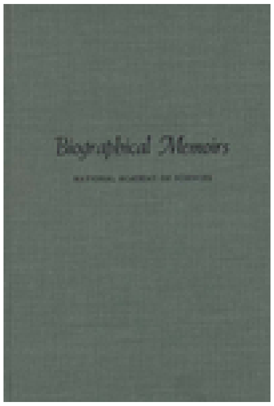

Biographical Memoirs V.50

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Biographical Memoirs

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Biographical Memoirs

Volume 50

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Contents

Preface

The *Biographical Memoirs* is a series of volumes, first published 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 should also include those aspects of their lives in their home, school, college, or later life that led them to their scientific career.

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

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Biographical Memoirs

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Detlev Wulf Bronk

August 13, 1897-November 17, 1975

By Frank Brink, Jr.

Detlev Wulf Bronk was born in 1897 in New York City, where his father, Mitchell Bronk, was pastor of the Ascension Baptist Church at 160th Street and Park Avenue. Det received his unusual Christian names through his mother, Marie Wulf, whose father was Detlev Wulf, a businessman in New York City. The family moved to Bayonne, New Jersey in 1900, where his sister, Isabelle, was born in 1903. His later youth (1912-1919) was spent in Troy, New York in the same region of the state where his forebears had lived for many years. Detlev Bronk is a direct descendant of Mattheus Brunck, a "smith" from the Rhenish Palatinate, who came to West Camp, on the Hudson River, in 1710. A grandson of Mattheus, Abraham, settled about 1797 on a farm near Duanesburg, Schenectady county, which became known as "the Bronk Place." Det's grandfather, Abram Bronk, grew up in Florida, New York. In nearby Manchester lived Cynthia Brewster, a descendant of the Elder William Brewster of the Mayflower Colony. Abram and Cynthia were married in 1856 after a prolonged courtship, much of it carried on by letter via packets to and from California. After their marriage Abram farmed "the Bronk Place" from 1856 to 1861, and Det's aunt, Anna Isabella, was born there.

Tracing the history of a contemporary American family that had one line beginning in 1710 and at least one other in 1620 is both interesting and complex. The available letters and documents are far too extensive for review here. Of present relevance is recorded evidence of a continuous thread of scholarship and a love of learning that linked the generations, even in times when practical concerns demanded most of each person's energy and attention. Det's grandfather, Abram, attended Union College for two years. He studied mathematics, was fascinated with astronomy, and read poetry. He was, in succession, a schoolteacher, a "forty-niner" in California,* a farmer in Duanesburg, and a storekeeper in Manchester. He was an able debater and kept notes on rules of public speaking. Also an avid reader who loved books, he left his family "more books than money" when he died in 1870. Abram had attributes that were to characterize his children and grandchildren, even though he died when his children—Isabella, Mitchell, and John—were very young. His widow, Cynthia, a religious woman and a teacher, propagated the thread of scholarship by providing an environment that permitted Mitchell and Isabella to develop their scholarly talents, each earning the degree of Doctor of Philosophy. Their brother, John, became a lawyer. Aunt Belle taught French Language and Literature as a member of the faculty of Swarthmore College from 1901 to 1927; she had a very definite influence upon Det's development and general education during his college years there. Mitchell, a Baptist minister with a Ph.D. in theology, was an author of several books on religious matters both historical and inspirational. His essays covered a variety of subjects including experiences of his own life and times. Mitchell had strong views on the defects in the then current educational system. He wrote of his training: "interminable reviews, tests, and nervewracking exams in my opinion are not [a sign of] real scholar

* Mitchell Bronk, *Discovering My Forty-niner Father* (Philadelphia: Judson Press, 1942).

ship. It does not result in a real love of literature."* According to my recollection of Det's account of his early education, he studied under guidance of his father until the age of ten. He graduated from the eighth grade in Bayonne in 1911, then enrolled in the Troy high school, graduating in 1915.

During his college years at Swarthmore, Det corresponded constantly with his parents. In January 1918, he wrote to his father, "I took the differential equation training although I never may need them, and as far as the general training resulting from mathematics goes I have had quite enough. But since I do not know what I am going to do I suppose I might just as well go on and prepare myself for physics or electrical engineering. It seems a shame sometimes that I cannot decide what to do and then go ahead and fit myself for that work." Perhaps this uncertainty derived from Det's urge to participate in the war effort. Soon after, he proposed in another letter to his father that he leave college and work with the Food Administration Office in Philadelphia. A follow-up letter to his mother asks her to be sure his father answered promptly and added that Aunt Belle approved. He became an inspector, enforcing the law regulating food prices through surprise visits to various food stores. However, as more of the older students enlisted, he wrote to his father, "I can't quite agree with you on the proposition of enlisting and leaving college. I most certainly would never have been content to stay through next year. I suppose a young man feels the call of country more and while cold logic may point to a continuation of college, I have found few red-blooded men who were willing to do so. The nation and the world as I see it is

* "An Old Fashioned Education," *Scribners,* 74 (Nov. 1923), No. 5. Similar opinions were part of the philosophy of higher education developed by his son, who later had an opportunity to implement some changes at The Johns Hopkins University and The Rockefeller University that fostered an individual's love of learning and judged accomplishment without emphasis on course credits and examinations.

facing the ultimate just at present and the most I can do for those principles which I believe right I want to do." In the same letter he added, "The very day after I wrote you last I saw the notice of those two deaths in the paper that you sent me and I thought what a very inappropriate time I had picked to write what I did about naval aviation. But I have only seen the notice of four naval aviators' deaths in the last year, and I not only read the papers but also *Aerial Age* each week and *Naval Air Service*." He stated that he would not go against their wishes but hoped they would see the matter from his viewpoint. He promised to take a long vacation at home before going to the Massachusetts Institute of Technology for ground training. His arguments prevailed and he was learning to fly at Pensacola Airbase in September 1918. At this time Det wrote to his mother,

> I've been up in the air nearly three hours now and drive the plane alone, of course with an instructor in the machine with me. My instructor, by the way, is a Phi Psi from Leland Stanford. It's a wonderful sensation, riding around up there in the clouds, and I wouldn't have missed it for anything in the world. On my first trip the instructor gave me the thrill of a couple of stunts; and I've been flying upside down already; never think anything about it; tho I'd hardly want to do it all by myself just yet. As for fatalities, there hasn't been one here for eight months, and there are always ten or fifteen machines in the air. Gee! I certainly do like it!

Sometime before leaving college to become an aviation cadet, Det had met and courted Helen Alexander Ramsey, a student in Aunt Belle's French language course. Like his grandfather, Abram, he continued his courtship by correspondence, and he and Helen became engaged. In a letter to his mother he gave instructions for carefully choosing an engagement ring. However, the separation was soon over. In December 1918 he earned his wings and was commissioned an Ensign. During the next nine months he was on leave but on call for active service, and in September 1919 he returned to college. He and Helen graduated from Swarthmore College in June 1920.

Det had received a B.S. in electrical engineering. Yet he accepted a position in a brokerage firm in Philadelphia. This did not last long, because in January 1921 he became an instructor in physics at the University of Pennsylvania, simultaneously taking courses in physical measurements, potential theory, and thermionic currents. Subsequent events and letters suggest that during this period he decided that advanced study and research in physics were essential for his future plans. That summer he studied at Harvard, choosing acoustics and advanced calculus in preparation for continuing his graduate studies in physics at the University of Michigan in the fall.

His courtship of Helen must have flourished through this busy period because in September 1921 they were married at Swarthmore in a ceremony performed by his father. Det's part in planning the wedding arrangements was also by correspondence. A letter from his mother assured him that his father had put off getting his haircut until the last moment so that he would look his best at the ceremony. Clearly, Det Bronk had a warm and confident relation with his family, both then and later. In 1946, his father wrote to him, "You have worked hard, sacrificially hard, and doggedly with little pushing except your own gumption, enterprise and ambition, and certainly deserve all the advancement and honors that have come to you—or shall—not to speak of the honor you have put over onto me and the family name. I have wanted to say this to you now, because at eighty-four it doesn't do to put it off." At this time the elder Bronk was living with his daughter in Germantown, Pennsylvania. Isabelle Bronk, a librarian at the University of Pennsylvania, took care of her father until his death in 1950.

In the fall of 1921 Helen and Det went to live in Ann Arbor, Michigan. He continued his graduate studies in physics, and she established their home in a house at 11 Ridgeway, where they lived for five years. Det liked to tell how he rebuilt and improved the house, a necessity then and an avocation later as he acquired a succession of houses at Sycamore Mills near Media, Pennsylvania; Penzance Point on Cape Cod; and Seal Harbor in Maine; and a cabin near Petersburg, New York. He planned the house at Sycamore Mills for several years and worked with the architect constantly as it was being built. During many more years the improvement of the grounds around this house and its maintenance and repair, including new roof shingles, were family projects, as was the construction of the cabin. Only the presidential houses at Johns Hopkins and Rockefeller University escaped his personal handiwork. Helen transformed all of these places into pleasant homes for them and their sons, John Everton Ramsey, Adrian Mitchell, and Mitchell Herbert. An important part of their family life was devoted to frequent hospitable gatherings of friends in their home. The Sunday night dinners at Hill House in Sycamore Mills were notable for good food, friendship, and interesting discussions. Scientists in the Johnson Foundation and graduate students were privileged to join the Bronk family and to meet visiting scientists. Det considered such occasions an enjoyable and valuable part of life, and Helen was an exceptionally gracious and friendly hostess. She participated fully in this aspect of his busy life. In this way she enriched the lives of her children and supported Det's efforts to emphasize essential qualities of a scholarly life. Their excursions to foreign places began in 1928 when they lived in Cambridge, England while Det worked with E. D. Adrian as a post-doctoral fellow. There were many other occasions for traveling together over almost fifty years. The final trip was to attend the 250th Anniversary celebration of the Russian Academy of Sciences in Moscow, shortly before Det's death.

MOTIVATIONS, OPPORTUNITIES, AND DECISIONS

In June 1922 Det received an M.S. from the University of Michigan and was enrolled as an applicant for the Ph.D. degree in physics. At that time he was working with Professors W. F.

Colby and C. F. Meyer on "An Extension of the Fundamental Infra Red Absorption Band of HC1." Four papers published between 1923 and 1927 with Bronk as co-author were based upon his measurements of the molecular spectra of several gases, using a diffraction grating for improved resolution.

In July 1924 he made inquiries for possible positions in engineering and academic physics. Of great significance for his future was a letter to Professor Arthur Willis Goodspeed, at the University of Pennsylvania, asking for a position there. In reply, Goodspeed said that all of the faculty in physics would be delighted to have him back "with a view of becoming a fixture." Because there was no vacancy then, Goodspeed and Charles Blizard Bazzoni came up with a different proposal. The latter was seeking funds to start a group in physics concerned only with research and instruction of graduate students. Bazzoni asked Bronk to consider such an appointment for the specific purpose of continuing investigation of the infra-red spectra of molecules. His letter starts, "As you know I have been developing a research section in this department in which I have endeavored to instill those ideals which are essential to the maintenance of the output in pure science.. . . There has been relatively little difficulty in acquiring a fair equipment, . . . the difficulty has been to get men mentally and temperamentally suitably constituted to carry out such work." This unusual opportunity must have seemed very tempting to Det, but he turned down the offer in September 1924 because a new vista of physical investigations of physiological mechanisms had attracted his attention. At this critical juncture in his professional development Bronk dropped the idea of finishing his degree in physics. He described this sequence of changes in a letter to Bazzoni: "The object of my going into the department of physiology for this year was to get a good grounding in biological and medical sciences which would enable me to effectively carry on research in biophysics. Dr. Randall [H. M. Randall, Professor of Physics at the University of Michigan] has always encouraged me in the belief that there is a large and undeveloped field in the investigation of physical laws in living organisms and has said that he would be glad to have such work carried on in his department. . . ." Bazzoni informed Det that H. C. Bazett, the Professor of Physiology at the University of Pennsylvania, wanted a physicist who was interested in working in physiology. This contact eventually led to correspondence and a meeting between Bronk and Bazett. In February 1925 Bazett asked Bronk to consider coming to his department to oversee work of a graduate student who wanted to do research in x-ray analysis of the structure of fibrin and muscle. In reply Bronk revealed that his main interest was the study of the nerve impulse, "its generation, conduction, transmission across the synapse, and manner of activation."

However, Det was not ready to accept any position. A letter to Bazett states, "I am not certain that I will be able to complete my thesis in time to take my degree this June—possibly not until the latter part of the summer. Last Spring, when I decided to take my degree in both physiology and physics, I found it necessary to give up the field of infra-red spectroscopy in which I had published three researches and in which I had a thesis well under way. In addition to building up a new research technique I have had to do much reading in Physiology and to take such courses as Physiological Chemistry, Histology, and Nervous Anatomy." And in March 1925 he wrote again, "I have finally developed what I believe to be entirely new methods for measuring conductivity, for determining pH, and for amplifying with vacuum tubes without drift or distortion. I am exceedingly anxious to use these methods on some very interesting problems which I have in mind and for which I have the set-ups practically completed. This work will carry me through the better part of another year."

In collaboration with Robert Gesell he published (1926-1928) seven papers based upon research related to his thesis.

They dealt with physiological properties of the respiratory and cardiovascular systems and with neural excitation of secretion from the salivary glands in mammals. He perceived clearly that physiological phenomena provided a rich field for discovery of fundamental biological mechanisms through research based upon physical methods. His letters to Professor Bazett at Penn and to President Frank Aydelotte at Swarthmore seeking an appointment earnestly proclaim that biophysics can be developed into a powerful intellectual discipline for understanding "the living state of matter." His research reports for the years 1923 to 1928 marked the transition of a physicist into a biophysicist concerned with the physical analysis of physiological processes in animals. The scientific roots of Det Bronk in engineering and physics are evident throughout his published research. The early investigations of blood flow exhibit a physicist's concern for improving quantitative measurements and a physiologist's insight into the importance of the neural mechanisms for controlling distribution of blood in an organism, a problem involving a knowledge of physics and of the engineering of machine-like control systems.

Letters written to him in 1924 and 1925 indicate that he was also considering a management or engineering post in commercial firms. For example, in January 1926 he decided not to accept a position with C-T Electric, a Philadelphia company manufacturing electric trucks. Of particular interest are several handwritten preliminary versions of his letters of application in which he develops a description of his qualifications for each post. Such a letter to James Gilbert White is particularly informative about Det's ideas in 1926:

> I took my undergraduate work in electrical engineering at Swarthmore College from which I graduated in 1920. As evidence of the nature of my work while there I might say that on graduation I was awarded the College medal for "character, scholarship, and leadership". I was president of the Student Government Association, a varsity debater, editor of the College

> Annual and Weekly, captain of the varsity debating team, and a member of two varsity athletic teams. My summer months were spent with the General Electric Company, the Western Electric Company and the Pennsylvania Railroad. During the war, I was secretary to the Philadelphia Food Administrator and later an ensign in the Naval Air Corps.
>
> During my last semester in college I spent half time as assistant power engineer with the Philadelphia Electric Company. The year following graduation I was assistant to Mr. L. J. Schumaker, president of several Philadelphia companies. I then decided that the intensive work and habits of analytical thought and investigation that come in research would be a valuable training so I accepted a position as instructor in Physics at the University of Michigan. Last year I was selected as the physicist to carry on research in the medical school in connection with the application of modern electrical methods to biological problems. I have completed four research publications including my doctor's dissertation.
>
> It has seemed to me that the type of position I am seeking exists in your organization. I do not desire purely technical nor research work; the University has assured me an attractive future should I care to continue in that work. Nor do I wish to go into banking or bond sales. The work I am looking for would lie between the two; it would perhaps be assistant to one of your executives, or involve the analysis of reports, or a study of special conditions in connection with construction or operation—anything that would offer a choice for hard work and growth towards a real opportunity in connection with management and administration. To such an opening I think I could bring habits of study, and analysis, willingness to work nights as well as day, the ability to get along well with people, some experience in writing and speaking, and a familiarity with office and business methods.

However, there were other irons in quite different fires. In April 1925 the Professor of Physics at Swarthmore, Winthrop R. Wright, asked Det to consider an appointment for one year as a physics teacher. In his reply Det revealed his aspirations for a career in research and teaching of biophysics and suggested that such a plan might be worked out at Swarthmore:

> My idea rather was that I be appointed assistant professor in biophysics, to divide my teaching time between physics and biology with perhaps allowance for a course in biophysics to be devoted to such things as mechanism of the sense organs, protoplasmic and nervous action, the electrical activity of the body, effect of light on living things—in short the physics and chemistry of life. . . . If you like, the one course would be

> something to develop in addition to the research, I would be the biophysicist which is something different than one of the younger ones in physics or biology, and my position would be recognition of the field and work which I am trying to develop as one of a very few. In fact I think I will be the first person to take the doctorate in physics and a biological science.
>
> I think my training would well fit me for such a cooperative job. My undergraduate work in engineering, my three years of teaching and study in physics including three researches and the handling of Professor Williams' graduate courses during his absence last year, and my work here in the medical school should enable me to tie together three fields which must come closer. We are coming to the point where we are beginning to know quantitatively and experimentally something about the physical mechanism of life, faint ideas of how we see and hear, how the electrical or nervous impulse travels along the nerve, the osmotic and electrical mechanism of secretion, etc. Such things are the basis of life, and the students of today should have some ideas of these most fundamental discoveries that are beginning to break through, should have their curiosities stimulated. I would like to try the job with a more intellectual group than a class of medical students; they are too professional in their interest. To have research work in such a thrilling field going on should help to stimulate real scientific interest among the students and bring credit to Swarthmore for lending support to a new thing which is on the verge of growing rapidly.

The offer made at this time (1925) was not satisfactory. Bronk was assured an opportunity for research and academic advancement in the newly reorganized department of Physiology at the University of Michigan Medical School. He did not want to give up this position without some similar assurances at Swarthmore. However, it seemed to Aydelotte that this young scientist was asking for too much, too soon, especially since he had not yet completed his thesis. Det wrote a long letter to Aydelotte explaining why he could not accept the appointment at Swarthmore. The main difficulty was insufficient funds to equip a new laboratory for research in physiology and biophysics. In principle, they were in agreement that Swarthmore would be an excellent place in which to promote the instruction of young men in this new combination, physics and biology. Det repeatedly emphasized that a department of physiology in a medical school created an atmosphere that tended to isolate him from physics. At Swarthmore he could teach physics and physiology and have the unique position of a biophysicist developing a new discipline. However, emphasis on research was necessary, and this research required a level of expenditure to which science departments of small colleges were not accustomed.

The matter was not dropped because all parties concerned liked Det's general proposal. Dr. Wright, the professor of physics, continued his correspondence with Det and his conversations with President Aydelotte. In a letter to the latter, Wright stated his view:

> It is not proposed that Bronk shall come here for research alone. He would teach one of the courses now given in general physics and the course in general physiology (somewhat amplified) and would plan to develop advanced courses in the common field. Whether this field is important enough to warrant Swarthmore in entering it as a pioneer is for you to decide. It is certain that but few men at present are prepared to develop it and that no man could teach it without the chance to investigate as he teaches. It is a science largely of the future, and we seem to have the chance to enter with the leaders.

In June 1926 Det was granted the degree of Doctor of Philosophy in physics and physiology. Professor Bazett of the University of Pennsylvania Medical School offered him an appointment as Instructor in physiology at Penn or space in his laboratory for research if he joined the Swarthmore faculty. The research opportunities at Penn pleased Bronk as did the academic position at Swarthmore in a post especially designed for him. Aydelotte could not give him research support and Bazett could not give him academic rank. In a manner that was to characterize much of his future activities, Det had arranged a collaboration among interested parties that was mutually satisfactory. The solution satisfied everyone and flourished for many years. In fact, it contained the connections that led to the directorship of the

Eldridge Reeves Johnson Foundation for Medical Physics established at the University of Pennsylvania Medical School three years later.

At Penn, in 1926, he helped Dr. M. H. Jacobs direct the research of graduate students in Bazett's department of physiology. For this he was appointed part-time instructor at $600 a year, and thereby earned official access to the laboratory for experimental research in physiology. A reappointment to the same position in 1927 indicates that this unusual arrangement had proved satisfactory to everyone concerned. Bronk thought that an official, though nominal, connection with the University of Pennsylvania Medical School was of value in relation to his research work in physiology.

From July 1926 through January 1928 Det was busy teaching at Swarthmore, developing an Honors program for premedical students there, and continuing his research in Bazett's laboratory at Penn and in the Marine Biological Laboratory (MBL) at Woods Hole during the Summer of 1927. He had one student, Sam Reynolds, doing research for a thesis as part of the requirements for an M.A. from Swarthmore. At that time Jacobs was director of the MBL, so Det was associated with him there and at Penn in directing the work of graduate students in physiology. In January 1927 he initiated steps toward working a year with E. D. Adrian, whose published work on neuronal action potentials was of special interest to Bronk. At the time he did not know Adrian, and his first letter provides an interesting self-description.

> My training and experience has been as follows. As an undergraduate I specialized in electrical engineering and practiced engineering for a year following my graduation in 1920. I then went to the University of Michigan as an instructor in physics for the next three years. During that time I gave the advanced courses in alternating currents and vacuum tube work, took graduate courses in physics and mathematics, and did research in the field of infra-red spectroscopy. I next joined the staff of the medical school at the University of Michigan and served as instructor in physiology

> for two years until appointed assistant professor last spring. In that time I took the degree of Doctor of Philosophy in Physiology and Physics and did research work on the electrical properties of the submaxillary gland, on the development of a thermoelectric method for determining the volume flow of blood, and on the application of this method to certain problems. This latter work was done in conjunction with Dr. Robert Gesell. Shortly after being appointed assistant professor at Michigan I came to Swarthmore to organize an Honors Course in Physiology. I am also on Dr. Bazett's staff at the University of Pennsylvania School of Medicine.

After he was accepted for research training by E. D. Adrian and A. V. Hill, the next step was to get a National Research Council Fellowship for this purpose. In relation to his application he wrote a letter to Dr. G. C. Huber, Chairman of the Fellowship Committee and a professor of anatomy at the University of Michigan. In it one can read an important viewpoint that guided his later teaching: "I do not wish to become a physicist working in physiology but rather a well-rounded physiologist with a physical and mathematical background." One step in the application procedure was an interview with a member of the fellowship committee. In view of their later association at The Rockefeller University, it is of special interest to know that the interviewer was Dr. Eugene Opie, then on the faculty at the University of Pennsylvania Medical School. Bronk was granted the fellowship for one year. By November 1927 the time of departure for England had been set for late January 1928. Whether he was to go to Adrian or Hill first was unsettled. On Bazett's advice he left that decision to be resolved by those two by the time of his arrival. He started with Adrian at Cambridge, and the work went so well that he did not get to Hill, in London, until November 1928. It is interesting that Bronk asked for an extension of his fellowship but was turned down by the Medical Fellowship Board in a letter dated April 1929.

Opportunities for research and the development of biophysics were numerous for Bronk, with his special preparation in physics and physiology. At this time Det was considering an offer as director of a proposed Institute of Optics at Rochester, and another in the department of physiology at Columbia University. In connection with the serious consideration of these offers, Bronk had many occasions to put in writing his thoughts about biophysics. In a letter to President Rush Rhees of the University of Rochester he wrote: "Would you be willing to widen the scope of the proposed institute to the extent of making it an Institute of Optics and Bio-Physics? I feel very strongly that the time has already arrived when the medical and biological sciences must depend for a large part of their development upon physics." In a letter to Professor H. B. Williams of Columbia University he wrote: "I was considerably influenced by your advice at Cleveland several years ago that a person with my interests would do better to be primarily affiliated with physiology and retain physics as a secondary interest rather than vice versa. My observation of workers who reverse their emphasis has convinced me of the soundness of your view." However, he had heard rumors of an institute for medical physics at Penn. In a letter to Bazett, March 1928, he specifically asked for "any news regarding the medical physics situation." In November 1928 he wrote Bazett that he had refused the post at Rochester because the proposed medical physics undertaking at Penn would be less restrictive in the range of biophysical research. Indeed, he preferred the arrangement with Aydelotte and Bazett at Penn and Swarthmore so much that he also declined the post at Columbia with Professor Williams. This latter decision was particularly difficult because Bronk's interest in the physical aspects of biological phenomena had been stimulated initially by reading papers published by Professor Williams. His decision may have been facilitated when he learned, while in England, that he was promoted to full professor at Swarthmore (after only one and a half years on the faculty) and head of the Department of Physiology and Zoology.

In January 1929 Bronk returned to Swarthmore College from his very successful year of research with Adrian and Hill in England. This experience and his earlier interest in electro-physiology determined the direction of his research for the next ten years. Indeed, his major scientific interest for the rest of his life was the physical basis of the neuronal activity that is fundamental to the regulatory functions of the nervous system. The creative collaboration with Professor Adrian at Cambridge is recorded in the group of papers published during the years 1928 to 1933. During the time when this scientific research was in progress these two men developed a mutual respect for each other's personal qualities that continued in a life-long friendship. Their studies of motor nerve activity helped Adrian establish the generalization that intensity of nervous action on other cells, including other neurons, is measured by the frequency of the nerve impulses in each neuron and the number of active neurons. This was a fundamental generalization for which Adrian later received (with Charles Sherrington) the Nobel Prize. These generalizations about the neural code were essential to understanding neural control of physiological phenomena, such as cardiovascular and respiratory regulation.

Bronk was now Professor of Biophysics and Dean of Men at Swarthmore. The arrangement seemed permanent-even a new house was to be built for him. He continued his association with Bazett, depending upon the laboratories in the physiological department at the University of Pennsylvania for his research purposes. Through Bazett's influence he was now being considered for the post of director of the new Foundation for Medical Physics to be established in the Hospital of the University of Pennsylvania. It was to be housed in a new building under construction at 36th and Spruce Streets-the Maloney Clinic Building. Alfred Stengel, the Vice-President in Charge of Medical Affairs and Professor of Medicine, was responsible for establishing the new Foundation and its relations to the University's hospital and the medical school. Through Professor Bazett he knew of Bronk's interest in the project and of his professional qualifications for participating in such an undertaking.

In February 1929 Stengel wrote to Bronk,

> I am delighted to learn, from Dr. Bazett, that you have returned to this country. I am writing to ask if you would not be good enough to see me sometime at your convenience when you may happen to be in Philadelphia, to let me go over certain matters regarding the Physics unit in the new Medical Clinic. I have been anxiously expecting your return, as you know of our needs and have been helpful to us in arriving at some preliminary plans. . . . I would wish, at the same time, to discuss with you other matters regarding the eventual development of this project and the possibility of your interesting yourself in it with us.

The conversations initiated by this letter led directly to an acceptance by Bronk, in April 1929, of an appointment as Johnson Professor of Biophysics and Director of the Eldridge Reeves Johnson Foundation for Medical Physics. In October, a letter to Stengel indicates that a staff had been appointed, that the laboratories were available, and that several investigators, including Ragnar Granit from Stockholm and W. A. H. Rushton from Cambridge, were arriving for a year or more of research in the new facility. Bronk's first major effort to create a unique institution devoted to advanced study and research was under way.

One year later the list of investigators was increased by the addition of John Donal, Frederic Gibbs, Harry Grundfest, and H. K. Hartline. Most of these investigators remained a few years, completing some research project and leaving for permanent positions elsewhere. Hartline, who arrived on the scene in April 1931, was associated with Bronk for forty-four years, continuing his research on vision, and moving with Det as the latter accepted administrative posts in a succession of new pastures where the fostering of science and graduate education on an ever broader basis seemed especially feasible.

In a letter to Adrian in May 1929 Bronk reported his acceptance of this new position as Professor of Biophysics at Penn. "I shall miss the delightful surroundings here [Swarthmore] and the student contacts but I have been so loaded down with teaching and administrative duties this term that I have practically no time for research. I feared this might grow into a permanent condition so I decided it was better to make the change now. I am sure that my period of training with you was one of my strongest qualifications . . . I will try not to disgrace you." It is interesting to read a similar letter that Bronk had received six months earlier, in November 1928, in which Adrian writes:

> I have one bit of news for you. You may have observed that I was more than usually moody in September. The fact was that I was thinking over the offer of a job much as you were in April [1928]. The job in question is a Royal Society research professorship like A. V. Hill's . . . I finally decided that I might as well accept, if only to encourage future candidates by lowering the much too high standard already set by Hill and Starling. So after this term I shall have no college teaching and only some advanced lectures to keep me up to date.
>
> See what you are responsible for! If we hadn't continued to deliver the goods in our work together I should have accepted the omen and refused the post. As it is there are awful misgivings in the background though naturally I can't pretend not to feel a great deal of pride at having the job offered to me.

Clearly, association in research for nine months had made these two men confiding friends—a relation that continued until Det's death.

Bronk regarded the Johnson Foundation as an exceptional opportunity for devoting all of his time to research and the teaching of graduate students. He continued a formal connection with Swarthmore as Lecturer. Indeed, from 1929 to 1932 Aydelotte consulted him on many administrative matters relating to faculty appointments, the Honors program, and the construction of a new laboratory building. From 1932 to 1933 he was was Director of the Department of Physiology and Zoology at

Swarthmore and was appointing and discussing the appointment of faculty as needed. This unorthodox relationship had begun in 1926 with a major appointment at Swarthmore and a research-oriented connection with Bazett at Penn. After 1929 it was a major research appointment at Penn and an academic and administrative connection with Swarthmore. This pattern of simultaneous important posts in several institutions was to characterize his future career. It reflected his predilection for simultaneous involvement in research, teaching, and administration. In fact, in the summer of 1932 he was off to England to work with Adrian again. In May 1934 these formal arrangements with Swarthmore were terminated, but he continued to advise Aydelotte, on request, for many more years.

RESEARCH, BIOPHYSICS, AND THE UNITY OF SCIENCE

One of Det's persistent research interests was the investigation of the neural control of the cardiovascular system in mammals. His work with Gesell provided a preliminary knowledge of cardiovascular physiology and some aspects of the neural control of muscles and glands. His year with Adrian put him in direct contact with ongoing research that was basic to understanding gradations of neural activity in terms of number of active neurons and frequency of impulses in each one. With this knowledge of the neural code and the new techniques for recording from single fibers, he and his collaborators provided, over ten years, essential experimental evidence for our present understanding of the neural regulation of blood pressure.

In physiological systems the neural regulation requires receptors for detecting and measuring the output of the effectors, as any good engineer would know. The transduction of tension and pressure into graded activity of individual neurons was investigated during the years 1929 to 1936. Among the receptors studied were Pacinian corpuscles in the mesentery and pressure receptors in the carotid sinus. Thus, the activity of some recep tors essential for cardiovascular regulation was described quantitatively. His first major paper (with G. Stella) on this subject, "Afferent Impulses in the Carotid Sinus and Aortic Nerves," was published in 1932. During the same period the patterns of efferent impulses from sympathetic ganglia were studied in relation to well-known cardiac reflexes. His last paper (1941) on this subject (with R. F. Pitts and M.G. Larrabee) concerned the role of the hypothalamus in cardiovascular control via the sympathetic nervous system, an investigation begun six years earlier. Thus, his investigations included receptors for detecting changes in blood pressure, the central control centers receiving the sensory signals, and the properties of the efferent neuronal systems directly controlling the heart rate and diameter of blood vessels-two of the mechanical factors determining blood pressure. In Det's interpretive reviews of these experiments (see *Harvey Lectures,* 1934), one can readily sense his delighted appreciation of the remarkable properties of this physiological control system. The implications for medical science and for applied physiology were clear to him, forming later a strong intellectual basis for his practical efforts in aviation medicine during World War II. In 1946, he summarized his views on "The Physical Structure and Biological Action of Nerve Cells, with Some References to the Problems of Human Flight."*

A central problem in understanding the action of any system of nerve cells is the description of the processes mediating the excitation of one nerve cell by another. Such synaptic regions occur in sympathetic ganglia, an anatomical component of the neuronal system controlling cardiovascular processes. In 1935 Bronk published a paper (with R. J. Pumphrey) on "Response of a Sympathetic Ganglion to High Frequency Stimulation." He perceived that a peripheral ganglion provided an excellent preparation for studying trans-synaptic excitation. It was gen

* *American Scientist,* 34(1946):55-76.

erally recognized that properties of such synaptic transmission were basic to the regulatory functions of nervous action. Two evident research problems involved measuring the temporal properties of a neuronal synapse and determining the physical and chemical mechanism involved. In collaboration with M. G. Larrabee and a few others, he devoted much research to this subject from 1934 to 1952. These extensive studies revealed important general properties of trans-synaptic excitation, including the discovery of prolonged effects of previous activity which was a new facet of temporal facilitation. The phenomenon was of special interest at the time because of its possible relation to short-term memory, a mysterious property of some parts of the brain. Indeed, Ragnar Granit selected this discovery, reported in 1947 by Bronk and Larrabee, to be one of the most important contributions from The Johnson Research Foundation. (See Citation for Franklin Medal from the Franklin Institute, 1961.) He stated, "If one of Bronk's contributions should be singled out for special emphasis, surely this is the one to choose. It has had a great influence on physiology." Other scientists emphasize instead his contributions to cardiovascular physiology. In the *Physiological Reviews* for January 1976 one can read that Bronk and Stella "had already elucidated in principle most of the fundamental properties of vasoreceptors known up to now." The papers referred to were published more than forty years ago.

Synaptic excitation involves release of a chemical from presynaptic nerve terminals and excitation of the postsynaptic cell by this chemical. The studies of chemical excitation of nerve cells and axons (1937-1946) were relevant to the finer biophysical analysis of this physiologically important phenomenon. The characteristic response to chemical excitation is a prolonged train of impulses. The cell is responding to potential gradients in the membrane caused by current flow from intrinsic sources of electromotive force. The chemical agent merely produces the conditions at the cell membrane that permit this electric current to flow. Such self-excitation is a property of axons too, provided the calcium is removed from the environment of the nerve. These studies of self-excitation of axons induced by chemical changes revealed an essential roll of calcium in stabilizing the excitability of axons at a sensitivity permitting conduction of impulses but preventing spontaneous production of impulses. The observations are relevant to the biophysical analysis of electrochemical events in excitable membranes and the role of calcium in such structures. Bronk's initial interest in these phenomena was recorded in 1930, and he fostered such research, with collaborators, as late as 1952, when he was President of The Johns Hopkins University.

Bronk measured, in 1928, the heat production of active nerve, using methods learned from A. V. Hill. From this experience he published, in 1931, a paper on "The Initial and Recovery Heat Production of Vertebrate Nerve." Ten years later he and his collaborators reinstituted investigations of these same energy transactions in terms of the oxygen uptake associated with nervous action, in trans-synaptic excitation and in axonal conduction. These experiments were made possible by developing precise methods for measuring the increased oxygen utilization by active nerve cells. Such biophysical studies relate the electrochemical events at the neuronal surface to the oxidative metabolism essential for maintenance of the ionic contents of nerve cells. Det continued an intellectual interest in this kind of research for the rest of his life. Even when his administrative responsibilities were heavy, he liked to take time to discuss any new finding that related to "the manner in which the organized living state [of cells] is maintained by an expenditure of energy."* He considered investigations of energy transactions in cells to be fundamental for the eventual unification of the conceptual structures of physics and biology, that is, biophysics.

* *Journal of Applied Physics,* 9(1938):139-142.

In 1938, after ten years of research and teaching in biophysics, Bronk evaluated its role in the further development of biology. He argued that the biology of cells must be understood in terms of molecular structures and their temporal changes, as "a problem in physical chemistry."* Therefore, basic biology was to be conceived as the study of structure, biochemistry, and biophysics of cells. He emphasized the need for advanced study of physics and biology because "there is a large body of facts and theories concerning living organisms which is unknown to the physicist."* Obviously, this opinion was derived from his own pioneering efforts twelve years earlier when he decided to take his advanced degree in physics and physiology. This summarizing paper, "The Relation of Physics to the Biological Sciences," closes with an expression of one of his life-long motivations, "the satisfaction which comes to workers in this field, for we have a rare opportunity to glimpse the essential unity of science. To comprehend this is the final objective of every natural philosopher."*

He liked to perform experiments with a potential for discovery but disclaimed interest in systematic, goal-directed research. The diverse experiments were motivated by progress toward a basic biological goal—the description of physiological mechanisms in terms of the properties of the participating cells. Thus, for regulation of blood pressure, the experimentally separable cellular actions were: afferent impulses generated by pressure receptors, activity of hypothalamic neurons processing these incoming signals, efferent impulses activating neurons in peripheral autonomic ganglia, impulses from ganglionic cells modulating the heart rate and activating smooth muscle cells to adjust the diameters of blood vessels. Clearly, the integrated physiological system provided the framework for correlating these experiments. Such a synthesis of cellular events into a mecha

* *Journal of Applied Physics,* 9(1938):139-142.

nism essential to the integrity of a living animal fascinated Det. In 1939 he wrote, "Only if we think in terms of cellular units and recognize that they are labile structures whose properties are continually fluctuating can we conceive of the fluctuating patterns of behavior carried on by fixed arrangement of cells and fibers." For him, research was an adventure into unknown territory in association with a few like-minded colleagues. He often visualized the ideal laboratory as a large room with several groups of investigators conducting diverse experiments all related to a basic biological problem. Later, when his administrative duties dominated his attention, he seemed content to be in or near a laboratory and to inquire how the last experiment had worked out. He enjoyed an account of a good experiment whether or not he had any part in it. He expected me to relate to him any unusual experiment that I came across in my reading. Two themes permeate Det's efforts to conceptually integrate his experimental observations. One was the common features of the neuronal signaling utilized in the control of a variety of physiological systems. The other was the necessity for a continuous flow of energy for maintenance of the excitability of the cellular components of these systems. His preoccupation with these aspects of organized systems of living cells plainly reflects his original interest in engineering and physics.

A REVERSIBLE TRANSITION, THEN PHYSIOLOGISTS IN WAR

In 1939, for reasons that are not clearly evident, Bronk negotiated and accepted a position as Professor of Physiology and Head of the Department of Physiology in the Medical School of Cornell University. In February 1940 he received the letter from President Edmund E. Day formalizing this appointment. At the same time a proposal to develop biophysics at the Massachusetts Institute of Technology (MIT) was being considered. And in March he was corresponding with Ralph Gerard on the merits of accepting an appointment as Head of

Physiology at the University of Chicago Medical School. Bronk believed that more participation in the teaching of physiology was essential for biophysicists and was pleased that Cornell welcomed him and a group of his associates. The simultaneous offers from Chicago and MIT promoted comparisons and indecision. Perhaps the reluctance for accepting "the group" at MIT was a major factor in his refusal of the offer to help develop biological engineering there. The letter from Karl Compton acknowledging Bronk's refusal of the post indicates his continuing contact as a member of the Visiting Committee for the Department of Biology and Public Health. In his letter refusing the post at Chicago, Det explicitly stated that his main interest then was to create a "school of physiology along lines that do not now exist." He thought this could be done better at Chicago or Cornell than at Penn. He favored Cornell in part because he hoped to utilize the talent at the Rockefeller Institute for Medical Research in the education of his graduate students.

It was during the spring of 1940 that Det began having doubts about leaving the University of Pennsylvania. In May 1940 a letter from A. N. Richards (Vice-President in Charge of Medical Affairs) indicates that he and Richards were informally discussing his remaining at Pennsylvania. In an exchange of letters between Day and Thomas S. Gates (President of the University of Pennsylvania) there is some basis for believing that President Gates may have failed to make clear to Bronk that the prospects for him, the Johnson Research Foundation, and biophysics at Penn were good. There was a brief period in which A. N. Richards of Pennsylvania and President Day of Cornell were discussing by letter the best way to help Det resolve his uncertainties. As late as July, President Day offered him a semester's leave with pay during which he could re-assess his decision. However, Det decided to make the move, and the transfer to Cornell was completed by September. The move was made by Det and other scientists from the Johnson Foundation, including H. K. Hartline, M. G. Larrabee, F. Brink, P. W. Davies, A. J. Rawson, J. P. Hervey, and G. A. Millikan. For one year they taught physiology to the medical students and pursued their research interests at 68th Street and York Avenue, New York City. A letter from Richards to Det in February 1941 indicates advanced plans for his return to the University of Pennsylvania. The excursion was terminated by Bronk who resigned in March 1941 and returned to his former post in Philadelphia. He took all of his associates with him. A copy of a letter from Richards to President Day in January 1941 clearly indicates that Richards was the prime mover in arranging for Bronk's return to Pennsylvania.

Two features of this episode are explicitly documented in Bronk's available correspondence. In his letter of resignation to President Day and Dean Ladd he states that as the year progressed he began to realize that investigators and graduate students concerned with physiological biophysics need ready access to physicists, chemists, mathematicians, and general biologists. They thrive in an intellectual atmosphere where such scholars are present. The exclusively medical environment of Cornell Medical School and the New York Hospital did not, in his opinion, provide the best circumstances for biophysics to flourish as an independent intellectual enterprise. On the contrary, in a medical school, biophysicists were generally regarded as technical specialists who could help others in medical research. As mentioned earlier, this was a philosophy that Det wanted to avoid in his own development as a biophysicist. The other explicit aspect of this event was the numerous enthusiastic letters he received from faculty members and department heads at the University of Pennsylvania welcoming him back. It is interesting to note that Richards and Bazett now agreed to let Bronk and the research staff of the Johnson Foundation participate in teaching physiology to medical students as they had done at Cornell.

The Institute of Neurology was created at the University of Pennsylvania Medical School in 1937 to provide closer professional relations among clinical neurologists and those engaged primarily in neurophysiological research. It was conceived as an intellectually cohesive organization that could provide a focus for cooperative efforts among members of various departments in the hospital and the medical school. Det was the director and principal organizer. The future of this project seemed uncertain when he went to Cornell. Among the letters welcoming him back to Penn in March 1941 is one from Dr. Perry Pepper in which a problem connected with the Neurological Institute is eagerly turned over to Det months before he actually returned to the Johnson Foundation. This Neurological Institute, with original purposes intact, continues today—evidence of one more of Bronk's ideas that resulted in a viable organization for the advancement of research on the nervous system and promotion of procedures to make fundamental knowledge available for use in solving clinical problems.

The correspondence between Bronk and the Comptroller at Penn from 1939 to 1940 suggests that funding of research and salaries for all the members of the Johnson Foundation was becoming very uncertain at the time. In contrast, the balance sheet for 1942, after Det's return from Cornell, was much improved. One notes the new sources of support related to the military problems—National Research Council (NRC), National Defense Research Committee (NDRC), and Office of Scientific Research and Development (OSRD). Perhaps this financial situation in 1939 and 1940 was a factor in Det's decision to transfer to Cornell Medical College, a move that permitted him to hold his group together financially. The return to Penn, for intellectual and professional reasons noted above, may have been facilitated by this increasing emphasis on war-related research, which had begun for some members of Bronk's group while at Cornell Medical College. In February 1941 Bronk had received a grant from the Division of Medical Sciences of the NRC in "support of your investigations in relation to the defense program." By April 1941, Bronk and Hartline were concerned with research on visual acuity in relation to the problems encountered by pilots flying at night. During the year at Cornell the research activities of some members of the staff were related to other military problems, particularly to cardiovascular and respiratory physiology during stress, as in submarines or high altitude flying. This trend accelerated after the return to the Johnson Foundation. Bronk became involved as a consultant in medical research related to military problems. A. N. Richards was head of the medical group (Committee on Medical Research) directing this national effort within the OSRD created by Vannevar Bush. Perhaps this was a major reason why he urged Bronk to return to the Johnson Foundation. Their previous friendship became an even closer association during the war years.

In a letter from Frank Jewett to Walter Miles in 1946, we learn a further explanation of why Bronk left Cornell Medical School and returned to the University of Pennsylvania in 1941. Bronk was sure that the United States would become involved in the war while President Day was of the opposite opinion at that time. Consequently, Day did not approve of Bronk's devoting his time and research efforts increasingly to war-related problems. At the University of Pennsylvania he was supported in his opinions, and his expanded efforts in war-related research were fully approved. His return to the Johnson Foundation merely transferred his base of operation because the research contracted through the NRC was continued and extended there.

Bronk's primary role in World War II was played out in Washington rather than in personal performance in the laboratory. He was effective in linking research scientists to the war effort through grants and contracts at the University of Pennsyl vania and elsewhere. He served the NRC as a Member-at-Large in the Division of Physical Sciences from July 1940 to June 1941. Then he received and accepted an appointment for three more years in the same division under the chairmanship of Dr. L. P. Eisenhart. However, Det was also a physiologist, and in September 1941 he became a member of the Committee on Aviation Medicine of the Division of Medical Sciences of the NRC under the Chairmanship of Dr. Lewis Weed. In April 1942 Weed asked Bronk to be chairman of a subcommittee of the Committee on Aviation Medicine (CAM) to advise the medical services of the Army and Navy Air Corps on problems related to oxygen and anoxia.

The war focused the attention of scientists upon practical problems defined by the military requirements. Primary concern with advanced study and basic research had to be put aside in favor of investigations and effective use of available knowledge for quick solutions adequate for military purposes. For Bronk this meant increasing involvement in aviation medicine insofar as the physiological stresses of flying created special medical problems requiring investigation. He also perceived that an educational effort was needed to permit flyers and their doctors to understand the physiological origins of these special aspects of aviation medicine. He conceived and directed the recruitment of a corps of physiologists for this purpose. General David H. W. Grant, the Air Surgeon, and his aide, Colonel Lloyd E. Griffis, backed his proposal, and Bronk was appointed Coordinator of Research in the Office of the Air Surgeon. For this project he was uniquely qualified by professional experience in physiology and by his flight training during World War I. He began recruiting young physiologists into the Air Force, enabling these physiologists to utilize their specialized talents to serve the military needs of the nation. These officers worked at all major flight training centers and at air bases elsewhere.

Some were engaged in research work within the military organizations.

Two major problems under investigation were the adverse effects of moderate deficiency of cerebral oxygen supply and the improvement of goggles utilizing infra-red radiation for making terrain visible to aviators landing on a dark field at night. Bronk was particularly interested in the physiological aspects of the former problem. His observations on the sensitivity of neurons to oxygen deprivation and his knowledge of the cardiovascular system fostered his interest in the delivery of oxygen to the brain. This interest was intensified by his involvement in physiological problems encountered by aviators flying at high altitudes or making pull-outs from steep dives during combat. He was elated when Philip W. Davies developed a form of oxygen-electrode suitable for measuring the concentration gradients near blood vessels on the surface of the exposed cortex of a cat. Now a direct measure of changes in the delivery of oxygen to the neurons in the brain was available. Bronk reported this development in the "Transactions of the American Neurological Association" in 1944 and in a penultimate research report (with P. W. Davies) in 1957.

