an unpredictable time, and establishing the embassy. The largest diplomatic issue, which continued throughout his time in Germany, concerned the Russians' destruction of the unity of the Berlin occupation and the obstacles this imposed on reconstruction in the Allied zone. Conant made frequent visits to Berlin and did as much as his position allowed to provide solutions to the countless problems that arose. Probably the most satisfying aspect of his role lay in his contacts with the German governmental, educational, and scientific leaders. He addressed many groups in German and was widely appreciated for his understanding of their ways and their problems. All this helped to hold in perspective the occasional harassment in Washington from politicians such as Senator

[5] *My Several Lives,* p. 591.

Joseph McCarthy, who reported that there were, in the libraries of the U.S. Information Agency, 30,000 books by Communist authors, "many of them in Germany." Much was made of this at budget time.

At the end of the first Eisenhower term (1957), Conant resigned as ambassador and turned with vigor to one of his long-standing interests, American secondary education. His final experience in Germany came in 1963, when he was invited by Mayor Willy Brandt and the Ford Foundation to spend a year and a half in Berlin helping with the establishment of a Pedagogical Center, designed to disseminate information about primary and secondary education through conferences and consultations with teachers, school administrators, and professors of education. He probably played a critical role in rallying support for this project. In other ways this stay in Berlin tied together his interests in German culture, science, politics, and education extending over a period of forty years. He unquestionably had an influence on adjustments that have been made in German education to keep it viable during drastic changes in political and intellectual climate.

Between 1957 and 1963, with the support of the Carnegie Corporation, Conant conducted a study in depth of American high schools. He had been keenly aware of the importance of this subject, both as a university president and as a statesman of science. In the first year, he and his staff of four visited 103 schools in 26 states; Conant himself participated in more than half of these visits. The first of the books to emerge from this study was *The American High School Today,* published in 1959, which offered specific recommendations for numerous improvements, especially in the teaching of foreign languages. Since the inclusion of an important degree of scope in the curriculum required a critical size of the faculty, Conant urged consolidation of small high schools into comprehensive schools. Criticism of American education was widespread at the time in the wake of the launching of the *Sputnik* satellite in 1957, and *The American High School Today* was on the best-seller list for several weeks. The controversy it provoked helped give impetus to extensive school reforms.

The next project was an examination of the schools of the inner cities such as Chicago, Philadelphia, and Detroit and of the suburban areas surrounding them. In the book *Slums and Suburbs* (1961), he warned of the excessive numbers of unemployed and out-of-school black youth, which he called "social dynamite"—a term whose aptness was widely appreciated in the social upheaval witnessed five years later. Although he urged vigorous governmental attention to a problem with which black leaders and white liberals were greatly concerned, he did not embrace the doctrine that the solution required artificial integration of schools where communities themselves were segregated. His solution was rather to correct the financial disadvantage under which many inner-city schools operated. This addressing of the problem as a purely educational and economic, rather than a racial, one cost him the support of some very active groups.

Equally controversial were the conclusions from an examination of teachers' colleges and schools of education. *The Education of American Teachers* (1963) included criticisms of the curricula of these institutions and also urged that certification of teachers be placed in the hands of bodies independent of the schools of education. This book aroused protest among professional educators, an uproar Conant partially escaped by being on his mission in Berlin at the time of publication.

In the last of the reports from this study of education, *Shaping Educational Policy* (1964), Conant urged greater involvement of state administrations in educational policies. An

Educational Commission of the States, recommended in this book, came into being a few years later and has since been useful in shaping consistent educational policies in the participating states. After his return from Berlin in 1965, Conant continued his writing and publishing for several years, spending the winters in New York and the summers in Hanover, New Hampshire.

My own first and principal contact with Conant was as a graduate student at Harvard from 1928 to 1931, during the first of his several lives. At the time of our first interview, in the spring of 1928, his life was complicated by an overdue move from an old, untidy laboratory into a fine new one, and keeping everything organized the while. His most memorable remark on that occasion to his prospective research student was: "Frankly, I'm a slave driver." I took this for the hyperbole that it was; it was already evident from the record that he was in academic chemistry to get things done, but none of his scientific work could have been done by driving slaves. His attitude toward his students and their research problems was always one of open-mindedness. A visit to a coworker in the laboratory would often open with "What's new?" If something interesting was reported, he rarely prescribed the next experiments, but was more likely to ask: "What are you going to do next?" The implied expectation that the student would have good ideas of his own was a constant stimulus toward its fulfillment.

I came to think of Conant as probably the most truly intelligent man I ever knew. For him, objectivity seemed to be a natural state of mind, rather than something for which one must strive. The habit of viewing the world as it revealed itself, rather than as he might wish it to be, was fundamental to Conant's professional, political, and administrative life. The importance of a problem or an activity was something inherent in its place in science or society, and completely transcended such subjective considerations as one's own pleasure in pursuing it. When, with a full range of choice, he repeatedly moved from a field where he had a strong position into something else not always even closely related, it was in pursuit of a bigger challenge, a more important activity. He chose the chemistry underlying the life process rather than more abstract principles and the conduct of a great university rather than any part of it. He responded to world events calling for rare insight along with decisive action. There was never any appearance of looking back, with the possible exception of a comment in his autobiography that, in retrospect, "the best years" had been those on the Harvard faculty.[6]

Although he probably knew that he could not endow others with his own perceptiveness and mobility in moving to ever more important things, he warned his students of the dangers of becoming too committed to their early research interests. After reading one former student's first independent paper, he wrote: "I hope you will not continue to work in this field. . . ." To another, who showed him a proposed plan for a National Research Fellowship: "If this is completely successful, will it be anything more than a footnote to a footnote in the history of organic chemistry?" Both students took his advice and lived to appreciate its wisdom.

Conant's participation in conventional competitive sports was apparently confined to a short period at the age of nine or ten when he and his boyhood friends had outgrown a preoccupation with toy soldiers and turned to football. During one season he was captain of a successful neighborhood team. As sports had to compete for his leisure time with an interest in electricity, and later in chemical experimentation, the latter's expanding fascination won out entirely by

[6] *My Several Lives,* p. 59.

the time the program at the Roxbury Latin School was well under way.

Although there is no record of any later interest in games or organized sports, Conant always enjoyed vigorous hiking and climbing in the hills of New England. He was aware, however, that the Presidential Range in New Hampshire would not even be called "mountains" in the world's mountain climbing circles. In his forties, while on a family vacation in the Sierra Nevada, he met a Harvard alumnus who skillfully introduced Conant to the techniques and pleasures of rock climbing in a roped party. After describing his bout with terror on the ascent of a 14,254-foot peak, Conant remarked: "If I had but known it, the twenty-four hours which had just passed marked a quantum jump in my psyche. I was ready to become an irrationally enthusiastic mountaineer." [7] In the following two summers he went rock climbing with groups from the Canadian Alpine Club, which brought him intense satisfaction. A year later, a rock climb on Mount Washington in New Hampshire brought a severe back strain that ended his mountaineering as abruptly as it had begun. It is possible to discern in this evolution of Conant's sporting life the same kind of idealism that pervaded his professional life, making him always responsive to the call of something greater, more exciting, or more important. Coming as it did just when he was learning to live without compelling problems of chemical research in which to immerse himself, perhaps the "quantum jump" into intensive mountaineering met a deep and personal need in a timely manner.

The vitality and rational resourcefulness of James Bryant Conant impinged in so many ways on the science, technology, education, and federal policy of twentieth-century America that it is certain that without him these aspects of life today

[7] *My Several Lives,* p. 198.

would have been the worse in a number of important respects.

His health failed in the summer of 1977 and he died in Hanover on February 11, 1978. He is survived by his wife, Grace Thayer Richards Conant, two sons, James Richards and Theodore Richards Conant, and five grandchildren.

The autobiography, *My Several Lives,* by James B. Conant (Harper & Row, 1970), is the definitive source of much of the information presented here. I am greatly indebted to George B. Kistiakowsky and Frank H. Westheimer, coauthors of the biographical memoir on Conant for the Royal Society. We exchanged notes and manuscripts, and at certain points borrowed phrases from one another. See also G. B. Kistiakowsky, "J. B. Conant," *Nature,* 273 (1978):793-95. I thank Dr. Clark A. Elliott, associate curator of the Harvard University Archives, for help in compiling a list of honors and honorary degrees

HONORS AND DISTINCTIONS

Awards

1932	William H. Nichols Medal, New York Section, American Chemical Society
1932	Charles Frederick Chandler Medal, Columbia University
1934	Medal of the American Institute of Chemists
1935	Medal of the Ford Hall Forum, Boston
1936	Commandeur, Ordre National de la Legion d'Honneur
1940	Jewish Veterans' Award for American Leadership
1943	Award for Distinguished Service to American Education, New York Academy of Public Education
1943	Benjamin Franklin Medal, American Philosophical Society
1944	Medal of the Boston City Club
1944	Joseph Priestley Medal, American Chemical Society
1946	U.S. Medal for Merit
1946	Civic Service Medal, Boston City Club
1946	Kentucky Colonel
1947	American Education Award, American Association of School Administrators
1948	Medal for Distinguished Service in the Field of Science, Roosevelt Memorial Association, Inc.
1948	Honorary Commander, Order of the British Empire
1949	Gutenberg Award, Book Manufacturers' Institute, Inc.
1951	Citation for Distinguished and Exceptional Public Service, City of New York
1952	Freedom House Award
1956	Charles Lathrop Persons Award, American Chemical Society
1957	Gand Cross of the Service Order of the Federal Republic of Germany
1959	Wodrow Wilson Award for Distinguished Service, Woodrow Wilson Foundation
1960	Research Institute Award, Research Institute of America
1960	Award for Distinguished Service in School Administration, American Association of School Administrators
1962	Fank H. Lahey Memorial Award for Leadership in Medical Education, Association of American Medical Colleges
1962	Award of the Association of Assistant Principals

1963	Presidential Medal of Freedom
1965	Sylvanus Thayer Award, U.S. Military Academy's Association of Graduates
1965	Great Living American Award
1967	Citation for Distinguished Service to Science Education, National Science Teachers Association
1967	Arches of Science Award, Pacific Science Center, Seattle
1969	Atomic Pioneer Award, President of the U.S. and Atomic Energy Commission
1977	Clark Kerr Medal, University of California, Berkeley

Elective and Honorary Memberships

National Academy of Sciences
Alpha Omega Alpha (medical honor society)
The Chemists' Club
Society of Chemical Industry
Educational Institute of Scotland, Honorary Fellow
American Institute of Chemists
Royal Society, Foreign Member
Royal Institute of Chemistry, Honorary Fellow
American Academy of Arts and Sciences
Deutsche Akademie der Naturforscher Leopoldina
Phi Beta Kappa
Sigma Xi
Alpha Chi Sigma

Honorary Doctoral Degrees

1933	University of Chicago
1934	Columbia University
	Stevens Institute of Technology
	Boston University
	New York University
	Tufts University
	Princeton University
	Yale University
1935	Amherst College
	College of Charleston
	University of Wisconsin

1936	College of William and Mary
	Oxford University
1938	Williams College
	Dartmouth College
1939	Tulane University
1940	University of California
	University of Pennsylvania
1941	Queens University
	Cambridge University
	University of Bristol
1944	University of Algiers
1945	McGill University
	University of North Carolina
	University of Toronto
1946	University of London
1947	University of the State of New York
	University of Illinois
	Hamilton College
	University of Lyon
	Baylor University
	University of West Virginia
1948	University of Massachusetts
	Northeastern University
1949	Yeshiva University
	Wesleyan University
	University of Michigan
1950	Swarthmore College
1951	Jewish Theological Seminary of America
	University of New Zealand
	Canterbury University College
	University of Melbourne
	University of Adelaide
1952	Colgate University
1954	Birmingham University
	Freie Universität Berlin
1955	Michigan State College of Agriculture and Applied Science
	Harvard University
1956	University of Hamburg
1960	Colby College
1961	Keio University
1966	University of New Hampshire

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With Raymond F. Schultz. The dissociation into free radicals of di-tert-butyltetra-diphenylethane. J. Am. Chem. Soc., 55: 2098-104.

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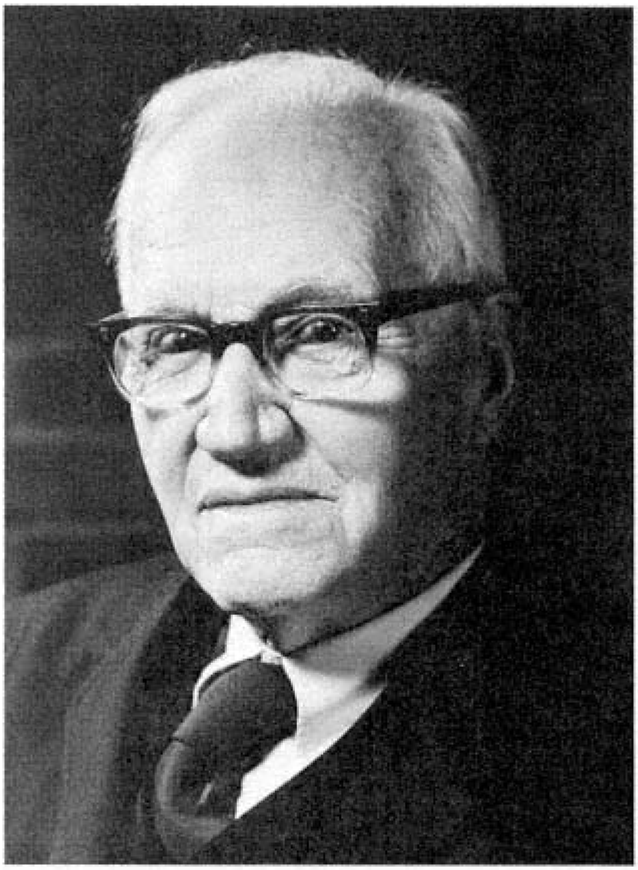

Griffith C Evans

Photograph by G. Paul Bishop

Griffith Conrad Evans

May 11, 1887-December 8, 1973

by Charles B. Morrey

Griffith Conrad Evans was born in Boston, Massachusetts on May 11, 1887 and died on December 8, 1973. He received his A.B. degree in 1907, his M.A. in 1908, and his Ph.D. in 1910, all from Harvard University. After receiving his Ph.D., he studied from 1910 through 1912 at the University of Rome on a Sheldon Traveling Fellowship from Harvard. He began his teaching career in 1912 as assistant professor of mathematics at the newly established Rice Institute, now Rice University, in Houston, Texas. He became professor there in 1916 and remained with the Institute until 1934. While he was at Rice, he was able to attract outstanding mathematicians, such as Professor Mandelbrojt of the University of Paris, and young mathematicians, such as Tibor Rado and Carl Menger, to Rice as visiting professors. Long before Evans left Rice it was internationally known as a center of mathematical research.

Evans was brought to the University of California at Berkeley in 1934 as a result of a nationwide search; he arrived with a mandate to build up the Department of Mathematics in the same way that Gilbert Lewis had already built the chemistry faculty. Evans struggled with himself to effect the necessary changes with justice. His innate sense of fairness, modesty, and tact, as well as his stature as a scientist, brought eminent success. By the time he retired in 1954, he had had the satisfaction of seeing the department evolve into one of the country's major centers of mathematical activity. His retirement did not diminish his interest in science nor subtract from his pleasure at seeing others achieve goals he cherished.

A few years before World War II, Professor Evans and others on the Berkeley campus recognized the importance of the fields of probability and statistics, and Professor Jerzy Neyman was brought to that campus by Evans in 1939 to organize the Statistical Laboratory. A period of rapid growth followed; by the close of World War II the Laboratory had transformed Berkeley into one of the three principal centers of probability and statistics in the country. The size and importance of the Laboratory continued to grow, and a separate Department of Statistics was established in 1955.

Shortly after coming to Berkeley, Professor Evans inaugurated a seminar in mathematical economics, which he graciously held in his home once a week. This seminar became internationally known, providing an inspirational educational activity and establishing a tradition of mathematical economics on the Berkeley campus that continues to the present. The seminar was attended by both students and faculty and promoted a friendly atmosphere in the department.

FUNCTIONAL ANALYSIS

In the first decade of the century, while Evans was a student, functional analysis was beginning to attract the interest of the mathematical community. Classical analysis was concerned with functions of real and complex variables, while functional analysis was concerned with functionals, that is, functions of "variables" that may themselves be ordinary functions or other mathematical entities. For example, if f

denotes any ordinary function continuous for $0 \leq x \leq 1$, we may define a functional F by the equation

$$F(f) = \int_0^1 f(x)\,dx.$$

Evans began his career as a research scientist before he received the Ph.D. degree. He published his first paper in 1909. During the ensuing ten years, he contributed a great deal to the development of the general field of integral equations and more general functional equations. His principal results concerned certain integro-differential equations and integral equations with singular kernels. His interest in this field had been greatly stimulated by his contact with Professor Vito Volterra at the University of Rome. He received early recognition for this important work in 1916 when he was invited to deliver the prestigious Colloquium Lectures before the American Mathematical Society on the subject "Functionals and their Applications" (see bibliography, 1918).

POTENTIAL THEORY IN TWO DIMENSIONS

In 1920 Professor Evans published the first of his famous research papers on potential theory. He was among the first to apply the new general notions of measure and integration to the study of classical problems. In the course of this research, he introduced many ideas and tools that have proven to be of the utmost importance in other branches of mathematics, such as the calculus of variations, partial differential equations, and differential geometry; for example, he used certain classes of functions that are now known as "Sobolev spaces."

Introduction to Potential Theory. The central idea in potential theory is the notion of the potential of a distribution in R_3. Given a distribution of mass g, we define its potential U by the equation

$$(1) \qquad U(M) = \int_W |MP|^{-1} g(P)\, dP$$
$$(W = R_3,\ M = (x,y,z),\ P = (\xi,\eta,\zeta))$$

whenever this is defined. In case g is Hoelder continuous[1] for all P and vanishes outside a compact set, then U is of class C^2 and its second derivatives are Hoelder continuous.[2] In this case:

$$(2) \qquad \Delta U(M) \equiv U_{xx}(x,y,z) + U_{yy}(x,y,z) + U_{zz}(x,y,z)$$
$$= -4\pi g(M), \qquad M = (x,y,z).$$

A solution that satisfies (2) with $\Delta U(M) = 0$ on some domain is said to be "harmonic" on that domain. Such a function has derivatives of all orders.

The fundamental problem in potential theory is the Dirichlet problem. Roughly speaking, this consists in proving the existence and uniqueness of the function U that satisfies Laplaces equation on a given domain G, is continuous on $\overline{G}$ (the closure of G), and takes on given continuous boundary values on the boundary ∂G of G.

Another problem, the Neumann problem, is to show the existence (and uniqueness except for an arbitrary additive constant) of a function V that satisfies Laplaces equation on G, is continuously differentiable on $\overline{G}$ and for which the

[1] A function g is Hoelder continuous on a set S if, and only if, $|\phi(P) - \phi(Q)| \leq L \cdot |PQ|^{\mu}$ for some constants L and μ, with $0 < \mu, < 1$ and all P and Q are both on S.

[2] See Oliver Dimon Kellogg, *Foundations of Potential Theory* (New York: Dover, 1929), p. 38 or 152. for instance: "This could be called a 'classical result.'"

outer normal derivative $\partial V/\partial n$ takes on given continuous values on ∂G.

The function $u(r,\theta)$, defined by[3]

$$(3)\quad u(r,\theta) = \begin{cases} \dfrac{1}{2\pi}\displaystyle\int_0^{2\pi} \dfrac{(1-r^2)}{1+r^2-2\quad\cos(\phi-\theta)} f(\phi)\,d\phi, & r<1 \\ f(\theta), & \textit{if } r=1, \end{cases}$$

is the solution of the Dirichlet problem in the case where G is the unit circular disc in R_2. In case

$$(4)\qquad \int_{\partial G} g(\phi)\,d\phi = 0 \quad\text{and}\quad \int_{\partial G} V(1,\theta)\,d\theta = 0,$$

the solution of the Neumann problem with boundary values $g(q)$ on ∂G proceeds as follows. Let

$$(5)\quad v(r,\theta) = -\frac{1}{2\pi}\int_0^{2\pi} \log\,[1+r^2-2r\cos(\phi-\theta)]\,g(\phi)\,d\phi.$$

It is easy to see that rv_r is harmonic on G, and

$$(6)\qquad rv_r(r,\theta) = \frac{1}{2\pi}\int_0^{2\pi} \frac{(1-r^2)}{1+r^2-2r\cos(\phi-\theta)}\,g(\phi)\,d\phi - \frac{1}{2\pi}\int_0^{2\pi} g(\phi)\,d\phi.$$

The first term on the right in (6) is the solution of the Dirichlet problem with boundary values $g(\theta)$. If (4) holds, the second term is zero and V is one of the desired solutions.

Among Evans' first results were those concerning the function

[3] This is Poisson's Integral Formula.

$$(7)\quad u(r,\theta) = \frac{1}{2\pi}\int_0^{2\pi}(1-r^2)[1+r^2-2r\cos(\phi-\theta)]^{-1}dF(\phi),$$

where $F(\varphi)$ is of bounded variation and periodic. Evans proved the following:

- the function $u(r,\theta)$ is harmonic in G, the unit disc in R_2;
- $\int_0^{2\pi}|u(r,\theta)|\,d\theta$ is bounded for $r<1$;
- $u(r,\theta) = u_1(r,\theta) - u_2(r,\theta)$ each u_i being harmonic and non-negative on ∂G;
- if $P = (1,\phi)$ is a point on ∂G, where $F(\phi)$ is continuous and $F'(\phi)$ exists and $F'(\phi) = f(\phi)$, then $u(r,\theta) \to f(\phi)$ as $(r,\theta) \to (1,\phi)$ "in the wide sense"; i.e., $(r,\theta) \to (1,\phi)$ remaining in any angle with vertex at $(1,\phi)$.
- If F and F' are continuous, then (7) reduces to the solution of the Dirichlet problem with continuous boundary values $f(\phi)$.

Conversely, if we assume that $u = u_1 - u_2$ where each $u_i \geq 0$ and is harmonic on G, then u is given by (7).

Early Discussion of the Dirichlet Problem. The first attempt to solve the Dirichlet problem was made by Green in 1828.[4] His method was to show the existence of a Green's function of the form

$$G(Q,P) = \frac{1}{r} + V(Q,P), \qquad r = (P,Q).$$

This function is the Green's function for the region R and the pole P. In terms of this Green's function we have

$$U(P) = -\frac{1}{4\pi}\iint_S U(Q)\,\frac{\partial}{\partial n}G(Q,P)\,dS,$$

[4] See Kellogg, *Foundations of Potential Theory,* p. 38.

where S is the boundary of R. This development is based, however, on the existence and differentiability of G (Q,P) which is obtained using physical considerations and so is not logically suitable for a mathematical derivation.

In 1913 Lebesgue gave an example of the impossibility of the solution of the Dirichlet problem.[5] The region R can be obtained by revolving about the x-axis the area bounded by the curves

$$y = e^{-1/x}, \qquad y = 0 \quad \text{and} \qquad x = 1.$$

This type of region is called a Lebesgue spine. It can be shown that the region obtained by revolving about the x-axis the area bounded by the curves

$$y = x^n, \qquad y = 0, \qquad x = 1, \qquad n > 1,$$

is a regular region; i.e., the Dirichlet problem is always solvable.

The Logarithmic Potential Function. A similar theory holds for the two-dimensional situations. One considers the logarithmic potential function in R_2, defined by

$$(8) \quad U(M) = \int_W \left[\log\left(\frac{1}{MP}\right)\right] g(P)\,dP, \qquad M = (x,y), \qquad W = R_2$$

whenever this is defined. If g is Hoelder-continuous for all P and vanishes outside a compact set, then U is of class C^2, and its second derivatives are Hoelder-continuous everywhere. In this case,

$$(9) \quad \Delta U(M) = U_{xx}(x,y) + U_{yy}(x,y) = -2g(M), \qquad M = (x,y).$$

A solution of (9) that satisfies $\Delta U(M) = 0$ on some domain is

[5] *Ibid.,* p. 285, 334.

said to be harmonic on that domain; such a function has derivatives of all orders.

The Dirichlet and Neumann Problems in Space. The solution of the Dirichlet problem in the unit sphere S is given by[6]

$$(10)\quad u(M) = \begin{cases} (4\pi)^{-1} \iint_S (1-r^2)(MP)^{-3} f(P)\, dS, & 0 \le r \le 1, \\ \qquad (r = 0M), & M = (x,y,z) \\ f(M) \qquad , r = 1 & (S = S_1, \quad S_r = \partial B(0,r)). \end{cases}$$

The solution of the Neumann problem with given values $g(M)$ of the normal derivative is obtained as in the case of the unit circle as follows. Let

$$(11)\qquad v(M) = -(4\pi)^{-1} \iint_S (MP)^{-1} g(P)\, dS.$$

Then it is easy to see that rv_r is harmonic and

$$(12)\qquad rv_r(M) = \frac{1}{4\pi} \iint_S (1-r^2)(MP)^{-3} g(P)\, dS$$

$$- \frac{1}{4\pi} \iint_S g(P)\, dS.$$

The first term on the right is just the right solution of the Dirichlet problem with the boundary values $g(M)$. If $\iint_S g(M)\, dS = 0$, then v is a desired function.

Evans and his colleague H. E. Bray proved a necessary and sufficient condition that a function u, harmonic on the

[6] This is Poisson's Integral Formula for three dimensions.

unit ball, be given by the formula

$$(13) \qquad u(M) = (4\pi)^{-1} \iint_S (1 - r^2)(MP)^{-3}\, dG(P)$$

for some distribution $G(e)$ on S, is that

$$\iint_S |u(M)|\, dS \quad \text{be bounded for} \quad 0 \leq r \leq 1,$$

or that $u = u_1 - u_2$ where u_1 and u_2 are non-negative and harmonic on $B(0,1)$. If $F(e)$ is a distribution on S, and if $\lim_{\rho \to 0} (4\pi,\rho)^{-1}. |F[B(P,\rho)]| = f(P)$, then $u(M) \rightarrow f(P)$ as $M \rightarrow P$ in the wide sense (i.e., M remains in a cone with vertex at P).

The Riesz Theorem. A function V is said to be "superharmonic" on a domain Ω if, and only if, (i) it is lower semi-continuous and $\not\equiv +\infty$ on W, and (ii) $V | M | \leq$ its mean value over the surface of any sphere with center M that lies with its interior in Ω.

Professor Evans proved that any potential function of a positive mass is superharmonic on any domain on which it is defined. Evans also gave the simplest proof of the following theorem due to F. Riesz:

Suppose u is superharmonic on a domain Ω, and D is any domain, the closure of which is compact and lies in ?. Then

$$u(M) = U(M) + v(M), M \in D,$$

where U is the potential of a positive mass on D and v is harmonic on D.[7]

[7] F. Riesz, "Sur des fonctions superharmoniques et leur rappaport à la theorie du potential," *Acta Math,* 48 (1926):329-43; 54 (1930):321-60.

Connection with Sobolev Spaces. In addition, Evans proved the following important theorems: Suppose U is superharmonic on a domain Ω. Then U (x,y,z) is absolutely continuous in each variable for almost all pairs of values of the other two and retains this property under one-to-one changes in variables of class C^1.[8]

Finally Professor Evans proved the following theorems: Suppose U is superharmonic on some domain and $U_\rho(M)$ denotes the average of U over the surface ∂B (M,ρ); then $U_\rho(M)$ is continuously differentiable over any domain $\Omega\rho_0$ (which consists of all M such that B $(M, \rho_0) \subset \Omega$) and □$U_0$® Ñ$U$ in L_2 on any such domain. A necessary and sufficient condition for a potential U of f (e) to have a finite Dirichlet integral is that ∫ U (M) df (e) exist. In this case U must belong to the Sobolev space H¦ on interior domains. Evans proved many more similar theorems.

A Sequence of Potentials (A Sweeping Out Process). Evans gave a simple proof that the limit of a non-decreasing bounded sequence of potential functions of positive mass each distributed on a fixed bounded closed set F is itself a potential of positive mass F. The limit of a non-increasing sequence of such functions, however, is not necessarily superharmonic (since the limit of a non-increasing sequence of lower-semicontinuous functions is not necessarily lower-semicontinuous).

Nevertheless, Evans showed how to associate a particular type of positive mass distribution with a particular type of non-increasing sequence of potential functions on a bounded, closed set F. To do this, Evans let U_1, U_2, . . . , be a non-increasing sequence of potentials of positive mass distributions f_1,f_2, . . ., respectively on F. Let U_0 be the limit

[8] See bibliography entries of 1935 for the three-dimensional case and those in 1920 for the two-dimensional case.

function. Clearly $U_0(M) \geq 0$ but is not necessarily superharmonic, although it is harmonic on T where T is the infinite domain lying in the complement of F, whose boundary $t \subset F$. The f_i are uniformly bounded. Hence there is a subsequence $\{i_n\}$ such that $\{f_{i_n}\}$ converges weakly to a positive mass function f on F (or a subset of F) Thus,

$$\lim_{n \to \infty} \int_W \phi(M)\, df_{i_n}(e_\nu) = \int_W \phi(M)\, df(e_\nu) \quad (W = 3 \text{ space})$$

for every bounded continuous function φ. Also, f is independent of the subsequence. Let U be the potential of f, then $f(e) = 0$ for all Borel sets $e \subset T$. Thus we may associate the positive mass function f with the non-increasing sequence $U_1, U_2, \ldots$, all the mass having been swept out of T. Since $f(e) = 0$ for all $e \subset T$, U must satisfy the Laplace's equation on T.

Professor Evans discovered a great variety of similar sweeping out processes.[9] He applied this type of process to sweep out a unit mass at a point Q in a domain T containing Q. This led to a number of interesting results and to a formula for the Green's function for T with pole at Q.

Capacity. The notion of the capacity of a set arises in the applications and was used by Evans and was developed at some length in the second part of his paper "Potentials of Positive Mass." [10] Evans also defined the idea of a regular boundary point. It turns out that a boundary point Q of a domain Σ is regular if, and only if, a barrier $V(M,Q)$ can be constructed at Q. Such a function $V(M,Q)$ is continuous and superharmonic in Σ, which approaches Σ at Q, and has a positive lower bound in Σ outside any sphere with center at

[9] See *Transactions of the American Mathematical Society,* 38 (1935):205-13.

Q. If every boundary point is regular then the Dirichlet problem is solvable.

Multiple Valued Harmonic Functions in Space. In 1896 [11] Sommerfelt developed a method of using multivalued harmonic functions in three space to solve certain problems in potential theory, particularly the diffraction problem for a straight line. In 1900 [12] Hobson used a combination of double-valued harmonic functions to obtain the conductor potential for a circular disc.

Evans showed that if s is a simple closed curve of 0 capacity (any curve with a continuously turning tangent has capacity 0), there exists a unique surface S bounded by s that has a minimum capacity among all such surfaces. If the part of S outside a neighborhood of s is composed of a finite number of sufficiently smooth pieces and $V(M)$ is the conductor potential for S, then Evans showed that V must satisfy

$$\frac{\partial V(Q)}{\partial n^+} = \frac{\partial V(Q)}{\partial n^-}, \qquad Q \subset S$$

where n^+ and n^- are the normals to S at Q. Moreover, if this holds on a smooth part of S, then S is analytic on that part. The proof of this involves "double valued" functions, the tract by Evans, "Lectures on Multiple-Valued Harmonic Functions in Space" (see bibliography, 1951), presents an extensive systematic development of a part of the theory of such functions.

A simple example of a multiple space of a type used by Evans is the double space H, which consists of all ordered

[10] *Ibid.*, 218-26.

[11] Mathematische Theorie der Diffraction, *Math Annalen,* 47(1896):317-74 and Über verzweigte Potential wie Rauma, *Proceedings of the London Mathematical Society,* 28 (1897):395-429.

[12] E. W. Hobson, "On Green's Function for a Circular Disc," *Cambridge Philosophical Transactions* 18(1900):277-91.

pairs (M,m) where $m = 0$ or $+1$ or -1, and

if $m = -1$, then $M \in R_3 - s$; if m = +1, then $M \in R_3 - s$;

if $m = 0$, then $M = s$, $s = \{(x,y,z): x^2 + y^2 = 1, \text{ and } z = 0\}$.

Geometrically, we may think of H as consisting of two infinite, flat, rigid, 3-dimensional sheets of the form R_3 - s joined together along *s*.

Such a space is a three-dimensional analog of a Riemann surface in the complex plane, and Evans' result led him into his extensive research on multiple-valued functions, his principal interest during his later years.

Since any multiple space is, by definition, a topological space, open, closed, and connected subsets of such spaces are defined and the usual theorems hold. Also harmonic, superharmonic, and subharmonic functions can be defined on domains $T \subset$ multiple spaces. Much of the existence and uniqueness theory for harmonic and potential functions is carried over by Evans to the case of multiple-valued functions, that is, single valued functions defined on multiple spaces. For example, Evans showed that there is a unique harmonic function that takes on given continuous boundary values at all regular points. Moreover the definitions and theorems about barriers carry over.

But there are many new results for infinite domains (on multiple spaces). For example, Evans proved that there is a unique function $\lambda(M)$ bounded and harmonic in T_1 □ T_2 □. . . □ T_n that takes on the values 1 at infinity on the leaf T_1 and approaches 0 at infinity on the other leaves. ($T_1 = T$ □ H_i where H_i is the *i*-th leaf of *H*.). Let T be a bounded domain $\subset H$, a particular space, and let A be a fixed point in T. Evans showed that there exists a unique Green's function with pole at A that has the following properties:

As a function of *M:*

(i) $\gamma(A,M)$ is harmonic in T except at A

(ii) $\gamma(A,M)$ is bounded except in a neighborhood of A and $\gamma(A,M) - 1/AM$ remains bounded near A

(iii) $\gamma(A,M)$ vanishes at all regular points of the boundary t of T

In addition, $\gamma(B,A) = \gamma(A,B)$ for A and B where A and $B \in T$. There is a unique function K (A,T) with the same properties, with A and M on $A = T_1$ $\square \ldots \square$ T_n. Also, $K(A,M) \to 0$ as $M \to \infty$ on any leaf. Finally Evans proved that there is a unique surface of minimum capacity that spans a given space curve s or a set of space curves $\{s\}$. It is the locus of the equation $\lambda(M) = 1/2$ where H is taken as a two-leaved space and the s_i are chosen as branch curves in H.

Finally, a version of Green's theorem holds for domains T on multiple spaces whose boundary consists of several branch curves $s_1, \ldots, s_r$ of zero capacity and several smooth surfaces.[13]

Mathematical Economics

Evans' work in mathematical economics was that of a pioneer. At a time when most economists in this country disdained to consider mathematical treatments of economic questions, he boldly formulated several mathematical models of the total economy in terms of a few variables and drew conclusions about these variables. Some of these expositions were based on the theories of Cournot (1837) and some are found in the book *Mathematical Introduction to Economics* by Evans.

