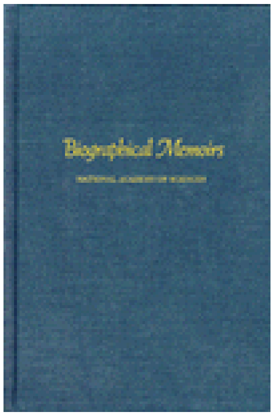

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Preface

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

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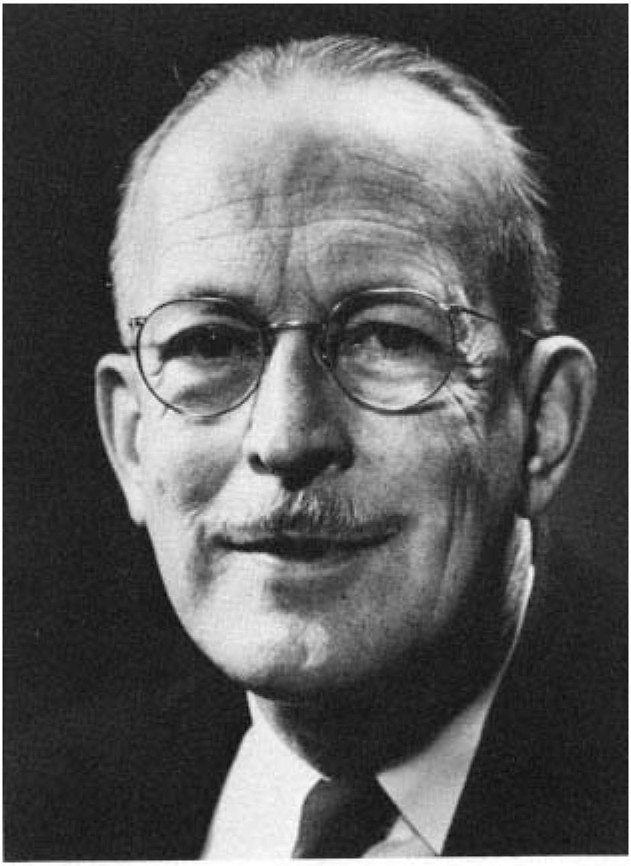

Richard M. Badger

Richard McLean Badger

May 4, 1896-November 26, 1974

By Oliver R. Wulf

The qualities of a very careful investigator, as well as those of a gifted teacher, combined to make Richard McLean Badger an unusual person. The meticulous care shown in his scientific work and in his teaching was also evident in his artistic activities, for he was an accomplished painter and a craftsman of great ability.

Badger died on November 26, 1974, at the age of seventy-eight. He had been a student, teacher, and researcher at the California Institute of Technology for more than fifty years.

Though born in Elgin, Illinois, several years of his boyhood were spent in Brisbane, Australia, to which city his family had moved. On the return of the family to Elgin, he completed his high school work there, following which he went through the Junior College of the Elgin Academy. After this he enrolled at Northwestern University, but World War I interrupted this portion of his career. He served in France in the 311th Field Signal Battalion of the Army.

Following the war he entered the California Institute of Technology, receiving there his bachelor of science degree in 1921 and his doctor of philosophy in 1924. He was appointed a research fellow at the Institute, a position he occupied from 1924 to 1928. In 1928-29 he was in Germany in postdoctoral work, as a National Research Council Fellow, at the Univer sities of Göttingen and Bonn. Following this he returned to the California Institute of Technology as assistant professor of chemistry and began, then, his long career of teaching and research.

Badger's many years of teaching undergraduates brought him the award of the Manufacturing Chemists Association for college chemistry teaching. This is presented to teachers of undergraduates who have been "personally responsible over a period of years for awakening in students a genuine interest in chemistry, for inspiring them to serious intellectual effort in studying that field, and for developing that interest into a continuing education."

His love and enthusiasm for the outdoors and the unexplored are well illustrated by the occasion when, in early days, he drove with a close friend to a point in the vicinity of the Big Sur on the coast of California to begin a long backpacking over rough and unmarked terrain to encounter friends who had started from another point on the coast and were moving toward them. He and his companion, after the meeting with the others, continued on to the point at which their friends had left their car, while the friends continued their hike to the point where Badger and his companion had left theirs, the group thus exchanging cars at the ends of the course for the homeward trip.

The writer of this memoir has been much interested in the oft-repeated instances, mentioned when in conversation with others in the course of this work, where Badger took students and colleagues on trips to the California deserts, which he so much loved and which he painted so beautifully.

In his research activities Badger was especially well known for his extensive investigations in the fields of spectroscopy and molecular structure that, with his many students and collaborators, he carried out over a period of four decades.

Though known principally for this work, he did his doc toral thesis in the field of thermodynamics under Professor Arthur A. Noyes. This was an ingenious experimental study of the ammonia, carbon (in the form of charcoal), hydrogen cyanide, hydrogen equilibrium, leading, when combined with heat and heat capacity data, to a value for the free energy of hydrogen cyanide. The investigation was a difficult one because at a temperature high enough for the equilibrium to be measured in a static system, the ammonia would be almost completely dissociated, yielding only a trace of hydrogen cyanide at equilibrium. Using a charcoal that was very active in establishing this equilibrium—and yet that did not decompose ammonia rapidly (this latter being in accord with the knowledge that charcoal is not a good catalyst for the ammonia synthesis)—Badger succeeded, using a flow method, in measuring the equilibrium constant near 800 K, studying, thus, the equilibrium with one of the components in a meta-stable condition, the ammonia dissociating only slowly in spite of being at a much higher concentration than corresponded to equilibrium with its own dissociation products at this temperature.

During and following graduate work he collaborated with Professor Richard Tolman, on the one hand in a theoretical study of the entropy of diatomic gases and the matter of rotational specific heat, and on the other hand, in an investigation that, it seems, may well have been the cause, or at least the main cause, of his entering the field in which lies the major portion of his life's work.

This latter work with Tolman was a study of the correspondence principle, in which, for the first time, a comparison of experimental data was made of its predictions as to the absolute—rather than merely the relative—strength of spectrum lines. The experimental data on the absolute intensity of spectral lines was, at that time, very limited. Tolman and Badger used Czerny's excellent measurements of the in tensities of several lines in the pure rotation spectrum of hydrogen chloride in the far infrared. To carry this study further it was clear that data under higher resolving power would be of much help. The report of this work with Tolman concluded with the remark that "An attempt to obtain further data for this purpose is already under way in this laboratory."

To aid in accomplishing such measurements, Badger devised two experimental improvements, which illustrate well, at an early date, his scientific craftsmanship and ability as an instrument maker: a balanced thermocouple and a special type of echlette grating, both of much help in spectrometric investigations in the region of very long waves where the energy available is small.

With apparatus incorporating these new helpful features Badger proceeded to measure the absolute intensity of the absorption of hydrogen chloride in the vicinity of 80 μ. Thus the extensive investigations throughout his life in the field of molecular rotation-vibration spectra had begun.

Foreseeing the importance of such spectra in the study of the structures of *polyatomic* molecules, where, of course, the main chemical interest lay, Badger chose ammonia as a first polyatomic molecule to investigate. This had a symmetrical pyramidal structure that could lead to some simplification in the increasing complexity of the spectra of polyatomic molecules. He early reported, in a brief note in *Nature,* the finding of an unexpectedly simple spectrum of six lines in the far infrared lying between 55 μ and 130 μ. This early note opens with an acknowledgment of the assistance received in the work from Mr. C. H. Cartwright, and it is followed directly by a paper in the *Physical Review* by Badger and Cartwright, "The Pure Rotation Spectrum of Ammonia." Thus began Badger's long series of investigations with a large number of graduate students and associates in the field of molecular spectra and molecular structure.

An intermission in the work at Caltech occurred at this time, when he spent the year (mentioned above) in Germany on a National Research Council Fellowship. For the first part of the year he was at Professor Franck's Institute in Göttingen. There he carried out an interesting research on the fluorescence from open (ambient-pressure) flames. Under such conditions of high pressure, one might have expected that deactivation by collisions would lead to quenching of the fluorescence. Nevertheless, this research showed clearly that pressure broadening offset the effect of deactivation and by increasing the absorption of the broad lines from the source exciting the fluorescence.

A return to the work on the spectra of polyatomic molecules was evident in the second part of the year, which he spent at Professor Mecke's Institute in Bonn. Badger and Mecke, recognizing the inherent difficulties in obtaining sensitivity and high resolution in the middle and far infrared, turned to the measurement of the spectra of polyatomic molecules in their overtones and combination tones, which lie in the near infrared and visible region of the spectrum. Here there were two important advantages: the use of photographic plates (which now could be sensitized for this region) permitting extended exposure times, and the high resolution obtainable with long-focus gratings.

Upon going to the use of a long-focus grating and high resolution, they encountered the interesting circumstance that with this considerable laboratory air path, there always appeared in absorption on their plates the lines of an oxygen molecule band at 7600 Å, well-known in the solar spectrum, Fraunhofer's A. This band had been measured earlier several times, but always in the solar spectrum where the lines were strongly broadened by the long atmospheric path and difficult to measure, and it had never been observed in absorption in the laboratory. This latter they now had under favorable circumstances for measurement. Since this transition in the oxygen molecule was of some theoretical interest, they proceeded to measure the many lines of the band and to study the combination relations of this electronic transition. The work verified Mulliken's term assignment of $^1\Sigma$—$^3\Sigma$ to it.

There followed with Mecke an extensive investigation and analysis of rotation-vibration bands of ammonia in the near infrared and visible. This established several features of the molecule—the frequencies of the three fundamental vibrations of the symmetrical NH_3 pyramid, the two moments of inertia, and the N-H bond length.

On return to Pasadena, and utilizing the high resolution obtainable with long-focus gratings and photography, Badger instituted a program for the investigation of the rotation-vibration spectra of a number of the simpler polyatomic molecules. This developed into a long series of studies with graduate students and associates, continuing into the spectra of molecules of increasingly complicated structure, and becoming the main portion of his life's scientific work.

An experimental observation by R. W. Wood and F. W. Loomis concerning the fluorescence of the iodine molecule indicated that there were two forms of the molecule, presumably ortho and para forms analogous to ortho and para hydrogen. This led Badger and Urmston at this early date to an interesting photochemical experiment involving separation of two forms of the same molecule.

Wood and Loomis had found that the iodine bands in fluorescence stimulated by white light differed from those in the fluorescence excited by the green mercury line λ5461 in that half of the lines were missing in the bands observed in the latter case. Badger and Urmston saw that it should be possible to remove from the gas the form of the molecule that absorbed the green mercury line if some molecule could be found that reacted preferentially with this optically excited species. This they found in the molecule of hexene, and they were able to show a small depletion of the number of molecules in the ortho form of the iodine molecule by this photochemical reaction.

Recognizing the importance of regularities in behavior from molecule to molecule in understanding the nature of the chemical bond, and giving consideration to the earlier attempts to express these analytically, Badger carried out an extensive survey of the information available on the force constant and internuclear distance in a considerable number of diatomic molecules.

The result of this survey led him to the expression for diatomic molecules $k_o(r_e - d_{ij})^3 = 1.86 \times 10^5$, where k_o is in 10^5 dynes/cm, and r_e and d_{ij} are in Ångströms, a relation widely spoken of as "Badger's rule." This is probably the best known of this type of relation, and what is particularly important, it can be extended in a rather simple manner, as Badger further showed, to polyatomic molecules.

To do this, however, was not entirely devoid of difficulties. There were but few cases available where one knew internuclear distances and at the same time had adequate vibrational data. Also it was difficult to know the best form of potential function to apply for polyatomic molecules. Since, in the study of molecular properties, it had been found that they may be expressed to a good approximation as sums of several individual parts, Badger found it convenient to express the potential energy as made up of three parts. The first and most important part was taken to be dependent only on the distances between atoms that are directly bonded to each other. The second part was dependent on the angles between the chemical bonds, and finally, the third part con tained terms arising from interactions between atoms not directly bonded to each other. These last terms are usually small.

Foreseeing the help that spectroscopic studies in the photographic infrared could contribute to an understanding of the special type of chemical linkage known as the hydrogen bond, Badger initiated in 1937 a series of researches that contributed greatly to the elucidation of this phenomenon, as it appears in both inter-and intramolecular bonding by hydrogen atoms.

Utilizing the excellent spectroscopic facilities that he had developed, he, with a considerable number of graduate students and postdoctoral fellows, studied, over the ensuing years, the spectra of a series of compounds in which this type of linkage occurred, each of these studies helping to clarify the manner in which hydrogen atoms act in forming such a bond.

Somewhat early in these researches and in a manner reminiscent of his previous study on the relation of force constant and internuclear distance in diatomic and simple polyatomic molecules (the study that yielded "Badger's Rule"), he investigated the relation between the energy of a hydrogen bond and the frequencies of the bands of an OH group involved in the formation of an intramolecular bond. He was able to throw interesting light on the character of the vibrations of the OH group in their dependence on the unusual potential function of such bonds.

During World War II Badger remained at Caltech working on fundamental physical problems for the Manhattan District and investigating the properties of smokeless powder for the Navy Bureau of Ordnance. He also was engaged in projects for the Office of Scientific Research and Development and the Army Air Corps. Important advances in tech nology and instrumentation during these years facilitated his distinguished work in infrared spectroscopy.

Following World War II, Badger, with his students and postdoctoral fellows, studied organic molecules of increasing complexity and of greater interest to chemists, introducing new experimental techniques in the course of the work. The spectra of urea and thiourea were among the early studies of this kind, which continued into the spectra of polypeptides and proteins.

A further excellent illustration of his unusual ability in designing and constructing apparatus is contained in instances that permitted extending these researches not only to organic molecules of greatly increasing complexity, but also to the optical investigation of these substances in the solid state. The work required the use of polarized infrared radiation and measurements of circular dichroism, working with minute crystalline specimens. It involved the construction of a "microilluminator" with a polarizer of silver chloride plates, suitable for measuring the absorption in the infrared of tiny crystals at low temperatures.

A still further illustration of his craftsmanship is contained in a mechanical model that he constructed to aid in the study of the vibrations of the peptide group, a model in which unusual attention was given to the character of the springs, helical springs being avoided because of the likelihood of their having vibrational modes of their own that would interfere. The type of spring used consisted of a single circular loop of spring-steel wire provided with diametrical projections for attachment to the atoms. The model gave automatically about the correct ratio of the force constants for stretching and bending of the peptide group.

Treating the increasingly complex spectra of ever more complex molecules presented new difficulties. This was both because of their not being resolvable into lines, even with perfect resolution, and because many or most of these molecules were unsymmetrical rotators. Under these conditions the features of their spectra that were of importance and actual help to the chemist were the envelopes of the bands. Badger gave special effort to the calculation of the theoretical envelopes of the bands of such molecules.

At the same time investigations were being carried out on a number of simpler molecules of special interest. Spectra in the visible and ultraviolet, as well as in the infrared, contributed to an understanding of their structures and of their photochemical behavior.

Uncertainties regarding the structure of the isothiocyanic acid molecule led Badger, with one of his students, to record a portion of the infrared spectrum of isothiocyanic acid vapor, an analysis of which contributed importantly to an understanding of the structure of this molecule.

The infrared spectrum and molecular configuration of hydrogen persulfide, the sulfur analog of hydrogen peroxide, were studied, the results strongly supporting a chain structure for the molecule.

The infrared absorption of the urea molecule in the crystalline state was recorded, working with single micro-crystals of urea and with polarized radiation. This was done using the microilluminator with polarizing attachment, mentioned above. The results of this study established reasonably well the complete planarity of the urea molecule in the crystal.

The structure of the ozone molecule had remained uncertain for a number of years during which Badger, with his students and postdoctoral associates, made several contributions to knowledge of the spectrum of this substance in the infrared, visible, and ultraviolet. Especially important in one of these was the finding of a new fundamental vibration, *vl.* This permitted a revised vibrational analysis that left little doubt that the molecule was in the form of an isosceles triangle with an *obtuse* apical angle, in accord with the structure indicated by electron diffraction studies.

The infrared spectra of hydrogen hypochlorite and of deuterium hypochlorite were studies in the 1-15 μ region. These seem to have been the first infrared spectral observations of these substances. The O-Cl and the O-H fundamentals were measured as well as the bending frequency, and the first overtone of the O-H stretching mode of HOCl was studied under high dispersion. This band was a good example of a hybrid band, a type of band named and first correctly interpreted by Badger and his associates. In this case the band was a band from a nearly symmetrical-top molecule with the top axis the axis of least moment of inertia.

Several studies of the spectra and structure of oxides of nitrogen and related compounds by Badger and his coworkers yielded important results. Thus, one of these having to do with the molecule NO_2, led to the observation in the infrared of two of the fundamental vibrational frequencies of the molecule and to a structure in accord with that indicated by electron diffraction observations.

Also, an extensive spectroscopic study of the infrared spectrum and the structure of gaseous nitrous acid, using both the molecules HONO and DONO, showed that this substance exists in two tautomeric forms, apparently trans and cis, the cis-form being the form of higher energy. A complete vibrational analysis was given, yielding the OH (and OD) frequencies (both in-plane and out-of-plane) for both the transand cis-forms. An estimate was given of the ONO angle in both the trans-and cis-forms, from which some conclusions were drawn regarding the electronic structure of the molecule. From the frequencies and the moments of inertia, estimates were made of certain thermodynamic properties of nitrous acid.

There followed later a further study of the infrared spectrum of NO_2, resulting in a remarkably complete description of the vibrational and rotational constants of the molecule.

Spectroscopic observations on the ultraviolet absorption of the NO molecule removed an uncertainty that had existed for some time concerning a possible pressure broadening in the gamma bands of NO, thought to have been observed by others. It was shown that such does not exist.

Professor Badger was famed for his teaching, especially in his undergraduate course in physical chemistry. His informal notes, prepared for the students, on his lectures and on the laboratory work were well known for their excellence and for having been carefully revised every year.

Badger's last scientific publication illustrates particularly well his concern for helping undergraduate students. In this research he had two collaborators. The work concerned the very weak transition in the oxygen molecule involving the low-lying $^1\Delta$ level. The writer of this biography, in referring back to that research, had occasion to look for the doctoral theses of the two collaborators, assuming that they had been graduate students. To his surprise no theses were catalogued under these two names, and on further inquiry he discovered that they were both undergraduates. This was very much in the tradition of Professor A. A. Noyes, who showed constant concern for undergraduate education, and under whom Badger himself carried out the research leading to his doctorate many years before.

The writer of this biographical memoir sincerely acknowledges his great indebtedness to Professor William H. Eberhardt, Dr. Edward W. Hughes, Professor John D. Roberts, Professor and Mrs. Verner Schomaker, and to the editor of the journal *Engineering and Science* of the California Institute of Technology for permission to use material from that journal. He feels strongly his gratitude for all of this help.

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Arthur M. Bueche

November 14, 1920-October 22, 1981

By Roland Schmitt

America's "R&D Triangle" was Arthur Maynard Bueche's favorite way of describing the unique contributions of universities, industry, and government to the nation's total technological strength. The soundness of that triangle was the principal focus of his extraordinary career in science, engineering, management, and statesmanship on behalf of technology.

Although he was employed in industry—by a single company for thirty years—Art Bueche's energy and enthusiasm led him to devote large segments of his time and talents to academia and government, while continuing to direct the technical affairs of General Electric with a style that earned the acclaim of his associates. He recognized that he could not do his full job as a leader of industrial technology without also fostering the strong roles of partners in the "triangle." For example, one of his major efforts during the year prior to his sudden death from a heart attack on October 22, 1981, was as key technical adviser to President-elect Reagan during the pre-inaugural transition period of late 1980 and early 1981. His writings and reports of that period include balanced and insightful comments on the respective roles of industrial, academic, and government technology, along with recommendations for improving the national economy, de fense, and the strength of the nation's educational system through cooperation and mutual respect. (Soon after his inauguration, President Reagan offered Dr. Bueche the post of presidential science adviser. Newspaper accounts at the time said Dr. Bueche had reluctantly declined for personal reasons. Although he never discussed this matter with them, his closest friends believe he had personal premonitions about his health that made him fear he could not give the White House position the all-out effort he felt it must have.)

Arthur Maynard Bueche[1] was born in Flushing, Michigan, on November 14, 1920. His father was an enterprising small-town businessman who put his son to work as a clerk in the family grocery store when he was eleven years old, and later as a millhand and mechanic in the family farm-implement business. During high school, young Art was very much involved in extracurricular activities, including debating, student government, track, football, plays, operettas, band, gleeclub, and orchestra. Near the end of his senior year, several of his high school teachers counseled him to study law at the University of Michigan. "But," as he wrote later, "it didn't work out quite that way."

Art's father wanted him to stay in Flushing and learn to run the family's flourishing businesses. His mother wanted him to go on to college, although she had misgivings about a career in law. Almost on a whim, based partly on the respect he had for his high school chemistry teacher but even more because of the ambition of a close friend and classmate, Art decided he wanted to be a chemical engineer. He enrolled at

[1] Art preferred that the name be pronounced BEEK'-uh, although he was always remarkably tolerant of the countless variations he inevitably encountered. His associates in chemistry suggested it was like "beaker," without the *r*, and a favorite in-house couplet made note of his role as GE's fourth research director:

Like Archimedes, shout 'Eureka'—
Whitney, Coolidge, Suits, and Bueche.

Flint Junior College,[2] riding a bus ten miles each day from Flushing, and ended up in a liberal arts course with a "major" in chemistry. Early on at Flint, an adviser told him he was better suited to the study of chemistry than of chemical engineering.

During his two years at the University of Michigan, where he received his B.S. in chemistry in 1943, Art began to recognize that his interests and aptitudes leaned more toward research than to formal course work. This interest survived and grew, even though his first major research effort—investigating the possibility that radioactive sulphur might have been produced in a large quantity of sodium chloride that had been stored for some years near the University's cyclotron—was, in his words, "a rather complete failure."

After nine months at Ohio State, the opportunity came for graduate work at Cornell University, which had been his original first choice. "Besides," he wrote, "Cornell paid slightly more." In some sketchy autobiographical notes written many years later, Art said, "At Cornell I shopped around for a thesis adviser and found many fine possibilities. Unfortunately, the adviser I wanted most was Professor (Peter) Debye, but he was reluctant to take on any more students. I guess I forced my way on him."

In January of 1946, soon after presenting his first paper (on thermal diffusion of polymer solutions) to the American Physical Society, Art was encouraged to forego his teaching duties—"although I enjoyed them immensely"—so that he could devote his full attention to research on synthetic rubber in a program directed by Professor Debye under contract

[2] In later years, when asked to provide biographical information for the records of various organizations or for people who were to introduce him on speaking occasions, Dr. Bueche always asked that Flint Junior College be included along with the University of Michigan, Ohio State, and Cornell. He was a firm believer in the importance of education at all levels and was a great supporter of junior colleges.

from the Office of Rubber Reserve. He later wrote: "The contribution that I made which was perhaps the most pioneering in nature was that involving the determination of the size of polymer molecules in solution. To the best of my knowledge, this was the first time that this had been done and I was encouraged by Professor Debye to use his lightscattering theories to accomplish this." (Debye had received his Nobel Prize in 1936 for studies of light-scattering phenomena.) The Debye-Bueche work on the size and shape of polymer molecules has been fundamental to further studies of solution behavior, chemical reactions, and viscosity. Bueche received his Ph.D. in physical chemistry from Cornell in 1947.

The young Cornell research assistant was lured to Schenectady, New York, and the General Electric Research Laboratory mainly on the strength of a candid—at times almost confrontational—interview with Dr. A. Lincoln Marshall. Marshall, who headed GE's chemistry research, was a crusty, driving, entrepreneurial leader whose forceful nature had played a key part in getting General Electric started down the road of manufacturing polymer products for applications other than electrical insulation. He recognized that young Bueche had unusual intellectual capacity; he hoped there was also the kind of restless spirit so essential to the job of moving research results to practical application with minimum delay. Marshall's hopes, although he later admitted he had some reservations about them at first, were to be amply fulfilled. Thus Bueche joined GE "at the bench" in 1950. He not only adapted himself to the pace of industrial research but also was soon fully enmeshed in it. One of the advanced ideas in polymer science in the late 1940s involved shooting a beam of high-energy electrons into a polymer and trying to get the electrons to cause desirable new connections—crosslinks—between the individual long chains. Marshall, who had worked on an early version of the idea back in 1925, initiated a project in 1951. It paired Bueche with a veteran physicist, Elliot Lawton. A million volt accelerator had become available due to the Lab's earlier x-ray work. In 1952, Lawton and Bueche used it to crosslink polyethylene. GE's Chemical Products Department immediately became interested, and work got under way leading to another new product, Irrathene®, a high-performance plastic that was the first ever made by electron irradiation techniques. Making crosslinked polyethylene at all represented a substantial achievement. However, making it by electron beam irradiation turned out to be too expensive for anything but specialty applications. But it catalyzed a new insulation technology.

The first half-dozen years in Schenectady, from 1950 until 1956, constituted Bueche's "research years." It was a time of wide-ranging exploration into new fields of polymer chemistry, of writing papers, and of producing patents at the rate of about two each year. Although he had first assumed a managerial title in 1953 (leading a small research team then called Polymer and Interface Studies), it was not until the late 1950s that his growing responsibilities forced him to spend a majority of his time in management, rather than at the bench.

When Marshall retired in 1961, C. Guy Suits, GE's research director, recognized Bueche as the obvious choice to head the Chemistry Research Department, which by that time was deeply involved in developments that would lead to General Electric's remarkable success in the engineering plastics business. As manager of chemistry research, Bueche had demonstrated incisiveness, ability to motivate others, increased understanding of business problems and their relationships to technological opportunities, and—on a day-to-day basis—fundamentally sound management skills. It was no great surprise, then, that Arthur M. Bueche was named to succeed Suits when the latter retired in 1965. Bueche was to continue the notable record of continuity among research directors at GE: Willis R. Whitney, the laboratory's founder, had served from 1900 to 1932; William D. Coolidge from 1932 to 1945; and Suits from 1946 to 1965. Bueche would extend this record so that the leadership of these four men would span seventy-eight years!

Although it may not have been surprising that Bueche succeeded Suits, there were shock waves within the ranks of GE technology when, in announcing Bueche's new appointment, the company also said it was combining the Research Laboratory with the Advanced Technology Laboratories to create a new entity to be known as the General Electric Research and Development Center. Thus Art Bueche's new job brought with it a major challenge. The former Research Laboratory, an organization with a long tradition of emphasis on fundamental research, had always been supported almost completely by GE corporate funds, and it had often been cautioned in the past by company management not to perform engineering or development work that might detract from its science-oriented mission. The former Advanced Technology Laboratories, earlier called the General Engineering Laboratory, was an institution that had suffered a variety of ups and downs because of its broad dependence on contracts for support, a place where short-range results were the principal priority, and an organization sometimes looked on as a "poor cousin," occupying quarters in the Schenectady Main Plant that were a far cry from the glamorous surroundings created for the Research Laboratory "out on the hill" at a site overlooking the Mohawk River in nearby Niskayuna. Art Bueche's assignment was to not only integrate these two disparate organizations into a cooperative, smoothly working whole, but also—of greatest importance—to "get them connected to the company" and in tune with

General Electric's growing technological needs and objectives.

On the night before Dr. Bueche died, he was honored in absentia by the Franklin Institute with its Delmer S. Fahrney Medal. The citation read on that occasion succinctly summarized how well the challenges of 1965 were met. It read, in part:

> From 1965 to 1978, under his leadership, this combined entity (the new General Electric Research and Development Center) achieved remarkable success, with the staff grown to more than 2000, including 800 scientists and engineers, and with laboratories in many domestic and overseas locations. Dr. Bueche's leadership of these operations has been recognized as an unusually outstanding example of managerial skill. He has been highly innovative in the development of effective approaches to both strategic and operational planning of technical work, in devising new technical liaison and technical information exchange techniques, in promoting and recognizing technical excellence, and in encouraging an extremely diversified company to utilize its varied strengths in new organizational and operations approaches.

Art Bueche himself once defined his job this way: "Our fundamental task is to spot the kind of person who at least demonstrates the potential for being the one in a hundred—one in a thousand—one in a lifetime—who may have the flash of true genius. Then our job, above all others, is to give these people, and their ideas, a chance to survive and grow." He would constantly ask his associates, "What's new? What's the new idea? Why can't we get this done faster? What are the obstacles? Let's get moving." He pushed, directed, stretched, and challenged people to reach beyond what they had thought they could accomplish. As one coworker told a news reporter preparing an article about Art Bueche, "It's tough to match his effort on the job, seven days a week. He sets an example that's difficult for people to follow. And this inspires them. He won't take no for an answer. And he wants to understand everything."

The achievements of the R&D Center under his leadership were many and noteworthy. In electronics, the accomplishments included the development of an advanced computerized axial tomography (CAT) x-ray scanner; the development of the first solid-state imager based on charge-injection device technology; invention of thermomigration, a process that reduced the time required for certain semiconductor processing steps; and the invention of the surface charge correlator, a new semiconductor device for analog signal processing. Achievements in new materials technology included development of a commercial process for fabricating cubic boron nitride, a man-made material second in hardness only to diamond; invention of polycrystalline diamond "compacts" for metal-cutting tools; the creation in the laboratory of the first synthesized gem diamonds; the first simple and inexpensive technique for fabricating ceramic parts of silicon carbide; invention of silicon/silicon carbide composites; and several high-performance plastics, including a family of resins based on a unique technology of polymerization by oxidative coupling. In the field of energy R&D, achievements included advances in the development of water-cooled gas turbines, sodium-sulphur batteries, coal-gasification technology, and the production of energy-efficient lamps.

Dr. Bueche's achievements brought him a variety of medals and honors, including eight honorary doctorates. They also brought him promotion within General Electric, to the post of senior vice president for corporate technology in 1978. This meant he became the company's top technical officer and spokesman and joined the corporate executive committee—but he also had to move from Schenectady to Fairfield, Connecticut. This required that he relinquish the direct day-to-day responsibility for the R&D Center, although the Center remained under his purview as a senior officer.

From the time he first became a company officer in 1965, Dr. Bueche recognized his role and responsibility as a public spokesman for technology. His platform appearances, before GE and outside audiences, averaged nearly one each week over a period of fifteen years. He was in great demand as an interpreter of technology and, toward the end of his career, as a forthright spokesman on technology policy and the appropriate roles of universities, industry, and government in the "R&D triangle." He also spoke to many international audiences: in Japan, the South American nations, Mexico, Canada, France, Italy, Spain, and Germany. He served as American chairman of the World Electrotechnical Congress in Moscow during 1977. Appearances in Great Britain included a Faraday Lecture at the Royal Institution (during which Man-Made ™ diamonds were actually produced "on the spot") and the Kelvin Lecture for the Institute of Electrical Engineers.

As an active member of the National Academy of Sciences, he served on the Academy Forum Advisory Committee and the Finance Committee. He was a member of the National Academy of Engineering and of the Executive Committee of its Council. He served as president and a director of the Industrial Research Institute.

In government, he was active on several advisory groups to the president's office on matters related to science and technology. He also served as a member or consultant with science and technology committees of the National Science Foundation, NASA, the U.S. Air Force, the National Bureau of Standards, and the Energy Research and Development Administration.

In education, he served on the Board of Trustees of the Rensselaer Polytechnic Institute, the Albany Medical College, and the Hudson-Mohawk Valley Association of Colleges and Universities. He was a member of Visiting Committees at Massachusetts Institute of Technology, Harvard, and Duke;

of the Advisory Committee of the School of Metallurgy and Materials Science, as well as of the Board of Overseers, for the School of Engineering and Applied Science at the University of Pennsylvania; and of the Advisory Board of the Institute of Materials Sciences at the University of Connecticut. His contributions to, and associations with, his alma mater, Cornell, were legion; among his assignments was chairmanship of the Council for the College of Engineering.

While a resident of Schenectady (from 1950 to 1978), his public service included board membership of Ellis Hospital and of Sunnyview Hospital and Rehabilitation Center. He also found considerable personal satisfaction in helping guide the affairs of one of the area's largest and most progressive banking institutions, the Schenectady Savings Bank (now Northeast Savings), as an active board member.

On Monday evening, October 19, 1981, Art Bueche served as chairman of a dinner meeting at GE's Fairfield headquarters, held to honor eleven Steinmetz Award winners, people from various GE business components who had made outstanding technical contributions during their careers with the company. Early the next morning, he suffered a massive heart attack. In spite of superb medical attention, including some pioneering new techniques, he died at St. Vincent's Hospital, Bridgeport, Connecticut, on October *22,* 1981.

His family[3] and associates moved at once to establish a fitting memorial: the Arthur M. Bueche Memorial Fund, currently administered by the National Academy of Engineering. Each year the NAE Awards Committee will select a recipient to be honored for "outstanding statesmanship in

[3] Dr. Bueche was survived by his four children: Kristine of Wilmington, North Carolina; A. John of Ellsworth, Maine; Margaret of Ballston Lake, New York; Elizabeth of Schenectady, New York; one grandchild; and two brothers, Frederick J. of Flushing, Michigan, and Bernard M. of Flushing, New York.

science and technology." The recipients will be asked to present lectures on science and technology issues, and a cash gift will be made to the school where the lecture is presented.

At the funeral service in St. John the Evangelist Church, Schenectady, on October 27th, one of Dr. Bueche's long-time associates said in his eulogy:

> Above all, Art Bueche was tough-minded. That was a quality he respected—highly respected—in others. And we respected, admired, envied him for his tough-mindedness—for the intellectual power—for the concentration—for the genius' attention to detail—for the searching questions that made us all recognize, so often, how far ahead of us he was in his thinking—for the willingness to devote energy, time, enthusiasm, and persistence to the task at hand, with a diligence and dedication the rest of us could only marvel at, hold in awe.

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Angus Campbell

August 10, 1910-December 15, 1980

By Clyde H. Coombs

Angus Campbell was christened Albert Angus Campbell in Leiters, Indiana, and his first publications appeared under that name. In 1946, at age thirty-six, his twelfth publication appeared (with George Katona as coauthor) under the name Angus Campbell—and that is what he was known as ever after. He once remarked that he felt he was nobody until he became just Angus Campbell.

He was the fifth of six children born to Albert Alexis Campbell and his wife, Orpha Brumbaugh. His father, the son of a farmer, went to high school in Ann Arbor, Michigan, and then on to the University of Michigan, where he graduated in 1897 with a degree in Latin and Greek. He returned to Indiana to become a teacher, principal, and finally superintendent of schools in Peru, Indiana. Angus's father had grown up in a strict Scottish Presbyterian atmosphere. It is said, perhaps apocryphally, that Angus's grandfather and great-uncle, returning from church one Sunday in their horse-drawn cart, passed by a lovely lake; one enjoined the other not to look at it on the Sabbath. Such values do not dissipate in two generations.

Angus was two years old when his father realized ten thousand dollars on an investment in a grain elevator. He moved the family to Portland, Oregon, bought a large house, and became a school principal. Angus grew up in Portland and attended the University of Oregon, where he received the B.A. in 1931 and the M.A. in 1932, both in psychology. He then transferred to Stanford University, where Kurt Lewin was a visiting professor in the summer and fall of 1932. Angus attended Lewin's lectures and read some of his articles, as yet untranslated from the original German. He always felt that Lewin had exerted a major influence on his education as a psychologist. A personal friendship developed that lasted throughout the remainder of Lewin's life.

The other major influence during Angus's graduate student years was Ernest Hilgard, who came to the Psychology Department at Stanford in 1934 and established an experimental program in human conditioning and learning. Hilgard served as a role model for Angus in research and teaching; Angus was his research assistant and later an assistant in Hilgard's popular course in elementary psychology. Angus was Hilgard's first doctoral student, earning his degree in 1936 with a thesis on eye-blink conditioning.

There were two academic jobs available to him that year, one at Ohio State University and the other at Northwestern University; Angus accepted the position as instructor in psychology at Northwestern. He was promoted to assistant professor in 1940. He went to Northwestern expecting to teach experimental psychology, but as Franklin Fearing had just moved from Northwestern to UCLA, Angus was asked to teach Fearing's course in social psychology. In so doing, he came into contact with Melville Herskovits, a social anthropologist at Northwestern, and attended his courses and seminars. In a very short time this influence, along with his own experience teaching social psychology, completed his transition from an experimental to a social psychologist. It was the track he was to follow for the remainder of his career.

At Herskovits' urging, Angus applied for and received a

Social Science Research Council fellowship to study social anthropology at Cambridge University during 1939 and 1940, but World War II ended his stay in England after half a year. He then moved the site of his work to the Virgin Islands. where he did field research among the black population on St. Thomas that resulted in a monograph examining the group's culture and personality. This was his first experience with field work and with research on race relations, both of which became major concerns of his professional life. Earlier, at Northwestern, he had met Jean Winter, a student in psychology, and during his stay on St. Thomas they were married.

Angus's intellectual transition from an experimental to a social psychologist fully matured when he left Northwestern to join Rensis Likert's Division of Program Surveys in the Department of Agriculture in Washington, D.C. Likert had been asked by Henry Wallace, then Secretary of Agriculture, to form a research unit to provide information useful in program planning and policymaking.

Likert had been developing the methodology of large-scale sample surveys for the Life Insurance Agency Management Association as a tool for scientific research, probing for attitudes, intentions, expectations, and trends that would reflect dynamic aspects of a society—and not merely a static description. With the advent of World War II, the Division of Program Surveys' areas of research expanded, and so did the personnel. Angus joined Likert's group in 1942, and thus was formed a professional (and personal) relationship that was to endure for life.

Other social scientists assembled by Likert who also became longtime research associates of Angus Campbell included Charles Cannell, Dorwin Cartwright, George Katona, Daniel Katz, and Leslie Kish, as well as others, like Theodore Newcomb, who were associated with particular projects.

This was a period of rapid development in survey research methodology, especially in probability sampling, interview techniques, and questionnaire construction. For Angus in particular, these developments were paralleled with experience in research administration, including the translation of the needs of management and planners into wellstructured, researchable problems followed by communication and interpretation of the research findings to clients and the public.

Two well-known studies undertaken by the Department of Agriculture Division 7 Program Surveys during this period were the War Bond Redemption Study and the Bombing Survey. The first had to do with determining a suitable policy for War Bond redemption, based on projections of consumer attitudes after the cessation of hostilities. The second was a study of the effect of bombing raids on the attitudes and behavior of civilians in Germany and Japan.

This was a new kind of social science. To preserve and develop it, the survey group wanted to move as a unit to an academic setting, continuing large-scale survey research useful to policymakers, managers, and operations planners. But the role and status of such research in an academic setting was not yet normalized, so an innovative arrangement with the University of Michigan was formulated in 1946. The Survey Research Center was established: The University provided housing, and some limited financial support based on teaching and academic services, and research program support was obtained from outside grants and contracts, with overhead funds retained by the Survey Research Center.

In 1948, after Kurt Lewin died, his group, then at the Massachusetts Institute of Technology, transferred to the University of Michigan as the Research Center for Group Dynamics. The two centers were joined to form the Institute for Social Research, with Rensis Likert as head and Angus

Campbell as assistant to Likert, as well as director of the Survey Research Center.

For the remainder of his life Angus carried substantial administrative responsibility, but continued to be active in research. After Likert's retirement in 1970, Angus succeeded him as director of the Institute, a post he relinquished in 1976 to return to research as a program director in the Survey Research Center.

Throughout this long period many important studies were conducted. Beginning in 1948, Angus collaborated with Robert L. Kahn in a study of presidential voting intentions, reported in a small monograph, *The People Elect a President* (1952). The election of 1948 represented a massive failure of pre-election polls to predict correctly the election of President Truman, a failure attributed to misguessing the actual vote of the late deciders. In contrast to the commercial polls, Campbell and Kahn refused to predict a victory for Dewey over Truman. They took this position in part because their data did not support it, and in part because they continued to collect data up to the day of the election, the latter being one of the reasons they adhered to a policy of nonprediction in all their subsequent election studies. Following this initial election study, Angus established a research program for the continuing study of election behavior, collaborating with Gerald Gurin and Warren Miller, and in later years with Miller, Philip E. Converse, and Donald E. Stokes. This program developed into the Center for Political Studies, another center within the Institute for Social Research.

This program produced a series of books, among which *The American Voter* (1960, published in collaboration with Philip E. Converse, Warren E. Miller, and Donald E. Stokes) is a landmark. It is based on national samples in the 1952 and 1956 elections, and smaller samples in 1948, 1954, and 1958. The purpose of this research was to examine the be havior of the voter as an individual and not to describe the electorate as a whole. Toward this end, they introduced ideas of "ideological conceptualization" and of a "funnel of causality."

The kinds of ideological conceptualization distinguished between persons in terms of the degree to which they used ideological concepts in making sense of political affairs. The notion of a "funnel of causality" was a metaphor for the narrowing down along a time axis from the more remote factors affecting a voter's decision, such as party identification and social class, to the more immediate factors of specific attitudes and candidates.

Although these concepts do not play a significant role in subsequent studies, they reflect a concern for explanatory theoretical abstractions of greater generality than the descriptive statistical relationships revealed in the data. The book was soon described as a classic, and it has had a seminal influence in political science. Angus was influential in establishing the Interuniversity Consortium for Political Research, which is, among other things, an archive of social and political data. The continuing series of election studies has been declared by the National Science Foundation "a national resource," the first such designation outside of the natural sciences.

Just a few years after *The American Voter* was published, another book by the same four authors, *Elections and the Political Order,* appeared (1966). Of its fifteen chapters, thirteen are papers published by them, separately or in collaboration with others, during the interval between 1960 and 1963. This collection reveals some of the cumulative potential in programmatic research made possible by the continuing series of election studies and an archive of data.

The chapters are organized into four parts, beginning with a focus on the individual voter and why he behaves as he does. The book progresses to elections as the units of analysis and collective (institutional) factors as explanatory variables. Party affiliation, political ideology, and two-party and multiparty systems are studied, using comparative data from France and Norway. Campbell's own contributions include an explanation for the puzzling regularity of the loss of seats suffered by the party in the White House in the offyear elections, referred to as surge and decline, as well as a classification of presidential elections.

Campbell had revealed an early interest in racial prejudice in his field work on St. Thomas, published in 1943. In 1967, in response to a government request, he and Howard Schuman directed a large study of racial attitudes in fifteen cities in North America. This resulted in a brief report for the National Advisory Commission on Civil Disorders published by the Government Printing Office. The report was followed a few years later by a somewhat longer book entitled *White Attitudes Toward Black People,* based on a secondary analysis of the data, and including some data from the Survey Research Center's election studies of 1964, 1968, and 1970. Since then, the Institute for Social Research has been monitoring trends in racial attitudes by repeating parts of that study every two years.

By the 1970s his interests had turned to social accounting more generally. He regarded the Institute's continuing research studies on voting behavior, political institutions, and race relations as prototypes for the study of more general social trends. With the support of the Russell Sage Foundation, he and Philip E. Converse edited a book entitled *The Human Measurement of Social Change* (1972). The twelve contributions contained in this book are concerned with possible psychological components and indicators of social change, such as attitudes and aspirations. The essays ranged over a variety of areas, including time budgeting, leisure, and eco nomic affairs. Angus's own contribution was on the relation of levels of aspiration and satisfaction to social change.

The Russell Sage Foundation provided support for a nationwide survey on the quality of American life, which resulted in the 1976 book of that title by Campbell, Converse, and Willard L. Rodgers. Measuring the quality of life is probably the granddaddy of all social psychological measurement problems and may be inherently impossible to achieve in the strict sense. On the other hand, there is an intuitively compelling reasonableness about the concept and a "need-to-know" that makes some social scientists and statisticians willing to brave the perils and to construct an index.

In their book on the quality of American life, seventeen specific domains of life experience were investigated, such as marriage, health, job, savings, and the like. A weighted additive combination of an individual's ratings on those components was used to predict an individual's global rating of his or her sense of well-being.

The book contains a wealth of data, but one of the more interesting findings reported is that subjective feelings of satisfaction do not always mirror objective reality in simple ways. Subjective ratings of variables like satisfaction with housing, standard of living, and utility of education, for example, did not just steadily increase or steadily decrease in their relation to some objectively measured variables like income, age, and education. They offered two explanations for this failure of the subjective to mirror the objective in a monotonic manner: accommodation, that is, adaptation over time; and constricted horizons, a consequence of lack of education limiting the salience of alternative situations.

Angus and his coauthors of *Quality of American Life* discuss problems of bias, interactions, individual differences, and other possible limitations on interpretation. They used the metaphor of "an exploration into unknown territory [to] provide benchmark data against which subsequent measurements could be compared."

This area of research—human happiness—became Campbell's research interest for the remainder of his life. In 1978 the National Science Foundation supported another study, and Angus summarized in nontechnical language the results of that study, along with material from four previous national surveys going back to 1957, in his last book, *The Sense of Well-Being in America: Recent Patterns and Trends* (1980).

Angus was a scholar of breadth in social science, recognized and listened to by sociologists and political scientists; he was especially pleased with awards received from diverse fields of social science. In addition to the Distinguished Scientific Achievement Award of the American Psychological Association, he had received the Distinguished Achievement Award of the American Association for Public Opinion Research (1962), the Lazarsfeld Award from the Council for Applied Social Research (1977), the Laswell Award from the International Society of Political Psychology (1980), and a Doctor of Letters, University of Strathclyde (1970).

He was a professor of both psychology and sociology at the University of Michigan, and, as further indication of his breadth, he was appointed, beginning in 1964, as a lecturer in the Law School, where he taught a seminar on sociolegal problems to advanced law students. At his home institution, the University of Michigan, he served on innumerable committees, particularly in sensitive situations such as in the selection of presidents and deans and in controversial situations where trust was a major ingredient. One feels the family atmosphere in which he grew up asserted an influence. His home institution honored him with the Distinguished Faculty Achievement Award in 1969 and asked him to deliver the Distinguished Senior Faculty Lecture Series in 1979.

He was asked to serve in many professional activities where his breadth and judgment were needed. To mention only a few, he was a consultant to the Ford Foundation in Poland and Yugoslavia in 1959, 1960, and 1961; on the Committee on SST-Sonic Boom, National Academy of Sciences, 1964-70; on the Advisory Committee on Consumer Expenditures, Bureau of Labor Statistics, 1960-64; on an advisory group to the Social Security Administration, 1961-64; and served with numerous other groups for the American Psychological Association, the Social Science Research Council, the National Research Council, and agencies of the U.S. government, including the executive office of the president.

One of Angus Campbell's major goals was to bring the findings of social science to the effort to improve the quality of life and human welfare. The catholicity of his research interests, his administrative talent, and his understanding and ability to communicate the results of social research outside the research community contributed greatly to his success in achieving his goal. But his basic personality and deep commitments were also major factors. At first contact he might have seemed a dour Scot, austere and impressive, somewhat forbidding. Yet on even short acquaintance, his warmth, his caring, his objectivity, and his integrity came through; his family was devoted, his friendships were close and lasting, his impact on students and social research strong and important.

I wish to thank Betty Jennings, his secretary for twenty years; Philip E. Converse; Robert L. Kahn; and Adye Bel Evans, librarian, Institute for Social Research, for providing me with biographical material. His wife, Jean W. Campbell, was especially helpful in providing information about his early background.

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W G Cochran

William Gemmell Cochran

July 15, 1909-March 29, 1980

By Morris Hansen and Frederick Mosteller

William Gemmell Cochran was born into modest circumstances on July 15, 1909, in Rutherglen, Scotland. His father, Thomas, the eldest of seven children, had begun his lifetime employment with the railroad at the age of thirteen. The family, consisting of Thomas, his wife Jeannie, and sons Oliver and William, moved to Gourock, a holiday resort town on the Firth of Clyde, when William was six, and to Glasgow ten years later.

Oliver has colorful recollections of their childhood. At age five, Willie (pronounced Wully), as he was known to family and friends, was hospitalized for a burst appendix, and his life hung in the balance for a day. But soon he was home, wearying his family with snatches of German taught him by a German patient in his nursing-home ward. Willie had a knack for hearing or reading something and remembering it. Oliver recalls that throughout his life, Willie would walk or sit around reciting poems, speeches, advertisements, music hall songs, and in later life oratorios and choral works he was learning.

Until Willie was sixteen, the family lived in an apartment known in Scotland as a "two room and kitchen"—a parlorcum-dining room (used on posh occasions, about twelve times a year), a bedroom used by the parents, and a kitchen.

In the kitchen food was prepared, cooked, served, and eaten; dishes were washed, laundry done, friends entertained, and homework accomplished. It was also the boys' bedroom in the form of an alcove-with-bed known as "the-hole-in-the-wall." The boys had a happy childhood, with mile-long walks to and from public school twice a day (lunch was eaten at home) and play at the oceanside.

Willie was a great achiever in school, usually coming in first. Oliver feels he had an irresistible urge to be first, often calculating closely just how much he would have to do to gain that end. Oliver recalls being worried about passing a professional exam and having Willie say to him: "I don't know what on earth you're worrying about; you only have to pass, I have to be first." He was referring to the Bursary Competition, open to all scholars in Scotland. And he was first, winning his fees to Glasgow University. Later he was in an even larger competition for the George A. Clark Scholarship, which provided support for four years and paid his Cambridge fees. Without winning these competitions, he almost assuredly would not have been able to attend either Glasgow or Cambridge.

Willie had no absorbing hobbies as a boy, although he dabbled in many things. Cycling, hiking, and walking in the hills were his chief physical activities. Later, studying and reading became primary. His scholastic prowess won him many books as prizes and created an extensive home library.

Cochran graduated with the M.A. from Glasgow in 1931 with first class honors in mathematics and natural philosophy (physics) and shared the Logan Medal for the most distinguished graduate in the Arts Faculty. That same year he entered St. John's College, Cambridge, and studied for the mathematics tripos (mathematics major) as a prelude to becoming a research student. As an elective, he chose a new course, Mathematical Statistics, taught by John Wishart. A fellow student believes that the Great Depression had interested him in the work of a Dr. Mess, who advocated thorough mathematical investigation of economic problems. He was doubtless also influenced by R. A. Fisher's work at this time. By now he had dropped the use of Willie, and among his colleagues he was known as Bill.

Bill was persuaded by Frank Yates to leave Cambridge without his doctorate to accept a position, a rare opportunity in the depression year of 1934, to do practical research at Rothamsted Experimental Station. Cochran never did receive an earned doctorate, although he received honorary degrees from The University of Glasgow (1970) and The Johns Hopkins University (1975).

During his six years at Rothamsted, Cochran pioneered with Yates in developing techniques for analyzing replicated and long-term agricultural experiments and for assessing the effects of weather patterns on crop yields. They also studied selection effects in non-random sampling.

At Rothamsted, Cochran gained a great deal of practical experience and became well known in his field. In 1937 he married Betty I. M. Mitchell, a plant pathologist.

After visiting Iowa State College (now University) in Ames in 1938, Cochran agreed to return there the following year to teach. The imminence of war in 1939 made him hesitate to leave Europe, but he felt he must keep his word. Under George Snedecor, in 1939 Iowa State was a center for statistical treatment of experimental work—at a time when modern applied statistics had little foothold in America. The emphasis in applied statistics at Iowa was then on sample surveys and experimental design. Cochran lectured on both topics in his first quarter, and these lecture notes matured over the next ten years into his two well-known texts on these topics.

Two of Cochran's three children were born in Ames, Elizabeth in 1940 and Alexander Charles in 1942.

In 1943-44 Cochran took leave to join the Princeton Statistical Research Group at Princeton University as a research mathematician. He was to work on Army-Navy research problems, including naval warfare and a survey of bomb efficiency, for the Office of Scientific Research and Development.

At Iowa State, Cochran and Gertrude Cox initiated their collaboration, which culminated in their book *Experimental Design,* published in 1950. In 1946, at Cox's instigation, Cochran left Iowa to organize and head the graduate program in experimental statistics at North Carolina State College at Raleigh. Cox envisioned this program as half of the Institute of Statistics, the second part consisting of a Department of Mathematical Statistics at the University of North Carolina at Chapel Hill, headed by Harold Hotelling. The Cochran's third child, Theresa, was born in North Carolina in 1946.

In January 1949 the Cochrans moved to Baltimore, where Bill became head of the Department of Biostatistics in the School of Hygiene and Public Health at The Johns Hopkins University. Here his interest in medical and health problems increased. Bill published a second book, *Sampling Techniques* (1953). His two books—along with his 1967 revision, at Snedecor's request, of Snedecor's *Statistical Methods*— became important reference texts and were widely translated. *Statistical Methods* is one of the most widely cited books in the scientific literature.

In 1957 the Department of Statistics was organized at Harvard University, and Cochran joined the staff, remaining nineteen years until he became professor emeritus in 1976. During his time at Harvard, his continued interest in biostatistics was reflected in his interaction with the Department of Biostatistics in the Harvard School of Public Health.

Cochran's Work

In discussing Cochran's scientific work, we open with his most famous theorem, and follow with selections of his work on the design and analysis of comparative investigations, with both experiments and comparative observational studies. After an overview of his work on counted data, we present some of his contributions to the theory and practice of sample surveys, followed by brief mention of other areas of work. With a few exceptions, we emphasize his advice and philosophy rather than the details of his technical work.

Cochran's first paper (1934), a mixture of algebra and analysis, brought into mathematical statistics an extremely valuable and widely used result, now called Cochran's Theorem:[1] Let X_j, $j = 1,2, \ldots, p$, be independent standard normal random variables with sum of squares Q. Let Q be decomposed into the sum of k quadratic forms Q_i, where Q_i has rank r_i, $i = 1,2, \ldots, k$. Then if one of the following three conditions holds, so do the other two: (a) $\sum_{i=1}^{k} r_i = p$, (b) each Q_i, has a chi-squared distribution, and (c) each Q_i is independent of every other.

Cochran (1934) himself exploited this result to show that analysis of variance can be extended to a variety of situations requiring adjustment for covariates.

Design and Analysis of Comparative Investigations

Agriculture. Over the years, sets of Cochran's papers focused on methods of value to many applied areas, including agriculture and biomedical research. At Rothamsted he ex

[1] The form cited is suggested by Maurice G. Kendall and Alan Stuart, *The Advanced Theory of Statistics,* 2d ed., vol. 1 (New York: Hafner Publishing Company, 1963), 360-61.

posited new developments in lattice designs, attributing the general method to Frank Yates. These designs help breeders of wheat, soybeans, corn, and small grains by permitting comparisons of large numbers of varieties (squares being preferable, such as 49, 64, 81, 100, . . .). He compares the performance of these designs with that of others (1941a, 1941b, 1943b).

Along with the descriptions of the methods and their strengths and weaknesses, Cochran continually emphasized the computational effort required in the analysis and the importance of being able to communicate the ideas to the investigator. Why should the half-day or day of calculation required for the analysis be of much concern when an agricultural investigation has already required considerable land for much of a season and several workers to carry it out? Perhaps Cochran realized that a computation that took him half a day might leave a breeder helpless. He was therefore eager to reassure the breeder of its feasibility. Indeed, he said (1941a, p. 355), "Extra complication in the statistical analysis may be a drawback to the widespread use of a design in other respects. If the experimenter does not clearly understand the assumptions involved in the statistical manipulations, or the reasons for them, he loses confidence in the final results of the calculations."

In several papers, Cochran gave substantial reviews intended to guide experimentation in specialized subject matters. For example, just before leaving the United Kingdom for the U.S.A., he presented a major review paper (1939a) on the design and analysis of long-term agricultural experiments that won plaudits during discussion from Sir John Russell, R. A. Fisher, J. Wishart, F. Yates, M. S. Bartlett, M. G. Kendall, and H. O. Hartley. Cochran dealt not only with formal design and analysis considerations but also with important features of the practical execution of these trials in the field: size and shape of plots, numbers of replications, choices of stratification or blocks, headlands and guard rows between plots, and the value of a year or two of a uniformity trial prior to a long-term field experiment, especially for a new crop. And he warned the statistician, "It is not sufficient for him [the statistician] to provide the best possible design to suit the size of the experiment; it is also his duty to advise whether he thinks the experiment as designed is worth doing, or whether it should be postponed until more resources are available" (1939a, p. 106).

With Gertrude Cox (1946a), he summarized the principal sources of variation in greenhouse experimentation (temperature, moisture, and shading gradients) and major designs that could control for such sometimes nearly overwhelming variables. Curiously, in 1946 they reported that they had no information about the possible benefits of moving pots around, although this is one advantage of the greenhouse over field conditions.

His article in the *International Encyclopedia of Statistics* (1978b) on experimental design contains an instructive postscript on the rise of the use of experiments in the social sciences and the encouragement given to this movement by the Social Science Research Council. That postscript relates more generally to his study (1976) of the history of experimentation. After introducing us to Arthur Young's total intolerance for any method but comparative experiments, Cochran notes (1976, p. 5), "This issue persists today. In reviewing the present state of knowledge about the relative merits of two therapies for hospitalized patients, we may find a few wellcontrolled experiments and a larger number of doctor's observations on their experiences with one or the other therapy. Young would seem to suggest that to consider the latter group is a waste of time."