In 1947, Richards, as Chairman of the Committee on Medical Research of the OSRD, wrote to all subcommittee chairmen a letter thanking each for his emergency services during the war. His letter to Det comments upon Det's effective, simultaneous leadership in the Johnson Foundation and in the CAM and as Coordinator of Research in the Air Surgeon's Office. (Note again Det's unusual flare for focusing his efforts on a common problem through several channels of management.) His contributions to the war effort received national and international recognition. In June 1948 he was awarded the insignia of an honorary Officer of the British Empire for "invaluable services to the Allied cause in medical research and development." The commendation for exceptional civilian service reads:

> For exceptional service to the Army Air Force [AAF] in initiating and supervising a program of aviation physiological training which greatly increased the safety and efficiency of all flying personnel. Through his extraordinary administrative initiative and skill, he achieved the successful establishment and fulfillment of the AAF altitude and training program, conceived and established the AAF night vision training program, and contributed outstandingly to the national welfare through the advancement and application of the knowledge of aviation medicine.

During the war, Bronk was heavily involved with work in national organizations, starting with the NRC and culminating in his appointment as Coordinator of Research, Office of the Air Surgeon, Headquarters Army Air Forces. He was a Special Consultant to the Secretary of War. In 1942, he became Chief, Division of Aviation Medicine, Committee on Medical Research, OSRD. In 1945, the war work was over. Det was directing the Johnson Foundation as it converted to more usual research activities. In the postwar years the scientific enterprise continued as an integral part of many federal agencies, and Bronk was a prime mover in the efforts to define the role of science in national affairs. His experiences and associations during the war led to his appointment, in 1945, as Foreign Secretary of the National Academy of Sciences (NAS) and, in 1946, as Chairman of the NRC. In addition, he was a member of: Naval Research Advisory Board; Scientific Advisory Board, Army Air Forces; Commission on Educational Scientific and Cultural Cooperation, State Department. He was spending much time in Washington.

PRIORITY: PERSONAL RESEARCH OR SCIENCE FOR HUMAN WELFARE

Det served the NRC several years before he became a member of the NAS in 1939. His first official role was in 1936 as Representative of the American Physical Society in the Division of Physical Sciences. In 1937 he was on the Executive Committee of the NRC. From 1940 to 1945 his participation in NRC affairs was greatly extended. He was active in the Division of Physical Sciences and the Division of Medical Sciences and was chairman of the Committee on Aviation Medicine. A major reorganization of the NRC occurred in 1946 when the Council of the Academy became responsible for appointing the Chairman of the NRC, making him subject to dismissal by that Council. Thus, the NRC was linked more firmly to the NAS through its Council, which was composed of elected members of the Academy. A letter from Frank Jewett appointed Bronk as Chairman of the NRC beginning July 1, 1946. He was thus presented with an opportunity to develop the postwar NRC, subject to the general approval of the Council of the Academy. In June 1946, Jewett reported, "In accepting the chairmanship as Dr. Harrison's successor, Dr. Bronk has relinquished a large part of his responsibilities at the University of Pennsylvania so as to be able to devote a major part of his time to administering the affairs of the Council [NRC]. The Council is thus assured of what is essentially full-time administration of its operation—something which the largely expanded work of the Council demands." Many of the peacetime versions of wartime problems handled by the OSRD were dealt with by expanding the committee structure of the NRC. Fortunately, the confusion of purpose represented by the proposal that the NAS-NRC should function as a national science foundation in administering support of basic research and advanced education of scientists had been put aside by 1946. Earlier (1945), Bronk had testified in favor of this proposal at Congressional hearings. Now he could, and did, proceed in the reorganization of the NRC, guided solely by the original chartered purposes, as reaffirmed by Jewett in a major report to the Academy in 1947. Newton Richards, as Vice-President for Medical Affairs, enabled Det to make the necessary arrangements with Penn so that he could become chairman and also continue as Director of the Johnson Research Foundation. Of great inter est is the fact that Richards became President of the NAS one year later. Again, he and Det were working together in promoting science at Penn and nationally through the NAS and the NRC.

Det took the chairmanship of the NRC in the belief that he could have great influence on the course of the imminent changes in national policy for science and its role in society. He declined a full-time appointment because biophysical research and the development of the Johnson Foundation continued to be a major interest. He believed the two activities to be mutually supportive, arguing that much of the work of the NRC had a scholarly scientific aspect, and that a link to national scientific affairs was important to the University. In accepting the chairmanship Det enjoyed the confidence of his predecessor, Ross G. Harrison, who expressed his satisfaction in leaving the NRC in such capable hands. As Chairman, Bronk was the instigator and prodder of many enterprises in the national interest. The participants were scientists and engineers confronted with a wide variety of national problems that required the mobilization of available knowledge and specific recommendations for further research. Some specific examples are: the Committee to Assess the Biological Effects of Atomic Radiation, the Committee on Sensory Devices, the Chemical-Biological Coordination Center, and the Highway Research Board. Of special interest to him were the expanding fellowship programs administered by the NRC and the creation of the American Institute for Biological Sciences. Available letters reveal that his speeches were effective in delineating the problems and motivating the necessary efforts. Bronk was a focus for coordinating and promoting the activities of those concerned with "human ecology." In one of his statements to Congress, Bronk described himself as "a biologist and a physicist who is concerned with the influence of modern technology on human welfare." A letter from Jewett in 1949 suggests that Det had successfully revitalized the NRC. Jewett writes:

> Tonight before dinner I lay down on the sofa for a rest and picked up the first paper on the pile. It was the minutes of the Academy Fall Meeting. I went through it casually, old stuff—until I came to the end—your report of stewardship of the NRC. Then I got a shot of nitro-glycerine and a great uplift.
>
> It was a grand story of achievement and one that should give you a feeling of satisfaction—I only hope you can find time to keep a guiding hand on NRC affairs even if you have to delegate more to others.
>
> While reading it I confess to a small twinge of regret at not having had a hand in it. However, I solaced myself a bit with the thought that in an antediluvian age, "when all the girls were twenty-one", I did have a hand in persuading you to take command of the battered ship. You're a skillful navigator and I don't think you learned it all on Vineyard and Nantucket Sounds either. You have put the NRC where I've always hoped it would be—as the real "guts" of the Academy structure. The combination of the freedom afforded by the Academy charter and the virility of a dynamic Council is unbeatable and a great thing for science and for America.

During this same period Bronk, as Foreign Secretary of the NAS, was very active in helping to restore normal relations with scientists in postwar Europe.

Bronk was prominent among those who decided that administration of science in the postwar period was of greater importance for the advancement of science than the continuation of a personal research career. The transition was not abrupt and not without difficult decisions. In accepting appointment as Chairman of the NRC, Bronk wrote to Jewett, "I am so grateful for your sympathetic understanding of the reasons why I felt unable to accept the post on a full-time basis, due to my loyalties and obligations to the University of Pennsylvania and my strong desire to continue research." The personal circumstances for his insistence on continuing at the Johnson Foundation while Chairman of the NRC are enhanced by a letter from his wife, Helen, reminding him that, "direct contact with important research is your first and most important interest." After six other relevant points she concluded: "a quotation keeps going through my head-'What shall it profit a man if he gain the whole world, if he loses his own soul?' Your soul is your research work." The last remark underlines Det's entire professional history after his 1927 choice of an academic career at Swarthmore and at Pennsylvania as opposed to engineering management. The documents and letters over twenty years reiterate his strong motivation to participate personally and directly in research. After accepting the Presidency of The Johns Hopkins University in 1949, and of the NAS in 1950, a gradual weakening of this basic premise occurred—as happens to so many competent scientists who are also able administrators. For a while he continued professional participation in research through a number of collaborators who were also interested in the biophysical aspects of a neuronal activity and trans-synaptic excitation. He always wanted to know what experiments were being planned, and he liked to contribute to discussions about possible interpretations of new data. At Hopkins and later at The Rockefeller University he maintained an office in the research buildings. The last research paper bearing his name (co-authors, P. Cranefield and F. Brink) appeared in 1957, about thirty-four years after his first. The title of the first paper was "The Structure of the Absorption Bands of Certain Organic Gases and Vapors in the Near Infra-Red," published in the *Physical Review,* and the last one was, "The Oxygen Uptake of the Peripheral Nerve of the Rat," published in the *Journal of Neurochemistry.* He was sixty years old; further contact with experimental research was so peripheral to the focus of his activities that he no longer felt personally involved. Over all these years, on appropriate occasions, Bronk recapitulated related aspects of his research interests and created a tentative synthesis of the accumulated observations. This series of papers constitutes a carefully organized account of that which he selected as most interesting and significant in his research and the related work of others. Among these résumés are his Harvey Lecture (1934) on "The Nervous Mechanism of Cardiovascular

Control," the Cold Spring Harbor Symposia talk (1936) on "The Activity of Nerve Cells," the S. Weir Mitchell Lecture (1938) on "Cellular Organization of Nervous Function," the Symposium on the Synapse lecture (1939) discussing "Synaptic Mechanisms in Sympathetic Ganglia," and a paper in a symposium on Chemistry and Medicine (1940) entitled "The Nervous Regulation of Visceral Processes." The final talk of this kind was his Croonian Lecture delivered in 1949 on "The Rhythmic Action and Respiration of Nerve Cells." (This paper, never prepared in final form for publication, exists as a series of notes and references to specific slides.)

His early research on the nervous control of the cardiovascular system has become incorporated in textbooks of physiology as a permanent contribution to physiological science. Largely for this research he was granted membership in the NAS in 1939. His achievements as a scientist were recognized abroad as well as in the United States. In 1948 he was elected a foreign member of the Royal Society of Great Britain. The *Philadelphia Inquirer* noted that only two other University of Pennsylvania scientists had that distinction, A. N. Richards and Benjamin Franklin. In 1953 he was elected a foreign member of the French National Academy of Science. In 1958 the Russian Academy made him a foreign member, citing his contribution to neurophysiology, biophysics, and aviation medicine.

Det Bronk developed as an inspired experimental scientist with broad interests in the scholarly enterprise. His talents for leadership were evident to older colleagues, and opportunities flowed toward him. He arrived on this scene at a time when academic leaders were expected to continue their creative scholarly work. He learned gradually that innovative leadership in activities supportive to the scientific enterprise precluded direct participation in the search for new knowledge. The mobilization of scientists for service in World War II fostered Det's transition from the laboratory to the administration of scientific activities on a national scale. One source of his personal motivation toward this task is well expressed in a paragraph from testimony at a Congressional hearing in 1945.

> After a war in which we have been forced to destroy vast quantities of our natural resources, it is well to give thought to the future sources of national strength. Fortunately, our greatest natural resource is one that need have no limits. I refer to our knowledge of the physical universe, our knowledge of plant and animal life, knowledge of the workings of our own bodies in health and disease. Such knowledge is a resource that can be increased indefinitely for the common good. Unfortunately, it can be lost through indifference and neglect. Accordingly, thoughtful citizens should derive confidence from the determination of Congress to insure the vigorous development of scientific research, so as to increase our national welfare and to prepare ourselves for the unforeseen problems of the future.

In retrospect this personal history seems inevitable. Det was dedicated to the furtherance of science, but his interests extended into other areas of scholarship. As an inspired leader in advanced study and research he had a deep concern for all creative people. He admired and wanted to understand creative scholars generally. One of the recurring topics in his discourse and writing was the desirability of unifying knowledge. He simply liked the idea of eventual unity of all knowledge. This frequently expressed theme was sometimes restricted to the unity of scientific knowledge. He found great satisfaction in bringing together scientists in many disciplines to pool their knowledge in relation to a national problem. Even in the practical uses of science, as in the NRC, he liked to describe how the inspired insight of one person into the creative research of another could eventually lead to some unusual synthesis in still another mind. He liked to point out that this coordinated utilization of available scientific knowledge to solve a problem was evidence for the essential unity of all science. In the absence of unity in the logical structure of basic science he found evidence for an intrinsic unity in the interlocking physical, chemical, and biological process characterizing many natural systems. In this idea he found strong personal motivation for his work as chairman of the NRC.

The American Institute of Biological Sciences (AIBS) and the American Geological Institute (AGI) were created by scientists who were concerned with the fragmentation of their discipline into subspecialties. Bronk played a key role in the formation, in 1948, of the AGI, as described in *Geotimes* for October 1973. This Institute, fostered by the NRC through Det's efforts, provided the mechanism for promoting the development of earth sciences as a unified body of knowledge. Somewhat earlier, 1947, Bronk had a similiar pivotal role in creating the AIBS. He described these events in *BioScience* (1972):

> The schism between the zoological and botanical sciences is difficult to recall now that molecular biology comprises both, and "biology" has been broadened to "the life sciences" which include the behavioral sciences, too. In these times when life scientists are much concerned with the quality of life and the environment, there is no longer need to justify our organization that embraces all the biological sciences and their social adjuncts. The hopes of Butler and Cleland and Steinbach, of Chambers and Fenn and Griggs and their colleagues, have been fulfilled. The AIBS is thus a heritage from them to those who believe that knowledge cannot be contained within boundaries.

A striking characteristic of Bronk's total bibliography is the extensive overlapping periods of writing on (1) experimental investigations, (2) the nature of the scientific enterprise, (3) the role of science in society and national affairs, and (4) the importance of higher education in our culture. His thinking on a wide variety of problems concomitantly is evident. In an interview published in the *Baltimore Sun* in 1949 he explained that relaxation for him was to shift his attention to a different concern or set of problems rather than to stop working. Thus, each day was divided into several periods, each devoted exclusively to a particular facet of his manifold interests. For relaxation he also went sailing or skiing. On such occasions he devoted total attention to the immediate activities with exclusion of all other matters. He apparently had a well-developed ability to select and control the focus of his attention, including an assigned period before sleeping used for reading, usually history, biography, and literary commentaries like John L. Lowes' *The Road to Xanadu* (a favorite). Novels were avoided, according to his own statement.

IN SUPPORT OF RESEARCH AND THE SCHOLARLY LIFE

In 1948 the trustees of The Johns Hopkins University were seeking a successor to Isaiah Bowman. In Bronk's files there is a copy of a letter sent to Charles Garland, Chairman of the Board, by S. R. M. Reynolds, a staff member of the Carnegie Institute situated at Johns Hopkins, recommending Bronk for the post. There is also a letter from Garland to Dr. Reynolds stating that his was the first letter to be received by the presidential search committee urging consideration of Det. Garland indicated that the Board's deliberations rapidly focused upon Bronk and his credentials. These letters are of special interest because Sam Reynolds was the Research Assistant who, in 1928, kept Det informed of events at Penn and Swarthmore during the latter's sojourn with Adrian in England. One can only surmise how important the letter was to the trustees as they considered potential successors to Bowman. In *Time* magazine for August 1948 Det's appointment as President of Johns Hopkins University was announced with the comment, "When Johns Hopkins asked M.I.T.'s Karl Compton to submit a list of candidates, he sent back one name—Bronk's. Largely on Compton's say-so the University scarcely considered its one-hundred-odd other candidates."

When Det was offered the Presidency of The Johns Hopkins University his role as an educator and a scholar received appropriate recognition. He saw an opportunity to make explicit some of his ideas about an ideal graduate school. As usual, his acceptance of this appointment was predicated upon further development of biophysics through the establishment of a department with research laboratories on the Homewood Campus and in the Medical School. He took five members of the Johnson Foundation with him, those most closely associated with his main interest in neurophysiological research. He thus reaffirmed his strong belief that biophysics was to develop on a par with biochemistry, both being fundamental to our understanding of the living state of matter. In addition to these specific interests he fostered implementation of his ideas for the proper conditions under which all scholars might live and work. He believed that students should be given the opportunity to advance in their scholarly activities in accordance with their individual capabilities. He created the "Hopkins plan" which permitted undergraduates to engage in advanced study without regard to their academic classification as undergraduates. This plan enabled persons to earn advanced degrees based upon demonstrated competence without the necessity of first completing the requirements for a B.S. or an M.A. In short, for the gifted and highly motivated student there was to be no distinction between college and graduate school; both were simultaneously available to him for developing his scholarly abilities.

In 1949 such ideas created a flurry of reappraisal of academic goals, purpose, and methods among concerned faculty at The Hopkins. The Bronk plan was reformulated by a committee of the faculty. The circulated document was vigorously analyzed and criticized by several faculty members. The final plan was a distillate of Det's proposal and the joint thinking of the faculty whose cooperation was needed. The idea of emphasis on scholarship in all student activities at all levels was endorsed, but the possibility for "erasing the difference between graduates and undergraduates" was provided only for the exceptionally able student. Others proceeded as usual. The key purposes of Bronk's plan were: development of the individual man according to his individual needs and encouragement to think independently, to engage in original work, and to expand his intellectual capabilities to their full potential. In 1950, at the annual convention of the Middle States Association of Colleges and Secondary Schools, Bronk proposed abolishing the distinction between undergraduate and graduate education. He proposed advancement based on learning and judged by high standards of accomplishment in advanced study and creative enterprises leading to the doctorate in six years. At Hopkins Det worked closely with the faculty committee to develop his plan for advancing students in accordance with their scholarly accomplishments rather than their exam grades and course credits. Later, at The Rockefeller University, he utilized fully the experience derived from discussions of these ideas among the more diversified faculty at Johns Hopkins University.

The role of the scholar in national affairs was never far from Det's thoughts in considering the purposes of a university. In a speech at the Applied Physics Laboratory he spoke of the important role of Johns Hopkins University in providing an intellectual home for scholars serving government. He emphasized mutual benefit to the university and to the governmental agencies in which the scholars served the country.

In his presidential message to the freshman class he wrote,

> Our nation has assumed heavy responsibilities for the material welfare of peoples in many parts of the world. But the people of the world desire of us more than food and more than shelter. They look to us as a nation to prove the power of free men to create for themselves and others a more satisfying life by preserving the rights of individuals to work for common purposes and to resolve differences of opinion by free discussion. The faculties and administration of The Hopkins have no higher mission than to fit you to assume your responsibilities for such individual action. By discharging that responsibility you will make your greatest contribution to the preservation of our strong democracy.

And how are they to do this? He continued, "We live in challenging times that offer great opportunities for service and satisfaction to those who have courage and trained minds. The Hopkins is traditionally devoted to exploration of the frontiers of knowledge. In this environment you will have unique opportunities to acquire the spirit of research which will enable you to play a leading role in a swiftly-changing civilization."

Among the many letters of congratulation in 1948 were those that expressed opposite views of two related subjects: (1) Det's intention to continue some research, and (2) the continuation of his chairmanship of the NRC. With regard to the first intention many expressed approval, saying that a president should be an active scholar. Others intimated that this was desirable but could not be done. On the second subject, some expressed hope that he would continue, but others implied that he would need to give up the chairmanship. The letters came from high school classmates, physics students taught by Det at Michigan, and many, many former associates in Penn, Cornell, the NRC, the Office of Naval Research, etc. A letter from Isaiah Bowman, in 1948, was especially gracious in saying that he was delighted to have a successor who would open up a new era for Hopkins: "I believe you will find the Presidency a most satisfactory career, but I place opposite that an equally strong belief that your maintenance of interest in biophysics represents a wise decision." In contrast, Frank Jewett wrote to Det in 1948 (in a postscript): "Are you still so adamant about keeping your hand in on actual research work in the laboratory that you wouldn't consider a really top line administrative position connected with research if it wandered your way?"

Thus, like other scientists and educators directly involved in the rapidly expanding national scientific enterprises, Bronk continued his academic career *pari passu.* He devoted much of his attention to the administration of Johns Hopkins. He perceived that a creative effort by faculty and administration was necessary to rejuvenate a dedication to advanced study that had become distorted by their earlier war efforts. From 1948 to 1953, he led the faculty and students to renewed adherence to the scholarly purposes of an ideal university. In his speeches during this time phrases recur such as "breadth in education," "fostering of curiosity," "freedom for self-determination," and "a university is a community of scholars." His special concern for science was reflected often. In 1949 he wrote, "I believe universities must be preserved as the home of science because in them it is possible to integrate learning. It is only in the universities that we can train true scientists." He repeatedly emphasized the need for individual intellectual freedom as essential to progress of scientific thought.

His strong intention to continue personal research work was completely abandoned sometime after he became President of Johns Hopkins University. The necessity of this drastic change of priorities becomes evident when the range of his self-imposed administrative responsibilities is contemplated. Like Ira Remsen in earlier times, he was President of Johns Hopkins and of the NAS simultaneously. In addition, he was a member of the Board administering the newly created National Science Foundation. As the recently elected president of the American Association for the Advancement of Science, he committed himself to its affairs for 1952. In 1951 he was appointed by President Truman to serve on the Science Advisory Committee of the Office of Defense Mobilization. He served on the National Advisory Committee for Aeronautics from 1948 to 1958. From 1950 to 1970 he was a member of the Board of Trustees and the Committee on Scientific Policy of the Sloan Kettering Institute. He had also become a member of the Board of Scientific Directors of The Rockefeller Institute in 1946 and, in 1951, was chairman of its committee to prescribe a future policy and recommend a new director for the Institute.

IN SERVICE TO SCIENCE AND THE NATION

The general approval of Bronk's service in the NRC was expressed by his election as President of the NAS at the annual meeting of the Academy in 1950. Bronk, nominated from the floor by the Section for Chemistry, was placed in opposition to Dr. James B. Conant, the choice of the Presidential Nominating Committee. A ballot was taken and Bronk received a majority vote. He refused to serve because he had personally urged Conant to accept the nomination. Conant was not present but graciously requested, when Vannevar Bush informed him by phone, that the election of Bronk be made unanimous. The occurrence was taken by many scientists as a signal of approval of the prevailing NRC-NAS procedures for relating science to national needs.

Det's predilection for pursuing his goals through simultaneous control of several channels of management was evident to his associates in Washington. He was encouraged in this approach by some (as in Jewett's 1949 letter). In contrast, Richards admonished him, in a letter written in 1950, to resign as Chairman of the NRC before taking up the duties of President of the Academy:

> Ever since your election to the presidency of the Academy, I have been hoping that you would undertake to find ways of divesting yourself of much of the active work of the Research Council.. . .
>
> Our conversation yesterday makes me fear that your thoughts are not going in that direction. If I am wrong I hope you'll show me that I am. Agreed that a closer drawing together of Academy and Research Council is desirable and that your experience with the Council, coupled with your opportunities as president of the Academy fit you uniquely to design such closer union, I cannot see how it is possible for one person, even you, with prime responsibility to a University, to do the active work of leadership of both the Academy and Research Council . . ..
>
> Your intense desire to be useful, your extraordinary capacity to see opportunities for usefulness and your knowledge of your own ability to improve them leads you to accept such a multiplicity of them that some must inevitably suffer—and so indeed must you.. . .
>
> Affectionately yours, Newton

This advice prevailed, and Douglas Whitaker became Chairman of the NRC in September 1950. After nine months of successful service he was succeeded by William Rubey who served for several years, during which time Det continued as President of the NAS.

When Det Bronk became President of the Academy in 1950 he began a central role in the national development of science that was to continue for twelve years. Indeed, "very little of a high-policy nature affecting science happened around Washington without showing the influence of Detlev W. Bronk." This sentence in a recent letter from Dr. S. Douglas Cornell (now Assistant to the President, NAS) summarizes what I have been told by many others directly involved in such matters at that time. No scientist took the ideas in the charter of the NAS more seriously than did Bronk. He devoted a large part of his life to reinterpreting their meaning in the postwar years. During these crucial years the basic policies that now define the place of science in our national culture were formed. New federal institutions concerned with science were created: Atomic Energy Commission, National Science Foundation, National Aeronautics and Space Administration, President's Science Advisory Committee, and Office of Science and Technology—and a science advisor to the President of the United States appeared on the scene. These events required an increased effort by the Academy, initially, to help define the scope and purpose of each new federal agency for science, and thereafter, to prepare itself for the wider range of scientific advisory service requested by these new agencies. Det had been well prepared for leadership of the Academy. As Chairman of the NRC from 1946 to 1950 he had learned how to mobilize and facilitate the efforts of scientists in various disciplines for the purpose of utilization of scientific knowledge in solving national problems. Furthermore, he had been Foreign Secretary of the Academy since 1945 and therefore experienced in promoting official participation of our national scientific societies in international scientific affairs. The range of his services and initiatives as President of the NAS are too extensive to review here.

A letter sent to me (January 1978) by Dr. Cornell reveals how he and John S. Coleman (now Executive Officer, NAS) recall Det's creative efforts to enhance the role of the Academy in the development and utilization of scientific knowledge for the welfare of our nation:

> Det saw the NAS/NRC also as a place where the health of basic science must be faithfully tended, and its role as the foundation of the whole technological enterprise must be emphasized. Not only must the case be constantly and powerfully made for ample financial support of basic research, but here lay an extraordinary opportunity to foster gatherings of scientists where the "state of the frontier" in various fields could be discussed and ideas could be exchanged. He took part himself in many such meetings. And he found other occasions for preaching the gospel. When his beloved sailboat was destroyed by a hurricane that took a capricious and unpredicted course, he reacted by seeking ways to interest more physicists, chemists, and mathematicians in the basic problems that underlie the science of meteorology. On the science/society frontier, he enjoyed nothing more than talking to a group of highway engineers, challenging them to take some time away from problems of materials and design in order to think in the broadest way about the future role of highways in relation to the welfare of a nation and its people. He hoped that each major unit of the NRC would gather a small group of creative thinkers in its field who could ponder the broader horizons and the deeper potentialities of their common enterprise.
>
> His courage and vision were manifested in very practical ways. As the activities of NRC grew under his leadership, the Council of the NAS was often concerned about over-extension. But he had no doubts. The potential service of NAS/NRC to science and the nation was limitless. He had to work to persuade a cautious Council to lease the first "outside" space for our activities, a tiny building two blocks from 2101 Constitution Avenue. By the end of his administration, there was "outside" space in eight different buildings, vastly exceeding the "inside" space. At the same time, Det was keenly conscious of the symbolic importance of a suitably impressive "house" of the NAS/NRC, and he tended that as well. He believed that man's spirit is ennobled by noble architecture and noble symbolism. The Supreme Court housed in the Empire State Building would not do. So he always had close to his heart the enlargement of the Academy's own "house", an enlargement that had been envisioned for the future in the original plans of Bertram Goodhue when he designed the building at 2101 Constitution Avenue in the 1920's. Det learned that the directors of the Equitable Life Assurance Society of North America were considering

a "gift to the nation" in celebration of the 50th anniversary of the founding of that Society. They invited him to discuss with them the kind of gift that would be most suitable. What more natural than that he should emphasize the intimate relationship between their interests and the furtherance of the life sciences, and what more natural than that they should then conclude that no more appropriate gift could be made to the nation than funds to add a "life sciences wing" to house the headquarters of the Academy's activities in that field? Heartened by this success, but never narrowly interested in just one area of science, he later spearheaded the campaign that raised funds from many sources for the balancing "physical sciences wing."

After World War II (1945) Vannevar Bush proposed, in a report requested earlier by President Roosevelt, the establishment of a national science foundation. The necessary legislation required congressional hearings, with scientists as the principal participants in formulating the bills and testifying before Congress. During the war, mobilization and cooperation among scientists in support of the national goals revealed the potential value of continuing the support of science as a national asset. However, in peacetime the long-range national goals did not provide the same unity of thought and purpose. Congress and the Administration tended to regard the scientific enterprise as a national resource that should be directed and controlled by legal procedures. The scientists and educators, so unified during the war effort, now revealed in legislative hearings extensive differences of opinion. This public revelation of individual freedom of thought, characterizing all university faculties, was not conducive to generating legislation that formalized the support and the direction of scientific research. Repeated efforts failed to establish a national science foundation. In the interim, the short-term, mission-oriented investigations, previously funded and coordinated by OSRD, were administered by various government agencies, Office of Naval Research (ONR), and NRC. The principal problem was that many scientists sought legislation that would insulate the scientific enterprise from political manipulation, whereas the Federal Administration sought con trol through the appointment of the managing board and the director.

Bronk, too, was engaged in testifying before Congressional committees concerned with this legislation. His testimony in 1945 on Bill S-1297 reiterated two principles that he believed essential for the long-range development of science as a national asset. He spoke in favor of "A Division of Basic or Natural Science for encouragement of research of a purely exploratory nature; uninhibited by the necessity of solving useful problems." Later he said, "I urge that means be found for giving many scientists of proven or potential competence the freedom to direct the course of their own investigations." During this period Det continuously exchanged letters with many other scientists involved directly with this portentous change in the relation of the Federal Government to the scientific enterprise They were very conscious that the form of the legislation would influence the direction of scientific development in the country for the foreseeable future. During the next five years they shifted tactics and made compromises, hoping to insulate the directorship from political control. However, President Truman insisted that the managing board and the director be appointed by the President of the United States. The advent of the Korean War provided a strong incentive for establishing a national science foundation to coordinate federal support for science in universities and other research institutions. Finally, in 1950, Truman signed the Bill establishing the National Science Foundation (NSF), which provided that the President appoint a board and a director.

The value of Bronk's advice in this creative enterprise was recognized when Truman appointed him a member of the first National Science Board in 1950. The Board, in turn, elected him Chairman of its executive committee. Det served as a member for fourteen years, during which time its primary policies were being formulated. As President of The Johns Hopkins Uni versity he was well versed in the problems, opportunities, and social value of advanced study and basic research—a primary concern of the new NSF. James B. Conant, Lee A. DuBridge, and Bronk were strongly in favor of initial emphasis by the NSF upon support of basic research and of fellowships to encourage competent students to choose a career in science. They, and many others, retained a direct role in academia while serving science from a strong position in Washington. For this reason the advancement of science in the nation was firmly linked to the advancement of science in our universities. There was strong support for the idea that basic science flourishes best in the free intellectual atmosphere of the university. The early policies of the NSF reflect this view.

Bronk was indisputably in the vanguard of those who guided the flourishing of the scientific enterprise in America following World War II. The political and social climate was favorable because of the effective service of scientists to the nation during the war. These years were marked by events that changed the national perception of the role of science in our society, as well as the rate of acquisition of new knowledge and of its application. The structure of these changes was a direct result of the actions of many scientists who donated a large part of their talent and attention to the promotion of science as a national resource. In 1955, the Board elected Bronk as Chairman, a vote of confidence that was made explicit in a letter from Lloyd Berkner. "This action of the Board will meet the unanimous approval of the scientific community, since we have all learned to have enormous confidence in your great wisdom and sound judgment as the leader of American science." Det continued as Chairman of the National Science Board for nine years.

I have never witnessed Det Bronk in action at a substantive meeting of the National Science Board, the Council of the Academy, or the Board of Trustees of any of the universities that he served. The following remarks by W. O. Baker are commensurate with my expectations based upon discussion with Det spread over many years and my recent reading of some part of his extensive files of correspondence on such matters:

> We need hardly say that the experience of the trustees [of The Rockefeller University] during Det's tenure was no less demanding than that of the students. Led by Chairman David Rockefeller, who fully matched Det's zeal for frontiersmanship and excellence, we became progressively informed on examples of how to make the institution respond to relentless, but always cheerful and expectant, demands for progress. Whether it was for approval of a seductive call to another illustrious scholar for the faculty, for the Tower Building Committee to house new work, or for the Kiley landscaping to delight the eye, Det never let a trustee languish.
>
> But of course, that had always been his way. For instance, his founding and initiating role in the National Science Board, after his appointment by President Truman as a Charter Member, was good preparation for his chairmanship from 1955 to 1964. The period through the 1960's saw the greatest growth of the National Science Foundation and its profound influence in the national community of research and education. The many weekend meetings, orchestrated by Det Bronk, in which the Centers of Excellence programs, new curricular supports, national research institutions from the Antarctic to the Rockies, and many other activities were conceived and pursued, represent an historic phase of Federal science and education. His relation with Congressman Albert Thomas, the crucial chairman of the Appropriations Subcommittee of the House of Representatives, was a particular delight to behold. They shared a zest for life and people which established lasting rapport. Those of us who attended the annual hearings in the period, when the budget and role of the Foundation were growing steadily, were charmed by the solid and confident exchanges between the master politician from Texas and the politic master from nearly everything else, including the National Science Board.*

Det has recorded his personal recollection of these events in "The National Science Foundation: Origins, Hopes, and Aspirations" (*Science,* 188[1975]: 409-414).

Again, it is interesting to note Det's style of achieving common goals through many overlapping channels of management. During the period from 1950 to 1962 he was president of a university, chairman of the Board of NSF, and president of NAS.

* *Recollections of Detlev W. Bronk by Colleagues and Friends, February 18, 1976.* (N.Y.: The Rockefeller University Press.)

Indeed, he also had direct administrative control of NRC by acting as unofficial chairman from 1954 until 1959, at which time the Council formalized the arrangement. By this act the NAS-NRC affairs became more firmly linked by making the President of the Academy function as the administrator of NRC affairs. This change of administrative responsibility was fostered by Bronk, an arrangement opposed by Richards and favored by Jewett at an earlier time. It was a major administrative change that proved useful and has persisted to the present.

The NAS is responsible for implementing the participation of our national scientific societies in international scientific affairs. It administers our national cooperation with the International Council of Scientific Unions in a wide range of disciplines. Det was highly motivated to promote such activities through his experience as Foreign Secretary of the Academy (1945-1950) and his strong belief in the universal character of significant scientific advances. Among the major projects of this kind during his presidency of the NAS was the International Geophysical Year (IGY)—1957-1958—which required four years of preparation. President Eisenhower was particularly interested in this undertaking and decided to institute the Vanguard satellite program as part of the participation by the United States. Then came Sputnik and the dramatic announcement by Lloyd Berkner of its launching—at a party for organizers of the IGY held in the Russian Embassy. This and related events are recalled by Bronk (aided by William T. Golden's diary) in "Science Advice in the White House. The Genesis of the President's Science Advisers and the National Science Foundation" (*Science,* 186[1974]: 116-121). It fell to Bronk as President of the NAS, to make a proper response to the Russian Academy of Science. It was a congratulatory statement emphasizing the universal quality of scientific accomplishments. Later, President Eisenhower spoke to the nation in the same vein, after an interesting consultation with Bronk on the role of science in national affairs. Bronk has traced a direct relation between these events and Eisenhower's decision, in 1957, to appoint a science adviser to the President, a proposal suggested by William Golden in 1950 and promoted by many over the intervening seven years. After the war there developed an ever increasing need for advice and counsel on scientific aspects of defense, reaching deep into foreign policy in relation to arms control and test-ban treaties. These concerns led to the formation of the Science Advisory Committee (SAC) within the Office of Defense Mobilization in 1951. Several years later, in accordance with the 1950 proposal for President Truman, SAC was conveniently transformed into the President's Science Advisory Committee, or PSAC, advising the President directly on scientific aspects of national policy. Simultaneously, also as proposed in 1950, the Office of Science Adviser to the President was created. These actions were precipitated by Sputnik much as the Korean War catalyzed the formation of the NSF. Thus, in 1957, the role of science in government was enhanced by Eisenhower's strong support, and science in the service of the nation achieved political, in addition to educational and technical, dimensions. Bronk was a charter member of PSAC and, appropriately, chairman of its subcommittee for foreign relations.

As the role of science in national policy increased, the requests by the Federal Administration for advice from NAS changed. In January 1958, Secretary of Commerce, Sinclair Weeks, asked Bronk to form a committee of the NAS to evaluate all scientific programs within the Department of Commerce. This unusual request exemplified the character of the new need for advice on scientific activities within the Federal Government and, according to Bronk, was one precursor to the formation of a new kind of standing committee within the NAS, distinct from the committees of the NRC. Similar requests for advice from the President's Science Adviser and from PSAC promoted this development. In May 1961 Bronk wrote to George B. Kistiakowsky a letter that initiated the formation of a "Committee on Government Relations." He stated that the purpose of Committee was

> to assist the President and the Council of the Academy in responding to requests for studies and evaluations of scientific needs of the country, including long-range planning of major facilities, coming from the Executive Office of the President (President's Science Advisory Committee and Federal Council for Science and Technology) and especially from Congressional Committees. It will also undertake such studies on its own initiative for transmittal to the above Government organizations. It will not be concerned with advisory activities for individual executive agencies such as those of various NRC committees.

Kistiakowsky, motivated by his experience as Science Adviser to Eisenhower, had perceived the need of such a standing committee in the Academy. He had urged its formation in discussions with Bronk. The committee described above was the precursor to the Committee on Science and Public Policy (COSPUP), established in 1962, that evolved under the dedicated leadership of Kistiakowsky into one of the most effective advisory instruments in the Academy.

FREEDOM FOR INQUIRY

Det Bronk was a staunch patriot who believed that the mobilization of scientific knowledge for the benefit of the nation was a duty for every scientist. In turn, he considered the support of basic science a proper function of a Federal Government concerned with the long-range welfare of the nation and its people. He foresaw that restraints on freedom of inquiry would surely arise as the scientific enterprise became increasingly recognized as a national resource for solving social problems related to defense, health, food, and technology. He knew that freedom of inquiry and dissent were essential in the search for new knowledge and he understood clearly that science must be free of politics and of nationalism. Among the ten listed objectives of his administration at The Hopkins (Annual Report, 1953) one can read: "Defend the right of scholars to investigate, debate and question conventional concepts and to seek new knowledge which [sometimes] fosters insecurity of established ways of thought and life." He also knew that acquisition and synthesis of basic scientific knowledge depended upon supporting the advanced study and research of a gifted few among our citizens. He believed that this could be adequately achieved only with federal financial support, and, in turn, he recognized that in a democracy such selective support required widespread understanding among the people of the origins and proper uses of science. To this end he spoke and wrote about these matters often after 1945, when his role in Washington began to assume a broader scope than obtained during the war.

One might suppose that his administrative experiences in Washington during World War II generated such views. This surmise would be wrong because in a paper on "The Social Obligations of the Scholar" in 1934 he wrote, "The scholar is not excepted. He will neither escape the questioning of society in rapid flux nor fail to benefit by an analysis of his social relations." Then he added, "How are we to secure from society conditions which will make possible the existence of the scholar?" Twenty years later, after much practical experience, he wrote,

> Progress requires courage. If we are to fulfill our rightful role in the furtherance of science, we need abundant courage. For this we are fitted by tradition and by the nature of our calling, for we are discoverers and teachers of new knowledge which is usually challenged and disputed. And so, there is no place in science for timid men and women who are unwilling to defend their necessary freedom for inquiry and free unprejudiced discussion. The furtherance of science requires courage to withstand the pressure of reactionary forces.*

To an impressive degree Det Bronk practiced what he preached

* "The Role of Scientists in the Furtherance of Science," *Science,* 119(1954): 223-227.

as numerous problems arose from the federal support of science in universities.

After World War II much of the research, as well as the training, of young scientists was done in the universities but with funds derived from federal taxes. Specific policies, defining the role of government in higher education and preventing infringement of the principle of academic freedom through politically motivated interference, were needed. During these same eventful years prominent scientists were confronted with another aspect of this problem arising from their increasing influence in national affairs, especially national defense. Some members of Congress concerned with un-American activities suspected the loyalty and integrity of some scientists. Progress in developing a productive national science policy was slowed by public reaction to such accusations. Not only were the careers of individual scientists disrupted, but unwise resolution of these problems also jeopardized federal support of basic science. One of the most difficult and unpleasant problems for scientists was how to deal with a prevalent Congressional attitude that all scientists supported by federal funds must have complete security clearance through the FBI, even graduate students holding fellowships from federal sources. All parties agreed that federal support of science was essential for defense and the general welfare of the country. The issue was the specter of political intervention into higher education through centralized control of research and advanced study. One proposed compromise was a special loyalty oath, but this was objectionable to many scholars. The Joint Congressional Committee on Atomic Energy brought the matter into focus first (1948) when they discovered that an alleged communist sympathizer had been granted a fellowship by the Atomic Energy Commission (AEC) to study advanced physics.

Bronk was frequently a negotiator in these matters, problems which threatened the federal support of science generally. As

Chairman of the NRC he was directly involved in the AEC fellowship program because recipients were selected "solely on merit" by a committee of the NRC. The Joint Congressional Committee on Atomic Energy was opposed to granting AEC fellowships solely on scientific merit. They were adamant that government funds must not be used to support the studies of persons who advocated "overthrow of the government by violence and subversion." Bronk clearly perceived that resolution of this problem required finesse to protect the freedom of inquiry by scholars in universities and to insure continuation of student aid from federal sources.

In 1949, a meeting of scientists was convened by the American Association for the Advancement of Science to consider legislation establishing a National Science Foundation which was marred by an amendment requiring loyalty oaths for scientists receiving federal funds for research. As Chairman of the NRC Bronk reported that: (1) the NRC opposes "clearance" procedures being required of scientists working in unclassified areas and (2) it continues to administer federal non-military fellowships that carry security restrictions by the law providing the funds. He stated that some Congressmen wanted security clearance procedures for all students with federal fellowships. He said the NRC position was weakened because individual scientists and scientific societies did not take a firm public position on the matter.

Later, in 1952, he was again involved in problems arising from NSF grants to "communistic" scientists, at a time when he was on the Board. Finally, he helped to construct a generally acceptable policy on the matter when he persuaded Sherman Adams, President Eisenhower's adviser, to request that a committee of the NAS be formed to study the problem and make suitable recommendations. The need for advice in forming federal policy in this matter is contained in Adams' letter to Bronk (January 11, 1955) with the explanation that, "calling upon the Academy in this way is in keeping with its Congressional charter to advise the Government in the formulation of policy to the end that the scientific resources of our country may be fully and effectively utilized." As President of the NAS Det appointed the committee with Julius Stratton as Chairman.

The committee recommended that scientists involved in unclassified research and advanced study need not be cleared. In April 1956, a letter from Sherman Adams indicated that the executive departments and agencies of the Federal Government would endorse the recommendations of the NAS committee on security procedures. A letter to Det from Stratton states, "On the whole, I feel that the great contribution of the Academy in this matter—and a very large part of the credit goes to you personally—is to have persuaded Governor Adams to take public administrative action in the matter." Bronk's persistent efforts in these affairs are also noted in a letter from D. E. Lillienthal, April 1956. He comments on the official action by Adams and continues, "This reminded me, of course, of the first time this precise question arose; it was under the AEC'S unclassified basic research fellowship program. With exemplary forthrightness, you made a case against this mischievous doctrine of FBI investigations in non-secret research. Although I took quite a beating at the time for doing what I thought the AEC Chairman should to defeat that move, I felt that we were right: I hope that now sanity is going to be restored." The acceptance of this principle by President Eisenhower as federal policy settled the matter for most scientists receiving federal support for unclassified research, including students. The strong interest of Eisenhower in promoting science to achieve national goals provided the right circumstances for a rational solution to the problem. However, the Congressional Committees continued their watchdog responsibilities and incorporated in relevant laws the specific requirement that Communists were not to receive federal money for research or for advanced study.

The problem of granting fellowships to avowed Communists rose again in 1960 when the House Committee on Un-American Activities charged that the NSF had done just that. Bronk, as Chairman of the Board, was again involved, although Alan T. Waterman, as Director, was held responsible. The case reached the Supreme Court in 1963, but in the interim the NSF revoked the fellowship in accordance with the laws under which NSF operated. Such events were rare, and this aspect of the law has seldom been invoked. Indeed, Waterman claimed initially that there were no legal grounds for revocation until the Congressional Committee convinced him otherwise. The loyalty problem for graduate students continued at least ten years after the AEC episode. In more general terms the problems were related to procedures for insuring the freedom of universities from political influence while maintaining federal support of advanced study and basic research. Each new case required new policies depending on contemporary views of the role of the Federal Government in higher education. Now, the problems revolve around equal rights legislation as a potential conflict with selection of scholars "solely on merit."

Thus, the security issue became a distinct instance of federal intervention into university affairs, forcing faculties to adopt relevant policies. Bronk was confronted with this aspect of the problem as President of The Johns Hopkins University. In March 1950, Senator Joseph McCarthy accused Owen Lattimore of being an active Communist. He stated that Lattimore was associated with organizations listed as subversive and was simultaneously an adviser in the State Department. At the time, Lattimore was director of the Walter Hines Page School of International Relations at The Hopkins. Bronk was confident that Lattimore would testify and clear himself and stated this to the press, without consultation with Lattimore who was on a mission in Afghanistan for the United Nations. Bronk asked Senator Tydings to arrange a full hearing for Lattimore. On the occasion he stated to the press, "I have seen no evidence that Lattimore is a pro-communist, but I have made no investigation. If national security is involved, of course, I feel anyone—even my own father or mother—should be investigated." When Lattimore testified, he was less than a cooperative witness and subsequently was charged with perjury. The Board of Trustees and some alumni were critical of Lattimore for his uncooperative behavior before the Senate Committee on Internal Security. Bronk decided to give him a leave of absence with full pay until the perjury charges were resolved. His action was supported by the Board and the Academic Council of The Johns Hopkins University. In 1953, the Walter Hines Page School of International Studies was discontinued and Lattimore assumed the title of Lecturer which he held until 1962.

The federal support of science within agencies of the government also invites interference from politicians who do not understand the objective character of good science. A typical instance was the dismissal in 1953 of A. V. Astin, the head of the National Bureau of Standards, because of a Bureau report that certain battery additives tested there did not improve performance of storage batteries. The Secretary of Commerce, Sinclair Weeks, considered the report an interference with free enterprise in marketing a product. Backed by the NAS, Bronk interceded to have Astin reinstated until a committee of scientists of the NAS could review the scientific evidence and evaluate the methods of testing used by the Bureau. At the time he was also a member of the Board of Advisers for the Bureau. It is interesting to note that Bronk handled his intervention in the Astin dismissal through reasoned discussion and that he and Secretary Weeks cooperated successfully for five more years until Weeks resigned in November 1958 (see page 54).

During this period, as President of both Johns Hopkins and NAS, Det spoke at commencements in all parts of the country. His themes were chosen to interpret the vital role of scientists and other scholars in a democracy. He stated frequently that these are difficult times requiring the use of reason and restraint of emotional reactions in the solution of our problems. He reaffirmed the necessity in a democracy of personal freedom and self-determination. He spoke often of the necessity of freedom of inquiry, as in scientific research. In this way he strove to make clear the problems faced by scholars in universities as they sought financial aid from governmental sources. Concomitantly, he formulated and organized his own thoughts on the relations between the scientific enterprise in universities and social goals of a democratic society. As President of the NAS he focused upon problems facing scientists, but as President of Johns Hopkins he was concerned with the similar problems of all scholars in universities. In 1950 he stated, "Precious values of democracy are best preserved if universities receive major support from the private sector—free of political forces of distortion." In a speech dedicating the New York Academy of Science building, he decried "secrecy in science" as the antithesis of sound investigations of natural phenomena and asked for reaffirmation of "the individual's right to know as a basic human right."