The simplest theory is the following: It is envisaged that there is only one commodity being manufactured by one producer, and one consumer. The cost of manufacturing and marketing u units of the commodity per unit time is $q(u)$ this

[13] For a full discussion, see Griffith Conrad Evans, "Multiply Valued Harmonic Functions. Green's Theorem," *Proceedings of the National Academy of Sciences of the United States of America,* 33(1947):270-75.

is called the cost function. The consumer will buy y units of the commodity (per unit time) if the price is p per unit; thus $y = \varphi(p)$ is the demand function. The market is in equilibrium *if* $y = u$, that is, if all the commodity is sold. Clearly the profit π made by the producer is given by

$$\pi = pu - q(u) = p\phi(p) - q[\phi(p)].$$

The producer is a monopolist if he can sell all he produces at any given price. In this case it would be reasonable to assume that the producer would set the price to maximize his profits. This leads to the equation:

$$\frac{d}{dp}\{p\,\phi(p) - q\,[\phi(p)]\} = \frac{d\pi}{dp} = 0.$$

In order to get a solution, we must know the functions $q(u)$ and φ (p). The simplified form for $q(u)$ is $Au^2 + Bu + C$. C represents the overhead and should be > 0. The average cost per unit is

$$\frac{q(u)}{u} = Au + B + \frac{c}{u},$$

which may reasonably increase ultimately, so that $A > 0$. The "marginal unit cost" is

$$\frac{dq}{du} = 2Au + B.$$

If dq/du is ≥ 0 for $u \geq 0$, we must have $B \geq 0$; we may as well assume $B >$ O. Clearly $\varphi(p)$ is *decreasing* and positive; the simplest form for $\varphi(p)$ is $ap + b$ where $a < 0$ and $b > 0$. If the market is in equilibrium, we have

$$y = u = ap + b \qquad p = \frac{u - b}{a} \quad \text{or}$$

$$\pi = u \cdot \frac{u - b}{a} - Au^2 - Bu - C.$$

In order to maximize π, one must have $d\pi/du = 0$. This yields

$$\frac{2u}{a} - \frac{b}{a} - 2Au - B = 0$$

$$u = \frac{b + ba}{2 - 2Aa}, \qquad p = \frac{Ba + 2Aab - b}{2a(1 - Aa)}.$$

Of course a monopolist may choose u (or p) to satisfy some other condition.

As a second theory, Evans assumes that there are two producers manufacturing amounts u_1 and u_2 of the commodity (per unit time). Let us assume that the producers are subject to the same cost function $q(u_i) = A_i^2, + Bu_i + C$ and there is produced only what is sold; that is, the market is in equilibrium. If we assume the same demand function,

$$(1) \qquad y = u_1 + u_2 = ap + b,$$

then the selling values are pu_i and the profits are

$$\pi_i = pu_i - (Au_i^2 + Bu_i + C), \qquad i = 1,2.$$

Additional hypotheses are needed to find p and then u_i. Suppose each producer tries to determine u_i so as to maximize the total profit, still assuming equilibrium. In this case, we say that the producers are cooperating. Then the total profit $\pi = \pi_1 + \pi_2$ is

$$\pi = p(u_1 + u_2) - A(u_1^2 + u_2^2) - B(u_1 + u_2) - 2C,$$

$$\pi = \frac{u_1 + u_2 - b}{a}(u_1 + u_2) - A(u_1^2 + u_2^2) - B(u_1 + u_2) - 2C$$

using (1) to determine p. Assume u_1 and u_2 are chosen to maximize π. *Then we must have*

$$\frac{\partial \pi}{\partial u_1} = \frac{\partial \pi}{\partial u_2} = 0$$

or

$$\frac{2(u_1 + u_2) - b}{a} - 2Au_1 - B = 0,$$

$$\frac{2(u_1 + u_2) - b}{a} - 2Au_2 - B = 0,$$

which determine u_1 and u_2 uniquely.

As a third theory, let us suppose that a producer is subject to the same cost and demand functions but has no control over the price. Then he will choose u to maximize

$$\pi = pu - Au^2 - Bu - C$$

for the given price; this yields $d\pi / du = p - 2Au - B$ to determine

$$u = \frac{p - B}{2A}.$$

This theory can be generalized to the case where there are n producers, each subject to a different cost and demand function, but who all set the same price p. Then the i-th producer produces u_i units where the total profit is

$$\pi = \sum_{i=1}^{n} \pi_i = \sum_{i=1}^{n} (pu_i - A_i u_i^2 - B_i u_i - C_i).$$

This will be a maximum if

$$\frac{\partial \pi}{\partial u_i} = 0 \quad \text{or} \quad p - 2A_i u_i - B_i u_i, \qquad i = 1, \ldots, n,$$

which yields

$$u_i = \frac{p - B_i}{2A_i}, \qquad i = 1, \ldots, n.$$

Similar problems can be solved in some cases where the u_i and p depend on time. Evans solved such a problem in which there is one producer with

$$q = Au^2 + Bu + C, \quad y = ap + b + hp', \quad A > 0, \quad B > 0, \quad C > 0$$

$$a < 0, b > 0, h > 0, p' = dp/dt, \text{ and } A, B, C, a, b, h$$

are all constants.

The term hp' is suggested by the consideration that the demand is greater when the price is going up than when the price is going down, other things being equal. This problem required more sophisticated mathematics. It is still assumed that $u = y$ for all t and that the rate of profit is $\pi = pu - q(u)$ and finally that the profit made during the interval (t_0, t_1) is

$$\pi = \int_{t_0}^{t_1} \pi(p, p')\, dt$$

is a maximum over any interval (t_0, t_1). This leads to the condition that the integral

$$(2) \quad \int_{t_0}^{t_1} \{p(ap + b + hp') - A(ap + b + hp')^2 - B(ap + b + hp') - C\}\, dt$$

is a maximum of any interval, where $\pi(p, p')$ is given by the integrand in (2). This is a standard problem in the calculus of variations. [14] From that theory we conclude that the Euler equation

$$(3) \qquad \frac{d}{dt}(\pi_{p'}) = \pi_p$$

[14] See any book on the calculus of variations, for example, G. A. Bliss, *Calculus of Variations* (Washington, D.C.: Mathematical Association of America, 1925).

holds. Carrying out the differentiation with respect to t in (3), we get for Euler's equation

$$(4) \qquad \pi_{p'p'} \frac{d^2p}{dt^2} + \pi_{p'p} \frac{dp}{dt} = \pi_p,$$

where

$$(5) \qquad \pi_{p'p'} \quad \text{means} \quad \frac{\partial^2 \pi}{(\partial p')^2}, \ \pi_{p'p} \quad \text{means} \quad \frac{\partial^2 \pi}{\partial p' \partial p}, \pi_p$$

$$\text{means} \quad \frac{\partial \pi}{\partial p}, \quad \text{etc.}$$

$\pi(p,p') = p\,(ap + b + hp') - A\,(ap + b + hp')^2 - B\,(ap + b + hp') - C$ and the derivatives in (5) are the indicated partial derivatives regarding p and p' as independent variables. Carrying out the differentiations in (5), we get

$$(6) \qquad \pi_{p'p'} = -2Ah^2, \ \pi_{p'p} = h(1 - 2aA),$$

$$\pi_p = a(2p - B) + hp'(1 - 2aA) + b(1 - 2aA) - 3a^2Ap.$$

Setting $dp/dt = p'$ in (4) and using (5) and (6), Euler's equation becomes

$$-2Ah^2p'' + h(1 - 2aA)p' = a(2p - B) + hp'(1 - 2aA)$$

$$+ b(1 - 2aA) - 2a^2Ap$$

$$<=> -2Ah^2p'' = 2a(1 - aA)p + b(1 - 2aA) - aB$$

$$<=> p'' = \frac{2a(1 - aA)}{-2Ah^2}p + \frac{b(1 - 2aA) - aB}{-2Ah^2}, \text{ i.e., } p''$$

$$= M^2p - N^2,$$

which is reduced to the form

$$\frac{d^2p}{dt^2} = f(p).$$

This is solvable by standard methods in differential equations.

This is one of the simplest cases. More sophisticated theories involving such things as taxes, tariffs, rent, rates of change, transfer of credit, the theory of interest, utility, theories of production, and problems in economic dynamics were worked out by Evans.

Evans' scientific career resulted in over seventy substantial published articles, four books, and several classified reports. It should be added, since it is such a rare occurrence among mathematicians, that he continued his productive work for many years after his retirement. He gave a number of invited addresses in Italy and elsewhere during that period.

Professor Evans was elected to the National Academy of Sciences in 1933 and became a member of the American Academy of Arts and Sciences, the American Philosophical Society, the American Mathematical Society (vice president, 1924-26; president, 1938-40); the Mathematical Association of America (vice president, 1934), and the American Association for the Advancement of Science. He was a fellow of the Econometric Society.

Evans was invited to give addresses in connection with the Harvard Tercentenary and the Princeton Bicentennial Celebration. He was also asked to give the Roosevelt Lecture at Harvard in 1949 and was Faculty Research Lecturer in Berkeley in 1950 and was awarded an honorary degree by the University in 1956. The Griffith C. Evans Hall on the Berkeley campus was dedicated in 1971.

During World War I, Evans served as a captain in the Signal Corps of the U.S. Army. During World War II, he was a member of the Executive Board of the Applied Mathematics Panel and was part-time technical consultant, Ordnance, with the War Department. He received the Distinguished Assistance Award from the War Department in 1946 and received a Presidential Certificate of Merit in 1948.

The charming hospitality of the Evanses is remembered with pleasure by those fortunate enough to have been guests at their home. And Evans' own keen, dry sense of humor was much appreciated by his many friends and associates.

Professor Evans married Isabel Mary John in 1917. They had three children, Griffith C. Evans, Jr., George William Evans, and Robert John Evans and many grandchildren.

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Potograph courtesy of Stanford University

Rudolf Kompfner

May 16, 1909-December 3, 1977

by J. R. Pierce

The successful pursuit of science and technology is something of a mystery. The way of endeavor is conspicuously marked by sterile studies and lucky flukes. Yet, I believe there are ways conducive to winning. These are illustrated in the work of Rudolf Kompfner. In writing about him, I hope that it will not be taken amiss if I refer to him consistently as Rudi. Few knew him as Kompfner or as Dr. Kompfner, and none as Rudolf.

Rudi's success in a field that he himself chose is indubitable; it is attested by numerous honors. In 1955 the Physical Society awarded him its Duddell Medal for his invention of the traveling-wave tube. This led him to give a lecture and later to write a book on *The Invention of the Traveling-Wave Tube.* He was made a fellow of the Institute of Electrical and Electronics Engineers and given its David Sarnoff Award in 1960 and its highest award, the Medal of Honor, in 1973. He received the Stuart Ballantine Medal of the Franklin Institute in 1960; the John Scott Award from the City of Philadelphia in 1974; the Sylvanus Thompson Medal of the Röntgen Society, incorporated with the British Institute of Radiology, in 1974; and the National Medal of Science in 1975. He was awarded an honorary doctor of technical science by the Technische Hochschule of Vienna in 1964 and the honorary de gree of doctor of science from Oxford University in 1969. He was a fellow of the American Association for the Advancement of Science and a member of the National Academy of Engineering and the National Academy of Sciences. He served well on important committees of these organizations. He also served as a member of the Board of Trustees of Associated Universities, Inc.

Rudi succeeded—despite very real difficulties—through hard work and a constellation of qualities so various that they might be thought inconsistent. He had a driving purpose and intense application in his chosen field. Yet this did not exclude a wide range of interests and enthusiasms. He loved all good things except poetry, yet he could and did live simply. Nothing daunted him, and few things seemed beyond his range.

He was warm and open and quickly became what seemed like a lifelong friend. Felix Bloch regarded Rudi as a close friend, though they met only two years before Rudi's death, when neither was young. People were attracted to Rudi, and Rudi was attracted to those whom he felt worthy of his interest. Others, he must have ignored. I remember a lunch with Rudi and a foreign visitor. I solicited the visitor's opinions, listened intently, and commented politely. After lunch, Rudi seemed almost annoyed with me. He asked why I had bothered with such a man—he was nothing. And of course, Rudi was right.

To discuss Rudi and his career and its significance is no easy matter. He lived in many places, did many things, and interacted with many people. He was born in Vienna, Austria on May 16, 1909, the son of Bernhardt and Paula Kömpfner. His father was an accountant and a composer of Viennese songs and waltzes who played the piano in a Heurigen in the outskirts of Vienna. Rudi had a book, published in 1913, that included several of his father's compositions.

Rudi himself exhibited an early musical talent, picking out tunes on the keyboard and learning which notes went pleasingly together by trial with, apparently, little error. Piano lessons failed to teach him to read music; he memorized instantly the pieces he was told to study.

Rudi seems to have learned to cope with difficulties of life as he learned music—through exposure and talent. Toward the end of World War I, through the armistice, and for some time thereafter, Viennese children starved because of a total Allied blockade. Rudi survived because he was put on a train by the Red Cross and sent—without his parents knowing exactly where he was going—to Sweden. The months there, during which he recovered from boils and other ills of malnutrition, were spent with a deeply religious family. Their attitudes impressed him and remained fresh in his memory, though they had little long-term influence on his own beliefs.

Early reading, and particularly the works of Arago, enamored Rudi of physics. This was his lifelong love, but he was not allowed to pursue his chosen career immediately or directly. Through the influence of his uncle, Fritz Keller, an architect, he studied architecture at the Technische Hochschule in Vienna, becoming a Diplom-Ingenier in 1933.

This was a difficult time for Jews in Austria. An Englishman, Roy Franey, who had married Rudi's cousin Mowgli Jonasz, was helpful in Rudi's coming to England in 1934. Franey later succeeded in getting Rudi's parents and sister out of Austria in 1938. After Rudi had served an architectural apprenticeship with P. D. Hepworth in London from 1934 to 1936, he became managing director of Franey's firm, Almond Franey and Son, Ltd., Estate Managers and Builders, London, from 1936 to 1941.

Here we have a man who had suffered starvation during a terrible war, had been shipped off to one foreign land as a child, had been compelled to pursue a career not of his own choosing, and then had to go to another strange land and make his way as an architect. We might imagine him as disillusioned, bitter, slighting his own work or forever cut off from that which he valued most. Not Rudi.

Indeed, I believe that he learned a good deal from architecture that was valuable in his later endeavors. One thing was an appreciation of the practical aspects of any art, including that of the builder. Another was that in order to accomplish something, one must make a start.

Rudi told of staring at a blank piece of paper on his drawing board after having been instructed to design a house. A senior draftsman came, leaned over his shoulder, and saw that he was having trouble. The draftsman drew a square on the paper and told him, "The secret of getting started is to start." Rudi had his start and proceeded with the design.

Rudi became an architect of some accomplishment. According to Rudi's recollection, during a civil service examination, C. P. Snow walked in, glanced through Rudi's dossier, and said:

> "Mr. Kompfner, I see you are an Austrian and an architect."
> Rudi agreed.
> "Mr. Kompfner, Adolf Hitler was an Austrian and an architect. Tell me which is the better architect, you or Hitler?"
> "What I built still stands," Rudi replied.

Indeed it does. Among his works is a house in south London, described and pictured in *Small Houses, £500-£2500* (edited by H. Myles Wright, London: The Architectural Press, 1937). It is an admirable building for a narrow (30-foot wide), dark site. Rudi also designed a number of artisan's flats in the Bermondsey District.

Rudi's experience in architecture had various influences in his life. He was acutely aware of buildings, their beauties and their failings. I remember his telling me that of two buildings neighboring St. Mark's in Venice: one is marvelous; the other, trash. He insisted that the south facade of the Parliament building in Vienna is a masterpiece—far superior to the front. He speculated that this was the work of some junior architect and had escaped attention and ruination. Of lesser note, driving past a house in Summit, New Jersey, that we both admired, he said that the second-story windows were too close to the roof, which was certainly so.

Another influence of architecture was that it enhanced Rudi's natural talent for drawing. In his Viennese days, Rudi produced some striking prints in a then-current style. Later, he provided twenty-three illustrations for a book, *3 Jungen Ziehen durch Kleinasien,* in Verlag *Das Bergland-Buch,* published in 1936 and written by Rudi's closest and lifelong Viennese friend, Theo Eder.

Rudi's talent for illustration was a joy throughout his life; he recorded home, family, and a one-time pet raccoon. It served a different purpose in illustrating his technical thoughts clearly. At the blackboard, most of us fumble in trying to convey our ideas; Rudi was never unclear, never at a loss. He insisted that his students picture things accurately. One student told me that he continually demanded, "draw it to scale." That can make a real difference.

While Rudi was practicing architecture and learning through it, he began to make his way in his chosen field of physics.

How does one turn from a practicing architect to a physicist? Rudi's approach was phenomenally original. He went to the excellent Patent Office Library in Chancery Lane and read journals and books in the evening. Sometimes Peggy Mason accompanied him. He had met her at the Westminster swimming club in 1935; they were married at Caxton Hall, Westminster on April 29, 1939.

Beginning in 1935, Rudi recorded in a series of notebooks those things that interested him most. These included the television camera tubes of Zworykin and Farnsworth, and later the microwave tubes of Heil and Varian.

Soon, original ideas and inventions appeared in the notebooks. Among these is the "Relayoscope," in which a pattern of light (an image) impinging on a nonconducting photoelectric grid was to be used to control the flow of an electron beam. This led to a triple-barreled British patent, number 476,31 1, applied for on June 4, July 27, and August 18, 1936 and accepted on December 6, 1937. The patent covers the functions of a television pickup tube and the reproduction of infrared, ultraviolet, or X-ray images as light images. Rudi tried vainly to market this invention.

As his reading progressed, Rudi's interests definitely turned toward microwave tubes. He had original ideas for explaining the Heil tube and the klystron.

What course Rudi's activities might have taken but for the coming of World War II in September of 1939 is an unanswerable question. In June of 1940 Peggy returned from work one evening to find that he had been taken to the Brixton Police Station for internment as an enemy alien. He was interned on the Isle of Man from June to December 1940. In some way, through the intervention of Peggy, relatives, friends, and people who knew of his work, including Hugh Pocock, the editor of the *Wireless Engineer,* and by declaring himself a stateless person, Rudi was released.

During internment, Rudi never lost his balance of judgment and was always considerate and charming. He met a number of German internees, with whom he studied and discussed physics. He shared quarters with Wolfgang Fuchs, a mathematician from Cambridge who is now a professor at Cornell. They became close and affectionate friends as well as collaborators in science. Together they wrote a paper on space charge effects in velocity modulated electron beams. This was eventually published in the *Proceedings of the Physical Society*. W. E. Benham, a somewhat eccentric expert on vacuum tubes with whom Rudi began to correspond on July 9, 1941, was helpful in this connection.

Prior to his internment, Rudi had submitted a paper on magnetrons to the *Wireless Engineer*. Hugh Pocock had taken this to the Admiralty, and it was deemed unpublishable in wartime. The Admiralty wrote to Peggy, asking for details of Rudi's qualifications. She went to the Admiralty in late 1940 to see if they might have use for Rudi. Frederick Brundrett said that Rudi should see them when he was released.

Rudi was released, and, though Brundrett said that he was "neither fish nor fowl" to them, in September of 1941 he was sent to the Physics Department of Birmingham University to work under Professor M. L. Oliphant. The work he found there on high-power magnetrons, the heart of wartime radar, was a revelation.

Characteristically, however, Rudi soon turned his own endeavors in a novel direction. The fruitful outcome was the invention of the traveling-wave tube, while trying to make a better klystron amplifier for radar receivers. His fundamental idea—the continuous interaction of an electron stream and an electromagnetic wave of the same velocity traveling along a helix—was ingenious, and the realization worked!

The invention of the traveling-wave tube is characteristic of Rudi's career in several ways. The big thing of the day was the magnetron. Rudi had already written a paper on the magnetron. But at Birmingham, the heart of magnetron research, he turned his attention to something else, and, almost singlehanded, he succeeded.

He succeeded in the face of contrary advice. "Experts" told him that a coiled wire, or helix, would not transmit microwaves. Rudi didn't believe them. He wound a helix himself and made measurements. He didn't just think and argue, he did something with his own hands, and it succeeded.

Then he built a traveling-wave tube. This was important, because what he found went beyond what he had expected. Initially, he had thought only of a strong action of the electric field of the electromagnetic wave on electrons traveling at the same velocity. He found that as the current of electrons was increased, the tube broke into microwave oscillation. Not only did the electromagnetic wave act to bunch the electrons, the bunched electron beam acted to strengthen the electromagnetic wave. The helix and beam together constituted an amplifier that gave a very high gain over a very broad band of frequencies.

Rudi analyzed this phenomenon by means of a series calculation: the effect of the field on the beam, the effect of the beam on the field, and so on, back and forth. He thus explained the "Kompfner dip"—a reduction of transmission at a particular electron velocity or accelerating voltage. Joseph Hatton, a young research student who had begun to work with Rudi, pushed the analysis further.

When I read of Kompfner's work through CVD (Committee on Valve Development) reports, I was astonished. I quickly worked out a wave analysis that explained the behavior of traveling-wave tubes more to my satisfaction, and, I believe, to Rudi's. I had been considering the effect of traveling waves on electron beams. But because I didn't think of that wonderfully simple circuit, the helix, and because I only calculated and didn't build anything, I missed the most important point—the mutual interaction between the electromagnetic wave and electrons that results in a very great amplification.

In 1944 Rudi, deep in work on his traveling-wave tube, was transferred, still as an employee of the Admiralty, to the Clarendon Laboratory at Oxford. While there, he met Neville Robinson, who was working at the Services Electronic Research Laboratory at Baldock. It was characteristic that when Robinson, pursuing some ideas of his own that were only vaguely related to Rudi's interest, modified the design of the helix in order to make a narrow-band amplifier, Rudi realized that while it might not work as Robinson intended, it might possibly work as a low-noise amplifier. It did.

Beyond his work on traveling-wave tubes, Rudi became haunted with the idea of a voltage-tunable traveling-wave oscillator. His interest persisted throughout the period during which he studied for his D.Phil. in physics, which he obtained in 1951. He made some theoretical and experimental progress toward this end, partly in collaboration with Neville Robinson.

In 1950 Rudi left the Admiralty and became associated with the Atomic Energy Research Establishment, but he continued to work at the Clarendon Laboratory on microwave tubes. I had hoped that Rudi might come to work at Bell Laboratories, and we had approached him shortly after the close of World War II. At that time he applied for a visa, which was long in coming. It was granted in 1951, and he came to the Bell Laboratories at Murray Hill, New Jersey on December 27, 1951. There he found the facilities necessary to continue his work on tunable traveling-wave oscillators, and in a short time he had demonstrated electronic tuning over an unprecedented range of 10,000 megahertz—a wavelength range from 6.00 to 7.50 millimeters.

Rudi's interest in microwave tubes extended over many years, and his contributions were various, including the use of coupled helices, novel means of focusing (Slalom focus ing), understanding of noise, and the effects of nonreciprocal loss. Eventually, he assumed greater responsibilities, becoming director of electronics research in 1955, director of electronics and radio research in 1957, and associate executive director, research, Communication Sciences Division in 1962.

In 1958 Rudi and I became interested in communication satellites. He was full of enthusiasm in pushing and augmenting an idea that had originally been mine. We published a paper in 1959 outlining the potentialities of such satellites. Rudi brushed up his spherical trigonometry, a subject of which I was utterly innocent, in order to calculate earth coverage areas. We traveled here and there, trying to get someone to *do* something. Finally, NASA did. The Bell Laboratories work on the Echo satellite, which was launched on August 12, 1960, was carried out in Rudi's department and under his inspiration and direction. He was also deeply involved in the Telstar experiment—the launching by AT&T in 1962 of a satellite that carried live television across the Atlantic for the first time.

But Rudi's influence at Bell Laboratories was wide and pervasive. He loved to hear and talk about novel things. He proposed new ideas without any concern for personal credit. Drafts of technical memoranda were typed on pink paper in those days. Nothing delighted Rudi more than to send out pink drafts for comment in order to stir up the reader. He said that pink was his favorite color. When what he proposed proved wrong, he was not afraid to change his mind.

The 20-foot horn-reflector antenna built to receive signals from the Echo satellite was equipped with a low-noise ruby maser amplifier built for Rudi by Derek Scovil. Charles Townes had invented the first maser in 1953, and one of Townes's students and coworkers, Jim Gordon, was already at work at Bell Laboratories. Early in 1960, when the horn reflector antenna was partially completed, Rudi invited

Townes and his students out to see it. Among the students was Arno Penzias, who was doing a thesis on radio astronomy. Penzias saw the potential of the antenna for work in this field, and Rudi was fascinated. In 1961 Penzias came to work at Holmdel, and two years later Robert Wilson, a radio astronomer from Caltech, joined him. In using the horn-reflector antenna and the ruby maser amplifier they discovered a sky background noise temperature of about 3K. This proved to be most significant; it demonstrated the existence of black body radiation left over from the big bang of creation, and in 1978 Penzias and Wilson shared the Nobel Prize for physics.

While he was interested in masers in connection with the Echo satellite, Rudi heard a talk at MIT on the superconductivity of niobium at comparatively high temperatures. This led him to think of a superconducting magnetic shield for masers. On his return to Bell Laboratories, he consulted Ted Geballe and Bernd Matthias, who were interested in these superconducting phenomena. He found that they had been unable to get the metallurgists to make the required alloys. Rudi excited the interest of Earle Schumacher and Morris Tanenbaum of the metallurgy group. This intervention led to a great deal of fruitful work, including the discovery by J. E. Kunzler of the niobium-3 tin alloy that had an unprecedented property of remaining superconducting in a magnetic field of .88 kilogauss. This work led to today's remarkable superconducting magnets.

As a part of his interest in communication satellites, Rudi wondered whether the advantages of a directive antenna on a satellite could be attained with an electronically controlled array, so that the attitude of the satellite wouldn't have to be controlled. He started with the Van Atta array, which could only send a signal back in the direction from which it had come. Working with C. C. Cutler and L. C. Tillotson, the Star array was devised. This makes it possible to guide a microwave beam down by means of a signal sent from the ground.

The laser, which can produce and amplify coherent light, is an optical maser. When T. H. Maiman first used a ruby laser to produce coherent light in 1960, Rudi became convinced that light waves would play an important part in communication. A complex program was initiated, involving many people and many ideas: sequences of lenses in large tubes buried underground, combinations of mirrors in place of lenses, and gas lenses that made use of the variation of refractive index of air with temperature.

Somewhat later, Rudi played a part in shifting the whole course of work in optical communication at Bell Laboratories. Neville Robinson remembers Rudi telling him of work at the British post office laboratories on highly transparent fibers—it was typical that Rudi went around looking for ideas. Stewart Miller, a leader in Bell Laboratories optical research, remembers that a paper written by K. C. Kao came to the attention of Rudi and himself.

Kao had made measurements that suggested that fibers made of glass or quartz much purer than any then in existence might transmit light waves over long distances with very little loss. Together, Rudi and Miller wrote a memorandum that stimulated Bell Laboratories materials people to work toward such super-transparent fibers. Such fibers have indeed come into being and into use. After Rudi left Bell Laboratories, they played an important part in his work at Oxford. Advances in the field of light-wave communication have involved many people, but Rudi played a very special role in proposing, arguing, and encouraging that all those concerned valued and appreciated.

In June of 1973 Rudi retired from Bell Laboratories and divided the rest of his years between Stanford University in the winter, where he became professor of applied physics and professor emeritus in 1974, and Oxford, where he was professor of engineering and a professorial fellow of All Souls from 1973 to 1976 and an associate member of the college from 1977 onward. At Oxford he did important work on scanning optical microscopes, on the metal coating of optical wires to make them stronger and more durable, and on holographic means for interconnecting single-mode fibers. Colin Sheppard carried things on while Rudi was away, and, as everywhere, Rudi was in close and inspirational touch with many, including Don Walsh and Hanz Motz, and also with his students, J. N. Gannaway, T. Wilson, and Peter Hale. Rudi inspired others to have good ideas and never sought credit for these—or sometimes even for his own.

The work at Oxford on scanning optical microscopes, in which the specimen is examined by means of a fine laser spot, was particularly productive. The device exhibited improvements in resolution, contrast, and depth of focus and proved particularly valuable in biological studies and in studies of integrated circuits. He hoped to gain further advantages through observing harmonics of the illumination light.

At Stanford Rudi's activities were various. Long before he went there, on April 5, 1966, he had written to Peggy of a Sunday evening meeting with Cal Quate, Marvin Chodorow, and Joe Pick, and had said that "the project of an acoustical microscope is now under way." On coming to Stanford, he was delighted by the marvelous acoustic microscope work under Quate and contributed considerably in this area, particularly concerning depth of focus.

The work of three graduate students whom Rudi supervised shows something of his interest: Celia Yeack worked on a nonlinear acoustic microscope in which the information-bearing signal is a harmonic of the frequency of illumination;

Steve Newton worked on a lenseless scanning optical microscope in which the sharp definition depends on holographic effects involving light from a pinhole aperture; and Heungsup Park worked on optical picosecond radar using dye lasers—a project intended to make it possible to examine the tissue under a person's skin.

To his students, Rudi gave endless time and sage advice and counsel. He insisted that they try the simplest way thoroughly before trying more complex solutions. He counseled them rather than dominating them. He was at once their friend and their hero. His explanations of other faculty members gave the students a deeper and wider understanding of different men and different approaches.

Beyond the work of his students, Rudi took a keen interest in the work of other Stanford faculty members, including Cal Quate, Marvin Chodorow, Tony Siegman, Gordon Kino, and Steve Harris. All welcomed his profitable discussions. He approached problems from a fresh and individual point of view. He wasn't always right, but often enough his fresh approach led somewhere. And it was an extraordinary quality of Rudi that, no matter how many projects he had in hand, he always had time to discuss and criticize a new one; no one ever found him too busy to listen.

Rudi was a member of the committee for Bill Colson's doctor's oral examination on free electron lasers. This was an idea that had originated with John Madey, who had given a complicated quantum mechanical explanation. During Colson's doctoral examination, Rudi insisted that there must be a classical explanation of the operation of the device. Rudi's last seminar at Stanford was devoted to a simple classical analysis of the original free-electron laser and related devices, some of them much earlier. And, at the end of a paper on the same subject written by W. B. Colson and S. K. Ride, the authors stated: "We are particularly grateful for the friend ship and guidance offered by Peggy and Rudi Kompfner, who will remain an inspiration."[1]

Rudi invited students, faculty, and friends to his home and discussed technical matters—and other things. At a point of great success, he celebrated with the secretary and the technicians as well as the students and faculty.

For three years, Rudi conducted in his garage and home a freshman seminar on how to do research. The students and he proposed projects. One among these was chosen by vote. Analyses and experiments were then made and models were constructed. The three years' projects were: a new form of windmill, an earthquake-resistant building on rollers, and a wheelchair capable of mounting a curb.

Rudi was disappointed that in the last year a favorite proposal of his lost by one vote. That was a very small swimming pool in which one could swim long distances against a current of water, without moving at all with respect to the pool.

Rudi's versatility and originality led to a number of ingenious ideas and contraptions. When a Picturephone terminal was installed in his office, he put an excellent likeness of himself in front of the camera tube, so that those who called him found him remarkably quiet and attentive.

In his home, Rudi built a set of swinging cat doors. When he found that a raccoon got in and stole the cat's food he arranged a complicated linkage of strings, pulleys, and hooks by means of which an intruder could be excluded or trapped. Later, Rudi fed an abandoned baby raccoon, which became a pet, and built a marvelous house for it and arranged an aerial tramway to carry food to it on winter days.

For several years, Rudi devoted a great deal of time and ingenuity toward producing four-legged chairs and tables

[1] W. B. Colson and S. K. Ride, "A Laser Accelerator," *Applied Physics,* 20(1979):65.

that would rest steadily on an uneven surface. Alas, a search revealed a number of patents. Nonetheless, Rudi finally did construct a table and a chair of his own design—operable, but not objects of great beauty, and perhaps of marginal utility. A different invention, a sort of mat or coaster to make the port and madeira decanters at All Souls slide more easily on the table top, has been an unqualified success.

Rudi's analysis of the noise level in the dining room of the faculty club at Stanford was sound. He argued that diners talk loudly enough to be heard across the table against the voices of other speakers. He worked out a quantitative theory. The Stanford dining room was so noisy, he showed, because diners shouted vainly across very wide tables in an effort to make themselves heard amid the din of the futile efforts of others to converse at nearby tables.

Eventually, the tables were made narrower, the diners at the same table could hear one another, and the hubbub subsided. I have not been able to trace the change directly to Rudi's insightful work.

Rudi's life was cut short in the full exercise of his powers and in full enjoyment of his family, his friends, and his world. He must have seen something of himself and of Peggy in his children, who acquired their qualities more by good example than in any other way. He lived to see his daughter married and to wheel his son's son around Palo Alto.

As he enjoyed all good things, I am sure that Rudi must have valued the many honors that came his way, but he was a modest man. His students told me that before leaving on a trip, Rudi usually told them where he was going. When he didn't, they felt that he must be going to receive some new honor, and so it proved to be.

Rudi's modesty added to the real joy that all his friends felt for him in his successes. I am grateful to have had this opportunity to write good and true things about him that he would not have said himself. But I wish to tell in words that Rudi himself wrote the true reward that his career brought him:

> The feeling one experiences when he obtains a new and important insight, when a crucial experiment works, when an idea begins to grow and bear fruit, these mental states are indescribably beautiful and exciting. No material rewards can produce effects even distantly approaching them. Yet another benefit is that an inventor can never be bored. There is no time when I cannot think of a variety of problems, all waiting to be speculated about, perhaps tackled, perhaps solved. All one has to do is to ask questions, why? how?, and not be content with the easy, the superficial answer.

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Colin Munro MacLeod

January 28, 1909-February 11, 1972

by Walsh McDermott

As a beginner in science, Colin Munro MacLeod was granted the most wonderful of gifts, a key role in a major discovery that greatly changed the course of biology. Great as this gift was, it came not as unalloyed treasure. On the contrary, for reasons that are not wholly clear even today, the demonstration by Avery, MacLeod, and McCarty that deoxyribonucleic acid is the stuff that genes are made of was slow to receive general acceptance and has never really been saluted in appropriately formal fashion. The event was originally recorded in the now famous paper of 1944 in the *Journal of Experimental Medicine,*[1] entitled: "Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types. Induction of Transformation by a Desoxyribonucleic Acid Fraction Isolated from Pneumococcus Type III."

The title tells the story; clearly this was an historic watershed. Sir MacFarland Burnett states that "the discovery that DNA could transfer genetic information from one pneumococcus to another heralded the opening of the field of molecular biology."[2] Writing in *Nature* in the month before MacLeod died, H. V. Wyatt[3] reports it as "generally accepted" that the field of molecular biology began with the appearance of this paper. Lederberg terms the work "the most seminal discovery of twentieth-century biology."

To make an important individual contribution to one of history's great scientific achievements was an act of creation of a special sort. It took place in the decade between MacLeod's twenty-fourth and thirty-fourth years. He could have rested on this achievement; he could have continued with it, thus emphasizing his role; or he could have gone on to something else. As things worked out, he followed the last-named road, influenced to an undeterminable extent by World War II.

But there are other forms of creation in science, and, in some of these, MacLeod also excelled. Before looking at these aspects of his life, it is worthwhile to pause a moment over the question of how he had been prepared so that he might make such great contributions. (Dr. Robert Austrian, in a sensitive and perceptive piece, has described MacLeod's early years.[4]

One of eight children of the union of a schoolteacher and a Scottish Presbyterian minister, the young MacLeod skipped so many grades in school that after being accepted at McGill University he had to be "kept out" a year because he was too young. His birth on January 28, 1909 took place in Port Hastings, Nova Scotia. In his early childhood, he moved with his family back and forth across Canada from Nova Scotia to Saskatchewan to Quebec. He obviously was a splendid student, for, as related by his sister, Miss Margaret MacLeod, he skipped the third, fifth, and seventh grades and graduated from secondary school (St. Francis College, Richmond, Quebec) when only fifteen years of age. His career as an educator started almost immediately. While being "kept out" of school to become old enough for McGill, he was induced to leave an office job to serve at the age of sixteen as a substitute teacher of the sixth grade in a Richmond school. He held this job wholly on his own for the entire year. These early signs of superior intellectual capacity were not a part of the stereotype "infant prodigy." Indeed a clear sign to the contrary was the fact that within only a few years he was on the McGill varsity hockey team—then, as now, a most impressive athletic achievement.