Cochran used the history article to include a little instruc tion on experimental design, as well as to get in a few licks about some consulting problems he had suffered. He suggested that most consulting statisticians will have had experience with an investigator who begins "'I want to do an experiment to show that. . . .' He knows the answer." Cochran used this remark as a springboard to discuss double-blind experiments. In a similar aside, Cochran used James Johnston's book on agriculture[2] to make an additional point. After describing Johnston's position that a bad investigation wastes money and leads to incorrect results in standard textbooks, as well as to the neglect of further research, Cochran said (1976, p. 9), "I have heard this point made recently with regard to medical experiments on seriously ill patients, where there is often a question for the doctor if it is ethical to conduct an experiment, but from the broader view-point [it is] a question of whether it is ethical *not* to conduct experiments."

Cochran used history to console the young scholar. Upon recalling that after Student's t tables had been available for fourteen years and practically no one used them, he said, "Young research workers who feel that the world is very slow to appreciate their results might be heartened by this example. The world is indeed a little slow at times to realize how brilliant we are" (1976, pp. 13-14). He sums up the history of statistics in agriculture by saying that it took a century to take two major steps: (1) to begin applying probability theory (already available in astronomy) to interpret quantitative experiments and (2) to establish efficient practical methods for the conduct of field experiments.

Bioassay. A sequence of papers (three with Miles Davis: 1963 1964, 1965a; and 1973) reported on Cochran and Dav

[2] J. F. W. Johnston, *Experimental Agriculture, Being the Results of Past and Suggestions for Future Experiments in Scientific and Practical Agriculture* (Edinburgh: W. Blackwood and Sons, 1849).

is's studies of bioassay, where the investigator wants to find the LD50, the dosage that kills 50 percent of the animals or insects. They studied sequential approaches using a grid of dosages. Animals are tested at an initial dose, and the outcome at that dose guides the choice of the next dose-up or down. In one version, if the first dose kills, the second dose is one step smaller; if it does not, the next animal gets a dose one step higher. This process continues. They recommended a two-stage approach. The first stage uses few animals with large steps until it locates a reversal, and the second stage uses the Robbins-Munro method with smaller steps.

Clinical Trials. His papers on the design of clinical trials (1961a, 1977b) had a rather general nature. In the first (196 la), he emphasized heavily the value of precise protocol, power, blindness, randomization, and design. The biostatistician of the 1980s—with special survival analyses, sequential designs, and balancing approaches—might be surprised, even affronted, to read (1961a, p. 71): "The planning and conduct of a clinical trial does not involve any difficult or esoteric intellectual principles. It is mainly a matter of hard work and attention to detail."

The second paper (1977b) was a group effort focused on surgical experiments in duodenal ulcer. Although Cochran had suffered a substantial illness, he was essentially recovered, but he did not want to take on any extra tasks. Consequently he refused to take part in a working group in the Faculty Seminar on Health and Medicine at the Harvard School of Public Health. But students and friends pleaded with him to change his mind, and in the end he chaired the Working Group on Protocol Issues. After two years of discussions in depth of the principal experiments in surgery for duodenal ulcer, the group produced a comprehensive list of medical and statistical criteria for consideration in further experiments. Most of the criteria have value for design, anal ysis, and reporting of comparative medical investigations generally, not just for surgery for duodenal ulcer. Again, care and precision in protocol were emphasized. The lists cannot be reproduced here, but a remark on follow-up to obtain information on nearly 100 percent of patients treated is worth quoting (1977b, p. 191): "A search produced few references to available techniques for guarding against follow-up losses. There seems to be no substitute for determination." They reported that at the Mayo Clinic high rates of followup have been "achieved by writing letters directly to patients and not going through their doctors; if no reply is forthcoming, the telephone is used. If the patient is not found, a vigorous search is undertaken, including use of bill-collecting agencies, who apparently have experience with similar problems" (1977b, p. 191).

Observational Studies. Program arrangers often asked Cochran to provide a substantial general paper on the conduct of comparative studies intended to decide causation. In discussing the advantages of matching subjects or materials as compared with the use of covariance adjustment in observational studies, he first noted that the methods perform almost equally well. "A difficulty which I have occasionally encountered with covariance is that some scientists have an inborn suspicion of adjustments to the data, and although the adjustments made in the covariance analysis are entirely objective, they may find a rather grudging acceptance" (1953, p. 687). (Although Cochran correctly stated that, given least squares, the adjustment itself is objective, the decision to make it usually is not; when many covariables are available, many subsets can be selected. The suspicious scientist has a right to some skepticism because an investigator could adjust for the subset that gave results most pleasing to him or her. Nevertheless, when the covariables for adjustment are chosen in advance of the investigation, the method is objective.)

Possibly his *Journal of the Royal Statistical Society, Series A* paper on observational studies (1965b), followed by papers in 1967 and 1972, formed the basis for his program to prepare a book on the planning and analysis of comparative observational studies. At his death, he left six and one-half of seven planned chapters completed. Moses and Mosteller edited it for posthumous publication (Cochran, 1983). The 1965b paper itself offers a substantial introduction to such investigations. Some quotations may be appropriate. The opening reminds us of Harold Dorn's[3] dictum to ask "How would the study be conducted if it were possible to do it by controlled experimentation?" (1965b, p. 236). In reviewing the dangers of loading a study with so many research questions that it may fall of its own weight, he confessed, "But when dealing with an imaginative investigator I do not find it easy to determine at what point one should adamantly oppose all further questions, however ingenious and interesting" (1965b, p. 240). In before-after studies—of health, for example—some investigators note that the initial questionnaire may alert participants to behavior they should beware of, and thus bias the study. Cochran said (1965b, p. 249), "My own view is that an educational programme that cannot improve health practices more than can a single questionnaire is not wrongly considered a failure. . . ."

When faced with a collection of studies yielding contradictory results, the applied scientist "cannot avoid an attempt to weigh the evidence for and against, since some results are so vulnerable to bias that they should be given low weight. . . . He should state such judgements forthrightly, remembering his duty to maintain even standards and, if possible, an air of calm detachment" (1965b, pp. 253-54). This last remark is a bit of tongue-in-cheek humor; Cochran was about to sug

[3] H. F. Dorn, "Philosophy of inferences from retrospective studies," *America Journal of Public Health,* 43(1953):677-83.

gest that someone else, while doing a good job, may have sometimes strayed just a bit from even standards.

Counted Data

Among Cochran's several systematic research programs, the analysis of counted data stands out (1936a, 1936c, 1937, 1938a, 1940a, 1942b, 1943a, 1950, 1952, 1954b). Maxwell, in his introduction to the first organized text on counted data, *Analysing Qualitative Data,* [4] said "I am indebted to . . . Professor W. G. Cochran from whose work I borrowed freely" (p. 9).

In studying both the distribution of diseased plants in rows of a field (1936a) and the persistence of one kind of weather (1938a), Cochran had occasion to derive and use the distribution of the number of runs in a binomial sequence where the probability of success on a single trial differed from 1/2, thus generalizing the work of Marbe and others. He also investigated the power of the sign test (1937).

The problem of chi-squared tests and the correction for continuity (1942b) come up in various ways. How small shall the observed counts in cells be before we abandon the attempt to use chi-squared, or pool cells, or find some corrective device? Repeatedly Cochran returns to this question (1936c, 1942b, 1952, 1954b). In the 1942b paper he gives a special formula and tables for handling the problem, tables still not widely used, we believe. In addition to these, the use of transformations (1940a) and the analysis of variance for data that come as percentages (successes divided by totals; 1943a) and data from matched samples (1950) produced major contributions to the field. The large papers concerning goodness-of-fit tests (1952) and strengthening the common chi-squared tests (1954b) offer a small education in them

[4] A. E. Maxwell, *Analysing Qualitative Data* (London: Methuen and Company. 1961).

selves. The 1952 paper (p. 324) lists rules for handling chisquared with small numbers in the cells, and the 1954b paper (p. 420) offers some slight revision of these rules based on further research. Indeed, these ten papers would form a small textbook on the analysis of counted data. The 1954b paper presents a large number of methods for strengthening the chi-squared tests and includes the essentials, together with a derivation in the appendix of the now-popular technique, sometimes called the Mantel-Haenszel method for combining results of several contingency tables.

One difficulty in reading Cochran's papers is that it is hard to know what may be original with him and what he regards as helpful exposition of known results. He often said of statistical research workers, "we all deserve more credit than we get for results others publish, and a little less for those we ourselves publish." His grounds for this remark were that many ideas in statistics float around for a long time before someone actually sets them down in good order and publishes them. Often we cannot nail down just exactly who had the original idea.

The utility of the common chi-squared test for goodness of fit has been much debated, partly because most statisticians including Cochran (1952, p. 336) agree with Joseph Berkson. He argued that, given enough observations, we would be sure to reject the normal distribution (and presumably any other distribution) as a model in any particular situation. (Amusingly enough, when Berkson gathered an enormous body of data to check whether radiation counts followed a Poisson process, theory and data agreed extremely well. On the other hand, Berkson's work on counting blood corpuscles showed that no standard distribution applied.) Cochran pointed out that Karl Pearson was aware of this difficulty, even when he invented the chi-squared test. Cochran struggled to suggest new approaches in these situations. He proposed that per haps instead of testing a point null hypothesis we should be testing whether a quantity falls into an interval; or that we should consider as the null hypothesis a broader family, near the one being assumed.

Sample Surveys

Cochran's initiation into sample-survey theory and practice came when he joined Frank Yates at Rothamsted. R. A. Fisher, with Yates and other colleagues at Rothamsted, had made remarkable advances in the theory of statistics as a tool of applied research in agricultural experiments. Modern theory and methods for sample surveys were substantially advanced by these developments, including the use of randomization in sample selection, already used to some extent in sample surveys.

Cochran's first paper directly related to sample surveys (1936b) demonstrated the importance of randomization in the selection of samples as distinguished from purposive or judgmental selection. Yates had earlier done an experimental demonstration of biases that resulted by allowing a judgmental selection of a "random sample" of plants. At a conference of the observers of the crop-weather scheme (for crop forecasting) in 1935, an experiment was planned to see to what extent the kinds of biases observed by Yates are common to all observers who make deliberate selections. Cochran analyzed the results of the experiment and concluded (1936b, pp. 74-75):

> It is obvious that samples that are picked by a process of randomization which gives every sample in the population an equal chance of being picked, must be representative of the population from which they are drawn and give an unbiased estimate of the quantity which it is desired to measure. Those who have little experience of the technique of sampling might, however, be unwilling to admit that they could not do as well, or better, by choosing the samples themselves. In this experiment, out of

> twelve observers, all of whom have had some training in sampling, not one managed to pick a sample that could be called representative of the material from which they were sampling. . . . What is even more serious and striking is that the individual observers were not consistent throughout the experiment; the positive bias in selection increased regularly as the mean height of the sampling-unit decreased.

This work helped establish the importance of randomization in both sample surveys and experiments.

In his work at Rothamsted, Cochran took advantage of the opportunities to be involved in practical studies in design and analysis of experiments and sample surveys. The sample-survey experience included, for example, evaluation of crop-forecasting methods based jointly on sample-survey information on the crop and on weather data (1938c). It also resulted in empirical analyses of survey data to evaluate the efficiency of alternative sample designs for agricultural studies (1938b). As was his usual practice, this paper included a rather exhaustive analysis, including one of the early efforts to balance the amount of work involved against statistical efficiency. He also developed procedures for making approximate advance speculations on sampling variances before results are available for analysis, as is essential in practical work on sample-survey design. In another study (1940b) he evaluated the gains that would result in estimating cereal yields by estimating the ratio of grain to total produce from the sample and applying the ratio to known information on the total produce.

In 1939 he published a paper entitled "The Use of the Analysis of Variance in Enumeration by Sampling," based primarily on his work at Rothamsted, but published after he had moved to the Statistical Laboratory at Iowa State College in 1939. In this pioneering paper he applied the analysis of variance to finite-population sampling by regarding the finite population as a sample from an infinite superpopulation. He conditions on the finite population and obtains estimators appropriate to the finite population that—with minor exceptions—agree exactly with those arrived at by direct application of probability-sampling theory. He illustrated the great convenience and power of the application of the analysis of variance to data available from a particular sample in evaluating the appropriate use of subdivision (now generally referred to as stratification), subsampling, choice of sampling units, and double sampling. He concluded:

> The results of a properly planned sampling investigation, in addition to providing an estimate of the accuracy of the sample, often provide estimates of the accuracy of various alternative methods of sampling which might have been used. These estimates are helpful in increasing the efficiency of sampling in future studies on similar material . . . . The estimate of the relative accuracy of two methods of sampling is shown to be in most cases a simple function of the variance-ratio, so that its sampling limits are easily obtainable. (p. 510)

In 1942 Cochran contributed an especially interesting result for sample-survey applications concerned with "Sampling Theory When the Sampling-Units Are of Unequal Sizes." The procedure is applicable in estimating a population average, $\bar{y}_p$, or total for a variable *y* where information on a correlated variable, *x*, is available for the total population and for each unit in the sample. Among others he considered a linear regression estimator of $\bar{y}_p$ of the form $\bar{y}_r = \bar{y}_s + b(\bar{x}_p - \bar{x}_s)$, where $\bar{y}_s$ and $\bar{x}_s$ are the sample means, *b* is the usual estimate from the sample of the linear regression coefficient, and $\bar{x}_p$ is the known population mean of the *x* characteristic. It was well known that this estimator is the minimum variance estimator of $\bar{y}_p$ if the population regression of *y* on *x* is linear and if the conditional variance of *y* given *x* is constant. Cochran, however, showed the exceedingly useful result that $V(\bar{y}_s)(1 - r^2)$, the well-known estimator of the variance of $\bar{y}_r$ for this particular case, is asymptotically valid in large samples for any population; that is, it is a consistent estimator of the variance no matter what the form of the regression of y on x. He considered weighted as well as unweighted regression estimators and compared these and other alternative estimators for varying sampling designs, as well as discussing the conditions under which each estimator is most efficient. As he pointed out, the regression estimator is relatively difficult to compute. While the regression estimator has been extensively used, its applications are limited by the difficulty of computing. In addition, in sample surveys that measure many characteristics the results for multiple characteristics are not additive; that is, an estimate for males plus an estimate for females will not necessarily be equal to the estimate for both sexes combined. Nevertheless, it has proved highly useful in many applications. It has also contributed to understanding the principles of estimation from sample surveys.

Systematic sampling, of which the simplest form is selecting every k^{th} unit from some kind of an ordered sequence, has long had intuitive appeal and has been widely used as a sample-selection procedure. The estimation of summary measures from such a sample, such as means, ratios, or regressions, is straightforward, but theory is not available for making consistent estimates of variances. Often variances are estimated by treating a systematic sample as equivalent to a stratified random sample. Some empirical studies have shown this to provide a reasonable approximation in many circumstances, but far from a satisfactory approximation in others.

In 1944 W. G. and L. H. Madow identified systematic sampling as a special case of cluster sampling, and provided theory and examined its characteristics under some alternative models.[5] Cochran extended these results in a paper entitled "Relative Accuracy of Systematic and Stratified Ran

[5] William G. and Lillian H. Madow, "On the theory of systematic sampling," *Annals of Mathematical Statistics,* 15(1944): 1-24.

dom Samples for a Certain Class of Populations," published in 1946. He observed that numerous studies of real populations had revealed that the variance among the elements in any group of contiguous elements increases steadily as the size of the group increases, and he constructed a model appropriate to such populations. In formulating the model, he regarded the observed finite population as a sample from a superpopulation in which (in what follows, *E* is the expectation operator):

$$E(x_i) = \mu, \; E(x_i - \mu)^2 = \sigma^2, \; E(x_i - \mu)(x_{i+u} - \mu) = \rho_u \sigma^2,$$

where $\rho_u \leq \rho_v \leq 0$ whenever $u < v$. He obtained average variances for samples from the possible finite populations from such a superpopulation.

For this class of populations he showed that:

> The stratified random sample is always at least as accurate on the average as the random sample and its relative efficiency is a monotone increasing function of the size of the sample. No general result is valid for the relative efficiency of the systematic sample. In fact, there are populations in the class in which the systematic sample is more accurate than the stratifed sample for one sampling rate, but is less accurate than the random sample for another sampling rate. If, however, the correlogram is in addition concave upwards, the systematic sample is on the average more accurate than the stratified sample for any size of sample. (1946b, p. 164)

He pointed out that while no unbiased or consistent estimate of the variance of the estimated mean is available from a systematic sample, an unbiased estimator can be obtained if one can properly make an assumption concerning the form of the population being sampled. Its validity will depend, of course, on the validity of the assumed population model.

Cochran published numerous other papers concerned with various aspects of sample surveys as he encountered them in consulting or otherwise became interested in them. For example, in a 1961(b) paper he examined alternative rules for establishing strata boundaries by comparing them empirically for several different forms of populations with varying amounts of skewness. In 1962 he jointly authored, with J. N. K. Rao and H. O. Hartley, a paper that proposed a simple procedure for unequal probability sampling without replacement. This approach had the advantages of simplicity of calculation and the ability to provide unbiased estimates of the variance of the estimators. This was a topic that received considerable attention at the time, and a number of different procedures were proposed by various authors.

The problem of nonsampling errors in surveys is one that has received extensive attention, and in 1968 Cochran prepared a review paper and extended some of the earlier work that had been done in this area. He concluded, as do others, that errors in measurement can sometimes seriously vitiate most standard statistical techniques and at other times have only trivial effects—depending on the size of the relevant response variances and covariances. He added that what seems needed at the present state of development of this area are many studies that permit the estimation of these variances and covariances, and that most of these studies should be embedded in ongoing surveys. "When an 'errors of measurement' study has to be conducted separately, as will sometimes be necessary because of the complexity of such studies, it is always difficult to reproduce the working conditions of an actual survey" (1968, p. 665).

In "Laplace's Ratio Estimator" (1978a), Cochran took an engaging historical tour. He reviewed the well-known estimate made by Laplace in 1802 of the total population of France. Laplace took a sample (by purposive sampling procedures) of communes in France and persuaded the government to have a population census taken in each of these. Births were registered throughout France, and therefore were known for each commune as well as the country as a whole. He then estimated the total population of France with the ratio estimator $\hat{Y} = Xy/x$ where X is the known total registered births, x is registered births for the sample communes, and y is the total population for the sample communes. The estimate was 28.4 million. Laplace then estimated the standard error of this estimate to be 108,000. In computing the estimated sampling error, Laplace assumed that the birth rate in each commune (and of course in all of France) was the consequence of sampling births and population at random with equal probability from the same urn, a finite superpopulation.

Cochran reported: "He found the large-sample distribution of his error of estimate to be approximately normal, with a small bias and a variance that he calculates" (1978a, p. 3). Cochran then points out that in computing the sampling error Laplace failed to recognize that the birth rates in the sample and in all of France were not independent, and states in a summary remark:

> It is unfortunate that Laplace should have made a mistake in probability in a book on the theory of probabilities. In his application, however, the mistake was of little consequence. His working out of the large-sample distribution of the ratio estimator and his concept of the superpopulation as a tool in studying estimates from samples are pioneering achievements. (1978a, p. 10)

Cochran wrote a number of review papers related to sample-survey topics (1938b, 1947, 1951, 1956) that provided lucid summaries of the state of the art at the time the papers were written and gave additional interpretations. Of course his textbook, *Sampling Techniques,* is a substantially comprehensive summary, with extensions of theory to round out topics and with reporting of empirical results to provide better guidance on practical implications of some of the methods. It is undoubtedly the most widely used textbook in teaching sample surveys, as is attested by the printing of second and third editions in 1963 and 1977.

Cochran's Other Contributions to Statistics and to Society

Cochran suggested that statisticians might profitably conduct a survey to find out how scientists use statistical techniques and how they are helped by them. He thinks it "might be very illuminating to statisticians if it could be carried out despite the obvious difficulties. Statisticians are, I think, rather quick to jump to conclusions about the kinds of problems which scientists in other fields are supposed to face, and about their presumed uses and misuses of statistical methods and ideas" (1952, pp. 334-35). Because he was writing in the *Annals of Mathematical Statistics,* he probably felt he was speaking only to the statisticians.

Having illustrated Cochran's propensity for returning to problems repeatedly, we shall not review all the topics where he carried on such a program. Instead we merely mention that these included: (a) the problem of weighting to combine results from several comparable experiments (for example, when the effects in the different experiments did not necessarily have the same true means or precisions and when precisions needed to be estimated); (b) the problems associated with both qualitative and quantitative discriminant functions; (c) the use of covariates in experiments and observational studies; (d) the effect of errors of measurement on regression, analysis of variance, and the analysis of counted data; and (e) special analyses for detecting outliers, for handling missing observations, for adding or removing a variable in regression, or for comparing scales of measurement.

Cochran was an exceptional teacher, beloved by his students. He directed four dissertations at North Carolina, fifteen at Johns Hopkins, and nineteen at Harvard. In addi tion he greatly influenced a large number of other students. They recall his clarity, wit, willingness to help, and use of practical examples culled from his experience. As one said, Bill "pulled it all together in a way that made it fun to calculate coefficients and to invert matrices. We wanted to do it because Bill would have been disappointed if we failed."

Bill had a great ability to get to the heart of any statistics problem with virtually no time lost. He was succinct and clear in his teaching and writing. He worked with his graduate students to try to make them understand where the problem formulation and inductive statistics ended and the deductive mathematics began. Bill displayed the great knack for linking the theoretical and the applied that Americans associate with statisticians trained in the United Kingdom, and he was able to explain complicated statistical information to investigators in language they could understand. Consequently he was a much sought-after consultant and an excellent committee member or head. His calm fairness and down-to-earth attitude assured attention to dealing with the core problem.

Cochran limited his committee participation to the amount of work he could handle. He chaired the committee appointed by the American Statistical Association at the request of the National Academy of Sciences to review the Kinsey, Pomeroy, and Martin study of sexual behavior in the human male, work that resulted in a book (1954a). He served as chairman of the Panel of Statistical Consultants, U.S. Bureau of the Census. He served on the committee to consider the effect of battery additives on the life of batteries, on the Academy Committee to the Atomic Bomb Casualty Commission, and on the Committee on Epidemiology and Biometry at the National Institutes of Health. The Subcommittee on National Morbidity Survey of the U.S. National Committee on Health Statistics, of which he was a member, submitted a report to the Surgeon General that was the basis, with little change, of the National Health Survey Act. A smoker, Bill was the only statistician on the Surgeon General's Committee on Smoking and Health.

Bill received many honors. He was at various times president of four major statistical organizations: the Institute of Mathematical Statistics in 1946, the American Statistical Association in 1953, the Biometric Society (which he helped found as a member of the organizing committee) in 1954-55, and the International Statistical Institute in 1967-71. He was elected to the American Academy of Arts and Sciences in 1971 and to the National Academy of Sciences in 1974. He was a fellow of the American Association for the Advancement of Science; honorary fellow of the Royal Statistical Society; and Guggenheim fellow, 1964-65. He received the Guy Medal of the Royal Statistical Society in 1936, the S. S. Wilks Memorial Medal (American Statistical Association) in 1967, and the "Outstanding Statistician" Award (Chicago Chapter, American Statistical Association) in 1974. He was editor of *the Journal of the American Statistical Association* from 1945 to 1950.

Personally, Bill was an unpretentious man with Scottish wit and humor. He was a believer in the fellowship of man, and one of the few things sure to elicit his anger was a bigoted comment. Although he preferred to work by himself rather than to collaborate with others, he was friendly to everyone and liked by all. He and his wife Betty, to the delight of colleagues and students, entertained frequently, and enjoyed square dancing, theater, music, and travel. Hundreds of statisticians from far-flung places attended Bill's retirement dinner in 1976.

The last several years of Bill's life were plagued with a series of medical problems. Nonetheless, after his retirement and his move to his Cape Cod home, he continued to travel, to teach, and to write. He died in Orleans, Massachusetts, on March 29, 1980.

We appreciate the advice and support of his wife Betty Cochran and brother Oliver Cochran, and of colleagues Arthur P. Dempster, John Emerson, Katherine Godfrey, David C. Hoaglin, Augustine Kong, Erich Lehmann, Lincoln E. Moses, Marjorie Olson, Katherine Taylor-Halvorsen, and Cleo Youtz. We have also benefited from correspondence with Richard L. Anderson and Geoffrey Watson and from their writings about Cochran cited in the references.

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J. B. Fisk

James Brown Fisk

August 30, 1910-August 10, 1981

By William H. Doherty

Early in 1876, the hundredth year after the signing of the Declaration of Independence, Alexander Graham Bell invented the telephone in Boston. He exhibited it a few months afterward at the Centennial Exposition in Philadelphia.

Ninety years later James Fisk, president of Bell Laboratories, looking ahead to the telephone's hundredth anniversary, suggested to me, a longtime associate, that a historical volume ought to be planned as a record of the development of telephone science over that period.

Several colleagues and I, going through the "Boston Files" of the earliest years, made an interesting discovery. The first trained scientist hired by the infant Bell company (late in 1885), Hammond V. Hayes, reminded us in many ways of our own Fisk. Hayes and Fisk came from old New England families. Both had studied at Harvard and the Massachusetts Institute of Technology. Both had earned doctorates in physics. But the resemblance ran much deeper, into their innermost personalities, their attitudes, their approaches, and their ways of operating: kindred spirits, aristocratic gentlemen both, born two generations apart.

The thousand-page volume produced in late 1975,[1] the eve of the telephone's centennial, covered the first fifty years (up to 1925, the year Bell Laboratories was incorporated). Fisk does not appear in that volume. He was not with us until 1939. But this memoir is about him, and its preparation has repeatedly recalled the approaches taken by Hammond Hayes in facing up to critical problems—human as well as technical—as the telephone art progressed from its primitive forms. Hayes had quickly seen that the scientific roots of telephony must extend into deeper soil than could be cultivated with the primitive tools of the early electricians and telegraph wiremen, scorned by Lord Rayleigh as "so-called practical men whose minds do not rise easily above ohms and volts."

The invention of the telephone had stirred up an intellectual ferment in the world of engineering and physics concerning electric waves and oscillations. Hayes, while facing a host of practical and "earthy" problems, sensed the need for a cadre of keen, academically trained minds. His first discovery was John Stone Stone, recruited from Johns Hopkins in 1890 through the recommendations of the renowned physicist Rowland, then on the Hopkins faculty. Following Stone came Campbell from MIT (with additional training at Harvard, Paris, Vienna, and Göttingen); Colpitts from Harvard; Pickard from Harvard and MIT; and Jewett from Chicago, brought over from the MIT faculty. These were the bright lights of the earliest days; their contributions, inspired by Hayes, demonstrated convincingly the importance of fundamental knowledge. Thus the pattern was established long before there was a corporate Bell Laboratories with Frank B. Jewett as its president (1925). And to Jewett's successors, of

[1] *A History of Engineering and Science in the Bell System: The Early Years* (*1875-1925*) (Murray Hill, N.J.: Bell Telephone Laboratories, 1975). Six additional volumes have completed the series.

whom James Fisk was the third, there has been no higher priority than to engage and stimulate the best intellects.

There had been no scientists in the immediate Fisk family. James, his sister Rebekah (Becky), and younger brother George were born in West Warwick, Rhode Island, to the southwest of Providence. Their parents, Henry James and Bertha (Brown) Fisk, natives of Providence, had been charmed by the Far West during a wedding trip. They subsequently took the children to Tacoma, and later to Long Beach, for their primary schooling. The elder Fisk was a sales manager in the canning industry; and when the mother—a beautiful lady and talented violinist—died as the children were nearing high school age, he contemplated going to Alaska for better business opportunities. At this point the maternal grandparents, the George Tilden Browns of Providence, urged that the children be placed in their care for their high school years. Judge Brown had retired as presiding justice of the Superior Court of Rhode Island. Becky writes that his whole life thereafter was devoted to his three grandchildren and their education. "They spoiled us and at the same time were very strict. . . . He would quiz us in the evenings after study time. . . . Gramp's greatest delight was seeing good grades on our report cards. Jim's were always the best and required the least effort." The boys were sent far across town to Providence Technical High School in preference to nearby public or private schools.

James entered the Massachusetts Institute of Technology in 1927, when he was barely seventeen years old. It was in January of that year that telephone service had been established across the Atlantic. For the first time it was possible to place a telephone call to London or Paris. It was not done by cable; the cable was nearly thirty years in the future. The medium was high-power, long-wave radio, the wave being transmitted from tall towers at Rocky Point, Long Island.

Two of the key people involved, Mervin J. Kelly and Ralph Bown—Kelly in the fabrication of powerful radio tubes, Bown in the painstaking study of wave propagation over the great circle route—would one day be Fisk's mentors at Bell Laboratories. They were physicist-engineers, and he would succeed both of them.

But even more glamorous, in May of that year, was another conquest of the Atlantic, the solo flight of Charles Lindbergh from New York to Paris. On his return the young aviator was acclaimed in many parades. One of these—which I witnessed, and Fisk was probably there—was from Boston through Cambridge along Massachusetts Avenue, passing MIT, which already had a vigorous program in aeronautical engineering, boasting an advanced design of wind tunnel. This was the field that appealed most to Fisk, and he pursued it enthusiastically, graduating with high marks in 1931.

The senior album of the MIT class of 1931 depicts Fisk as very active in extracurricular affairs, from smokers, proms, and field days through ROTC and varsity athletics (track and cross-country). A member of Kappa Sigma fraternity (as his brother George was to be, following him by three years), Fisk made Tau Beta Pi and was secretary of his class for five years following graduation. "Jim had a quiet dignity," writes a classmate, "that brought him many assignments, always discharged in a friendly manner and displaying uncommon ability."

As an aeronautical engineering student, Fisk came to know and work with Charles Stark Draper, a Stanford and MIT alumnus, a graduate student and faculty member specializing in aircraft instrumentation. In their work in the engine laboratory Draper became impressed with Fisk's astuteness and depth and urged him to become more involved in pure physics; in a postgraduate year as a research assistant in aeronautics Fisk did develop a strong interest in atomic physics, which led to a Redfield Proctor Travelling Fellowship for study in England. Redfield Proctor, MIT '02, former governor of Vermont, and long-time member of the MIT Corporation, had established these fellowships in the interest of promoting international student exchange.

Fisk's grant was for the year 1932-33 at Cambridge, with residence at Trinity College. This was a time of great excitement in British physics. It was in 1932 that Chadwick discovered the elusive neutron. And with the reputation of the Cavendish Laboratory for Experimental Physics—where Sir J. J. Thomson in 1897 and "discovered" the electron (that is, measured the charge-to-mass ratio e/m)—and of its director, Sir Ernest Rutherford, hailed as "the greatest experimentalist since Faraday," who had in 1910-11 established the minuteness of the atomic nucleus—there could not have been a more felicitous assignment for a lively and personable young American. Fisk appears to have relished it. He requested, and was granted, an extension of the fellowship into a second year. Among the friends made in England during that period, besides Rutherford (who died in 1937), I remember John Cockroft, who was lecturing in physics. Sir John remained in close touch with Fisk for many years.

After completing his second year (1934), during which he published two Royal Society papers (one with a coauthor) relating to the conversion coefficients of gamma rays, Fisk returned to the States to work at MIT for his Ph.D., which he received in 1935. The subject of his dissertation was "The Scattering of Electrons from Molecules," a topic suggested by Professor Philip Morse, who took a constant interest in the study.

Quantum theory had already accounted for most of the phenomena observed in experimental studies of the "collision cross-section" of atomic gases when bombarded by beams of electrons. In Fisk's thesis the theory was extended to the case of diatomic molecules, and the results compared with experimental observations on H_2, N_2, and O_2. The results were in reasonable accord, considering the rough assumptions that had to be made concerning the molecular potential fields; the most noticeable departures were attributable to inelastic collisions due to the low energy of excitation in H_2.

Following an additional year at MIT as a teaching fellow in physics, Fisk received an appointment as a junior fellow at Harvard. The Society of Fellows had been established through a gift from President Lowell. It included a small group of young men and women of exceptional ability, originality, and resourcefulness who were given residence, plus a stipend, with no specific requirements as to what they should study or teach. It was a happy and challenging situation for Fisk. Only twenty-six years old, he enjoyed living in Lowell House, one of the first three "colleges" newly built under Harvard's House Plan, down by the river, with the added privilege of dining informally once a week with the senior fellows. To add to the enjoyment of his first year, 1936 was the tricentennial year of Harvard's founding, a colorful year climaxed by ceremonies in September attended by many noted scholars and Nobel prize winners (including Eddington) from foreign countries. The University of Cambridge (the mother of Harvard) sent representatives from Fisk's Trinity College, from Kings, and from Emmanuel (John Harvard had been an Emmanuel man).

A hundred years before, at the bicentennial, Emerson had written:

> . . . Cambridge at any time is full of ghosts; but on that day the anointed eye saw the crowd of spirits that mingled with the procession in the vacant spaces, year by year, as the classes proceeded; and then the far longer train of ghosts that followed the company, of the men that wore before us the

> college honors and the laurels of the State—the long, winding train reaching back into eternity.

Thus Fisk became, in spirit, a Harvard man as well as an MIT man and a University of Cambridge man. As we came to know him a few years later, he was all of these—quietly, unostentatiously, but always generously.

A friend from MIT days, Ivan A. Getting, had become a Harvard junior fellow a year earlier. As an MIT freshman, Getting had had Fisk as his ROTC platoon commander. When Fisk, as a graduate student, had switched his interest to theoretical physics, which was Getting's field, the two had worked out problems together. Getting had then, after graduation, been awarded a Rhodes scholarship and studied physics at Oxford, receiving his Dr. Phil. in 1935.

Fisk brought with him to Harvard some of the designs for Van de Graaff electrostatic generators as evolved at MIT, and he and Getting proceeded to build an improved and compact machine for accelerating protons and deuterons up to 500,000 volts. The generator was not entirely completed when Fisk left the Society of Fellows two years later, and Getting continued its construction with the aid of a graduate student. There were two *Physical Review* papers coauthored by Fisk on features of the generator and its use in the physical laboratory.

Fisk's departure in June 1938 coincided with the termination of his celibate life. Shortly after his return from England in 1934 he had met Cynthia Hoar, a Concord (Massachusetts) girl whose family, like his, had a long New England background. They had met at Saint-Sauveur, P.Q., on a weekend of skiing, a sport relished by both; and their mutual interests, to be shared for nearly forty-seven years, included music. Cynthia was a pianist, and after Concord Academy she had attended the New England Conservatory and had studied for a year in Germany. Jim, Cynthia tells me, was a clarinetist (since high school days), and a good one. In later years at Bell Laboratories, characteristically, he never allowed us to suspect this endowment. Hammond Hayes had been like that: self-effacing, not seeking the limelight; a scholar talented in more ways than anyone knew.

Following a June wedding and a trip to Europe, Fisk and his bride moved to Chapel Hill, where he had accepted an associate professorship in physics at the University of North Carolina. He had presented a paper there at a National Academy of Sciences meeting in May on disintegration of nuclei by high-energy radiation—a topic of much piquancy, coming on the eve of disclosures from Europe on nuclear fission and the possibility of chain reactions. But after one academic year, the long arm of Mervin J. Kelly, director of research at Bell Laboratories, reached out and brought Fisk into the department Kelly had recently headed, now run by J. R. "Ray" Wilson, director of electronics research. Kelly, urgently seeking to build up the staff in modern physics, had heard about Fisk from William Shockley, who had joined Bell Laboratories after collaborating with Fisk at MIT in 1935-36.

Wilson, an alumnus of Reed College, Cal Tech, and Columbia, was a superb administrator. For all the shabbiness of their headquarters—a former biscuit factory in downtown New York—his and Kelly's men had produced some remarkable electron tubes. Their devices ranged from the world's tiniest (for the first electronic hearing aids) to a 250-kilowatt water-cooled monster—the world's largest triode, seven feet high—for super-power broadcasting. They had also furnished high-power tubes to J. R. Dunning at Columbia University for his first cyclotron.

Fisk's first supervisor was physicist J. B. Johnson, softspoken and gentlemanly, developer of the first practical cathode-ray oscilloscope tube, and famous for his analysis of electron noise in vacuum tubes and his identification of the *Wärmeeffekt* in electrical conductors, which became known as Johnson noise.

But the emphasis in Wilson's laboratory in the mid-thirties had been shifting toward the high radio frequencies, partly in support of new communications ideas and partly as our awareness of Churchill's "gathering storm" in Europe suggested new uses of radio that could be of military importance. One of these, the detection and tracking of ships and airplanes by means of pulsed radio beams—not yet called radar—was already being pushed in Army and Navy laboratories in the United States and Britain. In 1938 a program sponsored by AT&T, but at government request, was begun in secret in the radio laboratory of Bell Labs at Whippany, New Jersey.

William C. Tinus and I were put in charge of this work, and we immediately jumped to the 600-700 megahertz range, three to four times the frequency employed anywhere else, in order to achieve narrower radio beams for better angular precision and resolving power. We were encouraged by the work of Wilson's very clever physicist-engineers on highfrequency tubes and by the expertise in microwaves being developed for forward-looking Bell purposes by radio research engineers at our Holmdel laboratory under Harald T. Friis.

This is where Fisk came in. There was a crucial need for more transmitted power to increase range. At 700 MHz we could not get more than a kilowatt from any existing tube, even on a "pulsed" basis. We were being pressed by the Navy to go to even higher frequencies for still narrower beams, and by the Signal Corps to undertake a project called "bombing through overcast" that would require scanning the terrain or ocean from the air with the narrowest possible beam.

On October 6, 1940, Wilson and Fisk, accompanied by Kelly, were at our Whippany laboratory to witness tests on a new invention brought over in secrecy from England, the multicavity magnetron.[2] We had been alerted, and my colleague Russell Newhouse, co-inventor of the first radio altimeter, was prepared with a test setup that included a powerful electromagnet. He had built this to test an experimental 3,000-MHz (10-cm) oscillator devised by another of Wilson's ingenious tube men, A. L. Samuel.

The results with the British magnetron were astonishing. An outwardly simple device, it delivered bursts of 10-cm power roughly estimated at 10 kilowatts.

The radar picture changed overnight, and Fisk was commissioned immediately by Wilson and Kelly to set up a group to hand-produce 10-cm magnetrons as quickly as possible for use in planning new radars; to find out how to "scale" the magnetron to the 40-cm range so that it could be used immediately to beef up the radars already designed and being built in Western Electric factories for use on battleships, cruisers, and destroyers; and to solve the many fabrication problems associated with a device so radically new and not yet completely understood.

Within two months of the demonstration, but with Pearl Harbor still a year away, sample magnetrons had been made. As the months passed, under great pressure from the radar

[2] The body of the magnetron was a copper block—the anode—having a central hole with a cylindrical (indirectly heated) cathode located axially, plus six or eight surrounding holes connected to the central hole by narrow slots. The holes (plus slots) being essentially quarter-wave resonators, the iterative structure would support a wave traveling circumferentially, provided it could be reinforced by a circumferential movement of electrons at the right speed. This was accomplished by employing a strong transverse magnetic field so that the electrons emitted from the cylindrical cathode, instead of moving radially toward the anode, would be forced to follow a spiral path. The circumferential component of this motion (modified by its interaction with the fields at the successive slots) was then the source of microwave power.

development engineers and the hard-driving Kelly, Fisk's team proceeded with magnetron designs for manufacture, at the same time advancing in theoretical understanding of the intricate electron dynamics. Of the many inventions related to magnetron development, four resulted in patents issued to Fisk himself. His two physicist coworkers from the outset, with others soon added, were Paul Hartman from Cornell and Homer Hagstrum from Minnesota. A paper authored by all three was published after the war (1946) to cover the practical as well as theoretical aspects of the work, not only in Wilson's department but in many other contributing groups.

Wilson's laboratory, with splendid shop facilities and highly imaginative physicist-engineers already active in the new electronic art of "bunched" beams and resonant cavities, was a propitious environment. With the long wartime working day, six days a week (stretched out to twelve hours for train-and-ferry commuters from northern New Jersey), there was another fortunate ingredient—an esprit and dedication, along with the seriousness. Emanating from Wilson himself, and augmented by a prankishness going down the line—in which Fisk was often the ringleader and provocateur—this spirit was contagious and made everyone, including wiremen, mechanics, and clerks, an enthusiastic partner. Looking back on that period many years later, when vacuum tube research had moved from downtown New York to more sanitized and university-like quarters in New Jersey, Fisk reminisced in a speech to old veterans that "the sweet bakeshop aroma that hung over from the old biscuit factory may have inspired us to pump better vacuums," and suggested that "our instincts to be inventive may have been sharpened by the man-eating flies that shuttled between our place and the stables of New York's mounted police a half block away."

Encouraging to Fisk and his colleagues were reports from the armed forces on successful engagements—land, sea, and air—in which radars powered by their magnetrons had been decisive.[3]

Soon after the magnetron project was started, the National Defense Research Committee (NDRC)[4] established the MIT Radiation Laboratory, with the aim of mobilizing the nation's universities for defense. As Kelly's emissary in promoting collaboration between Bell Laboratories engineers and the staff there under Lee DuBridge, I found one of DuBridge's group leaders on gunfire-control radar to be Fisk's old MIT-Harvard friend, Ivan Getting; while his other MIT friend and mentor, Stark Draper, was inventing a lead-computing gunsight for naval machine guns, for which we at Bell Labs were designing an antiaircraft radar.

This fruitful collaboration included magnetron development, and as the war continued and it became possible to build magnetrons for even shorter wavelengths (3 cm and 1.25 cm), specialists from both the MIT and Columbia Radiation Laboratories joined forces with Fisk's group and made contributions of great value. These advances included very large improvements in power output and in frequency stability (the absence of unwanted modes of oscillation), plus the feature of tunability, technically difficult but quite valuable in an operational radar system.

Radar was a decisive element in the prosecution of World War II, and the British-invented magnetron, developed for

[3] The Navy Bureau of Ships, which had cognizance of shipborne search radar, including torpedo-directing radar for submarines, was especially diligent in reporting on submarine-based radar (the 10-cm SJ, followed by the 3-cm SS). One report cited a nighttime engagement in the Pacific in which fourteen torpedoes, in conjunction with the Navy's torpedo data computer, were used to sink seven ships in a Japanese convoy in the space of a few minutes.

[4] Serving with Vannevar Bush, chairman of NDRC, was Frank B.Jewett, president of the National Academy of Sciences and soon to retire as president of Bell Laboratories.

quantity production by Fisk and his colleagues, was its crucial component. The enemy had nothing comparable. The Presidential Certificate of Merit, recognizing Fisk's vigorous leadership, came in 1946. Even before this, with the war ending and still in charge of the magnetron group under Wilson, Fisk had been given a parallel assignment under Harvey Fletcher, director of physical research. One of the stars in Fletcher's department was C. J. Davisson, Nobel prize winner (1937) for his demonstration of the duality of electrons and waves. The contributions of Fletcher's men to achievements in Wilson's area, including magnetic structures for magnetrons, had been notable. It was Kelly's view, with Fletcher's retirement only a few years away, that Fisk could bring new strength to an area that was close to Kelly's heart—the fundamental properties of materials and the physics of the solid state.

As assistant director under Fletcher, Fisk organized a solid state physics group that only two years later was to come up with the epochal invention of the transistor—another Nobel achievement. He also set up a research activity in electron dynamics to provide a continuing background in fundamental theory for the more developmental type of work on microwave tubes that was increasingly engaging Wilson.

The war's end had allowed Bell people, emerging from some of their all-out military commitments, to think again about their own business. Many things urgently needed doing. To Ralph Bown, a Cornellian with a long background in radiophysics and wave propagation who had succeeded Kelly as director of research, there was one area especially where the time was ripe and the technology ready: the plunge ahead on a nationwide system of microwaves, beamed from tower to tower, with a capacity for thousands of telephone channels, plus network television.

Thus the postwar Bell Laboratories was an exciting place. So, too, was the Fisk household in Madison, New Jersey, which included three lively young boys and a grand piano—a Steinway, the gift of Cynthia's Massachusetts parents. Accordingly, we who had been close to Fisk and observed the increasing responsibilities given to him by Kelly and Bown were surprised to learn late in 1946 that he was leaving us to become a professor of physics at Harvard. We knew the academic life had always appealed, and that the blandishments of the Harvard physics faculty could be persuasive. At first we suspected that a bit of nostalgia for the Cambridge Concord environment was also involved, but this was not the motivation. Fisk was simply not ready to commit himself to a career inevitably leading to the management of research, or research and development, rather than the personal involvement as a scientist that had brought him such satisfaction.

The move to Harvard was delayed for a year to enable Fisk to respond to an urgent request from the newly formed Atomic Energy Commission to be its first director of research. In this capacity he was influential in emphasizing the role that should be assigned to basic research, as distinguished from reactor development, and introduced several programs to include such fundamental work in the AEC's plans (later he was to serve for six years, 1952 to 1958, as a member of AEC's General Advisory Committee).

After spending much of 1947 in Washington, with residence in Alexandria, Fisk was able to take on his Harvard commitment and to live with his family in historic, whitesteepled Concord, the home of Emerson and Hawthorne, and the locale of Thoreau's Walden Pond—"a gem of the first water which Concord wears in her coronet"—where Cynthia had gone swimming as a girl.

The Harvard appointment was to the Gordon McKay Professorship in Applied Physics, along with which Fisk was given an honorary A.M. and, in 1949, made a senior fellow in the Society of Fellows. The university catalog listed his courses as Elements of Mechanics (classical mechanics, for undergraduates and graduates) and Electron Physics, a reading and research course for graduate students.

Fisk's students gave high ratings to his lectures, but they also appreciated his mischievous dry wit, already so well known to his Bell friends. On occasion he would invite a student to accompany him to a Red Sox ballgame at Fenway Park, winding up the day with a round of his favorite cigars, Corona Belvederes.

In a neighboring office was Edward M. Purcell, also teaching physics and another veteran of strenuous war years. Two years earlier he had observed the phenomenon of nuclear magnetic resonance (NMR), for which he and F. Bloch of Stanford would receive the 1952 Nobel Prize in physics. Purcell writes concerning Fisk that it was "a joy to be able to talk with him about anything from freshman physics to high technology, and to draw from that deep reservoir of humane wisdom. . . . How great was Harvard's loss when Jim left we have of course no way of measuring. I often thought he might have become, and would have made, a great president of the university."

But Fisk did leave, after one year, despite his love for academe; this time the challenge presented by Kelly and Bown was irresistible. Fletcher was retiring in the summer of 1949, and Bown confided to Friis and me: "We're getting Jim back; and our idea is that he would eventually move into my job. I presume this would be agreeable to both of you." It was, with no reservations. An old friend was rejoining us.

In telephony there is a subtlety in the end product. The end product is human communication, not hardware. This subtlety seems to offer a glamour of an intellectual sort to intrigue an inquisitive mind. Thus a keen physicist quickly catches on to the fundamentals of telephony's dominant technologies, transmission and switching.[5] These are the fields requiring the greatest amount of organized engineering manpower, yet continuously sensitive and responsive to new ideas.

This was what Fisk came into in mid-1949 at our new headquarters at Murray Hill, New Jersey. His direct responsibility was for research in the physical sciences. But his broader assignment, as Bown's and Kelly's heir apparent, was to encourage communications researchers like Friis and me, in trying to envision the telephone system of the future, to look even farther beyond the horizon.

There are near horizons and far horizons. In the early nineteen-fifties we were looking ahead to the circular waveguide, using millimeter waves and providing a quarter of a million voice channels, as the long-distance medium of the future, at least over land routes. About 1955 John Pierce, an electron dynamicist of extraordinary imagination who, like Fisk, had worked under Wilson, made the audacious proposal that we communicate across oceans by means of microwave beams directed at orbiting satellites. And from over an even more distant horizon there beckoned optical fiber transmission—though with little hope, until the nineteen-seventies, for any but short distances. To all of these approaches, Fisk—advancing to vice president for research in 1954 and executive vice president in 1955—gave enthusiastic support and encouragement.

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[5] The term *transmission,* understood as the faithful transport of large bundles of voices over long or short distances, speaks for itself. The term *switching,* with its suggestion of the railroad yard, unfortunately conveys no notion of the fascinating complexity and intellectual challenge of this field. The French term *commutation* is scarcely better. The Germans at least employ *wählen*—to choose—for the dialing process. A German engineer could easily fashion a word—perhaps *Selbstwahlvermittlung*—to indicate what telephone switching really is: the prompt implementation of personally designated choices. Today, the choice is of one destination in a hundred million, handled in seconds.

In the equally important area of telephone switching, it had seemed to Kelly that the ultimate would be attained when the hundreds of millions of electromechanical contacts of the newest switching system, known as crossbar, could give way to electronic crosspoints. This, Kelly thought, would be a crowning achievement of solid state physics. It was not to come out that way. From over another horizon came the concept of "stored program control"—the idea of employing vast memories, with instant access thereto, whereby a great variety of new optional services, changeable on demand, could be provided to the telephone subscriber with no need for physical changes in the central office. This was the huge development program known as ESS (electronic switching systems); it was implemented in the 1960s and 1970s in thousands of central offices, using crosspoints that were still electromechanical, though miniaturized and highly refined. The ideal solid-state crosspoint, because of very severe requirements, did not appear until the 1980s.

In a mission-oriented laboratory of thousands of trained scientists and engineers, many of them with decades of experience, the prime requirement of a top executive is not inventiveness but leadership, a leadership that will bring out the best through inspiration and encouragement. It was for this job that Kelly wanted Fisk, and it proved to be Fisk's special genius. A problem he tackled early—and "head-on" (his favorite adverb)—was to develop a much-needed understanding amongst professional personnel of the company's policy on merit and rewards. At Kelly's behest Fisk and Frank Leamer, seasoned director of personnel under two administrations, formulated a statement of salary policy, including a graphic merit scale, that was available to any technical staff member for discussion with his superiors. The document was so clear, straightforward, and unequivocal that it evoked wide commendation in the personnel management world and was copied in many organizations.

Fisk was also strong on environment, the need for an atmosphere that encourages each scientist and engineer to use his talents to the utmost. "It takes an environment of stimulating associates, some of them patient, some impatient, some who sparkle brilliantly and some quietly persistent; individuals painstakingly selected over the years to insure mutual respect and establish a balance in their integrated skills." This statement was made in a 1966 address at the Southern Research Institute in Birmingham. And on a different point, moments later:

> Scientific advance comes, in large part, from interchange of knowledge with the world outside, with the academic world and with scientists and engineers of attainment and stature who are hammering at problems related to one's own. It is impossible to retain gifted men unless they are given freedom to discuss their work with others of renown in the scientific community, and the pass-key to that community is one's own prestige, attained through publication of results. Accordingly, it is short-sighted policy to delay or restrict publication beyond the very minimum required for patent applications, or discourage in other ways the driving urge of good scientists to be known and respected in their professional circles.

No predecessor or contemporary in Bell Laboratories—or perhaps anywhere—held these views more strongly than James Fisk, or was more unswerving in their implementation. They were the views Hammond Hayes held sixty years before: that the research support for a science-based industry must have the best people obtainable, must have its goals (in broad terms) clearly understood, and must provide an environment that will motivate and inspire toward their achievement. A part of this last was the recognition that there are some scientists who will do their best work when not constrained by rigid rules. A part of it was the deliberate bridging of departmental barriers, to promote collaboration between the disciplines (example: the solar battery, invented by a physicist, a chemist, and an engineer). Fisk was eloquent on this: "To achieve this necessary interaction it is not enough to rely on thermal diffusion, so to speak, across the interfaces. It must be worked at; it must be cultivated."

This concern was evident in his almost daily appearances in one department or another and his genuine friendliness toward people down the line, which continued after he assumed the presidency, succeeding Kelly, in 1959. In 1964, as plans were being made to add new "branch laboratories" located at Western Electric manufacturing plants—several of these having been highly successful—Fisk was gravely concerned lest this "decentralization" might be carried too far. He enjoined his colleagues to preserve at all costs, as he expressed it at our annual executive conference at Seaview that autumn, "the blessings of unity and compactness and close personal contact that have made it so easy for us to pull together and act as one Bell Laboratories."

Unsparing of himself in the interest of his government, in mid-1958 Fisk accepted an appointment by President Eisenhower to head a delegation of scientists to go to Geneva to lay the technical groundwork for a nuclear test ban treaty with the Soviets. It was something new in international negotiations for scientists to find themselves in such a role, knowing that the final decisions would be in the hands of the diplomats. Fisk earned high praise for the rare combination of skill, firmness, and tact with which he dealt with the Russians and their Moscow-dictated intransigence. The principal issue was the problem of verification, wherein it was necessary to agree on an adequate number of test stations to monitor noncompliance. In a second conference in late 1959, where Fisk and his partners presented indisputable evidence that far more test stations would be required than the Soviets would agree to, the delegates came virtually to a dead end. "It is quite impossible," wrote Frank Press,[6] then a professor

[6] Frank Press, "Scientific Aspects of the Nuclear Test Ban," *Engineering and Science* (December 1960):26-36.

at Cal Tech and a member of the delegation, "to feel secure with a treaty that allows too few inspections."

During the fourteen years of his presidency, while he continued to serve the government in many ways, as well as the cause of higher education, Fisk guided the Laboratories through some major developments. Perhaps most spectacular among these was the satellite program, beginning with the passive reflecting balloon *Echo* launched in August 1960 in collaboration with NASA and its Jet Propulsion Laboratory. And in the closing minutes of 1961, even as the big balloon made its 6,232nd orbit around the earth and sailed on into 1962, an advanced type of electronically equipped satellite, *Telstar,* complete with receiver, transmitter, and solar batteries, was receiving ground tests at Bell Laboratories for testing in space. It was still too early in the space vehicle art for geo-stationary orbits at 22,300 miles; and there were some worries about such an orbit, including the concern about the time delay (a half-second on each round trip), which could cause two fast talkers to become entrapped in their own rudeness. "If we cannot in the near future increase the velocity of light," quipped Fisk, "can we with some subtle attachment, not seen by the impatient user, soothe his impetuosity for those few minutes till he finishes his call. . . . so that communication by satellite may be smooth and uninterrupted not only for the chivalrous and gently bred, but for the rest of us as well?"

Intercontinental telephony by satellite, as is well known, passed from the hands of the Bell System to the Communications Satellite Corporation, organized by the government, and overseas telephone traffic has been shared between Comsat's facilities and AT&T's deep-sea telephone cables. The first of these (with thirty-six voice channels) had gone into service in 1956, using oceanbottom amplifiers ("repeaters" in telephone engineering jargon) every forty miles. Under Fisk's leadership the capacity of these systems, using transistors, increased to 800 channels, with a new 4,000-channel system envisaged before he retired.

The Fisk home in those years was a rambling farmhouse in New Vernon, New Jersey, with five acres for indulgence in a hobby Jim called "farming in miniature." One could drop by on a Saturday and find him riding jauntily over the furrows or adjusting a newly sharpened sickle bar on his tractor, but ready for a plunge with a guest in the Fisk pool, followed with a round of cigars. The Fisks loved the countryside, and Cynthia, having taught piano, conducted children's concerts for eleven years in nearby Morristown. One of the delights for the Bell Labs executive group known as the "cabinet" was a social hour and buffet at sunset time, after which some two dozen of us, plus wives, having participated in the Fisk largesse, could sometimes prevail upon Cynthia for a brief musicale. Many engineers are music lovers; I think telephone engineers especially, perhaps because through the science of sounds we know what music is "made of."

Fisk chose to retire from the presidency in 1973 at age sixty-two, remaining as board chairman for another year. His successor as president, Princetonian William O. Baker, a renowned physical chemist, Priestley medalist, and Perkin medalist, had joined Bell Laboratories in 1939, the same year as Fisk. Baker's contributions to the sciences of physical materials assured that the intricate bondings of atoms and molecules being elucidated by physicists in collaboration with chemists and metallurgists would bring into practical use new materials of scarcely hoped-for properties of benefit to communications and to industry at large.

In 1975 a signal honor and lasting tribute was paid to Fisk by the establishment of the James B. Fisk Merit Scholarship. Presented annually to outstanding boys and girls who are children of employees, the scholarship recognizes academic excellence, high character, and leadership, qualities Fisk respected and encouraged.

Fisk's retirement years saw continued advances under Baker and his successor, Ian M. Ross, English-born, from the University of Cambridge. Most dramatic were the microminiaturizing of complex circuitry (the new era of "chips") and the breakthroughs in optical fibers, which with lasers and other devices in the new art of "photonics" are providing a new long-distance communication medium of extraordinary capacity.

Less spectacular, but likewise affording Fisk much satisfaction, was the continued emphasis by Baker and Ross on a program Fisk had initiated, the application of computerbased systems to the complex operating problems of the telephone companies, with huge savings in manpower and expense.

Before the recently enacted divestiture, Harvard Dean Harvey Brooks wrote that "The Bell System represents the best example of a highly integrated technical structure in a high-technology industry and is widely regarded as the most successful and innovative technical organization in the world."[7] Although the System is now broken up, Ross is determined that the scientific quality and the innovativeness that his predecessors sponsored—as recognized in two more Nobel awards under the Baker and Ross regimes—shall remain undiminished.

The Fisks, while retaining their New Jersey home after retirement, were able to spend more time at Keene Valley in their beloved Adirondacks. To them the Adirondacks were what New Hampshire was to poet Robert Frost: not a place on the map but a region of the mind. And each year there was a trip to Europe to see friends, visit the universities, and

[7] Harvey Brooks, "Knowledge and Action: The Dilemma of Science in the 70's," *Daedalus* (Spring 1973): 125-43.

talk with representatives of companies on whose boards Jim served.