Det spoke often of the proper conditions for promoting creative scholarly advancements. Among these were careful selection of creative people and protection of their rights as thinkers who might disturb the status quo. In concert with many others, he tried to develop policies that would insulate the scholarly enterprise from military, political, or commercial control and exploitation. For over fifteen years, 1948 to 1962, he had dealt with many specific instances of such interference. He was guided in his actions by personal principles but learned the political realities that derived directly from federal support of advanced study and research. This experience led him and others to emphasize the importance of retaining as much private support of universities as possible—to insure that freedom of thought and inquiry would be encouraged and defended against centralized political control.

A COMMUNITY OF SCHOLARS WHO ARE SCIENTISTS

During some part of 1953 Det Bronk was President of The Johns Hopkins University, The Rockefeller Institute for Medical Research, the National Academy of Sciences, and the American Association for the Advancement of Science and also Chairman of the Board of the National Science Foundation. This was the year in which he made the transition from Hopkins to The Rockefeller Institute and was President of both for several months. Indeed, immediately after this event he became a member of the Board of Trustees at Hopkins in order to continue as adviser to that institution. The procedure and purposes of this arrangement are reminiscent of his continued role at Swarthmore College after he became Director of the Johnson Foundation and Professor of Biophysics at the University of Pennsylvania in 1929. He did relinquish one responsibility when he resigned as Editor of the *Journal of Cellular and Comparative Physiology* (JCCP) in 1953, a post he had held since 1939.

In his last Annual Report as President of Hopkins (1953) he listed ten items of academic policy he had hoped to develop when accepting the position in 1948. Two of these are particularly representative of his concept of the ideal university.

> (1) I believed that research, as the basis for thought and as prelude to action, was essential to modern life. The Hopkins seemed to be the ideal community of scholars for furthering such objectives with little emphasis on pedestrian instruction or on distinctions between faculty, graduate and undergraduate students.
>
> (2) Foster the unity of knowledge and reduce the significance of departmental barriers. The growth of knowledge and the increase of information regarding man and nature encourages specialization. But understanding requires comprehension of many related fields of learning. Unless creative scholars and students learn in universities which stress the unity of knowledge and scholarly endeavor, universities fail to provide the intellectual leadership sorely needed in our complex civilization.

From 1949 to 1951 Bronk and the Hopkins faculty worked to implement these ideas, and in 1951, as a member of the Board of Scientific Directors of The Rockefeller Institute, he became chairman of a subcommittee that was to nominate a successor for the director and to propose new policies and programs for the further development of that institution. At Hopkins the faculty had begun to de-emphasize the prevailing distinctions between graduate and undergraduate education. In addition, steps had been taken to encourage wide-ranging interdisciplinary study by reducing formal departmental requirements. Unfortunately, the majority of undergraduate students were not sufficiently prepared or not interested in proceeding rapidly into advanced study and research. For many disciplines a high degree of specialization is necessary and must be provided in training undergraduates. These considerations preclude establishing most universities as "communities of scholars devoted exclusively to wide-ranging advanced study and research." As Det continued his discussions of advanced education with the faculty at Hopkins and, simultaneously, his efforts on the committee considering the future development of The Rockefeller Institute, the idea of a graduate university of science emerged. This basic concept was in his final report to the Board of Trustees of the Institute submitted in March 1953, after he had accepted their offer to succeed Herbert Gasser.

In September 1953 when Bronk became President of the Rockefeller Institute for Medical Research, he was at last in a position to develop a unique institution devoted exclusively to the sciences, including the history and philosophy of science. He hoped to create a graduate university within which the young scientists learned from the more experienced, and all were there because of an intense dedication to increasing understanding through advanced study and research. Det was fully aware of the exceptional opportunity thus presented to him and proceeded enthusiastically to build his ideal university, making effective use of the existing faculty who favored his general proposals and putting no constraints on those who preferred to remain aloof from the enterprise. The Rockefeller Institute for Medical Research was composed of autonomous laboratories devoted exclusively to experimental investigations of medical, biological, and biochemical problems. The faculty had much experience in training postdoctoral fellows in research. Det perceived that his major tasks were to increase the range of intellectual pursuits by new appointments to the faculty and to provide for graduate education. Among the recommendations adopted by the Board of Trustees in June 1953 was a plan to initiate a program in advanced study and research in science for a small group of carefully selected students who were candidates for the degree of Doctor of Philosophy. In October 1954 he and John Lockwood (Secretary and Associate Counsel) reported to the Trustees their preliminary discussions with the Board of Regents of the State of New York. In January 1955 the Rockefeller Institute for Medical Research was incorporated under the Board of Regents with the right to grant the advanced degrees of Doctor of Philosophy and Doctor of Medical Science, as well as certain honorary degrees. In September 1955, ten graduate fellows began their studies as candidates for the Ph.D. The Rockefeller Institute had become a graduate university in fact, though not in name. Much later, in 1965, the Institute became officially The Rockefeller University. The continuity of purpose in Det's thinking about higher education in the sciences is clearly evident, starting at The Hopkins in 1949 and culminating in the first convocation for the granting of degrees at The Rockefeller Institute in 1959. At the Institute he was now immersed in one of the most imaginative enterprises of his administrative career. In this creative effort he was helped and encouraged by the strong support and good advice of David Rockefeller, Chairman of the Board of Trustees.

Det had a great appreciation for the special role in society of creative scholars, and now he had a rare opportunity to bring such people together at the new Graduate School and Research

Center. He invited many scholars for short visits, choosing those in areas of research not strongly represented at The Rockefeller Institute. Simultaneously, he initiated his plans for an enlarged permanent faculty. Of course, biophysics was introduced in 1953 when he accepted the presidency. Then came Paul Weiss, Edward Tatum, and Fritz Lipmann. He was elated when Samuel Goudsmit advised him that George Uhlenbeck, Theodore Berlin, and Marc Kac might be interested. Thus, the faculty in biology was expanded and a faculty in mathematics and physics created. Det wisely focused his attention on individual scientists and carefully avoided constructing any group that might function as a conventional academic department. The next appointment was Carl Pfaffmann who proceeded to help him bring in talented people concerned with a variety of basic investigations in the behavioral sciences. The final extension of the scope of scholarly inquiry within The Rockefeller University was marked by the arrival of logicians and philosophers. As a prelude to this innovation, he had earlier (1961) appointed Ludwig Edelstein Professor, and had stated to the Board of Trustees, "It is only of incidental significance that he is a distinguished historian of biology and medicine. It is of deep significance that he is a great humanist; as a community of scientists we have suffered too long from lack of association with scholars such as he who is versed in the origins of modern science and the influence of science on the ideas and habits of man." The intellectual range of this enlarged faculty represented Det's view of the range of interests that a scholarly scientist should have in good measure.

His greatest concern was the selection of students with a scholarly potential that was commensurate with that of an exceptionally competent faculty, all of whom were engaged in research. For many years he talked with every prospective student invited to the campus for interview. He befriended all who were admitted and encouraged them in their efforts as long as he perceived a continued sincere endeavor to succeed. He scorned only those few who, in any manner, deliberately misused their opportunities for full-time study and research. Det's description of the graduate program, in the Catalogue, was an expression of his vision of an ideal graduate university. The following excerpts from the Catalogue for 1967-1968 are, in effect, a résumé of his ideas about education that can be traced in published speeches as they evolved over a lifetime of service to universities.

> The purpose of this University is to further natural science and its application for the improvement of human welfare.
> The University is not an aggregate of departments dealing with specialized fields of science. It is a community of scientific scholars who are free to follow their interests in any field of scholarship.
> The students are few, and the faculty are many. This enables close association between the two, they live and work as junior and senior colleagues.
> Students must be capable of self-directed study. Although many courses are offered, teaching is done primarily in seminars, in tutorial conferences, and in faculty research laboratories. There is thus considerable freedom for the active process of independent learning.

He created a seminar for new students in which "the student deals with the significance and relation of ideas. At the outset of his career he is thus encouraged to develop a broad foundation of competence in many fields of science and to recognize the relations of his special field of interest to other areas of science. He is persuaded to broaden his concepts and become an independent thinker rather than a mere helper in a restricted part of another's highly organized program of research."

Det knew that only a few truly exceptional students could make effective use of such opportunities but believed these explicit idealized statements in the Catalogue would attract only able students who also had the courage to face the implied challenge. They must be exceptionally intelligent, he liked to say, but in addition they need self-assurance, self-discipline, and a great urge to learn through advanced study and research.*

* A more detailed review of these events is in my essay, "Detlev Bronk and the Development of the Graduate Education Program." In: *Institute to University, a Seventy-fifth Anniversary Colloquium, June 8, 1976* (N.Y.: The Rockefeller University Press).

I believe that most of the 125 students who received their diplomas from Det's hand in Caspary Auditorium would agree with the last paragraph in the memorial talk by Johns Hopkins III (an alumnus):

> It seems to me that a memorial service should do more than honor the dead. It should elicit new reflection and new dedication from the living. I think Det would have wanted to tell all of us who are associated with research and teaching to work a little harder, to put a little more faith in the student, and to make sure that The Rockefeller or any other university, and the enterprise of basic science, are passed on to the next generation in better and more effective condition than we found them. Perhaps, most of all, he would want to reaffirm his belief that, in this or any other endeavor, human relations and human dignity must be paramount.

MONUMENTS

A plaque in front of the Detlev Wulf Bronk Laboratory on the campus of The Rockefeller University describes him as Scientist, Educator, Humanist. One could well have added Patriot because Det Bronk's activities were strongly motivated by his urge to further the scientific enterprise for the welfare of the nation. Indeed, he believed the advancement of science to be in the public interest, materially and philosophically. With this description in mind the first outline of this biographical memoir was in three distinct sections. It was to be an account of his scientific research, followed by his role as teacher and university president, and terminated by an analysis of his concern with the relations of science to the welfare of the nation. This intention was somewhat strengthened by Det's description (1970) of his productive professional life in these words, "After fifty years as an engineer, biologist, and servant of universities and government, I still have faith in science and reason as sure means for creating an ever better quality of life." Of these fifty years he considered thirty spent "on the frontiers between science, technology, and public affairs .. ." The other twenty were devoted to research and the development of biophysics as a unified field of science. Then I systematically read through his published papers, other than research reports, and changed the plan. Despite his wide-ranging activities, there was evident a continuity of principle and purpose that unified his efforts. Furthermore, his manifold active roles were never end-to-end but extensively overlapping in time and intent. He obviously moved toward his larger goals by concomitant channels of influence achieved through simultaneous service in related institutions. For example, his views of the proper relations between universities and the Federal Government were represented simultaneously in the councils at Johns Hopkins University, the National Academy of Sciences, the National Science Foundation, and the President's Science Advisory Committee. Furthermore, as late as 1949 he concerned himself with experiments and their interpretation in a Croonian Lecture to the Royal Society of London at a time when he was Chairman of the NRC, President of The Johns Hopkins University, as well as Editor of JCCP. Clearly, his was an integrated life that cannot be partitioned readily into distinct segments. This memoir is written with the latter idea in mind and with an effort to exhibit that, for Det Bronk, "thought is a prelude to action." Because he usually preached what he practiced, I consider the following paragraph from a speech at a high school graduation, late in his life, to reveal his personal experience.

> Life is a wonderful journey through a beautiful world—it is filled with glorious adventures, and the only danger ahead is that we spend our time on the trifles and miss the rich experiences. I therefore challenge you to stop now and decide what you hope to get from the years to come and how you are going to live in order to realize those values which you consider most important. It is imperative that you make these decisions soon for the habits and attitudes toward life which you form or drift into during the next four or five years will largely dominate you throughout life.

I believe this philosophy explains the care with which Det critically evaluated his own motivations, talents, and opportuni ties during 1924 to 1926. His decision in favor of teaching and research was immensely strengthened by the subsequent stimulating year of research with Adrian and Hill. Through them he decided to emphasize research and advanced study as contrasted with college teaching. He found a unique opportunity for a career in research as Professor of Biophysics and Director of the Johnson Research Foundation in the University of Pennsylvania.

When Bronk left Philadelphia in 1948 he kept his home in Sycamore Mills, continued an active role in the American Philosophical Society, and participated in the affairs of the University of Pennsylvania as a member of its Board of Trustees. Nevertheless, the move to Baltimore terminated one of the most satisfying periods of his life. As noted by Frederick Seitz in a memorial talk at the National Academy, "His links to the University of Pennsylvania were clearly of a very special nature, encompassing as they did almost his entire professional lifetime, as well as his greatest period of self-discovery and subsequent fulfillment. Moreover, the Philadelphia community is so constituted that he could occupy a somewhat Franklinesque position —knowing and being known by almost every distinguished citizen."*

The various "institutions" derived from Det's creative ideas for the advancement of science have had a marked viability. The Johnson Foundation for Medical Physics (1929) was developed with the idea that research on basic biological problems would best serve the purposes implied in the name. That continues to be the policy now. The Institute of Neurology at Penn (1937) was conceived to unify knowledge of the nervous system, as studied in the laboratory and in the clinic. It continues with its purpose intact. The Hopkins Plan (1949) for permitting tal

* The story of Det's Philadelphia years will be told with enthusiasm and appreciation in a biographical memoir prepared for the American Philosophical Society by Britton Chance.

ented undergraduates to pursue advanced studies and to progress to the Ph.D. in accordance with their accomplishments has served many gifted young scholars and continues to do so. The Rockefeller University was conceived in 1953 as an ideal "community of scholars" and a unique graduate university for students of science. It continues, with some attenuation in scope, as a graduate university emphasizing the life sciences and related physical sciences. Det Bronk was a dedicated builder of institutions and an enthusiastic builder of buildings as well. His efforts range from modest additions to his first home in Ann Arbor to noble additions to the NAS. He participated directly in creating the campus for the new Rockefeller University, including lawns, flower beds, pools, new administration buildings, dormitories, and two large towers for research laboratories.

A resolution by the Council of the NAS expressed full appreciation of the effectiveness of Det's forty years of continuous service to science and the nation through his work in the NRC and the Academy. He was acclaimed as the founder of the American Institute of Biological Sciences in a memorial in *BioScience.* His effective service to the National Science Foundation was eloquently recognized in a tribute adopted by their Board. One paragraph in this tribute encapsulates his personality well:

"Detlev Bronk was truly a giant among men. He combined wit with wisdom, loved life, enjoyed people, and had interests of a universal scope. He was an avid skier and sailor and loved flying and mountain climbing. He gave equally of advice and concern whether to a young student or to a President of the United States. The world has lost an outstanding citizen and public servant, but will be forever enriched by the achievements and memories of a truly unique individual."*

* From "Tribute to Detlev Wulf Bronk adopted by the National Science Board at its 178th meeting, January 15-16, 1976."

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Vannevar Bush

March 11, 1890-June 28, 1974

By Jerome B. Wiesner

No American has had greater influence in the growth of science and technology than Vannevar Bush, and the twentieth century may yet not produce his equal. He was an ingenious engineer and an imaginative educator, but above all he was a statesman of integrity and creative ability. He organized and led history's greatest research program during World War II and, with a profound understanding of implications for the future, charted the course of national policy during the years that followed.

The grandson of two sea captains, "Van" Bush manifested his Cape Cod heritage in a salty, independent, forthright personality. He was a man of strong opinions, which he expressed and applied with vigor, yet he stood in awe of the mysteries of nature, had a warm tolerance for human frailty, and was openminded to change and to new solutions to problems. He was pragmatic, yet had the imagination and sensitivity of a poet, and was steadily optimistic. These essential qualities speak clearly in the foreword which he wrote in January 1970 for his book of reminiscences, *Pieces of the Action:*

> In my time, it has been my good fortune to have a piece of the action here and there in varied circumstances. It has been a pleasant experience for me to review some of the more rugged of these, and some of the more serene.
>
> Do birds sing for the joy of singing? I believe they do. The complexity of their songs is far greater than is needed for recognition or for marking

> of reserved areas. I have become acquainted with a catbird who obviously derives pleasure as he tries out little phrases on his own. Moreover, I believe that evolution produced birdsongs, and the joy that goes with them, because of the survival value they bestow.
>
> He who struggles with joy in his heart struggles the more keenly because of that joy. Gloom dulls, and blunts the attack. **We** are not the first to face problems, and as we face them we can hold our heads high. In such spirit was this book written.*

Van Bush gave the most comprehensive view of himself in *Pieces of the Action.* Characteristically, he despised pomposity and rather than write a formal autobiography he organized his recollections in a way that* would illuminate certain historical episodes and amplify some of his views of life. Written in a direct, down-to-earth manner, the book tells a great deal about the rugged, indomitable spirit of its author.

Bush's father, the Reverend Richard Perry Bush, was also a nonconformist in style and conviction. He started his career as cook on a mackerel smack at Provincetown, Massachusetts at the age of fourteen and worked his way through Tufts College by delivering coal to students' rooms. Although of a Methodist family, he became a minister in the Universalist Church and was a pastor in Everett, Massachusetts when his son was born on March 11, 1890. Story has it that the boy was named for the Reverend John Van Nevar, a colleague of the Reverend Mr. Bush. Between Vannevar Bush and his father there was a strong bond of affection, cemented by a good-humored appreciation in each one for the personality and idiosyncracies of the other. Both were members of the Masonic order, both were good outdoorsmen, and both were wide-ranging in their interests.

As a boy, Vannevar Bush loved to tinker. When his father became a pastor in Chelsea, where Vannevar attended high school, he had a versatile shop at home. After high school he moved on to Tufts College, where he received B.S. and M.S.

* Vannevar Bush, *Pieces of the Action* (New York: William Morrow, 1970), p. ix.

degrees in 1913. Also, while still in college, he secured a patent—the first of many—for a surveying machine, which he built with two bicycle wheels and a device using a pendulum, for integrating and recording horizontal and vertical measurements.

After graduating from Tufts, Bush worked for a time in the test department of the General Electric Company at Schenectady, New York, and then as an inspector for the U.S. Navy. He returned to Tufts in 1914 as an instructor in mathematics. He had higher goals, however, and one of them was to marry Phoebe Davis, a Chelsea girl. Having saved enough money for one more year of study, he proposed to earn a doctorate at the Massachusetts Institute of Technology in that one year so that he could qualify for a better job and afford to get married. There was academic skepticism that he could accomplish this, and he was warned that he would wreck his health; but in 1916, at the end of a year, he had earned a Doctor of Engineering, a degree at that time given jointly by MIT and Harvard University. His health was never better, a troublesome case of rheumatism having disappeared for good. That fall he and Miss Davis were married, and he became an assistant professor of electrical engineering at Tufts. His first technical paper, "Oscillating-Current Circuits by the Method of Generalized Angular Velocities," based on his doctoral thesis, was presented before the American Institute of Electrical Engineers in 1917.

At about that time, Bush became a consultant to American Research and Development Corporation (AMRAD), a small company with quarters on the Tufts campus which, with the backing of J. P. Morgan, was pioneering in the development of radio devices. When the United States entered World War I, Bush went to New London, Connecticut to engage in antisubmarine research for AMRAD. He developed a magnetic device for the detection of submarines, but because of faulty administrative coordination it was never used effectively—a circumstance that he would remember when he took charge of U.S. research dur ing World War II. "That experience," he wrote later, "forced into my mind pretty solidly the complete lack of proper liaison between the military and the civilian in the development of weapons in time of war, and what that lack meant."* He did not serve in the Navy during World War I, but he was a lieutenant commander in the Naval Research from 1924 to 1932.

In 1919, Bush joined the MIT faculty as associate professor of power transmission. He was placed in charge of the introductory course in electrical engineering and in 1922, with his colleague William H. Timbie, published a textbook, *Principles of Electrical Engineering.* Meanwhile, he had been made director of graduate study and of the Research Division of the Department of Electrical Engineering.

Bush not only continued to serve as a consultant to AMRAD, but was also largely responsible for its progress, despite numerous vicissitudes, toward success. He enlisted Laurence K. Marshall, who had been his roommate at Tufts, to provide business leadership. A new company, eventually named Metals and Controls Corporation, was formed to manufacture a thermostat invented by John A. Spencer, a staff member. Thermionic tubes for the booming radio industry were developed by another company, which took the name of Raytheon Manufacturing Company in 1925 and became a corporate giant. One of the tubes, the S tube, a gaseous rectifier, enabled the owner of a radio set to plug it into the household circuit rather than use what was known as a B battery. The tube was the subject of papers presented before the Institute of Radio Engineers and the American Institute of Electrical Engineers by the inventor, C. G. Smith, and Bush.

At MIT, Bush's interests turned toward computers. A former student, David O. Woodbury, recalls that in 1922 he was working on a master's thesis, assigned by Bush, dealing with three-

* *Pieces of the Action,* p. 74.

phase transients in alternating current motors. The research required onerous slide-rule computation, and Woodbury devised a small machine to do the work. One day Bush saw Woodbury using the machine and asked what it was. When Woodbury explained, the professor said, "Dave, give up all that slip-stick work and write us a thesis on your invention." Woodbury did, and sold the machine to General Electric Company.

The increasing complexity of power transmission networks stimulated further development in methods of analysis. Another of Bush's graduate students, Herbert R. Stewart, based a thesis on the Product Integraph, stating: "It was Dr. Bush's suggestion early in 1925 that a mechanical device should be developed to perform the continuous integration, which was the beginning of a continually expanding program of general solution of transients in networks by electromechanical means" (*A New Recording Product Integraph and Multiplier,* S.M. thesis, 1926).

The Product Integraph was the first in a series of analog computers which, though not direct ancestors of today's digital computers, led in the opening of the modern field of computation. In addition to Stewart, those closely associated with Bush in this development included Frank D. Gage, Harold L. Hazen, King E. Gould, and Samuel H. Caldwell. An advanced machine, called the Differential Analyzer, was completed in 1931 and was so successful that it was the model for the construction of similar machines elsewhere. It could solve sixth-order differential equations or three simultaneous second-order differential equations. Another complex device developed at that time by Harold Hazen and Hugh H. Spencer with Bush's leadership was the Network Analyzer, used in the simulation of power systems.

Preparation of the Differential Analyzer for solving a problem was a cumbersome process. Planning for a more versatile machine, which could be controlled by punched tape, was begun in 1935. Known as the Rockefeller Differential Analyzer because it was funded in part by the Rockefeller Foundation, it had 2,000 electronic tubes, 200 miles of wire, 150 motors, and weighed 100 tons. It was demonstrated for the first time in 1941 and throughout World War II was operated on a three-shift basis in the computation of Navy range tables and studies of fire-control systems, radar antennas, and other critical subjects.

Bush was by no means satisfied with the Differential Analyzer. As early as 1937 he wrote memoranda on the possibility of achieving greater speed with an electronic calculator—the Rapid Arithmetical Machine, as he called it. Preliminary studies of its feasibility and, in fact, of tubes and circuits that might be used were conducted, but investigators were diverted by war research demands, and it was not until the early 1950s that MIT began operating Whirlwind I, a high-speed, high-capacity, highly reliable digital computer.

Although Bush maintained a lively interest in such machines, his career had taken a new direction. He had strong views on education. For example, in "Critical Analysis of the Examination System of American Engineering Schools," he wrote:

> The student is hounded. In four years the student has to take some forty or fifty independently taught subjects in which he is examined formally a total of perhaps a hundred times, and informally several hundred times.. . . All but exceptional students become automatons.. . . Our examinations are poor.. . . Student memories are being taxed with data which any reasonable practicing engineer would keep in notes or a handbook.*

Dr. Karl T. Compton had become president of MIT in 1930, and as part of his program to strengthen the Institute, he reorganized it as three schools and appointed Bush vice president of the Institute and dean of the School of Engineering. In the latter position, Bush became virtually the operating executive. His national reputation was growing, and in 1934 he was

* Vannevar Bush, *Journal of Engineering Education,* 23, no. 5 (January 1933): 322-36.

elected to the National Academy of Sciences. The following year he served on the Committee on the Relation of the Patent System to the Stimulation of New Industries, organized by the Science Advisory Board of the National Research Council.

In 1938 Bush was invited to become president of the Carnegie Institution of Washington. President Compton was so loath to lose him that he suggested an arrangement by which he, Compton, would become chairman of the corporation and Bush would become president of MIT. Bush accepted the Carnegie invitation, however, and shortly afterward was also appointed chairman of the National Advisory Committee for Aeronautics (NACA). As he later put it, he soon "learned quite a bit of the mysterious ways in which one operates in the Washington maze."*

After World War II broke out in Europe in 1939, Bush and others became increasingly concerned by the lack of technological preparedness in the United States. He; James B. Conant, president of Harvard University, and Frank B. Jewett, president of the National Academy of Sciences and president of Bell Telephone Laboratories, were members of the Committee on Scientific Aids to Learning, formed by the National Research Council in 1937, and thus had occasion to meet together and discuss the subject. President Compton of MIT and Richard C. Tolman, dean of the Graduate School at the California Institute of Technology, also joined in these discussions. Irvin Stewart, who was secretary of the Committee on Scientific Aids to Learning, was likewise involved.

Out of the discussions came a plan for the establishment of the National Defense Research Committee (NDRC), which Bush described in four short paragraphs and submitted to President Roosevelt. At the end of ten minutes he had an "OK-FDR," and an order creating NDRC was issued on June 27, 1940, providing

* *Pieces of the Action,* p. 34.

nearly a year and a half of lead time before the United States entered the war. Bush commented thirty years later:

> There were those who protested that the action of setting up NDRC was an end run, a grab by which a small company of scientists and engineers, acting outside established channels, got hold of the authority and money for the program of developing new weapons. That, in fact, is exactly what it was. Moreover, it was the only way in which a broad program could be launched rapidly and on an adequate scale. To operate through established channels would have involved delays—and the hazard that independence might have been lost, that independence which was the central feature of the organization's success.*

Bush was appointed chairman, and other members of the committee, in addition to Compton, Conant, Jewett, and Tolman, were Conway P. Coe, Commissioner of Patents; Rear Adm. Harold G. Bowen, representing the Navy; and Brig. Gen. George V. Strong, representing the Army. Stewart became the executive secretary.

The organization was elaborated in 1942, when the Office of Scientific Research and Development (OSRD) was established, with Bush as its director. OSRD had three principal subdivisions at that time: the NDRC, with Conant as chairman; the Committee on Medical Research (CMR), with A. Newton Richards as chairman; and the Advisory Council, with Bush as chairman. The latter, which included the chairmen of NACA, NDRC, and CMR, as well as Army and Navy representatives, served as a coordinating group. In addition, Bush was chairman of the Joint New Weapons Committee of the Joint Chiefs of Staff and, when the Manhattan District was created, chairman of its Military Policy Committee, which functioned as its board of directors.

Although a certain organizational complexity was inevitable in so large a program, OSRD and NDRC operations were simplified

* *Pieces of the Action,* pp. 31-32.

by the fact that Van Bush was unquestionably the boss. He had the full confidence of the President and Congress. He was decisive and could be tough. "I remember one time when a section walked into my office and resigned as a body," he wrote. "I still do not know quite what the row was about. So I just told them, 'One does not resign in time of war. You chaps get the hell out of here and get back to work, and I'll look into it.' "* His wisdom and integrity were respected.

The organization was a remarkable invention, but the most significant innovation was the plan by which, instead of building large government laboratories, contracts were made with universities and industrial laboratories for research appropriate to their capabilities. OSRD responded to requests from military agencies for work on specific problems, but it maintained its independence and in many cases pursued research objectives about which military leaders were skeptical. Military tradition was that a war had to be fought with weapons that existed at its beginning. Bush believed that World War II could be won only through advances in technology, and he proved to be correct. In some instances, the armed forces were enthusiastically cooperative. In others, resistance to innovation had to be overcome. Bush, himself, went to Europe to make sure that the proximity fuse was introduced to the battlefield and used effectively.

The major exception to the policy of avoiding the building of government laboratories was in the development of the atomic bomb. After preliminary studies by NDRC and OSRD, it became clear that a colossal program would be needed, and Bush recommended to Secretary Stimson that the Army take over the responsibility. The result was the formation of Manhattan Engineering District by the Corps of Engineers. Bush,

* *Pieces of the Action,* p. 41.

with Conant as his deputy, maintained an active scrutiny of the enterprise.

Bush successfully confronted Sir Winston Churchill (and earned his wrath) in London in an argument over the terms of exchanging atomic information. He had the duty, after the death of President Roosevelt, of giving President Truman his first detailed account of the bomb. He was among those whose recommendations prevailed when the President decided—in spite of some objections—that the Smyth Report on atomic energy should be released. He urged the appointment of the Interim Committee to advise the President on use of the bomb and on postwar atomic energy, and he was then appointed a member of the committee. He was a participant in the "Atlee Conference" and prepared the final draft of an agreement with the British proposing control of atomic energy by the United Nations. He was a defender of Dr. J. Robert Oppenheimer. After the Atomic Energy Commission's (AEC) decision that Oppenheimer's clearance be cancelled, he stated: "It does not affect my complete confidence in Dr. Oppenheimer's loyalty and deep devotion to the security of the United States... Our internal security system has run wild."*

Bush did not have a central role in the formation of the AEC, but his voice was heard on this and other issues, such as military unification. He was influential in developing a policy of maintaining a high level of research for the military services and was instrumental in organizing the Office of Naval Research. But his greatest contribution was to launch an unprecedented national program in science and technology.

Long before the war was over, Bush began to devote thought to how the momentum of research could be sustained, with new peacetime goals. In a letter, President Roosevelt asked him to make recommendations on government policies for combating

* *Newsweek,* July 12, 1954, pp. 24-25.

disease, supporting research, developing scientific talent, and diffusing scientific information. Bush, on the basis of studies made by four committees which he organized, responded with a report titled "Science—The Endless Frontier," which provided a blueprint for far-reaching federal policies. "One of our hopes is that after the war there will be full employment," Bush said in the report. "To create more jobs we must make new and better and cheaper products. We want plenty of new, vigorous enterprises. But new products and processes are not born fullgrown. They are founded on new principles and new conceptions which in turn result from basic scientific research. Basic scientific research is scientific capital."*

Use of the term "basic research" was not a casual choice. Bush explained later: "There were some on Capitol Hill who felt that the real need of the postwar effort would be the support of inventors and gadgeteers, and to whom science meant just that. When talking matters over with some of these, it was well to avoid the word fundamental and use basic instead."† To provide an organization for the support of basic research, Bush proposed the creation of a National Research Foundation, which would administer fellowships and scholarships and would "place its research contracts or grants not only with those institutions which have a demonstrated research capacity but also with other institutions whose latent talent or creative atmosphere affords promise of research success."‡

Since 1942 Senator Harley Kilgore had been seeking passage of a bill providing for the support of science and technology, and in the spring of 1945 the bill was modified to provide for the establishment of a national science foundation. Its provisions, tending to favor applied research, were unacceptable to

* J. Merton England, "Dr. Bush Writes A Report: 'Science—The Endless Frontier,' " *Science,* 191 (January 9, 1976):2.

† *Pieces of the Action,* p. 65.

‡ "Science the Endless Frontier," p. 32.

Dr. Bush, whose own recommendations were embodied in a bill introduced by Senator Warren Magnuson. For two years there was debate on the bills. Finally a compromise bill was passed in 1947, with National Science Foundation (NSF) as the name for the new organization. It was vetoed by President Truman on grounds that the director would be appointed by the foundation's board rather than by the President and that he "would be deprived of effective means of discharging his constitutional responsibility."*

The bill was passed a second time, and Bush later related, "I managed to convince Truman he should not veto it again. But I did so on the basis that he was being given protection, a buffer against those coming to seek favors."†

An expectation had been that Bush would be chairman of NSF, but he asked President Truman not to name him to the board, saying, "I have been running about everything scientific during the war, and somewhat since, and I think people are getting tired of seeing this guy Bush run things around here. I think this outfit would be better if it had some new leadership. If you put me on the board, they will elect me chairman, and I do not think the body of scientists are going to like this continuation of one man in the top post."

President Truman remarked, "Van, you should be a politician. You have some of the instincts."

"Mr. President, what the hell do you think I've been doing around this town for five or six years?" was the response.‡

Bush continued to be "around town," and he saw NSF assume the kind of character he had envisioned for it. He served on its Advisory Committee on Government-University Relationships for two years. He was chairman of the Joint Research

* Detlev W. Bronk, "The National Science Foundation: Origins, Hopes and Aspirations," *Science,* 188 (May 2, 1975): 409-14.

† *Pieces of the Action,* p. 65.

‡ Ibid., p. 302.

and Development Board of the War and Navy departments in 1946-1947 and then chairman of the Research and Development Board of the National Military Establishment in 1947-1948. But he withdrew from active leadership in government affairs, and in 1955 retired as President of the Carnegie Institution. Of his service there, Caryl P. Haskins, his successor, observed that "His great gifts of intellect, of personality, and of administrative ability brought to the Institution one of the most formative and dynamic periods inspired by any president in its history, not even excepting the first, Daniel Coit Gilman."* One important accomplishment was an agreement between Carnegie and the California Institute of Technology for the joint operation of the Mt. Wilson and Palomar observatories.

During his retirement, Bush made his home on a hill in Belmont, Massachusetts, with a panoramic view of Cambridge and Boston. He was elected chairman of the MIT Corporation (of which he had been a member since 1932) in 1957 and was honorary chairman from 1959 to 1971. James R. Killian, Jr., former president, who succeeded him in these positions, commented that "Four M.I.T. presidents benefitted from his advice. They were, in fact, the students of his latter days. In this and other ways he showed unwavering devotion to the Institute and never lost his enthusiasm for its mission and potential.† MIT named its Center for Materials and Engineering the Vannevar Bush Building in his honor.

Bush had become a member of the board of Merck & Co., Inc. in 1949, and when George Merck, chairman of the board, died, he was elected to that position in 1957 and actively participated in the company's affairs. He had a deep interest in the advancement of medicine. In the formation of the Com

* Biographical Memoirs, *Year Book of the American Philosophical Society* (Philadelphia: American Philosophical Society, 1974), pp. 120-27.

† Memorial service for Dr. Vannevar Bush, MIT, October 4, 1974.

mittee on Medical Research under OSRD, he repelled reactionary influences. During World War II, the death rate from disease in the Army was reduced to 0.6 per thousand, compared to 14.1 during World War I, and this was due in part to the effectiveness of the committee's program, notably in making penicillin available early and in large quantities and in consolidating pharmaceutical industry talents. Bush's interest in medicine continued through the years, and he later invented an automatic microtome, a silicone rubber valve for the heart, and a gold valve for use in hydrocephalus.

Although Bush has been called a scientist, and justifiably so because of his broad and profound understanding of science, he preferred to regard himself as an engineer. He was always fascinated by practical applications of science and was never happier than when he could work with his own hands in their achievement. He had shops at his home in Belmont and his summer cottage at South Dennis on Cape Cod, where he not merely tinkered but also attacked difficult problems with high skill. He had fun devising a bird feeder that was inhospitable to greedy pigeons and blue jays, and he worked doggedly for years to solve the problems of gas and free piston engines. He obtained three patents for the latter, in addition to a score of other patents for devices ranging from thermostats to a machine for rifling guns.

At one time Bush had a turkey farm in New Hampshire, but throughout his life he was devoted to salt water and boats. He loved cruising and was too independent-minded for conventional racing. For his ketch he designed unorthodox but efficient sails, ignoring the disapproval of nautical conformists. He was enthusiastic about the potential of hydrofoil boats and participated in designing, building, and testing them.

The most persistent line of Bush's inventive endeavors involved technology for processing information. The Differential Analyzer was the most important product of such activity, but his interests led in other directions. At MIT in the thirties he designed a decoding machine for the Navy. In 1936 he initiated the development of a machine which he called the Rapid Selector, employing 35-mm film, on which microphotographed texts could be made quickly available by the use of photoelectric cells in scanning a coded index. His application for a patent was rejected, but development of the machine was carried forward until World War II interrupted, when the two men working on it were suddenly shipped off to Washington for decoding work in the Navy.

Bush did not lose interest in speeding up the cumbersome process of searching through masses of data. In 1945 he wrote an article for the *Atlantic Monthly* describing "memex," a system by which a researcher sitting at a desk could have almost instant access to microphotographed books, periodicals, and other materials and could use a mechanized "trail" to assist in searching for relevant information.

Twenty years later Bush took part in the inauguration of a program to develop such technology for library use, Project INTREX, which was undertaken by an MIT group. In an essay titled "Memex Revisited,"* he pointed out that the development of the digital computer, the transistor, video tape, and other such devices had heightened the feasibility of such mechanization but that costs would delay its achievement. And although Project INTREX demonstrated that technical problems could be solved, economic ones, as Bush feared, remained a barrier.

The stroboscopic light developed by Bush's former colleague in electrical engineering, Harold E. Edgerton, was used in the Rapid Selector. It was applied with greater success in Photon, a machine for setting type photographically, which was developed by Graphic Arts Research Foundation, Inc., a Cambridge

* Vannevar Bush, *Science Is Not Enough* (New York: William Morrow, 1967), p. 75.

based enterprise of which Bush was one of the founders. The computer-controlled "cold type" method is now widely used in the printing industry. Bush also held patents for a justifying typewriter and, with Professor Caldwell, for an apparatus for generating continuously variable mechanical operations.

Having had personal experience with patents as well as in administration, Bush maintained a continuing interest in the patent system and was active in seeking its improvement. As director of OSRD, he believed inventions developed with government funds should not be exploited for private profit, and he developed strong patent policies. He had resigned from the Raytheon board when he went to Washington, and although the company became one of the leading industrial contractors in the field of radar, he scrupulously avoided favoring it. At the end of the war, his friend, Laurence K. Marshall, president of Raytheon, claimed the right to patent certain inventions. Bush threatened to fight the issue in the courts. In the end, they agreed to the appointment of an impartial committee which would determine what patents Raytheon could claim, but their long friendship ended.*

Bush was a strong believer in free enterprise and the work ethic. "I had grown up with a deep-seated distrust of most social innovators, whom I regarded as a bunch of long-haired idealists or do-gooders," he wrote. He had been "appalled at some of F.D.R.'s political theory and practice," though his views mellowed as he came to revere President Roosevelt, and his loyalty to him was absolute.†

"I am all for a welfare state in which a powerful government seeks to protect its citizens against the cruelties of nature and chance, and incidentally against the rapaciousness of their fellow citizens," he said in an essay, "Poverty and Oppor

* Otto J. Scott, *The Creative Ordeal* (New York: Atheneum, 1974), p. 173.
† *Pieces of the Action,* p. 35.

tunity."* "But just trying to abolish poverty leaves me cold. · . . From here on we should not equalize real incomes if we wish to preserve our prosperity and our safety. In the great social pyramid, there should be tangible rewards for those who rise. A state in which all material rewards are cancelled out will not long exist in a turbulent world." He wanted to see "dignity and satisfaction for those who contribute to our well-being" and equality of opportunity for all. He thought that:

> To accomplish this, or part of it, may involve a return to the village, not isolated in the hills, but surrounded closely in the city, the local community looking after its own affairs, the informal groups that hang together because of common interests. Our trends have been in the opposite direction, centralization of power, dictation from above. Even so, there has never been a time, or a country, in all history in which barriers that block the individual's path to success, material or intellectual, were so broken down as here and now. This is the hallmark of our way of life.†

Although Van Bush had consorted with the powerful and himself had exercised enormous power, although he was a brilliant technologist, although he shared the awesome view of nature disclosed by science, his devotion to individualism and the ideal of a simple life was central to his character.

Bush had been in failing health for more than a year when he suffered a cerebral vascular accident, developed pneumonia, and died at the age of eighty-four on June 28, 1974. Mrs. Bush had died in 1969. Bush was survived by two sons, Dr. Richard Davis Bush, a surgeon, and John Hathaway Bush, president of Millipore Corporation, by six grandchildren, and by a sister, Edith L. Bush of Provincetown, Massachusetts.

* Vannevar Bush, *Science Is Not Enough* (New York: William Morrow, 1967), pp. 123-39.

† *Science Is Not Enough,* p. 138.

HONORS AND DISTINCTIONS

PROFESSIONAL AND HONORARY SOCIETIES

American Physical Society, Fellow, 1923

American Society for Engineering Education, Fellow, 1923; Honorary Fellow, 1961

American Institute of Electrical Engineers, Fellow, 1924; Honorary Fellow, 1950

American Academy of Arts and Sciences, Fellow, 1925 National Academy of Sciences, elected 1934

American Mathematical Society, Fellow, 1936 American Philosophical Society, Fellow, 1937

Franklin Institute, Honorary Member, 1947

Society of Naval Architects and Marine Engineers, Honorary Member, 1951

American Society of Mechanical Engineers, Honorary Member, 1955

American College of Surgeons, Honorary Fellow, 1956

Phi Beta Kappa

Sigma Xi

Tau Beta Pi

Eta Kappa Nu (Eminent Membership, 1950)

SOCIAL ORGANIZATIONS

Alpha Tau Omega

St. Botolph Club, Boston

Century Association, New York

AWARDS

Louis Edward Levy Medal, Franklin Institute, 1928

Lamme Medal, American Institute of Electrical Engineers, 1935

Research Corporation Award, Columbia University, 1939

Ballou Medal, Tufts University, 1941

Edison Medal, AIEE, 1943

Holley Medal, American Society of Mechanical Engineers, 1943

John Scott Award, Philadelphia City Trust, 1943

Gold Medal, National Institute of Social Sciences, 1945

Distinguished Service Medal, Roosevelt Memorial Association, 1945

Marcellus Hartley Public Welfare Award, National Academy of Sciences, 1945

Washington Award, Western Society of Engineers, 1946

Hoover Medal for 1946, AIEE, ASCE, AIMME, ASME, 1947

Distinguished Service Award, Tufts Alumni Council, 1947

Medal for Merit with Bronze Oak Leaf Cluster, President Truman, 1948

Knight Commander, Most Excellent Order of the British Empire, 1948

Medal, Industrial Research Institute, Inc. 1949

John Fritz Medal, AIEE, ASCE, AIMME, ASME, 1951

Award of Merit, American Institute of Consulting Engineers, 1953

John J. Carty Medal and Award for the Advancement of Science, National Academy of Sciences, 1954

William Proctor Prize, Scientific Research Society of America, 1954

Officer, Legion of Honor, France, 1955

New England Award, Engineering Societies of New England, 1957

Charles F. Kettering Award, George Washington University, 1952

1963 National Medal of Science, President Johnson, 1964

Great Living American Award, Chamber of Commerce of the United States, 1964

Citation, Brotherhood of Temple Ohabei Shalom, Brookline, Massachusetts, 1964

Wisdom Award of Honor, The Wisdom Society, 1965

First Annual Founders Medal, National Academy of Engineers, 1966

Distinguished Service to Science Education Citation, National Science Teachers Association, 1968

Atomic Pioneer Award, President Nixon, 1970

BOARDS

Life Member, Massachusetts Institute of Technology Corporation; Chairman, 1957-1959; Honorary Chairman, 1959-1971

Regent, Smithsonian Institution, 1943-1955

Trustee, Tufts College, 1943-1962 (Emeritus)

Trustee, Johns Hopkins University, 1943-1955

Trustee, Carnegie Corporation of New York, 1939-1950

Trustee, Carnegie Institution of Washington, 1958-1974

Trustee, George Putnam Fund of Boston, 1956-1972

Director American Telephone and Telegraph Co., 1947-1962

Director, Merck & Co., Inc., 1949-1962; Chairman of Board, 1957-1962

Director, Metals and Controls Corporation, 1952-1959

Director and Life Member, Graphic Arts Research Foundation, Inc., 1949-1974

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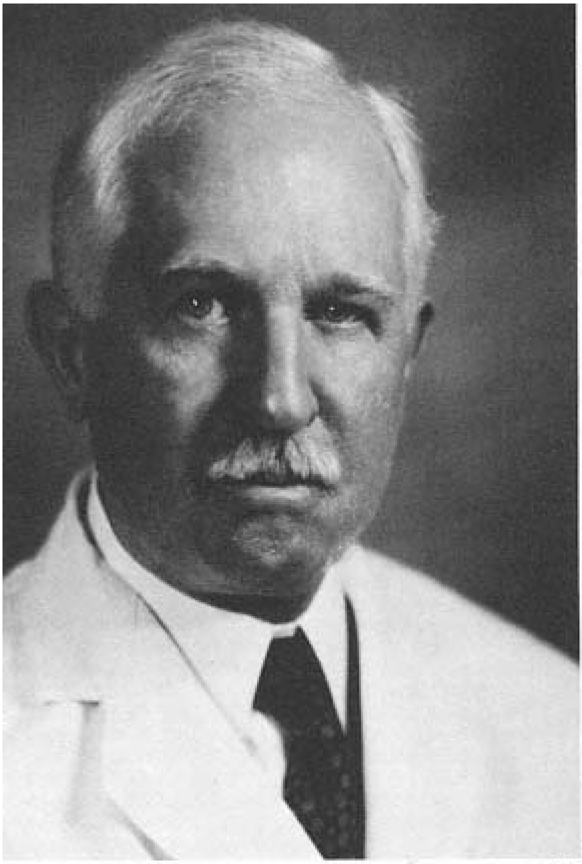

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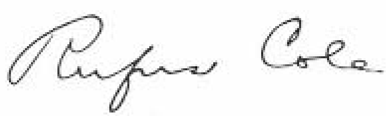

Rufus Cole

April 30, 1872-April 20,1966

By C. Phillip Miller

Rufus Cole, the first Director of the Hospital of the Rockefeller Institute for Medical Research and a pioneer in the development of clinical research, was born in Rowsburg, Ohio on April 30, 1872, the son of Ivory Snethen and Ruth Smith Cole. His father was a physician, as were two uncles. They and a fourth brother all served in the Union Army during the Civil War, Rufus' father as a contract doctor in Cincinnati. One uncle was killed in the conflict.

Cole's paternal ancestry was typically Yankee. James Cole had emigrated from England to Plymouth, Massachusetts in 1633. Succeeding generations lived in various places in New England, gradually moved west, and finally settled in Peru, Illinois. Cole's mother's family, the Smiths, also of Yankee stock, had lived in Ohio for several generations.