After two years of premedical education at McGill, he entered the Medical School and received his degree in medicine in 1932. In 1934, at the age of twenty-four, after two years of residency training at the Montreal General Hospital, he came to New York. Less than ten years later, he would make his own highly important individual contribution to the Avery-MacLeod-McCarty study.

The nature of the reception of this work was to test the remaining thirty years of his life, for its significance did not receive the early attention it might be thought to have merited. Shortly before MacLeod died, this aspect of the story formed the basis of several articles in scientific and popular periodicals.[5] He had the chance to see these, but sadly enough, he did not live to see the most extensive and authoritative account, published in 1976 by R.J. Dubos in his book, *The Professor, the Institute and DNA.*[6]

There is no intent here to attempt to add to this literature. The chance of painting a distorted picture is too great for one who was not close to the situation at the time. Moreover, the endpoint of "acceptance" is hard to measure, for in science it does not occur all at once like a directed plebiscite in a totalitarian state. Some highly knowledgeable scientists perceive the full significance of a particular discovery right away; others require longer. It is necessary, however, to cite the major events in the research itself in order to describe MacLeod's clearly definable and individual contribution. And, given that contribution, some mention of what happened to the recognition of the work is inescapable in telling the story of MacLeod's career in science. For it is the way the whole story seemed to him that could have had a telling influence on his subsequent career.

When he first arrived at the Rockefeller Institute, MacLeod fell under the influence—or spell—of O T. Avery, or "Fess" as he was called, who was the inspiring teacher of so many others, including Rene Dubos, Maclyn McCarty, and the late Frank Horsfall and Martin Henry Dawson.

Some years before, as related by Dubos, an old school friend of MacLeod's, Henry Dawson, had been asked by Avery to investigate the variations in pneumococcal colonial morphology from "rough" to "smooth" (R/S) then being studied by Griffith in England. Several years later, when Griffith[7] demonstrated that one pneumococcus type could be transformed *in vivo* into another, in effect a directed and heritable alteration, Dawson was captivated by the feat. Working with R. H. P. Sia, he was able to repeat the experiment and to produce the change.[8] Dawson had to abandon the project, which was taken up by J. S. Alloway,[9] who was able to show that the substance responsible resided in a thick, syrupy preparation.

The techniques used by Dawson, Sia, and Alloway were not at all reliable. Neither the phenomenon of transformation nor the harvesting of transforming principle could be reproduced with a high degree of predictability. A phenomenon of potentially great biologic significance had been clearly identified. Yet without methods to produce it with predictability and to extract its active principle in ways permitting precise characterization, any attempts to study the matter further were bound to be marked by frustration. Nevertheless, because of the potential significance of the phenomenon, Avery decided that the work must go on. He continued to see the first essential task to be the chemical characteriza tion of the active material, but the available techniques were obviously not sufficiently reliable to permit such chemical studies. It was at this point that MacLeod entered the picture in 1935. By improving the medium and isolating a consistently reproducible rough strain of pneumococci, MacLeod made it possible (with Avery's encouragement and counsel) to move the project from what was the study of a fascinating phenomenon, but one of irregular occurrence and not possible to assay, to a predictable one. The critical substance could then be fully characterized in chemical terms. The subsequent phase of the study, the actual conduct of these chemical studies, became the responsibility of McCarty.

Each of the six investigators who worked with Avery thus made a contribution to the solution of Griffith's mystery, but it is now fully conceded that the critical contributions were those made by MacLeod and McCarty under the continuing, brilliant intellectual stimulation, advice, and counsel of Avery himself. Oddly enough, as Dubos has described, although MacLeod and McCarty worked closely together on the project, they were not officially at the Institute at the same time, for in 1941, at age thirty-two, MacLeod became chairman of the Department of Microbiology at the New York University School of Medicine. He left the Institute as McCarty arrived. As the Medical School of NYU and the Rockefeller laboratories are both in the mid–East Side of Manhattan, it was easy for MacLeod to travel back and forth, and he maintained a continued and wholly recognized association with the project. In large measure, however, whether it was realized or not at the time, he had made his contribution. He had taken an almost formless, erratic phenomenon and made it into something predictable and measurable. This had to be done, and he did it. Thus, the problem had been brought to the very stage at which McCarty's own considerable biochemical ex pertise was exactly what the situation called for. Two years later (November 1943), the paper was submitted to the *Journal of Experimental Medicine.*[10]

In subsequent years, MacLeod continued to work on this problem in his laboratory at New York University, first with M. R. Krauss[11] and R. Austrian,[12] and at a later period with E. Ottolenghi.[13] It is appropriate to postpone discussion of these subsequent phases of his scientific career in universities and government and to dwell for a moment on the story of how the finding presented by Avery and his two younger colleagues in the 1944 paper was received.

A revolutionary concept, as pointed out by Kuhn,[14] does not usually increase knowledge by adding on to it; it is more apt to replace it. A problem in 1944, and a far greater one today, is how one can evaluate new research with implied revolutionary findings when, as a practical matter, one cannot employ the techniques necessary to repeat it.

The scientists who read the 1944 paper by Avery, MacLeod, and McCarty had, in theory, two choices: they could accept or deny the validity of the demonstration on the basis of comprehension, or they could repeat the experiments. To do the former requires an intimate knowledge of the reliability of the techniques. At first glance that is a statement of the obvious—something that occurs on the reading of any scientific paper. But such is really not the case. Most of the time, in biomedicine at least, published experiments represent logical sequences in a series of experiments on the same subject. The degree of reliability of the key methods is known to be understood by those intimately engaged in the field, and the rest take it on faith. When this is not the case—when the results depend on a new method—if the field is reasonably in the scientific fashion of the day, it contains other workers. These other workers soon define the limits of the technique. Obviously, this system depends on the judg mental decisions of presumed experts, but the scientific community and the public are protected against prolonged error by the competitive nature of the studies in a particular field. It is one part of the familiar "marketplace of ideas."

The trouble with the Avery-MacLeod-McCarty studies was that the approaches they used did not happen to be fashionable. They were not part of a race to glory, such as that described by Watson in the *Double Helix*. [15] Or, more accurately, the successful approaches that were used by the Rockefeller group were far out of the ken of most of those who were working actively to solve the question. Moreover, the nucleic acids were not believed to have any biologic activity nor was their structure well defined. There really was no community of competing investigators fully armed with the requisite techniques ready to jump in and repeat the experiments. Indeed, to do this would require assembling a team with the talents, experience, and expertise of Avery, MacLeod, and McCarty. What is more, it would have to be assembled from a markedly constricted biomedical research community, for by this time the U.S. involvement in World War II had begun.

Acceptance of the chemical basis of transformation might seem to have been slow, although clearly there was no set period within which it should have occurred. There is now a small body of published material on this question of acceptance by some of the people who were close to the field at the time. Some of these comments were recorded during the period in question or a little later; others are present-day recollections of what was thought at the time. As might be expected, these reports ranged from outright acceptance of the role of DNA to a definite interest short of conviction, to, at the other extreme, a belief that the phenomenon was not mediated by nucleic acid at all, but by minute amounts of contaminating protein. Stent believed the work had little im pact on genetics.[16] Lederberg strongly dissents from this point of view and presents important contemporary citations in support of that position.[17] Indeed, in the year following the original report, J. Howard Mueller[18] appears to have correctly perceived the whole story, as may be seen in his article in the *Annual Review of Biochemistry*. Dubos,[19] in his 1976 analysis of the entire record, suggests that one of the factors in the slow acceptance was the starkly noncommittal way the results were presented, which was notable even in a scientific report. In those days at the Rockefeller Institute, there was a philosophy concerning the style in which experimental results should be presented. This style was largely initiated by Avery but was also adhered to with conviction by most of his younger associates, especially MacLeod. In this style, the key words were carefully chosen to convey only that which had been clearly proved and nothing more; any suggested implications were rigorously excluded. Lederberg also credits this attribute, which he terms "Avery's own a-theoreticism," with helping to postpone "the conceptual synthesis that now identifies 'gene' with DNA fragment."[20]

Whether or not acceptance was slow, it evolved steadily. For Lederberg also mentions: "In 1946, at the Cold Spring Harbor Symposium, where Tatum and I first reported on recombination in *Escherichia coli,* we were incessantly challenged with the possibility that this was another example of transformation, a la Griffith and Avery."[21]

Dubos cites a summary by Andre Lwoff of a 1948 conference in Paris in which the genetic role of the nucleic acids is obviously accepted. But as Dubos also states:

> It took an experiment, outside of the Institute, with a biological system completely different from that used by Avery to win universal acceptance for the genetic role of DNA. Using coliphage marked with 32P (restricted to the DNA component of the virus) and with 35S (restricted to the protein component), Hershey and Chase at the Cold Spring Harbor Laboratory

> showed in 1952 that most of the DNA penetrates the infected bacterium, whereas most of the protein remains outside. This finding suggested that DNA, and not protein, was responsible for the directed specific synthesis of bacteriophage in infected bacteria. In reality, the interpretation of this wonderful experiment was just as questionable on technical grounds as was the chemical interpretation of pneumococcal transformation, but those obtained by Avery 10 years before, that the few remaining skeptics were convinced. The case for the view that DNA is the essential and sufficient substance capable of inducing genetic transformations in bacteria was not won by a single, absolute demonstration, but by two independent lines of evidence.[22]

In his Nobel Prize lecture,[23] Lederberg puts it in essentially the same way. He attributes to Avery and his colleagues the demonstration that the interpneumococcus transference of an inherited trait was through DNA, the broadening of the evidence to Hotchkiss,[24] and the reinforcement of this conclusion to Hershey and Chase,[25] with their proof that the genetic element of a virus is also DNA. Eventually such situations right themselves. Today if one looks in elementary texts on human genetics, the Avery-MacLeod-McCarty 1944 paper is cited, in effect, as the historic watershed.[26]

Little imagination is required for anyone who has ever been engaged in science to envision what a deep-seated disappointment the relative lack of formal recognition of his key contribution to the DNA work could be to a scientist, especially to one who was just starting out in his career. A sense of having in some way suffered an injustice would not be at all unusual. This could well lead to bitterness, particularly as the years went on and others reaped wide professional and public recognition for studies on DNA. But MacLeod would have none of this. Not for him would be the stereotype of the unhappy investigator living off scientific "might have beens." Indeed, as far as I have been able to ascertain, at no time did he ever publicly express, even by indirection, the thought that, in the DNA story, he had been slighted in any way.

MacLeod's seven years in Avery's "department" at the Institute were not all occupied by the work on the pneumococcal transforming factor. On the contrary, he was engaged in a number of other studies, as may be seen from his sixteen publications of this period, eleven of which list him as senior author. Two things are striking in looking over this list today. First, although a number of different topics appear to be involved, they almost all deal with host-parasite relations at the very time antimicrobial therapy was coming on stage, so that the influence of this intervention in the disease mechanism could also be embraced by the studies. Second, virtually all were concerned with pneumonia, notably pneumococcal pneumonia; there was one study on the so-called primary atypical pneumonia[27] just then coming into medical recognition. Given Avery's preoccupation with pneumococcus, the fact that MacLeod, working in his laboratory, published a number of studies on pneumonia may not seem too surprising. What is important, however, is that this interest led MacLeod to highly productive studies in his subsequent career.

MacLeod's start as a university professor coincided roughly with the entrance of the United States into World War II. Viewed in retrospect, the impact of so pervasive a force as World War II was bound to have deep and enduring effects on a young man just emerging as a leader in science. From this time on, three characteristics were prominent. He was forever conscious that the university department he headed was in a school for the training and education of physicians, he was deeply convinced of the social value of unfettered basic scientific research, and he felt a responsibility to contribute what he could to the shaping of public policy in that interface of government and the universities that developed so rapidly in importance dating from that time. To a considerable extent, all three characteristics tended toward self-effacement, and each one influenced the expression of the others.

Some contradiction exists between the fairly sharp sense of mission of a medical school and unfettered basic research as a major goal of one of its departments. MacLeod believed this contradiction could be resolved. He accomplished this not only by developing a highly organized and constantly renovated program of medical education but also by a quiet display to his associates of his own attitude concerning the choice of subjects for research. He constantly maintained the position that any question to be studied should be studied with the most penetrating and "basic" techniques and that the investigator was obligated to go where the study led him. There should be no emphasis or pressure to come up with new knowledge for practical application. By the same token, it was to be hoped that, in a medical school, the initial choice of a broad question for study would bear a clear relationship to disease in man.

At the beginning it was not possible to start building on this concept; in a nation at war, the research needs of the military came first. To a surprising extent, however, without in any way overlooking the military need, it was possible to carry on a certain amount of free inquiry. In part this was because a great deal of MacLeod's and the department's considerable contribution to the war effort came not from quick ad hoc laboratory experiments but from their ability to use a deep background in microbiology to advise and to help solve the disease problems of the military, which could arise virtually overnight.

With these concepts in mind, starting with the nucleus of microbiologists in the department when he arrived in wartime, and more rapidly thereafter, there assembled at NYU a group that brought the department recognition as one in a rapid growth phase characteristic of a basic discipline. Not only was the DNA story unfolding, with its many ramifications, but there were also the development of antimicrobial drugs and the rapidly widening capability to deal with viruses in the laboratory. The high national and international reputation of the NYU department was not founded only on research but, as has been mentioned, a great deal of departmental time and thought went into creating a teaching program. Indeed, it was the NYU group that was among the first, if not *the* first, to introduce the actual handling of viruses to the regular laboratory exercises in the medical student's course. This was done at a time when, in many of the academic medical centers throughout the country, any manipulation with viruses was considered something only for the research laboratory. Ironically, the desire to provide a research environment free from the pressure to seek results for immediate practical application yielded certain studies that ultimately led to important practical applications.

A look at the list of departmental publications for the fifteen-year period beginning in 1941 shows an unusual degree of diversity. It must be recalled that when the United States entered World War II, microbial disease represented a far greater portion of the total health threat to young adults than is the case in peacetime today. This portion was enlarged still further by the actual process of military mobilization. As young adults from all over the country were introduced to communal and often crowded living conditions, outbreaks of microbial disease of a sort not usually seen in civilian life became not infrequent. Pneumonia in its commonly recognized forms was a major threat. MacLeod had long been a student of this disease complex, and the departmental publications list shows a 1943 paper[28] by him on the newly recognized primary atypical pneumonia, a disease of considerable importance to the military. (He published the results of a field study of this condition with Hodges in 1945.)

Of greater ultimate importance was a series of studies on antipneumococcal vaccine. This work, done in the last years of the war, was a development of public health importance that is only now coming into its own. MacLeod was a senior author of the 1945 paper "Prevention of Pneumococcal Pneumonia by Immunization with Specific Capsular Polysaccharides."[29] Mothballed at war's end, largely because of the development of penicillin, this work formed the base three decades later for the antipneumococcal vaccine developed and clinically validated by R. Austrian,[30] who had been a research fellow with MacLeod and his lifelong close friend.

The early days of World War II were a period in which the limits of effectiveness of the sulfonamides introduced some five years previously were being defined; at the same time the extraordinary characteristics of penicillin were being discovered. Among the publications from the department are two by MacLeod: one on the sulfonamides alone,[31] the other on the differences in the nature of the antibacterial action of the two substances and the relations of these differences to therapy. Viewed today, such a presentation would seem far too elementary for serious consideration in a department with a strong orientation toward basic science. Yet in the early 1940s, it dealt with important and largely unanswered questions.

During the fifteen years in which MacLeod headed the department, in addition to his own work, there were five main lines of inquiry pursued by its members. These were: the studies of hemolysins and the studies of other streptococcal products that led ultimately to streptokinase and streptodornase; the studies of diptheria toxin and toxoid, including clinical observations; and studies of metabolic effects on mouse brain produced by viruses. Any one of these programs would have been considered a great feather in the cap of a department of microbiology. Taken together, they repre sented an extraordinary contribution to our understanding of the pathogenesis of microbial disease and hence in some instances formed the base for preventives or therapy.

MacLeod took an immense interest in all of' these activities and followed their progress in considerable detail. With some, for example, the studies that eventually led to enzymatic debridement, he participated sufficiently to coauthor one of the key papers (Christensen, 1945).

There were two lines of research activity, however, in which MacLeod's participation was complete. These were the further studies on various aspects of the transforming factor and a series of studies including field trials of' the development of an antipneumococcal vaccine.

In the studies on transforming factor, having first shown a relation between the quantity of capsular polysaccharides formed *in vitro* and the virulence of pneumococcal strains for mice, MacLeod and Krauss[32] showed that the transformation of a pneumococcus from R to S was genetically controlled quantitatively as well as qualitatively. In other studies with Austrian,[33] he was able to show the presence of an M protein that could be transferred from one pneumococcal type to another through the transformation process. (In a subsequent study, Austrian with Colowick demonstrated that it was possible to modify the fermentative activities of pneumococcus by means of the transformation reactions.)

The studies of immunity to pneumococci mentioned previously were extraordinarily complete and were published in a series of eleven papers[34] from 1945 through 1947. The specific capsular polysaccharides could be obtained by the methods first developed by Heidelberger. In collaboration with him, MacLeod and the group were able to demonstrate and to define the antibody response in man. A vaccine consisting of the specific capsular polysaccharides of four pneumococcus types was made and proved effective in the preven tion of pneumonia by these types in a military school during 1944-45.

In the brief period between the experimental production of small amounts of this quadrivalent vaccine and the end of the war, the vaccine was not tested again and the increasing use of penicillin immediately thereafter dampened further interest in the vaccine. It remained for Austrian, almost thirty years later, to successfully convince the bureaucracy and industry that it would be worthwhile to develop a 14-serotype vaccine and then, in clinical trials headed by himself, demonstrate its effectiveness.[35] This is an example of the length of the shadow that can be cast by one man, for Austrian[36] received his early education in research in MacLeod's laboratory at NYU.

In 1956 MacLeod gave up the responsibility of being chairman of a teaching department but continued his university career—first as John Herr Musser Professor of Research Medicine at the University of Pennsylvania and then in 1960 back at New York University as professor of medicine. He carried on research at both places—the most important being further studies on the transforming factor (E. Ottolenghi and C. M. MacLeod) and genetic transformation and so forth, with Ottolenghi.

These years in microbiology in the university, particularly the first decade starting in 1941 at NYU, represented a form of scientific creativity different from that at Rockefeller. He was creating a science department in a medical school. It was an achievement widely scrutinized and praised on the national scene; it was also one that inevitably caused a change in the nature of his own work in science.

There was the usual temptation to be selfish and husband time for his own research at the expense of engagement in the problems of the other departmental members. This he successfully resisted, but at the quite considerable sacrifice of himself, for to the usual departmental demands were added his wartime work, very little of which could be accomplished in his own laboratory. Instead, he had to spend substantial periods either in Washington or on trips (usually by train) to military installations scattered across the country. With considerable effort, he continued to keep abreast of the research going on in the department, and he did not fail to make his own contributions to the teaching program. But he had to obtain his intellectual satisfaction in science vicariously, as a student and adviser with engagement in the work of others. As seen above, this was not the case right from the beginning, but it developed on an increasing scale throughout the war years.

Thus, in the same way that the change from Rockefeller to NYU ushered him into a different form of scientific creativity, the end of World War II marked the beginnings of the third and final form, that of a highly respected science adviser to government. From this time on he was able to do relatively little in the laboratory, although he worked hard at his other departmental duties. Indeed, one of the major attractions of the offer of the Pennsylvania chair was the prospect, erroneous as events proved, that he would have much more free time for research. It is worth noting that in each of these three phases of MacLeod's life in science he was highly successful; in retrospect, each phase was a splendid preparation for its successor.

He was now starting to perform a function of a sort not hitherto performed in our society or, more precisely, not performed on anywhere near so large a scale. To be sure, some precedents existed. President Theodore Roosevelt obtained advice from medical leaders in New York City on whether to support Walter Reed when he became engaged in controversy during the construction of the Panama Canal. The National Academy of Sciences itself had, in its 1863 charter, the responsibility for advising our government on scientific (and certain other) matters "when asked." It was during World War II, however, that the interfaces of government, university, and industrial science became such an important sphere of activity, for the exigencies of the war happened to coincide with the early years of the great burst of biomedical scientific creativity. Only in part in response to the war, biomedical research and development increased thereafter because of the expanding productivity and intellectual liveliness of the field. Among other things, they had attained a high social value. New institutional forms involving government and the universities had to be created for their proper management and support. MacLeod's wartime work thrust him into this field, and his obvious skill in it made his deep involvement almost inevitable.

Today it is easy to forget how primitive was the institutional framework (including that in the Academy) available for managing affairs in this arena of society and government. In a very real sense, MacLeod was a pioneer in an activity now dignified in increasing numbers by a formal place in the university structure as a department or program entitled Science and Society or Science and Public Policy. With his terrific energy, he was not an occasional contributor to this scene—he worked at it virtually every day.

To serve productively in this field of science policy requires a whole set of characteristics that must be possessed, in addition to the ability to do scientific work itself. For, although the findings and the future picture of the various sciences are conceived in objective terms, it is also necessary to explain things in a convincing manner to a spectrum of people with little or no background in science. It is appropriate, therefore, in discussing MacLeod's contributions to science policy and as an institution-builder, to consider him as a person reacting to others.

Continued grace under what seemed to him as nonrecognition of a major role in an important scientific or biomedical contribution tells us one part of his character, but what kind of a person was he all-in-all? As one who interacted with others, MacLeod was a full-formed individual by the time he went to New York University, although the occurrence of considerable inner growth thereafter could be sensed by his associates. It was at about that time, in late 1941, that I first met him. Then and thereafter, he showed a number of wonderful qualities, a few of which were paradoxical in a way that is hard to describe. He would give the impression at one and the same time of approaching problems with a light touch, yet of taking them with all appropriate seriousness. He radiated competence. He also gave the impression of a great depth of knowledge of his chosen sector of science, microbiology, yet he managed to do this without the slightest hint of intellectual arrogance. The "light touch" was physical as well as behavioral; he would enter a room quickly, get off some bit of quick wit as a salutation, and be ready to go. He gave the impression of being in command of himself physically as well as emotionally and intellectually.

For all of these reasons, he was the perfect chairman and usually ended up in that spot. This job requires patience, something not too difficult to employ when a group is discussing things one wishes to learn more about. Almost invariably, however, he would know much more about the question than the group over which he was presiding, and yet he would not betray that fact. If the situation called for it, MacLeod *would* suffer fools gladly—he would not cause people to lose face. Like a skilled symphony conductor, he always seemed to know just what it was his committee members *did* know; he would extract it and weave it into the fabric of a group contribution. He was absolutely unflappable and he operated in a world—particularly in the last decade of his career—in which one crisis followed another daily as a part of the regular business of life. In a very real sense, these attributes were a carry-over into adult life of that remarkable performance when he was a highly precocious child, yet had no trace of being an infant prodigy. This patience was not only a kind tolerance for the shortfalls of others; it had a "Robert Bruce and the Spider" quality about it when applied to himself. This characteristic was presumably of great value also in the laboratory when he took on the until-then hopeless task of attempting to bring orderliness into the on-and-off phenomenon of transformation.

He was known to be readily approachable by young people who were considering careers in science, and he would have long sessions with them. In addition, he had that great gift of seeming to the young as if he were taking their ideas with seriousness, and most of the time he did. Understandably, he had great influence as a teacher of medical students and research fellows. This patience and tolerance for the shortfalls of others were not those of someone too goodnatured or popularity-seeking to be a good teacher. On the contrary, he could clearly communicate his disapproval to the young people in the laboratory when their approach and work habits were not of high quality, but it was done with kindness. With his sharp intelligence, his high standards for the quality of research, and his rich background in microbiology, he was a keen critic of newly appearing work in the field. He was not, however, an instinctively negative critic. When he heard or read about some new observation, he would talk about it with a sort of wonderment—almost that sense of innocent wonderment we fantasize in the young child walking for the first time through a daisy-studded field. He would hold forth eloquently on such new developments as a member of an informal monthly dinner club formed soon after World War II.[37]

Above all there was one characteristic he had and maintained throughout that was important in developing as a role model, if you will, in his field. Despite the frequent temptation, he never gave an inch on his values in science in order to be perhaps more persuasive to prospective donors or government officials in the hope of attaining acceptance of a particular program of the moment. In a figurative sense, he refused to "sell out," despite almost innumerable opportunities to do so.

In his career in science policy, he worked through four main institutions; three were in government, the other an institution with a special relationship thereto. They were: the War Department, later the Department of Defense; the National Institutes of Health; the President's Science Advisory Committee; and the National Academy of Sciences, to which he was elected in 1955.

In the early days of the war, a major part of his work had to do with the Army Epidemiological Board, which was attached to the Office of the Surgeon General of the War Department. At the end of the war, MacLeod became president of the Board. A few years thereafter, the Board was enlarged to embrace all the armed forces, and MacLeod was made president of the combined Boards. He continued to fill this position until 1955. It should be emphasized that his position as president, although part time, was nevertheless quite demanding; indeed, it necessitated that several days each week be spent in Washington. In short, it was at least a half-time job, even in the pattern of the long hours customary in the life of a research scientist. In addition to the time required, there was a considerable intellectual challenge because the Armed Forces Epidemiological Board included some ten or twelve individual commissions on various aspects of diseases or conditions important to the army. These comprised such subjects as streptococcal disease, influenza, mili tary wounds, peacetime trauma, pneumonia, and staphylococcal disease. It was necessary for the president of the Board to steep himself in all that was known and was being developed in each of these fields. MacLeod appeared to do this effortlessly, yet at no time did he give the impression of superficiality. He seemed deeply interested in each development in each field and would talk about them with the infectious enthusiasm he had for all feats of human imagination in the works of science. When his term as president was completed, he continued in an active role as a Board member, including membership on two of its commissions, and served also as chairman of the Scientific Advisory Board of the Walter Reed Army Institute of Research.

The medical research of World War II was largely conducted in the civilian laboratories and in a few centers set up by the military. Field studies were done mostly in the military cantonments. The total effort was financed in part by the army and the navy—the only U.S. military departments then in existence. The major portion of the wartime program, however, was supported and directed by a tight partnership between the Office of Scientific Research and Development, an arm of government, and the National Research Council, an arm of the Academy. Although formally separate, the two operated as one, even to the point of having an identical membership and chairmen for their committees. Despite his heavy commitments to the Army Epidemiological Board, MacLeod also worked hard in the OSRD-NRC programs, where he was chief of the Preventive Medicine Section of the Committee on Medical Research of the OSRD. Among the activities of this unit were large-scale studies of sulfonamide prophylaxis of streptococcal disease.

At the conclusion of the war in 1945, there was a sudden need to do something about finding continued support for certain of the research activities in the university laboratories that had been supported in the National Research Council program. The National Science Foundation was just being proposed at that time, but its actual creation encountered delays. These were caused, in part at least, because the mechanisms proposed by the scientists for the appointment and removal of its director were so unrealistic as to lead President Truman to veto the authorizing legislation. Meanwhile, the U.S. Public Health Service expanded a grant-making authority it had long had and created the extramural grant program of the National Institutes of Health.[38] A number of the wartime grants were immediately taken over and approved by the newly formed study sections of NIH. MacLeod was a member of the first one, the Antibiotics Study Section (1946), and continued as a member until the Section was merged with two others three years later.

This appointment marked the beginning of his long period of association with the National Institutes of Health. By service on various committees, commissions, task forces, and training grant committees, and as a frequent informal personal consultant to successive directors, he exerted a considerable influence in helping to shape the direction and quality of what became the quite extraordinary development, the whole extramural complex of programs conducted largely in the universities.

These NIH experiences in helping to build institutions linking the government and the universities and his work for the military occurred largely at the same time; both continued throughout his lifetime. In many instances the scientific substance of what was under scrutiny was similar under both auspices, but the range of biomedical subjects in the NIH world was understandably greater. Moreover, the issues of public policy involved tended to be different in the two programs. The work for the army and for the newly formed Department of Defense, and that for the NIH and the subse quently formed Department of Health, Education, and Welfare, gave MacLeod a considerable insight into how the two largest departments of the federal government operated from the time of their formal beginnings.

With this knowledge and the opportunity to help shape the developing relationships between government and science, MacLeod entered a new role in 1961 as chairman of the Life Sciences Panel of the President's Science Advisory Committee and a year later as a member of PSAC itself. This activity was followed in 1963 with his appointment by President Kennedy as the deputy director, Office of Science and Technology (OST), Executive Office of the President.

Stemming from the nature of its location in government, PSAC tended to operate in a crisis-like atmosphere, for it had to be responsible for authoritative advice over an extremely broad range of science and technology. MacLeod was the first to hold the position of deputy director. The thought was that whoever held the post should have a background in science that would complement that of the director, who, at that time, was Jerome Wiesner. Thus it was visualized that MacLeod's principal concerns would have to do with biology and medicine. This responsibility he did fulfill, but the demands on the PSAC operation were such that he also had to cover a considerably wider range of scientific subjects than those purely biomedical. Nevertheless, he was able to make a number of achievements in the biomedical field. Among these were: the in-depth report on the status and suggested future of the life sciences; the Task Force report on medical manpower; a report on the use of pesticides; and the U.S./Japan Cooperative Program in the Medical Sciences. The last named is a case in point. The OST then (as its successor, the OSTP today) was not an operating agency; hence, its visible accomplishments frequently took the form of a program lodged in some other part of government. This is not to say the program was neces sarily implanted there full grown. More often the idea was initiated or passed through OST, whose staff then shared in greater or lesser degree in the actual creation of the program. Thus much of the work in OST, although not secret, was by its nature unheralded, or at least the role of OST was not emphasized.

For example, the U.S./Japan program was worked out through the Department of State, but MacLeod had been chosen by President Johnson to organize and direct it and was chairman of the U.S. delegation from the start until his death. This program or institution is another instance of the creation of mechanisms whereby two governments and their respective scientific communities can engage in productive scientific work. Almost fourteen years old now, the program appears to be thriving.

An appreciable portion of MacLeod's work in OST, as with the U.S./Japan Program, involved international activities. This work abroad did not begin for him at OST, for he had long been active in the international field. Indeed, early in 1956, he was one of a group of four scientists to visit the U.S.S.R. These individuals probably represented the first official biomedical group to visit the Soviet Union since the end of World War II; indeed, there had not been many unofficial visits in the entire Stalin era. Two years later, MacLeod was appointed as the U.S. representative to an international group of distinguished scientists who formed the "charter members" of the Committee on Research for the World Health Organization. A year later he became chairman of the NIH Advisory Committee responsible for some five or six International Centers for Medical Research and Training located in Asia and South America.

In the following year (1960), he became deeply engaged in the problem of Asiatic cholera. The South East Asia Treaty Organization (SEATO) wished to focus some of its effort on health matters and made a formal request to the United States for scientific advice and counsel. Dr. James Shannon, the Director of the NIH, and the late Dr. Joseph Smadel, a former associate of MacLeod's at Rockefeller, appointed a small group to examine the health situation in the SEATO region and to suggest ways in which its problems might be productively attacked.

The group recommended the creation of a facility for intensive laboratory and field research on cholera. The facility was established initially with SEATO funds. It has been funded since from a variety of sources, notably the U.S. National Institutes of Health and the Agency for International Development. Throughout its existence, MacLeod served as a leader and wise counselor for this laboratory in which much has been accomplished by an international roster of distinguished investigators. Starting in 1963, MacLeod was chairman of the Technical Committee to the Laboratory. Indeed, he was en route to the Dacca laboratory when he died in his sleep at the London Airport Hotel. The wisdom of the initial choice of cholera for the major research effort was borne out, not only by the successful development of oral hydration as a treatment for cholera but also by its potentially great usefulness in the treatment of other diarrheal diseases.[39] The laboratory has now become the International Center for the Study of Diarrheal Diseases. Today it is recognized that diarrhea, particularly of infants, it probably the world's greatest killer, and the World Health Organization has recently launched a major program for its management.

In his years as a member of the National Academy of Sciences, MacLeod served in a number of roles, of which only two will be mentioned. He was elected a member of the Academy Council in 1964 and he was appointed to the NAS Board on Medicine. The latter was a group set up in 1966 to advise the Academy on the question of what kind of an institutional framework might be created by the Academy to meet the needs of society with respect to biomedicine. The Board led to the creation of the Institute of Medicine, and MacLeod was a member of its first Council.

MacLeod left the Office of Science and Technology in 1966 to become vice president for medical affairs of the Commonwealth Fund in New York City. Leaving a full-time position in government did not mean, however, that he had given up all governmental work. On the contrary, he continued as a very active adviser. He resumed his place on the President's Science Advisory Committee and continued his chairmanship of the U.S./Japan Program. From this time until his death roughly five years later, he spent his time in foundation and university work and as president of the Oklahoma Medical Research Foundation. Although he had gone to Oklahoma less than two years before his death, he had made an impact there with his great ability to help young people facing the problems of scientific research.

His major achievements in this final period had to do with his foundation work, which was largely concerned with helping to strengthen the teaching and research capability of biomedical institutions. He was able to expand his activity in this field by virtue of his membership (and frequent chairmanship) of the Health Research Council of the City of New York. This organization was a municipal fund-granting agency that he had helped to found in 1959. For more than a decade, it had been able to award some eight million dollars yearly to the support of the science base of New York City's biomedical institutions.

His interest in medical education was constant, irrespective of the extent of his university activities of the moment. When primarily engaged in work for the government, his efforts necessarily had to do principally with education and training in science and in biomedical science. His interest, however, was in medical education in its totality; in his few years as an executive of the Commonwealth Fund, he was able to concentrate largely on this field. Among his major accomplishments was the successful effort to convince the Fund to make a substantial investment in support of the medical education of black students. Moreover, by no means opposed to the "Centers of Excellence" concept, he was nevertheless among the first to encourage the university-based medical centers to concern themselves also with the broad societal issues of medical care. Probably the most carefully written of his analytic essays on the social choices before us regarding the support of medical education and its sciences is "The Government and the University," given as the dinner address in 1966 before the Association of American Physicians.[40]

These three different phases of MacLeod's scientific and professional life were largely sequential. There was his fine work in the laboratory culminating in the sharply focused scientific effort with Avery and McCarty that led to the identification of DNA as the material of heredity. In the second phase, there was the creativity involved in building a model basic-science department in a university. He led in the creation of an exciting teaching program. He assembled a group of splendid scientists, junior and senior, and provided the leadership and the environment in which they could attain their maximal potential. There was the third and longest phase in which he pioneered in an area essential to the proper life of science: science and public policy or the interface between science in the university and in government.

The writer had the opportunity to observe him on many occasions at work in each of the institutional frameworks in which he labored for so long a period. It was easy to see why he was so much in demand. He was responsible, knowledgeable, always even-tempered, and quick to sense a group tension that could he allayed by his quick wit.