I last saw Jim Fisk in New York at the Harvard Club—to both of us, at our age, a place of refuge in a perilous city. We had lunched with some Japanese guests who had been gracious to us in Tokyo. In parting we talked about another get together to discuss some speculations of Harvard's late Percy Bridgman on the Second Law. But this was not to happen. After visiting Spain in the spring of 1981, Jim and Cynthia were vacationing in August in the Adirondacks when he suffered, unexpectedly, an aneurysm in the abdominal aorta that he was not able to survive. His death on August 10, in neighboring Elizabethtown, came three weeks before his seventy-first birthday.

Faithful colleague Frank Leamer, hurrying over to Keene Valley from Saranac Lake for the services at the Congregational church, paid a warm tribute shared by all Bell people. Speaking of Jim Fisk as not only a distinguished scientist but a great humanitarian in his quiet, unassuming, and modest way, he recalled that Jim was also "a great nature lover and outdoor man. We often shared experiences in the wilderness and seldom-trod areas. He used to bushwhack to the mountain tops instead of following the beaten path."

A resolution of the Corporation of MIT, of which Fisk had been a member for twenty-two years and had become a life member, spoke of him as "a princely human being of uncommon modesty," who was "as much at home in the university as he was in the corporate boardroom, laboratory and the high offices of government." The resolution also lauded his personal generosity and strong support in major capital drives and his leadership in the selection of three successive Institute presidents.

Following the passing of her husband, Cynthia Fisk moved from New Jersey to Boxborough, Massachusetts, a few miles from her native Concord, purchasing a villa-type house on a hilltop with gardens and play-space for grandchildren. She continues with her piano in a local chamber music group. Living in this area she is able to see two of her sons often—Samuel, out of Brown and the Columbia Business School, with overseas experience in the Peace Corps, and now in psychological counseling, with an office in Cambridge; and Charles, a graduate of Harvard and of the Yale School of Music, a concert pianist and teacher of piano, music theory, and the history of music at Wellesley. Son Zachary, from Harvard and the University of California, is farther away, a distinguished young physicist at Los Alamos.

All of the Fisk family know that to Jim's associates he was not only a leader but a warm friend, a blithe spirit moving amongst us, giving added life to a dynamic profession; personifying the spirit of noblesse oblige; one of the noblemen of our time.

This memoir, written from a retirement haunt in the deep South, has benefited from notes graciously furnished by Cynthia Fisk, up in New England; by Dr. Fisk's sister Becky, Mrs. William Wilkinson, in Laguna Hills, California; and from the aid of an indefatigable lady at Bell Laboratories, Ruth Stumm, faithful researcher and transcriber.

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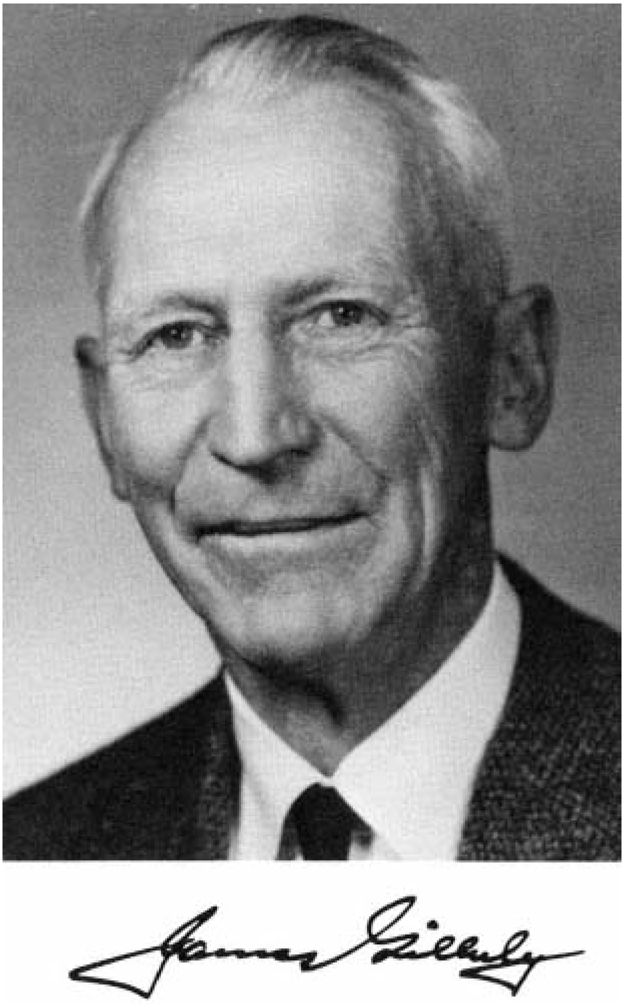

James Gilluly

June 24, 1896-December 29, 1980

By Thomas B. Nolan

Aaron Waters, in a "portrait" published more than ten years before James Gilluly's death, characterized him as a "pioneer of modern geological ideas."[1] The independence of thought implied by this characterization came naturally; both Gilluly's father's and mother's grandparents had immigrated to the United States as rebels against repressive or unpopular regimes.

Gilluly was born in Seattle, Washington, June 24, 1896, the son of Charles Elijah Gilluly and Louisa Elizabeth [Briegel] Gilluly. Charles Gilluly's grandfather had been a disciple of Robert Emmett and had left County Galway in Ireland in 1793; as Gilluly expressed it, his great-grandfather was "luckier than Robert, who was hanged." Successive moves from New York State to Michigan and to Kansas led the family to the State of Washington in about 1890. Louisa Gilluly was descended from a German emigré family that escaped from Wurtemberg as a result of an abortive attempt to set up a republic in 1830. The family settled in East Saint Louis, where Louisa was taught in German until she went to high school. Her family moved to the Kittitas Valley in Washington in 1890.

[1] Aaron C. Waters, "Portrait of a Scientist: James Gilluly. Pioneer of Modern Geological Ideas," *Earth Science Reviews/Atlas,* 5(1969):A 19-A27.

Much of Gilluly's early life was spent in Seattle, with brief intervals in British Columbia, when his father was employed there, and on his mother's family ranch in eastern Washington. Even early in life he was a voracious reader, and in both grammar and high school he was fortunate in having excellent teachers who recognized and encouraged his capacities (he regularly enrolled in courses for extra credit). His high school career was marked by his selection as valedictorian of his class; in addition, he was captain of the football team, editor of the school class book, and chairman of the junior prom.

Gilluly's mother died during his last year in high school, and he lived with his father in Calgary, Alberta. The outbreak of World War I brought him back to the States, where he lived on his uncle's farm until he entered the University of Washington in the fall of 1915. His university career, however, was interrupted from time to time, partly by the necessity of meeting his living expenses and partly by enlistment in the Navy when the United States entered the war. During this period, Gilluly worked in the mines at Butte, Montana, on surveying parties, in the steelmills near Spokane, and as a stevedore on the Seattle docks, experiences that made him, in spite of his relatively small stature, an effective end on the Washington football team. (He also was a member of the basketball team and manager of the track team.) In addition, he was active in fraternity affairs, managing the house as well as participating in the social life.

At the end of the war, Gilluly was acting as instructor of newly enlisted sailors as a noncommissioned petty officer. He received a commission as ensign at the cessation of hostilities.

Throughout this unusually busy period, Gilluly continued his voracious reading in an amazingly wide range of subjects. This was in addition to the course work that marked successive majors in civil and mechanical engineering, business eco nomics, and, finally, geology. This last shift was made in his senior year, at least in part because of the influence of a fraternity brother who later became a distinguished petroleum executive.

Gilluly's initial venture into geology after graduation from the University in 1921 was, to a degree, an unhappy one and was followed by an equally unsatisfying experience in insurance. He had, however, taken a civil service examination for junior geologist while a senior in college, and in the spring of 1922 he was offered a part-time assignment with the U.S. Geological Survey in Washington, D.C.

Here he began many lifelong associations and friendships. With M. N. Bramlette and W. W. Rubey, he enrolled in part-time graduate studies at Johns Hopkins University. The next year, again with Rubey and Bramlette, he continued graduate work at Yale University. Here he made further friendships, both with fellow students (W. H. Bradley, G. G. Simpson, and others) and with a faculty that included Adolph Knopf, Charles Schuchert, Chester Longwell, and H. E. Gregory, each of whom influenced him significantly.

Following completion of his graduate work, he was given a series of field assignments with the Geological Survey. His first independent project was to investigate the geology of part of the North Slope of Alaska—an area that was beginning to be of interest for its petroleum potential. It was a strenuous and trying introduction to Geological Survey fieldwork in a harsh and unknown environment. Gilluly, after arrival at Point Barrow by boat from Seattle, started out with canoes and two young Eskimo assistants along the Arctic to map one of the larger rivers flowing northward into the Arctic Ocean. The North Slope of Alaska here is flat and featureless; with the young Eskimos, who were unfamiliar with the area, the party entered the Topogoruk River, rather than the larger Ikpikpuk, which had been the objective of the pro gram. This expedition up the "wrong river" was duly noted at the next annual performance of the Survey Pick and Hammer Club!

Successive field assignments to eastern Colorado, the San Rafael Swell in Utah, the Oquirrh Range in Utah, the Adirondacks in New York, the Canal Zone in Panama, the Baker area in Oregon, and the Ajo and Tombstone districts in Arizona provided Gilluly with a broad background of experience in widely different geologic terranes that supplemented his voracious reading.

The field assignments resulted not only in a series of Survey reports of high quality but also by-product papers on particular phases of geology that were significant contributions to geologic literature. A further broadening of Gilluly's experience resulted from a Survey-assisted journey to Europe in 1931, primarily for a period of study with Bruno Sander in Innsbruck of the new field of petrofabrics. Gilluly broadened the trip to include a tour of eastern Europe, the first of several trips that greatly increased his familiarity with global geologic problems.

An offer to join the faculty of the University of California at Los Angeles—at nearly double his Survey salary—was finally accepted by Gilluly, and he moved to Los Angeles in 1940, although he continued his association with the Survey.

The outbreak of World War II, however, soon interrupted his university career, and Gilluly resumed full-time Survey work, initially on projects designed to alleviate the shortage in the so-called strategic minerals. In the early summer of 1944, when the mineral supply was to a considerable degree resolved, he transferred to the Survey's Military Geology Unit, a group of specialists set up to assist the Corps of Engineers in the planning for the Pacific Campaign. The unit prepared reports on such matters as water supply, air-strip locations, appropriate landing beaches, and the like. These activities, characterized as "terrain intelligence," proved to be extremely useful not only to the Corps but to the other branches of the military as well. Gilluly was first assigned to work with the military planning group in Australia, and later New Guinea, and was instrumental in the preparations for the invasion of the Philippines. He landed with the troops on Leyte after his group had recommended a landing area that proved to be far superior to the one originally selected. Similarly, a superior landing field for the airforce planes was proposed on the adjoining island of Samar, rather than one that had been planned for Leyte.

With the completion of his activities in the Philippines, Gilluly returned to the States and resumed his teaching at UCLA in the spring semester of 1945. He was a stimulating instructor, although a demanding one, and inspired great loyalty in those students who responded to his challenges, many of whom achieved their own share of distinction.

He became, however, increasingly intolerant of the machinery of university administration through service on the myriad committees that play a necessary and important role in the management of a major university. This dissatisfaction reached a climax in the McCarthy era, when the University of California was required to insist on a loyalty oath sworn annually by the faculty members. Rather than acquiesce in what he regarded as an intolerable personal and professional insult, Gilluly resigned his professorship and returned to full-time service with the Geological Survey in 1950.

The period from 1945 until 1950 marked a turning point in Gilluly's activities, and to some extent in his professional interests. Prior to his return to UCLA, he had been primarily a field geologist, and his major publications emphasized the areal geology of the regions that he studied, though by no means were more general problems neglected.

Now, however, his teachings required emphasis on the broad aspects of geology. He, together with A. O. Woodford and Aaron Waters, was persuaded by William Freeman to prepare a textbook of geology. The authors agreed among themselves that the book would "concentrate on the analysis of processes that are at work upon and within the earth," rather than present a category of descriptive facts and terms.

Principles of Geology was finally published in 1951. It had been finely honed through the mutual reviewing and critical reading of the three authors, and it was accepted by many universities as "the" geology text. It went through several editions, the last one prepared by Gilluly alone.

Gilluly's return to the Survey initially permitted him to resume the geologic fieldwork he so enjoyed, and in which he excelled. A detailed study of a large and geologically complex area in central Nevada was especially productive. Nevertheless, other responsibilities in the Survey and the National Research Council took an increasing proportion of his time and energy.

After a minor heart attack during his service as chairman of the Division of Earth Sciences of the National Research Council, and a minor accident during some renewed fieldwork in Nevada, he reluctantly gave up rigorous fieldwork and divided his time between extensive reading, in preparation for revision of the *Principles,* and travel over much of the globe in company with his wife, Enid. In these travels he was widely accepted as a major figure in geology and was given great assistance in his visits to areas of geological significance.

Although the last few years of Gilluly's life were marked by several illnesses and hospitalizations, these did not prevent the continuation of his quest for new experiences and new ideas. His last trip, however, was a personal and sentimental one, to the scene of the 1898 Klondike gold rush, particularly to the vicinity of Chilkoot Pass, where his father had nearly lost his life at the hands of the infamous "Soapy" Smith gang. The end came on December 29, 1980, after a brief illness.

Gilluly's impact on the whole field of North American geology—through his textbook and teaching, his numerous papers, and especially his personality and human relationships—has probably been as great as any of his contemporaries. He was a familiar and highly regarded participant in all the geological gatherings he attended, and he never tired of his discussions (in which his beliefs were always vigorously presented) with geologists of all ages and backgrounds.

Gilluly's early papers, which mainly related to the field assignments he carried out for the Geological Survey, not only recorded the results of his fieldwork but also, prophetically, concerned topics and generalizations that were later elaborated into thoughtful and comprehensive papers of widespread application. His early assignment to report on the geology and ore deposits of eastern Oregon, for example, resulted in the preparation of papers on the "replacement origin" of the albite granite in the area and the water content of magmas. These papers were followed some years later by *a Memoir of the Geological Society of America,* the "Origin of Granite." Gilluly served as chairman of the group of authors, as well as a major contributor.

Discussion of the plutonic granite rocks was again a major theme in the William Smith lecture to the Geological Society of London. The lecture encompassed another major interest initiated by the observations and conclusions reached during his earlier geologic fieldwork in the western United States concerning the nature and causes of the geologic structures that he mapped. This interest became a recurrent theme, and may be seen in the series of later papers concerning the distribution of mountain building in geologic time; volcanism, tectonism, and plutonism in the western United States; orogeny and geochronology; and crustal deformation in the western United States, among others. A major conclusion that he reached, and vigorously defended, was that orogeny was, in contrast to the widely accepted theory of "periodic dias trophism," in progress throughout much of geologic time.

The titles in the appended bibliography epitomize the scope of Gillulys geologic contributions; they are also a measure of the influence he exerted on geologic thought, both here and abroad. The contents of these papers reflect the tremendous range of Gilluly's reading. Less obviously, though probably in his opinion more importantly, his conclusions are firmly based on his recognition of the necessity for a thorough knowledge of the evidence provided by field observations. Finally, his papers are characterized by an independence of thought that was not influenced by popular or traditional concepts. His extensive reading, his emphasis on fieldwork, and his independence of thought seem to have been a natural response to the events that characterized his life, from his childhood days to his maturity as a recognized scholar. An important element in his response to these events was the ideal relationship he enjoyed with his wife, Enid Frazier Gilluly, over their married life of more than fifty years.

J. F. Smith[2] has made the following summary of the many honors that came to Gilluly during his lifetime, which, as he observes, were many and well deserved:

> He was the Faculty Research Lecturer at UCLA in 1948, Bownocker Lecturer at Ohio State University in 1951, and the 17th William Smith Lecturer at the Geological Society of London in 1962 (published in 1963). He received the Penrose Medal, Geological Society of America, in 1958; the Distinguished Service Medal, the highest award of the U.S. Department of Interior, in 1959; the Walter Bucher Medal, American Geophysical Union, in 1969; and an honorary Doctorate of Science from Princeton University in 1959. The University of Washington, his undergraduate University, named him *Alumnus Summa Laude Dignatus* in 1963, a highly prestigious award in that the University bestows it upon only one alumnus each year. He was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and an Honorary Member of the Geolog

[2] J. Fred Smith, Jr., "Memorial to James Gilluly," *Geological Society of America Memoirs* (1982).

> ical Society of London. In 1962, he served as Chairman, Division of Earth Sciences of the National Research Council. Jim also served on the U.S. National Committee on Geology and the Upper Mantle Committee of the International Union of Geodesy and Geophysics, and was a member of many professional societies. He became a Fellow of the Geological Society of America in 1927, was Vice-President in 1947, and was President in 1948.

Smith also has provided a fitting tribute in the memorial he prepared for the Geological Society of America:

> With the death of James Gilluly on December 29, 1980, at age 84, the geologic profession lost a powerful and imaginative protagonist whose contributions to science, and to the development of scientists, spanned well over half a century. Although Jim qualified as a specialist in many different disciplines at various stages in his career, and especially as a structural geologist, he was truly and proudly a general geologist. His enormous knowledge of scientific literature and his prodigious memory served him well in dealing productively with an exceptionally broad spectrum of geologic researches. A positive man who was always ready to accept or fling the gauntlet on subjects from geology to politics, Jim expressed his convictions strongly and with a quick wit. He was also a warm human being, a great believer in the rights of the individual, and a defender of the less fortunate. His knowledge was catholic, and he could recite an appropriate poem or a song from Gilbert and Sullivan as readily as he could recall an obscure scientific reference.

Finally, the citation by the University Orator at the time of Gilluly's receipt of the honorary degree of Doctor of Science from Princeton University is an appraisal that many of us regard as supremely fitting: "Dean of American field geologists, inimitable investigator of the inanimate, he is the spiritual descendant of the classical giant Antaeus, who was never so strong as when his feet stood on Terra Firma. Rockbound coasts hold no terrors for him—he analyzes them; he lifts up his eyes unto the hills—and explains their formation; his brilliant record places him in the forefront of the most impregnable of professions, for it is founded upon rock."

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Kurt Gödel

Kurt Gödel

April 28, 1906-January 14, 1978

By Stephen C. Kleene[1]

Two Papers (1930a, 1931a), both written before the author reached the age of twenty-five, established Kurt Gödel as second to none among logicians of the modern era, beginning with Frege (1879).[2] A third fundamental contribution followed a little later (1938a, 1938b, 1939a, 1939b).

Origins and Education, 1906-1930

Gödel was born at Brünn in the Austro-Hungarian province of Moravia. After World War I, Brünn became Brno in Czechoslovakia. Gödel's father Rudolf was managing director and partial owner of one of the leading textile firms in Brünn; his family had come from Vienna. His mother Marianne had a broad literary education. Her father, Gustav Handschuh, had come from the Rhineland. Gödel's family cultivated its German national heritage.

After completing secondary school at Brno, in 1924 Gödel went to Vienna to study physics at the university. The elegant lectures of P. Furtwängler (a pupil of Hilbert and cousin of the famous conductor) fed his interest in mathe

[1] For some details of Gödel's life, I have drawn upon Kreisel (1980) and Wang (1978, 1981); the authors kindly supplied me with copies.

[2] A date shown in parentheses refers to a work listed in the References (or for (Gödel, in the Bibliography), under the name of the adjacent author.

matics, which became his major area of study in 1926. His principal teacher was the analyst Hans Hahn, who was actively interested in the foundations of mathematics. Hahn was a member of the Vienna Circle (*Wiener Kreis*) the band of positivist philosophers led by M. Schlick, who was assassinated during a lecture in 1936. Gödel attended many of their meetings, without subscribing to all of their doctrines (see Wang 1981, 653). His doctoral dissertation was completed in the autumn of 1929, and he received the Ph.D. on February 6, 1930. A somewhat revised version was presented at Karl Menger's colloquium on May 14, 1930, and was published (1930a) using "several valuable suggestions" of Professor Hahn "regarding the formulation for the publication" (Wang 1981, 654; 1930b is an abstract).

Gödel's Completeness Theorem (1930a)

Since Frege, the traditional subject-predicate analysis of the structure of sentences has been replaced by the more flexible use of *propositional functions,* or, more briefly, *predicates.* [3] Using a given collection—or *domain D*—of objects (we call them *individuals* if they are the primary objects not being analyzed) as the range of the independent variables, a one-place predicate *P* or *P*(*a*) over *D* (also called a *property* of members of *D*) is a function that, for each member of *D* as value of the variable *a,* takes as its value a proposition *P*(*a*) A two-place predicate *Q* or *Q*(*a,b*) (also called a *binary relation* between members of D), for each pair of values of a and *b* from *D,* takes as value a proposition *Q* (*a,b*) and so on. In the most commonly cultivated version of logic (the *classical* logic), the

[3] I am endeavoring to give enough background material to enable a scientist who is not a professional mathematician and not already acquainted with mathematical logic to understand (Gödels best-known contributions. The memoir (1980) by Kreisel, about three times the length of the present one, includes many interesting details addressed to mathematicians, if not just to mathematical logicians. The memoir (1978) by Quine gives an excellent overview in just under four pages.

propositions taken as values of the predicates are each either *true or false.*

The *restricted* or *first-order predicate calculus* ("elementary logic") deals with expressions, called *formulas,* constructed, in accordance with stated syntactical rules, from: variables a, b, c, . . . , x, y, z for individuals; symbols P, Q, R, S, . . . for predicates; the propositional connectives ¬ ("not"), & ("and"), V ("or") and → ("implies"); and the quantifiers ∀x ("for all x") and ∃x ("[there] exists [an] x [such that]"). For example, taking P, Q, R to be symbols for predicates of one, two, and three variables, respectively, the expressions P(b), Q (a,c), R(b,a,aP(b), Q(a,c), R(b,a,a), ∀xP(x) ∀x∃yQ(x,y), ∀x((¬P(x)) → Q(a,x)), and ∀x((∃yQ(y,x)) → ¬ R(x,a,x)) are formulas.

In the classical logic, after making any choice of a nonempty domain *D* as the range of the variables, each formula can be evaluated as either true or false for each *assignment in D* of a predicate over *D* as the value or interpretation of each of its predicate symbols, and of a member of *D* as the value of each of its "free" variables. *Its free* variables are the ones with "free" occurrences, where they are not operated upon by quantifiers. In the seven examples of formulas given above, the eight occurrences of a, b, and c are free; the fifteen occurrences of x and y are not free, that is, they are *bound.* The evaluation process is straightforward, taking V to be the inclusive "or" (A V B is true when one or both of A and B are true, and false otherwise), and handling A → B like (¬A) V B. For example, taking *D* to be the non-negative integers or *natural numbers* {0, 1, 2, . . . }, and assigning to P(a), Q(a,b) and R(a,b,c) the predicates "*a* is even", "*a* is less than *b"* and *"ab = c",* and to a, b, and c the numbers 0, 1, and 1, as values, our seven examples of formulas are respectively false, true, true, false, true, true, and true.

Logic is concerned with exploring what formulas express logical truths, that is, are "true in general". Leibnitz spoke of truth in all possible worlds. We call a formula *valid in D* (a given non-empty domain) if it is true for every assignment in *D;* and simply *valid* if it is valid in every non-empty domain *D.*

To make reasoning with the predicate calculus practical, paralleling the way we actually think, we cannot stop to think through the evaluation process in all non-empty domains for all assignments each time we want to assure ourselves that a formula is logically true (valid). Instead we use the "axiomatic-deductive method", whereby certain formulas become "provable".

First, certain formulas are recognized as being *logical axioms.* For example, all formulas of either of the following two forms—the forms being called *axiom schemata*—are axioms:

$$A \to (B \to A). \qquad \forall x A(x) \to A(a).$$

Here A, B, and A(x) can be any formulas, and x and a any variables; A(a) is the result of substituting the variable a for the free occurrences of the variable x in the formula A(x). Furthermore, it is required that the resulting occurrences of a in A(a) be free; thus $\forall x \exists b Q(b,x) \to \exists b Q(b,a)$ is an axiom by the schema $\forall x A(x) \to A(a)$, but $\forall x \exists a Q(a,x) \to \exists a Q(a,a)$ is not. The axiom schemata (and particular axioms, if we have some not given by schemata) are chosen so that each axiom is valid.

Second, circumstances are recognized, called *rules of inference,* in which, from the one or two formulas shown above the line called *premises,* the formula shown below the line called the *conclusion* can be *inferred;* for example:

$$\frac{A, \qquad A \to B}{B.} \qquad\qquad \frac{C \to A(x)}{C \to \exists x A(x).}$$

Here A, B, and A(x) can be any formulas, x any variable, and C any formula not containing a free occurrence of x. The rules of inference are chosen so that, whenever for a given non-empty domain *D* and assignment in *D* the premises are true, then for the same *D* and assignment the conclusion is true. Hence, if the premises are valid, the conclusion is valid.

A *proof* is a finite list of formulas, each one in turn being either an axiom or the conclusion of an inference from one or two formulas earlier in the list as the premise(s). A *proof* is a *proof of* its last formula, which is said to be *provable.*

In one of the standard treatments of the classical firstorder predicate calculus (Kleene 1952, 82), twelve axiom schemata and three rules of inference are used.[4]

By what we have just said about how the axiom schemata (or particular axioms) are chosen (so each axiom is valid), and likewise the rules of inference (so truth is carried forward by each inference), *every provable formula is valid.* Thus the axiomatic-deductive treatment of the predicate calculus is *correct.*

But is it *complete?* That is: *Is every valid formula provable?*

The axiomatic-deductive treatment of the first-order predicate calculus, separated out from more complicated logical systems, was perhaps first formulated explicitly in Hilbert and Ackermann's book (1928). The completeness problem was first stated there (p. 68): "Whether the system of axioms [and rules of inference] is complete, so that actually all the logical formulas which are correct for each domain of individuals can be derived from them, is still an unsolved question."

It is this question that Gödel answered in (1930a). He established: *For each formula* A *of the first-order predicate calculus, either A is provable in it, or A is not valid in the domain* {0, 1, 2, . . . } *of the natural numbers* (and therefore is not valid).

So, *if A is valid, then* Gödel's second alternative is excluded,

[4] I am giving the version of the predicate calculus with predicate symbols instead of predicate variables, after von Neumann (1927). This I consider easier to explain.

and A *is provable.* This answers Hilbert and Ackermann's question affirmatively.

Let us say that a formula A is (or several formulas are simultaneously) *satisfiable* in a given domain *D* if A is satisfied (all the formulas are satisfied), that is, made true, by some assignment in *D.* Then A-is-satisfiable-in-*D* is equivalent to ¬A-is-not-valid-in-*D*.

Now *if* A *is satisfiable in some domain D, then* ¬A is not valid in that domain *D,* so ¬A is not valid, so ¬A is not provable (by the correctness of the predicate calculus), so by Gödel's result applied to ¬A, ¬A is not valid in {0, 1, 2, . . . }, so A *is satisfiable in* {0, 1, 2, . . . }. This is a theorem of Löwenheim (1915).

Restating the completeness theorem for ¬A: *Either* ¬A is not valid (that is, A *is satisfiable*) in {0, 1, 2, . . . }, *or* ¬A *is provable* (equivalently, a contradiction can be deduced from A).

Gödel also treated the case for an infinite collection of formulas $\{A_0, A_1, A_2, \ldots\}$ in place of one formula. The result is just what comes from substituting $\{A_0, A_1, A_2, \ldots\}$ for A in the immediately preceding statement, and noting that, if a contradiction can be deduced from the formulas $A_0, A_1, A_2, \ldots$, only a finite number of them can participate in a given deduction of the contradiction. Thus: *Either the formulas* $A_0, A_1, A_2, \ldots$ *are simultaneously satisfiable in* {0, 1, 2, . . . }*, or for some finite subset* $\{A_{i1}, \ldots, A_{in}\}$ *of them*, $\neg(A_{i1} \,\&\, \ldots, \&\, A_{in})$ *is provable* (and hence valid, so $A_{i1}, \ldots, A_{in}$ are not simultaneously satisfiable in any domain).

Now, *if the formulas of each finite subset of* $\{A_0, A_1, A_2, \ldots\}$ *are simultaneously satisfiable in a respective domain, then* the second alternative just above is excluded, so *all the formulas are simultaneously satisfiable* (this result is called "compactness"), *indeed in the domain* {0, 1, 2, . . . } (the Löwenheim-Skolem theorem). Skolem in (1920), in addition to closing up a gap in Löwen heim's (1915) reasoning, added the case of infinitely many formulas.

These satisfiability results, which are coupled with the completeness theorem in Gödel's treatment, have surprising consequences in certain cases when we aim to use a collection of formulas as axioms to characterize a mathematical system of objects. In doing so, the formulas are not to be logical axioms, but rather mathematical axioms intended to be true, for a given domain *D* and assignment of predicates over *D* to the predicate symbols, exactly when *D* and the predicates have the structure we want the system to have. To make the evaluation process apply as intended, I shall suppose the axioms to be *closed,* that is, to have no free variables.

In using the symbolism of the predicate calculus to formulate mathematical axioms, we usually want to employ a predicate symbol E(a,b) intended to express $a = b$, and usually written a = b. Then, for our evaluation process we are only interested in assignments that give E(a,b) the value $a = b$, that is, that make E (a,b) true exactly when a and b have the same member of *D* as value. Adding some appropriate axioms to the predicate calculus for this case (Kleene 1952, top 399), we get the *first-order predicate calculus with equality.* Gödel offered supplementary reasoning that adapted his treatment for the predicate calculus to the predicate calculus with equality, with "the domain {0, 1, 2, . . . }" being replaced in his conclusions by "{0, 1, 2, . . . } or a non-empty finite domain".

Cantor in (1874) established that the set of all the subsets of the natural numbers {0, 1, 2, . . . } (or the set of the sets of natural numbers) is more numerous, or has a *greater cardinal number,* than the set of the natural numbers. To explain this, I review some notions of Cantor's theory of sets. He wrote (1895, 481): "By a 'set' we understand any collection *M* of definite well-distinguished objects *m* of our perception or our thought (which are called the 'elements' [or 'members'] of *M*) into a whole." A set *N* is a *subset* of a set *M* if each member of *N* is a member of *M*. For example, the set {0, 1, 2} with the three members shown has the following 8 (= 2^3) subsets: {0, 1, 2}, {1, 2}, {0, 2}, {0, 1}, {0}, {1}, {2}, { }. Cantor showed that there is no way of pairing all the sets of natural numbers (that is, all the subsets of the natural numbers) with the natural numbers, so that one subset is paired with 0, another with 1, still another with 2, and so on, with every natural number used exactly once. Sets have the *same cardinal number* if they can be thus paired with each other, or put into a "one-to-one correspondence". Denoting the cardinal number of the natural numbers by $\aleph_0$, and adopting $2^{\aleph_0}$ as a notation for the cardinal of the sets of natural numbers, thus $2^{\aleph_0} \neq \aleph_0$ But the natural numbers can be paired with a subset of the sets of natural numbers (indeed with the *unit sets* {0}, {1}, {2}, . . . having one member each), so we write $2^{\aleph_0} > \aleph_0$.

Sets that are either finite or have the cardinal $\aleph_0$ are called *countable;* other sets, *uncountable.* The real numbers (corresponding to the points on a line) have the same cardinal $2^{\aleph_0}$ as the sets of natural numbers.

I have been tacitly assuming for the first-order predicate calculus (without or with equality) that only a countable collection of variables and of predicate symbols is allowed. This entails that only a countable collection of formulas exists.

Now suppose that we want to write a list of formulas $A_0, \ldots, A_n$ or $A_0, A_1, A_2, \ldots$ in the first-order predicate calculus with equality to serve as axioms characterizing the sets in some version of Cantor's theory of sets. Presuming that the axioms are satisfied simultaneously in some domain *D* (the "sets" in that version of Cantor's set theory) by some assignment (the one understood in his theory), it follows by the Löwenheim-Skolem theorem that they are also satisfiable in the countable domain {0, 1, 2, . . . }! (It is evident that they are not satisfiable in a finite domain.) That is, one can so interpret the axioms that the range of the variables in them constitutes a countable collection, contradicting the theorem of Cantor by which the subsets of $\{0, 1, 2, \ldots\}$ (which are among the sets for his theory) constitute an uncountable collection. This is Skolem's paradox (1923). It is not a direct contradiction; it only shows that we have failed by our axioms to characterize the system of all the sets for Cantor's theory, as we wished to do.

Suppose instead that we want a list of formulas $A_0, A_1, A_2, \ldots$ in the first-order predicate calculus with equality to serve as axioms characterizing the system of the natural numbers 0, 1, 2, ... Skolem in (1933, 1934) showed that we cannot succeed in this wish. He constructed so-called "non-standard models of arithmetic", mathematical systems satisfying all the axioms $A_0, A_1, A_2, \ldots$ (or indeed all the formulas that are true in the arithmetic of the natural numbers) but with a different structure (as the mathematicians say, not *isomorphic* to the natural numbers). In fact, as seems to have been noticed first by Henkin in (1947), the existence of non-standard models of arithmetic is an immediate consequence of the compactness part of Gödel's completeness theorem for the predicate calculus with equality.

Before giving Henkin's argument, I observe that we may enlarge the class of formulas for the first-order predicate calculus with equality by allowing *individual symbols* i, k, . . . which for any assignment in a domain *D* are given members of *D* as their values, and *function symbols* f, g, h, . . . , where, for example, if f is a symbol for a two-place function, its interpretation in any assignment is as a function of two variables ranging over *D* and taking values in *D*. Examples that come to mind for systems of axioms for the natural numbers are 0 as an individual symbol (with the number 0 as its standard interpretation), ' as a one-place function symbol (to be interpreted by + 1), and + and × as two-place function symbols (for addition and multiplication). Such additions to the symbolism are not essential. We could equivalently use predicate symbols Z(a), S(a,b), A (a,b,c), and M (a,b,c), where Z(a) is taken as true exactly when the value *a* of a is 0; S (a,b) when $a+1=b$; A(a,b,c) when $a+b=c$; and M(a,b,c) when $ab=c$. Here "equivalently" means that whatever can be expressed using the individual and function symbols can be paraphrased using the predicate symbols (but at a considerable loss of convenience). This is shown in Hilbert and Bernays (1934, 460 ff.) and Kleene (1952, §74).

Now take the proposed list of axioms A_0, A_1, A_2, . . . , which are true under the interpretation by the system of the natural numbers. I shall assume they include (or if necessary add to them) the axioms ∀x(x' ` 0) and ∀x∀y(x' = y' → x = y). Now consider instead the list A_0, Øi=0, A_1, ¬i=0', A_2 ¬i = 0", . . . where i is a new individual symbol. Each finite subset of these formulas is true under the intended interpretation of the old symbols and interpreting i by a natural number *n* for which ¬i = $0^{(n)}$ (with *n* accents on the 0) is not in the subset. So by compactness, A_0 ¬i = 0, A_1, i = 0', A_2, ¬i = 0", . . . are simultaneously satisfiable. It is easy to see that the satisfying system is isomorphic to one in which 0, 1, 2, . . . are the values of 0, 0', 0", . . . and the value of i is not a natural number—a non-standard model of arithmetic.

These illustrations will suggest the power of Gödel's completeness theorem (1930a) with its corollaries as a tool in studying the possibilities for axiomatically founding various mathematical theories.

Actually, not only was the Löwenheim-Skolem theorem around earlier than 1930, but it has been noticed in retrospect that the completeness of the first-order predicate calculus can be derived as an easy consequence of Skolem (1923). Nevertheless, the possibility was overlooked by Skolem himself; indeed the completeness problem was first for mulated in Hilbert-Ackermann (1928). Skolem worked with logic intuitively rather than using an explicitly described set of axioms and rules of inference. Gödel's treatment of the problem in (1930a) was done without knowledge of Skolem (1922), which Hilbert and Ackermann do not mention, and was incisive, obtained the compactness, and included the supplementary argument to make it apply to the predicate calculus with equality.

Vienna, With Visits To Princeton (IAS)

1930-1939

Gödel's father died in 1929, and Gödel's mother moved to Vienna. She took a large flat and shared it with her two sons, until she returned to her beautiful villa in Brno in 1937. Rudolf, the elder son, was already a successful radiologist in Vienna. The theater in Vienna appealed to her literary interests, and the sons went with her.

Gödel began in 1930 to work on the consistency problem of Hilbert's formalist school, which I will describe in the next section. His approach to this (as described in Wang (1981, §2)) led him to some results on undecidable propositions (preliminary to 1931a), which he announced at a meeting in September 1930 at Königsberg (1930c). von Neumann was much interested and had some penetrating discussions with Gödel, both at the meeting and by correspondence. In November 1930, Gödel's famous paper (1931a) was completed and sent to the *Monatshefte* (received November 17, 1930). It was accepted by Hahn as Gödel's *Habilitationsschrift* on January 12, 1932. (1930d) and (1930e) are abstracts of it and (1931d) is relevant to it.

From 1931 through 1933 Gödel attended Hahn's seminar on set theory (Hahn died in 1934), and took part in Karl Menger's colloquium, which yielded proceedings that reported a number of Gödel's results. In 1933 he was appointed a *Privatdozent* (an unpaid lecturer) at Vienna. In the academic year 1933-34, he went to Princeton as a visitor at the Institute for Advanced Study, and lectured on his (1931a) results; Rosser and I took the notes (1934).

He again visited the IAS in the fall of 1935. While he was there (according to Kleene (1978) and Wang (1981, Footnote 7)), he told von Neumann of his plan for proving the relative consistency of the axiom of choice and the continuum hypothesis by use of his concept of "constructible sets". He completed his plan three years later (1938a, 1938b, 1939a, 1939b), as will be discussed in the second section below.

On September 20, 1938, he married Adele Porkert. (She survived him by three years, passing away on February 4, 1981.) He returned to the IAS in Princeton in the fall of 1938. In the spring of 1939 he lectured at Notre Dame, and he returned to Vienna in the fall of 1939.

Gödel's mother, almost alone among her friends and neighbors, had been skeptical of the successes of Germany under Hitler. In March 1938, when Austria became a part of Germany and the title of *Privatdozent* was abolished, Gödel was not made a *Dozent neuer Ordnung,* (paid) lecturer of the new order, as were most of the university lecturers who had held the title of *Privatdozent.* He was thought to be Jewish, and once for this reason he was attacked in the street by some rowdies. Concerning his application of 25 September 1939 for a *Dozentur neuer Ordnung,* the *Dozentenbundsführer* wrote on 30 September 1939 (without supporting or rejecting the application) that Gödel was not known ever to have uttered a single word in favor of or against the National Socialist movement, although he himself moved in Jewish-liberal circles, though with mitigating circumstances. (The application was actually accepted on 28 June 1940, when Gödel was no longer available.) Gödel was bitterly frustrated. He was apprehensive that he might be conscripted into the German army, despite his frail health, which he believed rendered him unfit for military service. So at the end of 1939, he returned to Princeton, crossing the U.S.S.R. on the TransSiberian Railway.

As a sequel, his mother stayed at her villa in Brno. She was openly critical of the National Socialist regime (thereby losing most of her former friends), so she did not expect reprisals by the Czechs. She returned to Vienna in 1944. But after the war, under the treaty between the Austrian government and Czechoslovakia, she received for her villa only a tenth of its assessed value.

Gödel's Incompleteness Theorems (1931a), etc.

Cantor's development of set theory, begun in (1874), had led—beginning in 1895—to the discovery of paradoxes in it by himself, Cesari Burali-Forti, Bertrand Russell, and Jules Richard. For a quick illustration, I state the Russell paradox (for the others, and references, see Kleene (1952, §11)). Russell considered the set T of all those sets that are not members of themselves, which seemed to come under Cantor's definition of 'set', quoted above. Is T a member of T? In symbols, does $T \varepsilon T$? Suppose $T \varepsilon T$; then by the definition of T, not $T \varepsilon T$ (in symbols $T \notin T$), contradicting the supposition. So by reductio ad absurdum, $T \notin T$. Similarly, supposing $T \notin T$, $T \varepsilon T$. Thus both $T \notin T$ and $T \varepsilon T$!

The appearance of the paradoxes gave special impetus to thinking about the foundations of mathematics, beyond what was already called for by the very extensive reformulations of various branches of mathematics in the nineteenth century. By the mid-1920s, three principal schools of thought had evolved.

The *logicistic* school was represented by Bertrand Russell and Alfred North Whitehead. It proposed to make mathematics a branch of logic, in accordance with Leibnitz's 1666 conception of logic as a science containing the ideas and principles underlying all other sciences. They proposed to deduce the body of mathematics from logic, continuing from work of Frege, Dedekind, and Peano (see Kleene 1952, 4346). To avoid the newly discovered paradoxes, Russell formulated his theory of types (1908), in which the individuals (or primary objects not being subjected to analysis) are assigned to the lowest type 0, the properties of individuals (or one-place predicates over type 0) to type 1, the properties of type-objects to type 2, and so on. A rather definite structure was assumed for the totality of the possible definitions of objects of a given type. The deduction on this basis of a very large portion of the existing mathematics was carried out in the monumental *Principia Mathematica* (PM) of Whitehead and Russell in three volumes (1910, 1912, 1913).

Neither of the other two schools, the *intuitionistic* and the *formalistic,* agreed to start back in logic to deduce the simplest parts of mathematics, such as the elementary theory of the natural numbers 0, 1, 2, ... Indeed, it can be argued that mathematical conceptions on this level are already presupposed in the formulation of logic with the theory of types.

The *intuitionistic* school of thought dates from a paper of Brouwer (1908) criticizing the prevailing or "classical" logic and mathematics. Brouwer argued that classical logic and mathematics go beyond intuition in treating infinite collections as actually existing. As an example, each of the natural numbers 0, 1, 2, . . . is a finite object; but there is no last one. Mathematicians can often establish that a property is possessed by every natural number n by reasoning that involves working with only the natural numbers out to a certain point depending on n (maybe just with the numbers $\leq n$). Thus the infinity is only a *potential* infinity (an horizon within which we work). On the other hand, much of the existing classical mathematics deals with infinite collections as completed or *actual* infinities. Some reasoning with the natural numbers uses an actual infinite; for example, the application of the law of the excluded middle to say that either some natural number has a certain property *P,* or that is not the case (so every natural number has the property not-*P*). The use of infinite collections as actual infinities is pervasive in the usual theory of the real numbers, represented say using infinite decimals. Brouwer, in papers beginning in 1918 (exposition in Heyting 1971), proposed to see how far mathematics could be redeveloped using only methods that he considered intuition as justifying: that is, methods using only potential infinities, not actual ones. Brouwer was able to go rather far in this direction, at the cost of altering the subject substantially from the classical form as typified by the classical analysis that physicists are accustomed to applying.

The *formalistic* school was initiated by Hilbert in (1904), and he developed it with a number of collaborators after 1920. Hilbert agreed with the intuitionists that much of classical mathematics goes beyond intuitive evidence. He drew a distinction between *real* statements in mathematics, which have an intuitive meaning, and *ideal* statements, which do not but in classical mathematics are adjoined to the real ones to make mathematical theories simpler and more comprehensive. His real statements are those that correspond to the use of infinity only potentially, while an actual infinite is involved in the ideal statements. But rather than simply abandoning the ideal parts of mathematics, Hilbert had another proposal.

We saw above how the first-order predicate calculus, after logical propositions are expressed as formulas in a precisely regulated symbolic language, was organized by the axiomatic-deductive method. Whitehead and Russell, and Hilbert, proposed to do the same for mathematics generally, that is for very substantial portions of mathematics short of the paradoxes. As we saw, Whitehead and Russell proposed to make all of it logic, but not just first-order logic, within which mathematics is to be defined. Instead, Hilbert proposed to start with mathematical axioms as well as logical axioms. This can be done in the symbolism of the first-order predicate calculus, or using a second-order predicate calculus (with quantification of properties of individuals), or still higherorder predicate calculi. In proofs in a system obtained by adding mathematical axioms to the logical apparatus of the first-order predicate calculus (or, as we may call them, "deductions" by logic from the mathematical axioms), we are exploring formulas that are true for each domain D and assignment in D that satisfy the axioms. A symbolic language is first established with an exactly specified syntax (thus, a class of *formulas*) and then an exactly defined concept of *proofs* (by starting with *axioms,* logical or mathematical, and applying *rules of inference*) We call the result a *formal system.* (The first-order predicate calculus as described above is a formal system with only logical axioms.)

For Whitehead and Russell, our confidence in the result—the deduction of mathematics within PM—was to rest on our being convinced of the correctness of the logical principles embodied in their version of logic, inclusive of the theory of types, from which all the rest is deduced.

Hilbert proposed to "formalize" one or another mathematical theory, and he hoped to continue with the whole body of mathematics up to some point short of encountering the paradoxes, in formal systems. Typically, the mathematics formalized will in part be ideal and thus not supported by our intuitions. Then he wanted to look at such a system from outside. The formal system, looking just at its structure (apart from the meanings or supposed meanings expressed by the symbols, which guide the practicing mathematician) is a system of finite objects: symbols (from an at most countably infinite collection), finite sequences of symbols (like those constituting formulas), and finite sequences of finite sequences of symbols (like those constituting proofs). So there is the possibility of applying to the study of a formal system intuitive methods of reasoning in the real part of mathematics (using only potential infinities), which Hilbert called *finitary* (German *finit*).

In particular, Hilbert hoped by finitary reasoning to prove the *consistency* of each of his formal systems, that is, that no two proofs in it can end in a pair of contradictory formulas A and ¬A. This would show that mathematics, as it has been developed classically by adjoining the ideal statements to the real ones, is not getting into trouble. Thus Hilbert proposed to give a kind of justification to the cultivation of those parts of classical mathematics that the intuitionists reject. The mathematical discipline in which formal systems (often embodying ideal mathematics) are studied from outside in respect to their structure (leaving out of account the meanings of the symbols) as part of real mathematics, using only finitary methods, Hilbert called *proof theory* or *metamathematics.* Full-length expositions are in Hilbert and Bernays (1934, 1939) and Kleene (1952).

Now we are in a position to understand Gödel's (1931a) results.

Clearly, having embodied some part of mathematics in a formal system, a question of completeness arises just as we saw for the formal system of the first-order predicate calculus.

Specifically, Gödel considered formal systems like that of *Principia Mathematica* and systems constructed by the formalists that aim to formalize at least as much of mathematics as the elementary theory of the natural numbers. (A formal system that didn't do this much would be of rather little interest for the programs of the logicistic and formalistic schools.)

In such a system, propositions of elementary number theory can be expressed by closed formulas, that is, ones containing no free variables. Completeness should then mean that, for each closed formula A, either A itself or its negation, ¬A, is provable. That is, for the system to be complete, proofs in the system should provide the answer "yes" (A is provable) or "no" (¬A is provable) to any question about natural numbers "Is the proposition *P* true?" such that *P* can be expressed, under the intended meaning of the symbols, by a closed formula A. For example, with the variables interpreted to range over the natural numbers, if A(x,y) is a formula (with only the free variables x and y) expressing x < y, one of the two closed formulas ∀x∃yA(x,y) and ¬∀x∃yA(x,y) should be true—indeed the first is—and this one should be provable if the formal system is complete. The *open* formulas ∃xA(x,y) and ¬∃xA(x,y), in ordinary usage, are synonymous with their *closures* ∀y∃xA(x,y) and ∀yØ$xA(x,y), and neither is true.

Gödel's first incompleteness theorem of (1931a)— famous simply as "Gödel's theorem"—says that a formal system S like that described, if correct, is incomplete. *There is in S a closed formula G such that, if in S only true formulas are provable, then neither G nor ¬G is provable in S* (*although indeed under the intended interpretation G is true*).

To be more specific about the assumption of correctness, let us take into account the form of G, which is ∀xA(x), where for the interpretation the intended range of the variable x is the natural numbers. Here A(x) is a formula with the following property. Let us substitute in A(x) for the free occurrences of the variable x successively the expressions (called *numerals*) 0, 0', 0, ***x*** , . . . which express the natural numbers 0, 1, 2, . . . , *x,* . . . . I am denoting the numeral for *x* by " ***x*** ", and I write the result of the substitution as "A(x)". For each *x,* one of A(***x***) and ¬A(***x***) is provable. The assumption of cor rectness that Gödel made is that for no formula A(x) and natural-number variable x are there proofs in *S* of all of A(0), A(1), A(2), . . . , A(*x*), . . . and also of ¬∀xA (x). This assumption he called ω-*consistency.* (*Simple*) *consistency* is the property that for no formula A are there proofs of both of A and ¬A. By applying ω-consistency to ∀xA, where x is a variable not occurring free in A, ω-consistency implies simple consistency. Restating Gödel's theorem with this terminology: *If S is ω-consistent, it is* (*simply*) *incomplete, that is, there is a closed formula G such that neither G nor ¬G is provable in S* (*but G is true*).

How could this be? The fundamental fact is that in working with a formal system (apart from its interpretation), the objects we are dealing with (the symbols from a finite or countably infinite collection, the finite sequences of (occurrences) of those symbols, and the finite sequences of such finite sequences) form a countably infinite collection of linguistic objects. By pairing them one-to-one with the natural numbers, or using some other method of associating distinct natural numbers with them (as indeed Gödel did), each object of the formal system is represented by a number, now called its *Gödel number.* So indeed, since the formal system *S* is adequate for a certain part of the elementary theory of the natural numbers, we can express in *S* propositions that by the Gödel numbers actually say things about the system *S* itself. Now Gödel ingeniously constructed his G to be of the form ∀xA(x), where A(x) expresses "*x* is not the Gödel number of a proof of the formula with a certain fixed Gödel number *p*" and *p* is the Gödel number of the formula G itself! Thus G says, "Every *x* is not the Gödel number of a proof of me", or simply "I am unprovable." So, if G were provable, G would be false. So (assuming correctness), G is unprovable; hence (by what G says) G is true; and hence (assuming correctness) ¬G is also unprovable. It is easy to confirm that ω-consistency suffices as the correctness assumption in conclud ing that ¬G is unprovable, and simple consistency in concluding that G is unprovable.

Gödel's formula G, which says "I am unprovable", is an adaptation of the ancient paradox of *the liar*. The Cretan Epimenides (sixth century B.C.) is reported to have said "Cretans are always liars." If this were the only thing Epimenides said, could it be true? Or false? To take the version of Eubulides (fourth century B.C.), suppose a person says "The statement I am now making is false." If this statement is false, by what it says it would be true; and vice versa. Gödel's substitution of "unprovable" for "false" escapes the paradox, because a statement and its negation can both be unprovable (while they cannot both be false).

In two respects, Gödel's theorem, as given in (1931a), has been improved. Rosser (1936), by using a slightly more complicated formula than Gödel's G, replaced Gödel's hypothesis of ω-consistency by the hypothesis of simple consistency. The other improvement, to be explained next, is connected with a development that took place essentially independently of Gödel's (1931 a) and is equally significant.

At least since Euclid in the fourth century B.C., mathematicians have recognized that for some functions and predicates they have "algorithms". An *algorithm* is a procedure described in advance such that, whenever a value is chosen for the variable or a respective value for each of the variables of the function (or predicate), the procedure will apply and enable one in finitely many steps to find the corresponding value of the function (or to decide the truth or falsity of the corresponding value of the predicate).

In the 1930s, the general concept of an "algorithm" came under scrutiny, and Church in (1936) proposed his famous thesis ("Church's thesis" or "the Church-Turing thesis"). This states that all the functions of natural number variables for which there are "algorithms", or which in Church's phrase ology are "effectively calculable", belong to a certain class of such functions for which two equivalent exact descriptions had been formulated during 1932-34. Turing in (1937), independently of Church, arrived at the same conclusion, using a third equivalent formulation, namely the functions computable by an idealized computing machine (error-free and with no bound on the quantity of its storage or "memory") of a certain kind, now called a "Turing machine." The thesis applies to predicates, because a predicate can be represented by the function taking 0 as its value when the value of the predicate is true and 1 when it is false.

As Turing wrote (1937, 230): "conclusions are reached which are superficially similar to those of Gödel [in (1931a)]." Gödel (1931 a) showed the existence in certain formal systems *S* of "formally undecidable propositions", that is, propositions for which the system *S* does not decide the truth or falsity by producing a proof of A or of ¬A, where A is the formula expressing the proposition in the symbolism of *S*. Church (1936) and Turing (1937) showed the existence of "intuitively undecidable predicates", that is, predicates for which there is no "decision procedure" or "effective process" or "algorithm" by which, for each choice of a value of its variable, we can decide whether the resulting proposition is true or false.

In (1936, 1943, 1952), I established a connection between the two developments. The fundamental purpose of using formal systems (as a refinement of the axiomatic-deductive method that has come down to us from Pythagoras and Euclid in the sixth and fourth centuries B.C.) is to remove all uncertainty about what propositions hold in a given mathematical theory. For a formal system to serve this purpose, there must be an algorithm by which we can recognize when we have before us a proof in the system. Furthermore, for the system to serve as a formalization of a given theory, we must have, for the propositions we are interested in, an algorithm to identify the formulas in the system that express those propositions. Of course, we can start with the formulas of the system, if they have an understood interpretation, and take as our class of propositions those expressed by the formulas. For the formalization of the theory of the natural numbers, using Gödel numberings of the formulas and proofs, the algorithms can be for number-theoretic functions and predicates, so the Church-Turing thesis can be applied.

Gödel established his theorem for *"Principia Mathematica* and related systems". In my generalized versions of the theorem, I left out all the finer details of the formalization, and simply assumed that the purpose of formalization as described above is served, for the theory of the natural numbers. Moreover, I chose in advance a fixed number-theoretic predicate $P(a)$ so that every correct formal system fails to formalize its theory completely. Gödel's undecidable propositions in various formal systems are then all values of this one predicate. Thus: *There is a predicate $P(a)$ of elementary number theory with the following property. Suppose that in a formal system S* (i) *there are formulas A_a for a* = 0, 1, 2, . . . *given by an algorithm* (which formulas we take to express the propositions $P(a)$ for a = 0, 1, 2, . . .) *such that, for each a* = 0, 1, 2, . . . , A_a *is provable in S only if $P(a)$ is true, and* (ii) *there is an algorithm for determining whether a given sequence of formulas in S is a proof in S of a given formula. Then there is a number p such that $P(p)$ is true but Ap is unprovable in S. If moreover* (iii) *there are formulas $\neg A_a$ such that, for each a* = 0, 1, 2, . . . , $\neg A_a$ *is provable in S only if $P(a)$ is false, then $\neg A_p$ is also unprovable in S* (*so A_p is undecidable in S*).

The predicate $P(a)$ can be of a very simple form (suggested in Kleene (1936, Footnote 22), and used in his (1943, 1952): "for all x, $Q(a,x)$ where Q is a decidable predicate. (A fuller exposition is in Kleene 1976, 768-69.)

As I expressed the generalized Gödel theorem in a lecture at the University of Wisconsin in the fall of 1935 (with my (1936) already written, and knowing the contents of Church (1936) but not yet of Turing (1937)), the theory of the natural numbers—indeed just the theory of the limited part of it represented by the predicate $P(a)$—offers inexhaustible scope for mathematical ingenuity. No one will ever succeed in writing down explicitly a list of principles (given as a formal system) sufficient for providing a proof of each of the propositions $P(a)$ for $a = 0, 1, 2, \ldots$ that is true.

To recapitulate, by Gödel's first incompleteness theorem, as he gave it in (193 la), none of the familiar formal systems (like that of *Principia Mathematica*) and by the generalized version of the theorem, which Gödel accepted in a "Note added 28 August 1963" to the van Heijenoort (1967) translation of his (1931a) and in the "Postscriptum" to the Davis (1965) reprint of his (1934), no conceivable formal system, can be both correct and complete for the elementary theory of the natural numbers.

In Gödel's first incompleteness theorem (as stated above for *Principia Mathematica* and related systems), the unprovability of G follows from the assumption that S is simply consistent. By Gödel's numbering, the property of the simple consistency of S can be expressed in S itself by a formula, call it "Consis". And in fact the reasoning by which Gödel showed that "Simple consistency implies G is unprovable" can be formalized within S as a proof of the formula

$$\text{Consis} \rightarrow \text{G},$$

noting that G says "G is unprovable"! Therefore, if Consis were provable in S, by one application of the rule of inference shown first above, G would be, contradicting Gödel's first incompleteness theorem if S is consistent. So we have Gödel's second incompleteness theorem of (1931a): *If S is simply consistent, the formula* Consis *expressing that fact is unprovable in S.*

Hilbert's idea had been to prove the consistency of a suit able formal system *S* of mathematics by finitary methods. In the interesting case that *S* is a formalization embracing some ideal (non-finitary) mathematics, the methods to be used in proving its consistency should not include all those formalized in *S*. Gödel's second theorem shows that not even all the methods formalized in *S* would suffice!

The consequence is that, if Hilbert's idea can be carried out, it cannot be done as simply as presumably had been hoped. Methods will have to be accepted as finitary, and used in the consistency proof of a system *S*, that are not formalizable in *S*. Indeed, this has now been done for the arithmetic of the natural numbers by Gentzen (1936), Ackermann (1940), and Gödel (1958a), and for analysis (real-number theory) by Spector (1961), extending Gödel (1958a).

With the two incompleteness theorems of Gödel (1931a), the whole aspect of work on the foundations of mathematics was profoundly altered.