Rufus' father practiced medicine in Peru and the adjoining town of La Salle, Illinois. In his later years, Cole often recounted to his family his recollections of calls made with his father to visit patients in those towns, a boyhood experience that influenced him to become a physician. Rufus' mother encouraged him in his determination to continue his education beyond high school, although that required him to earn money as best he could to pay for it. In 1892 he entered the University of Michigan in Ann Arbor, where he graduated in 1896. He had origi nally intended to stay in Ann Arbor to study medicine, but in 1893 he was so impressed by an exhibit of the Johns Hopkins Medical School he chanced to see at the World's Columbian Exposition in Chicago that he changed his plans. Subsequently, he applied for admission to that school, and was accepted.

At the Hopkins, he came under the influence of William H. Welsh, William Osler, Lewellys F. Barker, and other celebrities on the faculty at that time. After his graduation in 1899, he was appointed to the resident staff of the Johns Hopkins Hospital and served his first year under Osler, for whom he retained an abiding admiration. This was Osler's last year before leaving Baltimore to become the Regius Professor at Oxford. Osler was succeeded by Barker, who, unlike his predecessor, was an advocate of the full-time system. Before he became Professor of Medicine at Johns Hopkins, Barker had begun a scientific career in anatomy at the University of Chicago. He had furthermore been much impressed by the excellent research he had seen in the great German university clinics. It was while he was still working in the Department of Anatomy at Chicago that Barker, in a speech at a dinner meeting of Hopkins alumni, advanced the novel idea that professors in the clinical disciplines should devote "whole time" to their academic duties and be relieved of the necessity of engaging in private practice.

When Barker succeeded Osler as Professor of Medicine, he began to establish laboratories adjacent to the wards and encouraged his house staff to engage in research employing scientific methods of the basic disciplines, thereby initiating a promising advance in American medicine. Never before had hospital laboratories served as more than convenient places in which to make routine diagnostic tests.

Cole was the first man appointed by Barker to take charge of one of the laboratories—the one designated the biological laboratory—and he proceeded to carry out a systematic clinical and bacteriological study of typhoid fever, a disease prevalent in Baltimore at that time. By culturing the blood of patients, he showed that typhoid bacilli were present early in the disease. This work attracted considerable attention because it had been carried out in a routine clinical laboratory. It started a program which was to grow as Barker had hoped and eventually contributed significantly to the development of clinical medicine in America.

Cole stayed on at the Hopkins Hospital until 1909, rising in rank with each succeeding appointment. In those days such long terms of residency training were not unusual at the Hopkins, providing the incumbent was a man of exceptional promise, as was Cole. He spent the year 1903-1904 at Robert Koch's Institut für Infektionskrankheiten in Berlin working in the laboratory of Professor Wassermann, and published a paper on the differential agglutinability of different strains of typhoid bacilli.

In 1908 Cole was married in La Salle, Illinois to Annie Hegeler, whom he had known ever since they were childhood schoolmates. It was an exceptionally happy marriage, to which were born three daughters: Camilla Ruth (Mrs. Thomas R. Smidt), Elizabeth Anne (Mrs. William G. F. Botzow), and Mary Hegeler (Mrs. Mary Cole Childs).

Mrs. Cole was the daughter of Edward Carl Hegeler, a metallurgical engineer born and trained in Germany, who with Frederick Matthiessen constructed in La Salle the first successful zinc smelter in America and established the Matthiessen and Hegeler Zinc Company, which quickly became a very prosperous enterprise. Hegeler was not only a successful mining engineer, but also a serious student of philosophy and religion who, in 1887, founded the Open Court Publishing Company, which has published many books in those fields and issued the periodicals *The Open Court* and *The Monist,* the latter of which is still being published.

Later in 1908 Cole was appointed the first Director of the Hospital of the Rockefeller Institute for Medical Research. Al though the Hospital was not to be opened until two years later, Cole immediately began to formulate his plans for its organization and, more importantly, for its purpose, which was to advance the scientific study of clinical medicine. In 1909, in order to gather ideas for his hospital, Cole, accompanied by Mrs. Cole and their baby daughter, went abroad to observe the work going on in the university clinics in Britain and on the Continent.

The beginnings and development of the Rockefeller Institute have been admirably described by George W. Corner in his *History of the Rockefeller Institute 1901-1953* (New York: Rockefeller Institute Press, 1964). It was the Reverend Mr. Frederick Taylor Gates who first aroused John D. Rockefeller's interest in establishing an institute for medical research. Mr. Gates, ever since his pastorate in Minneapolis, had been obsessed by the need for better medical care for the sick. Furthermore, his perusal of Osler's famous textbook had shown him how many diseases were imperfectly understood and convinced him of the importance of supporting research in clinical medicine.

During several years at the turn of the century, a Board of Trustees for the Institute was assembled, a procedure to which Mr. Rockefeller and his son John D. Rockefeller, Jr. devoted much thought. In 1902 Dr. Simon Flexner was appointed Director of the Institute, and in 1904 work was begun in rented quarters which served as laboratories until the completion of the first of the new buildings at their present site on York Avenue.

From the beginning, the Trustees had planned for a small hospital where a few patients might be studied intensively, for, as Dr. Flexner had mentioned in one of his plans for its organization, "the Institute should never lose sight of the immediate problems of human disease" and "there should be attached to the Institute a small hospital for the study of special groups of disease" (Corner, p. 89). The number of beds was finally set at fifty.

Cole persuaded the Trustees to adopt the "full-time" system, which meant that the Hospital should have no "attending physicians," i.e., physicians who earned their living in private practice. Cole insisted that his staff should be composed of men dedicated to the scientific study of human disease. They were to be clinicians competent not only to care for patients suffering from a particular disease, but also competent to carry out such laboratory investigations as might throw light on that disease, and they were to be paid salaries which would permit them to do so.

Although the Institute already housed laboratories in which distinguished scientists were busily engaged in research on their several problems, Cole insisted that his hospital be provided with its own laboratories so that his clinicians could pursue their investigations close to the wards which housed their patients. To this end, the most modern equipment was installed, not only for the laboratories but for the care of patients as well. Provision was also to be made for quarters for experimental animals.

It was a bold plan, one which envisioned the establishment of a hospital altogether unique at the time, and which, incidentally, was very expensive to operate. But Rockefeller was persuaded by his son to provide an endowment adequate for its support. One stipulation was made: patients were never to be charged for their hospital care.

The Hospital opened in October 1910 with beds for fifty patients. In order to avoid the problems of the customary nurses' training school, the entire nursing service was in the hands of salaried graduate nurses.

In planning the living quarters for his house staff, Cole did not forget the wisdom of John Shaw Billings, who, in designing the buildings of the Johns Hopkins Hospital, had allocated for the house staff spacious bedrooms with a pleasing outlook and with a library close at hand. Cole was thus able to offer his resident physicians very comfortable quarters with a bedroom apiece; an office for the head resident; a dining room where they ate breakfast and dinner; and a lounge, containing a small library, in which afternoon tea was always served—a pleasant occasion for informal discussion which often resulted in profitable exchange of ideas. For their midday meal the Hospital staff went to the lunch room in the main building and joined the men working in the laboratories of the Institute, an arrangement that afforded opportunity for friendly personal contact with some of the most distinguished scientists in America.

The staff of the Hospital worked together as a congenial and scientifically productive group. A telling indication of their respect and affection for Dr. Cole is the fact that, in speaking about him, they always referred to him as "the Director." Among the residents it was no secret that the comforts afforded them were designed to encourage them to postpone marriage and continue their scientific development. "Poverty and celibacy" was often mentioned as their way of life, although they were well aware that the senior members of the staff were all married and most of them financially independent. However, Dr. Cole himself had known poverty in his youth and realized the advantages of celibacy during the beginning of a young man's career.

The five diseases originally selected for investigation in the Hospital were pneumonia, poliomyelitis, syphilis, heart disease, and "intestinal infantilism," now known as celiac disease, which was included for a time because it was being studied by Dr. Christian Herter, a member of the Board of Trustees.

Cole chose as his special problem lobar pneumonia, a disease which at the time was so prevalent and caused such a high mortality that Osler called it "the captain of the men of death." Cole took under his wing a small team of young assistants who made an intensive study of that disease and its causative organism—the pneumococcus. By immunizing horses with type I pneumococci, they produced a serum which was the first effec tive treatment for pneumonia caused by that type. Eventually they published an important monograph on pneumonia, *Acute Lobar Pneumonia. Prevention and Serum Treatment* (see bibliography). Incidentally, it should be mentioned as an example of Cole's modesty that the authors were listed alphabetically, not with his name in first place.

One of Cole's great contributions resulted from his appointment as a co-worker of a young Canadian bacteriologist, Oswald T. Avery, who devoted the rest of his career to the study of the pneumococcus. Cole encouraged him and appointed chemists to assist him in his efforts to understand the chemical composition of that microorganism and then to explain the transformation of one type of pneumococcus into another. This work led eventually to the discovery that deoxyribonucleic acid (DNA) was the molecule that altered the heredity of the pneumococcus cell—a finding which initiated the subsequent experimental studies on heredity.

Besides these bacteriological and immunological investigations, many others were pursued in biological chemistry and physiology, each making some contribution to an understanding of one of the diseases being studied at the Hospital. Thus it was that under Cole's direction a vast number of contributions, many of them important, were made to the advancement of clinical medicine and hence to an understanding of disease in man.

During the military mobilization which anticipated the U.S. entry into World War I, Cole interrupted his duties at the Hospital in response to a request by the Surgeon General of the Army that he investigate several outbreaks of pneumonia among troops in army camps. He thereupon made a careful study of such data as were available on the incidence of pneumonia and its mortality, going into the Surgeon General's records as far back as the Civil War. He compared these data with such comparable ones as he could obtain for European armies. It should be mentioned in passing that at that time pneumonia was not included among the "controllable diseases" listed by the U.S. Public Health Service.

Cole pointed out in his reports that pneumonia was an important cause of death among troops—especially among raw recruits drawn from rural populations and housed of necessity in crowded barracks. He gave consideration to the advisability of vaccination against pneumococcus types I and II, but decided that procedure was impracticable under the circumstances. He stressed the important role of direct contact in the spread of pneumonia, an explanation not widely held at the time.

Cole called attention to the outbreaks of pneumonia which had occurred in Panama during the building of the Canal and also to those among the miners in South Africa. In both instances the highest mortality was suffered among the immigrant workmen, mostly blacks, who had previously escaped exposure to the causative microorganism and were therefore much more susceptible to it than the whites, who had acquired some degree of immunity.

He also pointed out that, although pneumococci were present in the throats of healthy individuals, it was types I and II which were the causative microorganisms in 60 percent of the cases of lobar pneumonia and that they soon disappeared during convalescence. Those types could, however, be cultured from the throats of immediate contacts and from the dust collected from the rooms of patients infected with either of them. These were pioneer observations on the epidemiology of pneumonia.

When Cole was sent by the Surgeon General to Fort Sam Houston, Texas to investigate a serious outbreak of pneumonia there, he was able to report from bacteriological examinations, in which he was assisted by A. R. Dochez and Avery, that, although a few cases of pneumococcal pneumonia were occurring, most of the cases of pneumonia were caused by infection with hemolytic streptococci as a complication of measles.

One of Cole's great contributions to American medicine was the training of the men who worked in his hospital and, like him, became dedicated to the study of disease. At the time he retired from the directorship of the Hospital, a tabulation was made of the positions then held by all the men, American and foreign, who had worked there. It showed that there were at that time 140 with university affiliation, of whom 112 held fulltime academic appointments in senior or junior positions, and 28 held part-time positions in academic institutions, including 3 deans and 11 with professorial rank. Among them were 22 who had been elected to the National Academy of Sciences and 46 to the Association of American Physicians, of whom 5 had received the Kober Medal, its highest award.

Such was the record of his accomplishment in the training of men to carry on investigation and instruction in clinical medicine and in the basic disciplines of biochemistry, physiology, and bacteriology in the medical schools of this and several foreign countries. It was a remarkable record of the results of Cole's stewardship as Director of the Hospital of the Rockefeller Institute for Medical Research.

One more of Cole's contributions to the development of clinical medicine in the United States resulted from his conclusion that the University of Chicago was the most appropriate institution in which to establish a medical school where all members of the faculty, including those in the clinical departments, would be on full time, i.e., would hold academic appointments that provided adequate salaries and forbade clinicians to engage in private practice. The University of Chicago already had on its campus strong departments in all the basic sciences which offered preclinical instruction for medical students who then went for their clinical training to Rush Medical College, an affiliated institution on the west side of Chicago.

Cole had for years held the opinion that clinical medicine, i.e., the study of disease in man, was no longer just an art or an applied science, but had become a discipline in its own right, worthy of recognition by a university as one of the biological sciences and accorded academic status comparable to that of chemistry or history or Latin. This goal was attained at the University of Chicago when hospitals were built on its campus, adjacent to the laboratories of the preclinical sciences, and staffed entirely by clinicians on full time.

After Cole's retirement from the Hospital of the Rockefeller Institute in 1937, he and Mrs. Cole gave up their apartment in New York and moved to Cohomong Wood, their beautiful Mt. Kisco, New York estate, which they owned for many years and had used as a weekend retreat from administrative duties at the Hospital. There they maintained the tradition of gracious hospitality which, through the years, had meant so much to Cole's associates, particularly the younger members of his staff. Although Mrs. Cole's death in 1951 brought great sorrow, Cole remained in the home they had created together, beloved and respected both by children and grandchildren, and by nieces and nephews, in whose accomplishments he took great pride and satisfaction.

Cole did not vegetate. He continued to be active for some years on a number of boards and committees and devoted a good deal of time to improving the nursing care of the sick in various institutions in the vicinity of Mt. Kisco, for such care had long been one of his concerns.

He also rekindled his interest in gardening and published papers on the distinguishing features of English gardens; he continued to paint in watercolors and oils; he wrote poetry, mostly unpublished, which demonstrated his ability to put down his thoughts in beautifully expressed cadences; and he took great pleasure in music.

In addition, he busied himself in things more strictly intellectual. One of his particular interests was the library of the New York Academy of Medicine, to which he gave his valuable collection of the works of Francesco Redi, the seventeenth-century Italian scientist and poet, many of them in first editions.

His most scholarly undertaking during the period of his retirement was the writing of a history of the social, political, and religious developments of the late sixteenth and entire seventeenth century in England, entitled *Human History: The 17th Century and the Stuart Family* (1959). This was an extraordinary accomplishment for a man of his age and predominantly scientific background, for it shows his ability to make use of source material centuries old. It also exemplifies Cole's ability to relate complicated historical events in easily read, enjoyable English prose.

Cole was able to continue his active intellectual life until his ninety-fourth year. He died on April 20, 1966 in Washington, D.C., where he had gone to receive the Kovalenko Medal, an award bestowed by the National Academy of Sciences on a member for "important contributions to medical sciences." Unfortunately, he was stricken with a rapidly fatal attack of pneumonia, the disease to which he had devoted so many years of study. He had, however, been able to enjoy the knowledge that he had been chosen the recipient of this most distinguished award in recognition of his contributions to the advancement of the study of disease.

To Rufus Cole's three daughters, especially to Mrs. Mary Cole Childs, the author is indebted for much helpful information about their father's life and personal interests.

HONORS AND DISTINCTIONS

Degrees

University of Michigan, B.S., 1896
The Johns Hopkins University, M.D., 1899
The University of Chicago, D.Sc. (Honorary), 1927
National University of Ireland, D.Sc. (Honorary), 1933

Student Honor Societies

Phi Beta Kappa
Nu Sigma Nu
Alpha Omega Alpha

Professional Appointments

The Johns Hopkins Hospital
Resident House Officer, 1899-1900
Assistant Resident Physician, 1900-1904
Instructor in Medicine, 1901-1904
Resident Physician and Associate in Medicine, 1904-1906
Assistant Physician in charge of the Biological Division of the Clinical Research Laboratory, 1906-1909
Research Student under Professor A. Wassermann, Robert Koch Institut für Infektionskrankheiten, Berlin, 1903-1904
Director of the Hospital of the Rockefeller Institute for Medical Research and Member of the Rockefeller Institute, 1908-1937;
Member Emeritus, 1937-1966
Board of Scientific Directors, International Health Division, Rockefeller Foundation, 1929-1936
Chairman, Finance Committee, District Nursing Association of Northern Westchester County, 1930
Board of Managers, St. Luke's Hospital, New York, 1938-1946
Board of Managers, Memorial Hospital, New York, 1938-1944
Advisory Committee, Department of Welfare, Westchester County, 1935
Consultant in Bacteriology, New York State Department of Health, 1936
Consulting Physician, Willard Parker Hospital, 1912-1920

Awards

Médaille d'Honneur de l'Assistance Publique de la République Française, 1926
Kober Medal, Association of American Physicians, 1938
Academy Medal, New York Academy of Medicine, 1953
Kovalenko Award, National Academy of Sciences (Posthumously), 1966

Memberships

Danish Society for Internal Medicine, 1920
Medical Society of Sweden, 1920
Copenhagen Medical Society, 1938
Institute of Medicine, Chicago, 1938
American Academy of Arts and Sciences, 1921
American Association for the Advancement of Science, 1912
Vice President and Chairman of Section N, 1927
American Association of the History of Medicine, 1925
American Association of Pathologists and Bacteriologists, 1915
Retired member, 1939
American Association of Immunologists, 1917
American College of Physicians, Fellow, 1937
American Medical Association, 1902
American Public Health Association, 1936
American Society for Clinical Investigation, Charter Member, 1908
President, 1915
American Society for Experimental Pathology, 1913
American Society for Pharmacology and Experimental Therapeutics, 1910
American Society of Tropical Medicine, 1909
Association of American Physicians, 1909
Vice-President, 1930
President, 1931
Kober Medalist, 1938
Charaka Club, 1924
President, 1939-1940
Harvey Society, 1911
Lecturer, 1913 and 1930
Vice-President, 1914-1917

President, 1921-1923
History of Science Society
Councilor, 1937-1940
Interurban Clinical Club
Vice-President, 1911-1912
President, 1921-1923
National Academy of Sciences, 1922
New York Academy of Medicine, 1909
Vice-President, 1920-1922
Academy Medal, 1953
New York Academy of Sciences
New York Clinical Society, 1917
New York Pathological Society, 1910
Practitioners Society, 1916
President, 1940
Society of American Bacteriologists, 1912
Society for Experimental Biology and Medicine
Society of Internal Medicine, 1910
President, 1912-1913

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Pneumococcus septicemia, meningitis and arthritis. Johns Hopkins Hosp. Bull., 13:143.

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The prevention of typhoid fever. J. Am. Med. Assoc., 42:1399.

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Typhoid meningitus. Johns Hopkins Hospital Reports, 12:379.

Exhibition of medical cases. Johns Hopkins Hosp. Bull., 15:258.

1905 Experimental streptococcus arthritis. Johns Hopkins Hosp. Bull., 16:114.

Case of typhoid fever showing some unusual features. Johns Hopkins Hosp. Bull., 16:118.

Pulsating emphysema. Johns Hopkins Hosp. Bull., 16:411.

Cystic kidney. Johns Hopkins Hosp. Bull., 16:411.

1906 Aetiology of acute articular rheumatism. N.Y. Med. J., 83:534.

1907 With J. C. Meakins. The treatment of gonorrhoeal arthritis by vaccines. Johns Hopkins Hosp. Bull., 18:223.

The opsonic theory and the treatment of infections by vaccines. Johns Hopkins Nurses' Alumnae Magazine, 6:57.

Note on the production of an agglutinating serum for blood platelets. Johns Hopkins Hosp. Bull., 18:261.

The vaccine treatment of infectious diseases. International Clinics, ser. 17, 2:1.

A summary of the study of opsonins carried out at the Johns Hopkins Hospital. Trans. Assoc. Am. Physicians, 22:533.

1908 With M. R. Smirnow. The pro-infective ("aggressive") action of normal blood-serum. Johns Hopkins Hosp. Bull., 19:249.

With A. B. Cecil. The axillary diastolic murmur in aortic insufficiency. Johns Hopkins Hosp. Bull., 19:353.

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1911 Typhoid fever. In: *A Handbook of Practical Treatment,* ed. J. H. Musser and A. O. J. Kelly, 2:176. Philadelphia: Saunders.

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1914 Pneumococcus infection and lobar pneumonia. Archives of Internal Medicine, 14:56. Also in Harvey Lectures, 9:85.

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Wallace Osgood Fenn

August 27, 1893-September 20, 1971

By Hermann Rahn

Wallace Osgood Fenn was born in Lanesboro (Berkshire County), Massachusetts and died in Rochester, New York in his seventy-ninth year, after a brief illness. He is survived by his widow, Clara Bryce (Comstock) Fenn; his children, William Wallace Fenn, Ruth (Fenn) Starman, Priscilla (Fenn) Roslansky, and David Bryce Fenn; and ten grandchildren. He led a most vigorous life and up to his very last days was working in the laboratory; during his last three years he shaped the new directions of the International Union of Physiological Sciences as its President. To many of his colleagues he was the Dean of Physiological Sciences, the last Renaissance Man, whose basic contributions covered so many areas and who had a remarkable perspective on the whole field of biology.

His forefathers settled in New England in the seventeenth century. William Wallace Fenn, his father, was a Unitarian minister who had married Faith Huntington Fisher, also from New England. Later his father became the Bussey Professor of Theology at Harvard and Dean of the Divinity School. Thus Wallace Fenn's childhood was spent in Cambridge, where he attended the Cambridge Latin School and entered Harvard with the goal of preparing himself for the ministry. However, when he started cutting his father's lectures to attend plant physiologist W. J. V. Osterhout's classes in biology, the foundations were laid for a career in physiology that was to span more than half a century.

Fenn graduated in 1914. His graduate work at Harvard with Osterhout was interrupted by World War I, during which he served in the Sanitary Corps of the U.S. Army and was commissioned a Second Lieutenant. Upon discharge in 1919, he finished his doctoral thesis in June, married Clara Bryce Comstock in September, and began his appointment as an Instructor in Applied Physiology in the Department of Industrial Hygiene at the Harvard Medical School under Cecil K. Drinker. Here began his classical studies of phagocytosis of solid particles by white blood corpuscles.

In 1922 he accepted a Rockefeller Travel Fellowship and was the first American to work in A. V. Hill's laboratory in London, England. This was followed by a six-month stay in H. H. Dale's laboratory at the National Institute for Medical Research in London. Returning to this country in 1924, he accepted the Chair of Physiology at the newly formed Medical School at the University of Rochester, New York. This position he filled for thirty-five years. In 1961 he was named Distinguished Professor of Physiology, a post he occupied until his death in 1971.

THE SCIENTIST

Fenn's first paper was published in 1916 in the *Proceedings of the National Academy of Sciences*. It was entitled "Salt Antagonism in Gelatin." His last paper, "Partial Pressure of Gases Dissolved at Great Depth," was published posthumously in *Science* in 1972. During the intervening half-century his 267 publications can be conveniently divided into four general areas: the physiology of muscle, electrolytes, respiration, and high pressure. In each area he laid foundations of new concepts, and when he was satisfied that he had made new basic contributions, moved on to explore new fields.

CONTRIBUTIONS TO MUSCLE AND ELECTROLYTE PHYSIOLOGY

The work that brought Fenn his first recognition was his study on the heat production of muscle, which he started in A. V. Hill's laboratory in 1922-1923. Fenn wrote: "In particular it can now be shown that there is a fairly good quantitative relation between the heat production of muscles and the work which they perform, and that a muscle which does work liberates, *ipso facto,* an extra supply of energy which does not appear in an isometric contraction."* It was A. V. Hill who referred to this as the Fenn Effect, and so it has been known ever since.

Fenn's heat data showed first of all that if a muscle shortens, no matter how little and no matter how lightly loaded, it produces more heat than during an isometric contraction over the same time period. He then showed that this extra heat production was proportional to the external work done by the muscle. It was clearly not determined by the load alone, nor by the change in length. This was the first evidence, and remains today the best evidence, that shortening is an active process and that muscle is not simply a prestretched spring shortening passively. The Fenn Effect has emerged as the nearest thing to a law that muscle physiologists have.

Following his pioneer work on muscle heat production, Fenn began to measure gas exchange by nerve and by muscle. To this end he had to invent a number of ingenious instruments to obtain the necessary specificity and precision. In 1927 he measured for the first time the quantitative amount of oxygen required by a nerve to conduct an impulse. Similar studies on the metabolism of contracting muscles led him to consider the role of electrolytes, particularly potassium, in nerve and muscle

* Wallace Osgood Fenn, "A Quantitative Comparison between the Energy Liberated and the Work Performed by the Isolated Sartorius Muscle of the Frog," *Journal of Physiology,* 58(1924):175.

activity. At the time, although it was known that muscle fibers were rich in potassium, almost nothing was known of the mechanisms by which cells accumulated and maintained a high potassium content.

The work ushered in the era of electrolyte physiology. Beginning in 1933 Fenn virtually created the field of potassium metabolism. He made the first determinations of potassium, sodium, magnesium, and calcium in nerve. He developed a new method for determining internal pH of muscle and nerve and obtained values that remain acceptable today. He showed that intracellular potassium was mobile, not fixed, and that muscle potassium shifted in response to various environmental factors.

Most importantly, he showed that during contraction potassium was lost from muscle in exchange for sodium, and that the process was reversed in recovery. For the first time he showed that sodium could penetrate muscle. These observations were clearly the necessary foundation for the Hodgkin-Huxley hypotheses concerning initiation and propagation of nerve and muscle impulses and the magnitude and polarity of electrical potential differences across cell membranes. As early as 1936, at the Cold Spring Harbor Symposium, Fenn said, "The explanation of a loss of potassium from a muscle during activity is a matter of fundamental theoretical importance. In terms of the theory which I have been using as a guide, it is interpreted as an increase in the permeability of the muscle membrane of sufficient extent to permit sodium, but not chloride, to enter. Every molecule of sodium which enters then displaces one molecule of potassium."*

Fenn showed that potassium escaped from muscle during contraction *in situ* and that a large part of this potassium appeared in the liver. He demonstrated that potassium uptake was

* Wallace Osgood Fenn, "Electrolytes in Muscle," *Cold Spring Harbor Symp. Quant. Biol.,* 4(1936):252-59.

linked with carbohydrate metabolism, particularly with glycogen deposition, and developed the concept that potassium tends to follow the Cori cycle. He was always quick to seize new opportunities. When radioactive potassium became available to him in 1939, he ingested a sample. Using himself as subject, he was thus the first not only to study the kinetics of potassium metabolism but also to demonstrate potassium incorporation into blood cells, previously thought to be impermeable. He showed that nearly all muscle potassium in the body is exchangeable, proving that high intracellular potassium content is not maintained by binding or sequestration of potassium, an idea which was consonant with his notion that potassium is maintained by an active energetic process.

CONTRIBUTIONS TO RESPIRATION PHYSIOLOGY

The entrance of Wallace O. Fenn into the history of respiratory physiology can be precisely dated. It was within days after the U.S. entry into World War II. At that time he was forty-eight years old and had established himself as the acknowledged leader in the physiology of muscle and electrolytes. He was to be recognized in 1943 by election to the National Academy of Sciences.

Wallace Fenn was drawn into respiratory physiology by his desire to contribute to the war effort. This was to be largely a war in the air, and from a military point of view, supremacy in altitude tolerance meant supremacy of air power. The airplanes of that day did not yet have pressurized cabins, but the possibility occurred that the human lung might be pressurized by application of positive pressure breathing. The question was whether man's lungs could tolerate a sufficient amount of pressure to raise the partial pressure of oxygen to a significant degree, or would the lungs rupture, or would the circulation stop? What were the limiting factors? What were the hazards?

What was known about respiratory physiology in general? This can best be answered by listing some terms which did not appear in the physiology textbooks of that era, but which are commonplace today. Such terms are: *positive and intermittent pressure breathing, pressure-volume diagram, work of breathing, pulmonary compliance, airway resistance, alveolar gas equation,* O_2-CO_2 diagram, ventilation-perfusion ratio, just to name a few.

Wallace Fenn had never worked in the field of human respiration. The equipment in his laboratory would be regarded as primitive by current standards. Among the more useful items were a few assorted spirometers, two or three Haldane machines, an equal number of Van Slykes, and several U-tube manometers. The most sophisticated instrument was a Millikan ear oximeter, which had been loaned to him by the Military. It carried a security classification of a fairly high level, and since no instruction manual came with it, it took some time and a visit to Glen Millikan himself before anyone could figure out how to use it properly.

In addition to this modest inventory of equipment, Fenn had three young instructors, all trained in biology departments. They knew all about such things as how fast the drosophila can beat its wings, how and why the rattlesnake changes color, and how to activate or inhibit enzymes found in grasshopper eggs, but none of them had ever blown a vital capacity; neither did they know the difference between complemental and supplemental air. L. E. Chadwick, A. B. Otis, and H. Rahn, living with their wives on postdoctoral stipends which were only a fraction of what a graduate student receives today, were the most unlikely crew to have been assembled for the unknown job that lay ahead of them.

Neither the equipment nor the staff was very impressive, and it seems doubtful that by present standards the project could have qualified for a National Institutes of Health grant. However, the major asset, recognizable even then, was Wallace Fenn himself. He was not put off by lack of ready-made equipment; he was well endowed with Yankee ingenuity, and he loved to improvise. He could, with whatever components happened to be handy, construct apparatus that would perform in a reliable and effective fashion. Everyone associated with him has memories of him in the laboratory surrounded by what at first sight appeared to be an unrelated jumble of strange wires and rubber bands, tubing, pulleys, lenses, light sources, mirrors, and other assorted bits and pieces. A more careful examination suggested there might be some order in the arrangement, and further observation would reveal that something of physiological interest was actually being measured and perhaps even graphically recorded. A relatively refined example was a device for the automatic recording of blood flow through the finger and its alteration by pressure breathing.

The high-altitude chamber was perhaps the crowning masterpiece of Fenn's ingenuity. He had received from the Committee on Medical Research of the Office of Scientific Research and Development a contract which provided the sum of $500 (five hundred!) for special research equipment. From this budget he bought a steel tank designed for the processing or transport of beer, commandeered the tree-spraying pump from the University Grounds Department, reversed its valves, and connected pump to tank. The result was a chamber which could go to simulated altitudes at the rate of 5,000 feet per minute. As he later said, "It surely was the worst high altitude chamber in the country, but a rare atmosphere is the same wherever you find it."*

Not only could he get the most out of primitive pieces of equipment, but he also seemed somehow able to evoke the best output from his staff. He did not tell people to do things. Rather, he pointed out things that needed doing and waited for some

* Wallace Osgood Fenn, "Born Fifty Years Too Soon," *Annual Review of Physiology,* 24(1962):1.

thing to happen. He worked hard himself and expected others to do likewise, but he recognized that there were individual differences in effective work patterns and did not try to impose his own habits on others. Although he kept rather regular working hours himself, he apparently was not perturbed by those with more erratic habits. Getting something done rather than compulsive adherence to a fixed schedule was the important thing.

In starting a new experiment he frequently took the lead by setting up apparatus himself rather than asking someone else to do it. Typically, he would insist on being the first subject in a new experimental procedure, and in experiments with pressure breathing and in the altitude chamber he extended himself on a number of occasions to the point of losing consciousness. He was a pioneer in every sense, and it was a blessing that his work antedated the Human Subjects Review Committee.

Fenn's intuitive approach to and logical analysis of the pressure breathing problem led him to develop two powerful concepts and to express them in the form of graphic relationships: the pressure-volume diagram of the lung and thorax, and the O_2-CO_2 diagram of the composition of alveolar gas.

Although the basic pressure-volume (P-V) diagram had been previously developed by F. Rohrer, Fenn conceived it independently, elaborated it further, and distilled into it some ten years of work and thought. Like all his work, it defined physiological boundaries, limiting values for muscle forces and the corresponding volumes of gas and blood. Within these limits were centered the normal operating range of pulmonary mechanics and the response of the system to positive and negative pressure breathing. It was not only a beautiful composition both artistically and scientifically, but it was also the foundation and framework of respiratory mechanics that would be further embellished by students during succeeding decades.

Fenn's second masterpiece, the O_2-CO_2 diagram, did for pulmonary gas exchange what the P-V diagram did for respiratory mechanics. With it he could represent all parameters of the alveolar gas and ventilation equations. He never claimed to have originated these equations, but he derived them independently, made sure they were correct, and put them in graphic form. As somebody put it, "That's when he made them sing." On the diagram he could show all possible compositions of alveolar gas and the arterial blood under any specified set of conditions. He could indicate normal ranges and limits of survival as well as the pathways followed during hyperventilation and asphyxia and during exposure to CO_2, altitude, or hyperbaric pressures. It could be used to demonstrate ranges of normal and impaired performance. It was indeed a theme that could be sung with many variations.

Although the P-V and O_2-CO_2 diagrams represent great masterpieces of Fenn's scientific artistry, he created, inspired, or contributed to many other works. To give a few examples: development of the concept of an optimal breathing frequency, measurement of alveolar pressure, dynamic pressure-volume curves presented for the first time on a cathode ray oscilloscope, development of an infrared CO_2 meter, and probably the first published continuous recording of CO_2 changes during a single breath.

Finally, one must mention two special contributions to respiration physiology, a lasting monument to his effort in this area: his book, *A Graphical Analysis of the Respiratory Gas Exchange,* which went through many reprintings, and his editorship of *Respiration* in the *Handbook of Physiology* series, both of these published by The American Physiological Society.

CONTRIBUTIONS TO THE PHYSIOLOGY OF SPACE AND OCEANS

From the mid-1950s Fenn became greatly intrigued with two new frontiers that began to unfold—man's explorations in space and the ocean depths. While his research continued in very basic experiments, their application was obviously directed to filling in the gaps of knowledge so that man could exist successfully in these new environments. He was in great demand as a consultant by space physiologists and tried to convince his more earthbound colleagues of the great new opportunities in physiology that unfolded as man ventured into space. Every problem Fenn "considered basic, if the investigator put some basic thinking into it." In that sense he felt that physiology as a science had gained immeasurably and would continue to grow as man went forth in orbit and into the oceans—thoughts that he expressed so well in his address "Physiology in Orbit."*

Wherever man went he needed oxygen as the life-sustaining gas, yet when it exceeded normal pressures it became poisonous. Fenn spent many years with his associates (R. Gerschman and D. L. Gilbert) in trying to understand the toxic nature of oxygen. Probably his most important insight was the recognition and demonstration that oxygen poisoning and X-irradiation effects have the same common mechanism.

He also turned his attention to the effects of high inert gas pressures upon the metabolism of unicellular organisms and the effects of hydraulic pressure on biological reactions. His last benchwork emphasized the importance of partial molar volume concepts as a tool for determining the volume that O_2 occupies within the hemoglobin structure.† His last research concerned itself with the theoretical concepts of partial pressures of gases dissolved at great depths. It was a thermodynamic interpretation published posthumously in *Science,*‡ where with his great modesty he asked for the help of physical chemists to develop this concept in greater detail, help which shortly appeared.§

* Wallace Osgood Fenn, "Physiology in Orbit," *The Physiologist,* 3(1960):20-26.

† Wallace Osgood Fenn, "Partial Molar Volumes of Oxygen and Carbon Monoxide in Blood," *Respiratory Physiology,* 13(1971):129-40.

‡ Wallace Osgood Fenn, "Partial Pressure of Gases Dissolved at Great Depth," *Science,* 176(1972):1011-12.

§ F. C. Andrews, "Gravitational Effects on Concentrations and Partial Pressures in Solutions: A Thermodynamic Analysis," *Science,* 178(1972):1199-1201.

SERVICE TO SOCIETIES

In spite of his active life as benchworker and teacher, few physiologists have given more of their time to professional societies. For twenty years Fenn served in many capacities in the American Physiological Society—as a member of the Board of Publication Trustees, Treasurer and Secretary, and its President (1946-1948). He later took upon himself the difficult task of writing the *History of The American Physiological Society: The Third Quarter Century, 1937-1962* (1963).

He served as President of the American Institute of Biological Sciences (1957-1959); as Chairman of the Advisory Council of the Life Insurance Medical Research Fund; as Chairman of the Physiology Study Section and the Physiology Training Grant Committee of the National Institutes of Health; and as a member on various boards of the National Institutes of Health, the National Academy of Sciences (Chairman, Section of Physiology, 1954-1957; Council 1966-1969), the National Research Council, and the National Science Foundation. But of all organizations, his greatest empathy was directed toward the International Union of Physiological Sciences (IUPS). This he felt was a vehicle that could bring under one roof physiologists from all over the world. For him physiology was the great encompassing science which could bring all men to a common outlook and worldwide understanding. He chaired the U.S.A. National Committee for the International Union of Physiological Sciences from 1946 to 1950, became Secretary General of IUPS (1959-1965), and edited the *History of the International Congresses of Physiological Sciences, 1889-1968.* His American colleagues elected him President of the XXIV International Congress of Physiological Sciences in Washington, D.C. in 1968, and on that occasion, which was his seventy-fifth birthday as well, he was elected President of IUPS for a three-year term, 1969-1971.

The XXV International Congress of Physiological Sciences in Munich, where Fenn was to officiate as President, was for him the crowning point of his lifelong devotion to physiology and a long sought-after personal reward to see thousands of physiologists from all countries assembled in peaceful discussion. Yet this goal eluded him. Shortly before the meeting he was suddenly taken ill and died in his home in Rochester, New York, on September 20, 1971.

A more intimate glimpse of Wallace Fenn's philosophy and attitude about the role of physiology in the world of science can be found in: "Physiology on Horseback," *American Journal of Physiology,* 159(1949):551-555; "Physiology in Orbit," *The Physiologist,* 3(1960):20-26; and "Born Fifty Years Too Soon," *American Review of Physiology,* 24(1962): 1-10.

THE MAN AND TEACHER

Reviewing his contributions as a leader and a scientist, one might well imagine a man who unconsciously dominated the scene on the public forum or in his own laboratory. Wallace Fenn was quite the opposite. By nature he was shy, a man who did not seek out his fellow man with a great hello. He kept his own counsel, and for many he was not easily approachable. Once such barriers were broken, he was a most friendly, completely unpretentious person who would listen to your story politely but only respond when necessary. In public forums and committee meetings his patience with conflicting points of view was on occasion wondrous to behold. While others fumed and fussed, he would sit in silence, but when he finally spoke, it usually ended the debate. He was a master in the art of compromise without compromising principles.

Many of his friends and colleagues remember the joyous weekends at the Fenns' summer cottage on Canandaigua Lake. There was always a cooperative program of felling trees, getting boats out, cutting wood, raking the beach, rebuilding the pier, projects in which everyone happily joined. His New England heri tage enforced his belief that work came first and then play, and it required real effort for him to sit with idle hands when there was a job to be done or a boat to sail. He had an amazing physical endurance and never took an elevator to his laboratory on the fourth floor. I remember his suggesting after a cafeteria lunch that we run up the four flights while holding our breath. He loved all types of physical and intellectual competition.

He had the knack of getting others to work with him without ever issuing an order. He simply expected people to come forward to help in a common cause, whether it was in teaching, research, or committee meetings. If they failed to do so, he would do the job himself without reprimand. He also had an amazing sense of timing; when a tough decision had to be made, he would wait, always just long enough so that either the problem evaporated or the involved person had solved it for himself, and feelings were never hurt.

Wallace Fenn had a soft spot in his heart for the proverbial underdog. Few people realize how much aid and comfort, protection, and encouragement he gave to those who had tough luck or seemed to have failed in their professional accomplishments. Nothing gave him more pleasure than to see someone fight his way up through temporary odds, real or imagined. All he asked from them was that they show pluck, patience, and perseverance. (Parenthetically, Fenn loved alliteration.)

Teaching he considered a most serious assignment. He was never satisfied with last year's lectures, and year after year tried new and better ways of getting a difficult point across. It was this quality of continuous striving that endeared him to his students and colleagues in lectures and seminars. His lectures to the students were exemplary in their clarity, and yet he was always able to instill the sense of wonder and the new challenges that lay ahead for a better understanding.

Wallace Fenn considered it a great privilege to work as an academician, but felt that this special privilege demanded an utter devotion and enthusiasm, as well as giving help to other colleagues, to his professional societies, and to his government when so called upon. For him continuous striving, striving to obtain his scientific goals, was more important than arriving. He was most embarrassed by the many honors that were bestowed upon him. For him these were not signals that he had arrived. A remarkable inner drive prompted him to continue his strivings, and during his last days, propped up in bed, knowing that the end was at hand, he was slavishly working on his final theoretical manuscript, mainly concerned that he had derived his equations correctly.

Spanning a most productive scientific career of more than half a century, many honors and recognitions (listed below) were bestowed upon him. These were given partly in recognition of the many accomplishments of a scientist whose vision and work had spanned so many areas in physiology and in part to honor a man who had given so much of himself that physiology as an encompassing science had become a greater science in the world.

A most unusual award was the dedication of the "Respiration Suite" to Wallace Fenn. It was composed by Jurriaan Andriessen and performed by the Dutch Wind Ensemble at Alphen, Holland, in the presence of several hundred physiologists and our Ambassador, to honor the man who had contributed so much to the physiology of the lung, which is also a device for the creating of harmonious sounds.* In 1968 a Jubilee Issue in his honor, on the occasion of his seventy-fifth birthday, was published in volume 5 of the journal *Respiration Physiology.*

Wallace Fenn will not be forgotten. During his unusually long and productive career he influenced in his quiet and selfless way many friends, colleagues, and students, both at home and abroad. He will always be admired and remembered as a great pioneer in physiology. He was always one step ahead of most,

* "Respiration Suite," *The Physiologist,* 6(1963):47-48.

and when he plowed a virgin field his furrow was straight and deep so that followers would not lose their way.

I am greatly indebted to Mrs. W. O. Fenn, Miss Augusta Dustan, Drs. Arthur Otis, Albert Craig, Pierre Dejours, Harry D. Bouman, Paul Horowicz, and Loren J. Mullins, and many others for sharing their remembrances.

AWARDS, HONORS, AND DISTINCTIONS

Chronology

1893 Born August 27, Lanesboro, Massachusetts
1910 Graduated from Cambridge Latin School, Cambridge, Mass.
1914 A.B., Harvard University
1916 A.M., Harvard University
1917-1918 Second Lieutenant, U.S. Army, Sanitary Corps
1919 Ph.D., Harvard University (Plant Physiology)
1919-1922 Instructor in Applied Physiology, Harvard Medical School
1922-1924 Traveling Fellow, Rockefeller Institute, A. V. Hill's Laboratory and H. H. Dale's Laboratory, London
1924-1959 Professor and Chairman of Physiology, The University of Rochester School of Medicine and Dentistry
1962-1966 Director, Space Science Center, The University of Rochester
1961-1971 Distinguished Professor of Physiology
1971 Died September 20, Rochester, New York

Memberships (Selected)

National Academy of Sciences (elected 1943)
American Philosophical Society (elected 1946)
American Academy of Arts and Sciences (elected 1948)
The American Physiological Society (President, 1946-1948)
American Institute of Biological Sciences (President, 1957-1958)
Society for Experimental Biology and Medicine (President, 1957-1959)
International Union of Physiological Sciences (President, 1968-1971)
International Academy of Astronautics
Undersea Medical Society
New York Academy of Sciences (Fellow)
Rochester Academy of Science (Fellow)

Honorary Memberships

1928 Harvey Society
1929 Alpha Omega Alpha
1951 Sociedad Argentina de Biologia

1961 Honorary Life Fellow, Rochester Academy of Medicine
1963 Italian Society of Experimental Biology
1965 Canadian Physiological Society
1965 Physiological Society of Great Britain
1965 Foreign Member, Accademia Nazionale dei Lincei (Rome)
1970 Academie Royale de Medecine de Belgique
1971 Rochester Museum and Science Center

Awards

1949 John F. Lewis Prize, American Philosophical Society

1958 Gold Medal Award, University of Rochester Medical Alumni Association

1961 Certificate of Merit, Rochester Academy of Medicine

1964 Daniel and Florence Guggenheim Award, International Academy of Astronautics

1964 Antonio Feltrinelli International Prize for Experimental Medicine, Accademia Nazionale dei Lincei, Rome

1966 Modern Medicine Award for Distinguished Achievement, Board of Editors, *Modern Medicine*

1967 Research Achievement Award, American Heart Association

1971 Johannes Müller Medallion, The German Physiological Society

1971 Ville de Monaco Medal

Honorary Degrees

1950 University of Chicago, D.Sc.
1959 Universidad San Marcos, Peru, Catedratico, Honorario
1960 Université de Paris, Docteur Honoris Causa
1965 Université Libre de Bruxelles, Docteur (Hon.)
1965 The University of Rochester, D.Sc. (Hon.)

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Paul Darwin Foote

March 27, 1888-August 2, 1971

By Allen V. Astin

Paul Darwin Foote was a man of diversified interest and talent who left a major imprint on many areas of science and technology. During his college days he started out to be an electrical engineer, was once tempted to become a lawyer and another time to be a classicist, but ended up with a major in physics. About 18 years before his death he gave the Home Secretary of the National Academy of Sciences the following account of his origins and youth.

> I was born March 27, 1888, in Andover, Ashtabula County, Ohio, and was one of the first children in the county to be issued an official birth certificate. My father at various times was a city and county superintendent of public schools in Andover, Madison, Chardon, and Jefferson, Ohio. Upon his retirement from teaching at 65, he became general agent of several northeastern counties for an insurance company and was active up to his death in 1939 at the age of 87. My mother was Abbie Lottie Tourgee, who died at age 56 in 1920 from septicemia following a tooth extraction, this occurring long before medical knowledge of antibiotics. Both parents were for many generations of American extraction but originally Father's antecedents came from England and Mother's from France. Mother was talented in literary matters and was always active in women's clubs. I had one brother, Ralph L. Foote, who was killed in an automobile accident in 1946. Our home life was most pleasant, one of our diversions being horseback riding and carriage driving. As a boy I always had my horse as well as a bicycle, and enjoyed many excursions with the children of other families. However, my home life terminated in 1905 at the age

of 17, for thereafter I was able to return infrequently for only a few days.

I attended Chardon public schools from 1893 to 1905, being graduated as valedictorian of my class. Avocations were chess, fishing, and music. I played the clarinet in the school orchestra and village band from age 12; later with college orchestras and bands, and during the early Twenties with the Washington D.C. symphony orchestra.