He had attributes somewhat unusual for a young person in science—at least in biological science in those days. He had a considerable interest in intellectual affairs outside of science as well as those of science, and he usually appeared willing, one might almost say eager, to stay up all or half the night in discussions about them. To these he brought a quick wit and great gifts as a raconteur—particularly as a teller of stories in Scot's dialect. Perhaps he possessed these behavior patterns, while others of his cohort in science did not, simply because lie had the physical strength others lacked. As Robert Aistrian has put it:

> One of Colin's remarkable attributes was his boundless energy. Despite the multiplicity of his responsibilities, his endless travels here and abroad, he never seemed to tire. He required less sleep than most men; and, after an animated evening of discussion with colleagues lasting into the wee hours, he could attend a meeting the next day without visible evidence of the influence of fatigue on his thinking.[41]

He had strong characteristics that in another person could have been defects. What in someone else might have been unattractive rigidity, in him was an enviable firmness and responsible consistency. While believing deeply in the social responsibility of science and in the need to work out ways to apply its useful products, he was equally deeply convinced of the importance of scientific inquiry of a completely unfettered sort. Even in his manner there were the contradictions—his small size, quick movements, and careful grooming might easily get the label of dapper—but not in him. In puzzling over why these apparent paradoxes formed an immensely effective person, one might say that the contradictions were in balance, but it was something more than that, it was really a matter of a disciplined control.

The greatest paradox of all was in personal relations. Here he gave much of himself; he had a wide circle of extremely devoted friends and was always open to their seeking of help. He gave much of himself, but he gave very little *about* himself. Several people who knew him well have commented that there seemed to be an extraordinarily large group of people, each one of whom considered themselves to have been a close personal friend of MacLeod.

Although he would not talk of himself in a personal factual sense, he would get into quite serious discussions about his philosophical beliefs. His view of life as I heard him express it on more than one occasion was based on the concept of immanence. He was fascinated with this idea. Unfortunately for a precise discussion, the concept of immanence has several rather different meanings. My own understanding from our numerous conversations is that MacLeod's immanence had the "God is everywhere" meaning. Certainly this fitted well with his unpretentious and utterly convincing wonderment about the effective intricacies and orderliness of living systems—a characteristic not so often met with in one who also was extremely interested in disease and the human condition.

Described in this way, he seems like a paragon of virtues—something I suspect he was, but cannot testify to because of the familiar phenomenon of our relative ignorance of the "other sides" of persons we know quite well. I did not know him in his roles as brother, husband, or father. I knew him as an extraordinarily capable member of the scientific community and an equally effective leader in the world of science and public policy. In short, I knew him in a certain environment, and it is the particular environment that is especially concerned in these archives. In the relatively broad confines of that environment, this is the way he was to me.

Tolstoy believed our method of classifying people by at tributing to each some particular leading quality was all wrong. He conceded that one could say that someone is more frequently kind, wise, or energetic than the opposite, but to him:

> Men are like rivers. The water is alike in all of them; but every river is narrow in some places and wide in others; here swift and there sluggish, here clear and there turbid; cold in winter and warm in summer. The same may be said of men. Every man bears within himself the germs of every human quality, displaying all in turn; and a man can often seem unlike himself— yet he still remains the same man.[42]

It is on this Tolstoyan scoreboard that the MacLeod career stands so high, for almost without exception, regardless of how wide or how cold the river, he remained the same man.

NOTES

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Samuel Marion McElvain

December 9, 1897-April 11, 1973

by Gilbert Stork

S(amuel) M(arion) McElvain was for his entire professional career a member of the chemistry department of the University of Wisconsin in Madison. That period—spanning the thirty-eight years from his appointment as instructor in 1923 (full professor from 1933) to his retirement in 1961—coincided with the explosive growth of organic chemistry. That growth resulted in no small measure from a handful of pioneers in a few universities who strove to bring some order to what was then a largely empirical field.

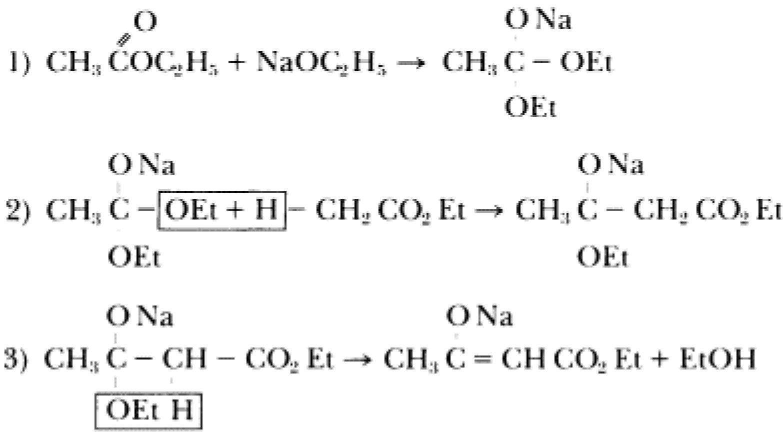
McElvain was one of those pioneers in the formative years of American organic chemistry. His work resulted in major contributions to the understanding of the mechanism of certain base-catalyzed reactions of esters and to the relation between structure and reactivity. It is this latter concern, together with an unusual ability to systematize these relationships, that led McElvain to what was probably his major scientific contribution, the discovery and study of the ketene acetals, a class of substances that proved to be of considerable synthetic interest, as well as of great significance with respect to the emerging theories of chemical reactivity.

McElvain's interest in relating chemical behavior to structure extended to biological activity, more particularly in the field of anesthesia. This led to another major contribution, the design of local anesthetics, some of which were developed into important and very successful drugs (Metycaine™, Surfacaine™) by the pharmaceutical house of Eli Lilly and Company.

Solomon and Eliza (Childess) McElvain had already had five children, four of whom died in infancy, when their last, Samuel Marion, was born on December 9, 1897 on a farm near DuQuoin, Illinois. The young McElvain attended the public school of DuQuoin, graduating from high school in 1915. The family then moved to St. Louis where, urged by his older brother who was a physician, McElvain enrolled in the St. Louis College of Pharmacy. Although this foray may have initiated his later interest in the design of possible anesthetics, McElvain became quickly convinced that his future lay elsewhere and, in 1916, he became a student in the Department of Chemical Engineering of Washington University in St. Louis. During his undergraduate days there, "Mac," as he had become known, partially supported himself through school by working in a drugstore during whatever spare time he had. Indeed, at the suggestion of the owner of the store, who had generously offered to pay his expenses if he should be successful, Mac traveled to Columbia, Missouri to take —and pass—the State Board examination to become a registered pharmacist. This little excursion did not, however, interfere with McElvain's chemical career; after his graduation from Washington University in 1920, McElvain started to work toward his Ph.D. in organic chemistry at the University of Illinois under the tutelage of Roger Adams. In 1923, just three years later, he received his Ph.D. and was appointed instructor in the Chemistry Department of Wisconsin. The young instructor wasted no time starting a research program that led him over the years into a number of areas I will now discuss. I have organized this brief survey as follows: I. The Acetoacetic Ester Condensation. II. Ketene Acetals. III. Chemistry of Pyridines and Piperidines.

I. THE ACETOACETIC ESTER CONDENSATION

It may be hard for today's reader to appreciate the rather nebulous approach to chemical mechanisms at the time McElvain began his research career. One of the then widely held mechanisms for the important Claisen self-condensation of ethyl acetate (the "acetoacetic ester condensation"), which is shown below, illustrates the situation:



McElvain examined, in a number of papers published between 1929 and 1934, some of the controversies then surrounding this reaction. He was able to show, by careful quantitative experiments: 1) that although sodium metal had repeatedly been claimed to be the initiator of the condensation reaction, the initiating reagent is actually the metal alkoxide formed by various side-reactions of the metal. This suggestion had indeed been put forward very early by Claisen himself, but it had subsequently been vigorously contested, inter alia, by Arthur Michael. 2) That the "acetoacetic ester condensation" could be applied, in contrast to previous claims, to form the -ketoesters derived from the homologs of propionic esters simply by removal of the alcohol generated in the condensation, thus driving it to completion. 3) That the previous failure to achieve the self-condensation of

McElvain examined, in a number of papers published between 1929

esters such as ethyl isobutyrate, which only have one hydrogen alpha to the ester carbonyl, is not an intrinsic property of such esters. He concluded that such condensations might succeed if stronger bases than metal alkoxides were used, and his suggestion to Spielman and Schmidt that these self-condensations might work with mesitylmagnesium bromide indeed led to success. Hauser, independently, made the same observation, using sodium triphenylmethide as the base.

These contributions were fundamental to the development of our present understanding of base-catalyzed reactions.

II. KETENE ACETALS

McElvain's interest in ketene acetal was originally aroused by a proposal in the literature that 1,1-diethoxyethylene, the diethylacetal of ketene, is an intermediate in the above-mentioned Claisen self-condensation of ethyl acetate. McElvain's view of the course of the Claisen condensation was incompatible with this suggestion. The properties ascribed to the supposed ketene acetal seemed to him highly unlikely, and he showed that the "intermediate" that had been isolated was actually a mixture of ethanol and ethyl acetate! The postulated structure did, however, seem intriguing, and McElvain set out to synthesize the real ketene acetal. Success was reported in 1936, and over the next nineteen years McElvain published thirty-seven papers encompassing the synthesis, properties, and synthetic usefulness of this unusually reactive class of substances.

McElvain quickly recognized (1940) that the ketene acetal structure was a special case of what Robert Robinson had labeled a "heteroenoid" system and as such should exhibit especially high nucleophilicity. It is also historically and pedagogically (more about this later) noteworthy that McElvain's second and third papers on ketene acetals (J. Am. Chem.

Soc., 62:964, 1281; 1940) were among the first papers in the United States to make explicit use of the "curved arrows" introduced a few years earlier, largely by Robinson in England, to indicate the motion of electron pairs attending bond formation.

A large number of novel reactions were encountered in the study of ketene acetals for which McElvain introduced a general method of synthesis, the dealcoholation of orthoesters by means of aluminum alkoxides. The study of the unusual chemistry of these nucleophilic species was to lead to a number of surprising reactions, such as the remarkable ability of the parent substance to form carbon-carbon bonds with particularly reactive halides such as benzyl halides.

$$C_6H_5CH_2Br + H_2C = C\begin{matrix} OEt \\ OEt \end{matrix} \rightarrow C_6H_5\,CH_2\,CH_2\,\overset{O}{\overset{\|}{C}} - OEt + Et\,Br$$

This reaction is not of great synthetic utility but has mechanistic analogies with the broad area of enamine chemistry, which was developed much later. Much more potentially useful was the reaction of ketene diethylacetal with acid chlorides, a reaction that leads to a very simple synthesis of *b*-ketoesters. This is shown below

$$R - \overset{O}{\overset{\|}{C}}\,Cl + CH_2 = C\begin{matrix} OEt \\ OEt \end{matrix} \rightarrow R - \overset{O}{\overset{\|}{C}} - CH_2\,\overset{O}{\overset{\|}{C}} - OEt + Et\,Cl$$

and will serve to illustrate the general type of reaction undergone by ketene acetals with strong electrophiles. Note that the β-ketoester thus formed is accompanied only by the volatile ethyl chloride, and that the reaction represents a completely general synthesis of the very class of compounds that had attracted McElvain's early interest in an entirely different context (see above). Every generation is condemned to redis cover part of what was known to the previous one, and it is interesting that a very similar reaction has been "introduced" recently that uses a mixed ethoxy trialkylsiloxy ketene acetal in place of the more convenient ketene acetal.

III. PYRIDINE AND PIPERIDINE CHEMISTRY

McElvain's early interest in pharmacology focused on cocaine analogs during his graduate work with Roger Adams at Illinois. By the time he started independent work as an instructor at Wisconsin, McElvain was convinced that structures embodying relatively simple elements of the cocaine structure might show both enhanced anesthetic activity and lowered side effects. McElvain's first independent publication (1924) describes the synthesis of benzoates of simple N-hydroxyalkylpiperidines. It is extraordinary that it includes the substance that eventually became, after its introduction by Eli Lilly, the widely used local anesthetic Metycaine™. This marked the beginning of a long and fruitful association between McElvain and Eli Lilly.

A number of additional substances of this general type were synthesized in collaboration with Thomas P. Carney, who was a postdoctoral associate of McElvain's in 1943 and 1944 and later became a vice president of Eli Lilly. I still remember the sight on Tom Carney's laboratory bench when all of the various piperidine hydrobromides he had made on one of his last days at work were stacked to dry on pieces of filter paper held on ring stands all over the laboratory. One of these substances eventually was developed by Eli Lilly as the clinically valuable local anesthetic Surfacaine™.

Although he returned to this pharmaceutical interest in anesthetics from time to time, McElvain mostly used it to spark important fundamental research both in piperidine and in pyridine chemistry. This area, like that of ketene acetal chemistry, remained a lifelong interest that found expression in over thirty papers. In pyridine chemistry, McElvain made important studies of the bromination and sulfonation of pyridines and introduced a number of much-improved methods for the synthesis of several of these heteroatom-substituted pyridines. He was the first to find a general synthesis of 3- and 4-piperidones by the use of the Dieckman cyclization of aminodiesters. In many cases the resulting ketones were further elaborated to potential cocaine analogs.

McElvain's involvement in piperidine chemistry, parenthetically, turned out to be of considerable interest to me: it led to my going to the University of Wisconsin for the Ph.D. because of the (correct) surmise that McElvain might show some sympathetic interest in my planned use of 4-piperidones in a quinine synthesis.

MCELVAIN AS TEACHER

Important as many of McElvain's scientific contributions have proven to be, his influence as a guide of the young people who elected to work with him for the doctorate was even greater—and this influence did not confine itself to the members of his group. McElvain was an outstanding teacher, and his graduate courses were extremely successful. One was the celebrated course in qualitative organic analysis for which he wrote a successful text *The Characterization of Organic Compounds* (1945; revised edition, 1953). Many first tasted in that course the thrill of discovery and acquired the skill of putting together the pieces of a chemical detective story, of weighing the importance of conflicting clues, and of exercising their powers of logical analysis. This, of course, was all without benefit of NMR or of infrared spectroscopy and without the help of gas, liquid, or thin layer chromatography! The course relied sometimes, as any course must have at the time, on color tests of dubious generality and made use of samples of "unknowns" that were very large by current standards. Much was learned in the course, in large part because of McElvain's insistance on clear, logical analysis.

In one other graduate course, or rather a portion of a graduate course that he shared with two other members of the organic staff, McElvain must have been one of the first to teach systematically the particular way to look at reactivity that was then called the "Electronic Theory of the English School." This referred to the analysis of the course of reactions based on an attempt at the rational prediction of the fate of electron pairs involved in bond making and breaking. The brilliant systematization Robinson had developed in an *Outline of an Electrochemical* (*Electronic*) *Theory of Organic Reactions* (1932) had been given wider circulation by the chapter written by J. R. Johnson in the first edition (1938) of Gilman's celebrated *Advanced Organic Chemistry.* This approach to chemistry was, however, far from common when McElvain started teaching it formally.

McElvain was able to project a certain avuncular sternness (everyone in his group referred to him as "Uncle Mac"). He was always accessible, his door open—but one had to have something to say. One did not drop in for casual conversation. Mac would usually be working at his desk on a manuscript that he would later turn over to Grace Legler, the organic group's secretary. He would raise one eye from his writing toward the visitor, who was expected to get to the point, unassisted. Having thus ensured that he would be spared trivialities, Mac then gave the matter his concentrated attention. Perhaps his sense of humor was even more appreciated because of this kind of aloofness. I will always remember the sudden hearty laughter with which he greeted an outraged report of my (presumed?) lack of cooperation in enforcing rigorous discipline on the undergraduates in the laboratory section in which I was a teaching assistant. He then arranged for me to get a University Fellowship to replace the assistantship for which the powers that be had found me no longer acceptable.

It took some time for the graduate students in Mac's group to appreciate the warmth behind the projected aloofness. It became quite evident when Professor and Mrs. McElvain entertained those of his group who had remained in Madison at Thanksgiving dinners in their home. And it was with great anticipation that, on several such occasions, we walked through the snow to the house on 2017 Adams Street.

One cannot help thinking that McElvain's special ability to instill high standards and respect for hard work, his emphasis on developing responsible self-reliance, at the same time that he was able to make his students aware of his genuine interest in their problems, were responsible for the remarkable fact that three of the students who received their doctorates in his group eventually won the American Chemical Society Award in Pure Chemistry. No other teacher of organic chemistry has yet equalled this record.

Professor McElvain was married in 1926 to Helen Roth of Madison. They had two daughters, Anne, who is now Mrs. William R. Frazier of Princeton, N.J., and Jane, now Mrs. Carl E. Jenkins of Bath, Ohio. I have had the good fortune to know Mrs. McElvain for many years. The cheerful enthusiasm, which I first encountered at the Thanksgiving parties I mentioned, was just as apparent when I had the honor of being McElvain Visiting Scholar in Madison in 1977.

It would seem appropriate to end this brief account of Professor McElvain's career by recording some of the more notable recognitions accorded him: he was chairman of the Organic Division of the American Chemical Society in 1945 and 1946; on the editorial board of the *Journal of the American Chemical Society,* 1946-1956; and he was elected to the National Academy of Sciences in 1949. The Regents of the

University of Wisconsin awarded him the title of Professor Emeritus when he retired early (at the age of sixty-three) in 1961.

The University of Wisconsin has recognized his leadership, which contributed so much to bringing its Chemistry Department to the front rank, his scientific accomplishments, and his loyalty to the University in a variety of ways. An Organic Symposium was held in his honor, in Madison, upon his retirement in 1961. The S. M. McElvain Professorship was created in 1972 (with Harlan Goering as its first incumbent). The S. M. McElvain Visiting Scholarship was established in 1977 and, finally, the organic laboratories in the Daniels Matthews Chemistry Building at Wisconsin were named the Samuel M. McElvain Laboratories of Organic Chemistry at a ceremony on March 15, 1979. A great honor befitting an outstanding man.

I wish to express my sincere thanks first to Mrs. McElvain, whose recent death before this could be published was a source of sadness to many, and also to professors Aaron J. Ihde and Harlan Goering who supplied me with much essential material.

PH.D. DEGREES TAKEN UNDER S. M. MCELVAIN

1928	Nelson W. Bolyard, James R. Thayer
1930	Charles F. Bailey, Kenneth Crook
1931	Robert N. Isbell, Charles F. Koelsch, Joseph Semb
1932	Arthur C. Cope, Edward A. Prill, J. M. Snell, Frank M. Strong, William B. Thomas
1933	Richard F. B. Cox, Benjamin W. Howk, Sulo A. Karjala
1934	Wray V. Drake, Norman G. Fischer, Edwin H. Kroeker, Lewis A. Walter
1936	John R. Roland
1937	Fred Beyerstedt, Arthur Magnani
1938	Harry M. Barnes, Stanford Moore, Charles W. Tullock
1940	Emanuel L. Foreman
1941	Harry Cohn, Harold G. Johnson, Allan K. Schneider, Robert F. Taylor
1942	Howard B. Burkett, Donald G. Kundiger, Philip M. Walters
1944	Edward L. Engelhardt, Arthur G. Jelinek, Robert E. Kent, James W. Langston
1945	Gilbert Stork
1947	Robert L. Clarke, Kurt J. Rorig, Calvin L. Stevens
1948	Michael J. Curry, Robert E. McMahon, Everett H. Pryde, Juel P. Schroeder, John Vozzam
1949	George P. Gregory, Robert E. Lyle, Jr., John C. Safranski, Jr., Burris D. Tiffany, James T. Venerable
1950	William B. Dickinson, Paul M. Laughton, Melyin M. Olson, Bryce E. Tate, Kenneth N. Warner, Jr., Spencer H. Watkins
1951	Archibald N. Bolstad, Gerald Gilbert, Herbert F. McShane, Jr., Leo R. Morris, Wallace F. Runge
1952	Clyde L. Aldridge, Edward R. Degginer, Charles H. Stammer
1953.	Loren W. Bannister, Averal T. Trimble, Jr
1954	Robert J. Athey, Richard S. Berger, Roy E. Starn, Jr.
1955	Edmund J. Eisenbraun
1956	Robert G. McKay, Jr., Phillip H. Parker, Jr.
1957	Robert B. Bates, David H. Clemens
1958	Rodney B. Clampitt, Thomas A. Lies, David C. Remy, Philip L. Weyna

POSTDOCTORAL ASSOCIATES

Paul R. Johnson (1938-1940)
Robert Bright (1940-1941)
Thomas P. Carney (1943-1944)
Luis A. Perez-Medina (1944-1945)
Raymond Mariella (1945-1946)

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August 15, 1878-February 15, 1963

by Chandler McC. Brooks

Walter J. Meek belonged to the generation of exclusively American-trained biomedical scientists that first demonstrated the competence of American scholarship and raised the level of American attainment in physiology to rival that of any other nation of the Western World. He was a member of the distinguished group of scientists who founded the University of Wisconsin's School of Medicine and established its high repute. At present and in the future, all who review the accumulated knowledge of the heart, especially the origin of the heartbeat, will refer to papers bearing the names of Walter J. Meek and his students and associates. Those who have written of Dr. Meek and their acquaintance with him have emphasized, rather than his scientific accomplishments, his extraordinary qualities of intelligence, industry, integrity, warmth of personality, and loyalty to family, medical school, friends, and his profession. He was held in respect and affection by his colleagues young and old.

Walter Joseph Meek was born in Dillon, Kansas on August 15, 1878, the son of William E. A. and Mary Hester (White) Meek. He was of Scottish, English, and Irish ancestry. It is not known exactly when the ancestors of the Meek family emigrated to America, but it is known that in 1750 they resided in northern Virginia. After the Civil War they emigrated by way of North Carolina and Knoxville, Tennessee to a homestead in east Kansas, near Abilene. Initially they had great difficulty raising crops because of grasshopper plagues.

The cattle drives from Texas to Abilene had stopped by the time Walter Meek was born, but Kansas was still "frontier" country—that period, now so romanticized, had not yet ended. Walter's sister—he had two brothers and a sister—was reputed to have been escorted home from a dance by Wild Bill Hickock, the marshall. When rather young, Walter accompanied a relative on an expedition into Indian territory to bring back an escaped prisoner; he slept out on the prairie by a campfire each night. (Later in life he continued to enjoy trips into the mountains and wild country. He took one such excursion with Herbert Gasser in 1933 or 1934, in an aircooled Franklin car, to the Southwest. They were among the first 5,000 or so visitors to see Rainbow Bridge, and in order to get there they had to ride part of the way on horseback and sleep out overnight in the open.)

Walter was much younger than his brothers and sister; he was an uncle at the age of six. His father died when he was only eight and his mother a few years later. Subsequently, he was brought up with his first cousins under a rather strict, conservative, puritanical regime. Later in life he became a staunch Republican and recounted with pride that the Eisenhowers lived in the town where he grew up. He and his cousins learned, he said, never to pick fights with the Eisenhowers because of their combative ability.

His cousins evidently helped cultivate his educational aspirations. One of them, Eli Sawtell, attended the University of Kansas and became very proficient in Greek and Latin. Dr. Meek told his son that he thought if Eli could graduate from college, he could too. Eli encouraged him, and he graduated from the University of Kansas in 1902. Although he had been president of the senior class and editor of the school paper, he still managed to graduate with the highest average attained at the University up to his day. He was a founding member of the Alpha Tau Omega Chapter of the University. He was elected to Phi Beta Kappa and was the first undergraduate to become a member of Sigma Xi. As a student he much prized his Phi Beta Kappa key and wore it continually, even when he was working summers in the wheat fields. Eventually he lost his key in those fields, but a man working in a grain elevator in Minneapolis found it in the wheat and returned it to him.

After graduating from Kansas, Walter Meek studied at Penn College in Iowa and the University of Chicago, obtaining a Ph.D. degree in physiology from Chicago in 1909. He taught at Penn College in Oskaloosa, Iowa from 1903 until 1908. He had attained the rank of professor of biology there when he was invited to join Joseph Erlanger at the University of Wisconsin. Erlanger's statement of purpose in offering an instructorship noted that he wished Dr. Meek: "To assist me with organization of, and the teaching in the laboratory." Dr. Meek served Wisconsin as instructor in physiology (1908-1910), assistant professor (1910-1912), associate professor (1912-1918), and professor (1918-1948).

When Erlanger resigned his position at Wisconsin to go to Washington University in St. Louis, J. A. E. Eyster, then professor of pharmacology at the University of Virginia, was appointed in his place. Eyster's primary interest was in research. He was not as outgoing as Dr. Meek was, and he was not a popular lecturer. He and Meek were quite congenial, however, and collaborated in research on the cardiovascular system for thirty years. Eyster was an excellent physiologist and deserves much credit for their mutual success.

Meek was a skilled administrator and students liked his lectures. On Meek's retirement, President Elvehjem said Dr. Meek was "the best classroom teacher under whom he had studied." Very soon Eyster and Meek exchanged jobs, and Meek remained chairman of physiology until his retirement in 1948. Eyster then reassumed the chairmanship and retained it until his own retirement in 1952. The first Eyster and Meek article was published in 1912. It was quite appropriate that William H. Howell, one of the founders of *Physiological Reviews,* should, and did, solicit an article from them for the first issue ("The Origins and Conduction of the Heart Beat," 1 (1): 1, 1921).

The first students to receive Ph.D. degrees from the Department of Physiology at Wisconsin were K. K. Chen, Chauncey D. Leake, and Ethel Ronzoni (Mrs. George H. Bishop). By 1952 some thirty-eight persons had received that degree. Dr. Meek's last student was Eleanor M. Larsen. The attainments of the department in Dr. Meek's time are described in a chronicle of the University of Wisconsin, Medical School, 1848-1948 (Paul F. Clark, *The History of the University of Wisconsin* [Madison: University of Wisconsin Press, 1967]).

In 1920 Meek became assistant dean of the Medical School. His interest in students made him an effective advisor. Among those he advised were premedical students from the University. It is said that at times he became briefly unpopular for advising some leading athletes to minimize their physical efforts and study more if they wished to become doctors. Some of them did just that, to the detriment of an occasional team record. In addition to advising premedics, Dr. Meek for many years was in charge of admissions to the Medical School. He found this selection quite taxing because some students he thought would make only average doctors turned out to be superb, while others he thought would be superb turned out to be mediocre. He continued to meet these responsibilities, however, and held the posts of assistant dean of medicine from 1920 through 1942, acting dean from 1942 through 1945, and associate dean from 1945 until his retirement. He was also trustee of the Madison General Hospital, a service he performed without remuneration for many years, and a member of the Governor's Advisory Committee on Medical Education.

Dr. Meek held the commission of major in the Chemical Warfare Service during the first World War. A chemical warfare unit was set up at the University, and from 1917 to 1919 Eyster and Meek were responsible for much of the work carried on there. Many of the initial investigations on the biological effects of mustard gas, lewisite, and phosgene were made by this unit. In order to facilitate their work the army was asked to provide a chemist. They sent a young drill sergeant who had majored in chemistry at Princeton, Chauncy Leake —called "Sarge" by the Meeks. This association started Dr. Leake on his career as a pharmacologist.

On December 26, 1906 Dr. Meek married Crescence Eberley. He met her on shipboard during his first trip to Europe. They had three children: Joseph Walter Meek, born May 2, 1912, professor in the Law School of the University of New Mexico, died 1954; Mary Crescence Meek, born May 20, 1917, served as a stewardess for American Airlines for many years; and John Sawyer Meek, born August 12, 1918, became professor of chemistry at the University of Colorado. One gathers that Dr. Meek and wife were a very congenial and adventure some couple. Their travels took them to Switzerland several times to hike over the high passes. They also attended the Passion Play, where Dr. Meek took many photographs on glass slides; after developing them he colored them for lantern slide projections, which followed notes written by Mrs. Meek detailing the costumes and their colors. Dr. Meek had climbed Pikes Peak at an early date, before there were auto roads to ascend, and in 1902 he visited Yellowstone. Mrs. Meek also found pleasure in such outings, and in 1914 the family spent the summer in Glacier National

Park. According to their son John, Mrs. Meek learned to drive a car long before Dr. Meek, but he was only the third professor at Wisconsin to own an automobile. After 1920 the family took many trips around the country. Dr. Meek occasionally (1926 and 1927) left them to vacation and explore the East Coast alone while he attended international meetings in Europe, but usually the whole family took their vacations together.

In many of his nonprofessional activities, Dr. Meek's puritanical work ethic and ingenuity probably made him somewhat overwhelmed. He was a bookbinder; for many years he bound all the journals to which he susbscribed. As mentioned previously, he was an enthusiastic photographer, doing all his own developing and enlarging. One of his accomplishments was to make hand-colored portraits of his children. Among the characteristics that impressed his son John as unique were Dr. Meek's tremendous memory and his great enthusiasm for anything he undertook. He was an avid gardener and a naturalist, a member of the Society of American Naturalists.

In 1924 Mrs. Meek inherited some pewter that had belonged to her great-grandmother. She thought it would be nice to have a table setting of pewter, so they began to collect old pieces. Some of their purchases were found to be defective, so Dr. Meek learned to repair them. Soon he was making molds, casting plates, and producing beakers, porringers, mugs, jewel boxes, and the like. Their collection became famous, at least locally. Once they had enough pewter to serve twelve people, Dr. Meek decided it should be used on an antique table. Next, it was decided that the table should stand on old-fashioned rugs. The couple began to hook rugs, fifteen minutes every morning and every evening. Gradually their house in Madison was filled with antique furniture they had purchased, repaired, and refinished. All this blended well with the copper fixtures they had themselves made for the house when it was built in 1912.

In the 1930s the youngest son, John, became interested in collecting stamps. This attracted his father's interest, with the result that Dr. Meek made albums, borders, and display cabinets lined with velvet. They soon were looking for plate shifts, double transfers, odd cancellations, and so forth. Everything was so well organized for display that when occasions for competitive display came, they usually obtained "best of show" awards. This interest in collecting and in hobbies never died. When Dr. Meek retired to Florida he became involved in collecting shells. Again, he made display cabinets and labeled each specimen with its scientific name and where he had found it.

Dr. Meek's daily schedule was: Up at seven, off to work at eight, home for lunch at twelve-thirty. He had trained himself to lie down at one, go to sleep instantly, and awaken at one-thirty. He then went back to work in his laboratory but returned home at five-thirty.

Dr. Meek, though not a sportsman, did play golf left-handed. When asked by his son why he did that when he was really right-handed, his father explained that when he was a student at Kansas some athletic activity was required. He had refused to work out in the dusty old gym, so he was told he had to engage in some physical activity if he expected to graduate. He was not a large man, so it was decided that golf would be acceptable. A professor who was left-handed gave him a set of clubs; that was the way he learned to play the game.

During the depression of the 1930s, a steam laundry in town failed, and Dr. Meek evidently had made an investment in it. Consequently, he became a member of the new board of managers and ultimately became the director and manager.

His administrative skills were brought into play and before long the business again became solvent. As far as is known, this was the only business venture undertaken by Dr. Meek.

Another avocation was the study of history. The well-known historian, Erwin H. Ackerknecht, and Dr. Meek's associate, Dr. R. C. Herrin, have written extensively in praise of Dr. Meek's contributions to the study of the history of medicine. His bibliography of 110 titles contains only six medico-historical papers, but Professor Ackerknecht states that they do not reveal the full extent of his contribution. He wrote or prepared papers chiefly about those who had contributed to pysiology, including the following titles: "Franz Joseph Gall" (1915), "Charles Bell" (1916), "Albrecht VonHaller" (1917), "English Medical Guilds" (1920), "Beginnings of American Physiology" (192 1), "The Gentle Art of Poisoning" (1922), "A Medical Reformer" (1923), "T. Rabelais" (1924), "The American Physiological Society" (1925), "Johannes E. Purkinje" (1927), "Carl Ludwig" (1931), "Du Bois Reymond" (1932), "Ernst Bruecke" (1933), "Fabricius, A Man Who Missed His Opportunity" (1935), "John Call Dalton" (1938), "Claude Bernard" (1939), "Richard Lower" (1940-41), "Medical References in Shakespeare" (1942), "The Endocrines" (1945-46), "Walter B. Cannon" (1948-49), and "The Adrenals" (1952-53). Most of these and other such papers were delivered at William Snow Miller's seminars on medical history at the University. Professor Ackerknecht regrets that many of the papers read in these seminars were not published but he states that the group of individuals to which Dr. Meek belonged did initiate a revival of interest in medical history in America.

For a brief period Dr. Meek taught medical history in the University. Incidentally, he treasured the fact that he was born the year Claude Bernard died. He never studied abroad but few had a greater interest in the physiologists of Europe.

His son reports that he was delighted to meet Pavlov at an International Congress and considered it a great privilege. It is surprising that he could accomplish so much and sustain such a variety of interests when he had a heavy teaching load dealing with physiology for the medical students. For some years he also lectured to home economics students. Initially he had only 100 such students in the course, but numbers quickly increased to over 160. He could describe the most complicated functions in such a way that all could understand. In the course for first-year medical students he chiefly taught neurophysiology. Another of his special duties relative to teaching and research was that he had charge of procurement of all dogs used in the medical school. At many sessions of the state legislature Dr. Meek had to appear and justify the use of animals to offset the criticisms and actions of the antivivisectionists. For many years he also prepared the questions in physiology for the Wisconsin State Board licensure examination in medicine.

In addition to all these other responsibilities and activities, and without much technical help, Dr. Meek managed to accomplish much in a number of fields of research. He published 110 scientific papers, many of these with J. A. E. Eyster. He conducted some early studies with A.J. Carlson on the limulus heart. He was the first in this country to employ the method of primary negativity in tracing the origin and course of the excitatory process in the heart. He detected shifting of the pacemaker during vagus stimulation and when the sinoatrial node was destroyed. He used timed X-ray exposures to study events of the cardiac cycle and the output of the heart. He was interested in the effects of exercise, hemorrhage, and plethora. His studies of the significance and consequence of the enlargement of the heart in athletes still receive attention.

Probably the most clinically relevant contribution made by

Dr. Meek was the discovery, in collaboration with Maurice H. Seevers and Ralph M. Waters, that catecholamines cause ventricular fibrillation in dogs anesthetized with cyclopropane. Further studies of the effects of catecholamines on ventricular irritability, conducted in cooperation with Orth, Murphy, Stutzman, and Allen, provided information concerning the mechanism of this action; they identified epinephrine congeners that did not produce serious ventricular irritability. Dr. Meek eventually chose phenylephrine as the best vasopressor agent for producing a rise in blood pressure without a resulting paroxysmal atrial tachycardia. This work, which he described in a Harvey Lecture delivered March 20, 1941, "Some Cardiac Effects of the Inhalation Anesthetics and the Sympathomimetic Amines," was of much interest to both pharmacologists and anesthetists.

Meek's later work was concerned with gastrointestinal physiology. He studied chemical transmission of vagal effects on the small intestine, the influence of intestinal distension on gastric motility, and the actions of adrenalin and general anesthesia on intestinal function. He studied the causes of intestinal obstruction and ulceration. Some summaries state that his work dealt mainly with the heart, circulation, gastrointestinal tract, and autonomic nervous system. He clearly studied other matters as well, as revealed by his bibliography. In *American Men of Science* Dr. Meek listed circulation, shock, and the effects of anesthetics on the heart as his three main areas of research.