We have described Gödel's celebrated results in (1930a) and (193la) in the context of the outlook on foundations at the time. He clearly addressed—and solved—problems existing at that time. Each of these results can be construed as a piece of exact mathematics: on the level of classical number theory in the case of (1930a), and of finitary number-theory in the case of (1931a). The picture mathematicians could entertain of the possibilities for the use of formal systems has been refocused by Gödel's discoveries. Now we know that their use cannot give a resolution of the foundational problems of mathematics as simply as had been hoped. But, in my view, formal systems will not go away as a concern of mathematicians. Recourse to the axiomatic-deductive method, as refined in modern times to formal systems, provides mathematicians with the means of being fully explicit about what they are doing, about exactly what assumptions they have used in a given theory. It is important to have this explicitness when they are engaged in conceiving new methods (as Gödel's first (1931a) theorem shows that for progress they must) and attempting to assure themselves of their soundness (which by Gödel's second theorem cannot be done simply by the metamathematical applications of only the same methods).

In the period we are reviewing (after (1930a) and prior to 1938a)), Gödel made a number of other significant contributions.

In (1934), building on a suggestion of Herbrand (see van Heijenoort (1967, 619)), he introduced the notion of "general recursive functions", which I studied in (1936). This is one of the two equivalent notions that were identified with "effective calculability" by Church's thesis mentioned above. Nevertheless, Gödel did not accept the thesis until later (Kleene 1981, 59-62). Concerning those notions and the third one of Turing, and generalizations of them, a very extensive mathematical theory has been developed (with an important role played by the Herbrand-Gödel notion) and applied to other branches of mathematics (Kleene (1981, 62-64)).

In (1931b), Gödel explained how some of his undecidable propositions become decidable with the addition of higher types of variables, while of course other undecidable propositions can be described. In a trenchant paper (1935), he showed that in the systems with higher types of variables infinitely many of the previously provable formulas acquire very much shorter proofs. He also offered contributions to the so-called *Entscheidungsproblem* (decision problem) for the first-order predicate logic (1930f, 1933b). This is the problem of finding an algorithm, at least for a described class of formulas, for deciding whether a formula is or is not provable. (1931 c) is historic as one of the first results on a "formal system" with uncountably many symbols.

The intuitionistic school under Brouwer came to recognize the advantages of formalization for making explicit the boundaries of a given body of theory. So Heyting in (1930a, 1930b) gave a formalization of the intuitionistic logic and of a portion of the intuitionistic mathematics. This has had various mathematical applications. Gödel's papers (1932c, 1932d, 1932e) were important contributions to the study of these systems.

The article of Smorynski and that of Paris and Harrington in Barwise (1977), and Dawson (1979), can serve as a sampling of the reverberations after nearly fifty years from Gödel's (1931a) incompleteness theorems.

Gödel's Relative Consistency Proof for the Axiom of Choice and for the Generalized Continuum Hypothesis (1938a, 1938b, 1939a, 1939b)

As remarked above, in Cantor's set theory, the set of the sets of natural numbers, and the set of the real numbers, have a cardinal number $2^{\aleph_0}$ greater than the cardinal number $\aleph_0$, of the set of the natural numbers, which is the least infinite cardinal.

In Cantor's theory, the cardinal number $\aleph_1$, next greater than $\aleph_0$ is identified as the cardinal of the set of all possible linear orderings of the natural numbers in which each subset of them has a first member in the ordering ("wellorderings"). Cantor's set theory would be greatly simplified if $2^{\aleph_0}$, which is the infinite cardinal greater than $\aleph_0$ coming to mind first, is actually the next greater cardinal. Cantor conjectured in (1878) that it is, that $2^{\aleph_0} = \aleph_1$. This conjecture is called the "continuum hypothesis" (CH); and it became the central problem of set theory to confirm or refute CH. Sixty years later, with the problem still unsolved, Gödel's results in (1938a, 1938b, 1939a, 1939b) put the matter in a new light.

Using Cantor's *ordinal numbers,* all the infinite cardinals can be listed in order of magnitude as

$\aleph_0, \aleph_1, \aleph_2, \ldots, \aleph_\alpha, \ldots,$

where α ranges over the natural numbers as finite ordinals, and then on into Cantor's infinite ("transfinite") ordinals. The "generalized continuum hypothesis" (GCH) is that, for each ordinal α, $2^{\aleph_0} = \aleph_{\alpha+1}$, where $2^{\aleph_\alpha}$, is the cardinal of the set of all the subsets of a set of cardinal $\aleph_\alpha$.

The theory of sets was axiomatized after the paradoxes had appeared. This consisted in listing a collection of axioms, regarded as true propositions about sets, including axioms providing for the existence of many sets but not of too "wild" sets such as had given rise to the paradoxes. (We recall the Skolem paradox about such systems of axioms in first-order logic.) As a standard list of axioms for set theory, I will take those commonly called the Zermelo-Fraenkel axioms. These arise from the first axiomatization by Zermelo in (1908) by using a refinement proposed by Fraenkel in (1922). One of the axioms, called the "axiom of choice" (AC), has been regarded as less natural than the others. One form of it says that, if we have a collection *S* of non-empty sets, no two of which have a member in common, there is a "choice set" *C* containing exactly one member from each set in the collection *S*. By ZFC I shall mean all the Zermelo-Fraenkel axioms, and by ZF the system of those axioms without AC.

Cantor had not been thinking of his conjecture that $2^{\aleph_0} = \aleph_1$ (CH) relative to a set of axioms. But after choosing an axiomatization, say ZF, there are three possibilities: (1) $2^{\aleph_0} = \aleph_1$ is provable (using elementary logic) from the axioms. (2) $\neg(2^{\aleph_0} = \aleph_1)$ is provable from the axioms. (3) Neither (1) nor (2). This is assuming the axioms are consistent, so that not: (4) Both (1) and (2).

What Gödel did was to exclude (2); he showed that adding

$2^{\aleph_0} = \aleph_1$ to the axioms will not lead to a contradiction (if a contradiction is not already deducible from the axioms without the addition).

To put the matter in its simplest terms, Gödel, using only things about sets justified by the axioms ZF, defined a class *L* of sets, which he called the "constructible sets", such that all the axioms are true when the "sets" for them are taken to be just the constructible sets *L.* In effect, *L* constitutes a kind of skeletal model of set theory—not all the sets presumably intended, but still enough to make all the axioms true. And in this model, AC and CH, and indeed GCH, are all true.

Since nothing is used about sets in this reasoning with *L* that cannot be based on the axioms ZF, it can be converted as follows into a demonstration that if ZF (taken as the formal system with the mathematical axioms of ZF and the logical axioms and rules of inference of the first-order predicate calculus) is (simply) consistent, so is ZF + AC + GCH (similarly taken). Suppose ZF is consistent, and (contrary to what we want to prove) that a pair of contradictory formulas A and ¬A (which we can take to be closed) are provable in ZF + AC + GCH. Let B^L come from any closed formula B by replacing each part of the form $\forall x C$ by $\forall x(x \varepsilon L \rightarrow C)$ and each part of the form $\exists x C$ by $\exists x(x \varepsilon L \ \& \ C)$, in effect restricting the variable x to range over *L.* Here xεL is definable within ZF. Now for the axioms A_0, A_1, A_2 . . . of ZF + AC + GCH, we can prove in ZF A_0^L, A_1^L, A_2^L, . . . , and then continue by the reasoning that gave the contradiction A and ¬A in ZF + AC + GCH to get the contradiction A^L and $\neg A^L$ in ZF, contradicting our supposition that ZF is consistent. Thus Gödel gave a consistency proof for ZF + AC + GCH relative to ZF.

It is natural to ask whether one can also rule out (1), that is, whether the negation $\neg 2^{\aleph_0} = \aleph_1$ of the continuum hy pothesis can be added consistently to ZF or indeed to ZFC (provided ZF is consistent). It remained for Paul Cohen in (1963, 1964) to do this. He accomplished this by using another model (quite different from Gödel's) in which all the axioms of ZFC and also $\neg 2^{\aleph_0} = \aleph_1$ hold. (For a comment by Gödel, see 1967.)

Thus, combining Gödel's and Cohen's results, $2^{\aleph_0} = \aleph_1$ is independent of ZFC. Similarly, combining results of Gödel and Cohen, AC is independent of ZF. These results of Gödel and Cohen have ushered in a whole new era of set theory, in which a host of problems of the consistency or independence of various conjectures relative to this or that set of axioms are being investigated by constructing models.

Princeton (IAS) 1939-1978

After Gödel's return to Princeton in 1939, he never again left the United States. He became a U.S. citizen in 1948. He received annual visiting appointments from the Institute for Advanced Study from 1940-41 on, became a permanent member in 1947, a professor (in the School of Mathematics) in 1953, and retired in 1976. He was keenly interested in the affairs of the Institute, and conscientious in work for the Institute, especially in the evaluation of applicants.

I have already reviewed most of his work that was published in his own papers. The last paper of his in the Bibliography (1958a), mentioned above, gives a new interpretation of intuitionistic number theory, which Wang (1981, 657) says "was obtained in 1942. Shortly afterwards he lectured on these results at Princeton and Yale." Gödel's (1944, 1947) are exceedingly suggestive expository and critical articles on Russell's mathematical logic and on the continuum problem.

In December 1946, Gödel presented a paper to the Princeton Bicentennial Conference on Problems of Mathe matics, published in 1965, suggesting a non-constructive extension of formal systems, or of the notion of "demonstrability", to be obtained by using stronger and stronger "axioms of infinity" asserting the existence of large cardinal numbers in set theory. He wrote, "It is not impossible that for such a concept of demonstrability some completeness theorem would hold which would say that every proposition expressible in set theory is decidable from the present axioms plus some true assertion about the largeness of the universe of all sets." The paper makes a similar suggestion regarding the concept of mathematical "definability".

Gödel and Einstein, both at the Institute for Advanced Study, saw much of each other. Because of Gödel's interest in Kant's philosophy of space and time, Gödel became interested in general relativity theory, on which he worked during 1947 to 1950 or 1951. Three short articles (1949a, 1949b, 1950) resulted. According to R. Penrose, as reported in Kreisel (1980, 214-15), "[these articles] were highly original and, in the long run, quite influential . . . . Gödel's work served as a cross check on mathematical conjectures and proofs in the modern global theory of relativity." (For summaries, see Kreisel loc. cit. and Dawson 1983, 266-67, the Addenda and Corrigenda to which reports on a controversy about it.)

Gödel was deeply interested in philosophy, and in the relevance of philosophical views to the mathematical problems with which his work dealt. Wang writes (1981, Footnote 9), "we may conjecture that between 1943 and 1947 a transition occurred from Gödel's concentration on mathematical logic to other theoretical interests which are primarily philosophical . . . From [his papers (1946, 1947)] one gets the clear impression that Gödel was interested only in really basic advances." Kreisel (1980, 204-13) calls Gödel's first proposal in (1946) "Gödel's programme", and discusses it while citing

Kanamori and Magidor (1977) for more complete references to the work done on the program over the last thirty-five years.

According to Wang (1978, 183; 1981, 658), Gödel worked on several papers (as early as 1947, perhaps), which in the end he did not publish. One of these was his Josiah Willard Gibbs Lecture, *Some Basic Theorems on the Foundations of Mathematics and their Philosophical Implications,* which he read from a manuscript to the American Mathematical Society on December 26, 1951 (I was present). Some of the ideas in this lecture are reported by Wang in the pages cited as Gödel (1974a). Gödel left a considerable quantity of notes (almost 5,000 pages, according to Kreisel 1980, 151). Undoubtedly, these will constitute a mine for scholars for quite some time into the future.

Gödel contributed reflections on some of his papers as emendations, amplifications, and additions to reprints and translations (see Bibliography, 1939, 1946, 1949b, 1963-66a).

Gödel was rather retiring. But he was kind and responsive to qualified interlocutors who took the initiative to engage him in discussions. So it has come about that various reflections and views of his have been reported with his permission in writings by other authors. Also, on occasions, he took the initiative to volunteer pronouncements other than in papers of the usual sort. I have included in this Bibliography all the material of these two kinds that has come to my attention (without attempting to draw a line between the substantial, and the slight). This accounts for (1931d) and all of the items after (1950), except (1958a) and (1963-66a). On the occasion of the award of an Einstein Medal to Gödel on March 14, 1951, John von Neumann began his tribute to Gödel (von Neumann 1951) with the words:

Kurt Gödel's achievement in modern logic is singular and monumental—indeed it is more than a monument, it is a landmark which will remain visible far in space and time.

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HONORS AND DISTINCTIONS

Awards and Memberships

Albert Einstein Award, Lewis and Rosa Strauss Memorial Fund (shared with Julian Schwinger), 1951
National Academy of Sciences, Member, 1955
American Academy of Arts and Sciences, Fellow, 1957
American Philosophical Society, Member, 1961
London Mathematical Society, Honorary Member, 1967
Royal Society (London), Foreign Member, 1968
British Academy, Corresponding Fellow, 1972
Institut de France, Corresponding Member, 1972
Académie des Sciences Morales et Politiques, Corresponding Member, 1972
National Medal of Science, 1975

Honorary Degrees

D.Litt., Yale University, 1951
Sc.D., Harvard University, 1952
Sc.D., Amherst College, 1967
Sc.D., Rockefeller University, 1972

Annotated Bibliography

I have listed the original items in the order of their authorship by Gödel, insofar as I could find information to base this on. Thus 1939b, communicated by Gödel on February 14, 1939, is put after 1939a, which is a set of notes by George W. Brown published in 1940 on lectures delivered by Gödel in the fall term of 1938-39; and 1934 and 1946, only published in 1965, are in the right order. This has involved using dates of presentation for the eleven items listed from *Ergebnisse eines mathematischen Kolloquiums* (ed. Karl Menger). Not listed are four brief contributions by Gödel to discussions in these *Ergebnisse* (4:4, 4:6, 4:34(51), 7:6), twenty-seven reviews by Gödel in *Zentralblatt für Mathematik und ihrer Grenzgebiete* 1931-36, six reviews by Gödel in *Monatshefte für Mathematik und Physik* 1931-33, and the Spanish translations in Mosterín (1981) of all of the papers of Gödel except 1932a, 1932b, and 1933a. (In Dawson (1983), the four *Ergebnisse* items are cited (just before [1933a] and as [1933], [1933d], [1936]), as well as the twenty-seven *Zentralblatt* reviews and the Mosterín (1981) translations, while the six M*onatshefte* reviews are cited in its Addenda and Corrigenda.)

The other translations and reprintings of Gödel's papers are cited within the items for the originals. Eight of the translations (with no inputs by (Gödel, as far as I know) are thus cited concisely through their reviews or listings in *the Journal of Symbolic Logic.* Books (not journals) are generally cited through the Reference section. For example, the English translation of 1930a is on pp. 583-91 of the book listed in the References as "van Heijenoort, J., ed. 1967."

A work entitled *Kurt Gödel, Sein Leben und Wirken,* W. Schimanovich and *P.* Weibel, eds., is to be published by Verlag Hölder-Pichler-Tempsky, Vienna. It will contain some of Gödel's works and various biographical and interpretative essays.

The Association for Symbolic Logic is arranging for the publication in the original (by Oxford University Press, ed. by S. Feferman et al.), and when the original is in German also in English translation, of all of Gödel's published works, with introductory historical notes to them and a biographical introduction and survey. (Volume I (1986) contains Gödel's published works up through 1936; the rest will be in Volume II, probably in 1986. A further volume or volumes are projected to contain a selection of unpublished material from Gödel's *Nachlass.*)

1930 a. Die Vollständigkeit der Axiome des logischen Funktionenkalküls. Monatsh. Math. Phys., 37:349-60 . (English trans: van Heijenoort, 1967, pp. 583-91, with two comments by Gödel, pp.

510-11; also see Kleene, 1978; reprinted in: Berka and Kreiser, 1971, pp. 283-94.)

b. Über die Vollständigkeit des Logikkalküls (talk of 6 Sept. 1930). Die Naturwissenschaften, 18:1068 .

c. [Remarks in] Diskussion zur Grundlegung der Mathematik [7 Sept. 1930]. Erkenntnis (1931), 2:147-48 .

d. Nachtrag [to the preceding remarks]. Erkenntnis, 2:149-51 . (Italian trans: Casari, 1973, pp. 55-57.)

e. Einige metamathematische Resultate über Entscheidungsdefinitheit und Widerspruchsfreiheit. Anz. Akad. Wiss. Wien, Math. naturwiss. Kl. 67:214-15 . (English trans.: van Heijenoort, 1967, pp. 595-96; reprinted in: Berka and Kreiser, 1971, pp. 320-21.)

f. Ein Spezialfall des Entscheidungsproblems der theoretischen Logik. Ergeb. math. Kolloq. (for 1929-30, publ. 1932), 2:27-28 .

1931 a. Über formal unentscheidbare Satze der Principia Mathematica und verwandter Systeme I. Monatsh. Math. Phys., 38:173-98 . (English trans., 1962, see J. Symb. Logic, 30:359-62; also in: Davis, 1965, pp. 5-38 (see J. Symb. Logic, 31:486-89); with a note by Gödel, in: van Heijenoort, 1967, pp. 596-616. Italian trans.: Agazzi, 1961, pp. 203-28; Portuguese trans.: Lourenço, 1979, pp. 245-90.)

b. Über Vollständigkeit und Widerspruchsfreiheit. Ergeb. math. Kolloq. (for 22 Jan. 1931, publ. 1932), 3:12-13. (English trans., with a remark by Gödel added to Ftn. 1: van Heijenoort, 1967, pp. 616-17.)

c. Eine Eigenschaft der Realisierung des Aussagenkalküls. Ergeb. math. Kolloq. (for 24 June 1931, publ. 1932), 3:20-21 .

d. Letter to Zermelo, October 12, 1931. In: Grattan-Guinness, 1979, pp. 294-304 .

e. Über Unabhängigkeitsbeweise im Aussagenkalkül. Ergeb. math. Kolloq. (for 2 Dec. 1931, publ. 1933), 4:9-10 .

1932 a. Über die metrische Einbettbarkeit der Quadrupel des *R3* in Kugelflächen. Ergeb. math. Kolloq. (for 18 Feb. 1932, publ. 1933), 4:16-17 .

b. Über die Waldsche Axiomatik des Zwischenbegriffes. Ergeb. math. Kolloq. (for 18 Feb. 1932, publ. 1933), 4:17-18 .

c. Zum intuitionistischen Aussagenkalkül. Anz. Akad. Wiss. Wien, Math. naturwiss. Kl. (for 25 Feb. 1932), 69:65-66 .(Reprinted, with an opening clause attributing the question to Hahn, in Ergeb. math. Kolloq. (for 1931-32, publ. 1933), 4:40; and in Berka and Kreiser, 1971, p. 186.)

d. Zur intuitionistischen Arithmetik und Zahlentheorie. Ergeb. math. Kolloq. (for 28 June 1932, publ. 1933), 4:34-38. (English trans.: Davis, 1965, pp. 75-81 (see J. Symb. Logic, 31:490-91). Portuguese trans.: Lourenço, 1979, pp. 359-69.)

e. Eine Interpretation des intuitionistischen Aussagenkalküls. Ergeb. math. Kolloq. (for 1931-32, publ. 1934), 4:39-40 . (English trans., 1969, see J. Symb. Logic, 40:498; reprinted in: Berka and Kreiser, 1971, pp. 187-88).

f. Bemerkung über projektive Abbildungen. Ergeb. math. Kolloq. (for 10 Nov. 1932, publ. 1934), 5:1 .

1933a. With K. Menger and A. Wald. Diskussion über koordinatenlose Differential geometrie. Ergeb. math. Kolloq. (for 17 May 1933, publ. 1934), 5:25-26 .

b. Zum Entscheidungsproblem des logischen Funktionenkalküls. Monatsh. Math. Phys. (received 22 June 1933), 40:433-43 . (For a correction, see Goldfarb, 1981.)

1934 *On Undecidable Propositions of Formal Mathematical Systems* . Mimeographed notes by S. C. Kleene and J. B. Rosser on lectures at the Institute for Advanced Study, Feb.-May , 1934, 30 pp. (Extensively distributed, deposited in some libraries, and listed in the J. Symb. Logic Bibliography 1:206; printed with corrections, emendations, and a Postscriptum, by Gödel in Davis 1965, pp. 41-74 (see J. Symb. Logic, 31:489-90). A relevant Gödel letter of 15 Feb. 1965 is quoted there on p. 40, and in Kleene, 1981, pp. 60, 62, and of 23 April 1963 in van Heijenoort 1967, p. 619. Portuguese trans. in Lourenço 1979, pp. 291-358.)

1935 Über die Länge von Beweisen. Ergeb. math. Kolloq. (for 19 June 1935, with a remark added in the printing 1936), 7:23-24 . (En english trans.: Davis 1965, pp. 82-83 (see J. Symb. Logic. 31:491). Portuguese trans.: Lourenço 1979, pp. 371-75.)

1938a. The consistency of the axiom of choice and of the generalized continuum-hypothesis. Proc. Natl. Acad. Sci. USA (communicated 9 Nov. 1938), 24:556-57 .

b. The consistency of the generalized continuum-hypothesis. Bull. Am. Math. Soc. (abstract of a talk on 28 Dec. 1938, publ. 1939), 45:93 .

1939a. *The Consistency of the Axiom of Choice and of the Generalized Continuum-Hypothesis with the Axioms of Set Theory* . Notes by G. W. Brown on lectures at the Institute for Advanced Study during the fall term of 1938-39. Ann. Math. Stud., no. 3. Princeton, N.J.: Princeton U. Press, 1940. (Reprinted 1951 with corrections and three pages of notes by Gödel; the seventh and eighth printings, 1966 and 1970, include additional notes and a bibliography. Russian trans. 1948, see J. Symb. Logic. 14:142.)

b. Consistency-proof for the generalized continuum-hypothesis. Proc. Natl. Acad. Sci. USA (communicated 14 Feb. 1939), 25:220-24 . (Reprinted in Felgner, 1979; corrections in (1947, Ftn. 23, = Ftn. 24 in the 1964 reprint); also see Kleene 1978 and Wang 1981, Ftn. 7.)

1944 Russell's mathematical logic. In Schilpp (1944, pp. 123-53).(Reprinted, with a prefatory note by Gödel, in Benacerraf and Putnam 1964, pp. 211-32; Italian trans.: 1967, see J. Symb. Logic, 34:313; French trans.: 1969, see J. Symb. Logic, 40:281; reprinted, with Gödel's 1964 prefatory note expanded, a reference supplied in Ftn. 7, and Ftn. 50 omitted, in Pears 1972, pp. 192-226; Portuguese trans.: Lourenço 1979, pp. 183-216.)

1946 Remarks before the Princeton Bicentennial Conference on Problems in Mathematics [December 17], 1946. Plans for publication of the papers presented at the conference fell through, as the conferees learned only much later. When the Davis anthology

(1965) was being planned, Kleene drew the attention of the publisher to this paper of Gödel and supplied a copy of the text that had been in his file since 1946, which with Gödel's permission (and Gödel's addition of a four-line footnote) was then published as Davis (1965, 84-88). (Italian trans.: 1967, see J. Symb. Logic, 34:313; reprinted, with trifling changes in punctuation and phrasing, and the substitution of "It follows from the axiom of replacement" for "It can be proved" at the end, in Klibansky 1968, pp. 250-53; Portuguese trans.: Lourenco 1979, pp. 377-83.)

1947 What is Cantor's continuum problem? Am. Math. Mon., 54:515-25 ; errata, 55:151 . (Reprinted, with some revisions, a substantial supplement, and a postscript, by Gödel, in Benacerraf and Putnam 1964, pp. 258-73; Italian trans.: 1967, see J. Symb. Logic, 34:313; Romanian trans.: Pârvu 1974, pp. 317-38; Portuguese trans.: Lourenço 1979, pp. 217-44; see (1958c) and (1973).)

1949a. An example of a new type of cosmological solutions of Einstein's field equations of gravitation. Rev. Mod. Phys., 21:447-50 .

b. A remark about the relationship between relativity theory and idealistic philosophy. In Schilpp (1949, pp. 555-62). (German trans., with some additions by Gödel to the footnotes: Schilpp 1955, pp. 406-12.)

1950 Rotating universes in general relativity theory. In: *Proc. Int. Cong. Math.* (*Cambridge, Mass., 1950*) vol. 1, pp. 175-81 . Providence, R.I.: American Mathematical Society, 1952 .

1952 [A popular interview with Gödel:] Inexhaustible. The New Yorker, Aug. 23, 1952, pp. 13-15 .

1956 Gödel expresses regret at Friedberg's intention to study medicine in: The prodigies. Time, March 19, 1956 , p. 83 .

1957 Kreisel (1962, pp. 140-42) states some results as having been communicated to him by Gödel in 1957.

1958 a. Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes. Dialectica, 12:280-87 . (Reprinted in *Logica, Studial Paul Bernays Dedicata,* Bibliotheque Scientifique no. 24, pp. 76-83. Neuchatel: Griffon, 1959. Russian trans. 1967, see J. Symb. Logic, 35:323; English trans.: 1980 [with a bibliography of work resulting from this paper], J. Philos. Logic, 9:133-42. According to the review of this translation by Feferman, Math. Rev., 81i:3410-11, there was an unpublished earlier English trans., which was revised several times by Gödel and "contained a number of further notes which considerably amplified and in some cases corrected both technical and philosophical points." Italian trans.: Cagnoni 1981, pp. 117-23; also see (1961) and Spector (1961).)

b. Kreisel (1958, pp. 321-22) attributes the substance of his remarks 2.1 and 2.3 to Gödel.

c. A statement by Gödel is quoted in Ulam (1958, Ftn. 5, p. 13).

1961 A postscript by Gödel is on p. 27 of Spector (1961).

1963-66a. Benacerraf and Putnam (1964), Davis (1965), and van Heijenoort (1967) include various contributions by Gödel to their reprints and translations of his (1930a), (1931a), (1931b), (1934), (1944), and (1947) .

b. A letter from Gödel is quoted in von Neumann (1966, pp. 55-56).

c. A 1966 greeting by Gödel is on p. (viii) of Bulloff et al. (1969).

1967 An extract from a 30 June 1967 letter from Gödel is in Rautenberg (1968, p. 20).

1973 A communication from Gödel of October 1973 is quoted in Greenberg (1980, p. 250).

1974a. Communications from Gödel are reproduced in Wang (1974, pp. 8-12, 84-88, 186-90, and 324-26).

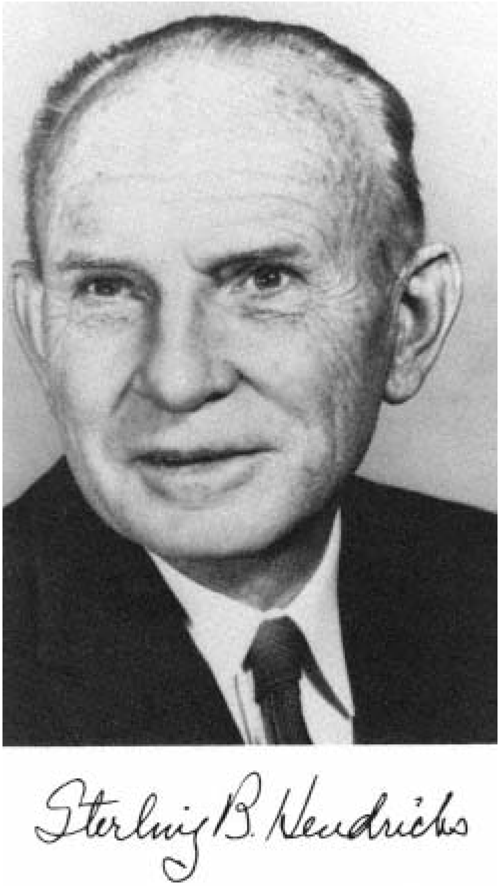
b. Gödel contributed a statement to the preface to Robinson (1974).

c. Reinhardt (1974, Ftn. 1, p. 189) reports on discussions with Gödel.

d. A 1974 memorial tribute to Robinson by Gödel appears opposite the frontispiece of Saracino and Weispfennig (1975).

1976-77a. Wang (1981), begins with the words, "The text of this article [but not the footnotes and section headings] was done together with Gödel in 1976 to 1977 and was approved by him at that time."

b. The text of Kleene (1978) is composed of communications from Gödel of May and June 1977. See Kreisel's review in the Zentralblatt (1979) 401:12-13 .



Sterling Brown Hendricks

April 13, 1902-January 4, 1981

By Warren L. Butler and Cecil H. Wadleigh

Freedom to inquire into the nature of things is a rewarding privilege granted to a few by a permissive society.

Sterling Hendricks
The Passing Scene, 1970

Sterling Brown Hendricks was born in Elysian Fields, Texas, a small village in the eastern part of the state. The family had deep roots in the Old South. When Texas seceded from the Union in 1861, the area around Elysian Fields sent a company of men, known as the S. B. Hendricks Company, to the Confederate Army under the command of Colonel Sterling Brown Hendricks, Sterling's grandfather. The colonel, a native of Alabama, grew up and studied law in Mississippi and moved to Elysian Fields in 1843, where he became a merchant and a farmer. He was also a scholarly man with a large library of books on law, religion, and the classics.

Sterling's father, Dr. James Gilchrist Hendricks, was born in Elysian Fields in 1854. He received medical degrees from Louisiana and Tulane universities in New Orleans and, after interning at Bellevue Hospital in New York City, returned home to practice medicine. Sterling's mother, Martha Daisy (Gamblin) Hendricks, was born in Caddo Parrish, Louisiana, in 1873. She graduated from Mansfield Female College in Louisiana as valedictorian of her class. After graduation, she went to Elysian Fields to teach school and met Dr. James Hen dricks, who was then a widower. They were married in 1893 and had five children; Sterling was the fourth.

Sterling received much of his early schooling from his mother. There was no high school in Elysian Fields (only a one-room school house), so Sterling lived with an aunt in Shreveport, Louisiana, during his high school years. Following his graduation, the family moved to Fayetteville, Arkansas, so that several of the children could attend the university there. Sterling graduated from the University of Arkansas in 1922 with a bachelor's degree in chemical engineering. He studied geology and chemistry at the graduate level at the University of Iowa in 1923 and received a master of science in chemistry from Kansas State University in 1924. Then, in the fall of 1924, he began his doctoral studies at the California Institute of Technology.

On entering Cal Tech, A. A. Noyes, the director of the Gates Chemical Laboratory, suggested to Sterling that he work on X-ray crystallography in the laboratory of Roscoe C. Dickinson. Dickinson, who four years earlier had received the first Ph.D. degree given by Cal Tech, was going to Europe that year, so Sterling worked with Linus Pauling, who had arrived in Dickinson's laboratory two years earlier to learn the techniques of X-ray crystallography. Thus began a close friendship that lasted until Sterling's death. Sterling received his Ph.D. degree in 1926, with a major in chemistry and with minors in physics and mathematical physics.

Sterling began his Ph.D. research with a reinvestigation of the structure of the minerals corundum, Al_2O_3, and hematite, Fe_2O_3, which had been studied earlier by W H. and W. L. Bragg. He confirmed that the positions previously assigned to the aluminum and iron atoms were correct, but the positions for the oxygen atoms were not. The refined structure provided a clearer understanding of the interatomic forces in these crystals. He also determined the structure of sodium and potassium azide, showing that the three nitrogens were in a linear array rather than in a cyclic structure—as had been proposed by some chemists. His Ph.D. thesis also included the determination of the crystal structures of several cupric chloride dihydrates. He also worked jointly with Maurier L. Huggins, a postdoctoral fellow in the laboratory, on the structure of pentaerythritol, $C(CH_2OH)_4$. Sterling pointed out that the pyramidal structure that had previously been proposed might be incorrect, and that another space group permitted a tetrahedral arrangement of the bonds around the central carbon atom. This latter structure was confirmed a decade later.

He continued structure determinations of simple organic compounds during two postdoctoral years—1926-27, at the Geophysical Laboratory of the Carnegie Institution of Washington, and 1927-28, at the Rockefeller Institute of Medical Research—with work that made important contributions to the chemistry of carbon compounds. In 1928 he joined the Fixed Nitrogen Laboratory of the U.S. Department of Agriculture. He was recruited by F. G. Cottrell, who hoped to benefit mankind by solving the problems of nitrogen fixation. In later years Sterling would often speak of Cottrell. Cottrell, as the inventor of the electrostatic precipitator, donated the returns from his patents to support research through grants from the Research Corporation. It was from Cottrell that Sterling gained an appreciation for practical applications of scientific research. For Sterling, the highest goal of science was to achieve a solution to an important practical problem.

In the years that followed, Sterling made monumental contributions to mineralogy and the study of soils. His early Ph.D. research on corundum and hematite was followed by studies of other minerals, including zircon, apatite, gypsum, kaolinite, anauxite, valentinite, alunite, the jarosites, dickite, halloysite, hydrated halloysite, talc, pyrophyllite, vermiculite, chlorite, montmorillionite, nacrite, cronstedite, glauconite, celladonite, gibbsite, endellite, and the micas. This work resulted in an extensive understanding of clays as important components of soils and of the structural basis of ion exchange of charged groups—central to understanding soil fertility. He used his expertise in X-ray diffraction and mathematical physics to determine the structure of phosphate fertilizers and also of bone. In terms of human welfare, one could make a strong case that Hendricks' most important research was in collaboration with soil scientists toward determining the structure of soil constituents. In 1930 Hendricks and Fry published the results of their research on soil colloids. This paper is now recognized as the most important elucidation of the nature and properties of soils ever published. A bit of history may be in order.

In 1850 a Scottish chemist by the name of J. T. Way published a paper on the power of soil to absorb manure. He had allowed moderately dilute solutions of neutral salts to seep downward through soil columns. He collected the percolate and found that its chemical composition was usually different from that of the applied solution. For example, when an ammonium chloride solution was applied, the percolate contained little ammonium; the percolate was mostly calcium chloride. Way concluded that there was an interaction between the applied solution and the soil particles. He erroneously concluded that the reaction was irreversible. The distinguished German chemist, Justus von Liebig, looked on Way's report with utter contempt and had no reservations in saying so. For the next three-quarters of a century a vigorous controversy prevailed among soil chemists; some supported Way and others Liebig. These arguments were settled for all time by the publication of the paper by Hendricks and Fry. By using X-ray diffraction procedure, they conclusively proved the crystalline nature of colloidal clay with the prevalence of negative charges that would absorb and desorb cations. They showed that Way was on the right road.

With the exponential increase taking place in world population, with the prevalence of famine and malnutrition on this planet, and with the finite limitation on the extent of arable soils available for food production, this research by Hendricks and Fry was of inestimable value. These findings opened the door to an exceedingly important understanding of the chemistry involved in maintaining high potential in soil productivity, and in providing a valid chemical basis for the reclamation of the alkali soils of arid regions.

In 1952 Sterling received the Arthur L. Day Medal awarded by the Geological Society of America for outstanding work in physics and chemistry advancing the geological sciences. The citation stated:

> Sterling Hendricks, an able technician and a masterful and imaginative theoretician, has been in the forefront of those who have given us a rational understanding of these most complex and most important minerals. His elucidation of the structure of layered minerals and his demonstrations of the dependence of clay mineral properties upon structural considerations have been outstanding. Not only has he provided specific data on the kaolin minerals and, with Ross, on the complex montmorillionite group, but he has at the same time developed fundamentals of broad application, as for example in his studies of the polymorphism of the micas and of the nature of the water layer, and in the determination of minerals with disordered structure and of minerals with random layer sequences. He has never been content merely to explain the well-behaved growths in the mineral world, but has gone on to decipher for us some of natures "mistakes."

Linus Pauling considers that Sterling's work on the clay minerals was his most important contribution to knowledge.

The work on soils and fertilizers also led to investigations of hydrogen bonds. Hendricks was among the first to use infrared spectroscopy for the study of molecular structure. Pimentel and McClellan wrote, some twenty-five years later in their book on the hydrogen bond, that this work provides " . . . the most sensitive, the most characteristic and one of the most informative manifestations of the H-bond. From this has grown the immense volume of work . . . ." Hendricks also became an expert in radiochemistry, and he showed how fertilizers tagged with radioactive phosphorus could be used to follow the uptake of phosphorus by the roots of plants. Of course, not all of his scientific endeavors were successful. Sterling tried to obtain a diffraction pattern from crystals of horse hemoglobin some five years before the first successful X-ray crystallographic studies of a protein were made in (Cambridge, England. His attempts failed because the protein denatured as the specimen was dried for mounting. He also attempted to obtain a diffraction pattern of a chromosome before it was known how nucleic acids could be separated in a native state. Thus, in the course of many successes he had some grand failures—but even the failures pointed toward forthcoming spectacular successes in biology.

Sterling's scientific career took an abrupt change in direction in the early 1940s. A brief history of this period and the subsequent developments is appropriate since it was in these new areas of plant physiology and photobiology that his most creative contributions to knowledge lie. In 1920 two scientists in the USDA, H. A. Allard and W. W. Garner, discovered that daylength was a critical factor in determining when during the course of the year a given species of plant would flower—a phenomenon which they called photoperiodism. By the middle 1930s the work on photoperiodism was being continued in the USDA by H. A. Borthwick and M. W. Parker, primarily in studies of the flowering of short-day plants (plants that flowered on a short day—long night regime). In the early 1940s they sought out a fellow USDA employee, Hendricks, to discuss how they should proceed in their investigation of the effects of light in photoperiodism. It was then known that brief irradiations with light given during the long nights would inhibit the flowering of short-day plants. They realized that they might be able to determine the action spectrum (that is, the effectiveness of different wavelengths of light) for this inhibitory effect of light on flowering, and they agreed to pool their scientific talents toward this end. World War II intervened, and it was not until 1944 that they began their collaboration.

The key to the early successes of this work lay in the experimental design of the action spectroscopy. A large spectrograph was constructed using two exceptionally large glass prisms, which Hendricks had used previously for his infrared studies of hydrogen bonds, and a large second-hand carbon arc lamp like those used in theatres of the time. Absolute energy calibrations were made across the spectrum using a thermopile that was calibrated against a standard lamp. Of equal importance to the success of the work was the knowledge of how action spectra should be measured. Hendricks understood that it was essential to keep the irradiation periods brief to extract the specific characteristics of the photoreaction from the great complexity of the biological response, which might be assayed some hours or days later. Borthwick and Parker provided the plants whose flowering response was sensitive to brief periods of irradiation, and Hendricks provided the irradiation fields of large area, high spectral purity, and adequate intensity. Within a year they had an action spectrum for the floral inhibition of a short-day plant, soybean, which showed a pronounced sensitivity to red light.

Action spectra were then measured on a number of different plants and on several different light-sensitive responses, including the floral inhibition of other short-day plants, the flowering of long-day plants where the night break irradiation induced flowering, several growth responses in etiolated plants grown from seed in darkness, and the germination of lettuce seed. All of these investigations yielded essentially the same action spectrum, with a peak action in the red near 660 nm. It was concluded that the same pigment was involved in all of these responses.

The experiments on seed germination were to provide key observations for elucidating the unusual photochemical properties of the pigment. It was known from earlier longterm irradiation experiments (by Flint and McAlister) that red light promoted the germination of lettuce seed. The USDA group expected to find their typical red action spectrum for this response. Flint and McAlister had also reported, however, that light in the near infrared region, just beyond the limits of vision, inhibited the germination—but the significance of the inhibitory effect of such wavelengths of light was generally unappreciated. The USDA group rediscovered the inhibitory effect of these far-red wavelengths of light. They demonstrated that seeds potentiated to maximal germination by a brief irradiation with red light could be inhibited to minimal germination by a subsequent brief irradiation with far-red light, and that these promotive and inhibitory effects were repeatedly reversible. The action spectrum for the photoinhibition of germination showed a maximum at 730 nm. Hendricks deduced from these experiments that the germination of lettuce seed was controlled by a pigment that existed in two interconvertible forms: a red absorbing form, P_R, with an absorption maximum at 660 nm, and a far-red absorbing form, P_{FR}, with an absorption maximum at 730 nm. He concluded that red and far-red light caused transformations between the two forms:

$$P_R \underset{\text{far-red}}{\overset{\text{red}}{\rightleftarrows}} P_{FR}$$

After the unusual property of photoreversibility had been found in the germination response of lettuce seed, the other red-sensitive photoresponses were reexamined. They were found to show the same type of photoreversible antagonism between red and far-red light. The unique and unusual pigment system appeared to be ubiquitous in higher plants and to control a number of physiological responses.

Hendricks was primarily responsible for the incisive insights that penetrated to the molecular level of the photocontrol process. A given degree of a physiological display would be used as an endpoint in a titration of responses versus incident energy. Whereas most plant physiologists of the time became lost in the great complexity of the biological system, Hendricks designed experiments in such a way that the complexities of the dark metabolism canceled out, leaving the pristine properties of the photoreaction to be revealed. The elegance of the approach culminated in a remarkable study. The physiological responses of seed germination and internode elongation of etiolated bean plants were titrated from both extremes of the reversible photoreaction, using red and far-red light. After making allowances for the light-scattering properties of the biological tissue and the quantum efficiencies of the photoreactions, Hendricks calculated—from the absolute energies required to achieve given degrees of response and the first-order nature of the photoreactions—that the molar extinction coefficients of the two forms were between 10^4 and 10^5 liters $mole^{-1}$ cm^{-1}. He concluded—on the basis of these high values for molar extinction coefficients and the absence of any visible color in albino mutants of barley, whose growth responses were fully sensitive to red and far-red light—that the pigment system was functional at very low intracellular concentrations. The insight and clarity of vision that allowed Hendricks to extract a molar extinction coefficient from a complex physiological display were char acteristic of his approach to science. Unfortunately, the paper reporting these findings was largely ignored. At the time, few workers in the field made the effort to follow the logic of the analysis.

Hendricks had deduced the essential molecular properties of this remarkable pigment system from the physiological studies by the early 1950s. The absorption spectra of the two forms and the reversible nature of the photoreaction were known from the action spectroscopy. It was proposed from the absorption spectrum that the chromophore of P_R was an open-chain tetrapyrole, similar to that of allophycocyanin. It was even proposed, on the basis of the low intracellular concentrations, that the pigment was an enzyme, and therefore a protein, and that P_{FR} was the active form of the enzyme. In addition to the photochemical properties, the physiological studies indicated that there was a slow dark transformation of P_{FR} to P_R. This dark transformation of P_{FR} back to P_R was proposed to be the basis of the timing mechanism that enabled photoperiodic plants to distinguish long nights from short nights. Nevertheless, most plant physiologists of the time did not believe that their subject matter was capable of revealing such molecular detail and, in the absence of direct proof, they were inclined to regard the pigment as a "pigment of the imagination."

Sterling's group had the good fortune to join another group headed by Karl H. Norris, an agricultural engineer who had developed several spectrophotometers that could accommodate dense, light-scattering materials. From time to time Hendricks or H. W. Siegelman, a plant biochemist who was then associated with Borthwick and Hendricks, would examine these samples in the spectrophotometer for photo-reversible absorbance changes in the red and far-red regions of the spectrum. All of the initial attempts with plant tissues that were known to be sensitive to red and far-red light were unsuccessful, and concern arose that this approach was hopeless because of the very low intracellular concentration of the pigment. Finally, in the summer of 1959, in spectrophotometric measurements of cotyledons from dark-grown turnip plants that synthesized anthocyanin under control by red and far-red light, the absorbance changes were found. The difference spectrum between the red and far-red irradiated tissue was precisely what the action spectra predicted, and the effects of light were fully reversible. Furthermore, the photoreversible nature of the pigment persisted in cell-free extracts of the plant tissue. The pigment was immediately shown to be a protein by heat denaturation, and Siegelman took on the task of purifying the material. The success of these measurements depended on finding a tissue that had measurable amounts of the pigment. For reasons that are still not understood, dark-grown seedling plants accumulate much higher levels of the pigment than are needed for photocontrol purposes in mature green plants. The pigment was dubbed phytochrome, which Hendricks seemed to resist initially, but he recognized the utility of having a trivial name and soon came to accept it.

All of the essential predictions that Hendricks had made over the years were confirmed once the purified material was in hand. The absorption spectra of P_R and P_{FR} were right on the mark. The reversible photochromic nature of the pigment persisted in the purified state, the pigment changing from a blue color in the P_R form to less colored, slightly more greenish hue in the P_{FR} form. The estimates of the extinction coefficients proved correct, and chromophore was found to be an open-chain tetrapyrole, of the type suggested, that isomerizes under the action of light. The chromophore is attached to a protein, and the P_{FR} form appears to be the active state of the combination. And the dark transformation of P_{FR} to P_R, which was postulated to be the basis of the timing mechanism of photoperiodic plants, was shown to occur in vivo by direct spectrophotometric measurements. Surely, if Alfred Nobel had seen fit to include the plant sciences amongst his prizes, Sterling Hendricks would have been a recipient.

Sterling's work on phytochrome and the physiological responses controlled by phytochrome continued to his death. Most of the initial speculations about the mode of action of phytochrome centered about the mechanism of gene activation. In studies of leaf movement, Hendricks and Borthwick made the seminal discovery that control was exerted at the level of membranes. After his formal retirement from the USDA in 1970, he continued studies of seed germination and the nature of dormancy with great vigor in collaboration with R. B. Taylorson. They decided to use seeds as media with which to probe the mechanisms of phytochrome action, as well as the basic nature of dormancy. They began by probing the nature of phytochrome action as affected by temperature change. Evidence began to indicate that cell membrane activity was involved in temperature effects on seed germination. Data revealed leakage of amino acids as a function of temperature. Changes in germination physiology were again found to correlate with observed effects of temperature on changes in fluorescence associated with membrane preparations.

The team of Hendricks and Taylorson pursued studies on the action of anesthetics as seed germination stimulants. Low molecular weight alcohols, aldehydes, and similar structures are active as anesthetics in animals. Anesthesia is associated with effects on cell membranes in animals. Membranes so treated tend to swell, and it was of interest to ascertain the counteractive effect of hydrostatic pressure on anesthetic action. The findings accordingly linked anesthetic action in seeds with membrane phenomena found in animal systems.

The studies led to the suggestion that dormancy control in seeds is a function of cell membranes.

Sterling's science was characterized not only by its depth of penetration but also by its incredibly broad scope. Over the years he lectured to scientific organizations and to university groups on the structure of matter, electron diffraction from gases, the nature of bone, hydrogen bonding in organic compounds, base exchange in soils, photosynthesis, plant nutrition, radioisotopes in agriculture and, of course, many aspects of photomorphogenesis in plants. Something of that breadth and depth was indicated by his election to the National Academy of Sciences. In the early 1950s, when the Botany Section was considering him for nomination, they found that he was also being considered by the geologists and the chemists. He was elected to the Academy in 1952, at a time when there were 480 members. He joined the Botany Section and was active in the affairs of the Academy for the rest of his life.

Sterling's great breadth of science is also indicated in the many honors that came to him over the years. There was the Hillebrand Prize in 1937, awarded by the Chemical Society of Washington for outstanding work using the optical properties of crystals in the analysis of atomic arrangements; the Science Award of the Washington Academy of Science in 1942, for discoveries about the rotation of molecular and ionic groups in crystals; and his election as fellow of the American Society of Agronomy in 1945, in honor of his discovery of the nature of soil clays and the significance of cation exchange. The Day Medal, which he received from the Geological Society of America in 1952, was mentioned earlier. He was the fourth recipient of that award. He also received the Distinguished Service Award from the U.S. Department of Agriculture in 1952 for his contribution of fundamental knowledge to the advancement of science. In 1954 he was elected president of the Mineralogical Society of America and a trustee of the American Society of Plant Physiologists. In 1958 he was in the first group of five recipients to receive the President's Award for Distinguished Civilian Service from President Eisenhower. Other recipients that year included FBI Director J. Edgar Hoover and Ambassador Charles E. (Chip) Bohlen. Hendricks' citation read: "His discoveries in soil clays, phosphate minerals, radioisotopes, plant physiology and fundamental chemistry made him one of the most distinguished and honored scientists of our time." He was elected president of the American Society of Plant Physiologists in 1959. He received the Rockefeller Public Service Award in 1960 and shared the Hoblitzelle Award in the Agricultural Sciences with H. A. Borthwick in 1962. He and Borthwick also shared the Stephen Hales Award from the American Society of Plant Physiologists in 1962. In 1968 he received the Distinguished Alumnus Award of the California Institute of Technology. He was awarded the National Medal of Science from President Ford in 1976, and in the same year the Finsen Award, which is the highest honor bestowed by the International Society of Photobiology. From 1974 to his death he was a member of the Committee for Research and Exploration of the National Geographic Society, where his great breadth of knowledge was put to good use to evaluate applications for research grants. There he was known as "a man for all seasons." As a member of that committee, he made field trips to Kenya, Tanzania, Jordan, Iran, and Israel. On the day of his death, the flag was flown at half mast over the National Geographic Society Building in Washington, D.C.

Outside of the laboratory, Sterling's main diversion was mountain climbing, and he seems to be regarded as highly among alpinists as he is among scientists. His obituary in the *Washington Post,* which was headlined: "Chemist Was in Group that Climbed McKinley," referred to him as "a chemist and a mountain climber of note." *Up Rope,* a mountaineering publication, after paying tribute to his accomplishments in science, stated that he was a pioneer in American mountaineering whose "attainments were comparable with or even superior to—if possible—those in science." His love of nature undoubtedly began as a boy in Elysian Fields, which was named for its lovely countryside of rolling hills and pine forests. As a graduate student, he back-packed about 100 miles through the Santa Lucia mountains of California, from Cambria to Monterey. He was also a long-distance swimmer, and at one time he attempted to swim around Catalina Island, but history does not record whether that attempt was successful. He was a member of the Alpine Clubs of the United States and Canada. During the 1930s, Canadian authorities officially recognized that he climbed four previously unscaled peaks in the British Columbian Rockies. In 1942 he was a member of the third party to conquer Mount McKinley in Alaska, North America's highest mountain. These excursions did not always go smoothly. In 1957 Sterling and a group of mountain climbers from the Washington, D.C., area planned an extensive expedition into the mountains of Western Canada just prior to the annual meeting of the Plant Physiologists, which was being held at Stanford University that year. Sterling arrived at those meetings a day or two late. He wore a body cast on the upper half of his body, with the excuse that he had taken a bad spill. It was learned later that the group, while roped together, had plunged some 250 feet down the side of the mountain. Sterling, who had a cracked vertebra and a broken shoulder joint but was still ambulatory, went for help. The journey out over rugged terrain involved two rappels and almost two days travel. The night was spent in bivouac on snow and ice, with no food and inadequate clothing. He had left his food and clothing behind so that the others might survive. He came to the meetings directly from the hospital; if it hadn't been for the upper body cast, it is doubtful that anyone would have known what had happened.

Sterling married Edith Ochiltree of Philadelphia in 1931. They were visiting their daughter, Martha O'Neill, and her family, including two grandchildren, in Novato, California, during the Christmas holidays in 1980. Sterling came down with the flu and took a vaccine shot in an effort to minimize the symptoms. He died shortly afterwards, on January 4, 1981, of the Guillain-Barré syndrome. At the time of his death he was still young in spirit, full of creative ideas, and deeply involved in productive lines of research.

We are indebted to Dr. Linus Pauling for having had access to the biographical memoir of Sterling Hendricks he wrote for *The American Mineralogist.*

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Carl L. Hubbs

Carl Leavitt Hubbs

October 18, 1894-June 30, 1979

By Elizabeth N. Shor,
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and John D. Isaacs

Carl Leavitt Hubbs, elected to the National Academy of Sciences in 1952, died on June 30, 1979, at the age of eighty-four. In 1975 he said: "I have been praised, or criticized, as the case may be, for being one of the last of the dying tribe of general naturalists, a disciple of natural philosophy."[1] Such he was, for while his expertise was fishes, he also contributed significantly to our knowledge of marine mammals, archeology and climatology, biography and history of science, evolution and ecology, and conservation.

His earliest known paternal ancestor was Samuel Hubbs, who emigrated from Scotland with two brothers prior to the American Revolution and farmed in the Mohawk Valley, New York State. His son Alexander Hubbs lived out his life in the same area. Alexander's son Daniel moved to Jefferson County, New York, then to Wisconsin in 1850, and to Minnesota in 1856.

Daniel's son Charles Leavitt Hubbs, father of Carl Leavitt Hubbs, was born on June 6, 1843, in Pamelia-Four-Corners, Jefferson County, New York, and moved with his father to Wisconsin. When he was fourteen, Charles began three years

[1] "Biological Oceanography, Geochronology, and Archeology Along the Pacific Coast of Middle America and California," talk given at University of Nevada, Las Vegas, April 17. 1975.

of employment with a book and stationery firm in New York City; in 1859 he joined a brother near Vicksburg, Mississippi, and in 1860 returned to his father's home in Minnesota. There he married and fathered six children before the marriage was dissolved. Following Civil War service with the First Minnesota Infantry regiment, he moved often and tried his hand at various trades: In 1866 he farmed in Missouri, in 1867 he was in the mercantile business in Minnesota, and in 1870 he tried the lumber business in that state. In 1873 he settled for a time in Edwards County, Kansas, as a farmer, dealer in real estate, county surveyor, and proprietor of a newspaper.[2] Carl later noted that during the years in Kansas his father shot from the declining herds at least one buffalo and one pronghorn antelope, and that "as one saving grace he preserved in alcohol the pronghorn's unborn fetus, which he [much later] gave me to be preserved in the Museum of Zoology at the University of Michigan—as probably the only extant specimen of that species from Kansas."[3]

In 1894, with his second wife and their son Leonard Goss Hubbs, Carl's father moved west to work placer claims in Arizona. Carl later recalled:

> Soon afterward, as the placer operation petered out, Dad took off for the Santa Fe Railroad at Williams [Arizona], leaving my mother and my brother, and me, still unborn, to follow by horse and wagon. Less skilled than Dad in the ways of the very scantily populated West, she got lost, and after wandering for three days and nights without food or water finally saw the headlight of the Santa Fe and managed to wave down the engineer before fainting. Soon after she reached Williams, I was ushered into the world on October 18, 1894 by some midwife, two months prematurely. . . . I was soon brought to California and got my first taste of the Mohave Desert. At Daggett Dad ran the water pump for the thirsty Santa Fe en

[2] *The United States Biographical Dictionary* (Chicago: C. S. Lewis & Co., 1879), pp. 342-43.

[3] "Preservation of Species and Habitats," talk given to Scholia (a San Diego club), October 9, 1973.

> gines. I must have been a real desert rat, for I walked freely at eight months and ran away from home at ten months (though I didn't talk until three years old and was diagnosed later as mentally defective). After a short spell in the town of Los Angeles, I was carried in 1896 to San Diego, when the population was about 17,000, with a high percentage of Mexican origin."[4]

Carl's father located an iron-ore property in the desert, which he later sold profitably, and in San Diego he served as an assayer and developed housing property.

In the open country of San Diego's mesas, valleys, and shoreline, Carl and his brother wandered freely:

> watching with boyhood interest the burrowing owls . . . catching horntoads for pets, and otherwise communing with nature . . . . For diversion we, or I alone, often paddled our tiny sneak-boat over San Diego Bay, then still in near primeval condition. I recall once chasing a Western Grebe . . . until at last it was exhausted and rose so close that I grabbed it by the neck [and was bitten] . . . . Once near the harbor entrance I saw close by a bull elephant seal, which in my childish fancy I thought to be a walrus.[5]

In later years Hubbs liked to recall that on one childhood trip to the beach of La Jolla (at the north end of San Diego), he had "envisioned a long building sweeping along the slope [where Scripps Institution of Oceanography is now located], containing case after case of magnificent sea shells, by which I, in a bright blue uniform, kept explaining the exhibit to the assembled public."[6]

Hubbs's parents were among a group that objected to a new ruling by the San Diego schools that children must be vaccinated against smallpox. Several families persuaded Katherine Tingley to open a private school (without required vaccination), which she was glad to do as an expansion of the

[4] *Ibid.*
[5] *Ibid.*
[6] "Some Highlights from My 61-year Career in Marine Biology." talk given at Scripps Institution, May 2, 1974.

organization that she headed: the Universal Brotherhood and Theosophical Society. The Society incorporated Egyptian lore, reincarnation, Greek architecture, music and drama, pacifism, and vegetarianism. Young Carl had not been doing well in the public school. The new one, the primary school of the Raja Yoga Academy, appealed to him at first, and he became a keen student; but after three years he rebelled against its militant discipline and was dismissed.[7]

Carl's mother was Elizabeth Goss Johnson Hubbs (she had been married briefly to a man named Johnson before this marriage), the daughter of Leonard Goss, a prominent lawyer of Cincinnati, Ohio. Carl recalled that his mother had told him in his youth that the British naturalist Philip H. Gosse was related to them. Elizabeth Goss had taught art and other subjects. Following her divorce from Charles Hubbs in 1907, she and the two boys returned to the Midwest for a year and stayed with various relatives. Of that time, Hubbs said much later:

> I saw much of nature that I had largely missed before. For the first time in my memory I saw lightning close enough to cause thunder, witnessed the colors of fall, enjoyed a chance to skate on ice, suffered lasting frostbite on breaking through the ice, experienced the reawakening of nature in the spring, dug in the rich Paleozoic fossil beds in Cincinnati, spent the summer on a farm in northern Ohio . . . often wandering away from field work to see new kinds of animals and plants; caught bullfrogs in the new state of Oklahoma, watched tornados come frighteningly close, and successfully disarmed and duly punished a nasty Indian boy who rushed at me swinging a big knife. Great fun being young and observant, preparing for the life of a naturalist. [8]

In the fall of 1908 Hubbs's mother returned to California. She settled in Redondo Beach, and with an associate ran a

[7] "Raja Yoga—Glass Domes Astride Point Loma," talk given to Scholia, May 12. 1970.
[8] "Preservation of Species and Habitats."

private school that enrolled her two sons. Earlier in San Diego young Carl had become much interested in seashells through the guidance of his maternal grandmother, Jane Goble Goss, one of the first women physicians. She made him a "proud partner in her moderately large private shell collection," which was, he said, "one of the greatest thrills of my boyhood. I continued the collection after her death, and at late high-school age spent long hours in the Los Angeles Public Library reading books on conchology to produce an illustrated phylogeny of molluscs—happily lost."[9]

At Redondo Beach, said Hubbs, his school training was

> good and intensive, leaving time for me to add to my shell collection; to fish off the old Redondo wharf when yellowtail were very plentiful and sardines seemed almost to fill the waters; to observe marine life in the tide pools at Rocky Point (I recall most vividly seeing the brilliant red and turquoise-blue young of the garibaldi); to wander over the then uninhabited Palos Verdes, where I found my first perfect arrowhead and observed Pleistocene fossils and Indian middens. [10]

Carl's mother married Frank Newton, who soon bought a twenty-acre ranch in the San Joaquin Valley, near Turlock, California. There Carl spent most of his high-school years and "plunged into nature study with a vengeance." He became an avid bird-watcher and with great pleasure once accompanied "one of California's greatest and most loved ornithologists," Loye Holmes Miller, on a field trip. His chemistry teacher, impressed with his scientific ability, urged him to attend the University of California at Berkeley to major in chemistry.