As a youngster from 12 to 17, I was general agent for three Ohio counties for the sale of aluminum combs. Few people in the late Nineties had ever seen this metal, and the combs—priced at 10 to 80 cents—sold readily by mail and house canvass. It was a lucrative business for a boy and permitted me to indulge in many extravagances. For example, a classmate and I living about a mile apart had the first two-way radio telegraph setup in Ohio, around 1900, when silver coherers were required as detectors. We also had x-ray equipment and fairly good basement laboratories for electrical experiments and photography. From age 12 to 15 I had won several prizes for photography in such national magazines as *Success* (that eventually failed to substantiate its name). As I was the only amateur photographer in the village, my services in this work were often used to advantage by real estate agents and property owners. My first experience in hydraulics occurred at the age of 14 in the high school physics laboratory where I had permission to work on week ends. I connected a water turbine to the compressed air line, and just had time to drop under the table when the blades and case embedded themselves in the walls and ceiling.

Although I could have received financial help from my parents, I worked my way through college. While attending Adelbert College, Western Reserve University, 1905-1909, I was employed part time by a Cleveland law firm and taught city night school, chiefly algebra. Life with the law firm was most exciting and several of the young lawyers, who later became judges, encouraged me during my three years' employment, to enter this profession. The firm was the official collector of bad accounts for the associated merchants and physicians of Cleveland. My first introduction to the petroleum industry occurred when, acting as a deputy constable, I served notice on a president of an oil company to appear in court in answer to a suit for an unpaid bill. He was a giant, and he attacked me with his fists, doing considerable damage. He was heavily fined in police court and later I filed suit for personal damages. When at the hearing it was discovered that I was only 19, our attorneys immediately withdrew the case as it might have had complications in my service as deputy constable. Collection of bad accounts and garnisheeing of wages in those early days was dirty and usually thrilling business for those who understood the procedures, but it enabled me to become personally acquainted with nearly every physician and grocer in Cleveland.

I was always interested in mathematics and physics, and planned to take the five-year course in electrical engineering in combination with the Case School of Applied Science. Shop credits were secured by summer class work at Case. However, after being graduated from Western University magna cum laude and Phi Beta Kappa, I accepted an offer to become a laboratory assistant in physics at the University of Nebraska and thereafter was trained in physics and mathematics rather than electrical engineering.

At Western Reserve, Professor Emerson, head of the English Department, tried to influence me toward English as a career. I enjoyed his private tutoring and even won the Early English Text Society Prize, a complete edition of English pre-Chaucer, a quite valuable set of books later donated to the University of Pittsburgh. However, the turning point in my career was undoubtedly due to Professor Whitman, head of the Department of Physics. In advanced studies at Reserve in both mathematics and physics, I was often the sole member of the class so that my training in general was by practical tutoring. Professor Smith tutored me in quaternions during my sophomore and junior years, and Professor Whitman and later Professor Montcastle in various subjects in physics. Professor Whitman believed in learning by experience. This was often costly in time but thoroughly rewarding. Once I constructed, in the machine shop, a rather complicated apparatus of brass assembled by soldering. For final cleaning I asked him if boiling in oil would be satisfactory. He suggested trying it, and I thereby learned to his amusement that the boiling point of oil was above the melting point of solder.

At Nebraska I spent two years, 1909-1911, with Professor C. A. Skinner using apparatus designed by Professor Brace. My thesis, published in *Physical Review* in 1912, contained data on the magnetic rotation of the plane of polarization and ellipticity of plane polarized light reflected from mirrors in a magnetic field. These data, including effect of strength of field and dispersion through the spectrum, still stand in the modern physical tables. While at Nebraska I continued studies with the Department of Mathematics and studied theoretical physics in quaternion notation under Professor L. B. Tuckerman. In 1911 I took the civil service examination for assistant physicist at the Bureau of Standards and received a mere passing grade. I knew that I had answered all the questions correctly in quaternion notation. Years later the person who marked my papers informed me that although he did not understand any of my mathematics, since I had failed to define the notation, I arrived at the correct answers and he marked my papers "Pass". Even this low grade was sufficient for appointment.*

* Paul D. Foote: Autobiographical Statement for the National Academy of Sciences, 1953, pp. 1-5.

Paul Foote's career at the National Bureau of Standards consisted of two separate periods: 1911-1916 and 1917-1927. During the first period he rose rapidly from a laboratory assistant to the Chief of the Pyrometry Section, in which position he carried on pioneering work in high temperature measurements and played a major role in the development of the pyrometer and automatic heat control industries, then in their infancy. Noteworthy among many publications in this period was a section on "Thermometry, Pyrometry, and Heat Conductivity" for McGraw-Hill's *Standard Handbook for Electrical Engineers* (1916). The pyrometry organization that Foote developed at the Bureau during this period has continued substantially unchanged for many years and achieved international recognition.

In 1916 Foote resigned from the Bureau of Standards to accept the position of assistant manager of the Fisher Scientific Company in Pittsburgh, at that time a small firm engaged in the manufacture of instruments for military use, especially telescopic gun sights. He shared in the invention of the F & F optical pyrometer and other temperature measuring equipment. The production of military instruments, as well as that of pyrometer and metallurgical equipment, had just become a successful manufacturing operation when the University of Minnesota asked that Foote be permitted to spend seven months at the University to deliver lectures and to establish a section on pyrometry in the Physics Department. The Fisher Scientific Company agreed to the arrangement and retained him as assistant manager, to handle business transactions by correspondence, during the period he spent at the University.

From the time he had joined the Bureau in 1911, Foote had taken graduate academic work in Washington, D.C., receiving credit for this work from various schools. This, together with courses taken at Minnesota, and a thesis on pyrheliometry, reporting work done jointly with the U.S. Weather Bureau, was accepted by the University of Minnesota in fulfillment of the requirements for the degree of Doctor of Philosophy in physics which was awarded in 1917.

The United States had entered World War I by the time Foote returned to the Fisher Scientific Company, and shortly thereafter, at the request of the Government, he was released for technical duty at the Bureau of Standards. At first he was engaged in various military technical projects, probably the most important of which was the organization and direction of the development of heat control processes for the manufacture of high-grade optical glass. This early work, along with the cooperative efforts of a large group of industries, private laboratories (including the Carnegie Geophysical Laboratory in Washington, D.C.), and government agencies, led not only to the successful manufacture of a fair grade of optical glass for use in World War I, but provided a cornerstone for the highly developed American optical glass industry as we know it today.

While he was at the University of Nebraska Foote began an important and long-lasting friendship with John T. Tate, who received his master's degree there in 1912. This friendship was renewed at the University of Minnesota, where Tate had come after receiving his doctorate under James Franck in Germany. Foote said: "Tate was one of my teachers, in fact all of the younger staff took courses under each other. Tate taught me statistical mechanics and the group, including Arthur Compton, Tate, McKeehan and Klopsteg were in my class on radiation theory."* Tate also came to the Bureau of Standards on temporary wartime assignment in 1917 to work in the Pyrometry section with Foote. Although both were primarily concerned with wartime problems, Tate, influenced probably by exciting new work he had observed in Göttingen in the spectral analysis of mercury and other metal vapors, interested Foote in beginning a study of atomic processes.

* Quoted in: "John Torrence Tate," *Biographical Memoirs,* 47:464. Wash., D.C.: National Academy of Sciences, 1975.

Following the war, Foote and one of his assistants, Fred L. Mohler, turned their attention increasingly to this then new field of atomic research. Although this field of work was beyond the scope of their organizational responsibilities for heat and pyrometry work, Bureau Director Stratton allowed Foote and Mohler considerable freedom (but without extra funds) to pursue their spectral studies of atomic processes.

Their work, which was described in one of the Bureau's annual reports as "Investigations in Electronics," included studies of the excitation and ionization potentials of simple molecules and the photo-ionization of alkali vapors. These findings provided important experimental support for the quantum theory of spectra. An important monument to their work was the well-known reference book *Origin of Spectra,* first published in 1922. About the time the book came out, the Bureau Director decided to give their work organizational recognition in its own right and he established in the Optics Division a Section on Radium, X-Rays, and Atomic Structure, with Foote as its chief.

Foote's interests were very broad, and conscious of the emerging importance of X-rays and radium, he dedicated some of his attention to the health hazards of radiation. Through contacts established with most of the leading hospitals and roentgenologists in the United States, the new Section initiated the standardization of X-ray dosages for therapeutic treatments and also set up standards for hospital X-ray installations and for the protection of operators and patients. The procedures developed were the foundation for modern X-ray practice. In 1926, at the request of Secretary of Commerce Herbert Hoover, Foote undertook a special mission to Europe to report on engineering and medical developments in X-ray and radioactivity. At the time practically all of the radium in America was measured and certified under Foote's direction, and it was he who presented a second United States gift of radium to his friend Madame Curie.

By 1927 more than seventy publications had been produced by Foote's new Section, and ever alert to new challenges, he began to consider leaving government service. He decided to accept the position of Senior Industrial Fellow on a new Fellowship on oil production technology established for its Production Department by the Gulf Oil Corporation at the Mellon Institute of Industrial Research in Pittsburgh, and he resigned from the Bureau in 1927. At this time practically no physicists were employed in the petroleum industry, since most of the technical work was conducted by chemists in the field of production, refining, and product development. Foote's immediate problem was the initiation of research on the application of physics to the discovery of oil fields and to the production of crude oil from these fields. His experience up until this time had been for the most part in academic type physics and he knew nothing about the petroleum industry and its problems. He spent the first two years studying the industry, traveling in the oil fields, and establishing in his own mind the problems presented in petroleum exploration and production.

In a short time this Gulf Fellowship on production had overflowed its limited space at Mellon Institute, and additional quarters were rented in office buildings to house a rapidly growing Geophysical Division under Dr. E. A. Eckhardt, who joined Foote in 1928. Eckhardt, also a former Bureau of Standards physicist, had already been engaged in geophysical work for another oil company. From that time on, the work expanded by leaps and bounds, and by the beginning of 1930 a large part of the Fellowship was transferred to the Gulf Production Company as a new Research Department occupying a new eighty-room laboratory building. The research staff numbered approximately ninety, and the work had expanded into all phases of oil field technology. Research activities continued to expand, and before long the Company was again renting space in other buildings in the area. The Research Department of the Gulf Production

Company became in 1933 the Gulf Research & Development Corporation, a full-fledged subsidiary of Gulf Oil Corporation. Foote was named Director of Research and Executive Vice President of this Company and was elected to its Board of Directors.

By 1934 the various groups associated with the Research Laboratory had become so spread out geographically that it was decided again to bring them all together. To this end a tract of forty-seven acres was leased near Harmarville, Pennsylvania, sixteen miles northeast of downtown Pittsburgh, and three main laboratory buildings with a few small auxiliary buildings were erected there. The new laboratory was occupied in April 1935. By this time the Company had broadened the scope of its activities to include major research projects in refining, manufacturing, and sales and had become the centralized research organization for the Gulf Oil Corporation. Shortly after its inauguration the Harmarville staff comprised approximately 250 employees, with another 250 in the field engaged in geophysical operations. In 1936 the name of the Company was changed to the Gulf Research & Development Company, its present name. In 1945, Dr. Foote was made a vice-president of the Gulf Oil Corporation and the Gulf Refining Company. Under Foote's leadership the Gulf Research & Development Company became one of the most complete, integrated petroleum laboratories in the world.

Paul Foote retired from the Gulf Research & Development Company at the end of 1953, having reached the usual industrial retirement age of sixty-five. For the next four years he engaged in a variety of consulting activities. One of these was a temporary part-time position on the staff of the National Academy of Sciences as the coordinator of the Academy's advisory services to the Office of Ordnance Research. During this period he moved his residence from Pittsburgh to Washington.

In September 1957 President Eisenhower appointed him to the post of Assistant Secretary of Defense for Research and Engineering. Foote became very much involved in trying to improve the military research and development program and in increasing the effectiveness of the advisory Defense Science Board. However, before he had time to bring about many significant changes he was forced to retire (in October 1958) because it was discovered that he was past the age of seventy and that he had had fifteen years prior government service at the National Bureau of Standards (NBS). Foote was amused over the fact that Civil Service regulations made a roadblock of his NBS years to his desire to stay through the balance of the Eisenhower Administration. At the time of his second retirement Dr. Foote was awarded the Defense Medal for Meritorious Civilian Service for outstanding contributions to the National Defense.

Paul Foote did not long remain idle. In 1960 he was persuaded to take a part-time position with the National Academy of Sciences to supervise the Academy's advisory services to the National Bureau of Standards. The assignment involved the staffing, scheduling, and coordinating of about sixteen discipline oriented committees, corresponding to the primary organizational units of the Bureau. Foote's wide contacts with the scientific and engineering community, coupled with his great interest in the Bureau, enabled him to organize highly competent committees to work effectively with the Bureau during a period of rapid growth associated with the post-Sputnik atmosphere. The Bureau valued most highly Paul Foote's efforts. Failing health led to Dr. Foote's resignation from this activity at the end of 1965.

Throughout the remainder of his life Foote's primary professional interest was the American Philosophical Society, where he remained active on the Society's Research Committee and was also a regular attendee, with his wife Miriam, of the Society's meetings. He was elected to the Society at the relatively young age of thirty-nine (1927) and served at various times as Councilor, Secretary, and a member of the Class I Membership Committee. He died at his Washington home on August 2, 1971.

Paul Foote was very active in professional society activities throughout his career, and he received many honors. He was president of the American Physical Society in 1933, secretary of the Optical Society of America in 1920, and vice-president of the Washington Academy of Sciences in 1936. He founded the *Review of Scientific Instruments* and was its editor for ten years. He was editor of the *Journal of the Optical Society of America* for a similar period and an associate editor of the *Journal of the Franklin Institute.* He was chairman of the small group of physicists that organized the American Institute of Physics in 1931.

He was elected to the National Academy of Sciences in 1943. Other honors and activities include: The Outstanding Achievement Gold Medal from the University of Minnesota, 1951; honorary degree of Doctor of Science from the Carnegie Institute of Technology, 1953; the Pittsburgh Man-of-the-Year in Science Award from the Pittsburgh Junior Chamber of Commerce, 1953; the Pittsburgh Award for outstanding service to chemistry from the American Chemical Society, 1954; the honorary degree of Doctor of Science from Western Reserve University, 1961. During World War II, he served as a consultant to the Office of Scientific Research and Development and to the Research and Development Board, and he was a member of the Executive Committee for Antisubmarine Warfare. He was also a member of the Industrial Advisory Group of the Atomic Energy Commission, the National Science Foundation Advisory Committee for Minerals Research, the Army Ordnance Advisory Committee, and the National Advisory Committee for Astronautics.

An insight into Paul Foote's broader interests and into his light good humor can be gleaned from some of his writings. About 1920 he published anonymously in the Taylor Instrument Company house organ a paper on "The Temperature of Heaven and Hell." By making scientific deductions from de scriptions of the states of various material substances as described in the Bible, Foote concluded that Heaven was hotter than Hell. The paper, or portions of it, are periodically reprinted—for example, in the journal *Applied Optics* (1972)—but all have attributed the paper to an anonymous source. Nevertheless, a copy of the original manuscript with Paul Foote's notations identifying himself as the author was found in his personal file after his death.

A second example is provided in his Presidential Address to the members of the American Physical Society in December 1933. In discussing the importance of science to American industry, he was critical of those who claim that scientists are motivated mainly by an altruistic search for truth and are not concerned with recognition or material reward. He protested the belief

> . . . that the true scientist is motivated solely by his spirit of curiosity, by his thirst for knowledge, and for the discovery of truth for truth's sake alone. For the benefit of the many academic scientists who believe this fiction I propose the following practical experiment. Let articles submitted to the *Physical Review* be carefully read by the editorial board and secure the sponsorship of the American Physical Society. Every approved paper will then be published anonymously with no possibility of determining the authorship or the institution from which the research emanated. Certainly nothing is lost to science in the anonymous publication of work sponsored by a competent editorial board. All the truth is there as before. If such a policy were adopted I believe we would have no publication problem on our hands, but assuming a few papers are received it would be interesting to observe the American Institute of Physics in its attempt to collect three dollars per page from each authors' institution.*

The final example is a serious-funny letter to the Editor of *Science* in 1964 on the subject "Noise." In the letter Foote laments the rapid increase in noise associated with modern living, chastises engineers and architects on their priorities, and con

* *Review of Scientific Instruments,* 5 (February 1934): 57.

cludes: "Eventually the problem will be solved. However, by the time that the building industry and architects are educated to the requirements, most of us will be immune to noise, buried under six feet of sod."*

Paul Foote had a full personal life, enjoying immensely his family, his music, his cabin cruiser, his automobile, and his clubs and professional societies.

He was first married, in February 1913, to Bernice Claire Foote, a cousin, and they had two children, William Spencer and Charlotte Jane (Mrs. John M. Hallewell). Each of his children presented him with three grandchildren. Bernice died in 1939, and about a year later Paul Foote married Sophie Miriam Shanks Sage, a widow and daughter of Robert Lewis Shanks of Greenwich, New York, a jeweler and watchmaker. From this marriage Foote acquired two stepsons, Robert L. and Evan T. Sage, and additional grandchildren.

Paul and Miriam spent many weekends and occasional long trips on the water in their well-equipped cabin cruiser, first acquired in Pittsburgh and later moved overland to Washington for anchorage in the Potomac. This activity led to an interest in navigation which Foote studied with his characteristic intensity.

During Foote's second period at NBS he helped organize among fellow scientists a chamber music group for which his excellence on the clarinet was a real asset.

Paul Foote must be classified as a man whose impact covered many fields: basic science, especially the early growth of quantum physics; scientific instrumentation, especially thermal and photoelectric measurement; the development of the petroleum industry; the effective use of science by industry, national defense and other governmental agencies, and the more effective operation of professional societies, especially to deal with their publication problems.

* *Science,* 143 (January 10, 1964): 101.

At the time of Foote's death Frederick Seitz wrote:

> With the death of Paul D. Foote on 2 August at the age of 83 U.S. physicists have lost one of their most important and creative links with the early history of the profession . . .. Few individuals in our time have been as dedicated as he was to the creation of relationships within the community of American physicists that would advance the role physics could play in our society. Our times call for more of the spirit that motivated Foote to explore new roles for the members of the physics profession.*

* *Physics Today,* 24 (November 1971): 73.

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William Zev Hassid

October 1, 1899-April 28, 1974

By Clinton Ballou and Horace A. Barker

Zev (Ze'ev) Hassid was born in Jaffa, Palestine, probably on October 1, 1899, although he seemed uncertain about the date of his birth and sometimes gave the year as 1897 or 1901. The name William was added after he came to the United States. His parents, Mordecai and Esperanza Hassid (Chassid), were born in Poland, but were Russian citizens at the time of his birth. His father was a lumber merchant who brought lumber from Russia to Palestine. When Zev was four years old, the family moved to a farm in the vicinity of Kremenetz in the Russian Ukraine, and his childhood was spent in this rural environment. He often wandered in the adjacent woods and fields, hunted for birds' nests, and associated with shepherds, so much so that his father scolded him and said that if he did not spend more time on his studies he would qualify for nothing more than a herdsman.

Russian and Yiddish were Zev's native languages. Little is known about his early education except that he was required to study traditional Jewish religious material. He did not accept this instruction readily and in later life rebelled by dissociating himself from most formal religious activities. Nevertheless, his early training was evidenced by a familiarity with and an occasional quotation from scriptures.

In 1912, Hassid was sent back to Palestine with a group of

Jewish children to continue his education in a Hebrew language school. His parents hoped that he would be admitted to the "Gymnasia Hertzlia" in Tel Aviv, but instead he was sent to the recently founded Agricultural High School in the Jewish settlement of Petah-Tikva. The curriculum included Hebrew, French, and Arabic languages, Hebrew religious studies, history, geography, and considerable science, plus professional studies in soil, plant nutrition, subtropical horticulture, animal husbandry, laboratory practice in analytical methods, and field experience in agricultural techniques. He graduated in 1916.

With the outbreak of World War I, Hassid was cut off from communication with his parents, who had remained in Russia. Consequently, his only income was what he could earn himself. While still in school and after graduation, he worked as a laborer in the orange groves and other farms near Petah-Tikva, Nikva-Israel, and Ben Shemen. His income was small, his food and clothing correspondingly poor. For a considerable period he lived on bread, watery soup, pumpkin porridge, and oranges; he was often hungry. His clothing was worn and ragged. Once his shoes were stolen, and he had to go barefoot for a considerable time. During this period Hassid was frequently ill. He suffered repeated attacks of malaria and dysentery; he also contracted typhoid fever, which he barely survived.

Palestine was controlled by the Turks until the British army invaded the country in 1917. Following the Balfour Declaration that same year, many young Jews joined the British army to help liberate the country from the Turks and create conditions favorable for the establishment of a Jewish homeland. Hassid volunteered for army service early in 1918, partly for patriotic reasons and partly to improve his standard of living. Initially, he was rejected by the army because of his poor health, but finally, through the intervention of James de Rothschild, an officer in the British army, he was accepted into the 38th brigade of the Royal Fusiliers (1st Judeans), later referred to as the Jewish Legion. At this time he became a British citizen and served for two years in the army, mainly as a clerk at supply depots and at General Headquarters, 3rd Echelon. He never engaged in combat; however, on at least one occasion he was close enough to the front to be within artillery range, and a large shell landed close to him but failed to explode. He also guarded prisoners and supplies in transit. The latter duty required him to travel as far as Beyreuth in Lebanon and Alexandria in Egypt. At the railway station in Beyreuth he once witnessed the ceremonial arrival of Lawrence of Arabia. In Alexandria he first heard of the University of California from a fellow soldier, Assaf Gur (Grazovsky), who had studied there.

When Hassid completed his military service with the rank of corporal in August 1920, he was awarded the British War Medal and the Victory Medal, bearing the inscription, "The great war for civilization." His superior officer provided him with the following recommendation: "Corporal Hassid has been employed as a clerk in the Battalion Orderly Room and at Records, 3rd Echelon. He is a conscientious, painstaking worker, reliable and capable, and as a soldier has borne an exemplary character."

After leaving the army, Hassid decided to use the accumulated savings from his pay to go to California to study agronomy at the University, with the intention of returning ultimately to Palestine to assist in the development of scientific agriculture. He traveled by way of Paris, New York, and Chicago, staying briefly in each city, and finally arrived in Berkeley in late 1920. His funds were almost exhausted, so he supported himself by doing odd jobs in stores and restaurants in Berkeley in the winter and by working as a farm hand in the vicinity of Fresno in the San Joaquin Valley in the spring and summer. He also taught Hebrew in the local synagogue.

Hassid first registered at the University of California in August 1921. However, his knowledge of English was so limited that he could not follow lectures well enough to take notes, and even reading textbooks required more time than he could spare between jobs. So after a week of frustration and mounting tension, he took a leave of absence from the University and moved to Fresno where he spent two years as a student in Fresno State College, majoring in Letters and Science with an emphasis on Chemistry, French language, and Mathematics. The following year he enrolled at the Southern Branch of the University of California at Los Angeles. His course grades were about average; he even failed one course in quantitative analysis and had to repeat it later. His undistinguished record during this period probably resulted from his inadequate command of English and the fact that he had to earn a living while attending school.

In August 1924, Hassid returned to the Berkeley campus of the University. He majored first in chemistry but later changed to general literature, the field in which he obtained a Bachelor of Arts degree in December 1925. Following graduation, he immediately started graduate studies in the School of Education, and in December 1926 he received a Certificate of Completion with majors in chemistry and general literature and minors in mathematics and physics. In the same month Hassid obtained a General Secondary School Credential from the State Board of Education, but he never taught in public schools. Instead, he worked for some months as a chemical analyst in a commercial company.

By September 1927, and possibly earlier, Hassid took a position as research assistant under Professor D. R. Hoagland in the Division of Plant Nutrition of the Agricultural Experiment Station. His main duties were the routine analysis of plant materials and soils for a variety of inorganic constituents, but he also obtained experience in growing plants in culture solution and studying the absorption of various nutrients. This work renewed Hassid's interest in plant research, and in August 1928 he again enrolled in the University, this time as a graduate stu dent in Plant Nutrition. During the following two years, while still working half time or more, he took courses in botany, plant physiology, and plant nutrition and prepared a Master of Science thesis dealing with the structure of the four isomers of penta-*O*-acetyl-D-galactose. Professor Hoagland formally supervised his thesis research, but the nature of the problem clearly indicates that it was inspired and guided mainly by Professor Walter H. Dore, who was interested in the structure of carbohydrates and had applied X-ray diffraction methods to their elucidation. After receiving the master's degree in August 1930, Hassid started to prepare for a doctorate in Plant Physiology. While visiting the beaches south of San Francisco, he observed the abundant fleshy marine algae, which appeared to consist largely of polysaccharides, and he decided to investigate the structure of the major component. This was the subject of his Ph.D. thesis, which was completed and accepted in December 1934, with Professor Dore as the chairman of the committee. Hassid had worked almost independently, however, using the methods developed by Haworth for carbohydrate structure determination, which were unfamiliar to Dore. Professor T. D. Stewart of the chemistry department, a member of the thesis committee, was particularly impressed by the clear results and logical presentation of Hassid's thesis, and his enthusiastic reaction helped Hassid to obtain an appointment the following year as a Junior Chemist in the Division of Plant Nutrition of the Agricultural Experiment Station.

The circumstances of Hassid's appointment as a Junior Chemist are amusing and illustrative of the method of making appointments in 1935. Professor Hoagland lectured and taught laboratory courses in plant biochemistry during the Fall term. Hassid had served for several years as a teaching assistant in the laboratory course, and Hoagland, who directed an active research program and served as Chairman of the Division, had come to depend on him for the preparation of reagents, setting up of equipment, and much of the instruction. But after receiving his Ph.D. in 1934, Hassid began to look for a better position than was provided by his assistantship. In the early summer of 1935, he received an offer of a position and told Professor Hoagland that he was planning to leave before the beginning of the Fall term. Hoagland was upset at this news because he was preparing to attend a botanical congress in Amsterdam and would not return to Berkeley until the beginning of the term and then would have no experienced assistant for the laboratory course. He finally asked Hassid whether he would stay on if he received an Experiment Station appointment. Hassid agreed. Hoagland consulted with Dean Hutchison, *who* found that the available funds were insufficient to provide the usual starting salary of $2,000 per year, but he could offer $1,800. Hassid accepted this although it was considerably below what he was offered outside the University. He never regretted his decision. In 1939 he received the additional title of Instructor, and so was launched upon his academic career.

Hassid's independent scientific research began with an investigation of the ethanol-extractable carbohydrates in the marine alga *Irideae laminarioides,* and this work led step-by-step to an interest in the biochemistry of carbohydrates that he sustained in one form or another for almost thirty-five years. In the initial study, he identified dulcitol (galactitol) as a major component of this plant extract and observed that reducing sugars were notably absent from acid hydrolysates. He then purified and characterized an abundant polysaccharide from the same organism and showed it to be a sulfated polygalactan. From methylation studies, he was able to conclude that the galactosyl units were probably joined by 1 ` 4 glycosidic linkages and that the sulfate was probably esterified with the hydroxyl group on carbon 6. These results led him to speculate that the metabolism in this alga might involve a relationship between galac titol and galactan analogous to that between glucose and starch in higher plants, but he never followed up this idea.

Hassid's study of the structure of the algal galactan was the first of a long series of investigations of polysaccharides. Many of the preparations were provided by colleagues with whom he was always happy to collaborate. C. B. Lipman called his attention to a very viscous substance produced from mannitol by an unidentified bacterium that had been isolated "from a mud brick taken from a wall in an old Roman village which was built about 400 AD in the western desert of Egypt." In a paper with W. L. Chandler (1937), Hassid characterized the viscous material as a polysaccharide containing about ten anhydroglucose units. In the following years he published papers dealing with the molecular structure of canna starch (with W. H. Dore), dog liver glycogen (with I. L. Chaikoff), the dextran formed from sucrose by *Betacoccus arabinosaceus* (with H. A. Barker), an insoluble polysaccharide derived from *Saccharomyces cerevisiae* (with M. A. Joslyn and R. M. McCready), and glycogen and starch derived from sweet corn *(Zea mays)* (with R. M. McCready).

The existence of the enzyme phosphorylase that converts glycogen and inorganic phosphate to a-D-glucose 1-phosphate had been demonstrated by C. F. Cori and G. Cori in 1936, and the reversal of this reaction was reported in 1939 by W. Kiessling. Hassid and R. M. McCready (1941) undertook the structural analysis of the biosynthetic product and showed that it had starchlike properties but that the molecules were unbranched in contrast to the highly branched natural polymer. In a short review for *Chronica Botanica* published in 1942, Hassid related the prevalent view that "the enzyme phosphorylase, and not amylase as had been previously assumed, is chiefly responsible for the synthesis and breakdown of starch in the plant." McCready and Hassid developed a convenient method for prepar ing pure a-D-glucose 1-phosphate on a relatively large scale, thus making this important compound readily available for biochemical studies. They also developed a procedure for determining the relative amounts of amylose and amylopectin in starch based upon the large difference in the absorption coefficients of iodine complexes of the two components. The absorption coefficients of different starch samples were found to correlate well with the degree of hydrolysis of the components by -amylase. Their results supported the conclusion of K. H. Meyer that amylose consists of long unbranched chains of glycosyl units.

This experience with carbohydrates prepared Hassid for an important collaborative effort with S. Ruben and M. D. Kamen (1939) representing the first application of radioactive carbon to the study of photosynthesis. The short-lived ^{11}C-isotope (20.5 minutes half-life) had become available through Kamen's association with the Radiation Laboratory at the University in Berkeley, and a study was carried out to determine the distribution of the label from ^{11}C-carbon dioxide when fed to barley leaves in the light or after they had been kept in the dark for various periods of time. Although some of the label was incorporated into carbohydrate, the results indicated that most of it was present in the plant in a water-soluble noncarbohydrate form. In an extension of this work, the green alga *Chlorella pyrenoidosa w*as utilized in place of barley leaves, allowing a much more efficient incorporation of the radiocarbon label. Studies of the kinetics of incorporation in the light and dark, and on the reversibility of the reaction, were carried out with Ruben and Kamen (1940). Although it was concluded that "the greater fraction if not all the C*O_2 has been reduced to-C*OOH," no specific identification of the initial product of photosynthesis was made other than that there were "at least one alcoholic hydroxyl and one carboxyl group in the active molecules." The product was later identified as phosphoglyceric acid by M. Calvin and his associates.

Other than a brief collaboration with S. Aronoff, A. Benson, and M. Calvin (1947) on the distribution of label from $^{11}CO_2$ in photosynthesizing plant tissue, Hassid's research on photosynthesis was not continued and his involvement appears to have been based more on an interest in carbohydrate structural analysis than in the fundamentals of carbon fixation in plants. However, it is apparent from the later turn of events that this introduction to the utility of radioactive tracer techniques for elucidation of biochemical processes had a strong influence on his development.

Hassid's initial appointment as a Junior Chemist in the Agricultural Experiment Station was followed by promotion to the academic staff as an Instructor in Plant Nutrition (1939) and as Assistant Professor in 1941. About this time, he developed an association with fellow colleagues H. A. Barker and M. Doudoroff that grew into a close scientific collaboration and a lifelong friendship. His first joint study with Barker concerned the structure of an extracellular (1`6) dextran produced by the bacterium *Leuconostoc mesenterioides* when grown on sucrose. Less than three years later, he was involved with Doudoroff and N. Kaplan in the beginning of an important study on the biosynthesis of sucrose. Doudoroff had been interested in the bacterium *Pseudomonas saccharophila* because it oxidized sucrose faster than the component monosaccharides—glucose and fructose. This was demonstrated to result from the presence of an enzyme that converts sucrose to *a*-D-glucose 1-phosphate and D-fructose. Doudoroff, Kaplan, and Hassid found that this reaction can be reversed, leading to the formation of sucrose as detected by the production of a nonreducing substance that yielded reducing sugar on acid hydrolysis. That it was indeed sucrose was proved by Hassid, Doudoroff, and Barker (1944) when they prepared 2.5 grams of the crystalline disaccharide by the action of sucrose phosphorylase on a mixture of 15 grams each of *a*-D-glucose 1-phosphate and D-fructose and showed that its properties were identical with those of commercial sucrose.

The enzymatic synthesis of sucrose resulted in some publicity that came to the attention of officials of the Coca-Cola company, who were having difficulty obtaining sucrose because of wartime rationing. The company sent a representative to Berkeley to ascertain whether commercial quantities of sucrose could be made by the enzymatic method. Hassid and his associates were away on vacation at the time, so the Coca-Cola emissary discussed the problem with Professor Hoagland and reported that his company was prepared to provide $500,000 for research on this enzyme if a commercial process of sucrose synthesis seemed feasible. Unfortunately, Professor Hoagland was pessimistic about the possibility of sweetening Coca-Cola by this method, and so further support of research of sucrose phosphorylase was left to the University and the U.S. Public Health Service.

Later studies on sucrose phosphorylase showed that it could transfer D-glucose to L-sorbose and D-*threo*-pentulose to form nonreducing disaccharide analogs of sucrose and to L-arabinose to yield 3-*O*-*a*-D-glucopyranosyl-L-arabinose. The mechanism of the phosphorylase reaction was investigated by isotope exchange reactions with ^{32}P-orthophosphate, which was shown to exchange into nonradioactive *a*-D-glucose 1-phosphate. With alternative monosaccharide acceptors such as L-sorbose, the enzyme was shown to transfer D-glucose from sucrose in the absence of orthophosphate. Clearly, the reaction involved an intermediate D-glucosyl-enzyme that could be formed either from a-D-glucose 1-phosphate or from an *a*-D-glucosyl derivative such as sucrose.

Near the end of the 1940s, Hassid's research began to take a new direction as he concentrated on the synthesis of radiocarbon-labeled sugars and on the chemical preparation of sugar phosphates. E. W. Putman was a major collaborator in developing methods for preparing labeled sugars from plant tissue after their biosynthesis by the photosynthetic fixation of ^{14}C-carbon dioxide. They applied the new methods of paper chromatography to the preparation of uniformly ^{14}C-labeled carbohydrates of high specific activity, including glucose, fructose, galactose, sucrose, and starch. Hassid generously supplied radioactive sugars both to his colleagues and to many scientists throughout the country before they became commercially available. These studies on the preparation of labeled sugars were extended to investigations of the route by which the carbon dioxide, once fixed as glyceric acid phosphate, was converted into various carbohydrates, as well as the processes by which labeled hexose was taken up and utilized for synthesis of sucrose and cellulose.

About this time, Hassid was joined by two graduate students, V. Ginsburg and E. F. Neufeld, in an active program dealing with the role of sugar nucleotides in the interconversion of carbohydrates in higher plants. In the initial studies they were joined by P. K. Stumpf, who had been appointed to the Department of Plant Nutrition in 1948, and who, as an undergraduate student at Harvard University, had worked on purification of potato phosphorylase with D. E. Green. Although Stumpf is best known for his investigations on lipid metabolism in plants, his earlier studies in sugar metabolism were influential in the development of Hassid's interests in this field. The discovery by L. F. Leloir (1951) of uridine diphosphate D-glucose and the demonstration that this substance served as a glucosyl donor for synthesis of disaccharides focused attention on the sugar nucleotides as intermediates in the interconversion of carbohydrates, and Hassid directed his concern to these substances in higher plants. With Ginsburg and Stumpf (1956), he investigated the occurrence of uridine diphosphate derivatives of D-glucose, D-galactose, D-xylose, and L-arabinose in mung bean *(Phaseolus aureus),* the latter two derivatives being found for the first time in nature. The same source yielded the uridine diphosphate derivatives of *N*-acetyl-D-glucosamine and D-glucuronic acid (with J. Solms and D. S. Feingold), whereas the guanosine diphosphate derivatives of L-galactose and D-mannose later were identified in the red alga *Porphyra perforata* (with J. C. Su), and guanosine diphosphate D-mannuronic acid was isolated from the brown alga *Fucus gardneri* (with T. J. Lin).

These studies on the natural occurrence of sugar nucleotides in plants were paralleled by investigations of their biosynthesis by the pyrophosphorylase reaction. A series of papers with Neufeld, Putman, Feingold, Ginsburg, and others delineated the presence in higher plants of pyrophosphorylases that formed the respective uridine diphosphate hexoses from reaction of uridine triphosphate with the 1-phosphate esters of *a*-D-galactose, *a*-D-xylose, -L-arabinose, *a*-D-glucuronic acid, *a*-D-galacturonic acid, and *N*-acetyl-*a*-D-glucosamine. Because these studies required the sugar 1-phosphates as substrates and such substances were not generally available at the time, considerable effort was devoted to the improvement of published syntheses and the development of new ones for the preparation of glycosyl phosphates. Later studies dealt with the enzymic phosphorylation of several sugars, including D-galactose, L-arabinose, D-glucuronic acid, and D-galacturonic acid (with Neufeld, Feingold, and others).

Hassid's international reputation attracted senior scientists from around the world to work in his Berkeley laboratory. One of these was Winifred N. Watkins from the Lister Institute of Preventive Medicine in London, who is noted for her studies on the structure and biosynthesis of the blood group antigens. In 1961 Watkins and Hassid undertook a study of the biosynthesis of lactose in mammary tissue. It had been claimed by

J. G. Gander, W. E. Petersen, and P. D. Boyer (1957) that bovine mammary tissue contained enzymes that converted uridine diphosphate D-galactose and *a*-D-glucose 1-phosphate to lactose 1-phosphate, and that the latter was hydrolyzed to free lactose. However, since uridine diphosphate D-galactose and *a*-D-glucose 1-phosphate are in ready equilibrium by way of uridine diphosphate D-glucose, one would expect ^{14}C-D-glucose to be incorporated equally into the two parts of lactose by this pathway, an expectation that was contrary to recorded observations by other workers. Watkins and Hassid reinvestigated this matter and established that uridine diphosphate D-galactose and free D-glucose were the precursors of lactose in lactating guinea pig and bovine mammary tissue and that the product was lactose rather than lactose 1-phosphate.

In addition to these results on lactose synthesis, Watkins and Hassid (1962) also made the important observation that mammary tissue contained an enzyme activity that transfers D-galactose to *N*-acetyl-D-glucosamine. Noting the occurrence in milk of oligosaccharides that contained this lactosamine unit, and finding that different mammary gland preparations gave different relative amounts of ^{14}C-lactose and *N*-acetyl-^{14}C-lactosamine when incubated with uridine diphosphate ^{14}C-D-galactose, they concluded that "different enzymes are responsible for the synthesis of the two compounds." This conclusion, and a later one by Helene Babad and Hassid (1966) that the soluble, purified lactose synthetase from milk was "very labile to further purification," proved to be incorrect, for it was found subsequently by K. E. Ebner and others (1967) that the mammary gland -D-galactosyltransferase is under the control of a specificity-altering protein, *a*-lactalbumin. In the presence of a-lactalbumin, the preferred acceptor is D-glucose, and lactose is the product, whereas in absence of the protein, *N*-acetyl-D-glucosamine acts as the acceptor to yield *N*-acetyl-lactosamine. The observations of Watkins and Hassid can be explained by the presence of variable amounts of a-lactalbumin in their lactose synthetase preparations, and the results of Babad and Hassid are explained as reflecting the first successful fractionation of these two proteins.

The existence of enzymes that interconverted sugar nucleotides without degrading the molecule had become recognized by the latter part of the 1950s, and Hassid turned to the study of some of these reactions in plants. With Neufeld and Feingold, he demonstrated the enzymic conversion of uridine diphosphate D-glucuronic acid to the nucleotide derivatives of galacturonic acid, xylose, and arabinose. In a later study, the enzyme activities that catalyzed these reactions were separated so that it was possible to show the 4-epimerization of uridine diphosphate D-glucuronic acid to the galacturonic acid derivative as an isolated step and to demonstrate the decarboxylation of uridine diphosphate D-glucuronic acid to uridine diphosphate xylose. An enzyme activity was also found that epimerized the xylose derivative to the arabinose derivative.

Throughout his career, Hassid was concerned with the fundamental question of how the sugar that is formed in a photosynthesizing plant is converted to disaccharides such as sucrose and into polysaccharides such as starch and cellulose. His earliest experiments dealt with the infiltration of radiocarbonlabeled sugars into plant leaves, but later they became more sophisticated with the utilization of well-defined sugar nucleotides as specific donors in cell-free enzyme systems. It was known that uridine diphosphate D-glucose was a precursor of a -1`4-glucan in *Acetobacter xylinum* (L. Glaser, 1957) and of the *a*-1`4-glucan, glycogen, in liver (Leloir, 1957). Feingold, Neufeld, and Hassid (1958) reported the synthesis of a -1`3-glucan from this sugar nucleotide by a digitonin-treated particulate transferase from mung bean, the product apparently being identical to laminarin, whereas R. A. Dedonder and Hassid (1964) observed formation of a -1`2-glucan in *Rhizobium japonicum.*

In a similar fashion, uridine diphosphate xylose was shown to be the precursor of a -1`4-xylan in asparagus *(Asparagus officinalis).* Hassid also investigated the synthesis of alginic acid (with T.-Y. Lin), the synthesis of pectin (with C. L. Villemez), and the methylation of the latter polysaccharide to form the ether and ester derivatives (with H. Kauss).

This interest in polysaccharide formation in plants led Hassid naturally to an investigation of cellulose biosynthesis and perhaps to the culminating point of his scientific career. Even today the study of cellulose biosynthesis in higher plants is fraught with technical difficulties that have hindered the definition of this system in the same detail that has been possible with other polysaccharide-forming reactions. In 1964, A. D. Elbein, G. A. Barber, and Hassid reported the formation of cellulose from guanosine diphosphate D-glucose and a particulate enzyme preparation from mung bean seedlings. Contrary to the finding of Glaser with a bacterial system, the plant synthetase had no activity with uridine diphosphate D-glucose. The polysaccharide product was characterized primarily on the basis of its alkali insolubility, which was similar to that of cellulose, and on the formation of radioactive cellobiose and cellodextrins by acid hydrolysis and by acetolysis. The study was complicated by the concomitant formation of a glucomannan from endogenous guanosine diphosphate mannose. However, a soluble enzyme system that made cellulose was eventually obtained (with H. M. Flowers, K. K. Batra, and J. Kemp, 1969), and this product was only slightly contaminated by mannose.

Some controversy concerning cellulose biosynthesis arose when D. O. Brummond and A. P. Gibbons (1964) reported that uridine diphosphate D-glucose was a precursor of a cellulose-like polymer in *Lupinus albus,* and L. Ordin and M. A. Hall (1967) observed a similar reaction in *Avena sativa.* These reports stimulated Hassid to restudy the roles of both uridine and guanosine diphosphate D-glucose in the synthesis of alkali-insoluble poly saccharides in *Phaseolus aureus.* He eventually established, with H. M. Flowers, K. K. Batra, and J. Kemp (1968), that the polymer formed with uridine diphosphate D-glucose in *L. albus* was an alkali-insoluble -1`3-glucan rather than cellulose, whereas that produced by *A. sativa* was a mixed -1`3- and -1`4-glucan. C. M. Tsai and Hassid (1971) succeeded in separating the two enzymic activities of *A. sativa* that make the two polysaccharides, and they found that the type of glucan made was dependent on the concentration of the sugar nucleotide donor.

From this brief survey, we can see that there were few features of carbohydrate metabolism in plants that escaped Hassid's touch, and much that we know about the role of sugar nucleotides in the interconversion of carbohydrates in plants is a direct result of his persistent effort. From the incorporation of labeled precursors into monosaccharides, to the conversion of the monosaccharides to their glycosyl 1-phosphates, to the action of the pyrophosphorylases in the synthesis of the nucleoside diphosphate sugars, to the interconversion of the resulting sugar nucleotides, to the polymerization of the activated monosaccharides yielding disaccharides and the homopolysaccharides, and finally to the modification of the polysaccharides by methylation—in summary, to almost every aspect of carbohydrate metabolism—Hassid contributed his full and devoted attention. Nor did his efforts slacken with age, for he continued working and writing and thinking science as though it was among the most important things in life, and to him it was. He also believed in the importance of communication as a force in scientific progress, for he was a prolific writer of reviews and contributed heavily to books and serials dealing with carbohydrates, the total of such articles being almost fifty.

Hassid's many contributions on the structure and synthesis of plant carbohydrates were recognized by a number of honors and awards. He received the first Sugar Research Award (1945) of the National Academy of Sciences (jointly with M. Doudoroff and H. A. Barker), the Charles Reid Barnes Honorary Life Membership Award of the American Society of Plant Physiologists (1964), and the C. S. Hudson Award of The American Chemical Society (1967). He was elected to membership in the National Academy of Sciences (1958) and the American Academy of Arts and Sciences (1969), and he was honored at the 6th International Symposium on Carbohydrate Chemistry (1972) as one of three outstanding senior American carbohydrate chemists. He was elected Chairman of the Division of Carbohydrate Chemistry (1949-1950), American Chemical Society, and he served as a member of numerous editorial boards, including those of the *Journal of Biological Chemistry, Annual Review of Biochemistry, Carbohydrate Research, Phytochemistry,* and *Analytical Biochemistry.*

Hassid married Lila Berlin Fenigston in 1936. They had no children, but any void this may have created in their lives was filled by the many friends who shared the warm hospitality of their home in the Berkeley hills. Lila was a gracious and vivacious hostess and an accomplished violinist who made their home a center for friendly social gatherings and for the performance of chamber music. She also had a talent for making fine English translations of Yiddish poetry, which were either published in Jewish periodicals or presented orally in a series of radio programs. Lila's appreciation and contributions to the arts provided a happy counterpoint to the scientific life of her husband.

Perhaps as a result of his severe childhood illnesses and early deprivations, Hassid never enjoyed robust health. For many years, he suffered from the effects of high blood pressure, and he had debilitating attacks of hyperthyroidism and hepatitis. In his early sixties, he suffered a severe coronary occlusion from which he never fully recovered. As he grew older he developed increasing coronary complications that finally resulted in his death on April 28, 1974. He left many friends who will remem ber him as a friendly, gentle, and soft-spoken person, but one who on rare occasions, when sufficiently annoyed, could display a strong temper. His personal warmth and generosity, coupled with his sincerity and modesty, attracted many friends, whom he treasured and often regarded as somewhat larger than life. He took pride in the accomplishments of his colleagues and students and spent much time and effort in helping to further their careers and in nominating them for promotions, awards, and honors of various sorts. He never tired nor stinted in helping those who he felt were deserving.

The information in this memoir relating to Hassid's early life in Russia and Palestine was obtained from my (H.A.B.) conversations with him, from letters (dated June 2, 1974 and June 25, 1974), and from an unpublished account of Hassid's early life written by Dr. Rivka Ashbel (prepared for the celebration of Petah-Tikva's seventieth anniversary and based on conversations between Hassid and Ashbel in 1949). Additionally, we had available a number of documents including Hassid's passport application, naturalization papers, and records of his service in the British army and service medals. Most of this material and some of Hassid's manuscripts and letters are kept in a file in the Bancroft Library at the University of California, Berkeley.