It is said of Dr. Meek that he seemed to have the power of anticipating trends of scientific development. This was manifest in his association with the American Physiological Society. Dr. Meek became a member of the APS in 1908. He was elected to the Council in 1915, and served as secretary from 1924 to 1929 and president from 1930 to 1932. After his presidency he returned to the Council for four more years. In 1933 he was appointed chairman of the newly founded Board of Publication Trustees, which has controlled the business and editorial policies of all publications sponsored by the American Physiological Society since 1935. At the forty-ninth meeting of the Society in Memphis (1937), he proposed establishment of *The Annual Review of Physiology;* this recommendation was approved. He served on the Board of Publication Trustees for most of his life and was to a large degree responsible for the Society's very effective publication policies and actions. His unusual powers of organization were frequently used by the Society. He was chairman of the Centennial Committee for the fiftieth annual meeting held in 1938. Dr. Meek also served in collaboration with Drs. W. B. Cannon and A. J. Carlson as chairman of the committee for selection and nomination of honorary members for the APS. He was one of the leaders of American physiology for half a century. He participated actively in scientific sessions held at annual meetings of the Society. Among the many papers he presented before the Society, the following were of greatest interest: "The Origin of Fibrinogen in the Liver," "The Initiation and Course of Cardiac Excitation," and "Distension as a Factor in Intestinal Obstruction."

I became a member of the American Physiological Society in 1934, a year after Dr. Meek's term as president had ended. I never had a conversation with him, but I observed his actions and I knew him through his friends. Dr. Meek was always busy, surrounded by friends, and thus less accessible than was Joseph Erlanger, for example, who always appeared to be alone and available to lunch with younger unknown men. I also knew many of Walter Meek's other famous associates better than I knew him: Herbert Gasser, George H. Bishop, A. J. Carlson, K. K. Chen, Chauncy D. Leake, and Ethel Ronzoni. All of these held Dr. Meek in high esteem. The men I knew of my generation who worked with Dr.

Meek also made known their respect and affection: Warren Gilson, M. H. Seevers, R. C. Herrin, W. B. Youmans, Paul Cranefield, and many others. I did hear him discuss proposals at meetings of the American Physiological Society; he was cautious, conservative, and not always on the winning side, but his opinions were respected. I was not studying the heart when Dr. Meek was most active in that field but I knew of his work. For many years I used in my lectures on the autonomic system an illustration of acetylcholine assay by Meek—partially because it surprised me that he was doing such work at a time when the field was dominated by Sir Henry Dale. One of my strongest impressions of Dr. Meek's perception and kindness was obtained from an "Appreciation of Walter B. Cannon" that he wrote in 1933 for the *Texas Reports on Biology and Medicine.* Cannon did not always receive from some of Meek's contemporaries the respectful treatment he deserved in meetings of the American Physiological Society. Those of us who were of Walter B. Cannon's school much appreciated Meek's statement.

Dr. Meek retired officially in 1948. He remained at Wisconsin as a research professor for one more year. After that he gave some historical lectures at the University of Texas and served on a committee to make recommendations concerning establishment of the medical school at Gainesville, Florida.

Dr. Meek had several heart attacks before retiring but recovered with bed rest. He developed diabetes at the age of fifty-five, and it became increasingly hard to control. His death occurred quitely at his winter home at Fort Myers Beach, Florida on February 15, 1963 at the age of eighty-four. His ashes are buried with his wife's in her family's burial plot at Westfield, Pennsylvania. Mrs. Meek died in 1973 at the age of ninety-two. She was able to attend the dedication of "Meek House," a part of the Witte Dormitory at the

University of Wisconsin. The University also published a biographic *Memorial Resolution* in honor of their distinguished faculty member, Emeritus Dean Walter Joseph Meek.

Biographical accounts indicate that physiologists in his day were not members of so many societies as is now required. In addition to the American Physiological Society, he belonged to the Society for Experimental Biology and Medicine, American Zoologists, American Naturalists, and The Harvey Society. He was listed in *American Men of Science.*

Dr. Meek received many honors during his life and posthumously. There is a Meek Library and a Meek House at Madison. In 1944 he was awarded membership in the Wisconsin State Medical Society and was recipient of its Man of the Year award. The American Society of Anesthesiologists elected him to honorary membership. In 1948 he was awarded an honorary degree (D.Sc.) by the University of Wisconsin. In 1949, one year after his retirement, he received a Distinguished Service Award from the University of Kansas. The excellence of Dr. Meek's scientific contributions was recognized by his election to membership in the National Academy of Sciences in 1947. His principal contributions to science are here listed.

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Photograph courtesy of the California Institute of Technology

Rudolph Leo Bernhard Minkowski

May 28, 1895-January 4, 1976

by Donald E. Osterbrock

Rudolph Minkowski was born in Germany near the end of the last century and died in California during the final quarter of this century. He was trained as a laboratory physicist, but worked most of his life as an observational astronomer. Using the largest optical telescopes in the world, he made important contributions to nearly every branch of nebular and extragalactic astronomy, but his most important contribution of all was to the identification and interpretation of cosmic radio sources. His monument is the National Geographic Society—Palomar Observatory Sky Survey. He guided, encouraged, and counseled a generation of radio and optical astronomers.

Minkowski was born in Strassburg, then part of Germany, on May 28, 1895. His grandfather had hurriedly moved his family to Königsberg from their native Russia less than twenty-five years before to escape the policy of anti-Semitic persecution adopted by the Czar's government. Rudolph's father Oskar, educated in Königsberg, became a physician, and at the time of Rudolph's birth he was a well-known pathologist on the Strassburg University medical faculty. His research had played a very important part in understanding the causes of diabetes. Rudolph's older uncle, Max, took over the Minkowski family business in Königsberg, while his younger uncle, Hermann, became a world-famous professor of mathematics, first at Zürich, then in Göttingen. He made many very important discoveries and is perhaps best known for his idea of the space-time continuum, which provides the simplest and best mathematical basis for handling the special theory of relativity.

As his father moved up in the academic hierarchy, Rudolph was educated in Gymnasia at Cologne, Greifswald, and Breslau and then entered the University of Breslau, where he studied physics and earned his Ph.D. in 1921. He had served in the German army during World War I. At the university he specialized in optics and spectroscopy, and his thesis, done under the supervision of Rudolf Ladenburg, was on the Na I D lines and the information on the physical and chemical properties of sodium that could be drawn from them. After receiving his Ph.D., Minkowski continued to work briefly at Breslau, with Ladenburg, and then at Göttingen, with James Franck and Max Born. He then moved on to Hamburg, where he started as an assistant at the Physikalisches Staatsinstitut in 1922; he became a Privat-Dozent in 1926, and he was appointed to a professorship in 1931. Minkowski's research at Hamburg was at first centered on atomic physics and spectral lines. He worked in close association with a vigorous group of physicists, including, among others, Albrecht Unsöld and Wolfgang Pauli.

Minkowski, however, had been interested in astronomy from childhood; at Hamburg he soon met Walter Baade, then a young assistant to Max Wolf at the Hamburg Sternwarte. Although he continued his spectroscopic and experimental quantum mechanical research at Hamburg, Minkowski's field of specialization shifted increasingly to astrophysics, and he published his first astronomical paper with Baade, F. Goos, and P. P. Koch in 1933. It concerned the interferometric measurements of the profiles of emission lines in the spectrum of the Orion nebula, a subject to which Minkowski's training and experience enabled him to make important technical contributions.

By this time Adolf Hitler had come to power in Germany. Minkowski had married Luise David in Leipzig on August 23, 1926. Her father, Alfons David, was a judge who had been appointed to the Supreme Court of Germany in 1917. Hitler became Reichschancellor in 1933, and one of his government's first actions was to order the universities to get rid of almost every "full-blooded Jew" who held a teaching position. Justice David was forced off the high court by the Nazis because he was a Jew. Although Minkowski and his parents were baptized Christians, their family was historically Jewish. Baade, who earlier had spent a year in the United States as a Carnegie International fellow, had emigrated to take a position on the staff of the Mount Wilson Observatory in Pasadena in 1931. He urged his friend to join him there and in 1935, the year in which Hitler proclaimed his so-called "Law for the Protection of German Blood and Honor," Minkowski, with his wife and their children Eva and Herman, left his homeland. At first he had only a research assistantship at Mount Wilson; in addition he gave a series of lectures on atomic spectra to the staff members, for which they contributed a little cash to help him get established in America. Before the year was out he had been appointed to a regular position and was on his way.

Under Director Walter Adams, the Mount Wilson staff used the 60-inch and 100-inch reflectors, the latter the largest telescope then in existence, in a highly compartmentalized observational research program. One staff member, Paul Merrill, studied the spectra of M giants, supergiants, and long-period variables; another, Roscoe Sanford, studied the spectra of carbon stars; and a third, Alfred Joy, studied the spectra of variable stars that were not long-period variables or carbon stars. Edwin Hubble headed the attack on the cosmological problem of the expansion of the universe, concentrating his observational work on the determination of the distances of the galaxies, while Milton Humason took their spectra to measure the redshifts. Baade worked on the stellar content and general properties of star clusters and galaxies. Except for Merrill, they all tended to think in astronomical, rather than physical, terms.

Minkowski, with his wide knowledge of spectroscopy, atomic physics, and applied quantum mechanics, became involved in all these studies, but specialized in work on gaseous nebulae and related objects. He began by using the fast, low-dispersion spectrographs designed for Humason's measurements of galaxies to take spectra of faint supernovae, the highly luminous exploding stars that flare up to a brightness comparable to an entire galaxy, as they were discovered in surveys by Fritz Zwicky, Baade, and others. Minkowski classified the emission-line spectra of supernovae and studied their development in time. The combination of his spectral classification with Baade's light curves led to the recognition that there are two different types of these objects and that in many cases a supernova's absolute magnitude, and hence the distance to the galaxy in which it occurs, can be estimated from a single spectrogram.

Minkowski obtained many spectra of various regions in the Crab nebula, which had recently been identified by Jan Oort and Nicholas Mayall as the remnant of a supernova that occurred in our Galaxy in A.D. 1054. He confirmed Mayall's result, that the spectra of the filaments of the Crab nebula indicated a high velocity of expansion, and that the amorphous region had a purely continuous spectrum. Minkowski measured this continuumn and correctly pointed out that it had no Balmer discontinuity, making it impossible to interpret as a thermal recombination spectrum. He did not realize at that time that the continuum was in fact nonthermal synchrotron emission; this was predicted much later by I. S. Shklovsky after the Crab nebula had been identified as a radio source, and it was observationally confirmed by optical polarization measured by V. A. Dombrovsky and M. A. Vashakidze and also by Oort and T. Wahlraven. Minkowski, from his spectra of the two stars identified by Baade as the possible supernova remnants because of their proximity to the center of expansion of the nebula, picked out the correct one—nearly thirty years later it became the first optically identified pulsar. Through the years Minkowski obtained spectra of the gaseous remnants of several other galactic supernovae. In particular, his measured radial velocities in the Cygnus loop and other roughly circular remnants have been widely used in theoretical discussions of the ages, distances, and energy outputs of these objects.

Planetary nebulae were another subject studied by Minkowski from his first days at Mount Wilson until years after his retirement. In his early work, he obtained spectrophotometric measurements of many low-surface-brightness planetaries that had been too faint for previous investigations, and he proved that their spectra were quite similar in overall pattern to the brighter objects. At Mount Wilson he organized a survey to find new planetary nebulae with an objective prism mounted on the 10-inch Cooke wide-angle camera. He used the 60-inch and 100-inch telescopes to take slit spectrograms of suspected planetaries turned up by this survey, objects with bright Hα and weak or nonexistent continua, and in this way more than doubled the number of known planetary nebulae. Minkowski then arranged for Karl G. Henize to take the same camera to South Africa and complete the planetary nebula survey in the southern Milky Way as part of his University of Michigan Ph.D. thesis.

Minkowski was engaged in a long program, originally in collaboration with Mayall at Lick Observatory, to measure the radial velocities of all the planetary nebulae, in order to study the kinematics of this old, disk system of objects that can be observed out to great distances from the sun. Eventually Minkowski obtained and measured nearly all the spectrograms himself, and, although he never published the individual velocities, he discussed the general results in a paper and in a review chapter published in 1965.

Minkowski was fascinated by the forms of planetary nebulae and invested large amounts of observing time with the 100-inch—and later with the 200-inch Hale telescope at Palomar—in taking direct photographs of individual objects. He used various combinations of glass filters and photographic emulsions to isolate narrow spectral regions around specific nebular emission lines, for instance, [O III] ll4959, 5007, and Hα + [N II] llX6548, 6563, 6583. These pictures, many of them taken in conditions of fine seeing, clearly illustrate the ionization structure of planetary nebulae, their frequently cylindrically symmetric overall structure, and their complicated fine structure, often consisting of filaments, condensations, knots, and the like down to the smallest resolvable scale. Although some of these planetary-nebula pictures were published by Minkowski himself, and more were used as illustrations in the two International Astronomical Union symposium volumes on planetary nebulae,[1] many of them have never been reproduced. Minkowski observed and analyzed the spectra of many individual planetaries, always trying to understand them physically: their masses, composition, temperature, and density structure, even their evolution.

As an expert in applied optics, Minkowski made many instrumental contributions to the Mount Wilson Observatory

[1] D. E. Osterbrock and C. R. O'Dell, eds., *Planetary Nebulae* (Dordrecht: D. Reidel, 1968), xv + 469 pp. and Yervant Terzian, ed., *Planetary Nebulae, Observations and Theory* (Dordrecht: D. Reidel, 1978), xxi + 373 pp.

spectrographs. Plane gratings were just coming into regular astronomical use in the 1940s, and Minkowski analyzed the curvature of the spectral lines they introduced, and how this effect may be corrected. His work on this subject received the sincerest form of appreciation when a paper was accepted and published in the *Astrophysical Journal* on the same subject over thirty-five years later, consisting entirely of results and conclusions that were included in Minkowski's original paper.

At Hamburg, Baade and Minkowski were close friends with the eccentric, one-armed optician, Bernhard Schmidt, who invented the Schmidt camera in 1930. This camera, a combination of a spherical mirror with a thin aspheric corrector at its center of curvature, forms a very fast, wide-field optical system. Used as photographic telescopes, Schmidt cameras can produce excellent deep exposures of nebulae and star fields, as shown by Schmidt himself, and later by Fritz Zwicky, with the 18-inch at Palomar Mountain. Minkowski was one of the leaders in pushing the use of Schmidt cameras in spectrographs, where they are far superior to the lens cameras previously employed. In particular, the *f*/1.5 conventional Schmidt and the *f*/0.67 solidblock Schmidt cameras designed by Minkowski for the 100-inch Newtonian spectrograph were faster and produced much better images than the older, thick-lens, microscopeobjective systems used by Humason for obtaining spectra of faint galaxies.

By the time Minkowski came to America in 1935, the design and construction of the 200-inch telescope for Palomar Observatory was well under way on the campus of the California Institute of Technology in Pasadena. It was built with funds provided by the Rockefeller Foundation, with the understanding that Palomar was to be operated jointly with Mount Wilson Observatory by Caltech and the Mount Wilson staff. Undoubtedly, Baade and Minkowski were among the strongest voices in urging Adams, Hubble, and the rest of the Observatory Council to recommend enlarging the project by building the largest Schmidt telescope in the world to supplement the largest reflector in the world. The result was the 48-inch Schmidt telescope at Palomar, a magnificent *f*/2.5 instrument that takes plates covering over six degrees square with excellent definition.

The first large task for the 48-inch Schmidt, after it went into regular operation in 1950, was the National Geographic Society–Palomar Observatory Sky Survey. The entire sky from the north pole down to declination –33° was surveyed in 935 preselected, overlapping fields. Two plates—a blue exposure, covering the wavelength region ll3600–4800, and a red exposure, covering ll6200–6700—were taken in immediate succession. If these exposures passed rigid quality and uniformity requirements, they were reproduced by a carefully standardized contact-print procedure and distributed to the research institutions that had ordered them.

Minkowski was in overall charge of this entire operation. He tested and adjusted the Schmidt telescope; set up the observing procedures; personally supervised Albert G. Wilson, Robert G. Harrington, and George 0. Abell, the observers who took nearly all the plates; and examined all the plates that passed their preliminary screening, made the final judgment as to whether or not they were acceptable, and constantly inspected the duplicate negatives and final prints and plates produced from them. His very high standards, coupled with his technical expertise and experience, made the resulting Sky Survey prints and plates an extremely high-quality body of research material. Every serious observatory and research astronomy department has a set, and they have been used for innumerable research investigations.

The survey was later extended to –45° declination, in one spectral region only, λλ 5400–7000, by John Whiteoak, using the Palomar 48-inch Schmidt. The far southern hemisphere is now being surveyed, in a very similar way, by the new European Southern Observatory Schmidt telescope at La Silla, Chile and the United Kingdom Schmidt telescope at Siding Springs, Australia.

When the 200-inch telescope was completed after World War II and went into operation with the 48-inch Schmidt at Palomar Observatory, Minkowski and his colleagues became staff members of Mount Wilson and Palomar Observatories, as the joint operation was named, and faculty members at Caltech. Ira S. Bowen, longtime Caltech laboratory spectroscopist and solver of the puzzle of the identification of the forbidden lines in gaseous nebulae, became director of the institution. Convinced of the advantages of Schmidt cameras for astronomical spectroscopy, he took personal charge of the high-dispersion coudé spectrograph of the 200-inch Hale telescope, but left Minkowski responsible for the fast, low-dispersion prime-focus spectrograph and the 48-inch Schmidt. Bowen had made the final choice of the basic optical parameters of the 48-inch, and as director he insisted that the Sky Survey be completed before the telescope was turned over to the research programs of individual staff members. Bowen and Minkowski had great respect for one another's optical and instrumental abilities, and they discussed new developments frequently.

Caltech started its own astronomy department, to which—in addition to Zwicky—Jesse Greenstein, Guido Münch, and I were the first three members appointed. Fred Hoyle was a frequent visitor. We had a regular, weekly Astronomy-Physics lunch at the Atheneum, the Caltech faculty club, at which Baade, Minkowski, and Armin Deutsch from the Mount Wilson offices were always faithful participants, usually along with a few others. Minkowski was always eager to hear of the latest developments in physics and astrophysics and happy to tell William Fowler, Richard Feynman, Matthew Sands, Leverett Davis, and the other physicists what he had been doing at the telescopes.

With the increased light-collecting power of the 200-inch, Minkowski was able to get better data on fainter supernovae, planetary nebulae, and other nebulous objects. But he soon found himself heavily involved in the problem of the optical identification of radio sources. Radio astronomy was born in the early observations of Karl Jansky and Grote Reber, but it came to vigorous life after World War II. Radio engineers and physicists such as E. G. Bowen, J. L. Pawsey, Bernard Lovell, Martin Ryle, John Bolton, R. Hanbury Brown, and Bernard Mills returned to academic and government positions in England and Australia from the wartime laboratories in which they had developed radar and other advanced detection, location, and identification systems. They had seen solar and celestial radio-frequency radiation by its interference effects, and resolved to study it to learn more about the universe. Although their first interferometers and reflectors gave only very rough angular coordinates of the individual radio sources (originally often called radio stars), the very bright source, Taurus A, was soon identified with the Crab nebula by Bolton and Gordon Stanley.

In late 1948 Bolton—who with Stanley and O. B. Slee had by then also identified Centaurus A and Virgo A with the optical galaxies NGC 5128 and M 87, respectively—wrote to several prominent optical astronomers to seek their help in making further identifications. He chose between Baade and Minkowski by flipping a coin. The luck of the toss decreed he should write to Minkowski; he did, and received a reply from Baade. Therefore Bolton addressed his next letter to Baade, and received a reply from Minkowski. They were both highly interested in the radio-source identification problem, and collaborated very closely in their investigations; Minkowski did all of the spectroscopic work and shared with Baade the taking of the direct exposures. As senior members of the Mount Wilson and Palomar Observatories staff, they were able to command large amounts of prime dark observing time with the two most powerful telescopes in existence, and they were willing to commit a sizeable fraction of their time to searching for the optical counterparts of radio sources.

Some of their first identifications were additional supernova remnants within our galaxy, such as Cassiopeia A and Puppis A; others were galaxies with strong broad emission lines, such as Cygnus A and Perseus A. These galaxies opened the fascinating hope of a whole new attack on the cosmological problem. Cygnus A, for instance, is a faint and insignificant optical object, but one of the strongest radio sources in the sky. Surely among the numerous weak radio sources there must be other galaxies physically similar to Cygnus A, faint only because of their great distances, greater than the distances of any galaxies then known. To recognize such objects was the task Baade and Minkowski set for themselves. The problem was always the insufficient accuracy of the radio positions. In their papers, in their correspondence, in their personal conversations with all the radio astronomers whom they met at conferences and who visited Pasadena, Baade and Minkowski constantly urged the necessity of improving the positional accuracy to the standard of optical astronomy—which the radio astronomers have by now essentially achieved, with probable errors of order one-tenth of a second of arc, rather than the several degrees of the early days.

By their pioneering identification work, Baade and Minkowski established that some strong radio sources are supernova remnants within our Galaxy and that many others are galaxies with strong emission lines in their spectra, sometimes distorted in form, sometimes heavily obscured by dust. They were less successful in their physical interpretation of the radio galaxies as galaxies in collision, an idea derived from Baade's earlier work with Lyman Spitzer on S0 galaxies. The spectra and forms of Cygnus A, Perseus A, and Centaurus A suggested that these objects were actual examples of colliding galaxies, but this interpretation has now generally gone by the board. Most astronomers and astrophysicists are now seeking the basic cause of the generation of magnetic fields and relativistic electrons that produce the observed nonthermal radio radiation in the galactic nuclei.

A still greater contribution to radio astronomy by Baade and Minkowski was the welcome and encouragement they extended to the early radio physicists. They passed on their optical knowledge, skills, and resources to the newcomers, helping them to become respected members of the astronomical community. Caltech's Owens Valley Radio Observatory was built as a result of Baade, Greenstein, and Minkowski urging that the Institute get into this important new field of research.

Baade's participation in the radio work ended when he retired from the Mount Wilson and Palomar Observatories' staff in 1958, at the age of sixty-five. After brief periods as a visiting professor at Harvard and at the Australian National Observatory in Canberra, he moved to Göttingen, where he died less than two years after his retirement, leaving unfinished many projects he had hoped to complete.

Minkowski continued to identify and obtain spectra of radio sources alone after Baade's retirement, but he in turn had to retire two years later, on June 30, 1960. A few months before his retirement, he used the 200-inch to take a spectrogram of the faint radio galaxy 3C 295, which had been identified by Bolton on a 48-inch Schmidt plate from an accurate radio position. On his spectrogram Minkowski identified the single emission line as [O II] λ3727, the only reasonable identification possible, and measured its redshift as $z = 0.46$, nearly half the velocity of light. This broke the observational barrier at about $z = 0.2$ for normal galaxies, which Humason had been struggling to surpass for nearly ten years. Minkowski's record redshift remained the largest known for a galaxy for over fifteen years, until it was topped by Hyron Spinrad, James Westphal, Jerome Kristian, and Allan Sandage with $z = 0.75$ for 3c 343.1 The first quasistellar radio sources, or quasars, were identified after Minkowski's retirement, again from very accurate radio positions. The riddle of their spectra was broken by Maarten Schmidt, who thus proved that they have very large redshifts. At the present writing, the quasar with the largest known redshift is OQ 172, with $z = 3.53$ or 91 percent of the velocity of light.

Minkowski also worked on another important problem, the mass-luminosity ratio in elliptical galaxies. Although in spiral galaxies it is possible to measure the rotational velocity as a function of distance from the center, and thus derive the mass distribution, in elliptical galaxies there are no H II regions or bright OB-star associations that can be observed spectroscopically, as there are in spirals. Minkowski realized that the only way to proceed was to obtain good, high-dispersion spectra of the nuclei of elliptical galaxies, measure the width of the absorption lines to get the velocity dispersion of the stars near the nuclei, and thus determine the central mass densities. The values of mass-to-luminosity ratios he derived in this way were the best available for many years, until they were recently supplanted by Sandra M. Faber, W. L. W. Sargent, and others using detectors much more sensitive than the photographic plates available to Minkowski.

After his retirement from Mount Wilson and Palomar Observatories, Minkowski spent the year 1960-1961 as a visit ing professor at the University of Wisconsin. He lectured on gaseous nebulae, supernovae, and radio sources and started C. R. O'Dell on a thesis on the evolution of planetary nebulae and their central stars, a thesis O'Dell completed under my supervision. Earlier, at Caltech, Minkowski had been an unofficial adviser for Abell's thesis on clusters of galaxies, for which I was also the official sponsor; I believe that these were the only two Ph.D. theses with which Minkowski was closely involved. After his year at Madison, Minkowski spent some months as a guest investigator in Australia, working with Bolton on radio-source identifications.

In 1961 Minkowski was appointed a research astronomer at the Berkeley Radio Astronomy Laboratory of the University of California. He and his wife moved their home to Berkeley, where they were close to both their children. There he continued writing up his own research results as well as several review articles and served as a constant source of encouragement and advice to faculty members, postdoctoral fellows, and graduate students. He collaborated on several projects, particularly on the properties of normal galaxies, with various Berkeley faculty members. In 1964 he took part in the Solvay Conference in Brussels on the structure and evolution of galaxies. In 1965 he formally retired a second time at the mandatory age of seventy, but continued to come to the campus regularly and discuss research. The last paper he wrote, fittingly enough, was a report on the accomplishments of the Palomar 48-inch Schmidt telescope, which he presented at a conference in Hamburg devoted to planning the European and United Kingdom southern-hemisphere Schmidt surveys.

Minkowski was elected to the National Academy of Sciences in 1957 and was awarded the Catherine Bruce Gold Medal of the Astronomical Society of the Pacific in 1961 for his distinguished services to astronomy. In 1968, at the cen tennial of the University of California, Minkowski received an honorary doctorate at the Berkeley commencement exercises for his outstanding astronomical achievements.

Personally, Minkowski was a large, friendly, bearlike person. He was much stronger than most astronomers, and he could always get an extra turn out of any screw, clamp, or guiding eyepiece adjustment. The designers and instrument makers at the Mount Wilson shops used to joke that their products had to be not only "astronomer-proof" but "Minkowski-proof."

His office, at the Mount Wilson headquarters on Santa Barbara Street in Pasadena, was famous for being the most cluttered of any in the building, no mean distinction. Over the years he had accumulated tremendous quantities of folders of measurements, calculations, drafts of papers, reprints, preliminary results, and the like. His standard procedure was to keep this material piled on top of his desk where he could get at it. Photographic plates of the objects he was studying, in paper envelopes, were immersed in these piles, which were heaped up to the angle of repose. If a visitor came into his office to discuss some planetary nebula with him, Minkowski would begin to talk about it; then dive unerringly into the right place in the right pile to come up with a plate of its spectrum; open the envelope; pull out the tiny glass plate that should have been, but often was not, mounted on a microscope slide; blow the cigarette ashes off of it; sometimes stick it together with scotch tape if it had fallen off the desk and broken in some previous conference; look at it through an eyepiece and describe it while a lighted cigarette dangled from his mouth a few inches from the plate; hand the visitor the plate and an eyepiece while he continued expounding; recapture the plate; wipe it off with the side of his hand; put it back in the envelope; and put the envelope back in the exact same place in the exact same pile. There was clearly a higher system to the mess, which he alone understood.

Minkowski was a very good observer. He was experienced, skilled, understood the telescope and the spectroscopes, what they could do and could not do, and what he himself could do and could not do. He was a curious mixture of patience and impatience. Setting on a faint planetary nebula or supernova remnant, he was impatient and eager to begin. For some years the night assistants at Palomar had a tape, surreptitiously recorded, of Minkowski talking to himself in the prime-focus cage of the 200-inch as he made a setting: "Where is that thing? . . . I think that's it over there. [Sound of slow motion motor] Damn! Wrong button [Slow motion again]. . . . There it is. . . . Now where's that little double to the left? . . . No that's not it. . . . [To the night assistant] Try a little west. . . . Stop! [To himself] Here it comes [Slow motion]. . . . Yes I think that's it. . . . Pull it down a little [Slow motion]. . . . Ah! Too Much! [Slow motion]. . . . Now I've got it [Sound of dark slide opening—then, to the night assistant] Start the exposure! I'll take three hours on this one. You can rest a while." This is followed by a long sigh, then he began to hum the "Ode to Joy" from Beethoven's Ninth Symphony, and the tape mercifully ends.

He was very patient in the guiding, doing a careful job all during the tiresome long exposures he took, but impatient to see the results. He would hurry into the darkroom with a recently exposed plate, develop it, give it quick rinse in water, plunge it into the hypo fixing solution, count thirty seconds, light a cigarette, and have the plate out of the hypo and be looking at it with his eyepiece before it had cleared.

I well remember when he showed me how to use the nebular spectrograph at the Newtonian focus of the 100-inch telescope one hot summer night in 1954. Minkowski, in his shirt sleeves, was wearing a fur-lined cap of the type popularized by the Chinese infantrymen in Korea a few winters before. Sweat was dripping down his face as he demonstrated how to raise the heavy spectroscope to see the star field, and then lower the instrument again to center the object and start the exposure. "Why do you wear that hat?", I asked him. "Makes a good crash helmet," he muttered, and I laughed. I understood what he meant the next month when, observing by myself, I hurried down the ladder from the platform with a just-exposed plate to develop, dashed across the observing floor toward the stairs to the darkroom, and slammed my bare head into the black, steel, absolutely immovable bottom end of the 100-inch. My head stopped right there, but my feet kept on going, and I was knocked out for a second. As I came to, lying flat on my back, feeling for the telescope above me, I resolved to pay a little more attention to what Rudolph was trying to teach me. With me, as with everyone else who approached him, he was extremely friendly, very helpful, and always happy to talk astronomy.

Minkowski worked very hard and single-mindedly on research. I once told Luise, his wife, that when I was a student, some years before, I had seen a magazine article about the staff members who were to work at Palomar and what their hobbies were. In a picture in that article Rudolph had been shown at his hobby, playing the piano, and I asked her if he still played. (Humason had been shown in the same article, washing his car.) She replied in her emphatic way "Oh, I remember that article. That reporter didn't understand anything! He wanted hobbies, but I told him none of those men had any hobby but astronomy! He wouldn't believe it. Rudolph used to play the piano, ages ago when he was young, but he hasn't touched it for years." How right she was. Yet he always enjoyed the outdoors and managed to find time for camping and fishing expeditions with his family in the High Sierras, just as in earlier years he had made time for climbing expeditions in the Alps with his fellow students.

Minkowski and Baade remained close friends all the years they were together at Pasadena. Baade, two years older, was far more intense, mercurial, and flamboyant. His conversation and lectures were peppered with stories, always apt, always interesting, sometimes true. He once correctly described his own voice as "sounding like a barking dog." Minkowski was quieter, more phlegmatiic, but never at a loss for words. Often the two of them were at Mount Wilson together, on the 60-inch and 100-inch telescopes, or later at Palomar, on the 200-inch and the 48-inch Schmidt. Then they would walk together slowly up to the domes in the evening and back to the monastery in the morning, discussing astronomy loudly, in voices that carried all over the mountaintops. As they argued vehemently in a queer mixture of German and English, it sounded to the uninitiated as if a battle were about to begin, but they always listened to one another and remained fast friends.

Minkowski and his wife were completely Americanized, in a way Baade and his wife never were. Rudolph and Luise and their children became naturalized United States citizens as soon as they could, in 1940. During World War II, Minkowski worked as a civilian scientist on one of the Office of Scientific Research and Development projects at Caltech, while Luise went to night school to learn to be a draftsman, which enabled her to get a job at the Lockheed plant in Burbank. They never thought of retiring anywhere but in America. Throughout their lives they were both very hospitable people; there were few astronomical visitors to Pasadena whom they did not entertain.

Even after his retirement from his second job at the age of seventy, Minkowski remained in good health until, in the 1970s, he began to suffer from kidney disease. His condition gradually worsened, although he was up and about until the end. In his later years he had become a television football addict, and the last game he saw, and enjoyed mightily, was the 1976 Rose Bowl in which UCLA roundly defeated Ohio State. He died in Berkeley of a sudden stroke on January 4, 1976. The Astronomical Society of the Pacific, under the leadership of Harold F. Weaver, organized a memorial symposium in Minkowski's honor, held at their annual meeting in June 1977 at Berkeley, in which reviews were presented of the various subfields of astronomy in which he had specialized. His wife and daughter attended part of the symposium, and Luise particularly enjoyed a luncheon in connection with it, at which she renewed her acquaintance with many old friends. Although she was in good health then, she was soon attacked by a very rapidly developing case of cancer and died on March 11, 1978. Their children, Eva Minkowski Thomas and Herman O. Minkowski, together with seven grandchildren, survive them in California.

In summary, Rudolph Minkowski was an outstanding observational astronomer and astrophysicist. His life was devoted to science. His work added greatly to our knowledge of planetary nebulae, supernovae and their remnants, radio sources, and galaxies; present research in these fields is along lines shaped in no small measure by his results.

This memoir is based largely on the written record of Rudolph Minkowski's research, published in the papers listed in his bibliography, and on my personal conversations with him and with his wife Luise over the years since 1953, when I went to Caltech as a young faculty member and began working closely with him. I have also been able to use clippings, articles, news releases, letters, documents, and reminiscences provided through the kindness of his children, Eva M. Thomas and Herman O. Minkowski, and of many of his friends, colleagues, and fellow scientists. I am deeply indebted to all of them for their help.

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Leonard Schiff

Leonard Isaac Schiff

March 29, 1915-January 19, 1971

by F. Bloch

Leonard Schiff was born in Fall River, Massachusetts. His father, Edward, descended from a Lithuanian family of rabbinical scholars, had come to the United States as a young boy. An early ancestor was an apothecary who took the name "Schiff," the old German word for a vessel used in his trade. His mother Mathilda (nee Brodsky), also of Lithuanian descent, was born in Brooklyn. She was an accomplished pianist and composer; her two sons and three daughters all began to receive musical education when they were still very young. She began to teach Leonard to play the piano when he was four years old, but two years later he took up the clarinet, an instrument that he loved and came to master remarkably well.

Leonard's school years were spent in Brooklyn after his family had moved there. This was a time of financial difficulties for Edward Schiff, who helped to alleviate them by freelance writing. Leonard was a precocious child; therefore he was quickly advanced in school in order to remain sufficiently occupied with his classwork. Although by no means one-sided, his special interest and talent in mathematics soon became evident; he later belonged to a small group of volunteers who received additional instruction from their mathematics teacher after school hours. The knowledge thus

(The asterisk denotes publications that do not report original research.)

acquired brought Leonard so far that he was well versed in calculus when he graduated from James Madison High School in June 1929.

Another family move brought Leonard to Columbus, where he entered Ohio State University at the age of fourteen. It was his desire to come closer to reality than he felt he could reach through pure mathematics that brought Leonard Schiff to the study of physics. The subject was taught at the School of Engineering, and he received the bachelor's degree (B.E., physics) in 1933. His first piece of research was done in the course of the next two years; it arose from close contact with L. H. Thomas, an outstanding member of the faculty and originator of the well-known Thomas factor in atomic physics. Leonard's investigation concerned the quantum theory of metallic reflection and analyzed the effect of surface properties. Published with Thomas as coauthor, it already shows the influence of Schiff's method, seen in much of his later work, of starting with fundamentals and proceeding systematically from there to the derivation of new results. He once told his younger brother Dan, also a physicist: "Physics is really simple—you only have to know the few basic facts whereupon everything else just follows."

After receiving the degree of master of science, Leonard left Columbus in 1935 to continue his graduate studies at the Massachusetts Institute of Technology. Although much younger than his classmates, he surpassed most of them in training and knowledge. He and some fellow students, including Marvin Chodorow, who became a lifelong friend, soon formed a group of close companions who gathered for hikes or similar occasions to talk about physics and other matters of shared interest. Clearly a hard worker, Leonard appeared self-assured while totally free of arrogance; and he was also blessed with a fine sense of humor.