Following another family move, Hubbs graduated from high school in Los Angeles in 1912 and continued at junior college in that city. There he came to the attention of George

[9]"Some Highlights from My 61-year Career."
[10]"Preservation of Species and Habitats."

Bliss Culver, a onetime field assistant to David Starr Jordan. Culver surreptitiously transferred Hubbs's interest from birds to fishes, encouraged him to collect the poorly known fishes of the streams of the Los Angeles plain, and persuaded him to attend Stanford University, which had become the center of American ichthyology under the leadership of Jordan.

Charles Henry Gilbert, a close associate of Jordan's and the chairman of the Zoology Department, became Hubbs's true mentor. He assigned his student, as an undergraduate job, the curatorship of the large Stanford fish collection. Hubbs also spent considerable time in the field during his college years, "over the mountains, along the bay, and along the coast," he said. On one of those trips in 1916 in a remote ocean area off southern Monterey County he thrilled at the glimpse of one sea otter, then assumed extinct in the area. Later he found that Joseph Grinnell knew that a small number survived there, but he had kept the knowledge to himself so that the remnant would not be destroyed. In the summer of 1915 Hubbs accompanied John Otterbein Snyder of Stanford in a survey of the fishes of the Bonneville Basin in Utah, and thus commenced a lifelong study of relict desert fishes.

Hubbs received an A.B. from Stanford in 1916 and began a semester of graduate work. Gilbert spoke highly of him: "My assistant Hubbs is going to be all that one could wish for. He has the proper attitude towards the work and is endlessly keen."[11]

The peripatetic president of Stanford, David Starr Jordan, had returned to the campus after a long absence, and during that semester Hubbs collaborated with him. A few years later Jordan described Hubbs as "the ablest student I have had for the last thirty years . . . . There is no one now

[11] Letter from Gilbert to John Babcock, October 10, 1916.

doing systematic work on fishes that has as keen an insight, or as accurate a mind, as Hubbs, and he is tremendously industrious."[12]

Hubbs retained a lifelong awe of the monumental man who long dominated American ichthyology, and in the late years of his own productive life sometimes voiced his regret that he would never be able to equal the written output of the prolific Jordan.

Early in 1917 Hubbs accepted the position of assistant curator in charge of fishes, amphibians, and reptiles at the Field Museum of Natural History in Chicago. He was awarded an M.A. from Stanford that June, in absentia. The following year he married Laura Clark, a fellow student who had received her B.A. in 1915 and M.A. in 1916 at Stanford, where she was teaching freshman mathematics.

Of his Chicago years Hubbs said: "After three busy years of service there, 1917-1920, involving also research and a bit of graduate work at the University of Chicago, I was abruptly fired for blatant insubordination. My indiscretion I must admit resulted in part from having been lined up for an appointment at the University of Michigan."[13]

That appointment, from 1920 to 1944, became a highly productive one for Hubbs. He was curator of the fish division in the University of Michigan Museum of Zoology, he advanced from instructor to assistant, associate, and full professor, he was awarded a Ph.D., he instituted research projects, and he published prodigiously.

In a program of upgrading the caliber of its faculty, the Zoology Department of the University of Michigan suggested to Hubbs that he should obtain a Ph.D. According to his later recounting, he pointed to his shelves of publications and sug

[12] Letter from Jordan to Roy Chapman Andrews, February 19. 1924. when Hubbs was working with Jordan on a collection of fishes from Japan.

[13] "Preservation of Species and Habitats."

gested that any of several of them would constitute an appropriate dissertation. Thus, his paper of 1926 was selected: "The Structural Consequences of Modifications of the Developmental Rate in Fishes Considered in Reference to Certain Problems of Evolution." He was awarded the Ph.D. in 1927, at a ceremony that he was too busy to attend.

Hubbs increased the fish collection of the Museum of Zoology through his own field work, from collections made by staff and students of the university, and by simple begging. With his family he collected in the intermontane basins of the American West during eight summers from 1922 to 1943. From a long excursion in the Orient in 1929, following his participation in the Fourth Pacific Science Congress in Java, Hubbs shipped back to the museum five tons of specimens. During 1935 he collected in remote areas of Guatemala, as one of a series of expeditions sponsored jointly by the Carnegie Institution of Washington and the University of Michigan.

Hubbs readily agreed to identify collections sent to him by other institutions, and as a result the museum was given many specimens. Collectors routinely sent him additional material; for example, his wife's sister Frances N. Clark, who served many years with the California State Fisheries Laboratory, provided him with many West Coast fishes. Robert Rush Miller has estimated that during Hubbs's tenure at the University of Michigan Museum of Zoology, the collection of fishes was increased from about five thousand to nearly two million specimens. [14] The emphasis was on freshwater fishes, especially those of North and Central America.

Laura Hubbs, in addition to raising three children, also worked in the Museum of Zoology as a cataloger. Together the Hubbses undertook a study of hybridization in various

[14] "A Tribute to Carl L. Hubbs," presented at annual meeting of American Society of Ichthyologists and Herpetologists, July 30, 1979.

fishes—in nature and in the laboratory. In the course of this work they discovered the matroclinous, gynogenetic reproduction of the all-female fish species *Poecilia* (formerly *Mollienisia*) *formosa,* the "Amazon molly." In earlier researches they had also developed—through carefully annotated genetic crosses—hybrid specimens of sunfishes (Centrarchidae) that were similar to so-called species in nature, and thus Hubbs was able to untangle taxonomic confusion in that family. Detailed analysis of natural hybrids led him to conclude that interspecific hybridization was especially frequent in freshwater regions that had been disturbed by Holocene climatic changes.

In 1930 the Institute for Fisheries Research was established to formalize the cooperation between the University of Michigan and the Michigan Department of Conservation. Hubbs was instrumental in setting up the Institute and served as its director for the first five years. Its programs included making biological inventories of lakes and streams, mapping lakes, investigating fish mortalities and water pollution, studying the age and growth of fishes and predation on them, and developing methods of improving lake and stream habitats. This led Hubbs into testing some techniques that he later questioned, such as introducing mosquitofish (*Gambusia*) for mosquito control and using poisons broadly to eliminate "trash fish."

In June of 1939 Hubbs was asked to serve as a field representative of the Department of the Interior to look into the administration of fish and wildlife in Alaska. After a brief interview with the irascible Secretary Harold Ickes in Washington, Hubbs spent the summer traveling throughout the territory, interviewing fishermen and game managers. He uncovered irregular conduct by some officials, illegal fishing operations, controversies over regulations, Japanese monopoly of the king-crab fishery, pollution from canneries, and peculiarities in the bounty on Dolly Varden as predators on trout. As a result of his report, several officials were fired, the bounty on bald eagles was discontinued, and an American fishery for king crabs was subsidized.[15]

Hubbs's publications while at the University of Michigan—in excess of 300—were almost entirely on fishes from throughout the world. In his 1922 paper, "Variation in the Number of Vertebrae and Other Meristic Characters of Fishes Correlated with the Temperature of Water During Development," he proposed an explanation for the effect of temperature that has been modified but not yet superseded. He devoted time in 1923 to helping David Starr Jordan analyze the largest collection of fishes from Japan ever made (according to Jordan), and their memoir was published in 1925. With Karl F. Lagler, Hubbs compiled a "Guide to the Fishes of the Great Lakes and Tributary Waters," first published in 1941 and revised several times.

While many of his papers were taxonomic, others summarized his studies of variation and its causes. Primary publications concerned groups that continued to interest him later, such as the lampreys, the catastomid fishes, and the subfamily Oligocottinae. A major series of papers was on the systematics, distribution, and habits of fishes of the order Cyprinodontes. Long-term studies on the fishes of isolated Great Basin waters culminated in the 1948 publication, "Correlation between Fish Distribution and Hydrographic History in the Desert Basins of Western United States," coauthored with Robert Rush Miller. Hubbs's interest in this subject never waned, and in 1974 with colleagues he published the monograph, "Hydrographic History and Relict

[15] "Investigations in Alaska in 1939 as Field Representative, Department of the Interior: An Historical Review of Natural Resource Problems In Alaska," talk given by Hubbs at University of Alaska, April 8, 1976.

Fishes of the North-Central Great Basin" (Hubbs, Miller, and Hubbs).

During his years at Michigan, Hubbs also began his service and devotion to the American Society of Ichthyologists and Herpetologists. As editor of its journal *Copeia* from 1930 to 1937, he increased publication to a quarterly cycle; as secretary in 1929 and 1930, he increased the society's membership considerably. He served as president of the society in 1934 and in 1946, and was reelected in 1947. Chiefly through his urging, the society became increasingly active in conservation of fishes, amphibians, and reptiles, beginning with the fish fauna of isolated desert springs. Hubbs became the first chairman of the society's Committee on Nomenclature. He also established a regional committee on nomenclature through the auspices of the University of Michigan Museum of Zoology to advise on local problems that could then be referred to the International Commission on Zoological Nomenclature. This led to considerable correspondence with other taxonomists and to his later participation in other scientific societies such as the Society of Systematic Zoology.

During the summer of 1943, at the invitation of Director Harald U. Sverdrup, Hubbs visited Scripps Institution of Oceanography. There he gave seminars and wrote two short papers with aquarium curator Percy S. Barnhart. The suggestion of the visit had come from Francis B. Sumner, whose retirement was imminent; Sumner was a geneticist who was then studying the causes of color changes in fishes. In September Sverdrup asked Hubbs if he would consider an offer from Scripps. "Your field of research would fit into our program admirably," wrote Sverdrup, "and your coming here would strengthen the general position of the Institution."[16]

[16] Letter from Sverdrup to Hubbs, September 1, 1943.

Hubbs declined. The staff at the University of Michigan museum had been severely depleted by wartime absences, and he was much needed there, but he made it clear that he was interested in moving to the West Coast at some later time.[17]

In May of 1944 Hubbs reopened the correspondence with Sverdrup, with the comment that "of several possible openings on the Coast I believe that one at Scripps might prove most alluring."[18] He was looking for a good base for research, what he called "a set-up that will make for real accomplishment."[19] Many of his prewar students were in military service, as were his two sons, and his daughter was married, so the time seemed suitable for a move.

Sverdrup held out a tentative offer (contingent on approval by the administration of the University of California), and Hubbs accepted, even though the salary at Scripps would be slightly less than he was making at the University of Michigan.[20] He also knew that his wife could not be employed at Scripps ("I recognize that many institutions have a rule or policy against employment of two members of one family.") He added: "I am more interested in the opportunities for research work than in the salary." This was his lifelong philosophy. He continued:

> We have had relatively little discussion regarding fields of research in which I might be engaged at Scripps. I would no doubt want to put considerable emphasis on systematic and variational studies of west coast marine fishes, particularly those in which speciation would be correlated with oceanographical conditions. I have two or three rather major pieces of work along such lines nearly completed, and I have set these aside with the idea that these jobs would be among the first to be completed if we go to the coast. I would no doubt be interested in exploratory work, for in

[17] Letter from Hubbs to Sverdrup, September 11, 1943.
[18] Letter from Hubbs to Sverdrup, May 2, 1944.
[19] Letter from Hubbs to Paul Needham, May 15, 1944.
[20] Letter from Hubbs to Sverdrup, May 19, 1944.

> stance with the fauna of the deep basins off the southern California coast. I will probably be interested too in detailed analyses of the distribution of fishes along the entire west coast, again as correlated with the oceanographic conditions.[21]

Hubbs had another California project in mind as well, as he wrote to W. I. Follett in Oakland, with whom he had been corresponding for a decade: "I look forward particularly to cooperating with you in making better known the California fish fauna. I no doubt will have new material published from time to time on the systematics and biology of the fishes but will definitely hope that you will maintain your plan to work toward a 'Fishes of California.' It will be a pleasure to make records and other information available for your project."[22]

Notice of Hubbs's appointment as professor at Scripps Institution came on September 1, 1944, with an announcement by University President Robert G. Sproul that Hubbs "is an exceptionally prolific writer. . . . His fertility in producing sound ideas is as amazing as is the energy he brings to his work."[23]

Among the goods that the Hubbses sent to their new location was his personal library of ichthyological and natural history items. He had begun accumulating a library while a student at Stanford, and he increased it actively through exchanges of reprints, membership in many scientific societies, and purchases. He had, for example, bought significant works on fishes from the library of Carl H. Eigenmann (some purchased by Eigenmann from British ichthyologist Albert C. L. G. Günther in 1910). Hubbs also received a number of books as review editor for *American Naturalist* from 1941 to 1947. During Hubbs's long trip to the Orient in 1929, his wife had set up the cataloging of his library, which was then

21 *Ibid.*
22 Letter from Hubbs to Follett, June 1, 1944.
23 University of California *Clip Sheet,* September 5, 1944.

continuously kept up-to-date as items were added. Hubbs estimated in 1944 that his library contained 40,000 books and reprints, as well as a collection of journals.[24] (In comparison, the Library of Scripps Institution of Oceanography in 1941 held 18,000 volumes, of which about 12,000 were bound periodicals, and 30,000 reprints.)

Hubbs arrived at Scripps in mid-October of 1944, near his fiftieth birthday, after collecting fishes in the Great Basin en route. Like other educational institutions, Scripps was in a slow period in 1944; wartime researches and military service decimated the staff and student enrollment. There were essentially seven senior academic staff members in residence, five students, and several research visitors. The institution's ship, *E. W Scripps,* was on loan to the Navy until 1947. Repairs and renovations to the buildings were almost impossible to arrange.

The Hubbses accepted the circumstances and turned immediately to marine researches. Although she was not on the payroll, Laura Hubbs continued to work alongside her husband, both at the office and in the field. In a seminar six months after his arrival, Hubbs commented that their collecting had been curtailed by gasoline rationing, so that "we have taken only 107 species to date." These included, however, several rare and a few new species of fishes. He annotated extensions of ranges, observed territorial defense in blennies in the aquarium tanks, began an analysis of the fish fauna of the kelp beds through specimens from the kelp harvesters and from lobster traps, and noted ecological effects on speciation. "We have managed," he commented, "to dip into the edge of the marine grab bag."[25]

One of Hubbs's concerns before reaching Scripps had

[24] Letter from Hubbs to Sverdrup, May 19, 1944.

[25] "Ichthyological Discoveries Since Coming to Scripps," seminar at Scripps Institution, March 30, 1945.

been where he would be able to store large series of specimens while he was studying them. He felt that Scripps Institution or Berkeley would not have such a facility and asked Sverdrup if he should arrange for storage at Stanford, the California Academy of Sciences, the University of Michigan, or even the National Museum.[26] Sverdrup replied that a postwar building program should make the needed facility available, [27] essentially assuring the establishment of what has become one of the country's largest collections of fishes. Hubbs devised the system for cataloging the collection and supervised it through many years of exponential growth.

His first opportunity to collect beyond the southern California coast came, oddly enough, from a movie star: Errol Flynn, the son of a marine biologist. In the summer of 1946, Flynn offered to let a scientist from Scripps Institution accompany him on his yacht *Zaca* from San Diego to Acapulco, Mexico. Hubbs leaped at the chance. It was scarcely a scientific expedition, but Hubbs did succeed in making extensive fish collections at several islands off the west coast of Mexico and in Acapulco Bay, areas that were then scantily represented in collections. He commented on what he thought was insular endemism in the fishes of Guadalupe Island, but later collecting established broader ranges for those species. Guadalupe Island, with its populations of marine mammals and its distinctive plants, became a favorite destination for Hubbs over many years.

In the spring of 1947, a multi-institutional program was created to try to determine why the catch of sardines off California had dropped dramatically. State and federal fisheries people joined forces with researchers from Scripps Institution and the California Academy of Sciences to attack the problem, with generous support from the state and com

[26] Letter from Hubbs to Sverdrup, August 17, 1944.
[27] Letters from Sverdrup to Hubbs, August 21, 1944.

mercial fishermen. Hubbs was active in establishing the format of the California Cooperative Oceanic Fisheries Investigation, and he derived support from it for fisheries researches for many years. The Isaacs-Kidd Midwater Trawl, which opened an almost untouched collecting region to biologists, was one of the program's earliest equipment developments (1950).

Simultaneously, from 1948 to the mid-1950s, Hubbs supervised researches by Conrad Limbaugh, financed by Kelco Company, to determine whether the cutting of kelp interfered with sportfishing. The project became an intensive study of the ecology of the kelp beds of southern California, with an inventory of their plant and animal life and interrelations. It was concluded that kelp harvesting had no detectable detrimental effect on fishing.

As noted earlier, Hubbs continued his studies of the isolated relict fish populations of the western states, chiefly with his son-in-law Robert Rush Miller of the University of Michigan. His concern over the extinction of some species of western freshwater fishes led Hubbs into major conservation efforts on their behalf. One of his earliest concerns was with the Devils Hole pupfish (*Cyprinodon diabolis*) He suggested the common name pupfish for the genus, for the seemingly playful behavior of these small fishes. In the 1940s he and Miller proposed that Devils Hole, the small spring that held the sole population of *C. diabolis,* be made a separate part of Death Valley National Monument. This was finally done in 1952 in a proclamation by President Harry S Truman. Through the years Hubbs monitored the spring regularly and participated in efforts to keep its water level adequate for the threatened pupfish. Others joined the conservation efforts for endangered western fishes, and in 1969 they formed the Desert Fishes Council, which Hubbs participated in each year with keen enthusiasm.

Hubbs's observations of anomalies in the distribution of fishes and other marine organisms along the Pacific Coast led him to try to determine the causes. In 1949 he published a landmark paper, "Changes in the Fish Fauna of Western North America Correlated with Changes in Ocean Temperatures." This much-cited and influential work documents changes in north-south fish distribution correlated with water temperature changes in historic time.

Hubbs enlarged his researches into archeology and an analysis of past climate. Many years of daily coastal temperature records were available from Scripps Institution and other California locations, so in 1948 Hubbs set out to extend the series southward into Baja California, Mexico. The roads there were dusty or sandy ruts, and in the rainy season muddy morasses, but he acquired a four-wheel drive vehicle and, accompanied always by Laura and sometimes by students or hardy guests, he jolted down the peninsula. For fourteen years they took temperature records once a month (with a few exceptions) at a series of sixty-one coastal stations extending 225 miles southward. "At first we waded out into fairly clear water [said Hubbs], and then, after some very cold-water duckings, and ankles painfully struck by rolling cobbles, we used a casting thermometer constructed in our instrument shop."[28]

The temperature runs, as Hubbs called them, quickly showed dramatic differences over short distances, in one case 12°C in two miles on either side of a projecting point (Punta Banda). He delimited alternating cold and warm areas along the Baja California coast; in the cold spots the fishes, invertebrates, and algae included species typical of the cold central California coast not found in the intervening warmer waters.

[28] "Biological Oceanography, Geochronology, and Archeology Along the Pacific Coast of Middle America and California," talk at University of Nevada, Las Vegas, April 17, 1975.

In another effort at establishing temperature data, Hubbs arranged in 1949 for approximately thirty volunteer yachtsmen to take lines of measurements of surface temperatures from their craft simultaneously on a single day, while he supervised from above in a Coast Guard airplane.

The trips in Baja California also disclosed considerable evidence of earlier human habitation there and drew Hubbs into collecting shell debris from aboriginal middens for its bearing on past climate. In 1951 he noted that "by supplying critical material from the ascertained cool and warm areas and by running controlled temperature experiments, we have been able to assist [Harold C.] Urey in checking his method of estimating past ocean temperatures through analyses of the oxygen-isotope ratios in mollusk shells."[29] These analyses led Hubbs to conclude that, in the southern California region, ocean temperatures since the end of the Wisconsinan Period (11,000 years ago) were generally warmer than at present, except from 2,500 to 600 years ago, when the ocean was colder. Throughout the period from 11,000 years ago rainfall was higher than at present—until about 400 years ago.[30]

In 1957 the La Jolla Radiocarbon Laboratory was established at Scripps Institution under Hans E. Suess, which made possible the determination of a large number of dates of archeologic and geologic significance. Throughout the 1960s Hubbs coordinated the submission of samples to that laboratory, and he compiled its first five reports. Samples he submitted were chiefly from shell middens dating from the modern to 7,500 years before present, from tufa and shell

[29] "Research in the Biological Sciences," talk at conference on The Place of Scripps Institution in the University, the State, and the Nation, at Scripps Institution, March 26, 1951.

[30] Carl L. Hubbs and Gunnar 1. Roden, "Oceanography and Marine Life along the Pacific Coast of Middle America." vol. 1. *Handbook of Middle American Indians* (Austin: University of Texas Press, 1964), pp. 169-70.

remnants along former shorelines of ancient Lake LeConte (Imperial Valley, California) and from offshore islands.

In 1973 Hubbs donated his large collection of archeological samples from Baja California and southern California to the Museum of Man in San Diego, where they have been cataloged and indexed for continuing use.

Hubbs's interest in marine mammals began during his first winter at Scripps Institution in 1944:

> At that time, no one, either among biologists or the general public, gave any serious thought to the gray whale, and the general assumption was that the species, if not extinct, had at least very largely abandoned its runs along the California coast to and from its traditional breeding grounds in the lagoons of Baja California in Mexico . . . . My first inkling that the parade of the gray whale along the coast of southern California had not totally ceased came in 1945 and 1946, when Henry Kritzler, a visiting postdoctoral fellow in Scripps Institution of Oceanography, reported to me his sighting of a few individuals about the kelp beds of Pt. Loma—incidentally observed as he was collecting fulmars . . . . This exciting news led me to establish a gray-whale monitoring project atop the roof of Ritter Hall of Scripps Institution, close to the shore. Here we installed in a small rooftop enclosure . . . an 18.5-power binocular instrument that I had secured from a soldier who had taken it on Iwo Jima. Willing associates and drafted graduate students took turns with me on 15-minute watches per hour throughout daylight, to count the whales going by, plot their positions and speed, and to note down their behavior.[31]

In 1947 Hubbs obtained permission from the commanding general of the Coast Guard in Washington to accompany mercy flights off Mexico. The first of those flights took him low over Scammon and San Ignacio lagoons in Baja California, where he had his first view of the calving locale. "This gave me an uncontrollable desire to go down to observe the whale life in one of the lagoons," he said. So he turned to

[31] "Initial Studies 1945-66 and Conservation Efforts 1956-73," talk to University of California, Berkeley Extension Special Program on "Life of the California Gray Whale," November 8, 1973.

Errol Flynn again, on the premise that very exciting movie shorts could be made at the lagoons. Flynn heartily agreed, and in February, 1948, arranged for a flight by helicopters and a small plane. Some of the footage on gray whales was incorporated into the movie short, "Cruise of the Zaca," released by Warner Brothers in 1952.

After a few years Hubbs relinquished the shore count of gray whales to Raymond M. Gilmore of the U.S. Fish & Wildlife Service. But in 1952 Hubbs took the opportunity to tally the gray whales in the calving lagoons by airplane, piloted by Scripps Institution physical oceanographer Gifford C. Ewing, "a superb pilot who flew his own planes and knew Baja California as few others ever have."[32] The annual aerial counts continued from 1952 through 1964, variously by the Hubbses and by Gilmore.

On his first visit to Guadalupe Island in 1946, Hubbs observed northern elephant seals (*Mirounga angustirostris*) and over the years he tallied the rise of the population there to at least 15,000 animals. With George A. Bartholomew he recorded the reestablishment of this once rare mammal on other islands off the west coast of Baja California and California.

The Guadalupe fur seal (*Arctocephalus townsendi*) was presumed extinct from about 1928 until Bartholomew found an old male on San Nicolas Island off southern California in 1949. He and Hubbs searched for more on Guadalupe Island without success in 1950, but in 1954 Hubbs finally did locate a group of fourteen on that island in the mouth of a remote cave. For some years he tallied the rise in population of that rare animal also.

Hubbs published twenty-eight papers on marine mammals and participated often in conferences pertaining to

[32] *Ibid.*

their habits, with emphasis on their conservation. As early as 1956 he and Gifford C. Ewing urged Mexican officials to establish a sanctuary for gray whales in lagoons of Baja California. The plan was finally implemented, with Hubbs's participation, in 1972. He also helped persuade Mexican officials to protect the elephant seal and fur seal on Guadalupe Island. Norris noted: "Few Americans have proved as adept as he in garnering the trust of foreign government officials and scientists necessary to achieve such international results."[33] Hubbs was an optimistic and diplomatic conservationist. He observed and commented sagely on national and international efforts to preserve whales, porpoises, sea otters, and other marine mammals. And for many years he prodded and measured every cetacean that was found ashore in the vicinity of San Diego, as he, like others, tried to resolve why these animals become stranded.

Beyond the time that Carl Hubbs devoted to his scientific researches, he accepted and carried out a great many outside commitments. At Scripps Institution he was in charge of the Division of Marine Vertebrates for many years until reorganization drew several units into a larger Division of Marine Biology. He devoted considerable time to the selection of new academics in biology when the institution acquired a major contribution from the Rockefeller Foundation in the 1950s. Always he was a conscientious committee member.

He served for several years on the International Commission on Zoological Nomenclature, patiently working out fine points of usage and priority. He reviewed books frequently, and he was often called upon to review scientific proposals. In later years he found himself obliged to write obituaries frequently. He wrote with facility, in longhand, and without

[33] Kenneth S. Norris, "To Carl Leavitt Hubbs, A Modern Pioneer Naturalist on the Occasion of his Eightieth Year," *Copeia,* 3(1974):581-94.

extensive rewriting, except for his longer technical papers; these he often revised through several editions (and multiple carbons). Most of his letters, sometimes very long ones, were dictated, without losing the thread of his discourse.

Immediately upon his arrival in California in 1944, Hubbs by invitation became a member and fellow of the San Diego Society of Natural History, that city's oldest scientific organization. In the following year he became a member of the board of directors, on which he served for thirty-four years. His primary concerns for the society were its scientific publications and its research program, both of which he subsidized as well as advised. Members of the museum staff often accompanied him on temperature runs and trips to Baja California islands.

By 1948 he also was serving on the Research Committee of the Zoological Society of San Diego, which operates the San Diego Zoo and Wild Animal Park. From 1952 to 1979 he was on the society's board of trustees, and he served on several committees, always urging research and conservation of endangered species.

In 1963 Hubbs was drawn into a new San Diego organization aimed at marine exhibits, which opened as Sea World in 1964. As a member of the executive board he emphasized the great value of and need for research in marine mammals. Quite to his surprise, this culminated in the establishment of the Hubbs-Sea World Research Institute, dedicated in 1977 to both Carl and Laura Hubbs.

In a warmly perceptive account, on the occasion of Carl L. Hubbs's eightieth birthday, his former student Kenneth S. Norris presented many facets of the personality of this "modern pioneer naturalist." [34] Norris credited Hubbs with tremendous energy, enthusiasm, and breadth, with thorough

[34] *Ibid.*

ness in his researches, complete dedication to science, an insatiable appetite for collecting, endless helpfulness to students and colleagues, and forthrightness. Hubbs was a selfassured man, confident in his abilities, yet always surprised at having his scientific accomplishments honored. On such an occasion in 1975 he commented, "I really don't know why I'm receiving this [award as Headliner of the Year]. All I've ever done in life was exactly what I wanted to do."[35]

What he wanted to do was to find out about nature: to observe, annotate, define organisms in their environment. The enormity of the task energized him, so that one admirer could say, "His visits to scientific institutions have left behind him many an exhausted colleague."[36]

Students and colleagues were awed by Hubbs's painstaking attention to detail and his command of ichthyology. The man himself was not awesome or pretentious. About five feet ten inches tall, and of stocky build, distinguished by straight, black, crew-cut hair that never grayed, he had a keen gaze with a slight twinkle in his eye. Even when engrossed in writing a manuscript or counting and measuring specimens, he accepted interruptions with good grace and turned to the new subject without pause. Amateur naturalists, commercial fishermen, and free-lance writers were received as cordially as colleagues.

His work was also his hobby. For many years he routinely was at his office on Saturdays, and often on Sundays. When a major paper was in process, he and Laura returned to the office in the evenings. Otherwise, he carried home each day a briefcase of unfinished items, and devoted his evenings to them. He also enjoyed many social commitments, especially through his participation in civic and scientific organizations. At his home—in Ann Arbor, on the Scripps campus, and

[35] *San Diego Union,* January 27, 1975.
[36] Citation on Fellows Award of California Academy of Sciences, 1966.

from 1954 on the bluff a mile north of Scripps Institution—the welcome mat was always out for visiting colleagues.

The recognition for his accomplishments in science went chiefly to Carl, but a great deal of credit should be given also to the wife who worked alongside him, maintained his files, kept track of infinite details, traveled with him, absorbed his outbursts of impatience, and welcomed their guests. Together they raised three children: Frances, wife of Robert Rush Miller; Clark, professor of biology at the University of Texas at Austin; and Earl; high school teacher of biology in Orange County, California. The legacy continues in the grandchildren, several of whom are scientists.

Hubbs was a prolific teacher of graduate students. Twenty-eight students received doctorates under his direction at Michigan, and an additional eighteen after his move to Scripps. The dissertation titles reflect Hubbs's broad interests, so that, in addition to studies on fishes, some were on crayfish, porpoises, and Amerindians. Hubbs did little formal teaching, and his students learned most by accompanying him in the field and standing at his shoulder as he worked at the microscope. Perhaps the most lasting lesson was learned as Hubbs examined every sentence and datum in his students' theses (and colleagues' manuscripts), both for logic and grammar. He retained a keen interest in his students' careers, and he took great satisfaction in their students, whom he enjoyed referring to as his ichthyological grandchildren.

Carl Leavitt Hubbs died on June 30, 1979, after a steadily disabling cancer of the kidneys. It had slowed him physically—but hardly mentally. Three weeks before his death he looked over with pride the first printed copy of "List of the Fishes of California," by Carl L. Hubbs, W. I. Follett, and Lillian J. Dempster, the project that he had promised to help Follett with in 1944. This one was done, and he was pleased—

although he really wanted it to be a much more annotated publication. It was difficult for him to let a project go; there were always unfinished loose ends that would make it better. But he did publish, very extensively, even when he knew that the last word on the subject could not yet be written. His collected works totaled 712 titles.

To Scripps Institution of Oceanography Hubbs willed his library and his personal papers. The library of 80,000 reprints and books and 125 linear feet of personal papers together constitute Hubbs Collection, housed in the Archives of the Scripps Institution of Oceanography, where they continue to be available to researchers.

NOTE: John D. Isaacs participated in the preparation of this account before his death in 1980. All manuscript material and correspondence cited here are from: *Carl Leavitt Hubbs, 1894-1979: Papers, 1915-1979,* 81-8. In the Archives of the Scripps Institution of Oceanography, University of California, San Diego, La Jolla, California 92093.

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Paul F. Lazarsfeld

February 13, 1901-August 30, 1976

By David L. Sills

Paul Felix Lazarsfeld was born and raised in Vienna. In 1933 he came to the United States as a Rockefeller Foundation fellow. He remained in America at the end of his fellowship, became a citizen, and for three decades was a professor of sociology at Columbia University. He died of cancer in New York City.

Although he was trained in mathematics, Lazarsfeld thought of himself as a psychologist; only in midlife did he identify himself as a sociologist. His major interests were the methodology of social research and the development of institutes for training and research in the social sciences. Because of the originality and diversity of his ideas, his energy and personal magnetism, his unique style of collaboration with colleagues and students, and the productivity of the research institutes he established, his influence upon sociology and social research—both in the United States and in Europe—has been profound.

In the years since Lazarsfeld's death, a substantial number of appraisals of his life and work have been published.[1] I shall attempt to summarize his rich intellectual legacy in this article. But first, let me convey some sense of Lazarsfeld the person by quoting from the writings of his former students Allen H. Barton and David L. Sills, both sociologists, and his son-in-law, the historian Bernard Bailyn. As these witnesses attest, neither Lazarsfeld nor his associates were able to distinguish very clearly between the man and the scholar.

Allen Barton's attempt to capture the essence of Lazarsfeld's personality is to be found in his. dramatic and rather subjective account of the history of one of Lazarsfeld's major inventions: the university-based social research institute. Barton notes that the concept of the university-based social research institute "was born in the mind of a social activist student in the intellectual hothouse of Vienna between the wars," who "created a penniless research center in a nearbankrupt society, and found his friends jobs studying unemployment." He calls Lazarsfeld "an intellectual Odysseus" and "an entrepreneur of intellectual conglomerates," who "brought new meaning to the words 'non-profit' as he used one deficit-ridden project to support another, and pyramided his intellectual assets from grant to grant." In the end, Barton notes, "the Bureau was demolished and hauled away

[1] Appraisals up to 1979 are listed in David L. Sills, "Publications About Paul F. Lazarsfeld: A Selected Bibliography," in *Qualitative and Quantitative Social Research: Papers in Honor of Paul F. Lazarsfeld,* ed. Robert K. Merton, James S. Coleman, and Peter H. Rossi (New York: Free Press, 1979), pp. 389-93. Subsequent publications include: Allen H. Barton, "Paul Lazarsfeld and the Invention of the University Institute for Applied Social Research," in *Organizing for Social Research,* ed. Burkart Holzner and Jiri Nehnevajsa (Cambridge, Mass.: Schenkman, 1982); James S. Coleman, "Paul F. Lazarseld: The Substance and Style of His Work," in *Sociological Traditions from Generation to Generation to Generation: Glilmpses of the American Experience,* ed. Robert K. Merton and Matilda White Riley (Norwood, N.J.: Ablex, 1980); James S. Coleman, "Introduction," in Paul F. Lazarsfeld, *The Varied Sociology of Paul F. Lazarsfeld,* writings collected and edited by Patricia L. Kendall (New York: Columbia University Press, 1982): Paul Neurath, "Paul F. Lazarsfeld and the Institutionalization of Empirical Social Research," in Robert B. Smith, *An Introduction to Social Research* (Cambridge, Mass.: Ballinger, 1983); Paul Neurath, "In Memoriam Paul F. Lazarsfeld: Paul F. Lazarsfeld and the Institutionalization of Empirical Social Research," in *Realizing Social Science Knowledge,* ed. B. Holzner, K. D. Knorr, and H. Strasser (Vienna: Physica-Verlag, 1983); and David L. Sills, "Surrogates, Institutes, and the Search for Convergences: The Research Style of Paul F. Lazarsfeld," *Contemporary Sociology,* 10 (May 1981):351-61.

to make room for a parking lot on 115th Street, while a Center for the Social Sciences rose on 118th Street, proclaiming a set of purposes almost identical to Lazarsfeld's recipe for his research institutes in Vienna, Newark, Princeton, and Columbia."[2]

Bernard Bailyn had the good fortune to have been both colleague and son-in-law; here is his recollection of a family visit by his father-in-law:

> A visit by Paul was like some wonderfully benign hurricane. There would be premonitory squalls for days in advance. Special delivery letters would begin to arrive long before he got there; telegrams and messages would pile up, occasionally an embarrassed assistant would appear on the doorstep having got the wrong day relayed through secretaries in two universities. The day before he was due there would be a flurry of frantic, often hilarious telephone calls rescheduling the flight, but then finally he would arrive. The cab would pull up in the driveway and Paul would struggle from the door clutching a briefcase overflowing with manuscripts, books, pipes, cigars, shirts, and some miscellaneous shoes. He would half run to the house in his odd, stiff-kneed, sideways-swinging walk; call gaily to his daughter; shake hands with the male members of the family with a slight European bow, heels together; and almost invariably, as soon as he was inside the door, say "The most *amazing* thing happened!," and out would come an extraordinary episode, told with barely suppressed laughter and high suspense—some bizarre coincidence—and the visit would be properly launched.[3]

In a summary sketch of Lazarsfeld's personality, David Sills singled out Lazarsfeld's quite remarkable capacity to carry out his intellectual activities with, and through, other people:

> Most of his major writings are coauthored, and much of his work day consisted of listening to, talking to, and instructing his students, colleagues, and co-workers: in class, in his office, in taxicabs, in his apartment, in a

[2] Barton, "Paul Lazarsfeld and the Invention of the University Institute." pp. 1718.

[3] Bernard Bailyn, "Recollections of PFL," in Merton, Coleman, and Rossi, *Qualitative and Quantitative Social Research,* p. 16.

succession of summer houses in New Hampshire; at breakfast, at lunch, and at dinner; at the blackboard, or pacing his office with a cigar, or seated in the faculty club with a double Manhattan cocktail in hand, Lazarsfeld seldom was or worked alone, and he was always working. What Allen H. Barton termed "the hectic Lazarsfeldian life style" went on to midnight or later; only then did he work for hours alone. [4]

The Vienna Years

Lazarsfeld came from a professional family, active in the musical, cultural, and political life of turn-of-the-century Vienna. His father, Robert, was a lawyer in private practice, rather unsuccessful financially, who often defended young political activists without fee. His mother, Sofie, had been trained in individual psychology by Alfred Adler. Lazarsfeld had three successive marriages: to Marie Jahoda, Herta Herzog, and Patricia L. Kendall—all his students, all his coworkers, and all accomplished social scientists. His daughter Lotte Bailyn is a social psychologist, his son Robert a mathematician.

Socialist Youth. Austrian socialism in the early decades of the twentieth century was not just another political movement, particularly for the Lazarsfeld family and friends. Long after the Vienna years, Lazarsfeld's boyhood friend Hans Zeisel recalled that time and place and noted that "for a brief moment in history, the humanist ideals of democratic socialism attained reality in the city of Vienna and gave new dignity and pride to the working class and the intellectuals who had won it." Socialism was integral to the familial, social, intellectual, and political environment of Lazarsfeld's early years.

He once said that he had become a socialist the way he had become a Viennese: by birth, and without much reflec

[4] David L. Sills, "Lazarsfeld, Paul F.," in *International Encyclopedia of the Social Sciences: Biographical Supplement,* vol. *18,* ed. David L. Sills (New York: Free Press, 1979), p. 419.

tion. But he was a socialist all right. When his mother's friend, the socialist leader Friedrich Adler, was arrested for assassinating the prime minister, Count Karl Stürghk, in August 1916, Lazarsfeld attended the trial. He was arrested for taking part in a courtroom demonstration when Adler was convicted. He was active as a leader in socialist student organizations; he created a monthly newspaper for socialist students; and he helped found a political cabaret that was to play a seminal role in the development of both the political and theatrical history of Vienna. Lazarsfeld's first publication, coauthored with Ludwig Wagner and published when he was twenty-three, is a report on a children's summer camp they had established according to socialist principles.

Although Lazarsfeld often stressed the importance of his early immersion in the socialist movement, his political activism did not survive his move to the United States. In later life he used to say that he was still a socialist "in my heart," and once he remarked that his intense interest in the organization of social research is "a kind of sublimation of my frustrated political instincts—as I can't run for office, I run institutes." His American students and colleagues found him to be essentially apolitical. Particularly because he studied voting behavior, he felt strongly that politics and scholarship should be kept apart.

The Wirtschaftpsychologische Forschungsstelle. Lazarsfeld received his Ph.D. in applied mathematics from the University of Vienna in 1925; his dissertation was an application of Einstein's theory of gravitation to the movement of the planet Mercury. While a student, he assisted Charlotte Bühler in her studies of early childhood and youth development. In 1925 he established a research institute dedicated to the application of psychology to social and economic problems—the Wirtschaftpsychologische Forschungsstelle. Years later, he recalled that at the time he established the Forschungsstelle, he also created a formula to explain his interest in applied psychology: "a fighting revolution requires economics (Marx); a victorious revolution requires engineers (Russia); a defeated revolution calls for psychology (Vienna)."

Karl Bühler became the Forschungsstelle's first president; a board, consisting largely of university professors and business leaders, was recruited; Lazarsfeld became the research director. Scores of small research projects were carried out—chiefly for business firms, but also for trade unions and city agencies. "[The Forschungsstelle] came to life in 1925," Hans Zeisel later recalled, "and sustained itself mainly on ideas, all of them more or less Paul's, on the unabated enthusiasm of its members, and on no money worth talking about." As with most of Lazarsfeld's projects, the participants never forgot the experience. Ilse Zeisel (Hans' sister, who had been an employee of the Forschungsstelle in the 1930s) remarked at the time of Lazarsfeld's death that "in the end it is to the Forschungsstelle and to Paul that we owe our existence if not more," a comment that expresses the intense, almost familial relationship that Lazarsfeld had with many of his associates.[5]

The Forschungsstelle was the first of four university related, applied social research institutes founded by Lazarsfeld. The others were the Research Center at the University of Newark, the Office of Radio Research at Princeton University, and finally the Bureau of Applied Social Research at Columbia University.

The Marienthal Study. The Forschungsstelle's most ambitious project was a study of Marienthal, a one-industry Austrian village twenty-four kilometers southeast of Vienna where the labor force was nearly all unemployed as a result of the severe economic depression in the years after World War I. The study was directed by Marie Jahoda, Lazarsfeld, and Hans Zeisel. The methods used were both imaginative

[5] Useful accounts of Lazarsfeld's Vienna years are contained in the *two* Neurath articles cited in footnote 1.

and eclectic: interviewing, participant observation, life history analysis, and a variety of unobtrusive measures, such as charting the circulation of the socialist party newspaper, which declined more during the years of widespread unemployment than did the circulation of a sports and entertainment newspaper. This lack of interest was interpreted as a measure of withdrawal from participation in political affairs. The circulation of books from the workers' library was also examined: although the borrowing fee was abolished during the years 1929-31, the circulation declined by almost half—a decline that was interpreted as an indication of apathy.

The Forschungsstelle carried out a great deal of innovative consumer research, and it contributed importantly to the development of this field by making the study of consumer decisions and radio audiences academically respectable. Nevertheless, it is *Marienthal,* a slim, clearly written volume, that remains the Forschungsstelle's most memorable product (Jahoda, Lazarsfeld, and Zeisel 1933). The study has impressed generations of social scientists by its integrated use of quantitative and qualitative observations. Robert and Helen Lynd, for example, in their *Middletown in Transition* (1937), repeatedly refer to the methods and findings of *Marienthal.* It contributed substantially to the methodology of community studies, and its major finding, that the prolonged unemployment of workers leads to apathy rather than to revolution, foreshadowed the widespread lack of resistance to Hitler. *Marienthal* was banned by the Nazis soon after it was published, and most of the copies were burned, but by 1978 it had become part of the sociology curriculum in German and Austrian universities. In 1979, a group of young Europeans undertook a restudy of the village. [6]

[6] See Birgit Flos, Michael Freund, and János Marton, "Marienthal 1930-1980," *Journal für Sozialforsrhung,* 23(1):136-49; and Michael Freund, Birgit Flos, and János Marton, "The Scars of Unemployment from the 1930s Are Still Visible," Aus*tria Today,* 4 (1982):49-53.

Career in the United States

Lazarsfeld first came to the United States in September 1933 as a Rockefeller fellow; he spent the academic year 1933-34 visiting universities, New Deal agencies, and market research firms. In most places he tried to learn by attaching himself to one or more research projects. With the enthusiasm, energy, hard work, and imagination that characterized his entire career, he sent a questionnaire to the eight other European fellows in his group to learn how they had adjusted to life in the United States.

At the end of the second year of his fellowship, Lazarsfeld decided to remain in the United States. The deteriorating political situation in Austria following the defeat of the Social Democrats in the civil war of February 1934 had made his return to the University of Vienna impossible; the Forschungsstelle was in the same deficit state he had left it in two years earlier; and his marriage to Marie Jahoda—who had remained in Vienna with their daughter—had ended. So he accepted the job of analyzing some 10,000 questionnaires from young people that had been collected by the New Jersey Relief Administration. Lazarsfeld soon transformed the project into the University of Newark Research Center, and became the director.

The Center survived its first year by carrying out studies for the public school system, the Works Progress Administration, and the Frankfort Institute for Social Research—then in exile. Located on the fringes of a small university, with only a handful of staff, its abiding meaning is that it was for Lazarsfeld the American rebirth of his Vienna Forschungsstelle.

The Princeton Radio Project. In 1937 the Rockefeller Foundation granted funds to Hadley Cantril, a Princeton psychologist, for a large-scale study of the social effects of radio. On the recommendation of Robert Lynd, Lazarsfeld was cho sen to be director. Cantril and Frank Stanton, then research director, and later president of the Columbia Broadcasting System, were appointed associate directors, and a broad study of radio programming, radio audiences, and the preferences of radio listeners was begun. The emphasis was on the secondary analysis of existing survey data; the content analysis of programs; and the use of the Lazarsfeld-Stanton Program Analyzer, a jointly developed device for recording the instantaneous likes and dislikes of experimental audiences, following the prototype developed at the Forschungsstelle in Vienna.

The Lazarsfeld radio research project virtually created the field of mass communications research. It asked why messages are introduced into the media and why people attend to them; that is, what gratifications or rewards people get from the media and what functions the media serve in their lives. Herta Herzog's studies of the audiences of daytime radio soap operas and (with Hadley Cantril) of the radio listeners who believed the famous 1938 Orson Welles broadcast of an invasion from Mars are examples, as are the studies of T. W. Adorno on the social roles of popular and serious music. Other communications research projects carried out by Lazarsfeld's associates are Bernard Berelson's study of "What 'Missing the Newspaper' Means," which used the occasion of a 1945 newspaper strike in New York City to ascertain the functions that newspapers serve in the lives of their readers, and Leo Lowenthal's 1944 analysis of the biographies of culture heroes published in popular magazines. Lazarsfeld's own research (1940) on the comparative effects of radio listening and reading was the first serious examination of this important question. His influence on the field outlived him: In 1983 the directors of social research of the nation's three largest networks—CBS, ABC, and NBC—were all former students of Lazarsfeld.

The Bureau of Applied Social Research. In 1939 the Rockefeller Foundation radio research grant was renewed but transferred from Princeton to Columbia University, where Lazarsfeld was first appointed a lecturer and in 1940 an associate professor of sociology. In 1944 the Office of Radio Research was renamed the Bureau of Applied Social Research. During the 1950s and 1960s, the Bureau expanded its program and grew steadily in terms of both revenue and staff. By the mid-1970s, its annual budget was more than a million dollars, and it employed at any one time more than 100 people, half of them full time. In 1977, however, a year after Lazarsfeld's death, the university withdrew its support, the Bureau closed its doors, and its legacy and library were transferred to a new Center for the Social Sciences, located on the Columbia campus.

The Bureau's offices were temporary and makeshift throughout its life span; it never quite became the established, university-based social research institute that Lazarsfeld had first dreamed of in Vienna. But it survived for forty years, generally amidst administrative chaos, and with conspicuously little financial support from the university. The research ideas it fostered, the leading social scientists who were trained there, the innovative research it sheltered, and its distinctive organizational structure have greatly influenced the institutionalization of the social sciences throughout the world.[7]

Lazarsfeld remained at Columbia from 1940 until his retirement in 1969; in 1962, he was appointed Quetelet Professor of Social Sciences, a chair that had been created for

[7] For historical accounts of the Bureau's activities, see Barton, "Paul Lazarsfeld and the Invention of the University Institute," and Phyllis Sheridan, "The Research Bureau in a University Context: Case Study of a Marginal Institution" (doctoral dissertation, Columbia University, 1979). See also: Paul F. Lazarsfeld, "An Episode in the History of Social Research: A Memoir," *Perspectives in American History,* 2(1968):270-337.

him. Unwilling to give up teaching altogether after his retirement, he traveled almost weekly to the University of Pittsburgh, where he served as Distinguished Professor of Social Sciences from 1969 until his death.

The Interaction of Theory and Method

During his fifty-two years of active professional life, Lazarsfeld made important contributions to four substantive areas in the social sciences: the social effects of unemployment, mass communications, voting behavior, and higher education. These contributions did not spring from a grand design, but were largely the results of historical accidents and opportunities seized. By his own accounts, Lazarsfeld studied the effects of unemployment in an Austrian village in the early 1930s because the Social Democratic leader Otto Bauer had ridiculed his plan to study leisure during a severe economic depression. He studied the impact of radio in the late 1930s because he was a poor immigrant in need of a job—and Robert Lynd found this one for him. His study of the 1940 U.S. presidential election grew out of a planned evaluation of U.S. Department of Agriculture radio programs directed at farmers: Lazarsfeld simply wanted to do a panel study. All his life he was interested in university organization, but his major study of higher education came about because in the early 1950s Robert M. Hutchins, the president of the Fund for the Republic, asked him to do a study of how college and university teachers reacted to McCarthyism. Throughout his life, he insisted that serendipity was at the core of the process of scientific discovery.

Methods of Analyzing Survey Research. When Lazarsfeld studied the effects of radio in 1937, he realized that because radio listening created no public records, such as circulation data, it needed new methods of accounting and study. He took the opinion poll—at that time used mainly for descrip tive purposes, to measure such features as the popularity or audience size of radio programs—and by the multivariate analysis of responses developed ways to measure the impact of radio on attitudes. This transformation of the opinion poll into multifaceted survey research constitutes one of Lazarsfeld's major accomplishments.

Several important procedures to follow in the analysis of survey data are described in "Problems of Survey Analysis" (Kendall and Lazarsfeld 1950), a pioneering codification of techniques for avoiding spurious causal relationships in the analysis of survey data and establishing the time sequence of variables.

The Panel Method for the Study of Change. A major finding of Lazarsfeld's research on radio listening is the tendency of audiences to be self-selected; that is, to tune in to programs that are compatible with their own tastes and attitudes. Accordingly, in order to sort out the causal sequences of such problems as the effect of listening upon attitudes versus the effect of attitudes upon patterns of listening, a method of determining the time order of variables was required. Drawing on his research in Vienna with the Bühlers, in which repeated observations were made of the same children over time, as well as on the earlier research of Stuart A. Rice among Dartmouth College students and Theodore M. Newcomb among Bennington College students, Lazarsfeld developed what he called the panel method, in which a sample of respondents is reinterviewed at periodic intervals.

The panel method is a form of longitudinal research; it is essentially a field experiment in which a "natural" rather than an experimental population is studied. Although Lazarsfeld cannot be said to have invented the panel method, it was his imaginative use of it, and particularly his innovative ways of introducing control groups into the analysis of panel data, that made him its earliest and most effective exponent.

The Study of Interpersonal Influence. Lazarsfeld used the op portunity provided by his pioneering study of the 1940 U.S. presidential election to test and extend the panel method as a field technique. The study was published as *The People's Choice* (1944), a spare and elegant book that has become a true classic. The substantive findings of the study are as important as the methodology. First, a great deal was learned about the psychological and social processes that delay, inhibit, reinforce, activate, and change voting decisions. People subject to cross pressures, for example, delay making a decision longer than do others. Second, the study uncovered an influence process that Lazarsfeld called "opinion leadership." It was found that there is a flow of information from the mass media, initially to persons who serve as opinion leaders, and then to the public. This process was termed the "twostep flow of communication" (Katz and Lazarsfeld 1955).

Techniques developed by Lazarsfeld for measuring interpersonal influence, opinion leadership, and networks of influence stimulated a wide variety of studies. His students and, in turn, their students, developed variants of the method and new fields of substantive application. [8]

The Analysis of Action. Throughout his long professional life, Lazarsfeld was intrigued by the problem of how to study "action" from the point of view of the actor. Much of his research, as well as the work of his students, concerned the codification of motives and conditions underlying people's behavior—a research procedure that came to be known as "reason analysis." At the heart of the procedure is the development of what is called an "accounting scheme"—a model of the action being studied that incorporates the dimensions of the act that guide the collection of empirical data. Many of the data in an accounting scheme are obtained by personal interviews, and in a crucial part of the interview the interviewer asks the questions necessary for the analyst to do what

[8] Examples of the application of these techniques by Lazarsfeld's students and their students are given in Sills, "Lazarsfeld, Paul F."

Lazarsfeld called "discerning": that is, determining not only that a person was *exposed* to a given influence, but that he or she acted in a certain way *because* of that exposure.

Lazarsfeld's initial article on reason analysis, "The Art of Asking Why" (1935), was published shortly after he arrived in the United States, and is based largely on consumer studies that he had carried out at the Forschungsstelle in Vienna. The article identifies three types of data that need to be obtained by asking "why" questions in studying consumer purchases: (1) influences that lead toward action, (2) relevant attributes of the product, and (3) motives of the purchaser. This formulation has a generality that goes far beyond consumer research, and has been widely used—with adaptations and extensions—by Lazarsfeld, his students, and others.[9]

The intensity of Lazarsfeld's interest in the study of action is indicated by an important 1958 historical essay and by the attention given to it in his two methods readers (1955, 1972), in his autobiographical memoir (1968), and in his essay "Working With Merton" (1975). He viewed the analysis of action as a way of merging the study of individuals with the study of the aggregate effects of individual actions, and thus as a way of merging psychology and sociology.

The Relationship between Individual and Collective Properties. Techniques for relating the characteristics of individuals to those of collectivities were termed by Lazarsfeld "contextual analysis." They involve characterizing individuals by some characteristic of the group to which they belong (the context).

[9] The best explication of reason analysis is in Hans Zeisel, *Say It With Figures,* 6th ed. (New York: Harper & Row, 1985), pp. 186-215, first published in 1947. Examples of the application of reason analysis to such topics as consumer choice, changes in voting intentions, choosing or not choosing trial by jury, choosing an occupation, getting married or divorced, going to a psychiatrist, joining a voluntary association, moving from one house to another, and not practicing contraception are given in Charles Kadushin, "Reason Analysis," in *International Encyclopedia of the Social Sciences,* vol. 13, ed. David L. Sills (New York: Macmillan and Free Press. 1968). pp. 338-43.

It is then noted how individuals who are similar in other ways differ in their opinions or behavior in accordance with their group. The characteristic of the group may be an aggregate of individual characteristics (as in "climate of opinion" studies) or it may be a so-called "global" characteristic that describes the collectivity as a whole. Lazarsfeld first made systematic use of the procedure in a 1955 study of social science faculty members in American colleges and universities (Lazarsfeld and Thielens 1958).

Mathematics in the Social Sciences. Lazarsfeld never abandoned his early interest and training in mathematics. He sought for many years to introduce improved mathematical methods into the social sciences with efforts such as his work in latent structure analysis and in dichotomous algebra. His own work was primarily in mathematical psychology, because he sought to model processes within the individual. His impact on mathematical sociology was of a different kind: he posed problems, he raised questions, and he organized the efforts of others. James S. Coleman dedicated his *Introduction to Mathematical Sociology* to him, and eight colleagues and former students contributed mathematically based articles to the 1979 *Festschrift.* [10]

Other Interests

The History of Empirical Social Research. As early as the Marienthal study, Lazarsfeld was fascinated by the historical development of research methods. At his request, Hans Zeisel wrote an appendix for the book that traces the history of what is called "sociography"—primarily community studies. But he did little systematic work on the topic until a 1959 interdisciplinary conference led him to prepare a paper

[10] See the articles by T. W. Anderson, Raymond Boudon, James S. Coleman, Leo A. Goodman, R. Duncan Luce, Anthony Oberschall, Peter H. Rossi, and Herbert A. Simon in Merton, Coleman, and Rossi, *Qualitative and Quantitative Social Research.*

(1961) tracing the history of quantification in sociology. In 1962-63 he gave courses and led seminars at the Sorbonne and at Columbia University on the history of quantification, which became one of his major interests during the remainder of his life.[11] His interest in the topic made him into something of a reverse missionary during the last fifteen years of his life: he attempted to convince Europeans that American style empirical social research had been strongly influenced by an earlier European empirical tradition. He was primarily responsible for—or exerted a strong influence on—the establishment of research institutes in Oslo and Vienna, and his visits to Paris and Warsaw greatly altered the nature of social research in these cities. He visited Paris frequently, where Columbia University's Reid Hall became almost his second home. He invited a number of Europeans to spend a year at Columbia, and in this way he enriched sociology on both sides of the Atlantic. When he died, Raymond Boudon and Jean Stoetzel, who had worked closely with him during his stays in Paris, wrote memorial articles for the Paris press, and practically every sociological journal in Western Europe published an obituary.