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Frank Lappin Horsfall, Jr.

December 14, 1906-February 19, 1971

By George K. Hirst

Frank L. Horsfall, Jr. was a clinician and a virologist whose influential leadership came primarily through his perceptive scientific experimentation, both in the laboratory and in the clinic, and also through his vast administrative skill.

He was born December 14, 1906 in Seattle, Washington, where he spent all his formative years until he was twenty-one. His father, a native Vermonter, was a prominent surgeon who maintained a large house on Capitol Hill, and Frank, the first of four children, was a high-spirited youth whose interests led him into a wide range of activities. By the time he entered high school, he had decided to become an engineer, and he spent afternoons and evenings with a friend rigging up the family Victrola for radio reception. In the course of his four years at high school, he participated actively in the student council, the boys' athletic association, the glee club, and the radio press association. He was valedictorian of his class.

During four years of college at the University of Washington he lived in a fraternity house, and during the early part of this experience he was uncharacteristically erratic in the pursuit of his studies. It was during this period that he went out for crew, a major sport at Washington, and made the junior varsity. One of his outstanding characteristics was a tendency to throw himself headlong into every new activity, and it is reported that he became a passionate oarsman, spending every moment of his spare time rowing or hanging around the boathouse. However, many years later, although he kept his oar on the wall, he completely disavowed any interest in rowing and for that matter in all other forms of athletics, either as a participant or as a spectator.

Midway through his college course he gave up the idea of becoming an engineer and decided to enter the medical profession. This involved some heroic scrambling to complete the course-work requirements, but in 1927 he followed in his father's footsteps by entering McGill University, in Montreal, Canada, and graduated in 1932, at which time he received the Holmes Gold Medal for having attained the highest scholastic record in his class.

After receiving his medical degree, he spent a year as a house officer in pathology at the Peter Bent Brigham Hospital in Boston. This was a common preliminary to a postgraduate education in surgery. It was during this year that he discovered that he was exquisitely sensitive to formaldehyde, a fact which markedly influenced later career decisions.

He responded characteristically to this disability by embarking at once on an extensive study of formaldehyde sensitivity in experimental animals and coupled this with a long series of experiments on himself. The result was two substantial papers on the subject, in which for the first time Horsfall was the sole or the senior author, and in them he acknowledged the assistance and advice of both H. Zinsser and S. B. Wolbach. This was the beginning of Horsfall's informal training in immunology, training which was to continue after another year at the Rockefeller Institute.

He returned to Montreal for a year's internship in medicine at the Royal Victoria Hospital and then signed up for a year in surgery, but shortly after starting the latter position he found that it was incompatible with his formaldehyde sensitivity, and he promptly resigned. Immediately thereafter, in the fall of 1934, he went to the Rockefeller Institute in New York City, where he was to remain with only minor interruption for the next twenty-five years. On giving up a surgical career, for which he was eminently qualified, he showed no outward evidence of regret and quickly plunged off in another direction, which involved him in clinical research coupled with basic research.

Horsfall came to New York at a time when the Rockefeller Institute Hospital was at a peak of its reputation in the academic medical world. Although it was a very small unit, it had a very big reputation for pioneering in medical research. The staff members were both clinical and nonclinical, and there was a well-developed mystique among them concerning the catalytic effect which clinical contact had on basic research.

A majority of the young people there were like Horsfall in that they came from purely medical backgrounds and were to spend formative postdoctoral years in this very stimulating environment learning, in an informal manner, the basic disciplines such as bacteriology, virology, immunology, and physiology. Through these young people the Hospital provided excellent medical care, and at the time Horsfall was added to the staff the pneumonia service had nearly completed a large and successful series of cases in which type I pneumonia had been treated with specific antipneumococcal horse serum. These results had aroused widespread interest, and such sera were coming into general use.

In addition to the time spent on clinical responsibilities, all of the younger staff members were engaged in some basic research project. Those on the pneumonia service often worked on model infections in mice. Among the older staff members there were some who did no clinical work at all, and the most outstanding of these was Oswald T. Avery. The exceptional thing about the Avery school was that its immediate goals were so great and still far removed from any clinical application. His very solid intellectual outlook set the tone for the whole department.

On entering this mixed atmosphere of clinical and academic science, Horsfall clearly and consistently showed his marked preference for those activities for which there appeared to be obvious relevant application to clinical work. Horsfall's emphasis on medical application would be demonstrated again and again throughout his career, even when he was not taking care of patients. He tackled his clinical responsibilities with enthusiasm. He was an eager therapist, and there are apocryphal reports about the strong restraints required to prevent him from testing in patients the enzyme capable of splitting the type III capsular carbohydrate, which had been developed by O. T. Avery and René Dubos. He was also an enthusiastic advocate of using type-specific serum in treating lobar pneumonia, and his influence was an important factor in switching the routine treatment at the Hospital from horse to rabbit antiserum.

This change in treatment was very successful and aroused wide interest. A large number of cases were treated with rabbit serum at the Hospital, and Horsfall and his colleagues published several extensive papers describing the clinical results. The use of this new method caught on generally. Commercial drug houses began producing rabbit antiserum, not only against types I and II, but also against a myriad of the rarer varieties.

Horsfall, working with Kenneth Goodner, accompanied this clinical work with a tremendous burst of laboratory activity in which they studied the comparative immunology of horse and rabbit sera in detail. These sera were found to differ from each other in a number of fundamental ways. In the space of a little more than a year, Horsfall and his colleagues published some twenty-three papers on these systems. Thirty or more immunological differences were highlighted, many of which were interpreted as suggesting distinct therapeutic advantages for the rabbit antibody.

Horsfall remained at the Hospital for three years and during his last year was Chief Resident Physician, a position which greatly increased his clinical responsibilities. Avery at the time was in the midst of his fascinating and fundamental work on the transforming principle. Horsfall was very familiar with all the current details of that work, which had been formalized by Avery into a series of personal lectures known as the Red Seal records. Frank Horsfall frequently sat at Avery's feet adsorbing the gospel, which he later was able to repeat in minute detail and often did so with great gusto. Yet he never did any research with Avery, much as he admired his whole approach. Their scientific style was entirely different, and Horsfall would have had a most difficult time adjusting to Avery's idiosyncrasies. For Horsfall, the transforming principle must have seemed far away from the world of patients.

Although the Rockefeller Hospital was at this time a prime source of candidates for academic positions in medical schools throughout the country, positions within the Hospital itself were at a premium. Nevertheless it became clear within a very short time that Horsfall's future would be in the Rockefeller Institute. It is difficult to assess the reasons for this early popularity which lay partly in his ability to see problems clearly, to apply himself to the problem with enormous energy, and then finally to present his results in a way which seemed simultaneously to be both conservative and expansive, romantic yet convincing. Thomas Rivers, who was a very influential person in American medicine at that time, was extremely devoted to Horsfall and played a large part later in promoting him as his own successor at the Rockefeller Institute Hospital.

It came as no surprise when in 1937 Horsfall resigned from the Hospital and accepted a staff position with the International Health Division of the Rockefeller Foundation. The change in employer was almost imperceptible. The principal Foundation laboratories were in fact housed at the Rockefeller Institute, and Foundation scientists were treated as part of the family. Horsfall's daily contacts with staff members in the Hospital and the Institute continued as before, including his exceptionally close relationship with Rivers, a friendship which was now further cemented by the transformation of Horsfall into a virologist.

The Foundation had made a reputation for itself in virology through its pioneering work on yellow fever, and its leaders were anxious to continue this record of preeminence through an attack on the influenza problem. Horsfall became head of a laboratory section, previously organized by Thomas Francis, Jr., and the time seemed really ripe for developing a prophylactic agent against epidemic influenza. The virus of influenza had been isolated only four years before, and the Foundation offered Horsfall a very large technical staff, abundant laboratory space, generous financial support, and a sizeable professional staff that was equipped by experience to do large-scale fieldwork and to tackle epidemic problems on an international scale.

It is somewhat ironic that Horsfall stepped out of the lobar pneumonia scene just as it was undergoing drastic change. The elaborate type-specific serotherapy routines which had been very painstakingly developed in the mid-thirties were briskly and permanently swept away with the advent of chemotherapy and antibiotics. This had little to do with Horsfall's change of employer at this time, but does reflect the sweeping pace of medical advances. During his three years on the pneumonia service, he had received excellent training in immunology. Plunging into virology was to be a new experience, and he quickly became a master of the fundamentals. Once again, he threw himself wholeheartedly into a new activity, which was to occupy his attention for several years.

Before leaving the Hospital, Horsfall married Norma Campagnari, who worked there as a nurse. It was a most successful marriage. She was a quick, merry, and very lively person and a gracious hostess. Horsfall's domestic life was always important to him, and he devoted much time to it in spite of numerous external demands. They had three children, and he was very much attached to them all.

The Foundation provided a proper setting for an individual of Horsfall's brilliant and expansive outlook. In addition to his major projects, he was able to devote much of his time to some very fundamental biological problems, and one of these areas was quantitative biology. He became involved with and obviously enjoyed the design and execution of mechanical projects like a low-temperature storage cabinet or a complicated ventilation system for a single room containing a large number of ferrets that had to be individually housed. The latter project was so successful that it enabled him to conduct experiments on the highly contagious distemper virus without danger of spontaneous cross-infection.

The research was nearly all on influenza virus, and at that time it was necessary to measure virus concentration by intranasal inoculation of material into mice. To achieve any reasonable sort of accuracy required the infection of large numbers of animals, followed by individual isolation during the incubation period. Nevertheless, Horsfall did very large, highly quantitative experiments with influenza, in which he attempted to work out the amount of antibody required to neutralize different amounts of virus in the inoculum. This was the beginning of a long and complicated experimental series on neutralization, carried out with great care and repeated many times in the years which followed.

The idea of these experiments was to develop some conception of the mechanism of neutralization. He pushed the problem as hard as he could with the techniques at his command, but it was not until many years later that the mystery of neutralization began to unravel, and it was learned that much simpler host systems were essential to understand neutralization mechanisms.

Shortly after starting work with the Foundation, Horsfall took time out for a six-month sabbatical with Arne Tiselius at Uppsala. At that time Tiselius was carrying out his important pioneer work on the electrophoresis of macromolecules. Although Horsfall published a couple of papers with Tiselius in the field of physical chemistry, it is clear that he could not use this experience in virology without wandering far afield from his customary goals.

The main attraction of the Foundation position was the possibility of studying human influenza infection on a vast scale and attempting prophylaxis against the disease. The Foundation was well equipped for this sort of venture. With its previous experience in yellow fever research, it had developed numerous experienced epidemiologists and other fieldworkers whose value in executing the experiments that were planned in New York would be difficult to exaggerate. In his first years with the Foundation, Horsfall carried out some routine but straightforward investigations of influenza epidemics. This work, in a very new field, carried the mark of competence with it.

A ferret colony was an essential resource in carrying out influenza work at that time, and during the course of some early experiments, a spontaneous epidemic of distemper broke out in the Foundation's animal house and nearly all the animals were lost. To prevent a recurrence of this disaster, all new animals were immunized, in most cases using a formalinized spleen from a spontaneously infected animal. Later, some of these immunized animals were found to have mysteriously acquired high anti-influenza titers. It occurred to Horsfall at once that distemper virus infection might have some sort of cooperative effect in inducing high-level and persistent antibody responses to influenza virus.

It was a fascinating idea, and a "complex" vaccine containing both influenza and distemper virus was quickly developed for use in human beings. The Foundation provided large-scale pro duction facilities for the new product, which was made by growing both viruses in chick embryos and then freeze-drying a formalinized extract. With the threat of a widespread epidemic (1940-1941), there was no time to do a lot of testing on preparations before they could be used in human experiments. There were some feverishly performed experiments in laboratory animals and man, but they were not completed in time to modify the large-scale experiments which were being contemplated in the field.

Large amounts of vaccine were prepared and shipped to Great Britain, which was at war, just in case the epidemic became intolerable. The British examined the preparations in great detail, but did not utilize an opportunity to try them. In the United States, however, Horsfall conducted a large-scale demonstration, vaccinating, under well-controlled circumstances, volunteers in a large number of state prisons up and down the Eastern Seaboard.

The experiment was carried out in magnificent style. E. R. Rickard, who was Horsfall's chief field marshal, was a master at this type of human experiment. Not only was the distribution of inoculations carefully controlled, but a great deal of data was obtained on individuals through attempts at virus isolation and by determining pre-and post-infection antibody titers. Fortunately, the inoculations were completed a relatively short time before the onset of the epidemic, and the use of a large number of population groups proved valuable in the final analysis of the results.

In spite of the excellence of organization and the occurrence of a large-scale epidemic in the wake of vaccination, the results were very disappointing. Although antibody rises had been induced in many vaccine recipients, the rise in titer was small and evanescent, and the reduction in case rate was in most institutions negligible or at best marginal. Only in a couple of places was there as much as a 50 percent drop in incidence following vaccination, and it turned out that in these places the batch of vaccine used had had the highest virus titer before formalinization.

When all the laboratory tests on the complex vaccine were finally in, the results explained in some degree the disappointing field tests. In general the virus content of the vaccine was low, and the initially promising effect of distemper virus in boosting antigenicity could never be reproduced. It is again rather ironic that, just as these results were being recorded, new methods of obtaining high virus titers, as well as high virus purity, were being developed. It wasn't until several years later that vaccines made with these improved reagents were tested and gave the first bona fide protection against influenza in the field. Even years later the secret of obtaining substantial and prolonged effects against epidemic influenza is still a formidable problem, and the earliest experiences with the "complex" vaccine were by no means unique.

Just as the episode of the vaccine trial was coming to a close, Horsfall was called back to the Rockefeller Institute Hospital, where he received a lifetime appointment as a full member. Essentially he took over Rivers' Department of Virology, but he peopled it with entirely new personnel, and in his new program he once again confined his attention to respiratory disease. Rivers, who was now Director of the Rockefeller Institute Hospital, played a very important role in shaping developments within that organization. The return of Horsfall, with the possibility that he might become the Hospital's next Director, brought Rivers' planning to a climax.

By now the United States had entered the war, and Rivers organized a naval unit at the Hospital, most of which later went to Guam under his command. The remainder of this unit stayed on at Rockefeller Institute with Horsfall as the commanding officer, and as a group they worked on respiratory diseases, especially those that they felt might have a virus etiology such as atypical pneumonia. This naval unit continued right on into the post-war period with little change in its structure, problems, or personnel.

Thus in 1941 Horsfall began the main period in his scientific life, which was to last about twenty years, and during which he had magnificent resources, was able to devote his full time to science, and was free to determine his own course of action. In the latter part of this period, his duties would become more and more administrative, culminating finally in his move to Sloan-Kettering. In 1941, however, the Rockefeller Institute provided a most salubrious climate for research. The place was populated by a scientific elite, mainly oriented toward biology and completely involved in research activity in a life which was unencumbered by teaching or institutional politics, as might be the case in a university.

Horsfall had a widely recognized and well-justified reputation for fostering research activities in his department in such a way that the individual investigators enjoyed both great freedom and superb stimulation. By now Horsfall had received a thorough, although informal, education in the immunology, bacteriology, and virology of the day; and characteristically, as in the past, he continued to work in areas which were closely relevant to infections in man. Hospital beds were available, and the group took on as its main project the search for the etiology of a recently developed entity called atypical pneumonia. In the previous few years this affliction had replaced lobar pneumonia as the principal acute respiratory disease of man.

In this new situation Horsfall again threw all of his energy into the attack, and a multipronged effort was mounted. A putative agent was isolated in mice by serial passage of human lung material. An agent was isolated in the mongoose from a similar source. Still other investigations involved the use of cotton rats. A gram-positive bacillus was isolated from human cases of atypical pneumonia, and the relationship of this organism to the disease was thoroughly explored. This led to the discovery of some rather nonspecific serological responses in the patients suffering from pneumonia. Some of the work suggested a relationship of PVM (pneumonia virus of mice) to the human disease. The volume and detail of their explorations were quite impressive, but none of these efforts led to any firm conclusions at the time regarding the etiology of atypical pneumonia.

The search for an agent by Horsfall's group and others turned out to be far ahead of its time, and it was not until the mycoplasmas were delineated as a separate microbial group that investigators were able to determine the causative organism of this kind of pneumonia. In retrospect it was then shown that some of the earlier "virus isolates" (e.g., Eaton's lung passages from cotton rats) did in fact contain what we know now is the agent.

It is pertinent at this point to reemphasize the fact that Horsfall's training in basic disciplines was informal and that he approached his main problems, like atypical pneumonia, as a physician. This absence of formal scientific training was also true of the majority of students who came to work with Horsfall, many of whom settled into prominent academic clinical positions after leaving Rockefeller. This approach to viruses primarily as agents of disease was quite typical of the virology of the time, and as a result of this approach, the sum of the work done (on say atypical pneumonia) lacked the cohesion that came later, after the discipline had developed a more impressive internal structure. The biological revolution which was to gain momentum in the sixties completely changed the face of virology, which in the forties and fifties was dominated by clinical viewpoints and hence was somewhat amorphous.

During this period at Rockefeller, Horsfall's attention was not confined to the problem of atypical pneumonia. Some years before, while working on influenza with Richard Hahn, he was making some mouse-to-mouse passages of human lung material, and he isolated an agent which he called PVM. This virus was normally latent in mouse colonies, but with passage its virulence could be enhanced so that it would cause pulmonary consolidation with a moderate mortality. This agent was to receive intensive study from time to time, in part because of its pneumotropism and also because it was thought to be related to the causative agent of some human pneumonias. The disease which it caused in mice was throughly studied in every aspect, including attempts at treatment and prophylaxis.

In the early stages of this work, it was found that the virus agglutinated red cells, and in addition it was shown that some substance in lung extracts also adsorbed to the virus. One of the most striking findings, however, was the discovery that certain high-molecular-weight carbohydrates had a distinct therapeutic effect on the pneumonia which the virus induced in mice. This unexpected finding came about when Horsfall, working with M. McCarty, was looking at the effects of the streptococcus MG (isolated from cases of atypical pneumonia) on PVM infection in mice. They found that a capsular polysaccharide from this bacillus had a profound therapeutic effect on the course of PVM infection in mice. The material was effective even after the disease had become well established and the intact macromolecule was necessary for therapeutic activity. Later on Horsfall and H. S. Ginsburg found that a similar polysaccharide from Friedländer's bacillus, group B, had an even greater therapeutic effect, and it also worked very well on mumps virus infection, in which the growth of virus was markedly suppressed.

All of the foregoing experiments were carried out on infections in complex organisms such as the developing embryo or the adolescent mouse. There never has been an adequate explanation of the curative mechanism at work, and the experiments have not been repeated with simpler host cell systems which became available later. Igor Tamm and Horsfall initiated a further series of chemotherapeutic experiments with the benzimidazoles and their derivatives on influenza virus infection, and thereafter the series was carried on by Tamm and others.

During this period Horsfall never forgot his early interest in influenza, and periodically he returned to influenza problems. It was a difficult virus to manage, but it had attractions which were then unknown for other agents; and Horsfall, always interested in quantitative biology, continued to study the relationship between hemagglutination inhibitors and virus-neutralizing antibodies. He was especially interested in the problem of antigenic strain differences between various influenza A isolates and did some very interesting work with I. Archetti showing that the antigenic profile of a number of influenza stocks could be quickly and permanently altered by *in ovo* passage in the presence of appropriate antibody. This type of experiment furnished the basic information through which the changes found in influenza virus from epidemic to epidemic could be explained, and this information is very fundamental to modern concepts of how the virus operates in nature.

Horsfall's interest extended briefly to other viruses from time to time, and his papers include reports on Coxsackie viruses, herpes simplex, mumps, and others. With some of these agents he was exploring the possibility of chemotherapeutic effects. He was also interested in the phenomenon of interference, in the effects of hormones (such as cortisone), and in metabolic inhibitors like fluoroacetate, which led eventually to the work with benzimidazoles.

Perhaps the most notable thing about this period of Horsfall's life was the long series of collaborators who worked with him in the New York laboratory, many of whom subsequently went out into key positions in the medical-academic world. The list includes such individuals as Lewis Thomas, Edward Curnen, Harry Ginsberg, D. A. J. Tyrrell, M. R. Hilleman, E. D. Kilbourne, Igor Tamm, F. M. Davenport, G. S. Mirick, and Maclyn McCarty. It would be impossible to find a more outstanding group of microbiologists who were connected with any other single laboratory. Because of the way in which Horsfall operated, these individuals often contributed as much as they received, and each one in his way left his mark on the group. It was part of Horsfall's genius that he could at the same time exert a strong influence over the events occurring in his laboratory and also allow great freedom of expression.

In 1953 Herbert Gasser retired as Director of the Rockefeller Institute, and the accession of Detlev Bronk to this post foreshadowed great changes for the organization and all who were connected with it. Within a couple of years, Bronk converted the Institute into a University of Science, took in students, enlarged the faculty, added to the physical plant, and severely shook up its reserved character. During this period of rapid change, Bronk was preoccupied with plans for expansion, and in addition he played a very active role as President of the National Academy of Sciences.

As Vice-President and Physician-in-Chief to the Hospital, Horsfall was now second in command. He assumed the responsibility for many of the day-to-day tasks in running such an institution, and during this period he was especially appreciated by the older members from Institute days and often devoted a great deal of time and attention to their special problems. He became further and further separated from the laboratory, where, fortunately, Igor Tamm was present to assist in carrying on the old tradition. Prior to this time almost all of Horsfall's papers were published under joint authorship, but thereafter he published alone, mainly reviews on such topics as chemotherapy of virus infections and others with which he was familiar. He never gave anyone the impression that he was unhappy with this change, as he slid almost imperceptibly into a totally different kind of existence.

Being second in command and defender of the old tradition under Bronk was not entirely easy, and when in 1959 the directorship of the Sloan-Kettering Institute became available, the trustees (including a Rockefeller brother as chairman) offered the post to Horsfall, who accepted and made the move just across York Avenue.

Replacing Cornelius Rhoades was difficult, for Sloan-Kettering at the time was virtually the single-handed creation of Rhoades, who had built it up in the course of some twenty years. He made many of the staff appointments and followed many individual research programs in a very personal way. Horsfall's style was entirely different. He made few immediate changes, but in time he developed his own very loyal staff. Previously the emphasis at Sloan-Kettering was on studies of the chemical carcinogens and on cancer chemotherapy. This slant was not changed at once, and much of this kind of activity was preserved. Naturally the new emphasis was on the viral etiology of cancer and on molecular biology, both of which were just coming into prominence elsewhere. Horsfall was a staunch advocate of basic research in biology, and he was eclectic in his approach toward the tumor problem. He was conservative in his estimates of future progress in the cancer field and was unwilling to make superficial and encouraging predictions. This attitude was repeatedly expressed in the numerous reviews on the cancer problem which he began writing at this time.

When Horsfall came to Sloan-Kettering there was already a large building program under way, which he saw to completion. He also played the most important role in added staffing. In regard to the latter, he was also very reserved and relied heavily on a board of scientific consultants, with whom he met several times a year and went over many of the details of ongoing scientific programs. In the ensuing discussions of scientific merit, he was forthright and outspoken and used the highest standards of judgment, but he also listened intently to advice that was given. Because of his conservative attitude, things changed slowly, and his effect on the institution became evident only in his latest years.

Outside of the institutions in which he worked, Horsfall's life was a continuous flurry of activity. He was especially well known for his leading role on various boards of both local and national character. On the federal level he was at various times a member of the President's Commission on Heart Disease, Cancer, and Stroke and of the Defense Science Board of the Department of Defense. In the years immediately after the war, he served as consultant to the Surgeon General of the U.S. Army and was on the Commission on Immunization of the Army Epidemiological Board. Outside of the government he was active in the National Foundation for Infantile Paralysis for many years, both before the vaccination campaign for poliomyelitis and after the focus of attention was switched to congenital diseases. He was a member of many scientific advisory bodies for such organizations as the Institute of Microbiology at Rutgers, the Oklahoma Research Foundation, and the Roscoe B. Jackson Memorial Laboratory at Bar Harbor, Maine.

Even more impressive was his devotion to the affairs of New York City, where he was a member of the Board of Directors of the Public Health Research Institute from 1955 on and was chairman of its Research Council for a large part of that time. In addition he was a very important influence in the organization and development of the Health Research Council of New York City, which for many years played a very important role in public health research.

To this sort of managerial responsibility Horsfall brought a very special talent. He was always completely familiar with the underlying structure in any organization that he served, and he always came to meetings fully briefed and with all of his homework well in hand. In the meetings themselves he played an especially important role in presenting and analyzing difficult problems, a role which he performed with such remarkable clarity that the problems would be understood by all, including the lay people who were present. Both in official and in private discussions, he made frequent use of hyperbole to drive home his point. His exaggerations were sometimes outrageous and were frequently coupled with a puckish manner. Even with long experience it was difficult to tell if he was serious.

Mention was made earlier of his overriding interest in clinical medicine, an interest which became obvious in a number of ways. In addition to devoting a large portion of his scientific time to problems which had clinical application, he also devoted a great deal of time and effort to writing and speaking, very often on clinical topics. He contributed a great many articles to some of the best-known medical textbooks, and these dealt generally but not exclusively with virus diseases and their treatment. He edited (with Igor Tamm) and wrote extensively for the third edition of the widely used textbook, *Virus and Rickettsial Diseases of Man.* He also wrote a large number of reviews on chemotherapy in which the emphasis was frequently on agents of potential use in man.

During his lifetime he received many honors, dating from the time of his graduation from Medical School. He was given the Eli Lilly Award in Bacteriology and Immunology in 1937, the Casgrain and Charbonneau Award in Medicine from McGill in 1942, the John Lewis Prize from the American Philosophical Society in 1959, and the 50th Anniversary Gold Medal Award of Peter Bent Brigham Hospital in 1963.

Horsfall was elected to the Academy in 1948 and served on its Committee of Science and Public Policy (COSPUP) from 1963 to 1966. He became a member of the American Philosophical Society in 1956 and of the American Academy of Arts and Sciences in 1967. He had memberships in many professional societies, including the American Society for Clinical Investigation and the Association of American Physicians, to which he was elected in 1937 and 1942, respectively. He was a member of the Harvey Society and served as its President in 1956, and he was a lifelong member of the American Association of Immunologists and its President in 1967. A long list of other societies includes the Royal College of Physicians and Surgeons of Canada and the Royal Society of Medicine of Great Britain. He received honorary degrees from the University of Alberta, McGill University, and Uppsala University.

Horsfall served on the editorial boards of a number of professional journals, including the *Journal of Experimental Medicine,* the *American Journal of Public Health, Virology, Excerpta Medica,* and the *Journal of Immunology.*

During his years at Sloan-Kettering, Horsfall lived on the top floor of the Hospital-Institute complex, but since he found life in city apartments to be unbearably stifling, he also maintained a home in upper Westchester County, New York, where he frequently spent long weekends. It was here that he enjoyed the pleasures of working with his hands. He was quite skillful as a carpenter and this and cultivation of the soil gave him relief from the strenuous tensions of administrative duties. It was here that he enjoyed the intimacy of family life. He is survived by his wife, Norma, who resides in Silver Spring, Maryland; a son Frank L. Horsfall III, a microbiologist and director of research in an organization concerned with environmental science; and two daughters—Susan Shahmanesh who lives in Brooklyn, New York, and Mary Sullivan who lives in Leesburg, Virginia. There are three grandchildren.

During the fall of 1970 Horsfall felt himself beginning to fail. He had decided to retire just before it was discovered that he had cancer, of which he died on February 19, 1971. His death left a void which was widely felt, not only among his colleagues and staff, but also in the entire scientific community.

It is a pleasure to acknowledge the considerable assistance of Mrs. Norma Horsfall and also of Marilyn Moor of the Memorial Sloan-Kettering Cancer Center in furnishing important information and a bibliography. I am also indebted to several previous biographers, especially an extended account by Colin McLeod, published in the *Yearbook of the American Philosophical Society* (1971, pp. 127-32). Informal short biographies by Igor Tamm, Alexander Bearn, and Chester Southam were also available. I am indebted to the *University of Washington Alumni Bulletin* for information collected at the time Dr. Horsfall received recognition as a distinguished alumnus.

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Otto Laporte

July 23, 1902-March 28, 1971

By H. R. Crane And D. M. Dennison*

Otto Laporte was a member of the small group of brilliant young theoretical physicists who received their training during the middle 1920s under the guidance of Arnold Sommerfeld in Munich.

Otto Laporte was born in Mainz, Germany. His ancestral lineage came from French Huguenot families who fled from France to Switzerland during the period of intense religious persecution in the late seventeenth century. They were later allowed by Frederick the Great to move to Prussia, where they and their descendants became, for the most part, civil servants in the Prussian State. It appears that Otto was the first member of his family to devote himself to science or any other scholarly career. His father was a career officer in the Imperial German Army, and his specialty was heavy artillery. During the years before World War I, Colonel Laporte was successively stationed in the heavily fortified cities of Mainz, Cologne, and Metz, and it was in these cities that young Otto Laporte received his early schooling.

After the war broke out, the family was evacuated from Metz and returned to Mainz, where Otto's mother's family lived. They remained there for the duration. The first four years following that move—Otto's twelfth to sixteenth—were formative ones, and

* Dr. Dennison died April 3, 1976.

his real interest in science became apparent. During that period he experimented extensively with optics and spectra, within the limitations of the meager materials that he could manage to come by in time of war. His father was able to get for him a glass prism from a periscope. Not satisfied with the dispersion of the glass prism, he proceeded to build a triangular cell of glass plates held together with glazier's putty and filled with carbon disulfide. When, in his words, "the carbon disulfide dissolved the putty, escaped and stank up our house terribly"* the method had to be abandoned. For a light source he built a satisfactory electric arc. He also built a spark coil that worked and embarked on building a much larger one; however, as he recalled, the tedium of winding the secondary overcame him, and the project was dropped. Having received an electrostatic machine from the effects of a cousin who was killed in the war, he did experiments that amazed onlookers—for example, making his sister's hair stand on end.

It surely must have appeared from this early work that if Otto Laporte was to have a career in physics, it would be in the experimental rather than the theoretical direction. This idea would have been strengthened by the fact that he was at the same time having difficulties with mathematics and English in school. This, he said later, was because the courses involved only repetitious drill and endless numerical calculations requiring interpolation of five-place logarithm tables. Since failure in these subjects would have been a blot on the family honor, he was provided with extra tutoring. Anyone knowing Laporte later in his life would be hard put to understand these early difficulties, because a command of these two skills, mathematics and languages, would stand out in a most striking way.

Perhaps one may conclude that even a bad experience in school cannot completely suppress the abilities of a gifted student. At any rate, Otto Laporte was soon to come under the influence of excellent and inspiring teachers.

* From a taped interview; see Acknowledgments.

The years immediately following the war were very difficult ones for the Laporte family, as of course they were for many others. In the early spring of 1920 the family moved to Frankfurt, to stay for only a year, where the young Laporte attended the University. It was here that he chose the branch of the road leading to theory rather than experiment. In a taped interview with Professor Thomas S. Kuhn of Princeton University, made late in his life (see acknowledgments), Laporte recalled how it came about.

> KUHN: You start out with this great interest in building things; you do a lot of experiments at home, some of them very elaborate. You go, though, to Sommerfeld, and your career is pretty much in theory. Was there a conflict here? Were you always clear that you wanted to be a theoretical physicist? How did these two sides of things relate?
> LAPORTE: I think it is due to the fact that more formal teaching goes on in theoretical physics. Experimental physics is not really being taught except on a much lower level. When I went to Frankfurt, right away I was under the influence of some very great men. The oldest one there was a mathematician named Arthur Schoenflies, whom you know, of course, as the man who first formalized the theory of the space groups of lattices, but who has many, many other great achievements. He was then an old man, but he gave *beautiful* courses. And then there were two younger mathematicians; one was Ludwig Bieberbach and the other, a small wizened man named Ernst Hellinger.
> KUHN: You went to lectures by *all of them* from the very beginning?
> LAPORTE: Yes.
> KUHN: You were at Frankfurt for only one year, yes?
> LAPORTE: Yes. And in physics I took the lectures of [Max] Born and [Alfred] Landé; Landé was then a young Privatdozent. From Born I took heat, which was what he was doing in his cycle. Now it should perhaps be said at this moment that it was a great disadvantage of the German system that you don't get any advice from appointed advisors. I just took the courses whose subjects interested me, and they were all much too difficult. Now if I had been in the clutches of some sufficiently energetic advisor he would have just told me not to take these courses, but I wasn't told that; I was completely on my own.

In the summer of 1921 the Laporte family found it necessary to leave Frankfurt. Their apartment was taken over by the German Army of the French Occupation, and the housing short age in Frankfurt became so acute that there was little chance of finding another place. The family moved to Munich. The choice of that city was greatly influenced by the facts that Arnold Sommerfeld was the professor of theoretical physics at the University of Munich and that he was establishing there one of the foremost centers of physics in Europe. Max Born had sent Sommerfeld a very enthusiastic recommendation regarding Laporte, and the young man found himself accepted and welcomed by the group of theorists who had been drawn there by the presence of Sommerfeld. He was given the initial task of talking at their seminar on a new paper by Albert Einstein and W. J. de Haas. The subject of the paper was new to Laporte, but he was helped by Wolfgang Pauli, who was Sommerfeld's personal assistant at that time. Other members of the group of young theorists were Werner Heisenberg, Gregor Wentzel, Karl Herzfeld, and Paul Ewald. This was a lively and very gifted company into which Laporte was plunged. His continuing acceptance by that group speaks well of the young Laporte's innate ability, since in point of fact his training had not really been extensive. Actually, when he arrived in Munich, he had never read Sommerfeld's famous book *Atombau und Spektrallinien,* which was the indispensable "bible" for most physicists of that era. This omission in Laporte's education was quickly repaired, however.

The central figure dominating and inspiring the group was, of course, Arnold Sommerfeld, and it is clear that Otto Laporte, who was then in his early twenties, acquired the clarity of expression and creativity which were to be his characteristics for the remainder of his life. In addition to attending Sommerfeld's lectures, Laporte had the opportunity for much personal contact with Sommerfeld, either at Sommerfeld's home or in the course of the long walks which they often took. These conversations included the classical subjects of hydrodynamics and electrodynamics as well as the developing field of quantum theory. Laporte's first independent research resulted in an article on the diffraction of electromagnetic waves around a sphere, an article which was published in the *Annalen der Physik* in 1923, just a few months before his twenty-first birthday. In this paper Laporte discussed a set of solutions that are now known as creeping waves or Regge poles.

The old, prewave mechanics version of quantum theory was at the height of its success in the early and middle 1920s. It had served to correlate, and in that sense to explain, a great many experimental results. It was, however, inherently illogical, and there was a growing distrust of the old theory and a search for what was to become the present-day quantum theory. Among the directions that the search took were those of attempting to understand the more complex atomic spectra, to classify the spectral lines, and to find the energy levels. Typical spectra of this type were those of iron, vanadium, and chromium.

At about this time Sommerfeld spent time in the United States, mainly as a visiting professor at the University of Wisconsin. He also traveled to the important spectroscopic installations—for example, the one at Mount Wilson. When he returned to Munich, he brought with him new spectroscopic datain particular, data on the spectra of iron and vanadium. Laporte set about the task of unraveling and understanding these. His analysis of the vanadium spectrum was published in December of 1923, while that of the extremely complex iron spectrum appeared the following year. This latter research formed the basis of his dissertation for the doctoral degree, which was awarded in 1924. In the course of his successful investigations of these spectra, he discovered the fundamental principle known among spectroscopists as the "Laporte rule." This rule classified the atomic energy states into two types, which he had called odd and even states. He found that no radiative transitions occurred between unlike states. This was the first statement of the principle that was later to be known as the conservation of parity. The principle has assumed fundamental importance in elementary particle theory, not only because of its almost universal validity, but also because of the great interest attached in recent years to certain exceptions.

The year 1924 was momentous for Otto Laporte in several respects. In addition to his completion of the analysis of the iron spectrum and the acceptance of his Ph.D. thesis, he was awarded one of the first of the International Education Board fellowships. These had been newly established by the Rockefeller Foundation for the purpose of allowing young post-doctoral scientists to continue their research anywhere in the world. Professor Sommerfeld had learned, while on his visit to the United States the year before, that these fellowships were about to be set up, and he highly recommended his student, Laporte, for one of them.

Laporte elected to spend the period of his fellowship, 1924-1926, in Washington, D.C. at the National Bureau of Standards. He was drawn there by the excellence of their spectroscopic laboratory and in particular by the presence of the very able experimental spectroscopist William F. Meggers. The advantages of the association were mutual: Meggers and the other experimentalists had been anxious to hear firsthand reporting of the theoretical advances that were being made in Europe in the fields relating to spectroscopy. In response to this need, Laporte was instrumental in starting a regular weekly seminar. This grew rapidly to include many of the scientists of the Washington area, and it continued to meet throughout Laporte's stay at the Bureau. Among persons included were Gregory Breit, Merle Tuve, Paul D. Foote, F. L. Mohler, Arthur Ruark, and Harold Urey. Most notable in the seminar series was Laporte's interpretation of the first three papers of Schrödinger, which introduced the matrix method in quantum theory. These talks, Laporte recalled later, required a great deal of work on his part. That period was a yeasty one in the Washington area, and it was remembered so fondly by the seminar participants that they staged a reunion twenty-five years later. In Laporte's words, "We felt sheepish as well as reminiscent. We gave another colloquium."*

The two years spent in Washington were formative ones as well as productive ones for Laporte. He published a number of spectroscopic researches. But perhaps a more important aspect for his future was his close association with experimentalists for the first time in his life. In spite of his early interest in experiments he had been entirely under the influence of theorists since the age of about eighteen. Here at the Bureau he became greatly impressed with the power of experiment, and this must have had much to do with the fact that he turned to experimental research in the last chapter of his career.

In the autumn of 1926 Laporte joined the staff of the Physics Department of the University of Michigan as an instructor. In the following year he became an assistant professor. By inviting Laporte, Professor Harrison M. Randall, the department chairman, wished to build up the field of theoretical physics at Michigan. He realized, however, that it would be necessary to create a nucleus of theorists if the venture was to be successful. Accordingly, in 1927 Laporte was joined by George E. Uhlenbeck, Samuel A. Goudsmit, and David M. Dennison, who remained his colleagues for many years.

In 1928 Laporte was invited to be a guest lecturer at the Imperial University in Kyoto, Japan. During the year he received an urgent request from Sommerfeld asking him to lecture in Munich during the period in which Sommerfeld was to be absent on a trip to America. Laporte was able to honor this request, although it meant cutting his visit to Japan somewhat short. It also meant that he had to make a nonstop journey of two weeks duration via the trans-Siberian railway in order to arrive in Munich on time, an adventure he did not soon forget.

* From a taped interview; see Acknowledgments.

His initial visit to Japan made a lifelong impression on Otto Laporte and led to his taking leaves of absence from the University of Michigan to lecture at the University of Tokyo in 1933 and again in 1937. During these periods he became proficient in the Japanese language and in the art and literature of Japan. He was truly an expert in the understanding and appreciation of Japanese poetry. While there, he submitted a haiku in a national competition and won recognition. In the interval between these visits, in 1935, Laporte became a naturalized United States citizen.

Otto Laporte's expertise in both the language and culture of Japan was to be of great value to this country from 1954 to 1955 and again from 1961 to 1963, when he served as scientific advisor to the American ambassador in Tokyo. The U.S. State Department cited Laporte for playing a key role in securing the landmark atomic energy agreement between the two countries. These were not the only occasions in which Laporte had served his adopted country with distinction. From 1949 to 1950 he was intelligence analyst for the U.S. Army of Occupation in Heidelberg, Germany.

A review of Laporte's bibliography shows that up until the early 1940s his principal researches continued to be centered in the field of spectroscopy, although a few of his publications related to various quantum mechanical problems, making use of his great knowledge of mathematics. In 1944 he began, in effect, a new career. He entered the field of fluid dynamics with a paper in which he found an exact solution to the problem of the lift of an airfoil of elliptical outline. Two years later he was presented with the unexpected opportunity to do experiments in fluid dynamics by means of a shock tube. Lincoln Smith, a member of the Michigan faculty, had somewhat earlier initiated work in this field and had assembled apparatus of an advanced design. Smith left the University in 1946, and Laporte took over the directorship of the project. This was a really new departure from his earlier work, since it meant taking responsibility for an experimental project and directing the graduate students who had started their theses under Smith. He did exceedingly well at it, and many successful investigations resulted. These included the use of reflected shocks to produce such high local temperatures that spectroscopic phenomena which had hitherto been inaccessible could be studied. He continually had graduate students in the shock tube project. His last doctoral student, John Yoder (Ph.D., 1971), constructed a cryogenic tube and investigated shocks in low-temperature hydrogen gas. The resulting measurements clearly showed those quantum effects that arise from the existence of the two forms of hydrogen, ortho and para.

In some respects the final chapter of Laporte's scientific life, which we have called a new career, was not really new but rather the convergence of a number of the interests he had had for a long time. The success with which he directed the experimental part of the shock tube program perhaps harks back to his boyhood project of constructing an arc and a workable spark coil. His interest in the hydrodynamical problems associated with the shock phenomena was surely related to the early days in Munich, where many of the discussions were centered about hydrodynamics. For example, his friend Werner Heisenberg wrote his Ph.D. thesis in that field. And finally, of course, Laporte's long experience with spectroscopic problems allowed him to recognize the possibilities of using the shock tube for observing spectra under conditions that had not been accessible before.

Otto Laporte was one of the charter members of the American Physical Society's Division of Fluid Dynamics and served as its chairman in 1965. He exerted strong influence in directing the attention of physicists to an area of study that was to be, for many of them, new and highly rewarding. Soon after his death the Division of Fluid Dynamics established, in recognition not only of his scholarship but also of his early guidance of the organization, the Otto Laporte Memorial Lectureship, to be given annu ally. Richard G. Fowler gave the first of the lectures in 1972. Fowler has published a paper* based on his lecture that is rich in detail on Laporte's contribution to the Division as well as on facets from the history of his career.

Special recognition should be made of Otto Laporte's graduate teaching. His early association with European lecturers in theoretical physics in an era when a certain elegant formality was practiced came through in Otto's teaching in a desirable way, but at the same time his lectures were not so structured that he could not pursue byways when it suited him to do so. What came through most strongly to the better students was his deep appreciation for the beauty of physics and of mathematical proofs. In the opinions of these select students the view of physics that they gained from him was of more future influence than the physics itself. Laporte's sense of mathematical discipline did much to maintain the "tone" of teaching in the whole graduate program, and his course was both a high point and a hurdle in each graduate student's career.

Almost by definition most people are amateurs in the areas of their hobbies or side interests. Not so with Otto Laporte. He had several hobbies, and he went into each with almost the intensity and erudition he gave to physics. His mastery of the Japanese language and knowledge of the culture that he gained during several years on assignments to Japan have been mentioned. He continued to cultivate these interests for the rest of his life; for example, he became an authority on netsuke, small carved figurines, of which he had a notable collection.

Otto Laporte was a horticulturist of much more than amateur standing, specializing in plants of the cactus and the euphorbia families and certain other succulents. His collection of these plants was extensive, particularly in respect to the euphorbias of the rare medusa form. He kept up correspondence with horticul

* *Phys. Today,* 26, no. 11 (1973):23-29.

turists in other parts of the world in his efforts to obtain unusual specimens. He published on euphorbias and cacti. One of the present writers (H.R.C.) once traveled with Laporte to New York to attend a physics meeting, where immediately upon arrival the two went to a botanical garden on Long Island. A botanist from South Africa was waiting there, by Laporte's prior arrangement, to talk about the Welwitschia plant, a plant that grows only in the Nambi Desert and in the south of Angola and that is estimated to live more than 2000 years. Laporte came away with some seeds as well as the information he sought. An everyday kind of incident was recalled by a former student: the student, while waiting for Laporte's class to begin, was gazing out of the window at a row of Lombardy poplars. Before he was aware of it, Laporte had come up behind him and was saying, "You know, all the Lombardy poplars in the world are one single tree—they propagate only by cuttings because they are males and the female is no longer extant."

Music was the third of Laporte's side interests. His own piano playing was passable but venturesome, and was indulged in for his own enjoyment. But his real expertise was in his wide knowledge of the literature of music and his wide acquaintance with professional musicians in this country and abroad. He was quite at home discussing the fine points of musical composition and performance with the best of experts.

Laporte was married to Eleanor Anders in 1933. She died in 1957. He remarried in 1959. His second wife, the former Adele Pond, and their three children, Claire, Irene, and Marianne, survive him.

Otto Laporte died March 28, 1971, a victim of cancer which progressed rather rapidly. His death came after his name had been slated for presentation for election to the National Academy of Sciences at the annual meeting in April of the same year. Taking an action it had never before taken, the Academy elected Otto Laporte to membership, posthumously.

ACKNOWLEDGMENTS

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CHRONOLOGY

1902 Born July 23, Mainz, Germany. Parents: Wilhelm Laporte and Anna Geyl. Sisters: Luise and Marianne (m. Roger Gillette).

1923 Analyzed iron and vanadium spectra. Enunciated Laporte rule.

1924 Ph.D., University of Munich, under Arnold Sommerfeld. Thesis: "Exact Treatment of Scattering of EM Waves by a Sphere."

1924-1926 International Education Fellow; studied in Europe, Japan, and finally in the United States in the spectroscopy section of the National Bureau of Standards.

1926 Joined University of Michigan faculty as instructor in physics.

1927 Promoted to assistant professor.

1928 On leave from University of Michigan; lectured at Kyoto Imperial University.

1933 Married Eleanor Anders (d. 1957).

1933 On leave from University of Michigan; lectured at Tokyo University.

1935 Became naturalized U.S. citizen.

1937 On leave from University of Michigan; lectured at Tokyo University.

1944 Changed field of activity from atomic spectroscopy to fluid mechanics.

1945 Promoted to full professor.

1946 Assumed charge of shock tube laboratory at University of Michigan.

1949-1950 Scientific Intelligence Analyst, U.S. Army of Occupation, Heidelberg.

1951 Initiated researches on reflected shock waves.

1954-1955 Served as scientific attaché at American Embassy in Tokyo.

1956 Cited by U.S. State Department as instrumental in securing atomic energy agreement between the United States and Japan.

1959 Marriage (second), to Adele Pond.

1961-1963 Returned to Tokyo as scientific advisor to the U.S. ambassador.