From the beginning, Schiff enrolled in the advanced courses offered at MIT, and it did not take long before he was also engaged in research. A course on statistical problems that he took from Robley D. Evans led to their collaboration on the statistics of counters and to a joint publication in 1936. With Philip M. Morse and J. D. Fisk as coauthors, an earlier publication of the same year concerned an altogether different subject, the collision of protons and neutrons. It is significant because it marks the beginning of Schiff's association with Phil Morse, to whom he felt drawn by personality as well as by their shared inclination toward mathematical physics, meant in the more literal sense than in the old usage for the entire field of theoretical physics. An emphasis on mathematics can be noticed here in that the potential is chosen to allow a rigorous analytical solution of the Schrödinger equation. The work is also significant because it represents the start of Leonard's occupation with problems of collision, a phenomenon which he later investigated in its many different aspects so that it runs as a strong thread through his life's work. In immediate continuation, he included the deuteron as collision partner and treated the related capture and binding process of neutrons, which yielded four more papers in 1937 and, by further extension, led in June of that year to his Ph.D. thesis under the supervision of Morse, entitled "Theory of the Collision of Light Elements."

He worked during the rest of the summer as a research physicist for the General Electric Company, whereupon a fellowship from the National Research Council for 1937 and 1938 enabled him to join the group of young theorists at the University of California in Berkeley and the California Institute of Technology in Pasadena. It was the presence of J. R. Oppenheimer, then a joint professor at these institutions, that had brought them together and greatly influenced their activities. Oppenheimer's quick grasp in the course of a discussion was most impressive, but the ensuing comments were often quite mystifying. One can see how Leonard's generally positive attitude enabled him to reconcile this fact with his deep desire for clarity by quoting from a memorial talk that he gave in Berkeley after the death of Oppenheimer: "I think he enjoyed talking in riddles; certainly there was an enigmatic quality to the choice of his words. Students and colleagues spent much time wondering about just what he had meant and often ended up with a much deeper understanding of the subject than they would have attained otherwise."

An appointment as a research assistant after the expiration of Schiff's fellowship extended this first period in California by two more years. The intimate contact with other highly gifted members in the group, among them W. E. Lamb and R. Serber, contributed no less than Oppenheimer's stimulating influence to his productivity during that time. The collaboration with Lamb led to their joint paper on the electromagnetic properties of nuclei arising from the meson exchange between their constituents. This started his interest in the role of mesons, which he continued to pursue through a series of investigations in the 1950's. The scope of his activities was further enlarged, partly through work with H. Snyder and others, to encompass a variety of subjects as different as radiative capture of electrons, liquid helium, and the quadratic Zeeman effect, thus demonstrating anew Schiff's extraordinary versatility.

Those three years, spent mostly in Berkeley, brought him enrichments not only through his scientific work but also through new friendships, particularly with Bob and Charlotte Serber, Dale and Nelle Carson, Art Kipp, and Bill Steinhoff. He greatly enjoyed the evenings of chamber music in the home of Martin Kamen where he sometimes joined the fine players with his clarinet. Last and most important, it was in Berkeley that he met Frances Ballard, then a student of history, and found in her the ideal companion of his future life. They married in 1941 after Leonard's move to Philadelphia; complementing each other in many ways, they had in the course of time a daughter, Ellen, and a son, Lee.

The academic career of Leonard Schiff began in 1940 with an appointment as instructor at the University of Pennsylvania, followed at intervals of two years by promotion to assistant and associate professor. His exceptional gift for teaching immediately became evident; he impressed the students so much by his perfect clarity and his pedagogic skill that they regarded him as the best teacher at the University. Leonard was not the man, however, to earn such a reputation at the expense of his research. Continuing his interest in the properties of liquid helium, brought from Berkeley, he was able in his first year as instructor to publish several papers on the subject, aimed particularly at a better understanding of the phase transition to the superfluid state. He still found time to deal in two more papers with the performance of the electron microscope before his publications were brought nearly to a standstill until the end of the war.

Like that of most American physicists, Schiffs research during this interval became directed towards the war effort and the announcement of results was restricted by security regulations. In contrast to the specialization common at this time, he sometimes served simultaneously on many different projects, several of them arranged through Gaylord Harnwell, the chairman of the Physics Department at Pennsylvania. The first was initiated upon a request to develop a device for measuring the purity of helium in blimps. Leonard provided the analysis of the data, but he also assisted his coworker Robert Hofstadter, who had entered the University six months before him, in ably performing some of the glassblowing required for the apparatus. Neither of them could anticipate that they were later to be colleagues at Stanford, where the friendship, cemented at that time, would lead to a most fruitful interplay of their common concern about electron scattering at high energies.

Schiff later participated in a number of projects that dealt with various aspects of submarine warfare. In close personal contact with the successive directors—F. Seitz, A. W. Lawson, W. E. Stephens, and P. H. Miller—he was associated with a group formed to study the operation of the crystal detectors used in radar systems. Research in this group led Walter Meyerhof, his former student and a future colleague at Stanford, to the discovery of surface states and to a Ph.D. thesis on the subject. It is most remarkable that all the activities described above did not prevent Leonard from carrying on some teaching and from serving in Harnwell's absence as acting chairman of the Physics Department from the summer of 1942 to April 1945.

At that time he was granted a leave of absence to join the atomic bomb laboratory at Los Alamos, at Oppenheimer's request, and he was among those who witnessed in the Trinity Test at Alamogordo the first explosion of an atom bomb. Deeply impressed by this event and the subsequent destruction of Hiroshima and Nagasaki, he wrote a letter to the *Forum of the Review of Scientific Instruments* entitled "Atomic Energy and Physicists" shortly before leaving Los Alamos in January 1946. It warns with remarkable foresight that other nations would be able to make atomic bombs and that they could be made much more powerful than those used against Japan. The physicists were asked to use their special position not only to support international control of nuclear weapons, but also to make the public aware of the great benefits that atomic energy could bestow upon mankind.

After his return to Philadelphia, he again carried out the normal teaching and research activities during his remaining year-and-a-half at the University of Pennsylvania. He further used the partial removal of government restrictions to promptly publish, with his collaborators, some of the results previously reported only in classified documents.

Leonard Schiff began the last and longest period in his academic life when he joined the Physics Department of Stanford University in the fall of 1947. Here, as elsewhere in the United States, the ways of physics had been greatly influenced by developments during the war, both in opening new avenues of research and through the generous support received from government agencies. At Stanford the principal new areas resulted from the vast improvements of radio and microwave techniques in the development of radar, now channelled into peace-time applications, the former used for the study of nuclear magnetism, the latter for the acceleration of electrons. Both developments were rooted in work done at Stanford before the war in a small department and with very modest means; now they were seen to have a much wider scope that pointed towards an extended period of promising future research.

While a great increase in staff did not seem to be indicated, it was recognized that the new era called for additional qualified members of the department. Edward Ginzton and his wife Artemas, a cousin of Frances Schiff, had known Leonard intimately for a long time. Upon Ginzton's suggestion, it was quickly agreed, both for reasons of personality and in view of Schiffs excellent record, that a position should be offered to him; it boded well for his future in the department that he accepted the offer of an associate professorship without much hesitation. To do justice to his great influence during nearly a quarter of a century at Stanford, the role of Schiff has to be considered in several different, yet not unconnected, aspects.

To begin with his part in affairs of the university, it was with the promotion to a full professorship, just a year after his arrival, that he succeeded Paul Kirkpatrick as executive head of the Physics Department. The following eighteen years, during which he remained in this position, were to give ample proof that a more fortunate choice could hardly have been made. Through strict adherence to democratic principles, Leonard gained the complete confidence of his colleagues and became their ideal spokesman in dealing with the administration of the University. The strengthening of the department by a number of excellent appointments and the move from inadequate quarters to a large new building with connected structures for administrative offices and auditoria were but two of the significant changes during the period of his chairmanship. Another major development resulted from the evergrowing scope and size of the department's research activities. While gratifying in many respects, the expected considerable expansion of certain activities caused problems since it called for special arrangements to allow their independent operation. This gave rise to the installation of applied physics as a separate department of the University and to the creation of the Stanford Linear Accelerator Center as a national laboratory; both events were accompanied by the transfer of some members of the department who became the core of a greatly enlarged staff. There followed a prolonged exchange of opinions about the relation of these new entities to the physics department with the strong involvement of the chairman until the issues had been sufficiently clarified and agreements were reached that worked to the greatest mutual benefit.

When Schiff decided in 1966 to give up the position as head of the department, he could look back with satisfaction to the achievements reached over his many years of unselfish devotion to the task that had been entrusted to him. Far from ending his services to the University, however, he continued for another three years as chairman of the Advisory Board, the highest position for the faculty at Stanford, and was elected as the first chairman of the newly formed senate during the University's turbulent year of 1968. In these as well as in a number of other functions, he never hesitated to give freely of his time and greatly contributed by his wisdom and judgment to the general welfare of the institution.

Coming now to his particular concern with education, Schiff realized the great importance of introductory courses for the development of a student and the experience required for their being taught well. He therefore saw to it that these classes were assigned to senior members of the faculty, setting a high standard by his own participation. The quality of his teaching came not only from an innate pedagogic ability but also from his having given a great deal of thought to the aims to be realized. He devoted much of an article about science in general education to clarifying the difference between pure science and technology and made a strong case for his conviction that the cultural rather than the utilitarian aspect of science should be the basis of scientific education. In a talk entitled "The Education of a Scientist," he warned against premature specialization and concentration on techniques—as advocated in the days of the Sputnik—arguing that a student of science will be best prepared for his future if he first acquires a broad knowledge. The talk ended with the following remark about the prospects of a scientist: "If he happens to become a university professor, perhaps his greatest ambition will be to develop a research student who will some day make a greater contribution to science than was within his own power"—a touching revelation of Schiffs personal feelings. In 1966, he received the Oersted Medal of the American Association of Physics Teachers for his "notable contribution to the teaching of physics" and Stanford's annual Dinkelspiel Award "for outstanding service to undergraduate education."

Schiff's widest influence as a teacher and scholar, however, has been achieved through his book, *Quantum Mechanics.* Although it deals with the very basis of modern physics, discovered twenty-five years earlier, there existed nothing comparable to this text before the first edition in 1949, and nothing like this volume was to appear for many years to come. Translated into many languages, among them Russian and Japanese, it is found today on most scientific bookshelves all over the world and has been instrumental in bringing up a whole generation of physicists. The second edition appeared in 1955 and the third in 1968, enriched each time to keep up with recent developments.

It is research, however, that was always of primary importance to Leonard, whose well-organized system of working enabled him to remain highly productive in the pursuit of his investigations without neglecting other activities. The variety of topics he chose again reflects a wide range of interests, but two major groups stand out: the recurrent occupation with the theory of collisions and with general relativity.

The collision and scattering of particles was mentioned before to be of particular significance in the work of Schiff. His attention to the subject received a fresh stimulus through the development of the linear electron accelerator at Stanford. As early as 1949 he discussed in an extended report the type of information that could be obtained with the new accelerators, and he emphasized the utility of electron scattering as a probe of nuclear and nucleon structure. The important results of the experiments carried out by Hofstadter and his collaborators were to amply fulfill his expectations. In close contact with the progress of their work during the following years, he significantly contributed to the analysis of the data and investigated related problems concerning the treatment of scattering processes. He reviewed the research activities in high energy physics that bear on nuclear struc ture in 1955, and once more in 1968, under the title "Low Energy Physics from a High Energy Standpoint," aptly chosen to characterize a field in which he can be truly said to have been one of the founders.

Except for the application of Mach's principle to a problem of rotating charges, suggested in 1939 by Oppenheimer, Leonard's many contributions to general relativity were made almost entirely during the last decade of his life. The first of them was motivated by certain arguments in favor of the idea that the gravitational mass of antimatter might be negative. Schiff showed in a careful analysis that this is incompatible with the virtual presence of positrons in an atom, demanded by the formalism of quantum electrodynamics. A corresponding negative contribution to the gravitational mass would result in so large a difference from the inertial mass that it is ruled out by the high accuracy with which the equality of these two masses has been experimentally established.

The decision to base this conclusion on empirical evidence rather than to simply state it as a necessary consequence of general relativity characterizes the principal concern of Schiffs later work in that field. While fully aware of the depth and internal consistency of Einstein's theory, he felt that it was supported by a somewhat slender body of observations which, furthermore, had yielded most of the relevant data only within a considerable margin of error. The equality of inertial and gravitational mass being the most accurately confirmed basis of general relativity, he showed that it imposed powerful constraints on the coupling of gravitation to systems of interacting particles. In the same context he argued that this equality might suffice to establish Einstein's much more far-reaching principle of equivalence, the "Schiff conjecture," which has stimulated a great amount of research in that area.

Another important contribution to general relativity was his proposal of a new test, with results to be obtained by measuring the rate at which the axis of an orbiting gyroscope would change its direction with respect to the fixed stars. He calculated that for an orbit at 500 miles altitude it would amount to about seven seconds of arc per year, caused mainly by orbital motion, but with a correction of the order of one percent due to the Lense-Thirring effect of the earth's rotation upon the gravitational field. The development of the apparatus for this delicate measurement by Fairbank and his collaborators at Stanford is still in progress, and the performance of the test is awaited with great interest.

A close connection between experimental undertakings and Schiff's contributions to the theory manifested itself in many of his other investigations as well. As an example, he discussed the measurability of nuclear electric dipole moments when the possibility of such a measurement in Helium-3 was brought to his attention. Another example is his treatment of the gravitation-induced electric field near a metal, motivated by an experiment to observe the free fall of electrons in a hollow metallic cylinder. His willingness and ability to be of help to the experimentalists was greatly appreciated while at the same time providing him with a good deal of personal pleasure. "I like to solve problems" was one of his sayings, and it is not surprising that he had a special admiration for Rayleigh, a man whose work encompassed nearly all the physics of his time and whose biography he planned to write after retirement.

Beyond his prolific activities at Stanford, Schiff showed a great concern through his engagement in numerous problems concerning science at large. As a member of the council of the American Physical Society, in early 1956 he sent a memorandum to the director of the American Institute of Physics in which he advocated the establishment of a journal, tentatively to be named the *Journal of Mathematical Physics.* He reasoned that the ever increasing bulk of the *Physical Review* called for one or more new journals and delineated the scope of this particular one "to include any paper that applies established methods of mathematics or theoretical physics to a problem of physical interest, where the novelty lies in the mathematical procedure rather than in the physical understanding which is attained." His proposal initiated the actions that led to the start of the *Journal of Mathematical Physics* in 1960. Because of his prominent part in the preceding deliberations, he was asked to become the chairman of the editorial board; he declined but served as associate editor during the first two years of *the Journal.*

His services were further sought, and Schiff freely consented for extended periods to join the editorial staffs of the *Physical Review,* the *Reviews of Modern Physics,* and several other journals. He was a fellow or officer in many learned societies, including the American Academy of Arts and Sciences and the National Academy of Sciences, where he was chairman of the Physics Section at the time of his death.

The extraordinary variety of Leonard's professional pursuits, indicated in the preceding account, and his careful attention to each of them might invoke the image of a man under constant severe pressure. Yet such was the wealth of his personality that he never lost his quiet and considerate way with others, nor was he forced to sacrifice the enjoyment of his family, the company of his friends, or his love of music and nature to devote himself to his equally beloved science. There was much that Leonard Schiff still had to give and wanted to give when a heart failure brought his life to a sudden end.

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Adolph Hans Schultz

November 14, 1891-May 26, 1976

by T. Dale Stewart

Residence in three successive countries—Germany, Switzerland, the United States, and then again Switzerland—serves to divide Adolph Schultz's life span of eighty-five years into four segments: one, a German period (from his birth on November 14, 1891 to ca. 1897); two, a first Swiss period (from ca. 1897 to 1916); three, an American period (from 1916 to 1951); and four, a second Swiss period (from 1951 to his death on May 26, 1976). The American period was not only the longest, but also the most scientifically productive; it comprised the peak years, between the ages of twenty-five and sixty, of his career.

I. GERMANY

Of the German period of Schultz's life few facts are available. He was the only son among four children born to Julius and Sophie (Frick) Schultz in Stuttgart. When he was about six years old his German father died, and his mother, a Swiss by birth, took the four children to Zurich. Some twenty years later he stated in the curriculum vitae appended to his doctoral dissertation: *"Ich . . . besuchte Schulen in Deutschland und zum grössern Teil in Ziirich und bestand im September 1910 die eidgenössische Maturitätspriufung."*[1]

Asterisk denotes articles in which the author's first name is spelled "Adolf."

[1] "Anthropologische Untersuchungen an der Schädebasis," *Archive für Anthropologie,* 16 (1917): 104.

II. SWITZERLAND

Schultz's first Swiss period saw him through not only most of his preparation for college, but his undergraduate and graduate training as well. As an undergraduate he spent three semesters at the University of Zurich and two at the University of Bern. In Bern he served on the side as a visiting assistant lecturer in zoology in Professor J. U. Duerst's *Zootechnisches Institut.* Then in April 1913, following his return to Zurich, he matriculated in the doctoral program at the University there under the supervision of Professor O. Schlaginhaufen and seven semesters later received his Ph.D. in anthropology.

For his dissertation Schultz undertook an anthropological investigation of the base of the human skull. Although the Anthropological Institute in Zurich had series of skulls from a number of racial groups, with the exception of Ancient Egyptians and recent Swiss (Daniser), none had a sufficient representation for Schultz's purpose. In order to bring all of his skull samples up to adequate size, he visited several institutions in Germany. In Professor W. Waldeyer's anatomy department in Berlin he obtained the skulls of some West African Negroes and Chinese; in Professor G. Schwalbe's anatomy department in Strassburg, Greenland Eskimos; in Professor A. Jacobi's department in the Royal Museum for Zoology-Anthropology-Ethnology in Dresden, Australians and Chinese; and in Professor J. Ranke's department in the Anthropological Institute in Munich, Australians and Chinese. In all he studied 394 skulls from six racial groups.

Schultz elected to take the majority of his skull measurements with the skull oriented in one or the other of two unconventional horizontals: glabella-basion and glabellainion. Otherwise he took a selection of conventional measurements not requiring reference to a horizontal. To explain the unconventional measurements, he supplied neatly drawn and lettered diagrams. He also drew by hand the rest of the illustrations. Notable, too, is the fact that the measurements, listed individually, are summarized statistically in the form of means, standard deviations, and coefficients of variation, all with their probable errors. I mention all this because later in his career his publications generally became richer in pen-and-ink renderings (including tables) and poorer in statistical analyses.

Three papers appeared in print ahead of the dissertation, two in 1915 and one in 1916. The first two refer to some of the same German skull collections from which he obtained data for the dissertation. This suggests that in advance of the visit(s) to the German institutions Schultz made plans to collect data needed for the investigation of three different problems. Here may be the beginning of the program of data collection for which he became famous. From this time on his examinations of specimens were so well thought out and so complete that, before many years would pass, he could dip into his data bank for much of what he needed to deal with a new problem or to summarize the morphological characteristics of a particular primate species.

These four publications also reveal a beginning shift in interest from traditional physical anthropology, which deals mostly with man, the highest primate, to a broader type of study (now called primatology), which deals with all the primates. The first of these publications (1915) makes no mention of nonhuman primates, the second (1915) makes slight reference to them, and the third (1916) gives them extensive coverage. The dissertation, which was published fourth (1917), was planned, of course, before this shift in interest had time to develop.

Reminiscing about this period of his life at the Third International Primatological Congress in Zurich in 1970, Schultz said:

> In my student years of 1910 to 1916 at the University of Zurich interest in primates happened to be unusually well represented in the Anatomical

> Institute under the direction of Ruge and in the Anthropological Institute, which had been founded by Martin, who was succeeded by Schlaginhaufen. Together these departments . . . had assembled very extensive collections of entire bodies and skeletons of nonhuman primates largely through the cooperation of the Swiss Büttikofer, the director of the Rotterdam zoo and distinguished student of Indonesian primates. This material served for great many important papers on primate anatomy by Ruge himself, his staff and his graduate students . . . At the same time the Zurich collections had formed the basis for such well-known primatological monographs from the anthropology department, as Mollison's pioneering report on body proportions, Schlaginhaufen's study of dermatoglyphics and Oppenheim's comparative data on cranial variability, for all of which unusually large series of specimens had been available. Last not least, in 1914 there appeared Martin's great *Lehrbuch der Anthropologie,* in which primates were dealt with in every chapter, confirming the close alliance between physical anthropology and primatology. . . .
>
> It is hardly surprising that as a young student of anthropology in the midst of so much primatological interest I soon came to feel that the study of nonhuman primates was really more fascinating and rewarding than that of mere man, whose morphology had already become known to what seemed to me then down to the last details."[2]

III. TO AMERICA

The first Swiss period of Schultz's life ended and his American period began when he came to the United States in the fall of 1916. The circumstances leading to this move are explained by Florence R. Sabin in *Franklin Paine Mall; the Story of a Mind* (Baltimore: Johns Hopkins University Press, 1934). One of the projects that Mall had in mind in 1913 for the new Department of Embryology, which he had induced the Carnegie Institution of Washington to establish at the Johns Hopkins Medical School in Baltimore, was an anthropometric record of the Department's collection of human embryos. Continuing the account in Sabin's words:

[2] *Folia Primatologica, 26* (1976): 6-7.

> Mall did not go abroad in 1913 [as was his custom] but asked me to consult for him the anthropologists in Germany, Switzerland and France and explain his problem of securing someone to measure human embryos with an adequate technique. As a result Dr. Michael Reicher was recruited from the department of Professor Schlagenhaufen in Zurich. He came to Baltimore and started the work, but when the war broke out he was obliged to return to Europe and Dr. Adolph Schultz, also from Schlagenhaufen's laboratory, was appointed [to continue with the work] (pp. 304-5).

By the time Reicher left Baltimore the number of his measured specimens had reached 385.[3] Although he hoped to return to Baltimore after the war and for this reason left his data behind, Schultz continued the work, and by the time he published on the subject in 1922 and 1923, he had extended the coverage to 623 specimens. Not until 1929, however, did Schultz get around to publishing the details of the technique he used in measuring the fetuses.

Two actions by Schultz during this period indicate how well he was adjusting to life in his adopted country: in 1924 he married, and in 1934 he became a naturalized American citizen. Travis Bader, who became his wife and ultimately was to survive him briefly, was from Virginia. I once visited them at their vacation retreat, an old family house in McGaheysville, located in the Shenadoah Valley some 75 miles in a direct line southwest of Baltimore.

While Schultz was working on the fetuses, he was also gathering data of other sorts, such as information concerning the prenatal sex ratio and the development of the external nose. The second subject led in 1919 to a contribution to the Carnegie's publication series: it represented the first of his seven *Contributions to Embryology* between 1919 and 1949.

Schultz's bibliography shows that by 1921 he was also studying primate specimens other than human. One of his papers that year reports the occurrence of a sternal gland in

[3] *Carnegie Institution of Washington, Yearbook,* 13 (1914): 105, 109.

an orang, and another describes fetuses of the Guiana howling monkey. Thereafter papers of this sort gradually increased in frequency; in other words, his shift in interest from physical anthropology to primatology, already evident before he left Zurich, was continuing and expanding in Baltimore.

This shift took another form in 1923 when Schultz participated in the first of four primate collecting trips to Central America. On the first trip, which had as its destination eastern Nicaragua, he was accompanied by O. O. Heard. George Wislocki and F. F. Snider joined them in 1924 on the second trip to the same area, generally described as the middle course of the Princapolka River and a tributary thereof, the Yao-ya River. The third and fourth trips, in 1929 and 1932, were organized by Herbert C. Clark of the Gorgas Memorial Institute for Tropical Medicine and centered on Chiriqui in western Panama. Originally designed primarily to acquire embryos and fetuses, the success otherwise of all these trips may be judged from the number of mature skulls alone collected: a total of 379 from among three species (howlers, capuchins, and spiders). A by-product of the second trip was an anthropological study of twenty-five and twelve adult Indian men of the Rama and Sumu tribes, respectively.

The first trip to Nicaragua was financed by Schultz personally, the second by the Carnegie and the Johns Hopkins Medical School. The participation of Johns Hopkins suggests that the school was already interested in having Schultz join its staff. In 1925 he accepted the position of associate professor of physical anthropology in the Department of Anatomy, the first such position in any American medical school.

To fill the vacancy created by Schultz's departure, G. L. Streeter, who had succeeded Mall as director of the Carnegie's Laboratory of Embryology, brought in C. G. Hartman from the University of Texas. This was a happy arrangement for Schultz, because Hartman at once set about establishing a colony of rhesus macaques on the top floor of the Carnegie building next door to the anatomy building, and he invited Schultz to maintain the colony's growth records. Schultz was also offered the remains of any members of the colony that died. In turn he generously shared these remains with his anatomical colleagues.

Out of this collaborative effort grew the precedent-setting book, *The Anatomy of the Rhesus Monkey,* edited by Hartman and Straus (1933). The chapter therein by Schultz, "Growth and Development," contains his observations and measurements of more than twenty animals born in the Hartman colony.

Between 1927 and 1938 Schultz had a small primate colony of his own populated by six chimpanzees (counting offspring) and an orang. These animals were kept in improvised quarters in a former stable behind the anatomy building. As a medical student at Hopkins in this period, I remember well the vocal and mechanical din created by these caged animals. The colony came to an end when the strength of the largest male chimpanzee—named "Dayton" by Schultz after the antievolution trial in Dayton, Tennessee—made it impossible to keep him confined to quarters.

Besides observing the living nonhuman primates around him, Schultz was always seeking the remains of those dying in captivity. Animal dealers, directors of zoos, and owners of circuses responded generously, but their shipments of dead animals occasionally led to amusing incidents. For example, there is the tale of the zealous prohibition agents in Washington's Union Station, who, after apprehending a zoo attendant bound for Baltimore, were abashed to find that the bag he was carrying, when opened in the midst of a crowd, contained a dead monkey and not the suspected liquid contraband. Other tales concern phone calls to Schultz at incon venient hours from irate clerks in the office of the express company demanding that he come at once and claim stinking packages. The odor was so bad sometimes, it is said, that he was forced to expose and examine these specimens on the fire escape of the anatomy building.

Of course, not all of the shipments were in such wretched condition. Among the most notable acquisitions were the huge gorillas "Congo" and "Gargantua." The latter gained for Schultz considerable publicity because *Life* magazine (December 5, 1949) published a large picture of him, caliper in hand, bending over the corpse stretched out on an embalming table.

Given a choice, Schultz preferred animals shot in the wild to animals that had died in capitivity. This being the case, he was quick to accept an invitation from Harold Coolidge to participate in a primate collecting expedition headed for southeast Asia in 1937. The other scientists on the Asiatic Primate Expedition (APE) included C. R. Carpenter and S. L. Washburn. In Thailand, the first stop for field work, the party proceeded to the city of Chiang Mai, 375 miles north of Bangkok; before leaving the country two months later they had amassed a total of 233 gibbons, along with representatives of other kinds of primates. Subsequently Schultz and Washburn spent three months near Sandakan in North Borneo collecting forty-four gibbons, seven orangs, and series of several kinds of lower primates. Most of the skeletons were returned to the United States in a roughed-out and dried state. Back in Baltimore, Schultz cleaned up those acquired for his personal collection, as well as those going elsewhere but which he intended to study.

The Anatomy Department at Hopkins provided few assistants for the staff. This mattered little to Schultz, because he was quite capable of dealing with his specimens once they were skeletonized; and this he often did, even to the point of numbering the bones and constructing the boxes to house them. He also measured the bones, wrote his manuscripts in longhand, and illustrated them with masterly pen-and-ink drawings. All this he carried out in a single large room with two windows on one side and storage shelves going to the ceiling on the other three sides. Considering that he expended so much of his time getting his data assembled and analyzed, it is remarkable that he published as much as he did.

I think it is unlikely that Schultz ever had one of his well-organized and beautifully illustrated manuscripts rejected by an editor. It should be noted, however, that during his years in Baltimore he had close connections with the founders and/or editors of the more important new journals devoted, at least in part, to primate studies: in Washington, A. Hrdlicka* of the *American Journal of Physical Anthropology* (1918); also in Washington, N. Hollister of *the Journal of Mammalogy* (1919); and in Baltimore, R. Pearl of the *Quarterly Review of Biology* (1926) and *Human Biology* (1929). Between 1918 and 1949 these four journals alone carried 36 of his articles. Among the larger pieces may be mentioned the 1930 article in *Human Biology* (136 pages, 23 hand-drawn figures) and the 1944 article in the *American Journal of Physical Anthropology* (129 pages, 30 hand-drawn figures).

Schultz's early intensive efforts to report the growth and development of particular primates, primarily through measurements, gradually became interspersed with efforts to provide interpretive summaries. A few titles will suggest the points he wished to emphasize: "Man as a Primate" (1931), "Characters Common to Higher Primates and Characters Specific for Man" (1936), "Variability in Man and Other Primates" (1947), "The Physical Distinctions of Man" (1950). These general articles, perhaps more than the others, left enduring impressions on the thinking of primatologists.

In the late 1940s a new trend in the field of anatomy, known as "histochemistry," arrived in force at Hopkins as a new head of the department took over. Schultz could find no indication in this change that the encouragement and support he had always received would continue, so in 1951, when he reached the age of sixty, he retired and went back to Zurich, taking with him his primate collection. Thus, after thirty-five years in Baltimore, Schultz's highly productive American period came to an end.

IV. BACK TO SWITZERLAND

The second Swiss period of Schultz's life began auspiciously with his resumption of Swiss citizenship. Schlaginhaufen, who in 1951 had reached his fortieth year as director of the University of Zurich's Institute of Anthropology, relinquished the position. Schultz was appointed director of the Institute and was also designated professor of anthropology in the University. The Institute provided a repository for his collection and a natural place for him to continue his studies: the professorship gave him further status with only limited academic duties. The portion of his bibliography covering this final period shows that, except for the year 1951, he continued to publish at about the same rate as he had in Baltimore: two to four articles a year, but now more often in German.

The incorporation of Schultz's personal collection of primate specimens into the Institute's collection resulted in a virtually unequalled primatological resource. From the combined collections Schultz selected for exhibition some of the more unusual specimens and others that illustrated evolutionary changes and phylogenetic relationships. Perhaps because he had never before had a display facility, he took special pleasure and pride in showing off the arrangements he had created.

In 1956 Schultz joined Dietrich Starck and Helmut Hofer to edit a new series of primatological monographs published by S. Karger of Basel, entitled *Primatologia* (*Handbook of Primatology*) In 1962 Karger announced the beginning of another monograph series entitled *Bibliotheca Primatologica,* with the same set of editors. The first fascicle of the new series, with Josef Biegert serving as temporary substitute editor for Schultz, constituted a *Festschrift* in celebration of Schultz's seventieth birthday. The fourteen articles contained therein were prepared by personal friends residing on four continents.

Seventy is the retirement age at the University of Zurich, so in 1962 Schultz relinquished the directorship of the Institute to Josef Biegert and in the University simply became Professor Emeritus of Anthropology. This second retirement, like the first, had little apparent effect on Schultz's output of publications until a few years before his death. Particularly noteworthy is the fact that in this period he published his first commercial book (1969). Translated from English into German in his lifetime, it has now been translated also into Italian and Spanish.

REFLECTIONS

The man whose scientific activities are chronicled above was of rather solid build and above-medium height. At first meeting his complexion and the color of his hair—sandy red until his late years—may have seemed to some to bespeak a testy disposition, but more often than not the opposite was the case; usually he was outgoing, humorous, warm-hearted, and generous. This is not to say, as a colleague has noted, that he was not "capable of moral indignation and strong language at misbehavior, professional or other."[4] Yet he rarely

[4] *American Journal of Physical Anthropology,* 46 (1977): 192.

engaged in public debate, being content perhaps to make his points with the overwhelming evidence contained in his published reports. Nor was his avoidance of debate the result of any lack of command of the English language, which he spoke fluently and with surprisingly little German accent.

Not given much to hobbies, he had, as noted, a remarkable talent for scientific illustration and perhaps for this reason appreciated art in general and often visited galleries. Nothing appears to have had any effect on his personal art style, however, not even contact in Baltimore with Max Brödel, the artist who manifestly raised the level of medical illustration in America.

As the above chronicle shows, his central aim from the outset of his career was to acquire as much data on the physiques of as many different kinds of primates as possible for the purpose of drawing therefrom broad generalizations and sound taxonomic conclusions. Eventually he had access to larger samples of many more different species of nonhuman primates than anyone before him. And everything he learned from his studies of these specimens seemingly ended up in publications. When asked, late in life, to state briefly for a biographical dictionary (*World Who's Who in Science,* 1st ed., 1968) his main accomplishments, he listed the following:

> Established correlations and differentiations between development in man and other primates; demonstrated [that] close similarity of man and apes early in life . . . diminishes through differing growth rate [s]: noted [as] human specializations [the] longest postnatal growth period and life span, latest beginning and ending of fertility [p. 1504].

Had he been offered space in which to list more of his accomplishments, quite likely he would have included his revelation of a host of facts regarding the relative variability of the different primate species, by sex and during growth. Among other things, this showed that, contrary to prevailing opinion, the great apes are more variable than man and most

Old World monkeys. On these grounds he cautioned human paleontologists against attributing too much significance to single hominid fossil finds. His observations led him also to take a conservative view of the established classification of the primates and in this regard to resist some of the radical ideas put forth by those using newer biochemical approaches.

A conscientious, innovative, and energetic worker, employing the basic techniques of physical anthropology, he played a major role in developing the young science of primatology into an important part of modern biology. Beyond doubt his many and varied contributions to this field made him one of the world's leading primatologists.

This memoir greatly expands the information about Dr. Schultz I was able to assemble in the obituary notice I prepared for the American Philosophical Society in 1976 (*Yearbook of the American Philosophical Society,* 1976: 118-22). At that time the excellent obituaries by Josef Biegert (*Folia Primatologica,* 26(1):1-23, 1976) and W. W. Howells (*American Journal of Physical Anthropology,* 46(2): 189-96, 1977) were not available to draw upon. I am indebted additionally to Dr. Biegert, as well as to Dr. Howells and Dr. E. G. Erikson, for generously supplying further information and to Dr. Biegert and Dr. Howells for kindly reviewing the final manuscript. The biographical sketch of Dr. Schultz published by Dr. Erikson in 1981 (*American Journal of Physical Anthropology,* 56[4]:365-71) contains personal reminiscences and ten photographs taken in Baltimore and Zurich.

HONORS AND DISTINCTIONS

Awards

Viking Fund Medal in Anthropology, 1948
M.D. Honoris Causa, Universität Basel (Schweiz), 1962

Memberships

National Academy of Sciences (Elected, 1939)
American Philosophical Society
American Anthropological Association
American Association of Anatomists
American Association of Physical Anthropologists (Second President)
American Society of Mammalogists
New York Academy of Sciences (Hon.)
Anatomical Association of Great Britain and Ireland (Hon.)
Zoological Society of London (Foreign Fellow)
Society for Human Biology (Hon.)
Société d'Anthropologie de Paris (Hon.)
Österreichische Akademie der Wissenschaften in Wien (Corr.)
Anthropologische Gesellschaft in Wien (Hon.)
Deutsche Gesellschaft für Anthropologie (Corr.)
Schweitzerische Gesellschaft für Anthropologie (Hon.)
Schweitzerische Naturforschende Gesellschaft
Zürcher Naturforschende Gesellschaft
Institute Italiano di Antropologie, Roma—già Societa Romana di Antropologia
International Primatological Society (Hon.)