The Utilization of Social Research. Lazarsfeld's career began with his founding of an institute for applied social research in 1925, and he never lost his interest in the practical applications of research. When his presidency of the American Sociological Association (1962) offered him the opportunity to set the theme for the annual meeting, he chose "the uses of sociology" (Lazarsfeld, Sewell, and Wilensky 1967).

Lazarsfeld's Circle

Throughout his life, Lazarsfeld worked intensively with students aid colleagues, and a full-scale intellectual biography would of necessity trace the intertwining of his career

[11] Examples of publications on the history of empirical social research that were stimulated by Lazarsfeld are given in Sills, "Lazarsfeld, Paul F."

with those of his associates, his "circle." Furthermore, his early years as an organizer of socialist youth activities established a pattern of leadership that he never fully abandoned: he was skilled at telling others what they should do, and then helping them do it. As his associate Morris Rosenberg once noted, "his most obvious impact is upon his students and, of course, on his students' students. When you read Pete Rossi, you read Paul; when you read Jim Coleman, you read Paul; when you read Charlie Glock, you read Paul; and so on and on."[12]

Major Associates. Hans Zeisel, who became a professor of law and sociology at the University of Chicago, worked with Lazarsfeld in Vienna at the Forschungsstelle, with Jahoda and Lazarsfeld on the study of Marienthal, and later with Lazarsfeld at the Bureau of Applied Social Research. His *Say It With Figures* (1947), a textbook that is more than a textbook, a manual that is more than a manual, now translated into six languages, is a product of their long collaboration. Zeisel's essay in the 1979 Lazarsfeld *Festschrift* is both a record of and a sentimental tribute to their lifelong association.

Robert S. Lynd, then professor of sociology at Columbia, befriended Lazarsfeld at the time of his first visit in 1933; for many years, the Lazarsfelds went to the Lynds' apartment on Thanksgiving Day or on Christmas Eve. Bernard Berelson, Frank Stanton, and Edward A. Suchman were early collaborators in his work on mass communications. Allen H. Barton, professor of sociology at Columbia, first studied with Lazarsfeld in 1947 and accompanied him to Norway in 1948 to help establish a research institute at the University of Oslo. They coauthored an important article on qualitative measurement (1951) and Barton was director of the Columbia Bureau from 1962 to 1977.

In Paris (and for one year in New York), Lazarsfeld had

[12] Personal communication: 1981.

a profound influence on Raymond Boudon; in Warsaw, where he was fascinated by the social research that was being done in the late 1950s to test the efficacy of various socialist programs, he was assisted primarily by Stefan Nowak. His collaborative relationships with his three wives were noted earlier. For years, he and the Columbia philosopher Ernest Nagel taught a successful graduate seminar on the logic of social inquiry. He had intense and complex relationships with two sociologists whose approaches to scholarly work were sharply at variance with his own: T. W. Adorno and C. Wright Mills. Both were critical of him; while critical of much of their work, he nevertheless went to great lengths to try to find common ground. More important than any of those named above in their effect on Lazarsfeld, and in his influence on their thinking, are two of the most eminent American sociologists of the twentieth century—Samuel A. Stouffer and Robert K. Merton.

Collaboration with Samuel A. Stouffer. Lazarsfeld and Stouffer first met in 1936. At that meeting, they agreed to collaborate on a monograph concerning the American family in the depression that was a part of a Social Science Research Council inquiry into the era directed by Stouffer (Stouffer and Lazarsfeld 1937). Thus was established what Lazarsfeld called "an alliance" that lasted until Stouffer's death in 1960. Their most notable collaboration was on the wartime research concerning the U.S. Army that led to a four-volume series, including the two volumes entitled *The American Soldier;* published in 1949 and 1950. Lazarsfeld also edited and wrote the introduction to a posthumously published selection of Stouffer's papers—although Stouffer had himself selected the papers.

It was Stouffer who first introduced Lazarsfeld to the flourfold contingency table—a concise way of demonstrating the relationship between two dichotomous variables—by drawing one on a luncheon tablecloth one day in Newark in 1937. During the war years, when Lazarsfeld was a consultant to the War Department, his ideas on latent structure analysis and on the causal analysis of survey data were worked out in discussions with Stouffer. Their personal and research styles were totally different: Lazarsfeld was the somewhat flamboyant, cultured European, raised as a socialist; Stouffer was the homespun, modest Midwesterner, raised as (and remaining) a Republican. Lazarsfeld surrounded himself with research assistants; Stouffer was famous for running his own statistical tables on the IBM counter-sorter outside his office door in Washington, and later at Harvard. Both knew how to organize research workers, and both were totally absorbed in obtaining ideas and findings—not from speculation, but the hard way, from data. Perhaps because he saw in Stouffer a more disciplined and self-effacing reflection of himself, Lazarsfeld considered Stouffer "the most important man of all of us . . . an outstanding mind in our generation"; Stouffer's effect upon his thinking, although subtle, was enormous.

Collaboration with Robert K. Merton. Lazarsfeld and Merton joined the Columbia faculty at the same time; in fact, their appointments were designed to resolve an internal dispute over whether the next major appointment to the Columbia sociology faculty was to be theorist or a methodologist. With Merton and Lazarsfeld, Columbia hoped to get both. It did, and for decades Merton was known as the major "theory" person in the department, Lazarsfeld as the chief "methods" person. To a certain extent this is true; most graduate students studied under both but aligned themselves primarily with one or the other. Hanan Selvin, like Patricia Kendall, one of the relatively few graduates of the department who worked closely with both Lazarsfeld and Merton, spoke for generations of students when he said in a 1975 *Festschrift* for Merton that "we were satellites, not of one sun, but of two, for Robert K. Merton and Paul F. Lazarsfeld so dominated Columbia during these three decades that no lesser figure of speech will do."

Lazarsfeld and Merton were close friends and colleagues for thirty-five years. There are few examples in the history of science of two such brilliant and accomplished colleagues developing and maintaining such a strong personal and scientific relationship for such an extended period. An attempt to explicate the relationship in brief compass is not a digression; rather, it is central to an understanding of Lazarsfeld's accomplishments.

It was primarily a professional rather than a social relationship; Lazarsfeld entitled his account of their collaboration "Working with Merton" (1975). As noted below, they were coauthors and coeditors of a small number of important publications. Moreover, Merton was the anonymous collaborator on almost everything Lazarsfeld published. On the title page of the copy he gave Merton of a long chapter on latent structure analysis, Lazarsfeld wrote: "Bob, this is the first item in 20 years you did not have to work on. P."

Each of their six published collaborative efforts reveals something important about their relationship. They coedited *Continuities in Social Research: Studies in the Scope and Method of "The American Soldier"* (1950), a brilliant attempt to enrich social theory by reappraising the wider effects of the series of attitude surveys that Stouffer had conducted among soldiers during the war. Their three coauthored articles on mass communications deal with the social and cultural meanings of the radio research they had both carried out. In their "Friendship as Social Process" (1954), Lazarsfeld recast a number of Merton's sociological propositions about friendship into formal, deductive mathematical terms and indicated their research relevance. And in "A Professional School for Training in Social Research" (1950), they reviewed plans for a new educational institution they had talked about for many years, but which never reached fruition.

In their life at Columbia together, it was Merton whose diplomatic skills were called upon to extricate Lazarsfeld from numerous troubles with the university, his colleagues, his students, or his sponsors. It was Merton who toiled over Lazarsfeld's drafts, applying to them the brilliant editing skills that have benefited so many of his students and colleagues. And it was Merton who established and maintained the close support with the Columbia University community that Lazarsfeld—more the outsider and much more the world traveler—only achieved toward the end of his tenure.

The substance of their professional friendship is more difficult to specify; elaborating it would be a worthy research project in itself. Certainly Merton's interest in the sociology and history of science influenced Lazarsfeld's work on the history of the empirical study of action, and on the history of methodology more generally. Lazarsfeld's concept of "global characteristics"—the characterization of groups based on characteristics that are not derived from the properties of individual members (population density is an obvious example)—influenced Merton's subsequent work in the sociology of science. Lazarsfeld had no opportunity to create a personal library during his first decade in the United States; when he first saw Merton's eclectic library, he resolved to develop his own, and did.

In his article in the Merton *Festschrift,* "Working with Merton," Lazarsfeld provided a detailed recollection of their relationship; in the Lazarsfeld *Festschrift,* Merton refers to Lazarsfeld as a "brother." Their students and colleagues know that these words only hint at the depth and complexity of their intellectual and personal companionship.

Recognition

Lazarsfeld received many acknowledgments of his accomplishments during his lifetime. He was president of both the American Association for Public Opinion Research (1949-50) and the American Sociological Association (1961-62), and he was an elected member of the National Academy of Education as well as the National Academy of Sciences. He received honorary degrees from Chicago and Yeshiva universities in 1966, from Columbia in 1970, from Vienna in 1971, and from the Sorbonne in 1972, the first American sociologist ever so honored. In 1955 he was the first recipient of the Julian L. Woodward Memorial Award of the American Association for Public Opinion Research, and in 1969 the Austrian Republic awarded him its Great Golden Cross, largely for his help in establishing the Institute for Advanced Studies in Vienna in 1963. He was a much sought-after consultant, speaker, and teacher. Shortly after his death, a Paul F. Lazarsfeld Memorial Fund was established in order to sponsor a series of lectures in his honor. In 1983 a large collection of his books and papers was dedicated as the Lazarsfeld Archives at the University of Vienna.

Legacy

Lazarsfeld's innovations in consumer research and his effect on the business and advertising communities were substantial. He was a major trainer and model for the generation of advertising and market researchers that matured in New York City in the decades following World War II. His work in communications research helped create it as a field of scholarship, and through his analyses of propaganda during World War II and his influence upon the research activities of the Voice of America, he helped create the field of inter national communications research. His model for the institutionalization of training and research in the social sciences is embodied in dozens of thriving research organizations around the world. He was one of the founders of the Center for Advanced Study in the Behavioral Sciences in Stanford, California. His use of the sample survey as a tool for causal analysis helped transform opinion polling into a scientific method, and his development and use of the panel method has enormously influenced a wide range of evaluations of the effect of educational or social reform programs.

Marginality. In spite of these achievements, Lazarsfeld felt that he was somehow an outsider in America, a marginal person, never at the center of things. Why did he feel this way? He thought that it was the result of his Jewishness, his foreignness, his heavy accent, and his interest in such a low-status activity as market research—but these reasons are not fully convincing. He lived his life, as he once put it, like a bicycle rider, always compensating so as not to fall off. He left his marginal position at the University of Vienna for equally marginal positions at Newark, Princeton, and (at least initially) Columbia. He left mathematics because he knew that he would never be in the first rank, but he never quite believed that events had transformed him into a sociologist. He approached every new research topic from a startlingly new direction, and he took pride in the originality of studies carried out at the Columbia Bureau, in contrast to the more traditional research carried out at other university centers such as those at Chicago and Michigan. Like an expert skier, who knows that the best snow is generally at the edge of the trail, his genius kept him carefully away from the accepted center of most problem areas. "But look," he would say with his hand raised, and then proceed to outline a highly original plan of action.

Lazarsfeld's self-perception of marginality was allied to his conception of his role in the social sciences: to be on the margin is also to be on the frontier. It can also be argued that his marginality contributed to the intellectual traffic between ideas and methods that made him a singularly influential figure in the history of social research. In a memorial article published in *Le Monde* shortly after Lazarsfeld's death, Raymond Boudon noted that "his work has attained the most noble form of marginality: many of the ideas which he introduced have become so familiar that hardly anyone bothers to attribute their paternity to him."

The Search for Convergences. One consequence of Lazarsfeld's sense of marginality for his intellectual activity was his never-ending search for convergences between different intellectual traditions—convergences that could serve to enrich both traditions. His search for convergences undoubtedly was a result of being Viennese: bold syntheses are characteristic intellectual products of Vienna. His collaboration with the theorist Merton is the most obvious of these convergences. Other convergences that he encouraged were between disciplines: psychology and sociology; mathematics and sociology; anthropology and media research; and sociometry and survey research. He sought both a convergence and a mutual understanding between the critical sociology of' the Frankfort school (see especially the writings of T. W. Adorno) and the dominant positivistic trend in American sociology, as well as between Marxist sociology and mainline European-American sociology.

Other convergences he sought were between the social sciences and the humanities. He used his early studies of radio to build bridges between the social sciences and such fields as literary analysis and music. He sought to relate the philosophy of science and empirical social research, historical analysis and opinion research, and logic and concept for mation. While some of his critics were accusing him of mindless quantification, he was spending time reading and talking with some of the nation's leading humanists, historians, and philosophers.

Finally, he sought convergences between different research traditions and methods. He made connections between small-group research and the use of sample surveys to study interpersonal influence (Katz and Lazarsfeld 1955) and between the use of fixed-choice questions in surveys and so-called open-ended interviewing. His work on concept formation (1966) and index construction (see Lazarsfeld, Pasanella, and Rosenberg 1972) is a monument to interdisciplinary borrowing and to making connections. With Allen Barton, he took a polemic of C. Wright Mills against the decline of "craftsmanship" and developed it into a scheme for studying the man-job relationship (Barton and Lazarsfeld 1955). And he encouraged the foremost qualitative researcher of the 1950s—David Riesman—to reinterview a sample of the respondents during his study of American social scientists (Lazarsfeld and Thielens 1958). A volume of interdisciplinary essays edited by Mirra Komarovsky, *Common Frontiers of the Social Sciences* (1957), was inspired by him and prepared under his general direction. A modern-day Leonardo da Vinci, he largely ignored the traditional specialization of knowledge and sought to find new truths by bringing people and ideas together.

The convergence in the social sciences that Lazarsfeld tried hardest to effect is that between quantitative and qualitative research. In almost every field in which he worked, he tried to fuse these two productive modes of inquiry: it was the theme with which he ended his presidential address to the American Sociological Association; the journal *Quality and Quantity* was founded in 1967 under his direct influence; and for all these reasons the *Festschrift* in his memory is en titled *Qualitative and Quantitative Social Research.* [13] In the words of James S. Coleman, he was "one of those rare sociologists who shaped the direction of the discipline for the succeeding generation."[14]

The author is particularly grateful for the assistance and suggestions of the following friends and associates of Paul F. Lazarsfeld: Albert E. Gollin, Patricia L. Kendall, Robert K. Merton, Paul M. Neurath, and Hans Zeisel.

[13] See Merton, Coleman. and Rossi, *Qualitative and Quantitative Social Research.*
[14] Coleman, "Paul S. Lazarsfeld," p. 1.

Selected Bibliography

This bibliography contains only a selection of Lazarsfeld's major scholarly publications. Extensive bibliographies may be found in Paul M. Neurath, "The Writings of Paul F. Lazarsfeld: A Topical Bibliography," *op. cit.;* in Lazarsfeld 1982; in Merton, Coleman, and Rossi, *Qualitative and Quantitative Social Research; op. cit.;* and in Sills, "Lazarsfeld, Paul F.," *op. Cit.*

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Esmond R. Long

June 16, 1890-November 11, 1979

By Peter C. Nowell and Louis B. Delpino

Esmond Long successfully combined two chief areas of interest in his lengthy career. In over fifty years of research on mycobacterial infections, he made major contributions that ranged from the biochemistry of tuberculin to the epidemiology of tuberculosis in different populations. At the same time he wrote extensively on the history of medicine and biomedical research, including definitive texts on the history of pathology.

Esmond Ray Long, known as "Es" to his colleagues, was born on June 16, 1890, in Chicago, not far from Northwestern University. His father, John Harper Long, was a professor of chemistry at Northwestern and, from 1913 until a year before his death in 1918, dean of the School of Pharmacy.

In 1906, after completing his secondary education at the University of Chicago's Morgan Park Academy, Esmond Long took a year of private instruction in chemistry from his father and his associates. In 1911 he received his A.B. degree from the University of Chicago, majoring in chemistry.

Earlier, under the influence of his mother's interests, Long had developed an abiding taste for literature, languages, and history. He had once even considered becoming a teacher of Latin. For the moment, however, it was the influence of his father, and of two distinguished professors of chemistry, Julius Stieglitz and John U. Nef, that was to prevail. Later, the sense of history inculcated by his mother's awareness of culture would surface, as would his literary bent, in Long's books *History of Pathology* (1928), *Selected Readings in Pathology from Hippocrates to Virchow* (1929), and *A History of American Pathology* (1962), as well as in his last substantial writing effort, *Development of the Department of Pathology in School of Medicine of the University of Pennsylvania,* privately published in 1977.

In 1918 Long received his Ph.D. degree from the University of Chicago's School of Medicine. His M.D. degree, from the Rush Medical College, then a part of the university, was awarded in 1926.

The extensive span of time between degrees was due largely to a prolonged bout with pulmonary tuberculosis whose onset came in 1913, when Long was in his second year as a medical student. He coughed up several mouthfuls of blood while playing tennis, and that evening he went back to the laboratory, stained his sputum, and found it full of tubercle bacilli. There had been no previous indication of ill health; on the contrary, during his premedical days at the University of Chicago, Long had been a member of the track team, specializing in the mile run.

Long spent the next five years undergoing the various forms of tuberculosis therapy then fashionable. These included dry-air treatments in a tent in Arizona, programs of modified exercise, nearly a year of bed rest in Seattle, and superalimentation with a diet rich in cholesterol. This regimen might typically comprise four quarts of milk and as many as a dozen eggs daily—in addition to three regular meals!

Throughout his incapacitation, Long kept abreast of the scientific literature and even managed to do some laboratory work. Toward the end of this period, in 1918, he worked as an assistant under Edward R. Baldwin, director of the Saranac Laboratory at the renowned tuberculosis center in Saranac Lake, New York, and one of Edward Livingston Trudeau's noteworthy successors. When Long was presented with the Gold-Headed Cane of the American Association of Pathologists and Bacteriologists in March 1971, he reflected on the time he had spent with Baldwin:

> I had made some preliminary studies, quite independently in a tiny laboratory I constructed in Seattle during my Western migration for the cure, learning the chemical requirements of cultures of a number of bacteria on chemically defined synthetic media. Baldwin gave me his full approval for proceeding on the same course with the tubercle bacillus. He and staff assistants taught me how to set up cultures of the bacillus, isolate it from patient and laboratory animals, and follow long periods of observation of the disease in guinea pigs and rabbits. He also made me the clinical resident in the Reception Hospital of Saranac Lake, where I examined patients with far advanced disease, flouroscoped them periodically, and gave them pneumothorax refills, for almost a year. I examined specimens for doctors in town, and with Baldwin's constant encouragement had a thorough grounding in the day-by-day practical care of tuberculosis patients, as well as in its more theoretical and laboratory aspects.

Baldwin provided further encouragement by ensuring that Long became acquainted with others who were investigating the same or similar problems at Saranac Lake: Lawrason Brown, Fred Heise, Homer Sampson, S. A. Petroff, William Steenken, and Leroy Gardner. "From each of these," Long recalled, "I learned something." The most fruitful moments stemming from Long's associations at Saranac Lake, however, were with Allen K. Krause, a graduate of the tuberculosis center's school of treatment and research, who had left for the Johns Hopkins University:

> Krause pushed forward the researches of Trudeau and Baldwin and developed a concept of tuberculosis that dominated American views of its pathogenesis for years, only to give way in time to more advanced concepts

> promulgated by others. I read assiduously everything that Trudeau, Baldwin, and Krause ever wrote, and consciously tried to pattern my own writing on the model set by Krause, who was a highly gifted writer and speaker, with an encyclopedic mind and prodigious memory of the literature on tuberculosis. He was a superb editor, a good critic of what went into the *American Review of Tuberculosis,* including what I wrote myself, and altogether an ideal to follow. Unfortunately his frail physique, which had carried him through illness with cancer of the bowel and pulmonary tuberculosis, gave way finally, with loss of his mind and spirit. It is a travesty of the times that this highly intelligent man is now almost forgotten.[1]

By 1919 Long had recovered sufficiently to return to Chicago, where under Dr. H. Gideon Wells, the acknowledged leader in what was known as "chemical pathology" at that time, he resumed his thesis work on purine metabolism. Long had initially come under Wells's teaching in 1911, when Julius Stieglitz recommended him as a chemical assistant to Wells. Long later recalled that he, Wells, and another assistant "shared the small quarters customary for a young professor of pathology in those days—Wells was only thirty-six, but seemed old to me—and I have been grateful ever since for the intimacy of our crowded room. We became closely acquainted without losing the relation of master and pupil."

Most leaders in pathology at that time were largely oriented toward the morphological aspects of the science. Wells was an excellent pathological anatomist and histologist, a fact often overlooked because of his renown as a chemical pathologist and immunologist. Reflecting on his mentor, Long wrote:

> Wells was jovial, always apt in expression, and witty in his outlook on everything we did. Those were the days of what he called "wash-tub chemistry." We ground up vast quantities of pathological tissues and analyzed

[1] Esmond R. Long, "Response to Presentation of the Gold-Headed Cane," American Association of Pathologists and Bacteriologists, March 1971.

> them by methods that were extremely laborious by comparison with those of today. When we wanted, for example, to find the guanine or adenine content of a tumor, we actually isolated the pure substances and weighed them, verifying them by their melting points. Today we would put a sample in an expensive optical system, press a button, and get a quantitative printout of almost everything in the sample. All pathologists are chemical pathologists now, and it is largely because of the refinements of present apparatus, and the relative ease with which facts are learned, that this has come about.[2]

Long completed his Ph.D. degree under Dr. Ludwig Hektoen in 1918. He continued his pursuit of the M.D. degree while teaching general and special pathology to second-year medical students. The courses entailed both day and night autopsies, conducted at funeral homes as well as at hospitals, and it was this rigorous schedule that gave Long his basic experience in postmortem dissection and microscopic pathology. During this same demanding period, he continued his own laboratory research as well as his scholarship in medical history.

Long spent the summer of 1921 at the Stanford University Medical School, in clinical study related to his protracted quest for the M.D. degree. During his return trip to Chicago in September, he visited Denver, where he renewed his acquaintance with a distant relative, Marian Beak Adams, with whom he shared great-great-grandparents.

Esmond Long and Marian Adams were married in June of 1922, and shortly afterwards they sailed for Prague, Czechoslovakia, for a rather unconventional honeymoon.

At the German University in Prague, Long spent six months with Anton Ghon, who was well known for his studies on the primary complex of tuberculosis and its relation to the allergy of the disease. Long honed his autopsy dissection

[2] *Ibid.*

technique, and under Ghon's guidance devoted special attention to the pathologic lesions of primary tuberculosis infection—the so-called "Ghon complex," a localized parenchymal area of disease and enlarged hilar or mediastinal lymph nodes. Of his days in Prague, Long stated:

> I made an almost ludicrous start with Professor Ghon. America was a little remote to most of the students in the autopsy room. Ghon and the rest of the staff and students crowded around the table to see how an American professor—I was an assistant professor by that time—made a postmortem examination. I was, to put it mildly, less than an expert by central European standards. Of my performance that day the less said the better. One by one the students drifted away, and at the end Ghon said he thought I would gain if I had a chance to observe their methods for a time.[3]

The time, Long noted happily, was brief. He was assigned the help of Ghon's own assistant, Koenel Terplen, and of an elderly yet still adept diener named Weidrich, who had been the personal diener of Karl Rokitansky in Vienna during the late 1870s. "That those hands helping me had done the same for Rokitansky, whom I ranked with Morgagni and Virchow, seemed as great an honor as working with Ghon himself," [4] Long recollected.

Long translated several of Ghon's papers into English. Their acquaintance grew, enduring beyond the visit to Prague, and the two men remained in touch until Ghon's death—ironically, of tuberculosis, the field to which he had contributed so brilliantly—in 1936.

Long resumed his interrupted Chicago research in 1923, focusing his studies on investigations into the nature of the active principle of tuberculin and on the varying inflammatory reactions to tuberculin in both normal and tuberculous

[3] *Ibid.*
[4] *Ibid.*

subjects. Of particular interest were the heightened responses to tuberculin in such tissues as the testis, kidney, cornea, and skin, as well as in a specific cell, the spermatocyte. This led to the development of the spermatocyte test.

Koch had been the first to record, in 1901, that tuberculin (extracts of the tubercle bacillus and the medium in which the bacillus had been grown) was toxic for the tuberculous guinea pig and nontoxic for the nontuberculous. As a diagnostic reagent in the human form of the disease, tuberculin injected subcutaneously has no effect on nontuberculous subjects but causes inflammation at the injection site in tuberculous patients. The problem seen by Long was that no test had yet been made with a measured quantity of the active principle of tuberculin.

"The reason for this," Long wrote, "is that we do not know what the active principle of tuberculin is. No preparation containing the active principle and nothing else has ever been made. We do not know whether there is a single active principle or several responsible for the tuberculin reaction. We are far from sure of the general chemical nature of the substance which is active. In gross chemical fractionation of tuberculin, activity remains with the protein portion. This does not mean that the active substance is necessarily protein. It may be merely absorbed by protein. Furthermore, on finer fractionation protein fractions which are not active can be separated from tuberculin. Hence chemical evaluation . . . is at present an impossibility."[5]

Long reviewed the tuberculin standardization methods then current, noting that in each the disadvantages far outweighed the advantages. Methods based on the lethal dose of tuberculin for tuberculous guinea pigs were too gross and,

[5] "Standardization of Tuberculin," *Journal of Infectious Diseases,* 37(1925):368-84.

being quantitative only in a "pass or fail" manner, precluded the establishment of a tuberculin unit. In the method of intracutaneous testing, tuberculin was standardized with respect to the skin of an allergic animal and later used on the skin of an allergic patient. Long found this method also uncertain, not only because of the great variability in the skin reactivity of tuberculous guinea pigs, but also because in weak concentrations the traumatic reaction could not be distinguished from the specific reaction. Two other approaches, the complement-fixation and precipitin methods, each furnished a unit whereby doses of tuberculin could be measured, but both also shared the serious drawback of complete dissociation between the standardizing test and the use to which the tuberculin was put.

Long had earlier observed that whether or not necrosis occurred in tissues injected with tuberculin was dependent on several factors, including the type of tissue and the dose of tuberculin. Skin tissues, for example, were relatively resistant; necrosis occurred only with strong doses. By contrast, tuberculin injected into the testes of tuberculous guinea pigs produced a severe reaction characterized by the coagulation of spermatocytes and their derivatives. At least a hundred experiments in Long's laboratory showed tuberculin to be nontoxic for the spermatocytes of nontuberculous animals, whereas it never failed to elicit reaction in the testes of tuberculous guinea pigs.

"There can be no question," Long stated in *the Journal of Infectious Diseases,* "that the reaction just described is a true tuberculin reaction. It is absolutely specific . . . . Furthermore, preparations of bacteria other than the tubercle bacillus (and other acid-fast bacilli), of which several have been injected, do not elicit the reaction. Finally, the type of reaction is histologically identical with that observed in the skin reaction, except that degeneration and necrosis are more pronounced.

This is to be explained as a result of the exquisite susceptibility of the delicate germ cells."[6]

The test had several other advantages. It could detect one-tenth of the minimum quantity detectable by the skin test. Reactions were far more constant, and microscopic sections of testes could be preserved as a permanent record of any test. Long concluded, "Necrosis and subsequent absorption of the spermatocytes is used as the basis for recognizing a positive reaction, and the limiting dilution at which this is observed under the conditions outlined above is considered to represent one unit of tuberculin."[7]

Meanwhile, Long's probing into the active principle of tuberculin continued in collaboration with Dr. Florence Seibert, who worked with him as a chemical assistant. Their collaboration ultimately showed the active principle to be protein in nature. In 1926 the findings of these investigations were published in the *American Review of Tuberculosis,* and in 1932 Long was awarded the Trudeau Medal by the National Tuberculosis Association as a result of these studies.

The year 1923 saw publication of the first edition of *The Chemistry of Tuberculosis,* which Long coauthored with H. Gideon Wells and L. M. DeWitt. Long's *History of Patholoy* was published in 1928, the same year he attained the rank of professor. The following year his *Selected Readings in Pathology from Hippocrates to Virchow* appeared. Throughout this busy period, and until 1950, Long also served as the special editor in medicine for *Webster's International Dictionary,* defining or approving the definitions of some 15,000 words.

In 1932, Long moved with his family to Philadelphia, where he became a professor of pathology at the University of Pennsylvania and the director of laboratories at the Phipps Institute for the Study, Treatment, and Prevention of Tuber

[6] *Ibid.*
[7] *Ibid.*

culosis, a department of the university. During this period Long continued his collaborative investigations with Florence Seibert into the active principle of tuberculin. Dr. Seibert was eventually able to crystallize and purify the substance, now known as purified protein derivative (PPD) and used as a standard dermal reactivity indicator in diagnosing tuberculosis. PPD became the tuberculin standard for the U.S. Public Health Service, and in 1952 was adopted as the international standard by the World Health Organization.

Long became director of the Phipps Institute in 1935, holding the position until his retirement in 1955. He was chairman of the Division of Medical Sciences of the National Research Council from 1936 to 1939, and president of the Wistar Institute of Anatomy and Biology in Philadelphia from 1939 to 1942.

A great deal of the research being done at the Phipps Institute involved environmental factors and racial differences in tuberculosis, the experimental pathology of the disease, and approaches to detection, prevention, and control. To this demographic base Long added his own knowledge of the metabolic and anatomic changes occurring in tuberculosis, thereby achieving a synthesis of the pathology of the active disease with its epidemiology. This enhanced understanding led to Long's appointment as a consultant on tuberculosis to the U.S. Army Medical Corps, as a lieutenant-colonel, during the Second World War. He was shortly afterwards made deputy chief of the Professional Services Division of the Office of the Surgeon General, with responsibilities involving the medical care of recruits, the development of hospital policies and standards, and other tuberculosis-related programs. When the war ended, Long's activities shifted focus to the treatment and prevention of tuberculosis among the population of strife-torn Germany. These various efforts led to numerous publications, and to a definitive text, with Seymour Jablon, entitled *Tuberculosis in the Army of the United States.*

From 1932 to 1948, Long served as a member of the Advisory Medical Board of the Leonard Wood Memorial of the American Leprosy Foundation. His responsibilities during part of this time involved directing experiments on the separation of leprosy bacilli from infected tissues. Although Long's interests had included the mycobacteria in general, this was his first principal involvement with leprosy, a field in which he was to earn further distinction in coming years.

In 1963 Dr. H. W. Wade, editor of the *International Journal of Leprosy* since its inception in 1933, announced his retirement. Dr. Chapman H. Binford, medical director of the Leonard Wood Memorial, nominated Long to be Wade's successor. Long was elected, assuming editorship of the journal in 1964. The journal was a year behind in publication. Long brought it up to date, frequently traveling by train or car from his retirement residence in Pedlar Mills, Virginia, to his editorial quarters at the Leonard Wood Memorial in Washington, D.C., a distance of 200 miles. During the winter months, when travel was uncertain, Esmond and Marion Long closed their Virginia home and lived in Washington at their own expense.

Long was strongly aware of the dependence of progress in leprosy research on advances made in the study of other mycobacterial diseases. In a 1965 editorial in the *International Journal of Leprosy,* he cited the leads obtained for the investigation *of M. leprae* through knowledge of the unique growth requirements of the etiologic agent of Johne's disease, a deadly, chronic mycobacterial enteritis afflicting cattle, sheep, goats, and deer. Long also noted the recent identification of *M. ulcerans* and *M. balneii* (later *M. marinum*) for use in the differential diagnosis of leprosy. These observations, as well as a desire to expand the scope and readership of the journal, prompted Long to recommend to the editorial board an appropriate subtitle for the publication. Long's suggestion was approved, and the first issue of *I.J.L.* 36 (1966) carried the new heading *International Journal of Leprosy and Other Mycobacterial Diseases.*

In 1966 the Longs returned permanently to Philadelphia, where Esmond, despite his "academic retirement," continued the active pursuit of his many scientific interests. During this period, Long was able to enjoy the renewal of friendship with a fellow scientist whom he had not seen for a quarter of a century. While on assignment with the U.S. Army in Europe following the war, Long had become acquainted with Dr. E. Freerksen of the Borstel Research Institute in Borstel, Germany, whom he helped in developing a tuberculosis research program. Early in 1970, Freerksen approached Chapman H. Binford concerning the possibility of the *International Journal of Leprosy's* publishing the proceedings of a colloquium on leprosy research to be conducted at Borstel that August. Binford in turn consulted with Long, who reestablished communication with Freerksen and agreed to be the final editor of the proceedings. Long was invited to attend the colloquium, but was unable to go. In his address at the opening ceremonies, Freerksen stated:

> Just about 25 years ago [Esmond Long] contributed decidedly to the fact that there exists today a Forschungs institute Borstel. With his spirit, combining humanity, loyalty, objectivity, and personal courage, he conquered all difficulties arising during this particular time and led the planning negotiations with the occupation to a positive conclusion. As editor of this colloquium he is continuing a line—not influenced by favor or disfavor—which he himself once began.[8]

The colloquium comprised some seventy-five papers and talks, many of them given informally with lantern slides.

[8] Chapman H. Binford, letter to Robert E. Stowell, February 5, 1971.

Rearranging these presentations to comply with the style and format of the journal, as well as coping with problems of language, proved a tedious chore for Long, who customarily was a very swift editor. He spent 800 hours assembling the colloquium material into a well-illustrated, easily read volume of almost 500 pages entitled *Leprosy Today,* which was published in 1972 as a supplement to *I.J.L.,* 2:39.

Long's publications in the late 1960s and early 1970s extended his half-century-long interest in mycobacterial diseases and in the history of medicine. He completed histories of a number of scientific organizations (some of which he had served earlier as president), including the American Association of Pathologists and Bacteriologists, the American Society for Experimental Pathology, and, as noted previously, a history of the Department of Pathology at the University of Pennsylvania.

Long maintained an active interest in many of these organizations, and many of the present generation of experimental pathologists (including this writer [P.C.N.]) found in his attendance at meetings of scientific societies and academic councils a living link with the history of the field. Both in conversation and in writing in his later years, Esmond Long displayed a remarkable capacity for placing in perspective the rapid developments of the present day and the technological advances that made them possible.

Altogether, Esmond Long was a member of some twenty scientific societies, and served as president of at least six. Among the most notable of his many awards were the Philadelphia Bok Award (1954) and the Gold-Headed Cane of the American Association of Pathologists and Bacteriologists (1971). His prolific writings over more than half a century included nearly 300 articles and editorials, and twelve books. He also delivered at least twenty special lectures and edited three scientific journals.

This brief listing of accomplishments provides some indication of the enormous contributions made by Esmond Long to the study of mycobacterial diseases and to the preservation of the history of medicine. It does little, however, to convey the type of man who was responsible for such a prodigious output. All those who knew and have written about Esmond Long have stressed the genuine humanity, kindness, and humility of the man. These qualities were evidenced throughout his long life, and were certainly apparent even to those who met him only in his later years.

It is unfortunate that Esmond Long's life ultimately was saddened by the passing in 1974 of his beloved wife Marian, after a long illness with leukemia. Long joined her in death on November 1, 1979, survived by his son, Esmond R. Long, Jr., his daughter, Judith L. Neal, a sister, Ariel Miller, and five grandchildren.

For sources used in preparing this biographical memoir of Esmond Long the authors are indebted to material by Robert E. Stowell, Olaf K. Skinsnes, Chapman H. Binford, and Paul R. Cannon.

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E K Marshall, Jr.

Eli Kennerly Marshall, Jr.

May 2, 1889-January 10, 1966

By Thomas H. Maren

The ideas and ideals of the nineteenth century are embodied in many men and women born in late Victorian times, and so live on to the present. In this tradition was Eli Kennerly Marshall, Jr., who served the Johns Hopkins University School of Medicine for thirty-five years, first as professor of physiology, then of pharmacology and experimental therapeutics. Now near the end of our own century, it is fitting to review and celebrate the life of a scientist who made giant strides toward the twenty-first.

Marshall was born in Charleston, South Carolina, on May 2, 1889. His father's family came from England in the early part of the century. His paternal grandmother, Susan, was the daughter of Eli Kennerly, a Virginian who migrated to South Carolina. His mother's family was more varied. One side of her family was English—his merchant grandfather (Brown) was a descendant of the Rev. Samuel Andrew, a founder of Yale, and George Treat, one-time governor of Connecticut. His maternal grandmother (Beckmann) appeared more exotic; Marshall's notes say her family included members of German, French, and Russian descent. He once mentioned that he was part Russian—a rather incongruous note—and there was no whiff of the East in his character. He retained throughout his life the accent and many of the at titudes common in Charleston, which in the days of his upbringing was a somewhat unique cultural enclave.

On both sides his family was "in trade" in Charleston. His father ran the successful shoe business built up by his mother's father, and they all lived in the maternal grandparents' pleasant home, surrounded by aunts and (mostly) female children. Life moved in stately and routine fashion; there were large, early breakfasts; dinners at 3:00; and late, cold suppers. An isolated and shy boy, he went to private schools and graduated first in his class from Charleston High School. No effort seems to have been made to widen his horizons; he was sent to the small but excellent College of Charleston. He graduated at the age of nineteen in 1908, the only chemist in a class of eight men. As he describes those days: "I was devoted to books, took no interest in athletics, and really led a rather narrow life of the mind. College, except in an intellectual way, was for me a failure. No lasting friendships were made, and as I see it now, my college was a high school and my post-graduate years in chemistry, a poor makeshift for college."

He embarked on these graduate years at Johns Hopkins in 1908; there had been some Hopkins teachers and acquaintances in the city of Charleston and at college. He lived in a boarding house near the old University on Little Rose Street; again he was quite isolated. It would be most agreeable to say, from the vantage of seventy-five years, that this unspoiled innocent found, at the golden dawn of Johns Hopkins, the inspiration he craved and deserved. Alas, this first year was "a shattering of illusions." Ira Remsen, who had been director of the Chemistry Department, was now president of the University and kept partial control of the department, with no strong successors. Marshall was assigned to a thesis advisor and a topic that he deemed "unthinkable," and he returned to Charleston the next summer in the fierce indignation that was to become so characteristic. He arranged to complete his graduate work at the University of Chicago, but his old college professor, Francis Parker, interceded at Hopkins and arranged for Marshall to choose his own thesis advisor, Associate Professor of Organic Chemistry S. F. Acree. He returned to Baltimore, but his notes about the time, written thirty-five years later, were full of exclamation points and attacks on those "old men" who dared threaten his freedom.

The next two years, finally, "were extremely happy and pleasant," despite the ebb of the department. Acree gave him plenty of independence and he read widely in the excellent library, including the works of Emil Fischer, Nef, and Gomberg. He had planned to go into industry, but unaccountably he became interested in physiological chemistry. In 1911 he took an assistantship in that subject in the Medical School with Walter Jones,[1] beginning the association in three departments that was to last forty-five years.

He received the Ph.D. in chemistry and sailed to Europe in the summer of 1912, with a letter of introduction (but with no advance notice and no place to stay) to Abderhalden at the Physiology Institute at Halle. He was accepted, but again seemed isolated: little English was spoken, his German was weak, and he did not care for the system in which Herr Professor gave directions each morning to the staff for the day's work. But the loner was to triumph: "I spent time reading in the small department library. . . . I wanted to study enzyme action . . . ran across literature on urease and decided to work with it when I returned to Baltimore in the fall." But if only Marshall could have visited Paul Ehrlich at Frankfort-am

[1] "Walter Jones," in *Biographical Memoirs of the National Academy of Sciences,* vol. 20 (Washington, D.C.: National Academy of Sciences, 1943), pp. 79-139.

Main! Did he even know then of Ehrlich, whose name was to be coupled with his a quarter of a century later?

Back in Baltimore that winter, Marshall did just as he planned and attacked the urease problem with great force. It turned from a purely chemical exercise into a methodological triumph for physiology and chemistry (Section I below). Marshall wrote, "It was quite worthwhile to be on the mountaintop for a short time."

Marshall thought Jones unimaginative and not interested in his work. Had he stayed with Jones, would he have slid off that mountain? Jones thought there wasn't much left to do in physiological chemistry, and he couldn't do much for Marshall anyway. But there was a deus ex machina, or more accurately, a godlike figure on the floor below—John Jacob Abel,[2] professor of pharmacology, already a world figure. Abel was a gentle farmboy and school principal from Ohio who had gone to Europe for seven years "to prepare myself for the 20th century." There he studied medicine, chemistry. physiology, and pathology before becoming one of the founding chairmen at Johns Hopkins in 1893. He had isolated epinephrine from the adrenal, begun work on the artificial kidney, studied chemotherapy of trypanosomiasis with antimony compounds, crystallized insulin, pioneered work on the posterior pituitary, and founded both the American Society of Biological Chemists and the American Society for Pharmacology and Experimental Therapeutics. Most significantly, he believed that "the investigator is the man whose inner life is free."

Marshall had caught Abel's eye; indeed, the two departments lunched together, an important tradition that was to last many years. Abel arranged for Marshall to transfer to pharmacology, but with the most significant and serious pro

[2] "John Jacob Abel," in *Biographical Memoirs of the National Academy of Sciences, vol.* 24 (Washington, D.C.: National Academy of Sciences, 1947), pp. 231-57.

viso: that Marshall would study medicine. Rather complicated arrangements had to be made, because at that time faculty were not permitted to study for a degree. Marshall ended up doing the basic sciences (except biochemistry, physiology, and pharmacology, which he never took!) at Wisconsin and Chicago during the summers, but somehow the rule was relaxed so that he did his clinical work at the Johns Hopkins Hospital. Marshall's medical training had profound implications for him, as well as for generations of his students; he never ceased to bless Abel for this advice. He received the Hopkins M.D. in 1917.

During those years he lived most contentedly at the old Johns Hopkins Club at the corner of Monument and Howard streets. Intellectually and socially, it was a rich period. There was a host of young scholars from the medical school and the university, who traded shoptalk, gossip, and beer on Saturday nights. Much later he recalled Barnett (statistics), Mustard (Latin), and Lovejoy (philosophy). An appealing scene is that of Edgerton, a Sanskrit man and later professor at Yale, reading Hindu stories to Marshall at midnight, over crackers and cheese.

This episode in Marshall's life ended with three events: his graduation from medical school, service in World War I, and marriage to a Hopkins classmate, Berry Carroll, of Columbus, Ohio. She later made a career as psychiatrist to the Children's Court in Baltimore, while raising three children. Marshall was assigned, with the rank of captain, to the Chemical Warfare Service in Washington, where he worked until the end of the war.

In this unlikely setting, Marshall made a major, independent discovery—Homer W. Smith,[3] who was destined to be

[3] "Homer W. Smith," in *Biographical Memoirs of the National Academy of Sciences,* vol. 39 (New York: Columbia University Press for the National Academy of Sciences, 1967), pp. 445-70.

come the world leader in renal physiology. But in 1918, Smith was an enlisted man from Cripple Creek, Colorado, where he had sold vacuum cleaners. Marshall noticed that a light always burned late in the back laboratory; investigation one night revealed a tall skinny young man (not unlike the captain himself) who stuttered and had a passion for chemistry, literature, and music. If the captain was burdened with two doctorates, the sergeant had none at all, and Marshall resolved to repair this. Meanwhile, they published three excellent papers on mustard gas, prepared in the quantitative and chemical spirit that was to characterize the work of both in the years ahead. There was some effort to get Smith into medical school after the war, but he ended with the D.Sc. from the Johns Hopkins School of Hygiene and Public Health, where he worked on the pharmacology of arsenic. Seven years later, when both were involved in the study of renal physiology, they met again in Maine, where they collaborated briefly in a pioneering study of vertebrate evolution in light of the development of the glomerulus. They were neighbors, friends, antagonists, colleagues, and rivals at the Mount Desert Island Biological Laboratory for thirty-five years. To ask for more would be unrealistic, in view of their very different characters.

Back at Hopkins in early 1919, Marshall and his new family happily faced a gas-lit apartment on West Baltimore Street, a low budget, and some interesting decisions. He thought of going with a drug company as research director, but there were no offers and Abel was unsympathetic to this. He was offered a professorship in the Peking Union Medical School, with responsibility for the combined departments of physiological chemistry, physiology, and pharmacology (the curriculum of the twenty-first century?), but turned it down with little thought. Only at the close of his life did he speak sadly of this—quite out of character for him—as a great missed opportunity. He took the more conventional way and accepted the chair of pharmacology at Washington University in St. Louis. But in less than two years, too little time for lasting impressions on Marshall or Washington, the offer came from Hopkins, through Abel, to succeed Howell in the chair of physiology. His single, short journey outside Hopkins was over. His only reservation in returning was that somehow he had never taken a course in physiology, but he reasoned that he had never taken physiological chemistry or pharmacology either and had already taught both.

There must have been a very special quality in Marshall that brought him to this distinguished chair at age thirty-two and led Hopkins to pass over the more orthodox candidates. His papers up to that time were surely of good quality, but there were no outstanding contributions to physiology. Of course, his training was remarkable; it may be noted that he was not an M.D.-Ph.D. in the modern sense of a combined degree. He had two separate and significant tracks to a career in pharmacology: chemistry *and* medicine. His scholarship, vigor, singleness of purpose, and forthright honesty could not have failed to impress.

His bibliography from 1910 to 1920 charts his gradual transition from pure chemistry to physiology and pharmacology. The urea method had opened the door to these later studies, notably on the effects of adrenalectomy on the kidney (Section I). It was not long before this promise and these gifts came to fruition. In October 1922 he read to the Johns Hopkins Medical Society the "Proof of Secretion by the Convoluted Tubules": he had discovered active transport! Some details of this finding and the ensuing controversy are given below (Section II). In the published paper (1923) he seems to have leapt fifty years over the heads of his contemporaries to bring an entire new field into focus. It was to be another twenty-five years before it gathered the great impetus that it has now.

In January 1923, just as his great paper with Vickers on the proof of secretion was published, Marshall sailed to Europe—"carefree and happy"—with his family. In this memorable year he met most of the scientists who had been only names to him. "E. H. Starling of University College was particularly nice to me . . . *we* sat in his little office in front of a small fire . . . we discussed Physiology . . . my going into it without orthodox training. Starling said 'we need men bringing gifts, a new point of view.' I then felt that maybe I could do something."

The next few months were spent in Cambridge. "Here, I worked with Joseph Barcroft (with whom I had had much correspondence, during the war, on gas warfare). This was a delightful time. We took a furnished house—had a 'general' and excellent nurse for the children. I enjoyed dining in College at the high table. My wife says that if I had not been married, I should have pulled every string possible to become a Fellow of one of the Cambridge Colleges and live the delightful life there."

He went to Edinburgh to confront Arthur R. Cushny, whose book, *The Secretion of Urine,* and "modern theory" were widely accepted. The theory embraced filtration and reabsorption only, even though Bowman and Heidenhain had spoken of secretion much earlier. Cushny was unmoved by Marshall's visit, or his papers, and still rejected secretion in the 1926 edition of his book. It is clear that Cushny could not accept the idea that cells reabsorbed *and* secreted or that different substances could be handled in different ways.

Marshall spent "several delightful weeks" in the leading physiology laboratory of Europe—August Krogh's in Copenhagen. "This was his old laboratory, an old house—machine shop and 'diener's' quarters on the ground foor, laboratory on the second floor, and Krogh's living arrangements on the top floor. There were no cupboards in the laboratory and one could roam around and see all the apparatus Krogh had made and used on high shelves. Krogh used to say that if one could theorize and reason correctly for five or ten minutes in physiology without doing an experiment, one was very lucky."

He returned to Baltimore greatly invigorated and no longer worried about his lack of training. The secretion problem occupied and stimulated him. The Physiology Department at Hopkins under Marshall was small and appears to have taken social as well as midday nourishment from Abel's pharmacology group. Like Abel, Marshall gave relatively little time or energy to medical school teaching. Their idea of curricular reform was probably to move toward the smallest number of class hours possible; at one time Abel was running about eighty hours for the entire course. Both men made their influence felt by force of character and example in subtle ways. In those far-off and very active days, Marshall appeared intense and somewhat remote to his colleagues; he is said to have changed little between 1925 and 1955. He enjoyed reading, and in younger days, walking, but had no hobbies. He liked good company in small doses and looked forward to lunches and dinners at the Hamilton Street Club in downtown Baltimore with a small and select group of lawyers, writers, businessmen, and Hopkins professors. He was something of an ascetic; the life of an English don would have been eminently suitable for him.

His physical presence matched his cast of mind. Tall, thin, handsome, well-groomed, and formal, with a strident voice bearing the accent of Charleston, he was uninhibited in giving opinions or criticism of scientific peers. He was famous for his (well-placed) profanity, but this too was selective and emphatic. Three old-fashioned staples were used with such skill (damn, hell, and bastard) that he never needed or even hinted at the sexual expletives. He had the social graces of his "caste," but no social ambitions or "snobbery." He was a very private person and would not share personal or family adversities.

As he grew older, his photophobia and intermittent claudication worsened, so he did not enjoy the outdoors. As we shall see, his scientific world expanded, but his private intellectual world continued to be less than that of the usual academic. In the 1920s and after he seemed to revert to the isolated ways of his boyhood. He had little interest in literature, art, religion, music, sports, or philosophy; thus he remained at a distance from most of us. The key was science, and to realize that despite his austere and (to some) frightening presence, he was fundamentally kind, supportive, and optimistic about himself and his close colleagues.

In 1932 Abel retired and Marshall was appointed to his chair, which was renamed Pharmacology and Experimental Therapeutics. There were several reasons for this rather unusual academic shift; dominant were the desire "to be the old man's successor," and the feeling, strong in Marshall at age forty-three, that his destiny lay closer to chemistry than to physiology.

There followed an unusual time, for at the peak of his intellectual power and prestige Marshall idled, waiting for chance or observation to point to the future. He was finished with the kidney; secretion was proved and accepted by all, and it interested him no longer. In this time of waiting, he did make an important observation in a different area from all his other major work: that in respiratory depression anoxia provides a major ventilatory drive mediated through the sino-aortic mechanism (Section III). This had the mark of

Marshall's contributions—a fundamental discovery and a firm base for human medicine.

Marshall held his chair until his retirement in 1955. During the twenty-three years of his tenure, he passed through three separate phases of research, which are considered below (Sections IV-VI). In the first, the sulfonamides, he became a principal actor at the dawn of chemotherapy and achieved world renown. His multiphasic and powerful training was brought to bear on the chemical, physiological, and pathological aspects of the subject. He was fortunate—or prescient—in having talented and dedicated young colleagues in medical pharmacology (Windsor Cutting and Kendall Emerson), bacteriology (Harold White), chemistry (Calvin Bratton), and chemotherapy (John Litchfield). These men, with Morris Rosenfeld, were the nucleus of an advanced teaching and training department, responsibilities that were not forgotten even though the guiding passion was research. For thirty-five years he sat on the Advisory Board of the Johns Hopkins Medical School and fought, among other things, for freedom in curricular matters, free time for students, departmental autonomy, and the highest standards for faculty selection. He was one of the first to realize the importance of clinical pharmacology and began to train men in this area as far back as 1936. While he recognized that such men must be good doctors, he was convinced that they must also be able to take their place as scientists in the pharmacology departments; this is now the pattern of this "new" field. He believed that basic pharmacologists should also have medical training; after World War II he sought and received private funds for a program to create pharmacologists by "training medical men in chemistry, or training chemists in medicine." Each man's three-to-six-year program fit individual requirements; the products of the plan are now nearing the ends of their careers in university, governmental, or industrial pharmacology.

It was characteristic of Marshall that no matter how passionately he felt about a subject, when he dropped it, he never looked back. This was his pattern with the study of renal secretion, sulfonamides, malaria, and cinchoninic acids. He scorned some of his followers who were "looking for the second decimal place," but some of those added depth to what he began. It freed him from the intellectual torments that Homer Smith suffered as he followed and classified the twisting trails of renal physiology decade after decade.

Marshall retired in 1955, and in the next decade served a variety of useful functions. He became an industrial consultant and an advisor to the National Cancer Institute. For four years he spent a month each year teaching at the new University of Florida College of Medicine. He continued to radiate the same intangible love for his subject and hope for its future that was the legacy of Abel's lunch table, and reached back to Schmiedeberg and Buchheim. Each year he gave several lectures on the kidney[4] and on chemotherapy using an historical perspective and a spare, clear delivery. It pleased him to say that he had known all the significant men in renal physiology after Heidenhain. There was no other formal teaching, but a string of pleasant days, given to consideration of ideas, observation of experiments, and pronouncements—often spiced with his well-adjusted profanity—in the direction of poseurs and incompetents. He became interested in the teaching program and in several of the medical students and kept his appointment until he could see "my first class graduate."

There was a most significant part of Marshall's life that was all but concealed from his colleagues in Baltimore and

[4] Reprinted in *The Physiologist, 9*, no. 4 (Nov. 1966).

his confreres in Washington. This was his role at the Mount Desert Island Biological Laboratory, at Salsbury Cove, Maine, where he summered for forty years. After he completed his renal work in the early 1930s, he stopped working at the laboratory, but at his home, looking over the western aspect of Frenchman's Bay, he read, wrote, talked to friends, and gathered energy and enthusiasm for the fall ahead. He followed the work in the laboratory with his usual attention to detail and provided an astonishing amount and quality of guidance for generations of investigators. It often seemed that most of the ongoing work in the laboratory had been presaged, at least qualitatively, by his thinking in those rich, early years. Sometimes these early observations were in his memory, sometimes in his notes, and surprisingly often in the literature; this last to the profound discomfort of the hapless investigator who had rushed off to Maine without a session in the library. Marshall was a somewhat different man in Maine than in Baltimore. At Hopkins, he persisted in the old fashion of calling departmental colleagues and students by their last names, and he kept aloof from their family affairs, except for one fine party each year in his home. In Maine, he slipped naturally into first names and showed true affection and interest for the younger men, as well as their wives and children. He and Mrs. Marshall were favorites at parties, and gatherings at their home were greatly enjoyed.

Although administration was a distant second to research in his life, Marshall could be most effective when required. This was evident in his directorship of both departments in Baltimore and at two occasions in Maine: the first during the reestablishment of the laboratory after World War II (along with Homer Smith and Roy Forster) and the second when he was president of the laboratory from 1960 to 1964. In these roles he appeared ruthlessly efficient, as if he realized so well how these tasks ranked compared with science.

Marshall's character was perhaps susceptible to (using a favorite word of his) more clean-cut analysis than most, for he possessed absolute integrity. In the light of this undiluted quality, his occasional (or frequent, depending on the company) lapses from the amenities should have surprised or disturbed no one. He was profoundly loyal to all the men who had ever worked with him. He was the antithesis of the organization man. Self-reliance was the key; he was an Emerson-like hero. In a certain cast of the stern face, he even resembled the Concord philosopher. Marshall stayed in his laboratory and worked; he shunned all the ordinary apparatus of success. Some of these came to him anyway; he was president of the Pharmacology Society in 1942 and was editor of its journal from 1932 until 1937. He was a member of the American Philosophical Society and the American Association of Physicians and received the LL.D. from the College of Charleston in 1941. Astonishingly, in view of all that he pioneered and accomplished, he won no prize, received no awards or special honors. Was this related to his utter independence, his forthright manner, his lack of political ambition, his disregard for compromise, his avoidance of commitment to a single field?

Such a man will not come again, for the times that produced him have vanished. He was a bridge between the nineteenth and twentieth centuries for science, medicine, manners, the South, Johns Hopkins, and Maine. The pharmacologists now training for the twenty-first century might turn to his scientific life and principles as guides to their future.

I. Urea and Early Kidney Studies

When Marshall was in Abderhalden's laboratory in 1912, he became interested in enzymes and decided to work on urease, entirely as an academic exercise. Back in Baltimore he got some soybeans from an Italian market on Gay Street, and was amazed at the ease and quantitative specificity with which the extracts decomposed urea. He knew

> [T]hat C.S. Hudson had used the enzyme invertase to determine cane sugar, and I was suddenly struck with the idea that urease could be used to determine urea and that the procedure would be extremely simple. That evening I left the laboratory with L.G. Rowntree and walked with him across Monument Street to the old Johns Hopkins Club. I asked Rowntree if there were any need for a simple, quick method of determining urea in urine, blood and body fluids. He said he was at the time very much interested in determining urea in blood, but the known methods were very difficult, cumbersome and time consuming (Folin's micromethods had just been published and had not come into general use). I told Rowntree I had a very simple method or at least could devise one in a few weeks. He replied that if I did that I could have a Chair in Physiological Chemistry in a very few years.
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> This conversation with Rowntree was a sufficient stimulus to make me go at the matter with great enthusiasm. I believe I talked to Walter Jones about the matter, but he was not at all interested. In a week or 10 days the job was done and my first publication from the Medical School was being prepared. I recall that during this exciting week, I never left the laboratory until 1:00 to 3:00 a.m. It was quite worthwhile to be on the mountaintop for a short time.
>
> One of the greatest compliments I have ever had was paid me by Otto Folin at our first meeting. It was at the Philadelphia meeting of the Biochemical Society (Christmas 1913). When I was introduced to Folin, he said that when he had read my paper on the determination of urea by using urease, he thought it was perfect rot. He continued and said that he had tried my method and it was o.k., and he wanted to congratulate me. This from Folin!—only a few months after my first paper on Physiological Chemistry had been published.