1965 Chairman, Division of Fluid Dynamics, American Physical Society.

1968 Sabbatical leave in Munich.

1971 Died March 28, Ann Arbor, Michigan.

1971 Elected, posthumously, to the National Academy of Sciences.

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1954

Philip Duryeé McMaster

September 14, 1891-March 20, 1973

By Walther F. Goebel

My first encounter with Philip McMaster was in the locker room of the squash court at the Rockefeller Institute more than fifty years ago. I had come to New York but a few weeks before as a research assistant in the laboratories of Dr. Oswald T. Avery. I was in search of exercise that day, and how well I remember the sound of the squash ball on the wooden walls of the court as I entered the locker room, and the shouting and cursing which issued from the tiny balcony above the court itself. Then, suddenly, Phil and his opponent emerged panting and laughing. He extended his hand to me and introduced himself. My first impression was of a man small in stature, witty, and cordial: something unique for me, for I had as yet made few friends in this new and overpowering metropolis. Friends we became, and we remained so from that day forth.

Philip McMaster was born at Chestnut Hill in Philadelphia on the fourteenth of September 1891. His father was John Bache McMaster, a distinguished historian who headed the Department of History at the University of Pennsylvania and a scholar still identified as the author of *History of the People of the United States.* His mother was Gertrude Stevenson of Morristown, New Jersey. There was always a constant flow of professors through the McMaster household, and because of this, Phil's academic background was assured. In this environment he was to remain throughout his long and productive life.

His early education in Philadelphia was obtained in private schools, and upon his graduation he entered Princeton University, from which he graduated in 1914. During his summers, as a boy, he accompanied his family to Kennebunkport, Maine, and to Cape Cod, where he developed his love of the outdoors and of the sea as well. These were hobbies which never left him. His early taste for biology undoubtedly arose when he accompanied his father and Professor Edward Conklin on frequent field trips to collect biological specimens. Conklin was professor of biology at the University of Pennsylvania and spent his summers at the Marine Biological Laboratory in Woods Hole. These forays were always made on bicycles, and the treasures which they collected were returned to the laboratory, where they were subjected to exhaustive scrutiny by the professor. Philip's summers in Maine were filled with water sports of every description. At the age of nine he was given his first sailboat, a craft some fifteen feet long, of which he soon became master.

After Philip graduated from Princeton in 1914 with the degree of bachelor of science, he entered the medical school of the University of Pennsylvania, where he was graduated the year World War I ended. Fortunately, McMaster was not devoted totally to scholarship during his college years, for as an undergraduate, not long after he entered Princeton, he was made coxswain of the freshmen crew—a happy choice, for he and water, both fresh and salt, remained inseparable during his entire life.

I am fortunate to have in my hand an account of his life and his scientific achievements ("Dr. Philip D. McMaster: His Work and Its Significance," unpublished), which he himself wrote for Dr. Herbert Gasser, the second director of the Rockefeller Institute. Phil was the younger of two sons, the elder of whom died of pernicious anemia at the age of twenty-five. But Phil was to live beyond the allotted biblical span to pursue his distinguished scientific career, all of it at our Institute. He him self says that during his boyhood there was but little evidence of a scholarly attitude on his part, despite the academic atmosphere in which he was raised. Personally, I doubt this.

Philip served as resident physician at the medical school hospital during the last year of his medical training at the University of Pennsylvania and again during the year following his graduation from medical school. He was commissioned as a first lieutenant in the U.S. Army shortly before the war was over.

At the termination of the war and in the autumn of 1919, he came to the Rockefeller Institute as a research associate in the laboratory of Dr. Francis Peyton Rous, where he embarked upon his first investigative work. In the ensuing pages I shall attempt a résumé of his more important contributions.

During his first three years at the Institute, McMaster collaborated intimately with Rous in their research. Among their achievements was the devising of methods for the intubation and sterile drainage of the gallbladders and bile ducts of a variety of animal species, a technique which permitted them to collect bile from the individual liver lobes of a number of different experimental animals and thus enabled them to compare them. These studies revealed that the normal gallbladder rapidly concentrates hepatic bile but that the diseased organ fails in this function. These observations were promptly utilized by clinical surgeons as a basis for diagnostic dye tests for the presence of gallstones and gallbladder disease. The basis for the test lay in the fact that in the normal organ the concentration of x-ray opaque dyes, when injected into the bloodstream, is excreted in the bile. This is not the case in the diseased gallbladder.

As a result of these studies it became important to learn whether the liver bile of animals lacking a gallbladder is excreted in a form more concentrated than that of those species possessing the organ. To test this, the pigment content of bile—an index of its concentration—was compared in two closely related species, namely, the mouse, which has a gallbladder, and the rat, which does not. Thus, the bile of rats was found to be several times more concentrated than was the bile of mice when collected by semimicro methods from individual lobes of the animals' livers. Yet when the bile of the mouse was collected from the common duct after the secretion had been acted upon by the gallbladder, the pigment content was several times greater than that of the liver bile. These studies and others on bile secretion, which extended over a period of several years, led to the development of wholly new techniques and threw new light upon the effects of diet inanition, exercise, and liver derangement on bile secretion. Their studies yielded, furthermore, convincing evidence that bilirubin had no other source than hemoglobin.

McMaster's three years of association with Dr. Peyton Rous served well as a period of initiation for the work upon which he was next to embark with Dr. Robert Elman, namely, the pathology and physiology of urobilin. This was a problem much disputed at the time and was one of considerable importance to the understanding of the mechanism of the pigment changes in certain liver derangements, including pernicious anemia. The two men developed a procedure for collecting sterile bile, a technique which allowed them to collect the secretions either from the whole liver or from a part. Prior to this experiment, it had been thought that only the damaged liver formed urobilin, an assumption that arose as a result of finding some animals bearing infected bile fistulas. Yet, when it became possible to obtain bile which remained sterile by using their intubation technique, the complete loss of the secretion from the body resulted in the total disappearance of urobilin and urobilinogen from the bile, feces, and urine. This occurred when the animals were subjected to severe liver damage, biliary obstruction, or blood destruction. On the other hand, all of these led to severe urobilinuria, a condition in which bile was permitted to reach the intestines. Thus it became clear that urobilin could not be formed by the damaged liver. In brief, these experiments settled once and for all the question of the origin of urobilinuria. Thus urobilin in the urine depended first on its absorption from the intestine, or the infected biliary tract, and next upon the failure of the liver cells to remove pigment. Their findings were subsequently confirmed by other investigators.

It was with Dr. Douglas Drury, whom I had met many years before on the first day with McMaster in the squash court locker room, that McMaster developed the technique for the partial or total removal of the liver of experimental animals. These two men were among the first to perform hepatectomies successfully enough that the animals survived sufficiently long to enable the investigators to study the effect of liver deprivation or insufficiency upon carbohydrate and fat metabolism. This work on hepatectomized rabbits revealed that the liver was the source of blood fibrinogen.

Shortly after the completion of this work, McMaster spent a sabbatical year at Harvard University, where he worked with Dr. Harry Murray in the field of psychology. Upon his return to the Rockefeller Institute he again joined Rous and his colleague Hudack in a study of the fluid interchange between the smallest blood vessels and tissues. They found that by injecting varicolored dyes in the bloodstream of rabbits and mice, they could observe the pattern and spread of the dye's passage through the various vessel walls and changes in vascular permeability resulting from a variety of physiological and pathological conditions such as light, trauma, heat, or cold. These experiments, to be described subsequently, were, I think, some of his finest.

McMaster next turned to a study of the lymphatic system and the mechanisms of lymph flow, an investigation in which Dr. Hudack again collaborated. The two found that when bright blue vital dyes were injected into the skin of animals superficially, and more particularly into the ears of mice, they rendered the minute lymphatics visible. These experiments pro vided an entirely new concept of the activities of the lymphatic system.

To me, these numbered among McMaster's most interesting and rewarding experiments. How well I remember his enthusiasm for the work as it was being executed, perhaps some four decades ago, not only on experimental animals, but upon himself and other normal human subjects as well. These experiments proved that instead of being passive drainage canals, the lymphatics were very active in the process of fluid exchange. Their walls respond rapidly to various influences such as sunlight warmth or a stroke that does not break the skin. For the first time, too, these investigators were able to render the lymphatic capillaries visible in the skin of man. When a blue dye was injected into the skin of the legs or arms, it was taken up into torn lymphatics and rendered them visible. The color appeared later as blue streamers in the draining lymphatic trunks running up the limbs.

In collaboration with Dr. Robert Parsons there followed many experiments to determine the influence of the factors responsible for the movement of peripheral lymph. Both the pulsation of blood vessels as well as the mechanical effects of muscular contractions proved to be of importance. The two devised ingenious methods for measuring the pressures which existed within cutaneous lymphatic capillaries and in the interstitial tissues outside of them, both under normal conditions and in edematous states. They found that a gradient pressure exists between the two tissues and lymph in the capillaries sufficient to account for the flow of lymph in motionless skin. The structural conditions in the interstitial tissues of the skin revealed that fluids move along fibrillae and fibers and that open tissue spaces, filled with free fluid, such as seems to be present in sections of fixed dehydrated tissues, probably do not exist in living intradermal tissue. Instead, a gelatinous ground substance is apparently present between the formed elements.

They next turned their attention to the flow of lymph under pathological conditions. By injecting dyes into the cutaneous lymphatics of edematous humans, they found differences of flow in various forms of edema. Thus, in patients with cardiac insufficiency and severe edema of the legs, the cutaneous lymphatics were more dilated than in those suffering from nephrotic edema or in normal individuals. On the other hand, patients who suffered from nephrotic edema, yet who had good cardiac action, exhibited a flow of lymph more rapid than in normal humans. It should be added that in the first group of patients, those with cardiac insufficiency, it appeared that the lymphatic valves no longer functioned and the patients suffered from valvular incompetence of the lymphatic vessels, allowing stagnation of lymph in peripheral areas and even permitting retrograde flow when slight pressure on the skin was made with the finger moving toward the foot, a fact which was not true in normal individuals.

These experiments of McMaster and his younger colleagues had far-reaching consequences. Other investigators employed his techniques to study cancer patients in order to trace the lymph drainage from the diseased areas and render the lymph nodes blue, hence visible, in regions where metastases could occur through the lymph stream. McMaster's studies brought ample evidence that injury to the skin, however superficial, invariably involved the lymphatics and that local intradermal injections were, in reality, a general injection because of rapid lymphatic distribution. Every injury that breaks the continuity of the skin permits bacteria, viruses, and other foreign matter to enter the lymphatics, and because of this drainage it is not improbable that the regional lymph nodes play an important role in the defense of the body against invasion by an infectious agent.

There now followed important experiments, first with Dr. Stephen Hudack and later with Dr. John Kidd, in which it was conclusively demonstrated that lymph nodes, draining skin sites injected either with bacteria or viruses, formed antibodies against these agents in very high concentrations. Confirmation of this important discovery was made in many other laboratories. These studies were extended to ascertain the type of cells within the lymph nodes themselves and within the spleen which might be responsible for antibody formation. The clear-cut proof which McMaster's experiments presented led to a resurgency of interest in the activities of lymphocytes.

With the entrance of our country into World War II, McMaster became completely involved in wartime activities, first with the director of the Rockefeller Institute—Dr. Herbert Gasser—and Dr. René Dubos, then later with Dr. George Hogeboom. This work was done under the auspices of the National Research Council and the Office of Scientific Research and Development and consisted of research itself as well as consultation with the army and navy in an effort to devise tests for war gases and prophylactic ointments against vesicants and treatment for vesicant burns. This work consumed nearly five years of McMaster's fruitful life, and knowing him as I did, and knowing his love of fundamental research, I am not wrong when I say that during this interval there were times when he was truly frustrated.

The war ended. Once again McMaster was free to return to his research and the important problems concerned with antibody formation. In this work he made use of azoproteins colored intensely blue to study their escape through the vessel walls and to use them as tracers in order to learn about their storage and localization during antibody formation. He observed that mice previously injected and then injected a second time some weeks later suffered intense anaphylactic shock. When anesthetized animals were placed in plasteline molds with their ears spread out on a white porcelain plaque, the vascular changes and the accompanying changes in blood flow could readily be observed in the unmolested tissue of the ear. An extraordinary local and general constriction and dilation of both arterial and venous vessels occurred, yet the capillaries showed no apparent reaction. This technique using these intensely colored antigens proved extraordinarily sensitive and enabled him to follow their fate in the mammalian body. Thus, their injection into the bloodstream of mice revealed that the antigens were taken up both by cells of the reticuloendothelial systems throughout the body, especially the Kupffer cells of the liver, and by macrophages and reticular cells of the spleen and the lymph nodes. In this manner they revealed certain of the sites from which the first stimuli to antibody formation arose.

These early studies on the sites of antibody formation in mice were considerably extended by McMaster when he next employed a much more highly diffusible blue azoprotein complex. When the latter was injected into the animal the complex was eliminated from the body with speed, in approximately two hours, and it was impossible to see any residual granules in the cells. Nevertheless, minute amounts of blue material, whether complete antigen or not, persisted in certain tissues of the animals. In order to understand more fully the mechanism of antibody formation, McMaster had to determine whether or not this was intact antigen or whether it was the chromophoric group which split off from the carrier protein. Without entering into detail, let me say that by means of very sensitive passive anaphylaxis experiments McMaster showed that the antigenic material itself and not the chromophoric group of the antigen persisted in very small amounts in the tissues of the experimental animals. Despite these findings, it still remained possible that the persistence of the protein antigens in donor mice might exist because the animals formed antibodies very poorly; hence the antigen might indeed persist because of a lack of antibody to destroy it. The experiments were therefore repeated in rabbits, animals which are well known to be excellent antibody producers. In these experiments, McMaster employed bovine gamma globulin as the antigen. The results were in essence the same.

By following the fate of the tracer antigen, McMaster and his co-workers next attempted to study the mechanism of antibody formation under various conditions. Since the first step in the formation of an antibody appears to be the capture of the antigen by phagocytic cells or even other cellular types, it seemed likely that something might be learned by observing the fate of the tracer antigen in mice which had been stimulated to form antibodies but prevented from doing so by large doses of cortisone. Although the drug reduced the size of the lymph nodes and spleen by some ninety percent, it was observed that the shriveled organs took up as much of the tracer as did organs of normal mice, thus demonstrating unequivocally that the inhibition of antibody formation did not result from the faulty uptake of antigen. Nor did it result from an impairment of the antigenicity of the antigen or from a more rapid destruction than that which occurs in animals given no cortisone. Mice injected with foreign protein were prevented from forming antibodies by the administration of cortisone for nearly two weeks, yet upon withdrawal of the drug, antibodies promptly appeared. Clearly, the antigen had remained in the organs of the mice in a form capable of engendering antibodies.

Cortisone did not inhibit antibody formation by interfering with the storage or distribution of the tracer antigen or by destroying it. The phagocytic cells, even under the influence of cortisone, continued to localize and store the tracer antigen. The undisturbed function of these cells suggests that they do not form antibodies, although they partake of the first step in antibody formation by capturing and holding the antigen. It is the cells of the lymphoid series, though greatly injured and reduced in number by cortisone, which appear to be the units which form the antibody.

At about this time there appeared accounts in the literature that certain bacterial endotoxins enhanced antibody formation, stimulating McMaster and his capable assistant Dr. Robert Franzl to examine the cellular changes accompanying the presumed enhancement. These collaborative studies of Franzl and McMaster extended well over a decade, from 1956, when Franzl first came to the Rockefeller Institute, until and beyond McMaster's death. As Franzl has said in a communication to this writer, "our task was to clarify some of the early mechanisms involved in antibody formation *in vivo,* more specifically those pertaining to the cellular uptake and subsequent fate of antigens and their presumed role in the induction of immune processes."

Sheep red blood cells were used as the antigen because of the exquisite quantitative method available for measuring the hemolysin formed. Upon the introduction of *S. typhosa* endotoxin into mice, together with the antigen via the same route, the two did indeed note a greatly increased antibody formation. Yet, surprisingly, antibody formation failed to appear if the endotoxin was administered prior to the introduction of the antigen.

Since the endotoxin, administered alone or administered before the antigen was given, produced a marked depletion of the lymphoidal elements in the spleen and mesenteric nodes of the mice, it presented an excellent means of preserving the type of cells which appeared in the relatively empty lymphoidal tissue both during the enhancement as well as the suppression of antibody formation. Thus, when large amounts of antibody appeared, all of the known cellular changes usually accompanying or preceding this in normal animals were enhanced. The proliferation of extraordinarily large numbers of large pyroninophilic cells was considered to be responsible for the early formation of antibodies. Activated germinal centers appeared in much greater numbers than are usually seen in mice forming antibodies to simple injection of antigen without endotoxin. These changes were all absent in the mesenteric nodes and spleens of control mice that formed no antibody. These findings and those reported in the experiments with cortisone make it seem probable that the suppression of antibody formation does not depend so much upon the fate of the antigen as upon the proliferation and development of lymphoidal elements in the injected mice.

In foregoing account, I have attempted to survey rather broadly the achievements of Dr. McMaster during his many active years at the Rockefeller Institute, now the Rockefeller University. To be sure, this attempt has been brief, and if the reader wishes to learn in greater detail McMaster's scientific objectives and goals, he must of course refer to the scores of scientific contributions which appeared in various journals over the decades and were in great demand by scientists throughout the world, a demand evidenced by massive requests for reprints. A perusal of this bibliography impresses one with the diverse spheres of interest which McMaster had and which expanded over his many fruitful scientific years. One need but read a few of the accounts to appreciate how meticulously and precisely he planned and pursued his work and to gain a broad insight into the mind of a truly splendid investigator.

McMaster held memberships in many scientific societies and in social clubs as well. The two he cherished most were the Century Club in New York City and the National Academy of Sciences (elected 1952). He is survived by his two children, Abigail P. (Mrs. Charles) Alling and Dr. Philip Bache McMaster, and by his charming widow, Elizabeth.

Now a different and equally pleasant task falls to my lot, to give you a picture of Dr. McMaster as a human being away from the laboratory, away from the animals and the microscope with which he worked, away from the stained sections over which he poured in reaching many of his important conclusions. This is not easy, for one usually sees his colleagues in a perfunctory manner; yet I knew him well.

First of all, Phil was forever a jovial person, witty and sharp in his judgment of people and of situations. On one occasion, however, I recall when his judgment was not good. It was when he asked me and two of my colleagues to sail his yacht from Huntington, Long Island to Woods Hole. Only one of us, Tom Hughes, who was then associated with the Rockefeller Foundation, knew anything at all about sailing and navigation. But under Hughes' short and forceful tutoring we made it, by good navigation, luck, and some dead reckoning, for we were engulfed in a heavy fog the entire distance.

His widow, Elizabeth, whom I also know well, has permitted me to read several personal letters which she received over the past fifty years from her husband, and from these I have gotten certain impressions that I otherwise would not have had, despite the fact that we were friends of many years standing.

At home, he was a charming host. He frequently entertained guests with his violin or his accordion, both of which he played with equal facility. His dress was at times a bit bizarre, for I saw him frequently on his way to the Rockefeller Institute, particularly in a snowstorm, dressed in the garb of a Maine woodsman, unconventional, to say the least, in the city of New York. His relationship to his wife was one of deepest affection, as attested to by two very personal letters which Elizabeth permitted me to read. They were written during World War II, when he was stationed in Florida. These letters also served to express his opinion of the shortcomings of military red tape. I have at hand, too, several letters written to Mrs. McMaster, in which the writers refer to Dr. McMaster and his work. I wish to quote from two of these, for they are from men of distinction in their own right. Dr. Merrill Chase of our Institute, in a letter to Elizabeth, says, "I look back over the years to Phil's exciting work in tracing the lymphatics of the human skin and in animals too. He was a highly ingenious research worker and I learned much from him." Then again in a telegram of condolence, James and Margaret German say, "We send love to you and our deepest sympathy. We will miss Phil because he was one of those few who showed that it was good to be human and alive."

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Maria Goeppert Mayer

June 28, 1906-February 20, 1972

By Robert G. Sachs

When in 1963 she received the Nobel Prize in Physics, Maria Goeppert Mayer was the second woman in history to win that prize—the first being Marie Curie, who had received it sixty years earlier—and she was the third woman in history to receive the Nobel Prize in a science category. This accomplishment had its beginnings in her early exposure to an intense atmosphere of science, both at home and in the surrounding university community, a community providing her with the opportunity to follow her inclinations and to develop her remarkable talents under the guidance of the great teachers and scholars of mathematics and physics. Throughout her full and gracious life, her science continued to be the theme about which her activities were centered, and it culminated in her major contribution to the understanding of the structure of the atomic nucleus, the spin-orbit coupling shell model of nuclei.

Maria Goeppert was born on June 28, 1906, in Kattowiz, Upper Silesia (then in Germany), the only child of Friedrich Goeppert and his wife, Maria née Wolff. In 1910 the family moved to Göttingen, where Friedrich Goeppert became Professor of Pediatrics. Maria spent most of her life there until marriage.

On January 19, 1930, she married Joseph E. Mayer, a chemist (elected to the National Academy of Sciences in 1946), and they had two children: Maria Ann, now Maria Mayer Wentzel, and Peter Conrad. Maria Goeppert Mayer became a citizen of the United States in 1933. She died on February 20, 1972.

Both her father's academic status and his location (Göttingen) had a profound influence on her life and career. She was especially proud of being the seventh straight generation of university professors on her father's side. Her father's personal influence on her was great. She is quoted as having said that her father was more interesting than her mother, "He was after all a scientist."* She was said to have been told by her father that she should not grow up to be a woman, meaning a housewife, and therefore decided; "I wasn't going to be *just* a woman."†

The move to Göttingen came to dominate the whole structure of her education, as might be expected. Georgia Augusta University, better known simply as "Göttingen," was at the height of its prestige, especially in the fields of mathematics and physics during the period when she was growing up. She was surrounded by the great names of mathematics and physics. David Hilbert was an immediate neighbor and friend of the family. Max Born came to Göttingen in 1921 and James Franck followed soon after; both were close friends of the Goeppert family. Richard Courant, Hermann Weyl, Gustav Herglotz, and Edmund Landau were professors of mathematics.

The presence of these giants of mathematics and physics naturally attracted the most promising young scholars to the institution. Through the years, Maria Goeppert came to meet and know Arthur Holly Compton, Max Delbrueck, Paul A. M. Dirac, Enrico Fermi, Werner Heisenberg, John von Neumann, J. Robert Oppenheimer, Wolfgang Pauli, Linus Pauling, Leo Szilard, Edward Teller, and Victor Weisskopf. It was the oppor

* Joan Dash, *A Life of One's Own* (New York: Harper and Row, 1973), p. 231.
† Ibid.

tunity to work with James Franck that led to Joseph Mayer's coming to Göttingen and gave him the chance to meet and marry her.

Maria Goeppert was attracted to mathematics very early and planned to prepare for the University, but there was no public institution in Göttingen serving to prepare girls for this purpose. Therefore, in 1921 she left the public elementary school to enter the Frauenstudium, a small private school run by sufragettes to prepare those few girls who wanted to seek admission to the University for the required examination. The school closed its doors before the full three-year program was completed, but she decided to take the University entrance examination promptly in spite of her truncated formal preparation. She passed the examination and was admitted to the University in the spring of 1924 as a student of mathematics. Except for one term spent at Cambridge University, England, her entire career as a university student was completed at Göttingen.

In 1924 she was invited by Max Born to join his physics seminar, with the result that her interests started to shift from mathematics to physics. It was just at this time that the great developments in quantum mechanics were taking place, with Göttingen as one of the principal centers; in fact, Göttingen might have been described as a "cauldron of quantum mechanics" at that time; and in that environment Maria Goeppert was molded as a physicist.

As a student of Max Born, a theoretical physicist with a strong foundation in mathematics, she was well trained in the mathematical concepts required to understand quantum mechanics. This and her mathematics education gave her early style of research a strong mathematical flavor. Yet the influence of James Franck's nonmathematical approach to physics certainly became apparent later. In fact, a reading of her thesis reveals that Franck already had an influence at that stage of her work.

She completed her thesis and received her doctorate in 1930.

The thesis was devoted to the theoretical treatment of double photon processes. It was described many years later by Eugene Wigner as a "masterpiece of clarity and concreteness." Although at the time it was written the possibility of comparing its theoretical results with those of an experiment seemed remote, if not impossible, double photon phenomena became a matter of considerable experimental interest many years later, both in nuclear physics and in astrophysics. Now, as the result of the development of lasers and nonlinear optics, these phenomena are of even greater experimental interest.

After receiving her degree, she married and moved to Baltimore, Maryland, where her husband, Joseph Mayer, took up an appointment in the Chemistry Department of Johns Hopkins University. Opportunities for her to obtain a normal professional appointment at that time, which was at the height of the Depression, were extremely limited. Nepotism rules were particularly stringent then and prevented her from being considered for a regular appointment at Hopkins; nevertheless, members of the Physics Department were able to arrange for a very modest assistantship, which gave her access to the University facilities, provided her with a place to work in the Physics Building, and encouraged her to participate in the scientific activities of the University. In the later years of this appointment, she also had the opportunity to present some lecture courses for graduate students.

At the time, the attitude in the Physics Department toward theoretical physics gave it little weight as compared to experimental research; however, the department included one outstanding theorist, Karl Herzfeld, who carried the burden of teaching all of the theoretical graduate courses. Herzfeld was an expert in classical theory, especially kinetic theory and thermodynamics, and he had a particular interest in what has come to be known as chemical physics. This was also Joseph Mayer's primary field of interest, and under his and Herzfeld's guidance and influence Maria Mayer became actively involved in this field, thereby deepening and broadening her knowledge of physics.

However, she did not limit herself to this one field but took advantage of the various talents existing in the Johns Hopkins department, even going so far as to spend a brief period working with R. W. Wood, the dean of the Johns Hopkins experimentalists. Another member of the department with whom she had a substantial common interest was Gerhard Dieke. The Mathematics Department, which was quite active at that time, included Francis Murnaghan and Aurel Wintner, with whom she developed particularly close connections. However, the two members of the Johns Hopkins faculty who had the greatest influence were her husband and Herzfeld. Not only did she write a number of papers with Herzfeld in her early years there, but also they became close, lifelong friends.

The rapid development of quantum mechanics was having a profound effect in the field of chemical physics in which she had become involved, and the resulting richness and breadth of theoretical chemical physics was so great as to appear to have no bounds. She was in a particularly good position to take advantage of this situation, since no one at Johns Hopkins had a background in quantum mechanics comparable to hers. In particular, she became involved in pioneering work on the structure of organic compounds with a student of Herzfeld's, Alfred Sklar; and in that work she applied her special mathematical background, using the methods of group theory and matrix mechanics.

During the early years in Baltimore, she spent the summers of 1931, 1932, and 1933 back in Göttingen, where she worked with her former teacher, Max Born. In the first of those summers she completed with him their article in the *Handbuch der Physik,* "Dynamische Gittertheorie der Kristalle." In 1935 she published her important paper on double beta-decay, representing a direct application of techniques she had used for her thesis, but in an entirely different context.

Later, James Franck joined the faculty at Johns Hopkins and renewed his close personal relationship with the Mayers. Also in that later period, Edward Teller became a member of the faculty of George Washington University, in nearby Washington, D.C., and she looked to him for guidance in the developing frontiers of theoretical physics. At about the same time, she became deeply involved in a collaboration with Joseph Mayer in writing the book *Statistical Mechanics,* published in 1940.

When as her first bona fide student I turned to her for guidance in choosing a research problem, nuclear physics was on the rise; and she told me that that was the only field worth consideration by a beginning theorist. She took me to Teller to ask his advice about possible research problems. Our resulting joint work was her first publication in the field of nuclear physics. My thesis problem on nuclear magnetic moments was also selected with Teller's help, and she gave her guidance throughout that work, suggesting application to this problem in nuclear physics of techniques of quantum mechanics in which she was so proficient. These two forays into the field were her only activities in the physics of nuclear structure until after World War II.

Her approach to quantum mechanics, having been so greatly influenced by Born, gave preference to matrix mechanics over Schroedinger wave mechanics. She was very quick with matrix manipulations and the use of symmetry arguments to obtain answers to a specific problem, and this ability stood her in good stead in her later work on nuclear shell structure, which led to her Nobel Prize. She appeared to think of physical theories, in general, and quantum mechanics, in particular, as tools for solving physics problems and was not much concerned with the philosophical aspects or the structure of the theory.

When she had the opportunity to teach graduate courses, her lectures were well organized, very technical, and highly con densed. She spent little time on background matters or physical interpretation. Her facility with the methods of theoretical physics was overwhelming to most of the graduate students, in whom she inspired a considerable amount of awe. At the same time, the students took a rather romantic view of this young scientific couple, known as "Joe and Maria," and felt that it was a great loss when they left Johns Hopkins to go to Columbia University in 1939.

At Columbia University, where Joseph Mayer had been appointed to an associate professorship in chemistry, Maria Mayer's position at first was even more tenuous than at Johns Hopkins. The chairman of the Physics Department, George Pegram, arranged for an office for her, but she had no appointment.

This was the beginning of a close relationship between the Mayers and the Harold Ureys, a relationship which was to continue throughout her life, as they always seemed to turn up in the same places in later years. Willard Libby became a good friend, and it was at Columbia that she first began to come under the influence of Enrico Fermi, although she had already met him in her first summer in the United States (1930) at the University of Michigan Special Summer Session in Physics. The Mayers also saw much of I. I. Rabi and Jerrold Zacharias during their years at Columbia.

She quickly put to work her talent for problem solving when Fermi suggested that she attempt to predict the valence-shell structure of the yet-to-be-discovered transuranium elements. By making use of the very simple Fermi-Thomas model of the electronic structure of the atom, she came to the conclusion that these elements would form a new chemical rare-earth series. In spite of the oversimplifications of the particular model, this subsequently turned out to be a remarkably accurate prediction of their qualitative chemical behavior.

In December 1941, she was offered her first real position: a half-time job teaching science at Sarah Lawrence College, and she organized and presented a unified science course, which was developed as she went along during that first presentation. She continued, on an occasional basis, to teach part time at Sarah Lawrence throughout the war.

She was offered a second job opportunity in the spring of 1942 by Harold Urey, who was building up a research group devoted to separating U 235 from natural uranium as part of the work toward the atomic bomb. This ultimately became known as Columbia University's Substitute Alloy Materials (SAM) Project. She accepted this second half-time job, which gave her an opportunity to use her knowledge of chemical physics. Her work included research on the thermodynamic properties of uranium hexafluoride and on the theory of separating isotopes by photochemical reactions, a process that, however, did not develop into a practical possibility at that time. (The much later invention of the laser has reopened that possibility.)

Edward Teller arranged for her to participate in a program at Columbia referred to as the Opacity Project, which concerned the properties of matter and radiation at extremely high temperatures and had a bearing on the development of the thermonuclear weapon. Later, in the spring of 1945, she was invited to spend some months at Los Alamos, where she had the opportunity to work closely with Teller, whom she considered to be one of the world's most stimulating collaborators.

In February of 1946, the Mayers moved to Chicago where Joe had been appointed Professor in both the Chemistry Department and the newly formed Institute for Nuclear Studies of The University of Chicago. At the time, the University's nepotism rules did not permit the hiring of both husband and wife in faculty positions, but Maria became a voluntary Associate Professor of Physics in the Institute, a position which gave her the opportunity to participate fully in activities at the University.

Teller had also accepted an appointment at The University of Chicago, and he moved the Opacity Project there, giving Maria Mayer the opportunity to continue with this work. It was accommodated in the postwar residuum of the Metallurgical Laboratory of the University where, in its heyday during the war, the initial work on the nuclear chain reaction had been carried out. She was hired as a consultant to the Metallurgical Laboratory so that she could continue her participation in this project, and several students from Columbia who had become graduate students at Chicago worked under her guidance.

The Metallurgical Laboratory went out of existence to make way for establishing Argonne National Laboratory on July 1, 1946, under the aegis of the newly formed Atomic Energy Commission. She was offered and was pleased to accept a regular appointment as Senior Physicist (half time) in the Theoretical Physics Division of the newly formed laboratory. The main interest at Argonne was nuclear physics, a field in which she had had little experience, and so she gladly accepted the opportunity to learn what she could about the subject. She continued to hold this part-time appointment throughout her years in Chicago, while maintaining her voluntary appointment at the University. The Argonne appointment was the source of financial support for her work during this very productive period of her life, a period in which she made her major contribution to the field of nuclear physics, the nuclear shell model, which earned her the Nobel Prize.

Since the mission of Argonne National Laboratory at the time was, in addition to research in basic science, the development of peaceful uses of nuclear power, she also became involved in applied work there. She was the first person to undertake the solution by electronic computer of the criticality prob lem for a liquid metal breeder reactor. She programmed this calculation (using the Monte Carlo method) for ENIAC, the first electronic computer, which was located at the Ballistic Research Laboratory, Aberdeen Proving Ground. A summary of this work was published in 1951 (U.S. Department of Commerce, Applied Mathematics, Series 12:19-20).

While carrying on her work at Argonne, she continued her voluntary role at The University of Chicago by lecturing to classes, serving on committees, directing thesis students, and participating in the activities at the Institute for Nuclear Studies (now known as the Enrico Fermi Institute). The University had pulled together in this Institute a stellar assembly of physicists and chemists, including Fermi, Urey, and Libby, as well as Teller and the Mayers. Gregor Wentzel joined the faculties of the Physics Department and Institute later, and the families quickly became very close, one outcome being the joining of the families by marriage of Maria Ann to the Wentzels' son.

Subrahmanyan Chandrasekhar, who had been on the faculty of the Astronomy Department for many years, also joined the Institute. A stream of young and very bright physical scientists poured into the Institute, and the atmosphere was stimulating to the extreme. To add to this exciting atmosphere, which in some ways must have been reminiscent of Göttingen in the early days, her former teacher and friend, James Franck, was already a member of the University's Chemistry Department.

The activities in the Institute reflected the interests of the leading lights, interests that were very broad indeed, ranging from nuclear physics and chemistry to astrophysics and from cosmology to geophysics. The interdisciplinary character of the Institute was well suited to the breadth of her own activities over the past, so that her Chicago years were the culmination of her variety of scientific experience. In keeping with this, she turned her attention at first to completing and publishing some earlier work in chemical physics, including work with Jacob

Bigeleisen on isotopic exchange reactions. Bigeleisen had collaborated with her in other work at Columbia University and at this time was a fellow of the Institute. At the same time, she began to give attention to nuclear physics.

Among the many subjects being discussed at the Institute was the question of the origin of the chemical elements. Teller was particularly interested in this subject and induced Maria Mayer to work with him on a cosmological model of the origin of the elements. In pursuit of data required to test any such model, she became involved in analyzing the abundance of the elements and noticed that there were certain regularities associating the highly abundant elements with specific numbers of neutrons or protons in their nuclei. She soon learned that Walter M. Elsasser had made similar observations in 1933, but she had much more information available to her and found not only that the evidence was stronger but also that there were additional examples of the effect. These specific numbers ultimately came to be referred to as "magic numbers," a term apparently invented by Eugene Wigner.

When she looked into information other than the abundance of the elements, such as their binding energies, spins, and magnetic moments, she found more and more evidence that these magic numbers were in some way very special and came to the conclusion that they were of great significance for the understanding of nuclear structure. They suggested the notion of stable "shells" in nuclei similar to the stable electron shells associated with atomic structure, but the prevailing wisdom of the time was that a shell structure in nuclei was most unlikely because of the short range of nuclear forces as compared to the long-range coulomb forces holding electrons in atoms. There was the further difficulty that the magic numbers did not fit simple-minded ideas associated with the quantum mechanics of shell structure.

Maria Mayer persisted in checking further evidence for shell structure, such as nuclear beta-decay properties and quadrupole moments, and in trying to find an explanation in terms of the quantum mechanics of the nuclear particles. In this she was greatly encouraged by Fermi and had many discussions with him. She was also strongly supported by her husband, who acted as a continual sounding board for her thoughts on the subject and provided the kind of guidance that could be expected from a chemist who, in many ways, was better equipped to deal with phenomena of this kind than a physicist. The systematics of regularities in behavior with which she was faced had great similarity to the systematics in chemical behavior that had led to the classical development of valence theory in chemistry, and whose fundamental explanation had been found in the Pauli Exclusion Principle.

It was Fermi who asked her the key question, "Is there any indication of spin-orbit coupling?" whereupon she immediately realized that that was the answer she was looking for, and thus was born the spin-orbit coupling shell model of nuclei.

Her ability to immediately recognize spin-orbit coupling as the source of the correct numerology was a direct consequence of her mathematical understanding of quantum mechanics and especially of her great facility with the numerics of the representations of the rotation group. This ability to instantly identify the key numerical relationships was most impressive, and even Fermi was surprised at how quickly she realized that his question was the key to the problem.*

While she was preparing the spin-orbit coupling model for

* Joseph Mayer gives the following description of this episode: "Fermi and Maria were talking in her office when Enrico was called out of the office to answer the telephone on a long distance call. At the door he turned and asked his question about spin-orbit coupling. He returned less than ten minutes later and Maria started to 'snow' him with the detailed explanation. You may remember that Maria, when excited, had a rapid fire oral delivery, whereas Enrico always wanted a slow detailed and methodical explanation. Enrico smiled and left: 'Tomorrow, when you are less excited, you can explain it to me.'"

publication she learned of a paper by other physicists presenting a different attempt at an explanation and, as a courtesy, she asked the Editor of the *Physical Review* to hold her brief Letter to the Editor in order that it appear in the same issue as that paper. As a result of this delay, her work appeared one issue following publication of an almost identical interpretation of the magic numbers by Otto Haxel, J. Hans D. Jensen, and Hans E. Suess. Jensen, working completely independently in Heidelberg, had almost simultaneously realized the importance of spin-orbit coupling for explaining the shell structure, and the result had been this joint paper.

Maria Mayer and Jensen were not acquainted with one another at the time, and they did not meet until her visit to Germany in 1950. In 1951 on a second visit, she and Jensen had the opportunity to start a collaboration on further interpretation of the spin-orbit coupling shell model, and this was the beginning of a close friendship as well as a very productive scientific effort. It culminated in the publication of their book, *Elementary Theory of Nuclear Shell Structure* (1955). They shared the Nobel Prize in 1963 for their contributions to this subject.

After Fermi's death in 1954, other members of the Institute for Nuclear Studies who had provided so much stimulation for her left Chicago. Teller had gone earlier in 1952, Libby left in 1954, and Urey in 1958. In 1960 she accepted a regular appointment as Professor of Physics at the University of California at San Diego when both she and her husband had the opportunity to go there.

Her appointment as a full professor in her own right at a major university was very gratifying to her, and she looked forward to the stimulation of this newest interdisciplinary group of scientists that was being drawn together there. However, shortly after arriving in San Diego, she had a stroke, and her years there were marked by continuing problems with her health.

Nevertheless, she continued to teach and to participate actively in the development and exposition of the shell model. Her last publication, a review of the shell model written in collaboration with Jensen, appeared in 1966; and she continued to give as much attention to physics as she could until her death in early 1972.

In addition to being elected to the National Academy of Sciences in 1956 and receiving the Nobel Prize in 1963, Maria Goeppert Mayer's honors included being elected a Corresponding Member of the Akademieder Wissenschaften in Heidelberg and receiving honorary degrees of Doctor of Science from Russell Sage College, Mount Holyoke College, and Smith College.

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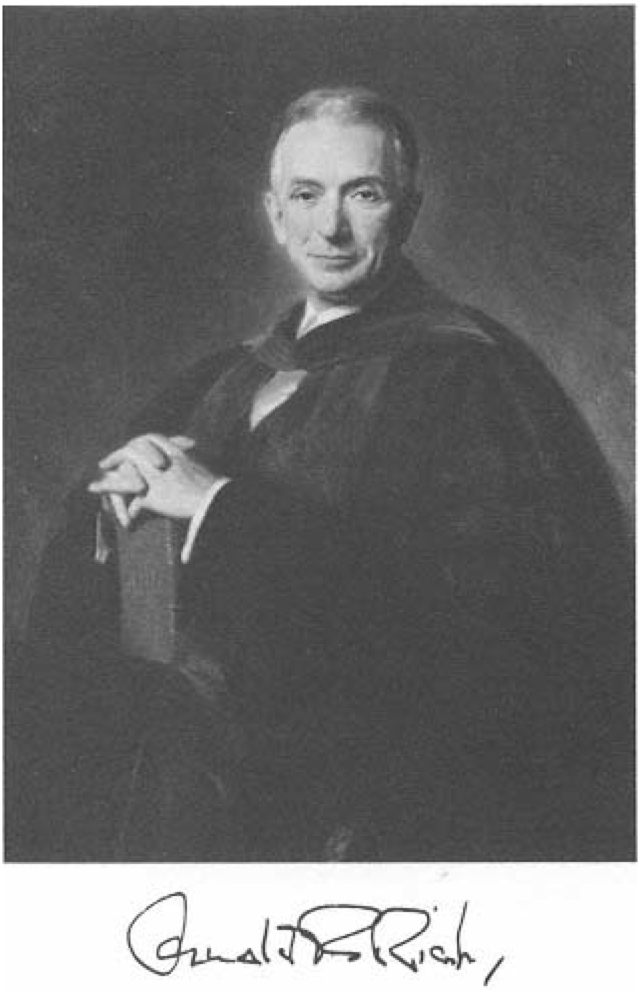

Arnold Rice Rich

March 28, 1893-April 17, 1968

By Ella H. Oppenheimer

Arnold Rice Rich was born in Birmingham, Alabama in 1893 and died in 1968 in Baltimore, Maryland shortly after his seventy-fifth birthday. He was the second of two children in a prosperous Southern family. His father, the owner of a mercantile business, allowed Arnold a free choice in deciding his future. Arnold's older sister, now Mrs. Leonel R. Weil of Montgomery, Alabama, never ceased to show interest in his career.

After elementary school education in Birmingham, Arnold was sent to a military preparatory academy—The Bingham School—in North Carolina. Whereas this military training did little to change Arnold's inherent dislike of regimentation and all forms of physical exercise, it probably was responsible for his erect bearing and slim figure. His trim appearance was pleasing in spite of his perpetual pallor, which mirrored a sedentary life. His most characteristic expression was a quizzical smile, whether in accord or dispute with his companions.

Following his preparatory school education, Arnold entered the University of Virginia. An elastic curriculum permitted a free choice of any number of subjects at one time and Rich, after a short sojourn of only two years, was given his A.B., and one year later his M`.A. degree, and was elected, as well, to Phi Beta Kappa. While at college, Rich had considered becoming a min ing engineer, but since he detested mathematics, his alternative choice for a career was biology. His Master's research, carried out in the zoology department at the University of Virginia, concerned the reactions of the probiscus of a flatworm (*Planaria albissima* Vejdovsky); this work, completed in Virginia, was published during Rich's third year at medical school.

Rich entered the Johns Hopkins Medical School in the fall of 1915 and received his M.D. in 1919 along with membership in Alpha Omega Alpha. He was associated with Hopkins for the remainder of his life. His specialization in pathology was accidental. In medical school he came under the influence of Dr. William H. Howell and was fascinated by this extraordinary scholar whose interest at that time was coagulation of the blood. Rich was soon immersed in related research projects; his findings on the "Nature of Metathrombin" and the "Changes in the Clotting Power of Oxalated Plasma on Standing" (see bibliography, 1917) were published while he was still a medical student. He did not allow the school curriculum to interfere with his research to any great degree.

One unexpected interruption did occur because of the participation of the United States in World War I. The medical students, in the fall of 1918, were regimented into the Johns Hopkins Unit of the Students Army Training Corps and because of Rich's previous military training, he was made a sergeant. Although the military regime did not hinder his pursuit of knowledge or further any athletic development, the war did change Rich's interest from theory to more practical medical problems; he therefore decided to specialize in experimental surgery. To this end, Dr. William Halsted, the Professor of Surgery, insisted that Rich devote himself to pathology for a year as preparation for a surgical internship. It was thus that Rich came under the influence of Dr. William G. MacCallum, and surgery lost its brilliant prospect to pathology.

Except for a sabbatical year studying with Dr. Hans Eppinger in Vienna, Rich remained in the Hopkins Pathology Department for his entire career. Flattering invitations from other institutions were always refused. Rich loved working at Johns Hopkins and living in Baltimore. He was appointed Professor of Pathology in 1944, and in 1947 he became the third Baxley Professor of Pathology, Chairman of the Pathology Department, and Pathologist-in-chief of the Johns Hopkins Hospital, adding his distinguished services to those of his predecessors, Drs. William H. Welch and William G. MacCallum. Although Rich became Professor Emeritus in 1958, he retained his interest in pathology until his death in 1968, which followed a long illness beset by cardiovascular complications.

I first met Arnold Rich when I was a medical student. I was fortunate to have had him for my instructor, and he always remained my preceptor, associate, and friend. Few could resist his enthusiastic teaching. His contagious interest in the study of disease changed many of his students into embryonic pathologists. To his students he embodied the ideal teacher whose standard was excellence in all spheres. This he did by example: Rich taught superbly and lectured brilliantly, vividly describing his material in his soft, slightly Southern-tinged tones. His meticulous autopsy dissections, similarly accompanied by flowing lucid analyses, always drew a large audience of students and staff.

Rich's influence was felt throughout the medical school and hospital, and he was consulted by members of all departments. This, in spite of the fact that as a careful and meticulous worker himself he might seem over-critical and discouraging. If Dr. Rich approved your work and encouraged its publication, you were assured of its worthiness; but he was ruthless in red-penciling observations he considered incomplete, equivocal, uncontrolled, or unimportant.

Rich's power of critical evaluation was especially apparent at the weekly Journal Club meetings of the pathology department.

A junior staff member would report and criticize the original contributions in a specific journal, but it was Dr. Rich who always made the pertinent comments on the value of each. To underscore his evaluations, he delighted in arguing *against* his true opinion to develop perspicacity of judgment in his young staff members. Rich's critical ability was further appreciated and utilized as a member of the editorial board of the *Bulletin of the Johns Hopkins Hospital* and as a member of the Hopkins Research Society. His opinions were sought not only by colleagues, but by former associates in advanced positions in other institutions.

A tribute to Dr. Rich's popularity was the overflowing audience of students and staff that attended his weekly clinical-pathological conferences (CPC), probably the most popular hour in the school curriculum. Dr. William Thayer was his first clinical opponent, then for many years Dr. Louis Hamman, and finally Dr. A. McGehee Harvey. Each of these clinicians added a personal delightful variation to the session. It was Dr. Rich's function to select the cases to be shown, and these were always instructive. In addition, Rich delighted to choose patients who could illustrate a hitherto unrecognized problem or lesion. Many of the CPC cases were subsequently published and frequently served as a basis for research.