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1923 Bregmatic fontanelle bones in mammals. J. Mammal., 4:65-77.
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Photograph courtesy of the Yale University Archives, Yale University Library

Edmund Ware Sinnott

February 5, 1888-January 6, 1968

by W. Gordon Whaley

Edmund Ware Sinnott was born in Cambridge, Massachusetts on February 5, 1888, one of two sons of Charles Peter and Jessie Elvira (Smith) Sinnott. Although he spent the first ten years of his life in Milwaukee, Wisconsin, as far as anyone can determine this interval had little effect on his overpowering dedication to New England. That dedication, however, did not limit his keen interest in worldwide affairs as they related to the advancement of science.

The period during which Edmund Sinnott rose to eminence in science was one in which reductionism had become the dominant mode. Many scientists came to disagree strongly with Sinnott's attempts to meld science with humanism—an attempt he considered essential to the development of what he called the "whole man." He felt only whole men could make proper use of rational science.

His choice of scientific fields was not surprising in view of his background. His mother was a descendent of the Reverend Henry Smith, the first minister in Wethersfield, Connecticut. Both his parents were teachers, no doubt lending encouragement to a career in academic life. He attended a grammar school that was run as a model school supervised by the Bridgewater State Normal School, at which his father spent his life as a geography and geology teacher. When he graduated in 1900 he went to Bridgewater High School, where he worked hard in advanced classical courses. He entered Harvard in the fall of 1904, having first expressed an interest in becoming a writer. Although he devoted himself largely to other pursuits, it certainly can be said that he wrote with style and distinction. He shortly chose zoology as his area of concentration because he had a particular interest in birds. (Many years later he would complete a unique book in collaboration with an ornithologist who had once been a colleague.) During his sophomore year at Harvard, he was offered an assistantship in botany and studied under Professor E. C. Jeffrey, who was engaged in an attempt to reclassify plants on the basis of comparative anatomy. Professor Jeffrey influenced a large number of individuals who became botanists and contributed to many diverse fields. Sinnott's first publications were, appropriately, in the field of comparative anatomy, but no single field in the biological sciences held his exclusive attention. While a Harvard undergraduate, he spent his summers on Cape Cod, where he built up a large collection of plants and became particularly interested in, and published studies on, the flora of Cape Cod ponds. About the same time he came upon some fossil wood in eastern Massachusetts and while still a student published a paper on the subject.

He was stimulated by the Harvard atmosphere and completed his bachelor's degree in 1908, his master's degree in 1910, and his Ph.D. in 1913. By the time he wrote his dissertation, he had turned to the study of reproduction in evergreens. His moves from one biological field to another substantially broadened his knowledge, and this wide base served him well throughout his life. He held various assistantships, the Austin teaching fellowship, and finally a Sheldon traveling fellowship, which allowed him to spend a year abroad, mainly in Australasia, although he managed to stretch it into a trip around the world. He spent this period in association with his roommate, Arthur J. Eames, who was later to become one of the country's foremost plant morphologists and anatomists.

After completing his Ph.D., Sinnott spent two years as an instructor in the Harvard Forestry School and the Bussey Institution. One must suppose that the latter association was particularly significant because some of the earliest studies in plant genetics in the United States developed there. These briefly held positions were followed in 1915 by an appointment to the Connecticut Agricultural College in Storrs as professor of botany and genetics. Here the die was cast, for a strong interest in the developing science of genetics was present. L. C. Dunn had become a staff member of the Agricultural Experiment Station at Storrs and was already pursuing essentially parallel lines of genetic studies with poultry, mice, and rats. Dunn's colleague at the Experiment Station, Walter Landauer, had been experimenting with poultry genetics, while D. H. .Jones was conducting a series of experiments that greatly influenced the emergence of genetic studies in maize at the Experiment Station in New Haven. In fact, scientists in the whole lower New England area were concentrating on genetics. The extensiveness of these studies related to T. H. Morgan's studies at Columbia. The result was the interpretation of genetics on a broad biological base.

Sinnott understood this biological base thoroughly and chose to contribute to it with a study of the genetics of the Cucurbitaceae. His initial interest was in pumpkins and squashes, but as he became increasingly intrigued by the relation of Mendelian genetics and the development of form, he turned to unusual forms among the gourds. The relationship of Mendelian factors to the development of particular forms was, and still is, largely an intractable problem. Sinnott tested many different approaches to relationships between genetic background and size and form, but, not surprisingly, he failed to find one that satisfied him. Nonetheless, adhering closely to the ideas extending from Mendel's work prevalent at the time, he related a great many characteristics of cucurbits to specific genes. For a while his life was occupied with page after page of Punnett squares. Then he complicated things with studies of linkage (and "crossing over"), which had been central subjects for T. H. Morgan and L. C. Dunn. Dunn had written his dissertation on linkage. Finally, though Sinnott confined his own work to fundamental genetic studies, he came to a full realization of the interrelationships between genetic and cytological studies as they were so pertinently developed by E. B. Wilson, both in his lectures and ultimately in his book, *The Cell in Development and Heredity*. Wilson's book was revised through a number of editions and has recently been reprinted as one of the classics in biological science. Almost a handbook, it has survived more than three-quarters of a century.

Sinnott followed the same pattern. He developed lectures for a course in genetics, and then with L. C. Dunn he turned the lecture material and other investigations into a book, *Principles of Genetics,* originally published in 1925. As Theodosius Dobzhansky, a collaborator on later editions with Sinnott and Dunn, has written in his memoir of Dunn:

> The second edition was published in 1932, the third in 1939, and the fourth and the fifth in 1950 and 1958 in collaboration with Th. Dobzhansky (although Sinnott's name was retained as the senior author). During the last years of his life, Dunn was sketching parts of what was meant to become the sixth edition. Translations of *Principles of Genetics* appeared in several languages (and so did a pirated edition in English printed in Taiwan). Most interesting is the fate of the Russian translation in the 1930s, more copies of which were published than the English original. It was widely used for several years, until Trofim Denisovich Lysenko and the Soviet government outlawed it, whereupon it came to be passed from hand to hand like a subversive tract.[1]

[1] "Leslie Clarence Dunn," in *Biographical Memoirs,* vol. 49 (Washington, D.C.: National Academy of Sciences, 1978), p. 82.

This book, too, became a handbook with worldwide distribution. It is still useful in interpretations of early classical genetics and modified illustrations from it are still used in such modern texts as James Watson's *Molecular Biology of the Gene,* which Watson introduced with a chapter on "The Mendelian View of the World."

Sinnott early became the editor of a long series of botanical and biological works published by McGraw-Hill. His own major works were a part of this series, as were those of several of his early colleagues, including Arthur Eames. This editorial position provided a means through which he could maintain his earlier interests while expanding his knowledge in related fields.

Perhaps Sinnott's most direct scientific contributions were his investigations of the development of form. Many of these pertain to animal studies, but botanically trained, he preferred to work with plants. He was not the original developer of morphogenesis but he was certainly one of the major contributors to it. He was a pioneer in his attempt to tie morphogenesis to specific genetic bases. In this he established the foundations of approaches being undertaken in the late twentieth century.

In alluding to his direct contributions in the combined fields of genetics and morphogenesis, one must not overlook the importance of his less direct contributions in many areas within the field of botany. Both his publications of investigations and his *Botany: Principles and Problems* stimulated and influenced large numbers of colleagues and students who were to go on to further studies in these areas.

Sinnott belonged among biologists who were as interested in ideas as they were in details of proof—not a popular category at that time. Perhaps this is one of the reasons it is difficult to pinpoint his own specific contributions within the great range of scientific affairs to which he addressed him self. He finally settled on an interpretation of the control of development of form as the result of what he termed "morphological fields." The term was one that few people accepted and even fewer understood. Sinnott's conclusion that development relates to morphological fields was, in effect, his way of saying that he had gone as far as possible at the time because knowledge and techniques still left much unknown.

When Sinnott went to the Connecticut Agricultural College, he succeeded Albert F. Blakeslee, who, though a botanist, had initiated some of the poultry genetics studies at Storrs. Blakeslee had moved to the Cold Spring Harbor Laboratory. These two men had known each other earlier at Harvard, and by the time of the move, Blakeslee had become intensely interested in studying the genetic background of differences in the genus *Datura.* This interest became the central focus of his investigations at the Cold Spring Harbor Laboratory, which, to this day, has maintained a dominant interest in genetic studies. During summers, in association with Blakeslee at Cold Spring Harbor, Sinnott pursued further basic genetic studies on plants in which he was less particularly interested than had been the case in his study of the cucurbits. Once again he had gotten into a situation that broadened his background.

Later he purchased an eighteenth century saltbox house and several surrounding acres in Woodbury, Connecticut, restored the house, and returned part of the land to an arable condition. This became a family summer home, but it must also have been the gourd center of America, for he had set out to study in detail the genetic background of size, form, and color in a plant group characterized by great diversity. Numerous scientists and their students were frequent visitors to his private Woodbury experiment station. He entertained them graciously but never without a lecture on the basic genetics of plants, illustrated by materials he collected on a quick run to the field.

After thirteen years at the Connecticut Agricultural College, Sinnott moved to Barnard College of Columbia University. T. H. Morgan had left Columbia, and one must suppose that someone saw the wisdom of replacing him with the fast-rising team of L. C. Dunn and E. W. Sinnott—one working primarily with animals and the other with plants. They had already published the first edition of *Principles of Genetics* while at Storrs.

It is difficult to establish priority among Sinnott's many contributions, but if the *Principles of Genetics* does not occupy the foremost position, it certainly comes close to it. Sinnott's *Botany: Principles and Problems,* which went through five editions, the last in cooperation with Katherine Wilson, was widely used but did not have the broad influence of the *Principles of Genetics.* His encyclopedic *Plant Morphogenesis,* which was not published until 1960, brought together a phenomenal range of studies on the development of form. It presaged interpretations that have come to the fore since its publication, and it laid a solid foundation for them. In a sense Sinnott's tremendous influence in this field of investigation came to an end when it began to seem possible to work out molecular and macromolecular bases for genetic control. He understood clearly that this was the direction of the future, however, and he turned his own interests to broader matters.

Sinnott was elected to the National Academy of Sciences in 1936. In 1939 he was appointed to the Columbia University faculty, and in 1940 he moved from Columbia to Yale to undertake a reorganization of the botanical work as Sterling Professor of Botany and chairman of the Department. This assignment called forth all his talents as a botanist, but it also brought his emergence as a spokesman for science. In this role he reached his peak when he became chairman of the Division of the Sciences and director of the Sheffield Scientific School in 1945, then, in 1950, dean of the Graduate School.

All along, the issue of stark unbridled science had bothered Sinnott, and he called for a spiritual outlook that he felt had to accompany science because, as he is often quoted as saying, "science alone may make monsters of men." In his later years he expressed his strong religious and philosophical views. His rationale was that the expression of these views strengthened science by indicating relationships among science, humanism, philosophy, and religion. *Two Roads to Truth,* in which he develops parallel rationales for science and religion, and *Cell and Psyche,* in which biological science and humanism are linked, had wide circulation. Though he came to concentrate on this sort of writing, he never forsook his interest in the problem of organic form.

In *The Problem of Organic Form,* published in 1963, he referred to Sir D'Arcy Wentworth Thompson, who published *On Growth and Form* in 1917, as the patron saint of morphogenesis. If Sir D'Arcy was the patron saint, Edmund Sinnott, another dedicated scientist, was fully ordained and occupied a position somewhat similar to that of Jonathan Edwards in the Great Awakening.

Katherine Wilson pointed out in her memorial article on Sinnott in the *Plant Science Bulletin* that:

> Dr. Sinnott's views and conclusions (on this subject) are most aptly summarized in his own words: 'Back of all the phenomena of genetics, biochemistry, and physiology stands the important fact that a living thing is an organism, that there is an interrelationship among its parts which is manifest in development, and that if this system is disturbed it tends, by a process of self-regulation, to restore itself. The most evident expression of this organization is the form of the organism and its structures. Morphogenesis, the study of the origin of form, thus assumes a central position in the biological sciences.[2]

On the national scene, Sinnott occupied a number of significant positions. He made a notable contribution as presi

[2] *Plant Science Bulletin,* 14 (*1968*): 6-7.

dent of the American Association for the Advancement of Science during its centennial year, and he called for the acceptance of science by urging that the brotherhood of man be developed through the brotherhood of science.

Two aspects of Sinnott's career can be summed up by statements accompanying awards to him. At its fiftieth anniversary celebration, the Botanical Society of America initiated annual awards for distinguished contributions to botany. Professor Sinnott was among the first recipients, and the award to him bore the following citation: "Edmund Ware Sinnott, morphologist, anatomist, geneticist, and botanical statesman, for his numerous varied and sustained contributions to plant anatomy, histology, evolution, and botanical theory." A later award honoring his contributions to Yale reads: "A loyal son of Harvard, by his stature as a distinguished scientist, administrator, historian, and great humanist he brought honor to this university and warm friendship to a legion of admiring colleagues both here and throughout the world."

He was noted for a view of science that knew no national boundaries and one that knew no division among scientists. He sustained this view with deep knowledge, intensity, articulateness, and affability—all linked to a remarkably stern self-discipline. The extent of his influence in laying solid foundations for succeeding generations is reflected in the posts he held and the honors he received. Lest it be thought that science and its relationship to philosophy isolated him from other things, it should be noted that he was a painter and a sculptor of ability, producing among his works a few deemed by experts to be of museum quality. Not surprisingly, he generally took objects of his beloved New England as his subjects.

Edmund Sinnott died on January 6, 1968. He was survived by his wife, the former Mabel H. Shaw of Bridgewater, Massachusetts, and their three children, Edmund Jr., Mildred, and Clara. Not too long before his final illness, he returned to Storrs, Connecticut to attend a meeting. An historian of science, he had himself long ago become part of the history of science. Nonetheless, he actively attended sessions and commented on presentations from his deep wisdom. In informal conversations he related developments in science, sometimes very specifically to sociological considerations such as the relation of heredity to poverty and disease. By this time the interpretation of the human condition had become for him a very complex weaving. Among the many threads were his concerns with the ever-changing scientific investigations; others had to do with spiritual matters. This thinking was expressed by Sinnott at the early, informal meetings of the Society for Growth and Development, of which he was a member of the organizing committee. The organization later developed into the Society for Developmental Biology.

He saw as other threads the influence of religion and social interactions. He was just as proud of one of his last works—a book on meetinghouse and church in early New England—because he saw this as part of the tapestry.

In a rather tongue-in-cheek article a national magazine once chided Sinnott for taking the crookedness out of crooked squashes and putting it into straight ones—thus giving both new characteristics by his genetic manipulations. One hopes in the present day that genetic manipulations and the wisdom and understanding Sinnott brought to science will combine and prevail.

HONORS AND DISTINCTIONS

Academic Degrees

1908	B.A., Harvard University
1910	M.A., Harvard University
1913	Ph.D., Harvard University

Honorary Degrees

1940	M.A., Yale University
1948	D.Sc., Northeastern University
1950	D.Sc., Lehigh University
1957	D.Sc., University of the South
1959	LL.D., University of New Hampshire
1961	D.Sc., University of Hartford

Award

1966	William C. DeVane Medal

Appointments

1908-1910	Austin Teaching Fellow and Assistant in Botany, Harvard
1911-1912	Austin Teaching Fellow and Assistant in Botany, Harvard
1913-1915	Instructor, Harvard Forestry School and The Bussey Institution
1915-1928	Professor of Botany and Genetics, Connecticut Agricultural College
1928-1938	Professor of Botany, Barnard College, Columbia
1938-1939	Professor of Botany, Columbia
1940-1956	Sterling Professor of Botany, Yale
1940-1950	Director, Osborn Botanical Laboratory and Marsh Botanical Gardens
1940-1949	Chairman, Department of Botany, Yale
1945-1955	Director of the Division of Sciences, Yale
1945-1956	Director, Sheffield Scientific School, Trustee and President of the Board, Yale
1949-1950	Department of Plant Science, Yale
1949-1950	Lyman Beecher Lecturer, Yale

1950-1956	Dean of the Graduate School, Yale
1956-1968	Sterling Professor of Botany Emeritus, Yale
1957-1958	Lecturer in Zoology, Yale

Editorships

Editor-in-Chief, *American Journal of Botany*
Chief Consulting Editor, *McGraw-Hill Publications in Botanical Sciences*

Learned Society Memberships

National Academy of Sciences, 1936
American Philosophical Society
American Academy of Arts and Sciences
American Association for the Advancement of Science, Fellow (Vice President, 1935; President, 1948)
Botanical Society of America (Treasurer, 1917-1921; President, 1937)
New England Botanical Club
Torrey Botanical Club (President, 1931-1934)
American Society of Naturalists (Treasurer, 1926-1928; President, 1945)
Society for the Study of Development and Growth
Sigma Xi
Phi Beta Kappa
New York Botanical Gardens, Board of Managers (1933-1940)

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William Hay Taliaferro

February 10, 1895-December 21, 1973

by David W. Talmage

William Hay Taliaferro was born prematurely on February 10, 1895 in Portsmouth, Virginia. His doctor considered his survival remarkable and gave the credit to his mother, Mary Watkins Leigh, for her solicitous care. He was a member of the ninth generation of Virginia Taliaferros and was descended from Robert (1626-1688). The name is English, and it is pronounced Toliver.

Both sides of his family were Virginia aristocrats who had become impoverished by the Civil War. Both grandparents were physicians, but his father was barred from the profession by a hunting accident that resulted in the loss of his right hand. With this background it is not surprising that William's boyhood ambition was to be a physician.

William attended public school in Portsmouth until the age of ten. He then went to Sunnyside Seminary, near Clarksville, Virginia, for three years. This finishing school for girls was run by three Carrington great-aunts who permitted William and several other boys to attend as day students. The boys added zest to the education of the girls by bringing snakes, frogs, bugs, and other novelties into the classroom. As far as his studies were concerned, William excelled in arithmetic, but he never could spell, although he took to Old English and enjoyed looking up the derivation of words.

Once he confided to Karl Lashley, his closest friend at Hopkins and thereafter, that he never knew whether "separate" had 2 *e*'s or 2 *a*'s in its middle, whereupon Karl replied that he never could decide whether "which" had a *t* in it or not.

In 1908, at the age of thirteen, William received a scholarship to Norfolk Academy in Norfolk, Virginia. This private preparatory school had a distinguished history. There he was grilled in the old-fashioned studies of mathematics, English, and history. He took all the science that was offered, but this was limited to the most elementary physics and chemistry. He was not interested in sports and spent his free time reading. He received the Class Prize in 1910 and the Ingram Prize in 1911.

William was shy and timid as a boy and was protected at times from the school bullies by a stronger older brother. This shyness carried over into adulthood as a remarkable gentleness that endeared him to his friends and students. Shyness did not suppress an early resourcefulness. To assure funds for his later education, William sold eggs from a pedigreed flock of hens. He devised an ingenious method of feeding them during classroom hours at Norfolk Academy. He turned an alarm clock on its side and balanced a weight on the clapper. At noon, the weight dropped and upset a box of feed. He always remembered those Plymouth Rocks with pleasure because of "their upturned heads and alert eyes around noontime."[1] Unfortunately, he later developed all allergy to many animal danders that kept him out of the animal quarters.

From the fall of 1911 through the spring of 1915 William attended the University of Virginia. In his junior and senior years, he had teaching assistantships under Dr. William A. Kepner, who not only took him on innumerable field trips

[1] W.H. Taliaferro, *Annual Review of Microbiology*, 1968.

and gave him valuable teaching experience *but* replaced his ambition to study medicine with a lifelong love of zoology. Zoology must have been a little-known profession in those days. When William announced at home that he had decided to be a zoologist, his brother remarked, "Good Lord, William, are you going to be a zookeeper?"[2] While at the University of Virginia, he published three papers with Dr. Kepner on the sensory epithelium of *Microstoma caudatum,* the organs of special sense in *Prorhynchus applanatus,* and the reactions of *Amoeba proteus* to food. William graduated from the University in 1915 with a bachelor of science degree after election to Phi Beta Kappa. He was invited to join the Raven Society of the University in 1944.

While at the University of Virginia, William was impressed with a book by Dr. Herbert S. Jennings of the Johns Hopkins University, entitled *Behavior of the Lower Organisms.* He determined to go to Hopkins and study under Jennings. When he arrived there in the fall of 1915, he found that Jennings had changed his field to genetics and was placed under the guidance of Dr. Samuel Mast. Nevertheless, he attended all the lectures given by Dr. Jennings and those by Dr. Burton Livingston on plant physiology. From the latter he learned to use porous filters that were useful in his research on antibodies to trypanosomes.

William's training in genetics led to the publication of a paper with John Huck on the inheritance of the sickling of human red cells in several Negro families. He and Huck concluded that this phenomenon, later called the sickle-cell trait, is inherited as a single, Mendelian dominant gene (S) that is not sex-linked. Others later showed, on the one hand, that the homozygote recipients of two S genes suffer from severe sickle-cell anemia, and, on the other, that the heterozy

[2] *Ibid.*

gote carriers of only one S gene demonstrate the sickle-cell trait *in vitro,* are free of the disease, and are resistant to the lethal effects of the virulent African malarial parasite, *Plasmodium falciparum.* This disease-host relationship has become a model for the study of genetics and evolution because it shows how an unfavorable gene may be advantageous under particular conditions.

While at Hopkins, William joined the Beta Theta Pi Fraternity. The boys christened him "Tolly," an affectionate name he carried the rest of his life. He was never called Bill. The boys also taught him to play poker and bridge and took him to the Saturday burlesque shows at the old Holiday Street Theatre. It must have been there that he started to accumulate the vast stock of stories for which he was famous.

In the summer of 1917, William was invited to assist in a course on invertebrate zoology at Woods Hole, Massachusetts. There he met Lucy Graves, a Yankee of *May-flower d*escent, who later became his wife. She had just graduated from Goucher College with a bachelor of arts degree after election to Phi Beta Kappa. Since William and Lucy were engaged to someone else at the time, they thought it safe and pleasant to date each other. In any case, the Goucher table where Lucy was a student must have received an unequal share of' the young instructor's time because Dr. Alice is quoted as remarking one day, "Taliaferro, in previous years, the Goucher bunch have always seemed intelligent, but this year they seem to need all your attention while I have to instruct the rest of the class."[3]

Tolly volunteered for service in the army in the fall of 1917 and was assigned to the Division of Chemical Warfare at Yale. While there he finished his thesis, passed his oral examinations, and received his Ph.D. from Hopkins in the

[3] L. G. Taliaferro: personal communication.

spring of 1918, at the age of twenty-three. This accelerated program was made possible by his exceptional training at the Norfolk Academy (he was excused from freshman English and mathematics when he entered the University of Virginia) and continuous attendance at the University of Virginia summer and winter. His thesis was on reactions to light in *Planaria maculata,* a small flatworm, with special reference to the function and structure of the eye. The experiments had been well-planned and carried out at the University of Virginia. The technique and analysis of the results were far in advance of their time.

When Tolly and Lucy left Woods Hole in 1917, they never expected to see each other again, but they continued to correspond. Then, in 1918, Lucy, who was abstracting German articles on gases in the Department of Chemical Warfare, was transferred to New York City. Tolly, who was assisting in a research program on respiratory gases under Dr. Yandel Henderson at Yale, had to go to New York City frequently to get pyrex glass blown for intricate assemblies of equipment. There he looked up Lucy to take her to dinner. His private's pay was stretched to capacity! Their friendship, however, blossomed to the extent that they became disengaged from their financées and engaged to each other. They were married in June 1919.

In the spring of 1919, when his stint in the army ended with the rank of second lieutenant, Tolly was awarded a Johnston Scholarship at Hopkins, but declined it to accept a position under Dr. Robert Hegner in the Department of Protozoology and Medical Entomology in the newly established School of Hygiene and Public Health at Hopkins. While there, the book by Hegner and Taliaferro on *Human Protozoology* appeared. Also, Lucy passed her oral examinations and received her Sc.D. with a thesis on avian malaria. Thus, she became a full member of the Taliaferro team, and the names Taliaferro and Taliaferro appear on many of the more than 100 contributions to science that are mentioned here. They were a perfect team —each respected the other for what each did best. Lucy always said that Tolly breezed in on the express with ideas and questions, and she and one or two assistants arrived later on the freight loaded with experimental data involving injections, blood smears, bleedings, autopsies, and tests galore. Then, as a team, they graphed, tabulated, arranged, rearranged, described, and summarized the results for publication. Usually all three processes were occurring simultaneously on different subjects: a manuscript was being readied for publication on old project one, experiments carried out on intermediate project two, and new ideas were being dreamed up for new project three. Some of the dreaming became sidetracked, delayed, or wrecked by sheer lack of time and energy.

William and Lucy also bought a house in Roland Park, a suburb of Baltimore, expecting to be settled there for years. It was not to be. In 1924 William was invited by Dr. Edwin O. Jordan to join the Department of Hygiene and Bacteriology at the University of Chicago. Three years later he was made a full professor. There he and Lucy remained for thirty-six years and did the bulk of their life's work. During that time they lived in one apartment for eight years and in a second one for twenty-eight years! Their research activities centered around host-parasite relationships and the mechanism of antibody formation with hemolysin formation as a baseline.

Although Tolly had never worked on parasites before, his newly found interest in genetics led him to study variability in *Trypanosoma lewisi,* a blood-inhabiting parasite of the rat. Frequent blood smears were made throughout infections lasting a month or more. Number counts and camera lucida drawings from such smears led him to the discovery that the rat not only develops lytic antibodies—the parasite counts fell in about a week after an initial rise and disappeared later—but also displays a peculiar property of inhibiting parasite reproduction—initial variability of the parasites fell near zero in ten days. Later, when he determined that this factor was an antibody, he called it ablastin. He always considered this discovery his greatest achievement, and it was a recurring theme for study throughout his life. In fact, the last research he published in 1971 with one of his students, Dr. P. A. D'Alesandro, was on the effect of adenine on innate and acquired immunity in the rat to *T. lewisi.* Others, too, were intrigued by the subject. A workshop on ablastin was arranged by D'Alesandro and was held at Rockefeller University on June 21 through 22, 1973. The resulting papers were published in *Experimental Parasitology* in 1975 (Vol. 38:303-69). Tolly and Lucy also studied other trypanosomes in mice, guinea pigs, rats, and dogs, but the mouse was the only host that yielded a reproduction-inhibiting ablastin against *T. musculi* (=*T. duttoni*) Tolly always had a yen to go to Africa with the hope that there he could study some indigenous hosts of trypanosomes, but the opportunity never materialized. In any case, these relationships made a significant impact on Tolly's own research and led much later to his concentrated attack on hemolytic antibodies.

The unraveling of such host-parasite relationships, combined with an orgy of reading, led to the production of *The Immunology of Parasitic Infections,* published in 1929. When Dr. Carroll Bull of the School of Hygiene at Hopkins, William's immunological consultant of those days, read the manuscript, he commended it, but wrote that the chapters needed summaries! Such a suggestion, though justified, caused consternation and the burning of much midnight oil.

Once the summaries were completed, the Taliaferros took their first trip to Europe. They spent Palm Sunday in London, saw the tulips spread out in bloom like vast oriental carpets in Holland, and spent a month in Paris with daily trips to surrounding areas. The magnificent cathedrals, with their brilliant red and glowing blue stained-glass windows, were an inspiration. It was a long-remembered episode in their life, which they were not able to repeat until 1956.

The research and the book brought Taliaferro wide recognition. He was invited to give the Harvey Lecture in 1931 and was elected an honorary member of the Harvey Society. He gave the Delamar Lecture at the School of Hygiene and Public Health in Baltimore in 1932. He was elected president of the American Society of Parasitologists in 1933 and received the Chalmers Medal of the Royal Society of Tropical Medicine of England in 1935.

From 1925 on, Tolly was also a member of the Innominates, a collection of about thirty basic scientists at the University of Chicago. They had a dinner meeting once a month at the University of Chicago faculty Quadrangle Club for a talk by one of the members. Afterwards, the men met their wives at the home where the ladies had dined.

During the work on the various trypanosome infections, Tolly was fascinated with the immunological problem of distinguishing between killing and reproduction-inhibiting effects of innate and acquired immune reactions of the host. He and Lucy then began an intensive study of the parasites that cause various malarias. Examination of these blood-inhabiting plasmodia had two distinct advantages. Blood smears could be used not only to obtain number counts of a given infection, but, if taken often enough, would reveal the reproductive cycle because the parasites usually grow and segment synchronously. Such cycles are usually completed every 24, 36, 48, or 72 hours, depending upon the species. The well-known periodic fevers of malaria occur at the time the plasmodia segment in the blood. Many were the evenings Tolly and Lucy carried home canaries, chickens, or monkeys and parked them in the bathtub in order to make blood smears during the night. This exhaustive and time-consuming search was not very successful as far as the main object was concerned. Parasitic lysis occurred, but no reproduction-inhibiting effect was found in the malarias except for transient ones during the dramatic parasiticidal crises.

The various malarias, however, proved to be invaluable for studying the cellular phases of immunity because plasmodial pigment remains for months as an indigestible marker in host phagocytic cells. This work was started with Dr. Paul Cannon and continued with Drs. William Bloom, Hugh Mulligan, and James Moulder; all were at the University of Chicago except Dr. Hugh Mulligan, who came from England by way of India. The onset of the enterprise was sparked by Dr. Alexander Maximow's work on inflammation following the subcutaneous injection of dyes or India ink. The malaria papers were beautifully illustrated by microscopic camera lucida drawings, and praise for these should be accorded Esther Bohlman Patterson, Tolly's artist from 1935 on.

Examinations of various tissues from infected animals indicated that the plasmodia inhabiting blood cells are phagocytized by macrophages in strategically placed organs, such as the spleen, liver, and bone marrow, and that the macrophages conspicuously increase in number, especially as acquired immunity develops, largely because of the heteroplastic division and development of lymphocytes and monocytes into macrophages. Tolly became convinced that the baffling and, up to this time, largely ignored lymphocytes were important in defense. In 1937 he and Hugh Mulligan coined the appropriate term, lymphoid-macrophage system (LMS), to embrace all cells involved in defense, including the lymphocytes. The term, reticulo-endothelial system (RES), however, had already been proposed ten years earlier by Aschoff to include cells in defense, although he expressly included lymphocytes. This term was so firmly entrenched in the literature that it is still the preferred term, although it now needs the tacit assumption that lymphocytes are e included since the role of lymphocytes in defense is well recognized and their nature is being intensively analyzed.

Taliaferro and Dr. Merrit P. Sarles also found a similar lymphocyte-macrophage relationship in rats experimentally injected subcutaneously with the small nematode worm, *Nippostrongylus brasiliensis.* In this case the strategically located organs, as acquired immunity developed, are the skin, lungs, and intestine.

From all this work and extensive reading, Tolly arrived at the conclusion that cellular reactions in all hosts are similar and stereotyped, whether the invader be living or nonliving, and they only differ superficially because of the invader's size, mode of entrance, and subsequent location in the host.

From 1926 through 1954, the Taliaferros took ten three-month trips for work at centers of tropical disease in Puerto Rico and Central and South America. Three of their journeys were to Panama for intensive work on malaria in monkeys. The Taliaferros always combined a great deal of pleasure with their work on these trips. They swam, played tennis, danced, rode horseback, and crossed the Andes by bus and boat in the justly famous lake region of Chile. Their first airplane trip in 1954 involved crossing the Argentine from Bariloche to Buenos Aires. Publications, in addition to those previously mentioned, included such subjects as a precipitin test in malaria and various tests on equine trypanosomiasis and helminth infections. The trips were subsidized by Dr. W. E. Deeks and the United Fruit Company, the Rockefeller Foundation, and the University of Chicago.

Tolly and Lucy also took lessons in Spanish, but they never got very far. Their assistants were too eager to learn English. Besides, when Tolly was excising a little piece of an already swollen spleen from a malaria-infected monkey for histopathological study, he didn't want to fumble around for the Spanish equivalent of hemostat if the spleen and an unforeseen capillary started spouting blood. Numerous animals were brought back from these trips, such as mice, a dog, agoutis, and monkeys—all containing parasites. On one trip they brought back a cebus monkey for their good friend, Heinrich Klüver of the University of Chicago, who used it for a behavioral study of its ingenious use of tools. This monkey caused a lot of curious amusement to passengers and crew when Lucy took him for his daily walk on ship deck. He was curious too, but apparently not amused. Two graduate students, Drs. Frances A. Coventry and Leslie Staulber, accompanied them on two trips.

In 1939 they called a partial halt to tropical research trips and started spending a month and a half of winter in southeastern Arizona. They made six trips to Los Encinos Ranch on the east side of the imposing Santa Rita Mountains and twenty-two trips to Kenyon Ranch on the west side. Only occasional letters and work on the ever-present unfinished manuscript interrupted these jolly, relaxing respites, which always included a morning horseback ride. In 1968 they took a cruise to Australia and the islands in between and thereafter spent their winters in Hawaii, Key West, or Puerto Rico.

Tolly and Lucy also learned to enjoy the many activities in Chicago's downtown. They attended the theater, movies, and musical programs; for years they had the same balcony aisle seats for the Friday afternoon symphony concerts at Orchestra Hall on Michigan Avenue. After the concerts, they often visited the Art Institute across the Avenue before hav ing a martini and dinner at the nearby University Club. Lucy took a day off once in a while for a shopping spree at Marshall Fields. William had frequent nonbusiness lunches at the men's round table at the Quadrangle Club. Their apartment was adequately cared for by dependable and trustworthy Negro women, except for ten years during the depression when they were fortunate to have a young Irish maid, Kathryn Quinn, who learned to serve them and their company beautiful dinners. Scientific reading was a must for them. In addition, Tolly was interested in economics and was a Civil War buff. He especially enjoyed books about Robert E. Lee.

Beginning in 1931, administrative duties claimed an increasing amount of attention and time. Tolly became chairman of the Department of Hygiene and Bacteriology, later known as the Department of Microbiology (1932-60); associate dean (1931-35) and dean of the Division of Biological Sciences, including the School of Medicine (1935-44); advisor to Chancellor Hutchins (1944-47); and associate editor or editor of the *Journal of Infectious Diseases* (1937-60). Tolly's patient gentleness, infallible sense of' humor, large stock of stories, and willingness to work long hours all made him an effective administrator, but robbed him of' time for the science he loved. He resigned from all administration outside his department in 1948. His attitude was clearly reflected in the reply he gave to Chancellor Hutchins when the chancellor asked him to take two trips in rapid succession. Tolly said this made him feel like Rosie. Rosie announced to the Madam one morning that she was going to resign, and the Madam replied, "Why for heaven's sake, Rosie, you took eleven trips upstairs last week." "Yes'm," replied Rosie, "it's them steps that's wearin' me out."

During World War II, Tolly was the leader of an interdisciplinary group organized to find effective antimalarial drugs. He and Lucy made a detailed study of the mechanism of quinine and in typical Toliverian fashion were able to insert some order into this complex problem. They concluded: (1) that quinine acts directly without metabolic transformation by inhibiting growth and reproduction of the parasites, (2) that both innate and acquired immunity are useful supplementary adjuncts, and (3) that the spleen assumes two important but antagonistic roles: it decreases the contact of parasite and drug while increasing acquired immunity.