Using this method, Marshall and Davis studied the distribution of urea in tissues and found that it was distributed evenly and rapidly, except for fat. They also measured its renal clearance. This was a signpost to much later work in kidney physiology and the pharmacology of sulfanilamide.

Two other significant studies were done in this decade, the first on the

[I]nfluence of the adrenals on the kidney. In it is stated, "The excretion of some substance by the adrenals which is necessary for normal kidney function, and the consequent interrelationship of the two glands serves as a very probable explanation of the results which are presented in this paper. Should it be found possible to prevent the renal changes in animals deprived of their adrenals by injection of adrenal extracts, it would support this hypothesis." About fifteen years later, others found it possible to prevent renal changes by injection of adrenal cortical hormone or extract. I recall clearly that in the first draft of this paper the adrenal cortex was implicated, but in revisions a more cautious statement was made.

Another series of observations resulted from my interest of the effect of the adrenals on the kidney. These were done with Kolls during the years 1915-1917. Essentially the important observation was that unilateral section of splanchnic nerve (or unilateral denervation at the renal pedicle) resulted in *anesthetized* dogs in the excretion of a much greater amount of water, chloride, bicarbonate, urea, sulfate and phosphate, but of the same amount of creatinine and phenol red by the denervated kidney as compared to the normal. In addition, compression of the renal artery could bring about a decrease of water and chloride with no change in the excretion of creatinine. The conclusion arrived at was that the changes were caused by vasodilation of renal vessels and increased blood flow to the kidney after section and that secretory action could not be attributed to the renal nerves. Some years later, others found that unilateral nerve section in the *unanesthetized d*og produced no change in the excretion of water or chloride. The interpretation then was that renal vessels were not normally in tone, but were put in tone under anesthesia. About a third of a century after our experiments on nerve section, I reconsidered the evidence. It seems that the above explanation cannot be correct as no change in the creatinine means no change in glomerular filtration rate and no change in phenol red, no change in renal blood flow. Others have now confirmed this by unilateral nerve section in anesthetized dogs. It looks as if the nerves are actually secretory in the sense of influencing reabsorption by the tubule—but why only in anesthetized and not unanesthetized animals? I have been puzzling for years to see how to attack this problem anew.

II. Renal Secretion

Marshall came to this work from the urea-adrenal-kidneyrenal nerve progression described above. Additionally and significantly, his friend Rowntree had twelve years earlier introduced phenol red as a test for renal function—50 percent of the dose was eliminated in the first hour. But since filtration rates were not known (until Rehberg, 1926), it was not apparent to anyone (except perhaps Marshall) that the phenol red data implied a second process.

The epoch-making paper with Vickers in 1923 contained two simple observations. Following intravenous injection in the dog:

- Phenol red was concentrated in the kidney to 12× that of plasma.
- The amount in the urine was greater than drug (unbound) filtered, even assuming that all the plasma was filtered, and using a very high value for renal blood flow—300 ml/min, a "safe maximum."

The authors concluded that "the problem would appear to be definitely settled, and satisfactory evidence would seem to exist that . . . filtration, reabsorption and secretion all play a role in the elimination of urine."

But it was not so easy—new ideas *are* dangerous and threatening. For one reason or another, these experiments were attacked by Cushny, Starling, A. N. Richards,[5] Rehberg, Oliver, and Ekehorn. If Marshall had any supporters (Homer Smith?), they are not in the record. A 1930 paper by Richards and Walker (*Journal of Biological Chemistry,* 87:479) records

[5] "Alfred Newton Richards," in *Biographical Memoirs of the National Academy of Sciences,* vol. 42 (New York: Columbia University Press for the National Academy of Sciences, 1971), pp. 271-318.

"an experiment technically one of the best," in which the ratio of phenol red in frog glomerular urine/plasma was 16. The astonishing comment follows: "It is impossible however to accept the result." There is no reference to Marshall in the paper.

His further work with Crane in 1924 supported secretion, but more interesting events lay ahead. In his wide reading, Marshall came across the fact that certain fish lack glomeruli; even better, his friend Alan Chesney (later dean and historian of Hopkins) told him that one of these, the goosefish (*Lophius piscatorius*) lived in the waters off Maine. Another, the toadfish (*Opsanus tau*) lived in the nearby Chesapeake.

Accordingly, Marshall went to Maine, and at the Mount Desert Island Biological Laboratory, with his student Allan Grafflin (later to become professor of anatomy at Hopkins), showed that phenol red was excreted in the goosefish without a glomerulus, along with many other low-molecular-weight substances, sugars excluded. He later found the same for the toadfish. The fight was nearing its close; the comparative method had paid off. But not before the redoubtable Richards cried out—after Marshall's paper at the XIII Physiological Congress in Boston—"he found the one beast in the world that will support his theories."

In 1931 Marshall wrote his final paper on the subject, bringing the 1923-1924 work into focus with new experiments in which glomerular filtration was measured. The supporting data from fish were cited and, most importantly (then and for the historians), there are three pages of rebuttal to his opponents of the past decade, the names a roll call of the renal establishment.

He finished this era with a masterly review (1934) of the comparative physiology of the kidney. Data on anatomy, physiology, and evolution were synthesized to show the un questioned power of the comparative approach, a view that was to be vindicated and amplified over the next fifty years.

III. Interlude: Respiratory and Cardiac Physiology and Pharmacology, 1932-1936

We hear best in Marshall's words an account of the rather unusual transition, when he walked upstairs from one Hopkins department to another and began a new academic life at age forty-five.

> In 1932 when I transferred from the Chair of Physiology to the Chair of Pharmacology and Experimental Therapeutics, the main problem which had interested me for more than a decade in regard to renal physiology—active secretion by the convoluted tubule—was settled. It was answered in the affirmative due mainly to the investigations of myself and coworkers. A great number of problems were now apparent and waiting for solution in regard to renal excretion. These were, however, mainly a quantitative study of the qualitative framework which had been established in the past quarter of a century. I deliberately decided not to engage on them, but to seek a new field of research. Some time was spent in 193233, in reorganizing the laboratory course in pharmacology. This led to an interest in respiratory stimulants which lasted until the fall of 1936. One important outcome of this work was the rediscovery of the depression of respiration by oxygen and an analysis of its mechanism.

Like so much else of Marshall's work, this study had direct clinical significance; it is a prime rule in accident rooms not to give oxygen to patients depressed with morphine, barbiturates, or allied drugs. With Rosenfeld he showed that oxygen removed the sino-aortic respiratory drive, which had recently been discovered by Heymans. In a typical vein, Marshall invokes the comparative method by recognizing that when the mammal is threatened with anoxemia, "it may adapt itself . . . to a primitive type of respiratory control (the sino-aortic rather than central) which is normal for lower vertebrates."

His interest in respiration was coupled to an exercise in physical chemistry in the work (again with Rosenfeld) on cyanohydrin equilibria. The goal was to take advantage of the widely varying rates of reactions between CN^- and aldehydes or ketones to find a compound to detoxify HCN, and also one that would slowly release CN^- as a nontoxic respiratory stimulant.

Belonging intellectually to this period, but done earlier, was the first measurement of cardiac output in an unanesthetized laboratory animal, using the Fick principle. His colleague Grollman extended this work and applied it to man, using acetylene rebreathing.

These papers made considerable advances in their several fields. They show breadth, economy of style, attention to important issues, and high technical competence in both physiology and chemistry. None of this work was done by technicians, and the entire budget of the Physiology Department was about $10,000 per year! Federal grants, of course, were unknown.

IV. Chemotherapy, 1936-1941

Looking back in 1952, Marshall wrote:

> It was amusing to be a free lance—no field of work—waiting for some accidental observation to point out a promising lead. . . . Then around 1936, I began to read of streptozon (Prontosil) and how it cured human cases of streptococcal and staphylococcal septicemia. Would that my friend Charles Hooper, Research Director of Winthrop (Metz or Bayer it was called) had not died a year or so before of pneumonia, which the new drug could have cured! Hooper used to come down two or three times a year and tell me of the new things cooking at the I. G. at Elberfeld, but always said, "That's what the German Johnnies say, I can't vouch for it. Do you want some of this new drug?" He would have brought Prontosil to me at least two years before I became interested in it.
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> In St. Louis in 1919-1920, I had become interested in the chemotherapy of bacterial infections. Nothing came of this interest except an unpub

> lished address before the St. Louis Section of the American Chemical Society. When successful bacterial chemotherapy arrived, I was ready for it; partly, I think on account of my meditation in St. Louis eighteen years before—and, I think some of my best contributions with my able collaborators have been in this field.
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> As I can see it, our significant contributions to bacterial chemotherapy were as follows. A simple, accurate and specific method was devised for the determination of sulfonamides in blood and tissues. Using this method, a quantitative study of the absorption, excretion, distribution and degradation of sulfanilamide was completed. These results had the effect of devising a rational basis of dosage—an initial loading dose and then a maintenance dose every four hours day and night. Soon dosage of the sulfonamides was based on blood concentrations rather than on number of grams administered by mouth. Quantitative methods were devised for studying the effectiveness of the sulfonamides on experimental bacterial infections in mice.
>
> As a result of our fundamental studies in bacterial chemotherapy, two new sulfonamide drugs were introduced by us into clinical use. In each instance, this has not only been the introduction of a new drug but of a new principle in bacterial chemotherapy which would be applicable to many other drugs. The chemical and pharmacological properties of the sodium salt of sulfapyridine were first described in a publication from my laboratory. This compound was introduced clinically for intravenous use and was the precursor of the use of sodium salts of other sulfonamides. Sulfanilylguanidine (sulfaguanidine) was prepared here. It had the unique properties of being fairly soluble in water, poorly absorbed from the gastrointestinal tract and highly active against various bacteria *in vitro.* This suggested that this drug or one having similar properties might prove useful in the treatment of bacterial infections confined to the intestinal tract, and not exhibit any toxicity on account of poor absorption. Both of these drugs were useful after their introduction; although, now utilizing the same principles, better ones have been found. Sulfaguanidine was sent to the Near East in 1941. Hamilton Fairley was much impressed by it. In "Medicine in Jungle Warfare" (Proc. Roy. Soc. Med., 38:195, 1945) he states that it was the considered opinion of many officers in the Australian Medical Corps that sulfaguanidine saved Port Moresby from the Japanese.

This description modestly recalls the facts, but not the excitement, triumphs, and significance of these five years.

Marshall and his collaborators were the architects of quantitative chemotherapy, and showed the direct line from laboratory to clinic. These observations and measurements were extraordinarily solid and precise and are still useful. Accurate chemistry was dominant in this thinking, and the standard was applied to bacteriological and pathological measurements. The blood level concept and practice were born here, along with the idea of drug distribution and decay. It is extraordinary how Marshall's work on urea—a quarter century earlier—prepared him for sulfanilamide.

The discovery of compounds more active and less toxic than sulfanilamide was anticipated by the quantitative data showing that cure of septicemia in mice was accomplished at different blood levels, depending on drug structures: sulfadiazine was sixty-four times as active as sulfanilamide in vitro, and eleven times in vivo. These techniques and conclusions had a profound effect on development of sulfonamides in the drug industry. Even the incidental findings, such as that sulfanilamide caused an alkaline urine and metabolic acidosis, were important. This was on the road to discovery of the carbonic anhydrase inhibitors.

V. Malaria, 1941-1946

World War II brought sulfonamide research to an end in Marshall's laboratory. Malaria was "the number one medical problem of the war" and attacked through a complex arrangement under the Emergency Management Act and the Office of Scientific Research and Development (OSRD), whose medical arm was the Committee on Medical Research (CMR). This interlocked with advisory committees of the National Research Council and the National Academy of Sciences. But it all worked!

The CMR was headed by A. Newton Richards, the distin guished professor of pharmacology at the University of Pennsylvania, and Marshall's great antagonist in the battle about renal secretion. There is every evidence that they worked beautifully together in the war years. The National Research Council held an organizing conference through their Division of Medical Sciences in July 1941, at which the following occurred: "After extensive discussion of possible lines of research, various reports were requested. Of those submitted, that of Dr. E. K. Marshall, Jr., foreshadowed what was to become the principal line of work. Special researches were immediately begun by several of the conferees."

Thus it transpired that Marshall became a prime mover in the government malaria program, which expanded enormously in the years to come and involved universities, industries, and exchange of information with our allies. Committees, panels, and conferences in Washington multiplied to an astonishing degree, covering the territory from basic parasitology to biochemistry to clinical medicine. They were headed by the most eminent (often also the best) men in the field. Baltimore became a secondary hub of the vast enterprise with the establishment of the Survey of Anti-Malarial Drugs, headed by Dr. Frederick Wiselogle.

This office arranged the testing of 13,000 substances received from hundreds of sources, and codified their activity over a four-year period. The work was accomplished by a devoted staff; there was no computer. Marshall was a consultant to this unit also, which was about 200 feet from the Pharmacology Department. Investigators who thought they (or their drugs, which came to the same thing) had been slighted in the program referred toughly to "the high command in Baltimore."

But unlike most of his colleagues at these councils in the two cities, Marshall also had a very busy laboratory run ning, in which chemicals were screened for their activity in the avian malarias. New methods of analyses were worked out; structure-action relations were analyzed; distribution of drugs in the body was measured. He was unsparing of himself and his younger colleagues; it was a crucial wartime effort, seven days a week with no holidays.

Rigorous quantification of the experimental avian infections was badly needed. Marshall was often at odds with the parasitologists; on some he vented ire laced with his usual expletives. Typically the chemists were his allies and friends. He relied heavily on Kenneth Blanchard, a polymath in all branches of chemistry who was to join Marshall's department after the war. It was all exciting and full of high idealism, but scientifically frustrating. Marshall did not like the idea of bird malaria—it represented not only an alien class but organisms alien to human malaria. But it was, at the time, the best they could do. Marshall's close scientific friend, James A. Shannon (later the enormously successful director of the National Institutes of Health as it became a major force in world research) was head of the Research Group at Goldwater Hospital in New York. These two had also an earlier bond, renal physiology. This curious link of topics was also common to the younger colleagues in the program: Berliner, Taggart, and Earle. It was Shannon, along with Alf Alving at the University of Chicago, who defined the role of the 4-amino and 8-amino quinolines in the human malarias. These men, along with Rotert Loeb, Lowell Coggeshall, Leon Schmidt, and many others, were dedicated to the closest attention to data obtained from the laboratory and human volunteers—it was a triumph of true cooperation. It was frustrating that there was no time to work out the many problems that arose almost daily. Yet scientific issues were not ignored, and one of great theoretical and practical importance emerged and captured Marshall's imagination. Should drugs be given to maintain a constant blood concentration in infectious disease? Marshall, having introduced this concept for the antibacterial sulfonamides, was nevertheless convinced that this did not apply to the quinolines in malaria, which had a different pattern of distribution and mechanism of action. It was typical that he was flexible, realistic, and outspoken in such an argument.

The situation was indeed critical. Supplies of quinine were cut off, and atabrine (quinacrine) turned the skin yellow and was the target of many unfounded rumors, typical of wartime, so the troops would not take it. Furthermore, it did not seem to be working well until the Shannon group showed that a loading dose was essential—an early triumph of what is now rather elaborately called pharmacokinetics. This surely reflected Marshall's principle of a loading dose for sulfonamides, now curiously forgotten in the use of antiinfective agents. Thus quinacrine was found superior to quinine and became the most important single contribution to the control of malaria in the Pacific War.

Meanwhile, the search for new agents continued. The discovery of chloroquine was a major accomplishment, resulting from study of some 200 4-aminoquinolines. This class suppresses *vivax* malaria and is a radical cure for *falciparum* malaria. A main goal of the program was to find a radical cure for *vivax* malaria, which was made particularly difficult by lack of basic knowledge about the exoerythrocytic cycle in man, and lack of a proper model in birds. Nevertheless, following an early German lead, intensive screening and pharmacology was done on several hundred 8-aminoquinolines, leading to the discovery of primaquine, still the chief drug for cure of this disease.

In April 1946 at a Symposium in Wartime Pharmacology at the Federation of American Societies for Experimental Biology, Marshall said farewell to this vexing and exciting period. It was about his average time for a subject, no matter how passionately pursued. He dropped malaria and never looked back.

VI. The Last Decade: Methods, Cinchoninic Acids, Ethanol

In the fall of 1946 the past had been cleared away, and Marshall at fifty-seven faced fresh problems with new vigor. He was interested in rational dosage schedules for chemotherapeutic agents, and still rather enraged that his dictum of constant blood levels for sulfonamides unthinkingly had been transferred to the antimalarials and to penicillin. He thought (but Jim Shannon did not agree) that he had demolished the malaria argument, and now with Gordon Zubrod he turned to penicillin and showed that a moderate dose interval does not weaken the effect, and that cure is a function of total dose. This was related to its lytic or bacteriocidal action and still guides use of the drug. Marshall's heart was still in chemistry; he would have loved to develop an analytical method for penicillin, but failed. He did succeed with streptomycin, however, and went on to a full-dress study of its pharmacology.

In the course of using a selection of cinchoninic acids as fluorescent ligands for antibiotics, Marshall noticed their anti-diuretic effect. (I had come to his laboratory in 1946 as a medical student and trainee: graciously he had allowed me two years to study and finish work of my own on arsenic and antimony left over from war work on tropical diseases.) He now asked me to find if the cinchoninic acids worked through the posterior pituitary or directly on the kidney. To his great disappointment (since no drug of the latter type was known), it was the former. Nevertheless, it introduced us to the hypothalamus and then the anterior pituitary; it was the dawn of their relations in the work of Geoffrey Harris of London. Now came a shared excitement, still felt a third of a century later; some of the cinchoninic acids synthesized by Blanchard (a clean-cut structure-action relation was found) stimulated the pituitary-adrenal system! It was also the year of ACTH (adrenocorticotropic hormone) and we had (possibly) a new drug for the rheumatoid and other connective tissue diseases.

Once again, it was as if Marshall had been waiting for this, and he sprang into action, mobilizing his clinical associates and students. He and I took an unusual step—a train trip out to Chicago—to attend the first ACTH conference. But the clinical trials of 3-OH-2-phenylcinchoninic acid were disappointing: there was some beneficial effect, but also enough toxicity to end the study. Marshall had no further interest in the theoretical aspect. He left it to his plodding student to section the hypothalamus of the rats (under the tutelage of David Bodian) with pituitary lesions and to guess that these drugs might be working at the level of the paraventricular nuclei.

In the last years, Marshall's luck and judgment continued to hold. He continued with talented and devoted young colleagues as he had for forty years, particularly Albert Owens, who was also his physician. He still selected important and unsolved problems. Marshall was always intrigued by ethanol; he now showed that the classical zero-order kinetics of decay from blood following the usual size dose changes to first order when the dose is small or at the tail-end of the curve. In the first case the systems for drug metabolism are saturated; in the second they are not. Finally, as a result of his own hospitalization and taking of chloral hydrate, he became interested in its metabolism, neglected since its introduction as the first synthetic drug 100 years before. The chief findings were that a very large fraction is oxidized to trichloracetate; the smaller amount reduced to trichlorethanol is responsible for the narcotic effect. Trichlorethanol and ethanol share common oxidative pathways, to aldehyde and acid. The data suggested that trichlorethanol itself is a more potent and reliable hypnotic than chloral hydrate; it is now marketed as trichlofos sodium, the salt of the phosphate ester of trichlorethanol.

In this last (1955) work, we find again the inspiring characteristic of Marshall's work since 1912: the synthesis of the basic sciences in the service of medicine.

I thank Drs. Robert W. Berliner and C. Gordon Zubrod for their review of this memoir, particularly their help with the malaria story. I am grateful to Dr. J. Wendell Burger for some personal insights into Dr. Marshall's character. The passages quoted in the text are from Dr. Marshall's manuscript memoir, written at various times between 1942 and 1955.

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Stanford Moore

September 4, 1913-August 23, 1982

By Emil L. Smith and C. H. W. Hirs

Stanford Moore, Nobel Laureate in Chemistry in 1972, was born in Chicago, Illinois, when his father, John Howard Moore, was a student at the. University of Chicago Law School (J.D., 1917). His father was a graduate of Westminster College in New Wilmington, Pennsylvania, and his mother (née Ruth Fowler) of Stanford University. His parents were married in 1907 and had met at Stanford. It is alleged that this was the origin of the given name of the son.

The education of Stanford Moore began at the age of four at a progressive school in Winnetka, Illinois. When he was six, his father moved to a teaching position in the Law School at the University of Florida; he later accepted a position with Mercer University in Macon, Georgia, where the boy attended the local public schools. In 1924 J. H. Moore became professor of law at Vanderbilt University, where he was to serve on the faculty until his retirement in 1949; he died in 1966 at the age of eighty-five.

In Nashville, Stanford was a student at Peabody Demonstration School, which was operated by the George Peabody College for Teachers. He was an outstanding student for the seven years he attended the school, and he maintained a straight A average. Initially his interests were in English and science, and he was fortunate to encounter a teacher, R. O.

Beauchamp, who aroused his interest in chemistry. In 1931 he entered the College of Letters and Science of Vanderbilt University, considering a career in aeronautical engineering or chemistry. Unable to decide between the two fields, he pursued both the liberal arts and the basic subjects of the engineering curriculum during his first two years. In his third year at Vanderbilt, he came under the influence of Arthur William Ingersoll, who Stan later credited as stimulating his enthusiasm for organic chemistry and molecular structure. As a result, Stan changed his major subject to chemistry and graduated from Vanderbilt in 1935 with the bachelor of arts degree, summa cum laude, and was a recipient of the Founder's Medal as the outstanding student in his class. Surprisingly, the Stanford Moore who tended to lead a rather reclusive life in later years and generally avoided nonscientific social activities was a socialite as an undergraduate. He was active in several clubs, president of his fraternity, and, as president of the student council, the organizer of the senior prom.

In the autumn of 1935, Stan entered the Graduate School of The University of Wisconsin with the support of a fellowship from the Wisconsin Alumni Research Foundation. Although he chose to major in organic chemistry, he elected to do his thesis research with Professor Karl Paul Link, a member of the Biochemistry Department at Madison. It was significant for Stan's later development that Link had spent some time—years earlier—in Graz, Austria, in the laboratory of Fritz Pregl, one of the pioneers in the development of microanalytical methods. Link required all of his students to master these microanalytical techniques. In retrospect it is apparent that Stan's background, first in engineering and then in microanalysis, had an important effect on his later collaborative work with William H. Stein in the development of important new methods of automated analysis.

Moore's thesis research (ultimately summarized in five papers) included a study of the reaction of *o*-phenylenediamine with various monosaccharides. The products of this reaction, a series of benzimidazoles, proved to be readily isolated as stable crystalline solids that lent themselves well to the identification of various monosaccharides. These derivatives continue to be so used.

With the completion of his doctoral dissertation (1939), it was clear that Stan's future was to be in biochemistry. Two attractive options were available: a four-year fellowship at Harvard Medical School and an invitation to become a research assistant in the laboratory of Max Bergmann at the Rockefeller Institute for Medical Research in New York. Bergmann had been one of Emil Fischer's outstanding collaborators, and he had continued after the First World War to make a series of notable contributions in protein and carbohydrate chemistry while at the Kaiser Wilhelm Institut fuer Lederforschung in Dresden. With the rise of the Nazi dictatorship in the early 1930s, Bergmann accepted an offer to join the staff of the Rockefeller Institute, and moved there in 1934. It was through Link's friendship with Bergmann that the invitation for Stanford to go to Rockefeller was presented. As Stan commented later, "The question of whether it would be wiser to go on to medical school or to enter immediately into chemical research was resolved in favor of the latter."

When Stanjoined Bergmann's group, he became involved in one of the principal concerns of the laboratory, the structural chemistry of proteins. Of particular interest was the development of methods for the gravimetric estimation of the amino acid composition of proteins by utilizing selective precipitants. This approach had been given new impetus two years earlier when William H. Stein joined the laboratory and showed that aromatic sulfonic acids possess desirable prop erties in that regard. Whereas earlier workers had concentrated on precipitants that formed highly insoluble salts with amino acids (the prototype was flavianic acid, used by A. Kossel and R. E. Gross in 1924 for the isolation of arginine), the research in Bergmann's group emphasized the fact that salts of extremely low solubility were unnecessary, provided that the precipitant was selective and that the solubility products of the salts were estimated and corrections were applied for the quantity of amino acid remaining in solution.

Bergmann suggested that Stan join forces with Bill Stein to develop the solubility product approach as a routine method for amino acids. There was no way at the time to realize that a scientific collaboration had been initiated that would last for the remainder of the lifetimes of these two young scientists, certainly one of the longest and most fruitful collaborations in the history of all science.

Bill and Stan concentrated their initial efforts on two sulfonic acid reagents—5-nitronaphthalene-2-sulfonic acid for glycine and 2-bromotoluene-5-sulfonic acid for leucine—and showed that good results could be obtained with hydrolysates of egg albumin and silk fibroin. But with the work well under way, the research had to be terminated when the country suddenly found itself at war at the end of 1941.

With the advent of the war, Bergmann's laboratory undertook research for the Office of Scientific Research and Development (OSRD). Their specific mission was to investigate the physiological actions of vesicant war gases (mustard gas, nitrogen mustards) at the molecular level, with the hope of developing therapeutic agents that might be helpful in overcoming the effects of these compounds on the human body. The rationale for the work was that adequate defensive measures for preventing the effects of these toxic compounds, as well as the retaliatory capabilities of the United States and its allies, would inhibit the use of chemical warfare agents. Fortunately, these agents were not used during the war.

While Bill Stein remained with Bergmann and his colleagues to conduct the research in New York, Stan enlisted in 1942 to serve as a technical aide on the National Defense Research Committee of OSRD to coordinate university and industrial efforts on the biological actions of chemical warfare agents. His base was in Washington, but he made frequent trips to Dumbarton Oaks, where the National Defense Research Committee had its offices. Later (1944), Stan was appointed to the Project Coordination Staff of the Chemical Warfare Service, which was directed by William A. Noyes, Jr. The experiences of the service were summarized in a volume published after the war, to which Stan (with W. R. Kirner) contributed an article on the physiological mechanisms of action of chemical warfare agents. When the war ended, Stan was in Hawaii with the Operational Research Section of the Chemical Warfare Service.

Max Bergmann died of cancer in 1944 at age fifty-eight. The war work of the laboratory, however, was continued by his associates until the end of hostilities in 1945. At that time most of them moved elsewhere, and the department that Bergmann had organized was dissolved. At this juncture Herbert Gasser, then director of the Rockefeller Institute, had the wisdom to offer Bill Stein and Stan Moore space in the former Bergmann department, along with the opportunity—on a trial basis—to continue the work on amino acid analysis that they had initiated before the war.

In the interim the collaborative efforts of A. J. P. Martin and R. L. M. Synge and their associates in England produced novel fractionation techniques, notably partition chromatography. Bill and Stan were aware of this research, although during the war journals arrived from England rather irregularly. When their collaboration was renewed in 1945, they decided to explore the possibilities afforded by partition chromatography for determining the amino acid compositions of proteins. Their work took place in parallel with that of Lyman C. Craig, whose laboratory was located on the same floor and who had been exploring the potential of countercurrent distribution in the fractionation of peptide antibiotics.

As a starting point, Bill and Stan decided to develop a column chromatographic method based on work by S. R. Elsden and Synge (1941), who had demonstrated that useful separations of amino acids and peptides could be obtained with potato starch as the matrix and various two-phase mixtures of the lower alcohols, such as n-butanol, with aqueous organic acids as the eluant. To render the procedure quantitative, a suitable micro method for the determination of amino acids in the column effluent was required. To this end, Bill and Stan studied the ninhydrin reaction, known since its discovery in 1911 to result in the formation of colored products from all amino acids. They discovered that reproducible yields of the product could be obtained when the reaction was conducted in the presence of a reducing agent, initially stannous chloride.

To monitor the progress of the separations effected on the starch columns, the eluate was collected in small fractions of equal volume; these were treated with ninhydrin under reducing conditions, and the colored products were measured spectrophotometrically. The concentrations of colored product in each fraction were plotted against fraction number to obtain a so-called effluent-concentration curve. The area under each peak on such curves gave the amount of amino acid in the sample.

Initially the fractions were collected manually, but the labor involved quickly led to the design and construction of an instrument in which each drop of effluent from a column was made to interrupt a light beam incident on a photocell, thereby incrementing a counter. Drops were collected into spectrophotometer tubes. When a predetermined number of drops had been collected, a turntable advanced to bring a new tube into line. Although this instrument was not the first fraction collector to be described, it became the prototype for the commercial instruments that soon appeared thereafter in laboratories around the world.

With these developments it became possible to refine the chromatographic procedures themselves. In the methods ultimately described in 1949, three runs were required to determine all the amino acids in a protein hydrolysate. Bill and Stan described the application of the method to the determination of the compositions of beta-lactoglobulin and serum albumin. The three runs required a total of less than 5 milligrams of protein, with a standard error of less than 5 percent, a remarkable achievement at the time. Recognizing the impact this methodology would have in biochemistry, Bill and Stan went to considerable lengths to provide detailed descriptions of all the necessary steps for the successful application of their procedures in other laboratories. Most of this information was circulated in the form of preprints, well in advance of publication, to anyone desiring access to it. They were to repeat this service to the biochemical community many times in subsequent years as improvements in methodology were made.

In 1949 Herbert Gasser decided that Moore and Stein had demonstrated the competence as independent investigators that he had hoped they would develop. As a result the research budget for their laboratory was increased substantially. This permitted the recruitment of postdoctoral associates and additional technical assistants over the next several years, increasing the scope of the research. Space limitations preclude mention of the numerous students and postdoc toral associates who from this time onward were affiliated with the Moore and Stein laboratory. The interested reader is referred to the bibliography of this memoir for further information.

Although the starch column procedures represented a breakthrough of the utmost significance in protein chemistry, there were some limitations. First, there was the slow flowrate of the columns (one complete analysis of a protein hydrolysate required two weeks). Moreover, a fresh column had to be prepared for each run, and the separations were sensitive to the presence of salts in the sample. Bill and Stan therefore decided to investigate ion-exchange chromatography on sulfonated polystyrene resins as an alternative. They were encouraged by the success attained by S. Miles Partridge in England in the preparative-scale fractionation of amino acids by displacement chromatography on such resins.

Effective separations of all the amino acids in a protein hydrolysate in a single run were quickly obtained by elution with sodium citrate and acetate buffers of increasing pH and concentration at various temperatures, but a great deal of painstaking effort was required to standardize the performance of the columns. The problems were finally overcome when more reproducible resins became available. The successful development of the ion-exchange methodology not only allowed a considerable reduction in the time required for analysis of a protein hydrolysate, but for the first time it permitted reliable analyses of the amino acid content of various physiological fluids: urine, plasma, and protein-free extracts of tissues. These methods also resulted in the discovery and estimation of new components of these fluids.

Concurrently, the potential of ion-exchange chromatography for the separation of peptides and proteins was developed. It was soon found that certain stable, basic proteins—notably bovine pancreatic ribonuclease and chymotrypsino gen, and egg-white lysozyme—could be chromatographed effectively on IRC-50, a polymethacrylic acid resin. The elution of these proteins from the exchanger occurred in a predictable way in response to changes in pH and ionic strength. Somewhat later the successful fractionation of histones from calf thymus was achieved.

Encouraged by these successes, in 1953 Bill and Stan felt the time had arrived to embark on the structural analysis of a protein. It should be recognized that when the events recorded here were taking place, little more was known about the fundamental chemical structure of proteins than in Emil Fischer's time. It was only in 1948, when Frederick Sanger and his students began elucidating the primary structures of the polypeptide chains in insulin, that convincing evidence was at hand to demonstrate that proteins have unique amino acid sequences. Sanger's success with the insulin chains (21 and 30 residues) showed that the structure of a polypeptide could be deduced from the sequences of smaller peptides derived from it by selective, partial hydrolysis with weak acid or enzymes. Sanger's work also emphasized the problems of separating the complex mixtures of peptides that resulted from such hydrolysis. It was clear, however, that the determination of the primary structure of longer polypeptide chains would be difficult, if not impossible, by the methods used with the relatively small polypeptide chains of insulin.

With this background the choice of a protein became a critical decision for Moore and Stein. They elected to investigate the small enzyme ribonuclease, which they had already studied, arguing that knowledge of its structure would almost certainly aid in understanding its enzymic activity. Their work was performed in parallel with that of Christian B. Anfinsen and his colleagues in Bethesda, but the approaches of the two laboratories were different and they functioned on a collaborative rather than competitive basis. Indeed, neither

Bill nor Stan relished competition in science, although they recognized its value in expediting progress.

The investigation of the structure of ribonuclease started with a sample of the oxidized protein that was hydrolyzed selectively with the proteolytic enzyme trypsin. The resulting mixture of peptides was separated by ion-exchange chromatography on a column of sulfonated polystyrene resin by procedures similar to those used earlier for the separation of amino acids. The compositions of these peptides showed that the entire sequence (124 residues) of ribonuclease was represented. To establish how these peptides originated, the oxidized enzyme was next hydrolyzed with chymotrypsin, a protease with a selectivity different from that of trypsin, to produce a second set of peptides that were likewise separated on sulfonated polystyrene. From the known selectivities of trypsin and chymotrypsin, largely elucidated years earlier by Bergmann and his colleagues, the order of the tryptic peptides in the polypeptide chain was deduced. Confirmation was obtained from another set of peptides isolated from a peptic hydrolysate.

As this work proceeded, it was evident that progress was limited by the rate at which amino acid analyses could be performed. With the manual methods then in use, a single run required almost three days and several hundred spectrophotometer readings. Thus, work was initiated in 1956 to develop automated amino acid analysis. It was only after extensive refinement of the instrumentation that the method was published in 1958. With the resins then available, the analysis time was shortened to twenty-four hours and the sensitivity permitted runs on as little as 0.5 micromole. Subsequent developments have resulted in a reduction for the time of analysis to an average of about an hour and increased sensitivity by two orders of magnitude. The benefits to our knowledge of protein chemistry that became possible with the use of the Moore and Stein analyzer have been incalculable.

It is a testimonial to the standards of excellence of Moore and Stein that both the original fraction collector they devised and the automated amino acid analyzer described in 1958 remain in perfect working order, the former now in the museum at Caspary Hall, the latter still in the laboratory in which it was assembled at the Rockefeller University.

The complete covalent structure of ribonuclease was published in 1963, the first such structure for an enzyme. It was then decided to investigate the inactivation of ribonuclease by iodoacetate. In a series of investigations in which the progress of the reaction at different pH values was followed by amino acid analysis, they showed that inactivation at pH 5 was the result of carboxymethylation on nitrogen-1 of histidine-1 19 or nitrogen-3 of histidine-12, but not at both sites of the same ribonuclease molecule. Inactivation at lower pH values was found to be caused by reactions with methionine residues; at higher pH by reaction with lysine-41. In this way they were able to propose that histidines-12 and -119 are close to one another in the active site of ribonuclease. This proposal, which proved to be central in further investigations of ribonuclease, was subsequently confirmed in other laboratories by x-ray analysis, and it permitted interpretation of kinetic studies and nuclear magnetic resonance work elsewhere that led ultimately to a detailed explanation of the mechanism of action for the enzyme.

The work on ribonuclease was recognized by the awarding of the Nobel Prize in Chemistry for 1972 to Moore and Stein, jointly with Anfinsen. Other honors are listed elsewhere.

As the number of students and postdoctoral associates in the Moore-Stein laboratory expanded, the scope of the work enlarged correspondingly. Here we can only mention some of these investigations. These included the determination of the amino acid sequence of pancreatic deoxyribonuclease; investigation of the reaction of cyanate ion with proteins; structural studies with pepsin; the mechanism of action and structure of streptococcal proteinase; studies of the sequence and the active site of ribonuclease T1; the isolation of 2',3'cyclic nucleotide 3'-phosphohydrolase and its inhibitor from brain; studies on ribonuclease inhibitors; and many studies on modifications of pancreatic ribonuclease. In addition there were many investigations by younger associates and colleagues in their laboratory at the Rockefeller University on which the names of Moore or Stein do not appear.

From the foregoing and the appended bibliography, it is evident that the collaboration of Moore and Stein continued at the Rockefeller even after Bill Stein suffered a crippling paralysis in 1969. The biographical memoir on William Stein was the last publication submitted by Stanford Moore, just a month before his own death. In this memoir Moore comments, "During the early years of our cooperation, Stein and I worked out a system of collaboration which lasted for a lifetime. Stein combined an inventive mind and a deep dedication to science with great generosity. Over a period of forty years, we approached problems with somewhat different perspectives and then focused our thoughts on the common aim. If I did not think of something he was likely to, and vice versa, and this process of frequent interchange of ideas accelerated progress in research. It also helped in writing papers; I never drafted a text that Stein could not improve." We include this quote to indicate that we cannot attempt to judge which of the two was responsible for specific contributions to their joint accomplishments and those with their collaborators.

With the exception of the war years (1942-45), Moore was away from the Rockefeller for only a year, beginning in 1950. He spent half the time in Brussels, Belgium, setting up a laboratory devoted to amino acid analysis, and half the year in Cambridge, England, sharing a laboratory with Frederick Sanger when the work on the amino acid sequence of insulin was proceeding. Stan felt that this year in Europe was important both in his development as a scientist and in furthering his interest and collaboration in international scientific activities.

Stan served the community of biochemists well as an editor, as an officer of the American Society of Biological Chemists, and as chairman of the Organizing Committee for the International Congress of Biochemistry held in New York in 1964. The Congress was a memorable event in its organization of scientific presentations and its gracious and efficient hospitality. During this Congress Stan began the custom of inviting eight to ten guests for breakfast and lunch in his suite each day, so that fellow scientists could meet in intimate surroundings with their colleagues. He continued this practice for another fifteen years at international congresses and at the annual meetings of the American Society of Biological Chemists. Only his declining health forced a termination of this custom.

Stanford Moore was intensively and single-mindedly devoted to science. He consciously avoided activities that did not involve science and scientists. This lifelong bachelor was an early riser, and he was at his desk or in the laboratory throughout the day and on the weekends. Yet he was a gracious and hospitable host to his scientific friends and associates. Nevertheless, he was a conscientious citizen, and for about a year in the 1960s he served on a grand jury in New York hearing testimony on the activities of organized crime (the famous case of the Cosa Nostra). Typical of Stan, after a day of hearings, he would spend the evenings and the weekends in the laboratory to keep up with his scientific work.

It should be noted that none of the methods and instruments designed by Moore and Stein was patented. Personal profit was far from their minds. Indeed, Stan Moore had little interest in personal possessions; his small office and bachelor apartment had the minimally effective furnishings. Stan's obsessive neatness both of person and surroundings was legendary.

In the last two years of life, as his health deteriorated, Stan lived with the awareness of progressive nerve and muscle degeneration from amyotrophic lateral sclerosis. He kept this knowledge to himself as long as possible, but characteristically finished his writing obligations, disposed of many of his personal possessions, and left his papers and files in a meticulously organized condition. He died in his apartment, a short distance from his beloved laboratory at the Rockefeller University where he had spent so many fruitful and satisfying years.

Stan's loyalty to the Rockefeller University and his devotion to biochemistry were reflected in his will, in which he bequeathed his estate "to be used as endowment toward the salary or research expenses or both of an investigator in the field of biochemistry." As Stan stated in a letter to President Joshua Lederberg of the University, which was delivered after his death, "I would like (to the best of my modest ability) to help a young scholar have the same opportunity that I had."

Although Stan had requested that no memorial service be held, his friends and former associates felt that he should be honored. This was done at "A Symposium on Protein Chemistry in Tribute to Stanford Moore" at the Rockefeller University on November 4, 1983, at which former members of the Moore-Stein laboratory presented their latest studies and others spoke on the contributions of these two memorable individuals.

In preparing this memoir we have used the brief autobiographical notes deposited in the Academy files by Stanford Moore. We are indebted to Joshua Lederberg, James M. Manning, and C. R. Park for their helpful suggestions and for supplying some information.

AWARDS, HONORS, AND DISTINCTIONS

Chronology

1913	Born, September 4, Chicago, Illinois
1935	B.A., Summa cum laude, Vanderbilt University
1938	Ph.D., University of Wisconsin
1939-42	Rockefeller Institute for Medical Research (later the Rockefeller University): Assistant, 1939-42; Associate, 1942, 1945-49
1942-45	National Defense Research Committee, Office of Scientific Research and Development (later the Chemical Warfare Service)
1947-49	Chairman, Panel on Proteins, Committee on Growth, National Research Council
1949-52	Associate Member, The Rockefeller Institute
1952-82	Professor and Member, The Rockefeller University
1950	Francqui Chair, University of Brussels
1950-60	Editorial Board, *Journal of Biological Chemistry*
1953-57	Secretary, Commission on Proteins, International Union of Pure and Applied Chemistry
1961-64	Chairman, Organizing Committee, Sixth International Congress of Biochemistry, New York (1964)
1968	Visiting Professor, Health Sciences, Vanderbilt University School of Medicine
1970-71	President, Federation of the American Societies for Experimental Biology
1974-82	Trustee, Vanderbilt University
1982	Died, August 23, New York City

Selected Memberships

American Society of Biological Chemists (Treasurer, 1956-59; President, 1966-67)

American Chemical Society

Belgian Biochemical Society (Honorary Member)

Belgian Royal Academy of Medicine (Foreign Correspondent)

Biochemical Society (Great Britain)

National Academy of Sciences (Elected, 1960; Chairman, Section of Biochemistry, 1969-72)

American Academy of Arts and Sciences (Elected, 1960) Harvey Society

Honorary Degrees

Docteur *honoris causa,* Faculty of Medicine, University of Brussels, 1954
Docteur *honoris causa,* University of Paris, 1964
Dr.Sc., University of Wisconsin, 1974

Awards Shared with William H. Stein

American Chemical Society Award in Chromatography and Electrophoresis, 1964
Richards Medal, American Chemical Society, 1972
Linderstrøm-Lang Medal, Copenhagen, 1972
Nobel Prize in Chemistry, 1972

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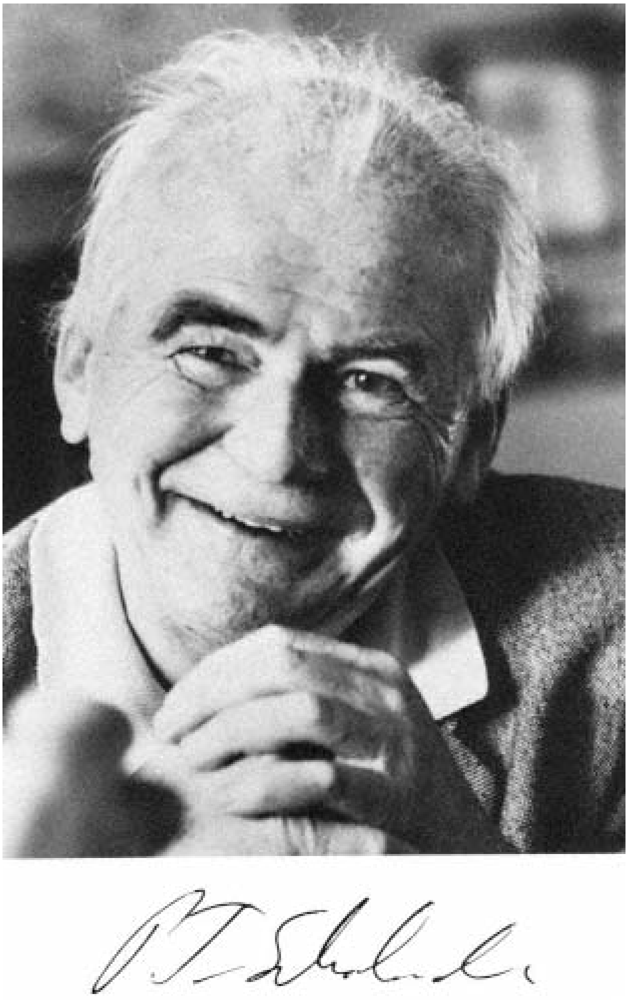

Per Fredrik Thorkelsson Scholander

November 29, 1905-June 13, 1980

By Knut Schmidt-Nielsen

The most impressive aspect of Scholander's scientific life is his versatility as a biologist and his ability to make significant contributions in a broad range of fields. He was first of all a physiologist, but his work always signified a fresh approach to broader biological problems and principles. He had an ability to ask the right questions, to conceive of simple experiments and design the necessary equipment, to utilize novel approaches, and to present simple and logical answers to important questions. As a person he was warm and enthusiastic, generous and kind, and utterly unconcerned with the hassles of form and bureaucracy.

Scholander was born in Sweden and grew up in a family of talent and culture. His father was an engineer; his mother, Norwegian born, was an accomplished professional pianist; his grandfather was a prominent architect, as well as a writer and musician, and a professor at the Royal Academy of Arts. Scholander has related that, as a small boy, he crawled underneath his mother's Steinway grand piano when she practiced, smothered by the waves of emotion in the music. He himself became an accomplished violinist, which I first discovered when late one night I found him playing in the laboratory to a record that omitted the first violin.

Because of the divorce of his parents, Scholander moved to Norway, where in 1924 he matriculated at the Faculty of Medicine of the University of Oslo (then Christiania). At that time medical students started with Latin, philosophy, chemistry, and physics, whereupon followed years of preclinical work in anatomy, physiology, and so on. Although Scholander was terribly bored by much of this, he continued his medical studies and completed his medical education in 1932, within the normal time span. Scholander has described how his textbooks were always full of marginal notes about explanations he did not believe, but his grades did not reflect his brilliance. There was a rumor among the university students—probably untrue—that he completed his final medical examinations with the unique distinction of having obtained the lowest grades ever given a passing student.

Scholander's years as a medical student became significant in a very different and unexpected way. Walking home from the University one day, bored with tedious coursework, he picked some lichens off the trees along the street. At home he found a flora for lower plants and soon had the lichens identified. He continued collecting, and when his flora proved inadequate, he sought further help from the professor of botany, Bernt Lynge. Lynge recognized the unusual talents of the young student, who rapidly became an outstanding lichen specialist. Lynge himself, because of severe arthritis, was unable to go to Greenland and offered Scholander the opportunity to take his place as a botanist on several arctic expeditions. As a result, Scholander spent three summers (1930-1932) in Greenland and Spitzbergen. His contributions to lichenology, particularly his revision of the family Umbilicariaceae (1934b), placed him among the world's foremost lichenologists at that time.

The young physician—who was never to practice medicine—had become a botanist. He was approached by the pro fessor of systematic botany, Jens Holmboe, who suggested that he should present himself for a doctorate in botany, and in 1934 he was awarded the Dr. Philos. by the University of Oslo. It might be expected that his dissertation would deal with lichens, but this would be uncharacteristic for Scholander—he was awarded the doctorate based on a monograph of the vascular plants of Spitzbergen (1934a).

During the arctic expeditions, Scholander had been intrigued by the many seals, polar bears, and diving birds he saw. He clearly saw that many important questions needed answers, such as "How do diving seals get enough oxygen?" and "Why don't they get divers' disease as humans do after descending to similar depths?" He obtained working space in the basement of the University Physiological Institute, and with the aid of the professor of anatomy, the geneticist Otto Mohr, he was awarded a small university research fellowship that enabled him to pursue his interests in physiology. He developed new methods for the continuous recording of the respiratory metabolism of diving animals, and one of his publications (1937b) caught the attention of the distinguished Danish physiologist August Krogh, who immediately understood Scholander's genius for design of experimental apparatus. At that time I was a student in Copenhagen, and when Scholander came to give a lecture on his studies of diving animals, I sat there completely spellbound by his brilliant presentation and the simple and logical answers he provided to questions that long had puzzled physiologists who contemplated the mysteries of diving physiology.

The most characteristic differences between a diving seal and a human are strikingly simple. First, when a seal begins a dive, it exhales and dives with a minimal volume of air in the lungs. It may seem counterproductive to dive with little air in the lungs, but it greatly reduces the amount of nitrogen taken up by the blood and the tissues during the dive. This seems to be the key factor in how seals and whales avoid divers' disease, or the bends, which is so tragically well known from humans who ascend after dives to great depths. The oxygen-carrying capacity of the blood, however, is much greater in the seal than in humans. Its blood volume is relatively larger, and both blood and muscles contain much larger amounts of hemoglobin, and thus hold more oxygen, than in mammals in general. A seal's most characteristic response to an experimental dive is to slow the heart down to a few beats per minute; the blood is diverted to the most vital organs, notably the central nervous system and the eyes. The muscles, which are able to function anaerobically through the formation of lactic acid, receive no blood and thus acquire an oxygen debt that is repaid when oxygen is again available at the termination of the dive. These important results were summarized in a monograph (1940a) that remains the foundation for what we understand today of the physiology of diving animals.

At about the same time, Laurence Irving at Swarthmore College had also done distinguished work on diving physiology, and August Krogh arranged for a Rockefeller fellowship for Scholander to continue his studies of diving physiology at Swarthmore. Scholander, however, was still in Norway when the Second World War broke out, and the Rockefeller Foundation cancelled all fellowship awards. When Krogh was informed about this, he sent an urgent telegram to Scholander that he should immediately leave for the USA, and Scholander obtained space on the last ship that left Norway for the USA. Once landed, he of course received the cancelled fellowship, and the studies of diving animals were continued in collaboration with Irving.

However, as the United States became involved in the Second World War, Laurence Irving went to the Air Force, and Scholander followed him there. Both Irving and Scholander made substantial scientific and practical contributions during their military service. Scholander became involved in the testing of military equipment and survival gear. He developed simple and reliable field methods; for example, he studied the conditions under which field stoves caused carbon monoxide poisoning in tents and snow houses, he tried sleeping bags under blizzard conditions, and tested covered life rafts during storms in the Aleutian Islands.

During this time he also arranged a quick and unauthorized rescue mission to an airplane that had crashed at the tip of the Alaskan peninsula. One survivor had been seen, but rescue teams had failed to reach the site over land and Scholander's camp commander had vetoed parachute jumps. Going in by plane would be insubordination, but Scholander found a young pilot who, risking his career, volunteered. Another medical doctor and a priestjoined them, and the three, who had never jumped before, came down successfully, although with some difficulty. They found that two men had perished, but three survivors were taken care of by the M.D. while Scholander took on the cooking, greatly helped by the plane's cargo of sacramental wine for Passover, or "Hangover" as Scholander called it. Eventually the survivors and the rescuers were taken out by a bush plane from Anchorage.

I have later heard that Scholander, as a result of the successful Aleutian rescue, was to be courtmartialed for insubordination. As the case slowly moved through the military channels, it became known to Detlev Bronk, who was angered by the bureaucratic stupidity of punishing one of the most effective scientists in the Air Force. By some unknown intervention, the case was aborted at the highest level in Washington.

After the years of military work, Irving and Scholander, with support from the Navy, established a research laboratory at Point Barrow, Alaska. Among the most important results were measurements of the metabolic heat production and the insulation needed for warmblooded animals, mammals and birds, to keep warm in the arctic climate. It turned out that larger animals, such as foxes and eskimo dogs, could sleep at temperatures as low as -30 or -40°C without any increase in heat production. Smaller animals, however, started to shiver at temperatures well above freezing, and they needed increased heat production to stay warm and maintain a normal body core temperature of 37°C. The information was in agreement with the simple physical laws of heat exchange, and the studies clearly showed that below a certain temperature, the lower critical temperature, the metabolic rate increased linearly with the decrease in temperature. This model for the responses of warmblooded animals to low temperatures has remained the model for virtually all later studies of this nature (1950c,d,e).

The arctic work included studies of coldblooded animals and of plants, and also the tolerance of various organisms to freezing. Scholander devised a simple and elegant method for determining the amount of body water in a mosquito larva that was actually frozen to ice inside the intact animal. He showed that these, when they were thawed out, were still alive and unharmed after more than 80 percent of their body water had been frozen to ice (1953e).

Scholander's many ingenious methods for the analysis of minute samples of gas became very helpful in an unexpected area. He noticed that gas bubbles tended to develop under ice, and also that a chunk of ice chopped from a glacier gave off bubbles of gas when put into a drink to cool it. Scholander determined that ice is virtually impermeable to gases; he froze thin sheets of ice, 0.1 mm thick, and floated them on cold mercury. He introduced small volumes of gases under the ice and attempted to measure changes in their composition. He was never able to detect any penetration, and con cluded that ice at -10°C is at least 70,000 to 80,000 times more impermeable to gases than a layer of water. This made him wonder whether the air trapped deep in the Greenland glaciers contains a permanent record of the composition of the atmosphere in earlier periods (1956b,e; 1958c,g; 1960c; 1961c; 1962g).

After the two years of work at Barrow, Scholander left the Arctic Research Laboratory. A. Baird Hastings, professor of biological chemistry at Harvard University, was a person who fully understood Scholander's genius, and he offered him a position as research fellow. During two years in Hastings' laboratory, Scholander developed a series of elegant micromethods for blood and gas analysis, and started applying these to solving some of the intriguing problems of how gases are secreted into the swimbladder of fish. Depending on the depth at which a fish is found, the gases in the swimbladder are under high pressure. Gases that are dissolved in the surrounding seawater are at partial pressures close to those in the atmosphere, and they are secreted into the swimbladder against pressures that in deep sea fish may amount to several hundred atmospheres. The micromethods Scholander had developed were ideal for these studies. He now demonstrated that not only oxygen, but also nitrogen, is secreted against tremendous concentration gradients. That this is possible for oxygen, a relatively reactive gas, was perhaps not surprising, but how could nitrogen, a presumably totally inert gas, also be secreted actively against such concentration gradients? The answer to both puzzles lay in the unusual vascular supply to the swimbladder and in physical principles that, once they were understood, were simple to explain. The blood supply to the swimbladder forms a counter-current system that is an essential part of the secretion mechanism (1954a,b).

The swimbladder studies were continued during the three years of Scholander's association with the Woods Hole

Oceanographic Institution as a physiologist (1952-1955). The result was a series of papers that now form the foundation for our understanding of swimbladder function (1955g, 1958f, and others).

In 1955 Scholander was called to the University of Oslo as professor and director of a new Institute of Zoophysiology. His three years in Oslo were very active. He continued studies of the adaptation of humans to cold and answered questions of how primitive humans can keep warm in cold climates. Eskimoes, Lapps, and Australian aborigines were compared with Norwegian students. The latter slept outdoors at temperatures around 0°C, naked in a single-blanket sleeping bag. For an unacclimatized person this is intensely uncomfortable and it is impossible to sleep, but after five or six days of acclimatization the students had no trouble. The most notable change in their physiology was a substantial increase in metabolic heat production. (The motivation of the students to go through with these uncomfortable experiments seemed to be related to their being granted permission to hunt reindeer.)

When the Australian aborigines were studied, it was found that they could sleep through a cold night with a normal metabolic rate. The difference was that the aborigines permitted the temperatures of the legs to drop to around 10°C. In other words, they had adapted by permitting their peripheral temperatures to drop instead of increasing their metabolic rate. This, of course, is a more economical approach; keeping the legs warm requires a substantial increase in metabolic heat production.

After three highly productive years in Norway, Scholander was brought to the Scripps Institution of Oceanography through the efforts of its director, the oceanographer Roger Revelle, and he remained there until the end of his life. With tremendous enthusiasm he took up one problem after another. Among these were some important problems of plant physiology. The question of how water can be brought to the top of the tallest trees had always intrigued him, and studies that he had initiated while he was at Woods Hole and in Oslo were continued (1957a, 1958b, 1961d). The fact that suction, in combination with the cohesion of water, can explain the ascent of the sap led to later studies of osmotic phenomena in other plants, and eventually to the development of the solvent tension theory of osmosis. Important in this connection was a series of studies of mangroves and their salt balance and the question of how they manage with their roots in seawater. These studies started on the Cape York Peninsula in northern Australia (1962h) and continued for several years (1965a,c; 1966a; 1968b).

The solvent tension theory of osmosis was founded on Scholander's experience in plant physiology. A series of elegant experiments, in part carried out in collaboration with H. T. Hammel, also of Scripps, provided easily comprehended support for Scholander's theories (1971c,e). The theories were severely criticized by several physical chemists, who based their arguments on classical thermodynamic arguments. Scholander's ingenious experiments remained unchallenged; only their interpretation could be disputed. Although Scholander's view is simple and intuitive, it is undoubtedly more convenient to adhere to the traditional way of thinking about osmotic phenomena. I can only regret that I do not have the competence to evaluate the controversy and give proper perspective to the significance of Scholander's contributions in this field.

A most important development during Scholander's many years at the Scripps Institution was the design and building of the research vessel *Alpha Helix*. During his many field expeditions, Scholander had understood the importance of bringing adequate equipment into the field, where the important physiological problems are evident. He conceived of a laboratory ship that should be able to take a group of a dozen or so scientists to any area of the world. The ship should be equipped with modern laboratories and an excellent machine shop. This was based on Scholander's own experience; he was a skilled machinist and this had helped him during his many successes in designing new methods and equipment. He saw the absolute necessity of always having at his disposal a good machine shop that could solve problems as they arise under field conditions.