It was in his research that Rich made his greatest impact on the field of pathology. He was instrumental in interesting his students and young pathology staff members in his work and utilized their aid in his extensive experimental investigations. His first important contribution elucidated the origin of bilirubin and the bile pigments. This important physiological process had previously been poorly understood and controversial. Rich's studies culminated in his classic review, "Formation of Bile Pigment," for the *Physiological Reviews* (1925). In this he concluded that hemoglobin, derived from destroyed red blood cells, is the sole source of bile pigment; its normal site of origin is in reticuloendothelial cells alone, especially the Kupffer cells, and the epithelial liver cells have no role in the formation, but only in the excretion of the bile pigment. Additional studies in this field resulted in Rich's delineation of jaundice into two types on the basis of pathogenesis. The first, retention jaundice, results from overproduction of bile pigment in conditions that are associated with a decrease in the excretory power of the liver, such as immaturity, anoxemia, and fever. The second type—regurgitation jaundice—is caused by reflux of bile from the liver canaliculi into the blood stream in the presence of duct obstruction or liver cell necrosis. Published in 1930, this work remains, with only slight modification, the basis for the present concept of jaundice, its clinical diagnosis, and its treatment.

Dr. Rich's next consuming interest was in the field of inflammation and hypersensitivity, especially as related to tuberculous infection. This motivated his investigations for many years. With the assistance of several co-workers, he was able to demonstrate that acquired resistance in the host is independent of the hypersensitive inflammatory reaction, and the latter, injurious to the host, may be eliminated by desensitization without impairment of immunity. These findings were summarized in the *Physiological Reviews* in 1941. Continued research clarified the pathogenesis of the spread of the tubercle bacilli in the body and revolutionized the concept of the disease "tuberculosis" and its myriad manifestations. These monumental studies were published as a deservedly famous book, *The Pathogenesis of Tuberculosis,* in 1944, revised in 1951 and subsequently translated into Spanish and Japanese.

Extensive investigation of the mechanisms of hypersensitivity and immunity led to additional knowledge in pneumococcal infection and syphilis. But of greatest importance was Rich's demonstration that the lesions of periarteritis nodosa, rheumatic carditis and pneumonitis, and some forms of glomerulonephritis were caused by the anaphylactic type of hypersensitivity. This work was instrumental in stimulating other scholars to investigate the basic mechanisms of the pernicious interactions of antigen and antibody which produce disease in the human body.

During Rich's long career, in addition to basic research, he made numerous important observations in the field of pathological anatomy and histology and clarified the pathogenesis of previously poorly understood conditions. With his colleagues, he demonstrated that portal cirrhosis in rabbits could result from repair following liver cell necrosis caused by a diet deficient in vitamins B_1, B_2, B_6, and nicotinic acid. By time-lapse cinemicrography of cells *in vitro,* he first depicted the characteristic locomotion and nature of the "acute splenic tumor" cell. He helped clarify the pathogenesis of acute hemorrhagic pancreatitis by proving that activation of trypsinogen by enterokinase is not a necessary step in the production of this lesion, which may result therefore from liberation of unactivated pancreatic enzymes in the parenchyma following rupture of ductules.

Rich showed the relation of the "tubular" lesions of the adrenal cortex to acute infection, described a peculiar focal interstitial form of nephritis that may occur in acquired syphilis, and first noted the obstructing pulmonary arteriolar lesions that occur in tetralogy of Fallot. His description of idiopathic interstitial fibrosis of the lungs was made in conjunction with the clinical observations of Dr. Louis Hamman, and this condition now bears the name "Rich-Hamman disease."

Dr. Rich was the recipient of many honors. In 1954 he was elected a member of the National Academy of Sciences. Prior to that time he was a consultant to the Chemical Warfare Service, to the Surgeon General of the United States Army, and to the Tuberculosis Control Division of the United States Public Health Service. In 1947, Rich received the certificate of honor of the American Academy of Tuberculosis. In 1951, France gave him its top award, making him a Chevalier of the Legion of Honor. He was a committee member of the National Research

Council from 1947 to 1952. In 1951, Rich became the Chairman of the Scientific Advisory Board of the Armed Forces Institutes of Pathology. He was the U.S. Department of State delegate to the International Congress of Allergy, Zurich, at which time he was granted an honorary M.D. by the University of Zurich and was made a fellow of the International Association of Allergists.

Further honors followed. In 1952, Rich was appointed an honorary committee member to celebrate the 50th Anniversary of the discovery of anaphylaxis in Paris. The University of Toronto honored him in 1956 as the man who during the preceding ten years had contributed most toward practical knowledge in medical arts and science. In this year he also became an honorary fellow of the British Royal Society of Medicine. The Kober Medal of the Association of American Physicians was presented to Rich in 1958. Even in his retirement, Rich continued to receive further honors. He was made an honorary Mickle fellow of the University of Toronto, and the Gardner award was given him at this University for his research on the allergic effects of certain drugs. He was presented the Gordon Wilson Medal by the American Clinical and Climatological Association, the Trudeau Medal by the National Tuberculosis Association, an honorary plaque by the Japanese Society of Tuberculosis, the Seaman award by the Association of Military Surgeons, and an award by the American College of Physicians.

The stupendous numbers of honors and awards received by Rich for his work in medical science might suggest that his interests were confined to this field. Not so! His talents were notable in many diverse directions, and it is difficult to separate his scientific from his personal life.

Rich met his future wife, Helen Jones, in 1915, while still at the University of Virginia, through a mutual interest in music. Miss Jones continued her musical education and career and did not marry Dr. Rich until 1925. Mrs. Rich remains a talented pianist and composer. There are two daughters and five grandchildren. The elder daughter is Adrienne Rich, the famous poetess who, to date, has received twenty prizes and awards for her published works. Cynthia Rich Glauber, four years younger, writes and also teaches creative writing at Harvard University. Both girls received a unique, unconventional classical education from their parents. The Baltimore census did not discover their existence until Adrienne was nearly eleven and Cynthia seven. At this point, the truant officers of the Baltimore School System insisted they attend conventional school. Their placement in a proper class was not facilitated by their ignorance of mathematics, their fluency in languages—modern and classical—and a remarkable appreciation of history, art, and world conditions.

The Rich hospitality was delightful. Dr. Rich enjoyed entertaining his staff members and held many of the Pathology Journal Club meetings in his attractive home. Once the discussion of current journals was completed, Mrs. Rich, aided by her daughters, would treat our group to delicious homemade cakes and potent punch (a secret recipe) and join in the general conversation. Often an informal musicale would follow with Mrs. Rich at the piano, Dr. Rich playing his violin or viola d'amore, and a junior staff member playing a cello or a wood instrument. These sessions were delightful and lasted well into the night: invariably the pathology staff would arrive late for work the following morning. This passed unnoticed by Rich, who abhorred the early morning and whenever possible arrived for work near noon, but remained in his office or laboratory until any hour at night. He enjoyed detaining an associate with him to discuss, in an informal manner, current problems in the department, music, literature, politics, or ethics. Time would pass heedlessly while families at home awaited a delayed supper. The scope of Dr. Rich's interest was unlimited, and he did not limit the time he devoted to others.

Probably the two main nonscientific concerns of Rich were music and literature. He was a member of the Chamber Music group that included the late "Bard of Baltimore," H. L. Mencken, and he enjoyed as well the rich musical environment offered by the City of Baltimore. He read voraciously in the classics, English, and foreign literature. His sharp critical ability was evident in his analyses of modern writings. He himself wrote with ease, and although his compositions seemed as uncontrived and fluent as his speech, he admitted that he rewrote every sentence innumerable times before satisfied of its clarity. He did thorough research in any subject that drew his interest; his studies on the "Source of the Nile" were almost as extensive as those of Alan Moorehead. One further quality in his writing must be added: he had a delightful sense of humor. This was always apparent in his original presentations, and was exemplified by an elfin twinkle as he read a treatise such as "In Defense of the Double Bed."

Rich was modest, but his vision was wide and clear. He was a free thinker and in two fields was known as a nonconformist. In an era of specialization in the medical sciences, Rich advocated comprehensive knowledge without splintering of activities. His interest in pathology was universal, enveloping all facets of disease. He did not limit his studies to a specific sex, age, or portion of the body. He was proud to be a "general" pathologist.

Rich's nonconformity was apparent in a second direction. His stimulus to work was love of work; monetary rewards were unimportant to him. He urged this precept on his juniors, but unfortunately lost many a staff member who was unable to survive on the meager salary provided by Rich. Government grants were anathema. He would not consider applying for outside funding which might necessitate modification of his work or its direction. As a result, the pathology department, supported entirely by Johns Hopkins funds, remained small during Rich's tenure. This was in keeping with the Rich precepts of quality and excellence which influenced not only his immediate associates but also spread far afield to other institutions and countries. He was responsible for directing many promising students into the specialty of pathology. Arnold Rice Rich will be remembered by them and by his peers in this field as a great pathologist.

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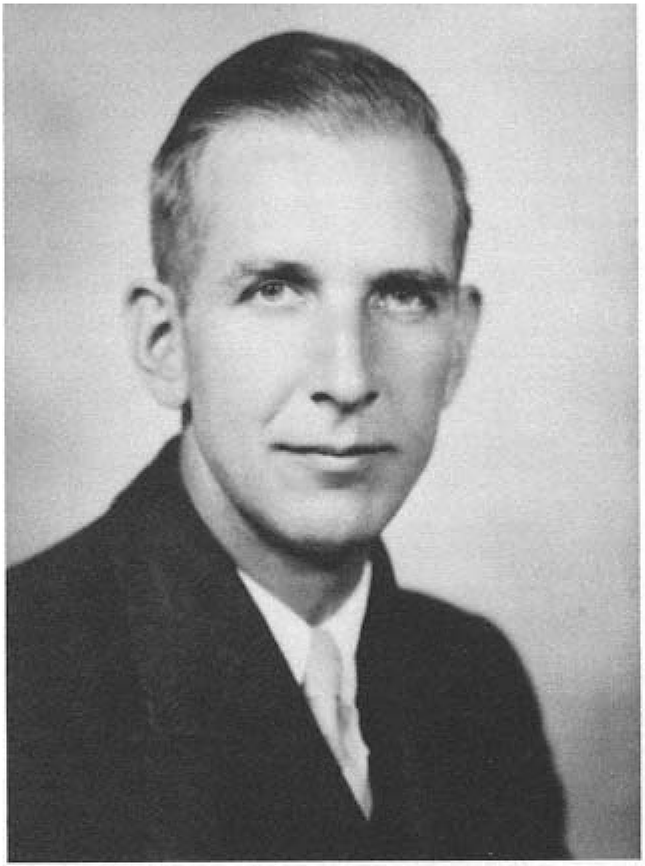

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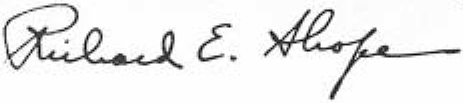

Richard Edwin Shope

December 25, 1901-October 2, 1966

By Christopher Andrewes

In these days of fashions in research and dependence upon sophisticated equipment, it is refreshing to know of people like Dick Shope. He was a born naturalist: he found his own problems in the field and sought their solutions there and in the laboratory, using simple techniques. He talked to farmers and veterinarians in his native Iowa and learned from what they had to tell him. In the laboratory he usually worked alone, doing essential things, including post-mortems of pigs, with his own hands.

He was born on Christmas Day 1901 in Des Moines, Iowa. His father was a prominent physician there, and from him and his mother he inherited genes of German, Scottish, English, Pennsylvania Dutch, and Indian origin. He enjoyed an open-air life with hunting and fishing on holidays. From ten onward he earned money by milking cows and looking after farm stock, especially poultry.

At seventeen he went to Ames to register in the School of Forestry, but as the registrar's office was not open, he proceeded to Iowa City and registered as a pre-medical student. At medical school he did well both in his studies and in sports, qualifying in 1924. Thus his college education in medicine and his boyhood experiences on the farm combined to produce a man excellently qualified to contribute to knowledge of animal diseases.

After qualifying, he became an instructor in pharmacology at the University of Iowa and among other activities did work on the chemotherapy of tuberculosis. Because of what he did in that field he was invited to join the laboratories of the Rockefeller Institute at Princeton to work under Dr. Paul Lewis. At this time he married a fellow student, Helen Ellis, and the first few years of their marriage were, financially, difficult ones. Nor was the work on tuberculosis particularly rewarding.

In 1928, however, he left the field of tuberculosis to work on hog cholera and thus began a career in the field of virology that was to continue for thirty-eight years. While investigating hog cholera in the field, Shope saw his first outbreak of swine influenza, and he proceeded to study this, with Paul Lewis, in 1929. They soon isolated a bacterium, *Haemophilus influenzae suis,* similar to Pfeiffer's bacillus, at one time thought to be the cause of influenza in man.

The *Haemophilus* was regularly present in the bronchial secretions of infected pigs, but cultures failed to reproduce the disease. Shope wrote, "We were at this stage of the game, in almost the identical predicament regarding the role of *H. influenzae suis* that investigators of human influenza had been in regarding the Pfeiffer bacillus at the close of the 1918 pandemic."*

At this time Paul Lewis died of a yellow fever infection contracted in the laboratory, and Shope carried on by himself. He now made sterile filtrates of infectious material and administered them to swine intranasally. The filtrates did indeed produce symptoms but nothing as severe as real swine flu. A few had fever, some coughed, and most had apathy and loss of appetite. Leukopenia was regularly present. Sacrificed animals had changes in the lungs, but whereas swine flu victims showed extensive collapse, bronchitis, and bronchiectasis, those suffering

* Ricketts Lecture, 1964. See Bibliography.

from what Shope now called "filtrate disease" had minimal changes. It seemed likely, however, that a virus was concerned. Shope later wrote, "Instead of one agent . . . of possible etiological importance, here were two such agents."* Now, to quote G. W. Corner, "Acting upon an improbable conjecture, Shope administered the bacillus and the virus at the same time, whereupon the animals amazingly came down with typical influenza characterised by severe pneumonia."†

Shope naturally wondered whether his findings had any bearing on the causation of influenza in man. He had not long to wait; in 1933, two years after he had described his findings, Wilson Smith, Patrick P. Laidlaw and I reported that a virus from human influenza would infect ferrets. I then visited Princeton and compared notes with Shope, thus beginning a very close friendship that endured until his death. Over the years we exchanged many long, highly controversial, and often hilarious letters about all aspects of influenza and many other subjects.

After 1934 there followed a period of consolidation in the swine flu work. Recovered pigs were found to develop neutralizing antibodies and to be immune to reinfection. The disease was found to pass readily from pig to pig by contact, but curiously enough, though the virus and *Haemophilus* would pass together from a swine flu-infected pig, only the virus was transmitted in subsequent serial contacts. Then, only the filtrate disease appeared unless the contact pig happened to be carrying the *Haemophilus.*

It was soon shown that swine flu virus, like the human one, would infect ferrets, and the further, important fact emerged that when it was given intranasally to anesthetized ferrets, they developed pneumonia instead of only nasal symptoms. Patho

* Ricketts Lecture, 1964. See Bibliography.

† *A History of the Rockefeller Institute 1901-1953* (N.Y.: Rockefeller Inst. Press, 1964).

genicity of swine flu for mice was also established. Tests on both sides of the Atlantic showed that the swine and human viruses were antigenically related, though not identical. In cross-immunity experiments, only partial protection was produced by the heterologous virus.

Swine were found to be susceptible to infection by the human virus; this gave rise to filtrate disease unless the *Haemophilus* was present also. During 1937, sera were obtained from pigs at farms near two institutions where flu outbreaks were in progress: the results showed that the pigs too had become infected with the human viruses, though no adverse symptoms among them had been observed. These observations suggested that swine influenza, which had first been observed in the Midwest in 1918, might have originated from the transmission of the pandemic virus from man to pig. This idea was put forward independently by Laidlaw in Britain and by Shope. Remarkable confirmation came from studies of antibodies in people of different ages. Shope found, in 1936, that hardly any human sera from children aged twelve or less would neutralize the swine flu virus while many from older persons did so. This suggested that a virus antigenically related to swine flu had been present in the human population up to 1924 but not later. (One may reasonably suppose that the virus responsible for the 1918-1919 pandemic persisted for a few years after that catastrophe.) Work in several laboratories has confirmed these suggestions, and the relation of swine flu virus to the pandemic strain is generally accepted as being highly probable.

The discovery of the swine influenza virus and the bearing of the findings on human disease remain Shope's greatest contribution to knowledge. The next phase of his work, beginning in 1941, is much more controversial. His observations in the field had taught him that the disease was commonly absent during the summer but might break out explosively in October and November. Moreover this might happen, perhaps after the onset of inclement weather, in several farms simultaneously. There was no question of direct transmission of virus from one farm to another; it seemed rather that virus had been seeded into the herds beforehand and then activated in many pigs at the same time. After pursuing other clues which proved unrewarding, Shope concluded that swine lung worms were acting as intermediate hosts. Ova laid by these worms are passed in the pigs' feces and taken up by earthworms in which the eggs hatch and undergo further stages in development. Pigs are fond of eating earthworms; the lungworm larvae are thus ingested and eventually reach the pigs' lungs, thus completing the cycle. Shope concluded that lungworms from flu-infected pigs would carry the virus in an inapparent or "masked" state throughout this cycle. On regaining a position in the lungs of fresh swine, the masked virus would not have its pathogenic properties restored until some stress to the pig had triggered something off; only after that did respiratory illness result. Shope had no difficulty in infesting earthworms with lungworms from flu-infected swine and in passing them back to fresh animals, which he called "prepared" swine. Disease was most readily provoked by repeated injections of these swine with cultures of *Haemophilus:* it could also be activated by exposure of the pigs to hard weather. Unfortunately the outcome of the experiments was irregular; success was obtained in only about half the attempts. Moreover, virus could never be demonstrated in lungworms by direct tests.

Opinions are much divided as to the validity of Shope's explanation of the facts. Some have accepted it as gospel, others have been wholly skeptical. The technique to test its truth has been beyond the reach of most workers: so few have attempted it. Those who have, have met varying success. I myself was unsuccessful: Shope gave me infected earthworms to take back to England, but there the attempted provocation did not lead to any disease.

Shope was wont to argue that had it not been for the fortunate existence of a suitable, available, intermediate host, swine flu could never have persisted in North America. On the other hand, I used to argue with him that neither the earthworm with which we were most concerned, a species of *Allolobophora,* nor the pig were native American animals and that I could not believe that a complex biological cycle involving three species could be evolved almost overnight.

One can now look at influenza in better perspective. It is now known that strains of influenza A virus infect man, pigs, horses, several other mammals, and many species of wild and domesticated birds. Only among swine in North America is there a suggestion that a complex cycle involving worms is concerned. Elsewhere, and particularly in man, outbreaks of influenza start mysteriously and explosively: there is, in general, no possibility that a cycle in worms plays any part. One must, I think, conclude that though swine flu virus may well persist in lungworms and earthworms in North America, it probably does so passively and is not of as great epidemiological importance as Shope supposed.

In 1930 Shope's attention was drawn to "mad itch," a violent, distressing, and fatal disease of cattle in the Midwest. He showed that it was caused by a virus transmissible to rabbits, and that it was endemic among pigs, in which it was comparatively harmless. Cattle contracted infection through contact with pigs. He finally proved the identity of mad itch with pseudorabies, a disease prevalent in parts of Europe. Later he studied another disease of pigs—swine pox—and showed that it could be, though it was not necessarily, transmitted through the agency of pig-lice. He also published evidence that hog cholera virus might persist, as swine flu virus appeared to do, in lungworms.

Shope's three most outstanding discoveries followed each other in rapid succession: swine influenza in 1931, the rabbit fibroma in 1932, and the rabbit papilloma in 1933.

The infectious fibroma, often referred to as the Shope fibroma, was discovered on a shot cottontail rabbit *(Sylvilagus)*. Minced material from this was inoculated into domestic rabbits *(Oryctolagus)* and readily produced growths, especially in young animals. Inoculation into the testis was most successful; intradermal and intraperitoneal injections gave less constant results. The growths consisted of proliferating fibroblasts; in the overlying epidermis eosinophilic granules were seen, though only in the cottontails. Shope emphasized that this was a tumor only in the broadest sense of "a local swelling consisting of a mass of new tissue."* This was wise, since the tumors normally regressed. One persisted as long as seventy-seven days in a cottontail, but regressions occurred much earlier in domestic rabbits. It was soon shown that the growths had a filterable cause since an infectious agent passed a Berkefeld V filter. There was, however, no evidence of spread by contact. When infection was transferred, it was evident that the host's cells were being infected: it was not a question of transplanting a graft. Recovered rabbits were immune to further infection and developed neutralizing antibodies in their sera.

The character of the infection suggested to Shope a possible relationship to rabbit myxoma. This infection, of South American origin, causes local lesions in the native rabbits, another *Sylvilagus* species, but in domestic rabbits causes fatal disease. Shope found that rabbits recovered from his fibroma were largely resistant to the myxoma virus: they still developed local lesions, but almost all survived. Some measure of cross-immunity was also apparent in tests with antisera against the two viruses. The fibroma virus has been used subsequently as a practical method of immunizing rabbits against myxomatosis.

By analogy with myxomatosis it seems likely that the fibroma is mechanically transmitted by insect bites, but Shope's investigation of this in the field was never completed.

In 1936 Shope sent me some fibroma material, which I duly

* *Journal of Experimental Medicine,* 56(1932):793. See Bibliography.

inoculated into rabbits' testes. Most surprisingly there appeared only acute inflammatory lesions instead of the expected proliferative changes. Moreover, many inoculated rabbits developed generalized pock-like lesions on their skins. This "inflammatory" strain was antigenically identical to the original one. Shope noted similar changes in one of his strains, and we collaborated in work that seemed to indicate that a mutation had occurred in the direction of greater virulence for rabbit cells.

Another development of work on the fibroma was the finding in several laboratories that various factors could cause the benign self-limited fibroma to become a generalized, persistent, or even fatal infection. These factors included the simultaneous injection of carcinogens or cortisone, application of X-rays, or the use of very young rabbits. The importance of work on the fibroma is its demonstration that the distinction between infection and neoplasia may be largely artificial: Shope's fibroma is one agent which bridges the gap.

Still more important in this connection is Shope's rabbit papilloma. Cottontail rabbits shot in Iowa and Kansas frequently have horns or warts on their skins. Shope found that material from these would readily produce warts on the skins of cottontail or tame rabbits when rubbed into the shaved and lightly scarified skin. The warts usually began to appear after six to twelve days: they might regress after a time or persist indefinitely as tall, often black, horns. The warts proved to be caused by a virus that gave rise to neutralizing antibodies: recovered animals were immune to reinfection. In cottontails the warts could be passed in series without difficulty, but the warts in domestic rabbits, even though well-developed, commonly failed to be transmitted to further animals. Shope devoted much attention to this matter: he did occasionally obtain successful serial transmissions in the tame rabbits, but failures were the rule. The important discovery was then made that the tame rabbit warts, though apparently virus-free, would lead to the production of specific neutralizing antibodies when injected into rabbits intraperitoneally. The virus, or an essential part of it, was still there, perhaps in a masked form. This brought out the point that a virus might be the cause of a neoplastic condition, yet not be directly demonstrable. Critics of the work maintained that the difference between the wild and tame rabbits' warts was a purely quantitative one, but further work rendered this unlikely. Though many may be reluctant to believe in the "masked" swine flu virus in lung-worms, the "masked" papilloma virus seems to be genuine. Shope suggested that it might survive as infectious DNA, but since it gives rise to neutralizing antibodies, there must be more of it remaining than that.

The work gained a new dimension when it was found that in many tame rabbits the warts progressed and became carcinomatous. This change, though common in domestic rabbits, was rare in cottontails. Shope, at this time, was busy with many problems, so he generously gave the material to Francis Peyton Rous. What Rous did with the rabbit cancers during the next thirty years is a matter of history.

When the war came, there were fears that the enemy might seek to interfere with food production in North America by introducing the very infectious disease rinderpest into American cattle. Shope, attached for this purpose to the Army, was asked to take charge of a joint United States-Canadian project to produce an effective vaccine. Stringent precautions were necessary to prevent the escape of the infection, and laboratories were accordingly set up on Grosse Isle, a small island in the St. Lawrence River below the city of Quebec. Here Shope, with a staff of five other scientists, worked in strict isolation, and in the course of nineteen months produced an effective vaccine by growing and attenuating the virus in hens' eggs. This has since been used on a large scale in the field.

With this work completed, Shope asked to be transferred back to the Navy, which had been his original wartime assignment. T. M. Rivers was in charge of a U.S. Naval Medical Re search unit and, in 1944, got together a powerful team including Shope. There was little knowledge of what medical dangers might threaten men attacking islands in the Far East. When the invasion of Okinawa was planned, Shope was a member of a medical team that landed with the assault party in April 1945. A laboratory was established and was at times under fire. Fortunately the disease hazards were not found to be great, and before long the group was ordered to return to Guam. Even under war conditions Shope's inquiring mind sought fresh opportunities. He collected in the Pacific a number of molds, hoping to find one that would be of chemotherapeutic value. One of them, from Guam, grew on the cover of a photograph of his wife, Helen, and later this yielded an extract, which he called Helenine, having activity against several viruses *in vivo*. It was proved later that this was due to a nucleoprotein in it capable of stimulating interferon production.

Soon after the war, in 1947, fell a severe blow. Shope, essentially a country lover, had enjoyed being able to live near Princeton University on a small farm where he could keep a cow and poultry and grow vegetables. Then, with no previous warning to the staff, the Trustees of the Rockefeller Institute decided to close down their Princeton branch, offering the staff the opportunity to work in their main Institute in New York. Shope hated the idea of having to work in a city, especially as he needed facilities for work with large animals. So in 1949 he resigned and accepted a position as Assistant Director of the Merck Institute for Therapeutic Research in Rahway, New Jersey. Here he continued his work, chiefly on Helenine. But work within a commercial organization was quite foreign to his temperament, and in 1952 he returned to the Rockefeller Institute (now the Rockefeller University) in New York, living in an apartment across the street from the Institute but going back whenever possible for a weekend at his "gentleman's farm" near Princeton.

Because of his wide knowledge, common sense, integrity, and complete sanity Shope was a valued member of many committees, too many for his liking: at one time or another he served on no less than forty-five. Many of these were government or state committees, particularly those dealing with diseases of animals. Later he served on several of those concerned with cancer research.

Shope received many honors. He was elected to the American Philosophical Society (1944) and the National Academy of Sciences (1940). He received honorary degrees from the universities of Utrecht, Rutgers, Giessen, Chicago, Pennsylvania, Iowa, and Yale, as well as many prizes and awards including the U.S. Army Legion of Merit and the Albert Lasker award.

In the course of his work he became infected with two serious virus diseases, lymphocytic choriomeningitis and eastern equine encephalomyelitis, but in each case he made a good recovery. Some years before his death he underwent a major operation when a small abdominal cancer was found. For a long time there was every hope that it had been completely eradicated, but the hope was not realized, and he died on October 2, 1966. He is survived by his widow, three sons (Richard, Robert, and Thomas), a daughter (Nancy), and numerous grandchildren. The three sons are all following their father's footsteps in adding to medical and veterinary knowledge.

This account tells something of Shope's very great achievements. It is more difficult to give a picture of his personality. His was an unspoilt nature. He had simple tastes and above all a vivid sense of humor. He was fond of recounting anecdotes, and they became more remarkable, and rather less credible, with each time of telling. From the age of six his summer vacations were spent by Woman Lake, Minnesota, where he and his brother hunted and fished. He returned there again and again to one or another site by the lake. I like to remember him as he was during those happy days at Woman Lake.

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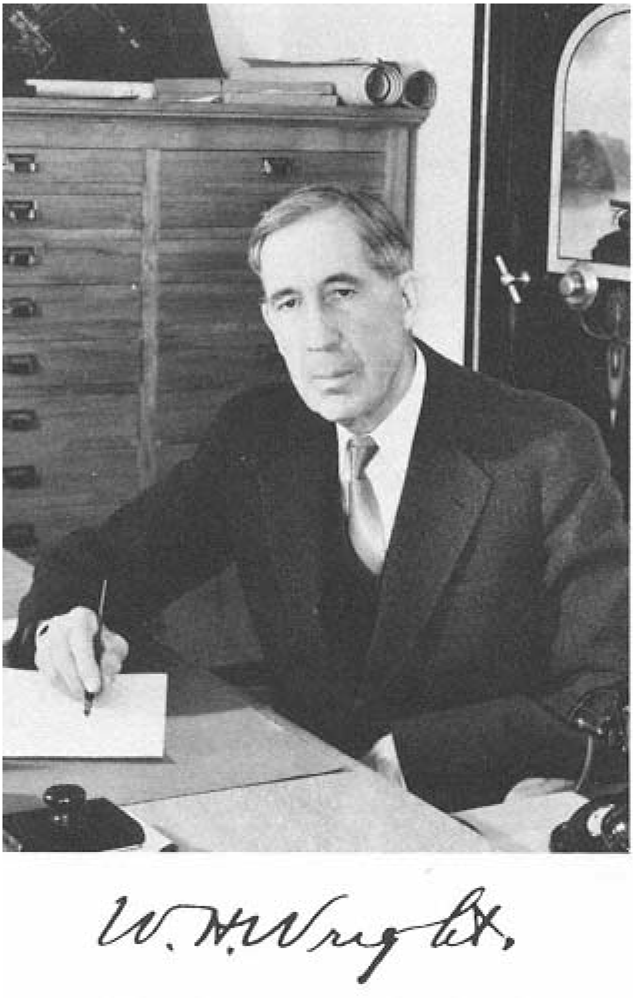

William Hammond Wright

November 4, 1871-May 16, 1959

By C. D. Shane

In response to a terse telegram from Director Edward S. Holden —"Waiting for you. Seven hundred [dollars annual salary]. Answer when you will be here"—William Hammond Wright came to the Lick Observatory in August of 1897. The salary was better than he had anticipated, the scientific opportunity tempting, so as soon as he concluded his work at Yerkes Observatory, Wright left for Mt. Hamilton. He remained on the staff of the observatory for 47 years, serving as Director from 1935 to 1942.

Wright was tall and dignified in appearance, deliberate and quiet in manner. His whimsical sense of humor was sometimes as surprising as it was delightful. Although he had a lively imagination and his scientific work over the years covered a wide range of subjects, he was always able to concentrate effectively on the problem at hand until he brought it to a reasonable conclusion.

Wright was among the pioneers in the developing field of spectroscopy. He measured stellar radial velocities and observed the spectra of novae, gaseous nebulae, and comets. He did extensive color photography of the planets and organized an improved program for accurate measurement of the proper motions of the stars. He was skillful and resourceful in the design and use of instruments, making valuable contributions to the improvement of spectroscopic equipment, and was himself a careful observer and sound interpreter of his observational data. So fortunate a combination of talents as Wright possessed is seldom found.

William Hammond Wright was the son of Selden Stuart and Joanna (Shaw) Wright. His parents, both Virginians, were members of distinguished southern families. His father graduated from William and Mary College in 1842 and came in 1860 to San Francisco, where he practiced law and later became a probate judge. His mother outlived Judge Wright by 26 years. She took an active part in the life of San Francisco and founded chapters both of the Colonial Dames and of the Daughters of the Confederacy. In a personal letter written in 1935, Wright paid tribute to his parents:

> My father was simple and charming in his manner and made hosts of friends. I think he did not know the meaning of fear.. . . While his professional life was successful, his real interest centered in his family. His devotion to my mother, and hers to him, was such that after nearly half a century I refer to it with diffidence as something too sacred to probe or disturb. To us, his children, he was affection itself, and we had his companionship during every moment that could be spared from his professional preoccupations: It was from him, in walks over hills and along streams, that we learned to love the world.

Wright was born in San Francisco on November 4, 1871, one of the younger of 12 children. He attended the public schools in San Francisco and then entered the University of California, from which he graduated in 1893 with a B.S. degree in civil engineering. This training and a subsequent course in shop work gave him certain technical skills that proved of great value in his later work with astronomical instruments. During two years of graduate work at the University, his interests turned more and more toward mathematics and astronomy. He wrote Holden asking that he be granted the privilege of studying astronomy at the Lick Observatory during the coming vacation. The request was approved and Wright spent the summer of

1895 on Mt. Hamilton. The next year he went to the University of Chicago and the newly established Yerkes Observatory. Here he had the good fortune to work with George E. Hale, who, as he noted in a letter, "treated me all along with the kindliest consideration." Hale's infectious enthusiasm no doubt turned Wright further toward the developing field of astronomical spectroscopy. W. W. Campbell had started to work in that field at Lick Observatory, and Holden invited Wright to come and work with him, which Wright did in the fall of 1897. Hale wrote to Campbell: "I am very sorry we are going to lose Mr. Wright. I do not know when I have met a man who seemed to me so promising." Wright had accepted Holden's offer of $500 "if there is enough money." But matters turned out more favorably than anticipated, and on July 15, 1897 Wright was appointed assistant astronomer on the Lick Observatory staff at $700 per annum.

In 1901 Wright married Elna Leib, the daughter of Judge and Mrs. Samuel Leib of San Jose. Until his retirement, the Wrights made their home on Mt. Hamilton. There were no children.

During his first few years at Lick Observatory, Wright participated in the main observatory program, that of taking and measuring the spectrograms of a selected list of stars. He introduced several improvements in the instrumental equipment, in particular a new mounting for the spectrograph which materially decreased the flexure and consequently improved the accuracy of the measures. He also determined the orbits of a number of spectroscopic binary stars, observed the spectra of comets and of gaseous nebulae, and, with Campbell, made extensive observations of the brilliant new star, Nova Persei.

Wright continued his work at Mt. Hamilton until 1903 when he went to Chile as Acting Astronomer in charge of the D. O. Mills expedition. For a number of years Campbell had planned a temporary observatory in the southern hemisphere for the purpose of extending the coverage of the sky for his radial velocity program. Funds were provided by Darius Ogden Mills, a California financier. A reflecting telescope of 361/2 inches aperture, a spectrograph, and the metal parts for a dome were built and prepared for shipment. It was anticipated that Campbell would go to Chile to select a site and to initiate the program. But while testing the telescope just before shipment, Campbell was seriously injured, and the full responsibility for the expedition fell to Wright.

On February 28, 1903 Wright, Mrs. Wright, and Dr. H. K. Palmer sailed from San Francisco. They landed in Valpariso on April 18th at the beginning of a month-long strike of launch hands. During that time most of the Lick equipment remained on lighters in the bay. With great ingenuity Wright managed to get the telescope mirrors, weighing about 900 pounds with their packing, aboard a passenger rowboat and safely ashore. From Valpariso these went by rail to Santiago. When the strike ended late in May the remainder of the equipment was brought ashore and similarly forwarded.

Meanwhile Wright and Palmer had scouted for a suitable site, and they finally settled on the middle peak of Cerro San Cristobal, a ridge on the outskirts of Santiago rising about 1000 feet above the valley. As Wright wrote to Campbell, many compromises had to be made in selecting the site because of peculiar conditions that "did not bear on the observing, not the least the matter of bandits." This added some hazard to the daily climb to the observatory from Santiago where the Wrights and Palmer found homes. In addition to securing a site, Wright found it almost equally important to establish cordial relations with the many government officials, which he did most skillfully to the great benefit of the expedition.

The equipment arrived in Santiago late in May. The telescope had suffered only minor damage in transit, but the dome parts were so badly rusted that it was impossible to read the markings that keyed their positions in the structure. Some parts had to be heated and reshaped. Here again Wright's mechanical skill and his ability to "make do" proved invaluable. Ground for the observatory was broken on May 27th. Despite constant frustrations and delays, the first observations were made September 11th. By the end of November the observatory was completed and had settled to systematic observations. Despite Wright's comment that "Chile is a poor place to rush a job," this was just over nine months after the expedition left California.

Wright remained in Chile for three years and laid a firm foundation for the observatory's continuance under a series of observers until 1926. During Wright's administration some 900 spectrograms of 250 different stars were secured of a quality quite comparable to those taken on Mt. Hamilton. The major portion of the observing was done by Wright, most of the measuring by Palmer. Although the observatory was initially planned for only a few years use, the dome and telescope, now owned and maintained by the Catholic University, are still operable, with apparently few changes since their installation. This attests to their sound design and construction.

Following his return from Chile in 1906, Wright was mainly occupied for several years in analyzing and preparing for publication the results of the expedition. During this period he also directed his attention to several problems, including the study of novae and gaseous nebulae which he later developed into major fields of research.

Including his early paper with Campbell on Nova Persei, Wright published twenty-three articles on novae during the period from 1901 to 1933. Observations of at least ten novae are recorded in these papers, all of which were directed toward a systematic description and an understanding of the very complex spectroscopic phenomena accompanying these stellar explosions. The spectrum of a nova undergoes a bewildering variety of changes from the time of its outburst until, as it grows fainter, it comes to resemble that of a gaseous nebula with a faint hot central star. Wright's detailed descriptions and his classification of the various phases through which the nova spectrum passes have contributed greatly to current understanding of the nova process.

The spectra of novae are closely associated with the spectra of the gaseous nebulae. It was therefore appropriate that Wright should have investigated the nebulae, though there is no reason to believe that the one subject initially suggested the other. His first paper on the nebulae, in 1902, was a study of the wavelengths of some nebular lines. Eight years later he published another paper on the same subject, and his interest in the nebular spectrum continued until his final contribution in 1934.

The results of Wright's nebular work to 1918 were assembled in a monograph in the Lick Observatory Publications, Volume 13. Here he gives the wavelengths of sixty-nine lines, of which twenty-nine were believed to have been observed for the first time. The study included the stellar nuclei of the planetary nebulae which he found from their spectra to be very hot stars. Some contained emission bands characteristic of the Wolf-Rayet stars.

By means of photographs with a quartz slitless spectrograph which he designed and had built for the Crossley reflector, he was able to obtain spectra consisting of monochromatic images of the nebulae in the different spectrum lines. Some images differed markedly in size from others. The cause was not understood at the time, but it was subsequently shown to be due to the effects of temperature and density which depended on distance from the hot central star. With the quartz spectrograph, Wright discovered in nebulae the continuous spectrum which extends beyond the limit of the Balmer series of hydrogen. This continuous emission spectrum had first been observed by Evershed in the chromosphere of the sun.

Wright extended his observations as far as possible into the previously unexplored regions of the spectrum. In the ultraviolet the efficiency of his quartz spectrograph was further enhanced by having the Crossley mirror aluminized, which was done at the Mt. Wilson Observatory. The higher reflectivity in the ultraviolet permitted observations to the limit of the atmospheric transmission.

When Wright started work on the nebulae, observations in the red portion of the spectrum were very difficult owing to the low sensitivity of commercial photographic plates in that region. He systematized the process, developed by R. J. Wallace, of bathing ordinary commercial plates in certain aniline dyes so as to obtain consistently clean and sensitive plates extending into the red. Later, with the introduction of a new dye, plates of very good sensitivity in the near infrared were obtained. Thus he was able to extend the discovery and measurement of nebular lines over the full range of wavelengths possible with these new techniques.

The favorable oppositions of Mars in 1924 and 1926 suggested to Wright that he might take this opportunity to use similar techniques in observing that planet over a wide range of colors. He photographed Mars with the Crossley reflector, using a microscope lens to enlarge the image formed at the focal plane. C. E. K. Mees of the Eastman Kodak Research Laboratory cooperated by having prepared for Wright's use special plates sensitized with red and infrared dyes.

Wright took many photographs in a spectral range extending from the ultraviolet to the infrared. He found that the surface features of Mars appeared with progressively increasing contrast toward the longer wavelengths. The only fixed markings that appeared in the violet were the polar caps, and he showed that these were atmospheric phenomena. The south polar cap as photographed in longer wavelengths was reduced to a small central white core which appeared to rest on the surface of the planet. The disc of the planet in violet light (= 4400) was measurably larger than in the infrared (= 7600). This suggested that the diffused violet light surrounding the planet represented a scattering atmosphere about sixty miles in thickness. Wright carefully looked for possible sources of error in interpreting these measures and felt that this interpretation was correct. However, later investigators have suggested other causes that may have contributed to the observed effect.

Wright also noted and studied extensively both the well-known yellow clouds that appeared on Mars from time to time and the blue clouds that occurred on the violet photographs. In addition he observed certain other Martian atmospheric phenomena. He photographed Jupiter on sensitized plates and found striking differences between the violet and the infrared images, but contemporary knowledge was too limited to suggest any clear interpretation.

Twice Wright took part in solar eclipse expeditions. In 1923 he led the Lick Observatory expedition to observe the eclipse near Ensenada, Baja California. Unfortunately, cloudy weather prevented any observations. In 1932 he was a member of the expedition to Fryeburg, Maine, where he successfully photographed the solar corona with greatly improved equipment.

When in the second decade of the present century evidence accumulated that the spirals and certain other types of nebulae were separate stellar systems very remote from our galaxy, Wright's thoughts turned to using these objects in establishing a fixed coordinate system to which the motions of the stars could be referred. It has been customary to refer stellar motions to a system based on the average motions of large numbers of stars. This is satisfactory if the motions are entirely random, but if they are systematic, there is no assurance that the coordinate system is a truly inertial one. Systematic motions in fact do exist because of the rotation of our galaxy. Wright's plan was to photograph the accessible part of the sky at two epochs separated by several decades. The star positions would then be measured for each of the two epochs, using the small faint galaxies as a framework of coordinates. From a comparison of the measures at the two epochs, the stellar motions could then be accurately determined.

Wright recognized that in order to pursue the project using external galaxies, it would be necessary to have a telescope that would photograph large fields of faint galaxies with reasonably sharp definition. With Frank E. Ross's design of large field astrographic lenses about 1920, the concept became a practical possibility. In 1934 the Lick Observatory obtained funds from the Carnegie Corporation to acquire an astrographic telescope of 50 cm. aperture and 350 cm. focal length. The lens was made by J. W. Fecker according to the design by Ross, and the mounting by the Warner and Swasey Company following Wright's general specifications. From the mid-thirties until he left Mt. Hamilton, Wright devoted the major part of his time and thought to planning and installing the telescope and organizing the program of observation. When he finally left the observatory in 1944, the telescope was essentially ready for use.

In 1947 observations on the program were started, and the first series of photographs was completed in 1954. Wright was present when both the first and the last plates of the initial series were taken. More than twenty years after the start of the first series, the second series was undertaken. The project in competent hands is now yielding results such as Wright looked forward to. It is a tribute to his imagination, foresight, and careful planning.

Wright served as Director of the Lick Observatory from 1935 until 1942, when he retired at 71, well past the normal age for retirement. He continued two years more as astronomer. During his directorship he greatly strengthened the staff of the Observatory through the addition of several young astronomers who later attained eminence in their fields.

Wright's astronomical work was interrupted very briefly at the time of World War I. Although by temperament he was in no way inclined toward a military life, he had a very strong sense of duty toward his country. In the summer of 1916 he attended a Citizens Training Camp in Monterey, and later he applied for a commission in the Coast Artillery. In the fall of 1918 he went to the Aberdeen Proving Ground in Maryland where he was commissioned a Captain of Ordnance. He had served only a month when the war ended, and early in 1919, after an honorable discharge, he returned to the University. He taught briefly in Berkeley, and returned to the Lick Observatory on June 30th of that year.

From childhood Wright had a keen appreciation of nature and the open country. It is fortunate that his life work was in so congenial a setting as Mt. Hamilton. He walked in the back country and during his earlier years he enjoyed deer hunting. He often found a solitary trail more conducive to scientific thought than his desk, and on occasion he advised a student to think out some knotty problem on a quiet hillside. For many years Wright was an active and enthusiastic member of the Sierra Club. He took part in many of their summer trips and independently camped over a wide area of the California mountains. In 1925 he was appointed a Director of the Sierra Club and he was an honorary Vice-President until his death. Throughout his life he battled to save the National Parks from exploitation. A letter to the Secretary of the Interior, written in 1950, said in conclusion: "I am anxious that Americans may continue to experience the delight of solitude in the wild places."

Wright had a strong, indeed an almost quixotic sense of justice. This is exemplified in a letter he wrote to the Regents of the University in 1932, during the depression years. He had been advised of a salary advance of $500 a year and while expressing gratification, he asked that his salary be continued at $5500. He replied in part:

> In view of the present distressing economic situation I certainly had not anticipated a raise in salary, and after a few moments of reflection I feel quite sure that it would make me uncomfortable to accept one. What with

> the shrinkage of incomes of substantially everyone, and the voluntary acceptance of reduced compensation by workmen and other wage earners all over the country, the time hardly seems opportune for salary increases. I feel we should consider ourselves lucky in the maintenance of the status quo.

With difficulty the University finally persuaded him to accept the increase.

He held strong opinions on political and international matters involving moral questions. Subsequent to World War I he was a member of the Save-the-Children Federation and contributed to the support of an adoptive child. Always an advocate of universal military training, he wrote frequent letters to members of Congress urging his viewpoint. Much of his time after retirement was spent in an attempt to arrange an adequate University pension system. Although he shared in the benefits of this, his interest and strenuous efforts were certainly in large part due to his dedication to righting a wrong. Meticulous in assigning credit for scientific work to his colleagues, he expected the same consideration in return.

Wright had three degrees, a B.S. from the University of California (1893), a D.Sc. from Northwestern University (1929), and an LL.D. from the University of California (1944). He commented: "The first was much harder to get."

He was a member of several societies, Phi Beta Kappa, Sigma Xi, the National Academy of Sciences (elected in 1922), and the American Philosophical Society (elected in 1935). He was also a Foreign Associate of the Royal Astronomical Society which awarded him its gold medal in 1938. He received the Janssen Medal of the French Academy in 1928 and the Henry Draper Medal of the National Academy of Sciences the same year.

During the last years of his life, Dr. and Mrs. Wright lived in their San Jose home. He died on May 16, 1959 at the age of 87. Perhaps his career may best be summarized in his own mod est words: "Since graduating with the class of '93, I have been doing largely what came next."

The primary source of information for this memoir was the unpublished letters, both to and from Wright, as well as Wright's published papers, in the Lick Observatory Archives. For his family background I consulted material in the Library of the California Historical Society in San Francisco and the State Library in Sacramento. I was closely acquainted with Wright for forty years and much of the content of the memoir is based on personal knowledge.

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