All this work on malaria led in 1949 to a comprehensive review of the lymphoid-macrophage system as the site of general cellular defense and antibody production. In addition, a second classical review appeared on the effects of radiation on the immune response. This paper was originally written as a classified document for the U.S. Atomic Energy Commission and, after being brought up-to-date each time, was published in 1951 as a definitive, much-needed review and in 1964 as a book to which Dr. Bernard Jaroslow was added as an author. These publications served as a background for the subsequent hemolysis work. All work thereafter was subsidized in part by grants from the University of Chicago and the U.S. Atomic Energy Commission.

Tolly continued to receive recognition for his contributions to parasitology. In 1939 he was made the Eliakim H. Moore Distinguished Service Professor at the University of Chicago. He received this news in Florida where he and Lucy were spending a short vacation and it seemed to be one of the biggest surprises of his life. In 1940 he was elected to the National Academy of Sciences and in 1941 to the American Philosophical Society. In 1946 he received an honorary doctor of science degree from the University of North Carolina. In 1947 he gave the Ludwig Hektoen Lecture to the American Society of Bacteriologists in Philadelphia and the Con vocation Address at the University of Chicago. His talk was on the need for work in basic science and the dangers of governmental subsidies. In 1949 he received an honorary doctor of letters degree from Temple University, the Mary Kingsley Medal from the Liverpool School of Tropical Medicine, and was made an honorary fellow of the Royal Society of Tropical Medicine and Hygiene of Great Britain. In 1953 he was made an honorary member of the Faculty of Medicine at the University of Chile and in 1954 was elected president of the American Society of Tropical Medicine and Hygiene.

After returning to full-time teaching and research in 1948, Tolly decided to change his research base. He wanted to study antibody synthesis and needed an accurate quantitative antigen-antibody test for a suitable nonreproducing antigen. His aim was to study lysis by itself. None of the parasiticidal antibodies had been quantitatively measured, and the quantitative precipitin test, then in vogue for measuring certain nonliving antigens, was extremely time-consuming. He then, in one of his brilliant guesses, ordered one of the newly perfected Klett-Summerson colorimeters. This machine, he found to his incredulous delight, would measure small differences in the hemoglobin freed from lysed red cells and, therefore, could measure hemolytic antibodies. The simple test he worked out was used to measure a 50 percent hemolytic end point for titers of serums from rabbits injected with sheep red blood cells. The titers then could be partitioned off into linear segments to determine parameters of time and rate of production. The Taliaferros thus obtained from large numbers of rabbits frequent, precise hemolysin measurements that lent themselves to statistical analysis.

In the subsequent work, using normal rabbits as well as irradiated ones, the Taliaferros had the enthusiastic collaboration of their graduate students, Eugene Janssen, Laurence Draper, Dieter Sussdorf, Peter Stelos, and Bernard Jaroslow. Results accumulated at an astonishing rate. A few of the outstanding findings may be summarized. (1) The hemolysin response to one intravenous injection of sheep red blood cells into rabbits is characterized by a latent period followed by a rapid rise to peak titer and a slower decline. (2) A reinjection of the same amount of red cells stimulates an anamnestic response of somewhat similar intensity but appreciably sooner and at a faster rate. (3) The parameters are modified by the route and amount of red cells injected. (4) Changes in hemolytic titer depend upon concomitant formation and decay. (5) The spleen forms most of the hemolysin, but the bone marrow and lymph nodes continue to produce it at a low level over much longer periods. (6) Two antibodies occur with identical specificity but with different physicochemical characteristics. This finding was a forerunner of the separation of antibodies into five different classes.

The effects on the hemolysin response of X-radiating the whole rabbit or parts of it, such as the spleen or appendix, were also intensively studied. The following conclusions were reached. The hemolysin response is maximally and radically depressed when red cells are injected one or two days after near lethal doses (500-700 R). This conclusion had been previously well-documented in the literature for various immune systems, but findings of recovery and restoration as well as stimulation and enhancement of the response by X-rays were little known or even unknown in any immune system. These conditions were established by pinpointing large and small X-ray doses at different times before and after an intravenous injection or reinjection of a small, standard amount of red cells. Some of their surprising results may be briefly mentioned. (1) Statistically significant results involve a delicate balance of factors. These include variability in host reactivity and the size, time, and location of the dose of X-rays with respect to the amount of red cells injected into the host. (2) Recovery from X-ray depression occurs but depends upon the injury involved. (3) Normal induction and peak titer can be restored in rabbits irradiated with 400 R one day before the injection of a combination of red cells and certain substances, such as yeast extract, colchicine, or nucleic acid derivatives. (4) The hemolysin response is significantly stimulated when red cells are given to the rabbit just before small doses (25-100 R) or when red cells are given one or two days after the spleen alone is X-rayed with large doses (5,000-10,000 R). (5) The anamnestic response is less affected. (6) Stimulation and restoration of the response were accounted for by the X-ray release of nucleic acid degradation products normally in short supply in the host.

Tolly and I also worked on the synthesis of antibody, but for this we had to resort to the quantitative precipitin test in rabbits during a secondary response to bovine serum albumin. Tolly's knack of devising clever experiments to test key hypotheses was well illustrated by our method of transferring antibody-forming capacity of spleen cells from one animal to another. Either the donor animal or the recipient was given 35S-labeled amino acids. Only when the label was given to the recipient was radioactivity found in its antibody, thus proving that the transferred cells from the donor contained only the machinery for making antibody and not an inactive precursor. We also found, among other things, that long-lived precursors do not occur, that antibody is not stored during the latent period, and that actual synthesis and release of antibody into the serum take less than an hour once the mechanism is operative. These experiments played a key role in the development of cell-selection theories in 1957.

Although the Taliaferros retired from the University of Chicago in 1960, they moved their laboratory about 30 miles west—to the Argonne National Laboratory—and continued their work until 1969. This move necessitated a whole new life pattern. They had to buy a house and learn to own and drive a car.

Honors for their work continued. In 1960 Tolly received the Condecoracion al Merito, Bernardo O'Higgins, de Primera Clase, Chile. In the same year, Volume 27, pages 1 through 148 of the *Journal of Infectious Diseases* was dedicated to him and contained papers solely by his students and associates. In the same year there was a testimonial luncheon and volume of letters given him by his students and associates. The following letter from that volume, written by Robert M. Hutchins, who was president and then chancellor of the University of Chicago from 1929 through 1951, indicates the esteem in which Tolly was held by his associates.

> I have known William Taliaferro for more than thirty years. From the start I was impressed by his scientific integrity and his administrative dexterity. I need not add that I was also impressed by the charm of his personality and the wit of his conversation. Here you have the ideal university man. We shall not look upon his like again.

Not all the honors were without work. In 1960 Taliaferro and Dr. J. H. Humphrey of England were asked by the Academic Press to *start Advances in Immunology*. They guided Volume 1 in 1961 and Volume 2 in 1962 through publication and then entrusted the chore to others. From 1956 on, Tolly was invited to give various opening or closing addresses at symposiums or international congresses at Oak Ridge, Rutgers, Stockholm, London, and Rome, as well as lectures at the Naples Zoological Society, the Pasteur Institute, and the University of Glasgow. Other honors also followed. In 1961 he was elected an honorary member of the British Society of Immunology. An international symposium on "The Effects of Ionizing Radiation on Immune Processes," honoring him and Hugo Frick, was sponsored by the Atomic Energy Com mission and the University of Kansas and was held at that university on 5-7 September 1961. In 1962 he was given the Pasteur Award by the Society of Illinois Bacteriologists and delivered the Gehrman Lecture at the University of Illinois Medical School. In 1968 he was asked to write his autobiography for Volume 22 of *the Annual Review of Microbiology.* This prefatory chapter outlines his approach and contributions to science. In 1969 and 1970, Volumes 1 and 2 of *Immunology to Parasitic Animals* were dedicated to him, and he wrote the foreword. These two volumes were a direct descendant of Tolly's 1929 *The Immunology of Parasitic Infections.* They contained many chapters by former students and associates and were a fitting acknowledgment of and climax to his work on host-parasite interrelations.

In 1969 Tolly and Lucy celebrated their golden wedding anniversary and retired, although several more of their papers appeared after that date. William Taliaferro died on December 21, 1973.

Tolly has been adequately recognized for his contributions to parasitology. *The Immunology of Parasitic Infections,* published in 1929, was a milestone in the physiological approach to parasitology, and his numerous awards and honors attest to its importance. He was probably the first to recognize the importance of combining a study of the activities of the parasite with a study of the host's responses. This effort led him inevitably into the study of immunology per se. Tolly's careful, thorough, and precise analysis of the hemolysin response along with his earlier emphasis on the cellular nature of immunity played a key role in redirecting immunology into the mainstream of biology. This pioneering advance has never been adequately acknowledged. Perhaps Tolly's unique ability to work closely with numerous collaborators and his generosity in giving others credit for various phases of the work disguised his contributions. In any case, he seemed to be ruled by a kind, considerate unselfishness and enlivened and brightened any group with his sparkling ready wit, timely joke, and sunny smile. He derived a mischievous delight in telling his classes of medical students that he was a typical parasite in that *he* always derived benefit and took advantage of the work of others. Above all was Tolly's interest in science. He was a pathfinder in formulating hypotheses of how nature works and in devising methods aimed at solving the questions that arose. I know that for myself the development of the concept of cell-selection arose during almost weekly lunches with him during the period from 1952 through 1957. I also know that the invitation and encouragement to write the associated concept of "Immunological Specificity" for *Science* in 1959 came from this same source. How many other graduate students and collaborators owe a similar debt may never be known.

In conclusion, I would like to append to this biography a poem my wife and I wrote at the time of Tolly's retirement from the University of Chicago in 1960 because it expresses, in a clumsy way, the admiration and deep affection we had for him.

Lines Composed in a High Fever and at a Low Oxygen Saturation
(*With apologies to Samuel T. Coleridge*)
For Talifer did mother naitch,
A scientific dome decree,
Where comp and hemolysin ran,
Through test tubes numberless to man,
To set a tracer free.
So thrice four sets of complement
Compared with one to battle went;

And here were racks of shiny copper plate
Where stood a thousand test tubes in a row,
And here was Lucy, ever working mate
Directing and helping to plan the show.
But oh! that deep scientific dome which granted
The ideas which shaped the plan
A magic place! both holy and enchanted
Filled with stories ne'er decanted,
Nor would pass a censor's ban.
It is a miracle of rare device
A scientific dome with humor nice.
Could we revive within us
The magic of his song
To such a deep delight 'twould win us
That with music loud and long
We would build his life in verse.
Alas! We are not poets.

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Photograph courtesy of Jean Raeburn

Robert Erastus Wilson

March 19, 1893-September 1, 1964

by L. William Moore
and Donald L. Campbell

Robert Erastus Wilson was inspired by a competitive fervor for excellence, which he applied to such disparate fields as science, education, business, and public affairs. He was the son of a mathematics professor, and his interest in science was awakened early. He developed a high regard for analytical reasoning, which served him well in all of his undertakings. As his college chemistry professor noted, "Bob would make a good research man—he's quite sure there's a better way to do everything than the way now used."[1] This was indeed to become his guiding principle.

In 1954 he said about himself: "I have made few outstanding scientific discoveries. My principal contributions to science were probably in the field of generalizing scattered facts, theories, and observations and in applying scientific principles to the solution of practical problems."[2] In the exercise of this philosophy, he obtained eighty-nine U.S. And

NOTE: The Academy would like to express its thanks to Manson Benedict for his invaluable contributions to this memoir. Dr. Benedict generously contributed the comprehensive section concerning Dr. Wilson's government service, as well as a good deal of information in the section entitled "Honors and Distinctions."

[1] "Dr. Robert E. Wilson Retires," *Standard Torch,* March 1958, p. 5.

[2] Robert E. Wilson, "Autobiographical Statement" (1954), p. 2, Archives of the National Academy of Sciences, Washington, D.C.

fourteen foreign patents and published more than 100 technical papers. He was recognized with three important scientific medals and other awards and with eighteen honorary college and university degrees.

His serious pursuits were accompanied by an unfailing sense of humor. He was asked once what his middle initial stood for. "I've been trying to keep that a secret," he grinned. "In accordance with family custom, I was named Robert after one grandfather and Erastus after the other. I once checked to see if I could not have been given the middle name of the other grandfather, but found out that it would have been Ebenezer." [3] He also was fond of telling friends the story of "four significant facts" about his life, which he duly related to the Academy in 1954: "1. I married a secretary in 1916. 2. I hired my first secretary in 1919 (Catherine V. Ogilvie). 3. Both of them are still with me. 4. They are good friends!"[4]

THE EARLY YEARS

Robert Erastus Wilson was born March 19, 1893 in Beaver Falls, Pennsylvania. He was reared as the eldest of four children of William H. Wilson, who was a mathematics professor first at Geneva College, Beaver Falls, Pennsylvania, and then at The College of Wooster (Ohio), from 1900 until his death in 1907. At that time, Bob Wilson was fourteen years old. Since the family had little money, his mother, Madge (Cunningham) Wilson, formed a college boarding club in their home near the campus. The children all helped by waiting tables, washing dishes, and performing other chores. All four graduated from college, and Bob and his brother were able to finance their graduate work almost entirely through scholarships and their own efforts.

[3] "Robert E. Wilson Retires," p. 2.
[4] Wilson, "Autobiographical Statement," p. 4.

Bob attended public school through the eighth grade (skipping the first, second, and fifth), then preparatory school and The College of Wooster. He said, "I liked all forms of science, mathematics, and mechanical drawing; I disliked history or anything else which seemed to rely primarily on memory as against reasoning. My professor of chemistry was more responsible than any other individual for awakening my interest in science in general and chemistry in particular."[5]

Wilson was graduated magna cum laude from The College of Wooster in 1914 with the degree Ph.B. He took pride in knowing that his father, in 1889, and one of his daughters, in 1943, were also graduated from Wooster with top honors.

In 1916 he married Pearl M. Rockfellow. They were the parents of three daughters: Doris Mildred (Mrs. Louis 0. Blanchard, Jr.), Lois Marian (Mrs. James A. Scott), and Janice Marjorie (Mrs. William E. George). In a contemporary account of the parent Wilsons in 1958, an article says, "Their evenings alone are usually spent sitting across from each other at a big, thirty-one-year-old, two-sided mahogany desk in their apartment overlooking Lake Michigan. While Bob, with his bulging briefcase on the windowsill, reads reports or works on a speech, Pearl works with her household accounts, on one of her scrapbooks, or writes to one of her two hundred correspondents."[6]

After graduation from Wooster, Wilson went to the Massachusetts Institute of Technology, where he received his B.S. in chemical engineering in 1916. He describes his early work as follows:

> My first scientific contributions were with regard to methods of measuring the vapor pressures of hydrated salts, which were described in my

[5] *Ibid.,* p. 1.
[6] "Robert E. Wilson Retires," p. 3.

undergraduate thesis at MIT but were not published in the *Journal of the American Chemical Society* until 1921—"Some New Methods of Determination of Vapor Pressure of Salt-Hydrates." This work led to what was probably my first substantial scientific contribution, though it took the form of posing a question, not giving the answer. The question was: How could one reconcile the kinetic theory of vapor pressure with the phase rule? For example, in a mixture of hydrated and a dehydrated salt, under the kinetic theory one would expect the number of the water molecules escaping to be proportional to the number of "vacant spaces" present on the surface. Under this theory, the vapor pressure should vary roughly in proportion to the degree of hydration of the salt. However, both the phase rule and experimental evidence state that if you have a mixture of hydrated and unhydrated salt, the vapor pressure is the same whether it is 1 per cent or 99 per cent hydrated (assuming the salt has only one crystalline hydrate).

I put this question up to several of my professors at the Massachusetts Institute of Technology, including such outstanding men as Arthur A. Noyes and Warren K. Lewis, neither of whom was able to give the answer. I was then fortunate enough to be assigned to the General Electric Laboratory at Schenectady, New York, for a summer job after I was graduated from Massachusetts Institute of Technology, and I put the question to Irving Langmuir. He, too, was unable to answer it but thought the question was quite intriguing and important in connection with a paper he was then writing. The next morning he called me up to ask if I had the answer, which, of course, I did not. He then said that he had the answer and that it would constitute an important part of his forthcoming paper on the characteristics of the solid state. He pointed out that the only way to reconcile the two theories was to assume that, in the case of the hydrated salt, molecules left or entered the crystal surface only *at the boundary* between the two phases—in other words, the water molecules on an undisturbed surface of hydrate were relatively stable, and likewise water molecules which struck a completely dehydrated surface were not able to stay, but at the boundary between the two phases, the forces were closely in balance, and the vapor pressure was that required to substantially equalize the number of molecules entering and leaving the boundary; at slightly higher vapor pressures, the water molecules would leave until it was all dehydrated and vice versa.[7]

[7] Wilson, "Autobiographical Statement," p. 1.

Wilson remained at MIT in 1916, serving as research associate in the Research Laboratory of Applied Chemistry under William H. Walker. In 1917 he became consulting chemical engineer for the Bureau of Mines in Washington, DC. In World War I, he served as captain and then major, at age twenty-five, of the Chemical Warfare Service. He and Dr. James B. Conant were the youngest majors in the service. Wilson directed the CWS research division. He made a number of important contributions to the creation of more efficient gas absorbents of various types, including soda limes, impregnated charcoals, and the like, for gas masks. In 1919 Wilson returned to MIT as director of the Research Laboratory of Applied Chemistry and associate professor of chemical engineering. From 1919 to 1922 he was also associated with Arthur D. Little, Incorporated.

During his early years at MIT Wilson published outstanding papers on the mechanism of corrosion of iron, the mechanism of lubrication, and the flow of fluids through pipelines, "all of which tended to bring order out of rather chaotic subjects," as he put it. He also developed accurate methods of measuring the effective volatility of motor fuels.

THE TRANSITION TO INDUSTRY

Wilson moved from the MIT campus to industry in 1922. He brought with him the insight into the problems of business he had developed as he helped MIT set the pattern of cooperation with industry, working with such university clients as Vacuum Oil Company, US Steel, Standard Oil (New Jersey), General Motors, Goodyear, Pittsburgh Plate Glass, and others. To leave ajob he liked, with an income of $10,000 a year, he set his price high, at $14,000, when he was invited to join Standard Oil Company (Indiana) in the position of assistant director of research in the company's laboratory near Chicago. That seemed too high a salary for a young man of twenty-nine, in the estimation of Standard's chairman, Robert W. Stewart. Stewart balked at the figure—until he talked with the young man. That convinced him, and Wilson was hired at his own figure. He remained with the company for thirty-six years.

In the field of oil refining, Wilson developed many new methods of reducing evaporation losses in storage, improvements in cracking, and the coking of residual fuels by what is known as the "delayed coking process." He also contributed substantially to the assembly of fundamental data concerning the properties of petroleum hydrocarbons, the solvent extraction of lubricating oils, and the use of propane as a refining agent for the separation of wax or, under other conditions, the separation of asphalt from the heavier fractions of petroleum.

Dr. Wilson, as he was commonly addressed both within and outside the company, continuously showed his mettle as he progressed in the corporation. From his beginning position in Standard Oil, he advanced to director of research and head of the Development and Patent Department and then to membership on the Board of Directors. He moved into broad management responsibilities in 1934 when he became vice chairman and later president of a principal subsidiary, Pan American Petroleum and Transport Company, with headquarters in New York City. PAPTCO functions were later transferred to Standard Oil's American Oil Company (now Amoco Oil Company).

When the time came for Standard to replace its top management in 1945, it looked to Dr. Wilson and A. W. Peake, whose company experience had been in crude oil and natural gas exploration and production. Under a relatively new team-management concept, Dr. Wilson was elected chairman of the Board of Directors and chief executive officer, with direct responsibility for all staff departments, and Mr. Peake was elected president in charge of operations. When Dr. Wilson retired from company service thirteen years later, the company, one of the ten largest corporations in the United States, had doubled its net worth.

GOVERNMENT SERVICE

In 1940, while president of Pan American, Wilson was placed in charge of the Natural Gas and Petroleum Section of the National Defense Advisory Commission. Working three days a week as a dollar-a-year man, he served as technical adviser to the government on oil-industry matters and stimulated manufacture of 100-octane gasoline and synthetic rubber. In 1940 and 1941 he served as consultant to the Petroleum Unit of the Office of Production Management, where he fostered close relationships between the Army and Navy and the petroleum industry and helped establish petroleum product specifications. In 1942 he served on four committees of the Petroleum Industry War Council, composed of seventy-eight oil company executives. In 1945, at the request of the U.S. Treasury, he served as one of the four managing directors of the General Aniline and Film Corporation, a German company seized at the end of the war.

Before his retirement from the petroleum industry, Wilson prepared for his second career by accepting an appointment from President Eisenhower in 1956 as a member of the nine-man General Advisory Committee of the U.S. Atomic Energy Commission.

Wilson served so effectively on this advisory committee that in 1960 President Eisenhower named him one of the five commissioners of the U.S. Atomic Energy Commission. As a commissioner, he led the successful effort to amend the Atomic Energy Act to permit private ownership of special (fissile) nuclear material, and he stimulated expansion of U.S.

nuclear generating capacity. He was interested in the use of nuclear power as an instrument of national policy and as an economic benefit to the United States in foreign trade. He formulated U.S. policy in cooperating with friendly nations to develop nuclear power and to provide an assured source of enriched uranium with safeguards to prevent its diversion for military uses. Wilson strongly supported development of the centrifuge method for enriching uranium, because of its reduced power consumption compared with gaseous diffusion. He differed with the Commission's decision to delay development of the centrifuge because of its capability to produce weapon-grade uranium-235. He stated very strongly that one could not legislate against technical progress; he believed that one should utilize new developments and solve the political problems associated with them. If Wilson's advice had been followed, the United States might not have lost its world leadership in supplying enriched uranium.

Wilson resigned from the Commission on February 1, 1964 because of failing health. He received a personal letter from President Johnson that read, in part:

> Your outstanding performance as a commissioner and the high esteem and respect with which you are regarded by your fellow commissioners as a scientist, a businessman, and a public servant must be a source of great satisfaction to you as your years of public service come to an end.
>
> As a result of your foresight and determination, we have a stronger and more self-reliant private atomic energy industry today.
>
> I join all of your friends and a grateful nation in thanking you for your years of fruitful and beneficial service.

Chairman Glenn T. Seaborg of the Atomic Energy Commission stated: "The entire atomic energy program will miss Dr. Wilson's services. He brought to the Commission not only an extensive technical background, but a broad experience in business and finance."

Later in 1964 Dr. Wilson contributed further to the national atomic energy program by serving as an official adviser to the U.S. delegation to the Third United Nations International Conference on the Peaceful Uses of Atomic Energy held in Geneva, Switzerland. There his career as a scientist, engineer, and public servant was cut short by a stroke. He died in the Geneva Cantonal Hospital on September 1, 1964. At that time Glenn Seaborg said: "Dr. Wilson's wide experience and wisdom, imparted with vigor and generous spirit, greatly enriched the development of atomic energy in the United States and in the world."

THE PUBLIC AND PRIVATE MAN

Although for many years he held senior industrial executive positions, Wilson was recognized as one of the eminent chemical engineers in the United States. He was awarded the Chemical Industry Medal in 1939, the Perkin Medal in 1943, the Lord Cadman Memorial Medal in 1951, the Northwestern University Centennial Award in 1951, and the Washington Award in 1956.

Dr. Wilson maintained his participation in professional organizations through the years. He was chairman of both the Division of Physical Chemistry and the Division of Industrial and Engineering Chemistry of the American Chemical Society, certainly an unusual combination. He also served as a director of the American Chemical Society and of the Society of Automotive Engineers.

All his life he was never far from the concerns of formal education. He was a life member of the Corporation of Massachusetts Institute of Technology, a trustee of the University of Chicago, and chairman of the board of The College of Wooster (Ohio). Moreover, his deep interest in the future of education led him to establish, in 1952, a philanthropic foun dation financially supported by Standard Oil (Indiana) and dedicated to the aid of educational and other public institutions. It is now named Amoco Foundation.

Both as a scientist and as a businessman, Dr. Wilson felt a strong need to communicate his views. In addition to his technical writings, he wrote scores of articles for a wide range of publications, including the *Saturday Evening Post* (1953), appeared on radio and television programs, and delivered more than five hundred public addresses; he had to turn away requests for fully a thousand more. His subjects ranged from atomic energy to religion, and his convictions were strong. He used to joke, "Among businessmen I pose as a scientist; among scientists, as a businessman."[8] Among churchmen he spoke for both business and science: "Most scientists, as they learn more about the wonders of nature, grow in respect for the Creator, many of whose wonders they are barely beginning to understand, let alone duplicate." [9] In his speeches, Dr. Wilson often compressed man's five hundred thousand years of development into fifty years, in order to illustrate recent progress. In this time scale, man had his first printing press only two weeks ago—and only within the last day did he have radio, television, rayon, nylon, sulfa drugs, and 100-octane gasoline. In 1956 the Illinois Society of Certified Public Accountants bestowed its first annual Public Information Award on Dr. Wilson.

Dr. Wilson was a teetotaler and also refrained from the use of tobacco, but he enjoyed candy and desserts. Once at a dinner with business associates, he was teasingly asked whether he was aware that there was some alcohol in the cherries jubilee he was relishing at the end of the repast. He instantly responded, the story goes, that it was quite all right if one took it with a spoon.

8 "Robert E. Wilson Retires," p. 6.
9 *Ibid.*

His competitive nature was demonstrated in his fondness for golf (he played in the low 80s for years) and his dedication to playing bridge. His concentration in golf was so intense that at times when he had the honor he would drive and then immediately stride off in pursuit of the ball, momentarily forgetting that three others remained to tee off. During the years that he donated a silver cup as a prize for low gross in American Chemical Society golf tournaments, he was always one of the strong contenders; he won it once.

Dr. Wilson's name is memorialized in the Robert E. Wilson Award, which is presented for outstanding chemical engineering contributions and achievements in the nuclear industry. The award has been sponsored annually, beginning in 1967, by the Nuclear Engineering Division of the American Institute of Chemical Engineers.

HONORS AND DISTINCTIONS

Honorary Degrees

1931	Sc.D., The College of Wooster (Ohio)
1940	Eng.D., Polytechnic Institute of Brooklyn
1941	LL.D., Colby College
1947	LL.D., Northwestern University
1948	L.H.D., University of Tulsa
1952	LL.D., Lake Forest College
1953	LL.D., William Jewel College
1953	LL.D., Hamline University
1954	H.H.D., Bradley University
1955	LL.D., University of Akron
1955	L.H.D., Shurtleff College
1955	H.H.D., Parsons College
1955	Sc.D., Drexel Institute of Technology
1957	LL.D., Washington University
1957	LL.D., Huron College
1958	LL.D., Colorado College
1961	LL.D., American University
1963	Sc.D., Geneva College (Beaver Falls, Pennsylvania)

Academic Positions

1916-1917	Research Associate, Research Laboratory of Applied Chemistry, Massachusetts Institute of Technology
1919-1922	Director, Research Laboratory of Applied Chemistry, and Associate Professor of Chemical Engineering, MIT

Military Service

1918-1919	Captain and Major, Directing Research Division, Chemical Warfare Service

Governmental Positions

1917-1918	Consulting Chemical Engineer, Bureau of Mines
1940	Natural Gas and Petroleum Section, National Defense Advisory Commission
1940-1941	Consultant, Petroleum Unit, Office of Production Management

1942	Support committees, Petroleum Industry War Council
1956	Member, General Advisory Committee, U.S. Atomic Energy Commission
1960-1964	Commissioner, Atomic Energy Commission
1964	Official Advisor, U.S. Delegation, Third U.N. Conference on Peaceful Uses of Atomic Energy

Awards and Honors

1939	Chemical Industry Medal
1943	Perkin Medal, Society of Chemical Industry
1951	Lord Cadman Memorial Medal, British Institute of Petroleum
1951	Northwestern University Centennial Award
1951	Pennsylvania Ambassador Award, Pennsylvania State Chamber of Commerce
1956	Washington Award, Western Society of Engineers
1956	Public Information Award, Illinois Society of Certified Public Accountants
1964	Award to Executives, American Society for Testing and Materials

Memberships in Learned Societies

Alpha Chi Sigma
American Chemical Society
American Institute of Chemical Engineers
American Nuclear Society
American Philosophical Society American Society for Testing and Materials
Delta Sigma Rho
The Indiana Society of Chicago
National Academy of Sciences
Newcomen Society in North America Phi Beta Kappa
Royal Society of Arts, London
25-Year Club of the Petroleum Industry

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1919

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1920

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1923

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1924

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1,615,407. With F. M. Rogers. Continuous distillation of petroleum-vacuum-pipe still. (Filed 10/11/24; issued 1/25/27.)

1,815,753. Antiknock fluid compositions. Additional component to reduce freezing point. (Filed 11/8/24: issued 7/21/31.)

1,599,108. Bromine manufacture from brines. (Filed 11/24/24; issued 9/7/26.)

1,654,200. With H. V. Atwell. Continuous coking method. Deposit and removal on nickeliferrous metal. (Filed 11/26/24; issued 12/27/27.)

1,676,610. Distillation of oils—stripping residue and recycling stripper vapors through furnace coil. (Filed 12/22/24; issued 7/10/28.)

1925

1,632,259. With W. H. Bahlke. Continuously indicating hydrometer which compensates for variation in temperature. (Filed 1/5/25; issued 6/14/27.)

1,547,141. Prediluted motor oil. (Filed 1/15/25; issued 7/21/25.)

1,731,479. Fractioning column construction—pancake reflux coils, etc. (filed 1/15/25; issued 10/15/29.)

1,716,939. With R. D. Hunneman, W. H. Bahlke, and F. M. Rogers. Bubble tower construction. (Filed 1/31/25; issued 6/11/29.)

1,898,414. Pressure shell pipe still cracking. Segregation of shell into zones. (Filed 3/13/25; issued 2/21/33.)

1,791,209. With R. D. Hunneman. Vacuum-steam distillation. Temp. 675-760° F. Pressure 75 mm. (Filed 4/1/25; issued 2/3/31.)

1,751,182. Vacuum pipe still steam distillation with centrifugal separator. (Filed 4/3/25; issued 3/18/30.)

1,758,590. Superheated steam—vacuum distillation. Nozzle and target. (Filed 4/4/25; issued 5/13/30.)

1,700,392. Automobile radiator cooling fluid. Specific hydrocarbon fraction. (Filed 4/21/25; issued 1/29/29.)

1,712,187. Pressure shell cracking of oils followed by lower pressure tube cracking of residue. (Filed 6/29/25; issued 5/7/29.)

1926

1,924,520. With E.J. Shaeffer, G. W. Watts, and E. P. Brown. Flash distillation of hot pressure tar. (Filed 4/10/26; issued 8/29/33.)

1,825,378. Control valve for use on hot cracked streams. (Filed 5/27/26; issued 9/29/31.)

2,021,471. Cracking—stripping tar with light vapors from cracking. (Filed 10/18/26; issued 11/19/35.)

1,996,091. Cracking—methods of heating oil in furnaces. (Filed 11/1/26; issued 4/2/35.)

1927

1,654,201. With H. V. Atwell. Continuous coking apparatus of U.S. 1,654,200. (Filed 1/21/27; issued 12/27/27.)

1,737,347. "Solid Billet" heat exchanger. (Filed 1/22/27; issued 11/26/29.)

19,701 (Reissue). "Billet" heat exchanger. (Filed 1/22/27; issued 9/10/35.)

1,726,281. With J. E. Moore and C. W. Chenicek. Breather bag construction—method of weighting. (Filed 4/1/27; issued 8/27/29.)

1,778,475. With W. H. Bahlke. Bubble tower—dam construction and location. (Filed 8/6/27; issued 10/14/30.)

1928

1,966,746. Distillation equipment—multicoil pipe still—multiple columns. (Filed 5/16/28; issued 7/17/34.)

1,831,053. Prediluted oil—diluted prior to dewaxing and dewaxed. (Filed 7/2/28; issued 11/10/31.)

1,859,322. Underwater storage of volatile hydrocarbons—submerged open bottom hemispherical tank. (Filed 7/5/28; issued 5/24/32.)

2,090,245. Coking—"Delayed." (Filed 12/31/28; issued 8/17/37.)

1929

1,899,918. Bubble tower construction. (Filed 10/14/29; issued 2/28/33.)

1,841,691. Aeroplane fuel tank breather. Absorbs water and vapors. (Filed 11/29/29; issued 1/19/32.)

1930

1,950,201. Molecular (vacuum) distillation apparatus. (Filed 1/2/30; issued 4/25/33.)

1,906,033. "Molecular" or vacuum surface distillation apparatus. (Filed 1/2/30; issued 4/25/33.)

1,871,937. Furnace construction vertical cylindrical radiant section, refractory target protects superimposed convection section. (Filed 3/28/30; issued 8/16/32.)

1,960,885. Destructive hydrogenation of pressure tar—two coil common reactor chamber. (Filed 5/21/30; issued 5/29/34.)

1,883,211. Method of concentrating caustic soda. Pipe stilling. (Filed 10/20/30; issued 10/18/32.)

1,958,528. Destructive hydrogenation—liquid followed by vapor phase. (Filed 11/28/30; issued 5/15/34.)

1,991,971. Coking. Coking zone superimposed by a fractionating column. (Filed 12/31/30; issued 2/19/35.)

1931

2,123,457. Tree spray—white oil and antioxidant. (Filed 1/16/31; issued 7/12/38.)

2,009,367. Cracking oils—fractionation of products in a series of fractionating towers at successively lower pressure. (Filed 6/1/31; issued 7/23/35.)

2,077,656. Dewaxing—propane and light diluent. (Filed 8/31/31; issued 4/20/37.)

2,004,560. Antioxidant R-amino hydroxy benzene stabilized leaded motor fuel. (Filed 9/18/31; issued 6/11/35.)

2,029,687. Countercurrent liquid—liquid extractor. (Filed 12/18/31; issued 2/4/36.)

1932

1,992,014. With T. H. Rogers. Gasoline plus color-unstable antioxidant plus color stabilizer. Ex alpha naphthol plus tributyl amine. (Filed 1/26/32; issued 2/19/35.)

2,023,110. Color unstable antioxidant in motor fuel stabilized by addition of polyhydroxy benzene compound. (Filed 5/2/32; issued 12/3/35.)

2,026,336. Propane dewaxing—chilling method. (Filed 6/20/32; issued 12/31/35.)

1,907,924. Process for carbureting air with normally gaseous hydrocarbons. (Filed 6/30/32; issued 5/9/33.)

2,096,949. Liquid fractionation propane (deasphalting) pressure tar—increasing bitumen content. (Filed 7/5/32; issued 10/26/37.)

2,096,950. Solvent extraction and dewaxing of lubricating oils—solvent recovery. (Filed 10/6/32; issued 10/26/37.)

2,029,688. Countercurrent liquid—liquid extractor. (Filed 12/3/32: issued 2/4/36.)

1933

2,029,690. Countercurrent liquid—liquid extractor. (Filed 7/10/33; issued 2/4/36.)

1934

2,064,708. Cracking—back flushing pressure relief lines. (Filed 6/30/34; issued 6/30/34.)

2,086,487. With W. H. Bahlke and F. W. Sullivan, Jr. Solvent extraction—deasphalting multiple solvents. (Filed 5/29/34; issued 7/6/37.)

1935

2,090,907. Furnace construction multiple radiant sections with wall tubes, single roof section, single convection section. (Filed 1/26/35; issued 8/24/37.)

2,143,882. With P. C. Keith, Jr., and M. J. Livingston. Propane deresinating of oils. (Filed 8/15/35; issued 1/17/39.)

1937

2,221,708. Heater construction (furnace with several vertical banks of tubes fired from both sides). (Filed 6/16/37; issued 8/13/40.)