Support for building the ship was obtained from the National Science Foundation, and in 1966 the ship started on its first cruise, the Billabong Expedition to the Great Barrier Reef. A National Advisory Board, initially chaired by Baird Hastings, evaluated applications for use of the ship. Over the fourteen years the ship remained in service as a floating physiological laboratory, expeditions went to Australia, the South Seas, the Amazon, the Antarctic, the Galapagos, the Bering Sea, and other sites. Several hundred scientists from all over the world have participated actively in these expeditions, and the records of Scripps Institution show that the work on the *Alpha Helix* has resulted in a total of 547 publications in recognized scientific journals—an impressive record for the relatively modest funds invested in the *Alpha Helix.*

Not only through the *Alpha Helix,* but throughout his life, Scholander became a seminal figure for physiologists who were concerned with the problems that animals and plants encounter in nature. He was an immensely enthusiastic person, a true naturalist who perceived interesting scientific problems wherever he moved. He led a restless and highly productive life. A large number of scientists, now active and recognized around the world, have been associated with Scholander and have been influenced by his stimulating and dynamic personality. In 1951 he married the daughter of Laurence Irving, Susan, who remained his devoted companion the remainder of his life.

Throughout his productive life, Scholander retained the ability to open up new fields of investigation. He was not only a brilliant scientist, but also an enthusiastic and charming person, engaging, warm, and generous. His way of managing the increasing constraints imposed by the bureaucracy of university life and government support of science was merely to ignore them. As an example of his charming and untraditional personality, I wish to relate his study of wave-riding dolphins. Many of us have seen dolphins or porpoises that playfully ride along just in front of the bow wave of a ship, motionless and effortlessly remaining in place. This, of course, aroused Scholander's curiosity. It belongs to the story that the free ride of the dolphins had been the subject of theoretical analysis by fluid dynamicists, but Scholander thought that the theory, to use his own words, "missed the boat." When on board a Norwegian sealing vessel, he rigged up a simple device—with a streamlined vane attached to a spring to measure the forces in the bow wave—and he found that these might indeed explain how a dolphin gets the needed forward thrust to remain in the wave (1959a,d). Characteristically, Scholander concluded his paper with these words: "This, I believe, is the way dolphins ride the bow wave, and if it is not, they should try."

Scholander died in his home in La Jolla, California, on June 13, 1980. He was seventy-four years old. Ten days earlier he had broken his hip in a fall in his laboratory, but after a brief hospital stay he was home again and enthusiastically planning for the summer's activities, which included a trip to Europe. A sudden collapse ended his long and productive life.

The material I have used to prepare this biography has been assembled with the help of Susan Irving Scholander and the present director of the Physiological Research Laboratory at Scripps

Institution of Oceanography, Dr. Fred N. White. Archival material has been made available to me by Elizabeth N. Shor, also of Scripps Institution of Oceanography. My own familiarity with P. F. Scholander's life comes from my admiration for his scientific contributions and from many years' close friendship. Other good friends have helped me by reading my manuscript.

HONORS AND DISTINCTIONS

Education

M.D., University of Oslo, 1932
Dr. Philos. (Botany), University of Oslo, 1934

Professional Appointments

1932-1934	Instructor of Anatomy, University of Oslo
1932-1939	Research Fellow in Physiology, University of Oslo
1939-1941	Rockefeller Fellow
1939-1943	Research Associate, Department of Zoology, Swarthmore College
1943-1946	U.S. Air Force. Commissioned as Captain, 1943; Major, 1946
1946-1949	Research Biologist, Swarthmore College
1949-1951	Research Fellow, Department of Biological Chemistry, Harvard Medical School
1952-1955	Physiologist, Woods Hole Oceanographic Institution
1955-1958	Professor of Zoophysiology, University of Oslo, and Director of Institute of Zoophysiology
1958-1973	Professor of Physiology, Scripps Institution of Oceanography, University of California, San Diego
1963-1970	Director, Physiological Research Laboratory, University of California, San Diego
1973-1980	Emeritus Professor, Scripps Institution of Oceanography, University of California, San Diego

Honors and Awards

Rockefeller Fellow, 1939-1941
Soldiers' Medal for Valor (for Aleutian rescue), 1945
Legion of Merit (for pilot ejection seat), 1946
Norwegian Academy of Sciences, 1955
American Academy of Arts & Sciences, 1959
National Academy of Sciences, 1961
American Philosophical Society, 1962
Cosmos Club, 1964
John Simon Guggenheim Fellow, 1969-1970
Doctor of Science, University of Alaska, 1973
Royal Swedish Academy of Sciences, 1974
Doctor of Science, Uppsala University, 1977
Fridtjof Nansen Prize, Oslo, 1979

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d. With L. Irving and S. W Grinnell. Experimental studies on the physiology of diving mammals. Science, 92:483 .

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b. With L. Irving. Experimental investigations on the respiration and diving of the Florida manatee. J. Cell. Comp. Physiol., 17:169-91 .

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d. With L. Irving and S. W. Grinnell. Significance of the heart rate to the diving ability of seals. J. Cell. Comp. Physiol., 18:283-97 .

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c. With L. Irving and S. W. Grinnell. The regulation of arterial blood pressure in the seal during diving. Am. J. Physiol., 135:557-66 .

d. With L. Irving and S. W. Grinnell. On the temperature and metabolism of the seal during diving. J. Cell. Comp. Physiol., 19:67-78 .

e. With S. W. Grinnell and L. Irving. Experiments on the relation between blood flow and heart rate in the diving seal. J. Cell. Comp. Physiol., 19:341-50 .

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William H. Stein:

June 25, 1911-February 2, 1980

By Stanford Moore

William H. Stein began his autobiographical sketch for the 1972 volume of Nobel lectures as follows:

> I was born June 25, 1911, in New York City, the second of three children, to Fred M. and Beatrice Borg Stein. My father was a businessman who was greatly interested in communal affairs, particularly those dealing with health, and he retired quite early in life in order to devote his full time to such matters as the New York Tuberculosis and Health Association, Montefiore Hospital, and others. My mother, too, was greatly interested in communal affairs and devoted most of her life to bettering the lot of the children of New York City. During my childhood, I received much encouragement from both of my parents to enter into medicine or a fundamental science.

His early education was at the Lincoln School of the Teachers College of Columbia University. It was a so-called "progressive school" of the time; in addition to fostering interest in the creative arts, music, writing, and sports, the curriculum included well-taught courses in chemistry, physics, and biology, with field trips that he enjoyed. From those years, he used to recall that his first scientific project as a student was as an avid collector of moths and butterflies. At sixteen, he transferred to a preparatory school in New England, Phillips Exeter Academy, which offered a demanding educational experience that he felt strengthened his work habits and his precision of writing.

In 1929 he matriculated at Harvard, as had his father and his older brother before him. He majored in chemistry; the scientific background thus achieved led him to spend one year as a graduate student at Harvard in chemistry. It was suggested to him, however, that he might enjoy the developing subject of biochemistry more than organic chemistry *per se.* As a result, in 1934 he transferred to the Department of Biological Chemistry at the College of Physicians and Surgeons, Columbia University. He found the environment most challenging. Hans Clarke, the chairman, had succeeded in gathering a stimulating faculty and a group of unusually gifted graduate students from around the world. Nearly a dozen of those students became outstandingly productive biochemists.

During his graduate-student days, in 1936, William Stein married Phoebe Hockstader. His wife and their three sons—William H., Jr.; David F.; and Robert J.—were to be invaluable resources for a creative scientist throughout a career in which science and family were intimately interwoven. Stein lived on Manhattan most of his life, with an interlude in Scarsdale, New York, while the children were of school age. He enjoyed summer retreats both at Cos Cob, with opportunities for tennis and swimming, and, in the later years, at Woodbury, in Connecticut.

William Stein completed his Ph.D. thesis in 1937, under the guidance of E. G. Miller, Jr.; the subject was the amino acid composition of elastin. Thus began a lifelong concern with the chemistry of proteins. His first experiment was the preparation of elastin from the ligamentum nuchae of the ox. In the course of applying some of the gravimetric methods of the time, he used two precipitants that had been developed by Max Bergmann—potassium trioxalatochromiate for glycine and ammonium rhodanilate for proline. He was introduced to these methods by Erwin Brand at Columbia, who had worked with Bergmann in Germany.

Bergmann had arrived in the United States from Dresden in 1934 to become a member of The Rockefeller Institute for Medical Research in New York. When Stein completed his studies for the Ph.D. degree at Columbia in 1937, it was a logical progression for him to move southward on Manhattan to join the Bergmann group. Again, Stein found himself in an exceptionally stimulating environment; Bergmann was attracting a talented international group of postdoctoral assistants, many of whom became prominent biochemists. Among the current Academy members from this group are Joseph S. Fruton, Emil L. Smith, Klaus Hofmann, and Paul Zamecnik.

There were two main lines of investigation in the Bergmann laboratory: the specificity of proteolytic enzymes and the structural chemistry of proteins. Stein initially applied his talents to the task of trying to improve gravimetric methods for amino acid determination. His first contribution to methodology was his introduction of the concept of the solubility product method in an attempt to permit quantitative results to be obtained with reagents that gave sparingly soluble salts of amino acids. Stanford Moore joined the Bergmann laboratory in 1939, arriving via Vanderbilt and Wisconsin. Bergmann suggested that Stein and Moore pool their efforts to see whether the solubility product method could be developed into a practical analytical procedure. Through careful attention to the details of gravimetric analysis, using two of the reagents introduced by Bergmann, they were able to determine glycine with 5-nitronaphthalene-1-sulfonic acid as the precipitating agent and leucine with 2-bromotoluene-5-sulfonic acid. The method was applied to hydrolysates of egg albumin and silk fibroin. But the future was to provide methods that were to be less tedious and more micro.

At this stage, the research on amino acid analysis was interrupted by the war years. The laboratory was engaged under contract to the Office of Scientific Research and Development to look for possible therapeutic agents for vesicant war gases through study of the physiological mechanisms of action of mustard gas and the nitrogen mustards. Stein was a coauthor of a series of fundamental papers concerned with the chemistry of the reactions of mustard gas and related compounds with the functional groups of amino acids and peptides.

During the war years, in 1944, illness took the life of Max Bergmann at the age of fifty-eight. The members of the laboratory carried the war work to completion in 1945, at which time most of them moved on to other positions.

For three years, Moore had been out of the laboratory serving the Office of Scientific Research and Development in administrative capacities in Washington and on other wartime assignments. Stein and Moore debated whether to accept appointments elsewhere or to ask the Director of The Rockefeller Institute, Herbert S. Gasser, whether he would give them a chance to see what they could accomplish on the Rockefeller scene. Gasser offered the two young investigators an opportunity, on a trial basis, to initiate a research program that might merit continued support.

With that challenge, they started with the premise, born of the Bergmann years, that accurate establishment of the amino acid compositions of proteins is a first step toward progress in determination of their chemical structures. In 1945 it was possible to take a completely new look at the problem of amino acid analysis. The renaissance in chromatography stimulated by A. J. P. Martin and R. L. M. Synge in England, together with Lyman C. Craig's development of liquid-liquid countercurrent distribution in his laboratory just down the corridor from the Bergmann department at Rockefeller, brought to the attention of biochemists the potential resolving power of multi-plate separations. After weighing the possibilities for speed, resolution, simplicity, and quantitativeness, Stein and Moore decided to try column chromatography. Thus began several busy years of close collaborative effort on the development of methods and equipment.

After initial experiments in which the fractions were collected by hand, a photoelectric drop-counting fraction collector was built to expedite the collection of the effluent in a series of small fractions of precise volume; it was the prototype for the commercially built fraction collectors based upon this principle that became widely used in biochemistry. Then a simple and sensitive quantitative method for measuring the concentration of amino acid in each tube was needed. The ninhydrin reaction had been introduced by Ruhemann in 1911. The blue-colored product is sensitive to oxidation; in initial trials the results did not obey Beer's law. When the reaction was carried out anaerobically, the yield was improved and linearity was approached, but such a procedure was inconvenient. An oxygen-free environment in solution in an open tube was attained by including a dissolved reducing agent, such as stannous chloride or the reduced form of ninhydrin (hydrindantin). A water-miscible organic solvent (first, methyl Cellosolve, and later, dimethyl sulfoxide) was added to keep the blue-colored reaction product (diketohydrindylidene-diketohydrindamine) and hydrindantin in solution. This method of measurement was first used to monitor the peaks eluted from columns of potato starch operated with n-butanol-water as the solvent system, a type of chromatogram first tried by Synge. With a neutral solvent, the use of a preliminary wash with 8-hydroxyquinoline was found essential to prevent metals in the starch from distorting the separations. Over a period of several years, quantitative systems with starch columns were developed for determining all of the common amino acids of protein hydrolysates and were applied to the analysis of β-lactoglobulin and bovine serum albumin in 1949. The results were welcome, but it took two weeks to run the three starch columns required.

Faster analyses became possible when finely powdered ion exchange resins became available for chromatography. With sulfonated polystyrene resins, several years of exploratory chromatograms led to the use of buffers of different pHs at different temperatures for the serial elution of all of the common amino acids of proteins and of physiological fluids. In the early 1950s, the time for each analysis was reduced to about five days.

With an efficient chromatographic method at hand, the next stage was to render the process automatic. This project in instrumentation was undertaken in cooperation with Darrel Spackman and led to an automatic amino acid analyzer in 1958. The eluent was pumped through the resin bed at several atmospheres of pressure. The ninhydrin color was developed in the flowing stream and the optical density was recorded potentiometrically; a hydrolysate was analyzed in an overnight run. Subsequent academically and commercially introduced improvements have utilized finer resins and higher pressures and have attained sixty-minute analyses. The resulting amino acid analyzers found a worldwide market and represented the first widely used form of highperformance liquid chromatography.

In the writing of the papers on methodology every effort was made to include all of the details needed for effective use of the methods. This enterprise was facilitated by circulating preprints to biochemists who expressed an immediate interest in using the procedures and who could check the completeness of the experimental directions in advance of publication.

During the early years of our cooperation, Stein and I worked out a system of collaboration that lasted for a lifetime.

Stein combined an inventive mind and a deep dedication to science with great generosity. Over a period of forty years, we approached problems with somewhat different perspectives and then focused our thoughts on the common aim. If I did not think of something, he was likely to, and vice versa, and this process of frequent interchange of ideas accelerated progress in research. It also helped in the writing of papers. I never drafted a text that Stein could not improve.

The methodology was developed with the primary aim in mind of opening new approaches to the study of the chemical structures of proteins. After the first four years of the above studies, Gasser decided that the two young investigators were making enough progress to merit being hosts to a postdoctoral fellow. In 1949, they attracted Werner Hirs from Columbia University. A key decision at that time was the choice of the protein to study. In England, Frederick Sanger had his classic studies on insulin well under way, for which he was able to use qualitative methods in large part. The study of longer polypeptide chains would gain from quantitative analyses at each step. And an enzyme would be an appealing subject for study because the structural knowledge could provide a baseline for determination of specific residues involved in the enzyme-substrate interactions. Bovine pancreatic ribonuclease, a protein about twice the size of insulin, was readily available and had been partially purified at Rockefeller by Dubos and Thompson and by Kunitz. Hirs extended the technique of ion exchange chromatography to ribonuclease on a polymethacrylic acid resin.

In 1952 Gasser decided that Stein and Moore qualified as members of The Rockefeller Institute for Medical Research; the title became professor when the institution assumed its role in graduate education as The Rockefeller University under the administration of Detlev Bronk.

The research on chromatographically purified RNase A was then extended, with Hirs, to the development of methods for the ion exchange chromatography of peptides obtained by tryptic and chymotryptic hydrolysis of the chain in which the four disulfide bonds had been split by oxidation. J. Leggett Bailey joined the project to study the peptides liberated by pepsin. Thus began the collection of data from which a sequence for the enzyme could be deduced, with the invaluable additional aid of the sequential degradation reaction newly introduced by Pehr Edman in Sweden in 1950.

At the time that these studies on ribonuclease were begun, Christian B. Anfinsen and his associates at NIH also turned to ribonuclease as an appropriate molecule for structural study; the combined results from the two laboratories (experiments in progress were freely discussed) expedited the solution of the problem. The determination of the final sequence, to which Darrel Spackman and Derek Smyth were contributors at Rockefeller, also depended upon a key observation at NIH by Erhard Gross and Bernhard Witkop, obtained through the application of their ingenious method of cleavage at methionine residues by cyanogen bromide. Thus, for the first time, the chemical formula of an enzyme could be written.

Derivatization experiments were then undertaken at Rockefeller in order to identify residues at or near the active site. Iodoacetate was the first reagent studied. The enzyme was known to be inactivated by the reagent; at that time rapid reaction with iodoacetate was thought of primarily as an indication of-SH groups. When it was established that ribonuclease did not have any-SH groups, an evident task was to ascertain what was happening. Through experiments begun by Gerd Gundlach, amino acid analysis was used to show that, depending upon pH, the reagent could alkylate primarily methionine, histidine, or lysine residues in the enzyme. Arthur Crestfield showed that in thirty minutes at pH 5.5, the principal reaction was with the imidazole group of one of two specific histidine residues, yielding a carboxymethyl group either on the 1-nitrogen of histidine-1 19 or on the 3-nitrogen of histidine-12. Through Robert Heinrikson's data on the effect of carboxymethylation of the ε-NH_2 group of lysine-41 on these reactions, it was possible to conclude that in the three-dimensional structure of the enzyme the reactive nitrogens of histidine-12 and histidine-119 were about 5 Ångstroms apart at the active site of the catalyst and that the ε-NH_2 group of lysine-41 was 7-10 Ångstroms from nitrogen-3 of histidine-12. These three-dimensional predictions, made on chemical grounds, were borne out by the subsequent x-ray crystallographic analyses of Frederic Richards and Harold Wyckoff at Yale.

One of the questions posed to George Stark was whether these two uniquely reactive histidine residues would still be especially reactive toward iodoacetate if the molecule were unfolded in 8 M urea. As expected, they are not. But in one of these experiments he detected, by amino-acid analysis, a side reaction that turned out to be carbamylation of lysine residues by traces of cyanate in the urea to give homocitrulline. This observation served as one of several reminders that, as demonstrated in 1828 by Wöhler, ammonium cyanate and urea are in equilibrium. One of those thus reminded was Anthony Cerami, then a student in another laboratory at Rockefeller. Some years later, when he heard that urea was being administered to patients with sickle cell anemia, he wondered whether cyanate merited consideration as the possible active agent in such an experiment. He elicited the cooperation of James Manning, who had recently joined the Stein and Moore laboratory. From the investigations of the two young men, there grew a decade of research on the effectiveness of the carbamylation of hemoglobin S in converting the molecule to one of almost normal physiological function.

From RNase A, with 124 amino acid residues, attention was turned to bovine pancreatic deoxyribonuclease; chromatography on phosphocellulose yielded a homogeneous preparation of DNase A, which proved to be a glycoprotein with a single peptide chain of 257 residues. The sequence of DNase A was established in 1973 as a result of several years of researches by Paul Price (as a graduate student), Teh-yung Liu, Brian Catley, Johann Salnikow, and Ta-hsiu Liao. Additional experiments were conducted by Tony Hugli, Bryce Plapp, and Dalton Wang. The result was a thorough knowledge of the chemistry of the enzyme, its existence in four chromatographically distinct forms (A, B, C, and D), and identification of special features of each isozyme.

Stein, throughout his life, in his generous manner, took a special interest in facilitating the careers of scholars whose sojourns in the laboratory made possible the exploration of many facets of the researches. A number of enzymes were the subjects of studies of specific aspects of protein structure and function. For seventeen years, Stein was the principal investigator on a grant from the National Institute of General Medical Sciences, NIH, to study that subject. Some of the enzymes, in addition to RNase and DNase, that the laboratory studied with partial support from that grant were: bromelains (with Shoshi Ota), chymotrypsin (with Denis C. Shaw), pepsin (with T. G. Rajagopalan, T. A. A. Dopheide, and Roger Lundblad), streptococcal proteinase (with Teh-yung Liu, William Ferdinand, Brenda Gerwin, Norbert Neumann, Michael C. Lin, and Michael Bustin, in cooperation with the enzyme's discoverer, Stuart D. Elliott), ribonuclease T_1 (with Kenji Takahashi), 2',3'-cyclic nucleotide 3'-phosphohydrolase from brain (with Arabinda Guha, David C. Sogin, and Robert J. Drummond), and carboxypeptidase Y (with Rikimaru Hayashi).

Stein took a particular interest in the application of the chromatographic methods to the analysis of physiological fluids for amino acids. One of his earliest uses of the procedure was in a quantitative study of the major ninhydrin-positive constituents of human urine. He extended his studies to cystinuria, and the laboratory, in collaboration with Alexander Beam of The Rockefeller Hospital, investigated the aminoaciduria of Wilson's disease. Part of that study required adaptation of the method to the measurement of blood plasma amino acids as well. In cooperation with Harris Tallan, the free amino acids of mammalian tissues were also surveyed in detail. Out of these several studies grew the identification of 3-methylhistidine and tyrosine-O-sulfate as normal constituents of human urine, and the observations that acetylaspartic acid is a major metabolite in brain and that cystathionine is a principal amino acid in human brain. In a study with Alejandro C. Paladini, phenylacetylglutamine was found to be a normal major metabolic product in human urine.

One of the first applications of the amino acid analysis procedure was to human hemoglobin A prepared electrophoretically by Henry Kunkel of The Rockefeller Hospital; the absence of isoleucine was demonstrated to be one criterion for the purity of this protein. That study led to a reexamination (with R. David Cole) of the cysteine content of human hemoglobin.

Stein combined his research efforts at Rockefeller with service in a number of capacities on the national and international scenes. He was a visiting professor at the University of Chicago in 1961, and at Harvard University in 1964. His lectures there, throughout his travels, and to graduate students at Rockefeller conveyed an exciting picture of the horizons that new methods were opening in the study of the chemical structure of proteins. He was a member of the Medical Advisory Board of Hebrew University-Hadassah Medical School, Israel, 1957-70; trustee of Montefiore Hospital in

New York, 1947-74; and member of the Council of the Institute of Neurological Diseases and Blindness of NIH, 1961-66. He was vice-chairman of the U.S. National Committee for the International Union of Biochemistry, 1962-63, chairman, 1965-68, and chairman of the Publications Committee for the Sixth International Congress of Biochemistry held in New York in 1964. He and his wife enjoyed the worldwide travel that was an integral part of his career and the opportunity to host the scholars from many countries who came through New York City in the course of their journeys. His major pastimes were directly related to the life of a scientist with international interests.

The American Society of Biological Chemists drew upon his editorial skill, beginning in 1955, when he was elected to the Editorial Committee. He was chairman of the Committee from 1958 to 1961. He was an active participant in the search that led to the appointment of John Edsall to the editorship of *The Journal of Biological Chemistry* in 1958, upon the retirement of Rudolph J. Anderson. Stein joined the Editorial Board of *the Journal* in 1962. In his drafts of editorial letters he was quick to praise a fine manuscript, to decline an inadequate one, and careful to explain in gracious detail the options for revision when that seemed necessary. Two years later he was asked by Edsall to assume one of the three associate editorships.

As Edsall's ten-year term as editor drew toward a close, Stein was asked by the Council of the Society to consider the position. He accepted and took a leading part in setting up the administrative procedures that would facilitate the handling of the increasing numbers of manuscripts that were being received as the *Journal* grew in size. The changes included the organization of a permanent central office at the Society's headquarters in Bethesda to which all manuscripts would go initially. Stein had earlier had a key role in the recruitment of Robert A. Harte as full-time executive secretary of the Society; Harte was made business manager of the *Journal* and Edith Wolff was the first executive assistant. A staff was thus established to handle the innumerable business details associated with receiving more than 3,000 manuscripts annually and managing the publication of editorially acceptable texts.

Stein's foresight in centralizing the first step in the editorial process, with the view of facilitating the transfer of the editorship to the next recipient, was tested—tragically—much too soon. He was stricken by major illness in 1969, after a year and a half in the position.

In 1969, while in attendance at an international symposium on proteolytic enzymes being held in Copenhagen, Stein developed a high fever that prevented him from giving the paper he was scheduled to present. A few days later, en route home by air, paralysis began. He barely survived the acute phase of the disease, which was diagnosed as a severe case of Guillain-Barré syndrome. After a year of hospitalization, he remained a quadriplegic. He met this tragedy with great courage and with preservation of his sense of humor. His wife and their three sons helped him immeasurably to meet the almost unbelievable frustrations of a disabled person possessing the intellectual drive of a creative brain that was fully functional to the last day. The Rockefeller University, under the administrations of Frederick Seitz and Joshua Lederberg, was host to Stein's determined endeavors during eleven years of this difficult existence. He, of course, had to relinquish the editorship of *the Journal* in 1969, but he continued to provide welcome advice, both on specific manuscripts and on matters of policy, to Herbert Tabor, his successor.

Stein had the pleasure of seeing the subject of RNase grow in interest rather than taper off. When the laboratory's work on the enzyme was begun, the protein was viewed as a catalyst of rather limited physiological interest; it was recognized as one of the enzymes of the digestive tract. Work from several laboratories on the presence of RNases of the pancreatic type in most mammalian cells and of a widely distributed specific inhibitor of the enzyme broadened the scope of the subject. Stein followed with enthusiasm the researches of the young associates in the laboratory on the isolation of the inhibitor in pure form from human placenta and the establishment of its molecular properties. He made the uncomfortable journey by wheelchair to his office at Rockefeller whenever he felt able, and he was a valuable consultant to all of the members of the laboratory. On occasion, they would meet at his home for informal seminars on current research. He took an active interest in reading manuscripts and his editorial skill was always helpful. With the stimulating cooperation of Phoebe Stein, their home continued to be the scene of visits by scientists from around the world who enjoyed the opportunity to discuss both biochemistry and the issues of the times, which he continued to analyze with keen perception.

A life of sixty-eight years filled with unusual measures of accomplishment, acclaim, and suffering came to a close on February 2, 1980, when William Stein died suddenly from heart failure at his home in New York.

Among the many honors that Stein received were election to the National Academy of Sciences in 1960 and to the American Academy of Arts and Sciences in the same year. Awards that he shared with Moore were the American Chemical Society Award in Chromatography and Electrophoresis (1964), the Richards Medal of the American Chemical Society (1972), the Kaj Linderstrøm-Lang Award, Copenhagen (1972), and the Nobel Prize in Chemistry (1972), shared also with Christian B. Anfinsen. Academic honors included D.Sc. *honoris causa* from Columbia University (1973), D.Sc. *honoris causa* from the Albert Einstein College of Medicine of Yeshiva University (1973), and the Award of Excellence Medal from the Columbia University Graduate Faculty and Alumni Association (1973).

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Curt Stern

Curt Stern

August 30, 1902- October 23, 1981

By James V. Neel

There have been three remarkable periods in the history of modern genetics. The first of these, in the early years of this century, encompasses the rediscovery and confirmation of Mendel's findings and the enunciation of the chromosomal theory of inheritance. The second period is characterized by a concatenation of discoveries regarding the more precise mechanisms of chromosomal behavior, largely based on the use of *Drosophila* and emanating from the "fly room" at Columbia University. The third is the period in which we now find ourselves, initiated in the 1940s by the seminal observations of Avery and collaborators and the later phage work demonstrating that the essential genetic material was DNA and the demonstration, using *Neurospora,* that genes have essential and specific roles in the synthesis of proteins. This was followed by the elucidation of the structure of DNA, leading to a cascade of discoveries concerning DNA fine structure and how it can be manipulated. Each of these flowerings dominated the conceptual biological thinking of the time.

NOTE: This memoir is an expanded version of a manuscript published in the *Annual Review of Genetics.* I am deeply indebted to the many people who have contributed their remembrances and perspective to the writing of this memoir.

The Early Years

Curt Stern, the subject of this memoir, must be regarded as one of the principal participants (and last survivors) of the fly room period. [1] Born in Hamburg, Germany, on August 30, 1902, the first son of Barned and Anna Stern, he early displayed a strong interest in natural history, ranging from microscopic studies of pondwater to the zoological collections that enliven parental responsibilities. In these interests he received unusual support from two extraordinary high school teachers, who encouraged him to undertake the study of zoology. His father, who was in the dental supply business, and his mother, a schoolteacher, were also highly supportive as their son's biological interests unfolded. Following the family's move to a suburb of Berlin, he entered the University of Berlin in 1920. He must almost immediately have found his way to the Kaiser Wilhelm Institute, where he conducted his doctoral studies. He received a Ph.D. from the University of Berlin in 1923, at that time the youngest person ever to receive the degree from the university. There is no record of an undergraduate degree. Attendance at the university required one to two hours of commuting each way. To achieve his degree so early under these circumstances was an early, clear signal of the remarkable combination of high intellectual ability, photographic memory, and stamina that was to characterize his career.

Stern's Ph.D. thesis was a descriptive cytological study of mitosis in a protozoan of the order *Heliozoa,* under the direction of one of the truly eminent protozoologists of that era, Max Hartmann. One perceives here the momentum of a high school fascination with microscopic pond life. By the time he finished his thesis, he knew he would not remain a

[1] The only remaining survivor of the "fly room" known to the author is Helen Redfield (Mrs. Jack) Schultz.

protozoologist. Reading widely, he became intrigued by genetics. A paper by Richard Goldschmidt, then director of the Institute, on the basis for crossing-over caught his attention. Stern felt Goldschmidt's interpretation was incorrect and wrote a critique, which, after great hesitation, he submitted to Goldschmidt. (He was at that time probably the most junior fellow at the Kaiser Wilhelm Institute.) Some six months later, Goldschmidt returned the paper without comment, but shortly thereafter called Stern into his office. Those of us privileged to see Goldschmidt in action after his immigration to the United States will recall the personification—at least superficially—of the Geheimrat Professor. One can imagine the trepidation with which Stern approached the meeting. With few preliminaries, Goldschmidt offered Stern a fellowship, recently funded by the International Education Board of the Rockefeller Foundation, to study genetics with the Morgan group. There apparently was never any discussion of the critique.

Drosophila

Stern arrived at Columbia in 1924. The fly room was near or at its zenith. Morgan in 1910 had demonstrated sex-linked inheritance in *Drosophila* and then the recombination of two sex-linked alleles, one responsible for white eye and the other for rudimentary wing. These exciting discoveries, along with the obvious potentiality of *Drosophila* as an experimental organism, had attracted to Morgan's laboratory a now historic triumvirate—Sturtevant, Bridges, and Muller. The publication by Morgan, Sturtevant, Muller, and Bridges of "The Mechanisms of Mendelian Heredity" in 1915, which brought together the early data on autosomal as well as X-chromosome inheritance and linkage, with linkage maps and the evidence for nondisjunction, had made clear how major the developments at Columbia were. One result was a rising number of students and visiting investigators, who, together with Sturtevant, Muller, and Bridges, were all crowded into a room 16 by 23 feet in size, which contained eight working desks! (For a forthright statement of the physical "togetherness" of this setting, and especially the space alloted to Stern, see Provine 1981.)

There quickly ensued several "beginner papers," but the first major result of this fellowship, published in 1926-1927, was the demonstration by a combination of genetic and cytological techniques that the anomalous genetic behavior of the "bobbed bristles" trait could be explained by the Y-linked inheritance of the responsible allele (plus a homologous locus on the X). Until then, the Y-chromosome of *Drosophila,* although associated with male fertility, had been considered as otherwise genetically empty. Although Y-linked inheritance had previously been demonstrated by Schmidt in the fish, *Lebistes reticulatus,* this demonstration that abnormalities of Y-chromosome behavior (that is, the occurrence of XXY females) accounted for abnormalities in the inheritance of the bobbed trait was unusually elegant for the times, no doubt benefiting greatly from the cytological demands of Stern's Ph.D. thesis.

His second major contribution appeared in 1931: the demonstration, using cytologically abnormal X-chromosomes, one with an X-Y translocation, one with an X-IV translocation, that the genetic phenomenon of crossing-over was accompanied by a physical exchange between the chromosomes. (Simultaneously, Creighton and McClintock quite independently demonstrated the same phenomenon in corn.) This was shortly followed by an ingenious demonstration (back to the Y-chromosome) that as he added supernumerary Y-chromosomes bearing the bobbed allele to *Drosophila,* the trait gradually disappeared, understandable now that we know the bobbed alleles are characterized by varying degrees of underproduction of ribosomal RNA. At that time the demonstration that adding enough defective genes would produce normality was novel.

Stern's last major contribution to our understanding of the chromosomal basis of inheritance was published in 1936. He had returned to Germany in 1926, but had come back to the United States in 1932, on a second fellowship from the Rockefeller Foundation, spending the year at the California Institute of Technology in the company of a remarkable collection of geneticists: Morgan, Sturtevant, Dobzhansky, Bridges, Schultz, Emerson, Darlington, Kaufmann, and Lindegren. He had married an American citizen, Evelyn Sommerfield, in 1931. In 1933, when he was due to return to Germany, Hitler came to power, a development whose tragic implications for German Jews has been only too well documented. While Stern remained in the United States, Evelyn returned to Germany to seek some cautious advice from his colleagues. What she learned convinced them it would be wise not to return. (He became a U.S. citizen in 1939.) Stern accepted a temporary position at Western Reserve University, but quickly moved to the University of Rochester, where he was to remain until 1947, serving from 1941 to 1947 both as chairman of the Department of Zoology and chairman of the Division of Biological Sciences.

Shortly after he arrived at Rochester, he began to investigate what was then a puzzling phenomenon: the occurrence in female flies heterozygous for one or several sex-linked recessive alleles of epidermal spots manifesting the effects of one or all of these alleles or even, if the alleles were on different but homologous chromosomes, "twin spots," exhibiting the phenotypes associated with both alleles. A classical analysis revealed that the only consistent explanation required a previously unrecognized phenomenon, mitotic crossing-over. I would like to suggest that this paper was the end of the "chromosomal era" in the history of *Drosophila* as an experimental organism. It was to continue to provide insights on other problems, but the story of how its chromosomes behaved (in the classical sense) was now essentially complete.

In several autobiographical sketches, Stern has emphasized that he never attempted any grand research design, but simply "followed his nose." The beauty of *Drosophila* was how quickly one could move from one major issue to the next, providing you knew how to manipulate the fly stocks—at which Stern was probably second only to Muller. That nose led him unerringly to basic issues. Space permits mention of only three of the outstanding contributions subsequent to 1936 that depended on experiments with *Drosophila.*

1. *Isoalleles.* The recessive allele *cubitus interruptus* (*ci*) located in the fourth chromosome, causes a gap of variable length in the fourth wing vein. Working with strains thought to be isogenic except for fourth chromosomes of different origins, as well as with strains in which a deficiency of the region encompassing the *ci* locus was present, and manipulating temperature, Stern in 1943 demonstrated that normal alleles of *ci* differed greatly in their potency, as measured by their ability to modify the expression of the *ci* trait in heterozygotes or hemizygotes for this locus. He termed these different normal alleles "isoalleles." This demonstration of a range of genetic variation beyond that easily envisioned presaged (and may now find an explanation in) the demonstration years later of extensive inapparent biochemical variation. At that time there was still concern amounting to disbelief among some biologists and paleobiologists that the kinds of traits arising through mutation in *Drosophila* could possibly serve as the stuff of evolution. Since these alleles of small effect presumably arose from the mutational process, this direction of attention to traits of lesser effects played a signifi

cant role in what Mayr has termed "the evolutionary synthesis" (Mayr and Provine 1980).

2. *Genetic effects of low-level radiation.* During and after World War II, Stern, in collaboration with Spencer, Caspari, and Uphoff, was drawn into studies of low-level radiation effects, studies sponsored by the Rochester branch of the U.S. Army's Manhattan Engineering District. The question, in the context of the advent of the atomic bomb, was obvious: Was there a threshold in the genetic effects of radiation? The finding—now a cornerstone of radiation genetics—was clear: "Viewing all experiments together, it appears that radiation at low doses, administered at low intensity, induces mutation in *Drosophila* sperm. There is no threshold below which radiation fails to induce mutations" (Uphoff and Stern 1949). It was undoubtedly this background that led to a term (1950-1953) on the Advisory Committee to the Division of Biology and Medicine of the Atomic Energy Commission, a critical period in the development of the AEC's policy of broadly based research into radiation effects.

The work on the effects of low-level radiation had used sex-linked lethals as indicator traits. Since it was felt that radiation produces disproportionate numbers of lethals (as the normal spectrum of mutation is understood), an important question was the effect of these "recessives" when heterozygous. Given the ratio of heterozygotes to homozygotes predicted by the Hardy-Weinberg formulation, for a rare allele even a small heterozygote effect for an autosomally inherited recessive lethal could outweigh the impact of the occasional homozygote. Stern—with Carson, Kinst, Novitski, and Uphoff—in 1952 established that under their conditions, the average viability of heterozygotes for sex-linked lethals was 96.5 percent normal, a figure still standard. Their data did not permit any distinction between the effects on viability of spontaneous and induced mutation.

3. *The extent of cell autonomy in gene expression and the genetic control of patterns.* In 1947, Stern left Rochester to become professor of zoology and, in 1958, professor of genetics as well, at the University of California, Berkeley; he retired from these positions in 1970. At Berkeley he returned to an old problem; the basis of the attack had been laid with the 1936 paper on somatic crossing-over and segregation. These studies had demonstrated a high degree of cell autonomy in the expression of genetic constitution in *Drosophila.* On the other hand, transplantation experiments (with Hadorn) had demonstrated that the color of the vasa efferentia was dependent on that of the testis attached to them and not on the genetic constitution of the ducts themselves, and that the shape of the testis (spiral or oval) was dependent on the genetic constitution of the sperm ducts to which it was attached (1939-1941). In the 1950s and much of the 1960s, most of Stern's research efforts, often in collaboration with his students, were directed toward the difficult problem of the genetic control of differentiation, and especially of patterns, still using *Drosophila* as an experimental organism.

The various types of regularly arranged chaetae, so obvious when one inspects a fly, proved most useful in these interests. Studies with Hannah-Alava, employing genetic mosaics of various derivations, demonstrated a new level of complexity in embryological determination: Differentiation of the male sex-comb (a specialized row of chaetae) depended on a field effect within which the development of the sexcomb teeth was determined—down to very small patches of cells—by the sex (maleness) of the cells. These and other studies led to what Stern in 1954 termed the "prepattern hypothesis." Prepattern was a descriptive term for any kind of spatial differentiation in development, development being regarded as a succession of prepatterns. Within the prepat tern there are singularities, to which developing cells respond according to their genetic competences. Much of his later work on this subject, well summarized in 1978 by his colleague and collaborator Dr. C. Tokunaga, was directed toward defining, largely through the use of genetic mosaics, the nature of prepatterns, singularities, and competences. The familiarity with *Drosophila* mosaics these inquiries demanded inspired an omnivorous interest in mosaics of all types, reflected in the Prather Lectures at Harvard in 1965, in which he summarized his activities in this field.

One of Stern's last papers on *Drosophila,* in 1969, once again illustrated his ability to take full advantage of *Drosophila* as an experimental organism. The earlier studies on somatic cell crossing-over had not provided a clear approach to the relative frequency of somatic cell versus meiotic crossingover. Since then, in the 1950s, compound or complex loci had been recognized in *Drosophila melanogaster,* such as the "white eye" (*w*) locus, within which meiotic crossing-over could occur. As a manifestation of the remarkable degree of cell autonomy in *Drosophila,* each individual facet of its compound eye expresses its genotype as regards pigmentation independently of the other. Stern scored female flies heterozygous for two different *w* alleles between whose mutational sites the frequency of meiotic crossing-over had been determined, for red-colored spots in their white eyes. Four such spots were found in a total of 6,137 flies. No such spots were observed in the eyes of 27,557 controls. On the assumption that the red spots resulted from somatic cell recombination in a cell of the developing eye disc, and that the eyes of the flies scored collectively provided a minimum of 9×10^6 mitoses in which the results of somatic recombination could be observed, these 4 spots suggested a frequency of recombination between these mutational sites of less than 1 or 2 in

2×10^6 mitoses (1 if the exchange was nonreciprocal, 2 if reciprocal). This was 400 to 800 times less common than meiotic crossing-over in the same region.

Human Genetics

Technically, Stern's advent into the other field of genetics with which his name is so prominently associated, human genetics, dates from a paper entitled "Welche Moglichkeiten die Ergebnisse der experimentellen Vererbungslehre dafur, dass durch verschiedens Symptome charakterisierte Nervenkrankheiten auf gleicher erblicher Grundlage beruhen?", published in 1928 in *Nervenarzt.* It is a very clear statement, directed to physicians, of a principle now commonly accepted: Indistinguishable phenotypes may have very different genetic bases. His serious entry into the field, however, can be dated more precisely to 1939. That year he supervised his first graduate student seminar in the field of human genetics. As one of the half-dozen students who met weekly for a semester, I still have the list of papers he chose for review. Two impressions stand out. First, he had managed to select what little solid data existed; the contrast between the then and the now of human genetics, developments within a single generation, is simply staggering. Second, given the excesses of American eugenicists, and especially the Nazi racism from which he personally had suffered, one might have expected some bitterness and an occasional diatribe on these monstrous perversions; Stern kept the discussion all science.

This occasion must mark the beginning of the most successful textbook on human genetics ever written. In the course of three English editions (1949, 1960, 1973), *Principles of Human Genetics* sold 62,337 copies; there is no way to estimate accurately the number of copies sold of the German, Japanese, Spanish, Portuguese, Hindi, Polish, and Russian translations! Lest I seem to be equating success with sales, it should be made clear that the content of this book was a major factor in the coming of age of human genetics. Each edition was a major rewrite; the last, which appeared in 1973 and encompassed 891 pages, occupied much of his time during the last years of his tenure at Berkeley. Although Stern himself, in an autobiographical note written in 1974, attributed the genesis of his book primarily to the needs of premedical students, the psychoanalytically oriented must wonder if its scrupulous objectivity was not a Stern response to the perversion of genetic thought to which he had been so intimately exposed.

Stern also authored a dozen papers on human genetics, albeit none with the impact of his *Drosophila* work. He was for many years interested in Y-linked inheritance in man, and devoted his presidential address to the American Society of Human Genetics in 1957 to this topic. There he pointed out that of the fourteen traits that were candidates for Y-linked inheritance, all but one (hypertrichosis of the pinnae) were known from only a single pedigree—strange indeed from what we know of recurrent mutation. This fact—along with the ambiguities in most of the pedigrees—made him most suspicious of the validity of the evidence for such linkage, the single exception being hypertrichosis of the pinnae. His interest in this trait led to field work in India, his only field work in human genetics. The resulting 1964 publication, with Centerwall and Sakar, left the issue open, simple autosomal dominance or recessiveness being possible alternative explanations.

The Man

Stern was a small man whose whole being was dominated by eyes that could be both probing and dancing simulta neously. His tact and consideration of others were legendary. Yet beneath that gentle exterior was one of the toughest, most uncompromising (on principle) men I have ever met.

He and his wife Evelyn (neé Sommerfield) had three daughters, Hildegard, Holly Elizabeth, and Barbara Ellen. The successive generations of graduate students who passed through the Stern home as these children were maturing sensed an extraordinary warmth of family life, and as we passed on to family responsibilities ourselves, we also realized the extra burden we had inflicted on Mrs. Stern in connection with the many meetings held in their home, and the charm and equanimity with which she treated us.

Stern's ability to present complex issues with simplicity and lucidness made him a popular teacher and sought-after lecturer. This ability was not restricted to his scientific peers and graduate students; at Berkeley, his course on general genetics regularly drew between 120 and 150 students, and his course on human genetics for nonscience majors for years drew some 300 students. (He also found time for genetic counseling.) The series of twelve half-hour films on genetics that he narrated, sponsored by the American Institute of Biological Sciences in 1961, has been used all over the world. Two lectures that illustrate the why of this popularity are the Prather Lectures delivered in 1965 at Harvard, entitled "Genetic Mosaics and Other Essays," and "Genes and People," delivered in 1966 as a special lecture for laymen in connection with the Third International Congress of Human Genetics.

As best the record can be reconstructed (primarily by his longtime associate, Dr. C. Tokunaga), Stern supervised the doctoral theses of some thirty students, and at least another dozen individuals had postdoctoral associations of varying durations. We, his students, have had very different scientific experiences with Stern, depending on the nature of his work as we passed through his laboratory. But one experience we have all shared has been the exposure to an uncompromising perceptiveness and adherence to standards. Nevertheless, even as he drew attention to some less than brilliant act on our part, he somehow left us reassured there was still hope we would make it someday (see also Lucchesi 1983). During my first year of graduate work with Stern, I committed a major genetic faux pas. I do not remember what it was, but it would be impossible to forget how Stern handled it. Having straightened me out, he paused a minute. I was waiting for the next blow to fall when he smiled and said, "You know, Jim, in Germany we have a proverb—great men make great mistakes." The application of this amazing quality was not restricted to his graduate students—how many nervous young scientists delivering an almost-first paper have had their day made by the right comment from Stern.

More than most scientists, Stern kept an eye on the historical and philosophical aspects of his discipline. It was he who in 1943 directed geneticists' attention to Weinberg's independent formulation of what we now term the Hardy-Weinberg Law. Likewise, in 1950 he felt obligated to point out how Boveri, far from having delayed the recognition of the chromosomal basis of genetic linkage, as suggested by Punnet in his historical account in 1950, had with amazing insight predicted genetic linkage in 1904. He was cosignatory on a little note in *Lancet* that led to the replacement of the unfortunate term "mongolian idiocy" by "Down's syndrome." Finally, impressed by inaccuracies in the available translations into English of Mendel's papers and the source material concerning their rediscovery, he collaborated with Sherwood on a *Mendel Source Book,* published in 1966. And repeatedly during his career, he returned to the thoughts expressed in "The Journey, Not the Goal," in 1944, a plea for a "a wider and deeper realization of the value of science as an expression of a de tached essence of human existence." While of course aware of the significance of his own contributions, his two autobiographical notes (and all his papers) are scrupulous in their recognition of his intellectual debts to others (Stern 1971, 1974).

Later Years

Honors came early to Stern, and continued to accumulate until his final, incapacitating illness. He was in turn president of the Genetics Society of America (1950), the American Society of Human Genetics (1957), the American Society of Naturalists (1962), and the Thirteenth International Congress of Genetics (1973). He served as editor of *Genetics* from 1947 to 1951. Elections to learned societies included the National Academy of Sciences (1948), the American Philosophical Society (1954), and the American Academy of Arts and Sciences (1959). He was twice a fellow of the Guggenheim Foundation (1955, 1963). Awards included the Kimber Genetics Medal of the National Academy of Sciences in 1963, the Mendel Silver Medal of the Czechoslovakia Academy of Sciences in 1965, the Adair Award of the American Gynecological Society in 1967, and the Allan Award of the American Society of Human Genetics in 1974. He was Benjamin Franklin Lecturer at the University of Pennsylvania in 1951, Faculty Research Lecturer of the University of California in 1964, and Prather Lecturer at Harvard University in 1965. He received honorary degrees from McGill University (1966) and the University of Munich (1972). His bibliography encompasses five books (*Multiple Allelie,* 1930; *Faktorenkoppelung u. Faktorenaustausch,* 1933; *Principles of Human Genetics,* 1949, 1960, 1973; *The Origin of Genetics,* with Eva Sherwood, 1966; and *Genetic Mosaics and Other Essays,* 1968) and approximately 230 individual coauthored journal publications.

Earlier it was suggested that Stern's passing severs almost the last personal link with the "fly room" era. There is perhaps another symbolism in his death. During his productive years—mid 1920s to the end of the 1960s—a front-rank geneticist was expected to be conversant with *all* important contemporary genetic developments. This Stern was, and if his encyclopedic memory failed him on a point, his ability—in his cluttered office—to put his hands on just the right paper was amazing. The incredible burgeoning of genetics in the last twenty years seems to have signaled the end of the comprehensive intellects in this field. Indeed, read carefully, the last edition of his text on human genetics (1973) was showing the strains of one man writing a text doing justice to all the developments even in a single field of genetics.

The diagnosis of Parkinson's disease was made in 1970. He confronted the slow progression of this insidious and tragic disorder with insight and dignity, withdrawing from intellectual activities before the cognitive impairment characteristic of the late stages of the disease was apparent to the uninitiated. His last major presentation was, fittingly, the Presidential Address to the Thirteenth International Congress of Genetics, in 1973. In 1974, upon the receipt of the Allan Award of the American Society of Human Genetics, he declined to give the usual address, saying quietly (and well in advance) "Jim, I am not up to it; please speak for me." He died of cardiac failure complicating his Parkinsonism on October 23, 1981.

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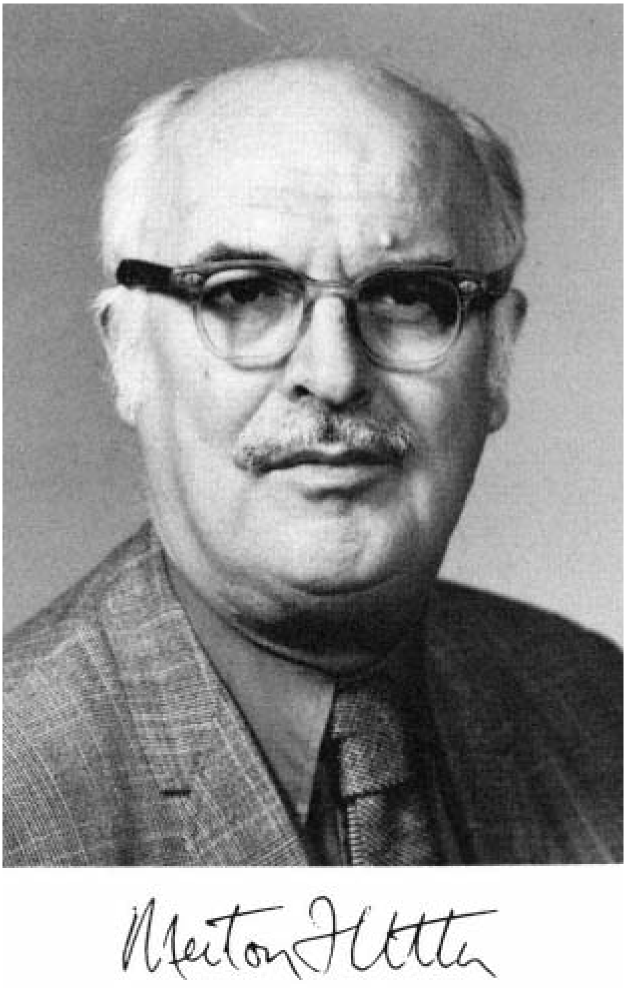

Merton Franklin Utter

March 23, 1917-November 28, 1980

By Harland G. Wood and Richard W. Hanson

The most significant contribution to biochemistry made by Merton F. Utter was his demonstration that certain reactions of gluconeogenesis differ from those of glycolysis. For many years it was widely held that the synthesis of glucose (gluconeogenesis) in mammalian liver occurs by reversal of the Embden-Meyerhof pathway by which glucose is converted to pyruvate and lactate (glycolysis). Merton Utter and his coworkers showed that this concept is incorrect. They discovered phosphoenolpyruvate carboxykinase and pyruvate carboxylase, two enzymes that in concert convert pyruvate to phosphoenolpyruvate by a sequence that differs from the glycolytic pathway.

This discovery opened new vistas in the study of metabolism, and over the past decade it has become evident that the two enzymes discovered by Utter and coworkers are also important in the regulation of both carbohydrate and lipid metabolism. Utter, together with Dr. Bruce Keech, demonstrated that acetyl-CoA regulates the activity of pyruvate carboxylase, thus providing one of the first examples of allosteric control of an enzyme. Furthermore, the rate-limiting step in gluconeogenesis is catalyzed by phosphoenolpyruvate carboxykinase, whose levels in mammalian liver and kidney are regulated by insulin, glucagon, epinephrine, and gluco corticoids. This enzyme has been extensively studied as a model for the action of these hormones on gene expression in mammalian tissues. Prior to his death, Utter's studies had increasingly centered on the interface between disease processes and basic biochemistry. His laboratory was considered one of the leading centers studying inborn errors in the metabolism of pyruvate, and his collaboration was constantly sought by clinical investigators anxious to verify the absence of specific enzymes in patients suffering from various diseases. The scope of his science was broad. He stood for precise and excellent experiments, and his advice was sought on a wide variety of subjects. His was a keen intellect, but he was always modest and friendly, and was possessed of a sharp wit. Merton Utter's interests extended to all aspects of life: science, sports, politics, literature, and the arts.

Utter's Background

Merton Franklin Utter was born at Westboro, Missouri, on March 23, 1917. His parents were Merton Franklin Utter, Sr., and Gertrude R. McMichael Utter. His father and grandparents, Mr. and Mrs. L. P. Utter, had moved to Missouri from Trempealeau, Wisconsin. His maternal grandparents were Mr. and Mrs. A. R. McMichael of Coin, Iowa. Most of his ancestors came to New England and New York from the British Isles in the seventeenth and early eighteenth centuries. When he was a few months old, Merton's parents moved to New Market, Iowa, where his father was a banker, and his early school years were spent there. His mother gave piano lessons and played piano and organ for churches most of her life. It was from her that Merton acquired a deep and lifelong love of music.

In 1930, when Merton was in the eighth grade, the family moved to Coin, Iowa. He graduated from the high school there in 1934 and entered Simpson College at Indianola, Iowa. The death of his father in an auto accident in the summer of 1935 interrupted his college studies briefly, but he graduated from Simpson in 1938, supporting himself through scholarship aid and by managing the campus bookstore. He was a fine athlete and excelled in track and basketball. At Simpson he distinguished himself in the field of chemistry, and on the advice of his professor he enrolled in graduate school at Iowa State University at Ames. In 1942, with the aid of fellowships, he was able to complete the work for his Ph.D. degree, which he received in microbiology in the laboratory of Dr. C. H. Werkman. In that year he was appointed an instructor in bacteriology.

On September 2, 1939, while at Ames, he married Marjorie Manifold, whom he had known since high school. Members of her family were also longtime residents of Coin and vicinity (Page County). In 1944 the Utters moved to Minneapolis, where he was assistant professor of physiological chemistry at the University of Minnesota, and in 1946 they moved to Cleveland, Ohio, where he was appointed associate professor of biochemistry at Western Reserve University School of Medicine. A son, Douglas Max Utter, was born on December 8, 1950. In 1956 Merton Utter was promoted to professor, and in 1965 he became chairman of the Biochemistry Department and continued in that position until 1976. Thereafter he devoted his full time to research and teaching in the Department of Biochemistry.

He and his family spent three years abroad on leave of absence from Case Western Reserve University. In 1953, he traveled with his family to Adelaide, Australia, where he was a Fulbright Fellow at the University of South Australia. In 1960 he served as visiting professor at Oxford University in England and in 1968 at the University of Leicester. Recently, Sir Hans Kornberg reflected on the year spent by the Utter family in Leicester.

> It seemed appropriate that 7 years later they, Marge, Mert and Doug, should come back to Leicester. They lived around the corner from us and every morning either Mert would come and ring my doorbell and I would hastily wipe the last vestiges of breakfast toast off my face and then walk with him across the park; or I would call for him on wet days in a monstrous car, a 12 seater. When you walk with someone for a whole year you get to know him pretty well. Mert had a tremendous interest in the comparative side of biological phenomena. We used to talk about this sort of thing trying to discover the reason why, for example, you have a perfectly good enzyme, pyruvate carboxylase, which a perfectly good bacteria like *E. coli* should resolutely refuse to use, and instead it used PEP carboxylase but used the same mechanism of control. And we would play games like *what if,* and *supposing that.* This to me brought out the one feature of my association with Mert which I remember distinctly with the strongest affection. He was a tremendous person to be with because he would toss ideas around and he, like me, had this fatal fascination for playing on words. We would usually end our walks giggling helplessly as we went into the department where they must have thought us ready for certification as lunatics.

A Summary of his Research

Early Research. Utter's first scientific paper was published in the *Iowa State College Journal of Science* (1940) and was entitled "The Preparation of an Active Juice from Bacteria." Utter was always modest and unpretentious. A title such as, "A Unique and New Procedure for Preparation of Active Enzymes from Bacteria" would have been more to the point and sounded more sophisticated, but that was not his style. The solubilization of bacterial enzymes was a significant accomplishment. At that time, soluble enzyme systems capable of fermenting carbohydrates had not been demonstrated in bacteria, and consideration of their intermediary metabolism was in large part based on what was known from studies of enzymes from yeast and animal tissue.

Those were the "horse and buggy" days of biochemistry. The citric acid cycle had just been described by Krebs, and many details of the Embden-Meyerhof pathway were not completely understood. There were no commercial sources of enzymes or of coenzymes such as adenosinetriphosphate (ATP) and nicotinamide diphosphate and triphosphate (NAD and NADP). It was a time of "do-it-yourself or go without." To solubilize enzymes, bacteria were mixed with ground glass and the mixture was forced between the interface of two tightly interfitting cones. For this purpose, a glass tube was sealed to the neck of one Erlenmeyer flask and the bottom of the flask was cut off. A second Erlenmeyer flask was sealed off at the neck so that it fit inside the open end of the first Erlenmeyer flask. The inner flask was attached to a motor to cause it to rotate within the outer flask. A mixture of the bacteria, together with ground glass, was placed in the tube of the outer flask, and the mixture was forced, using considerable effort, from the tube between the rotating cones using a plunger. These were the depression years, so if a beaker was broken, it was saved and the glass was put in a ball mill to replenish the ground glass. This procedure for the preparation of bacterial enzymes was used for many years by researchers in C. H. Werkman's department.

At about the same time, a mass spectrometer for measurement of ^{13}C was being constructed by the group in the laboratory, as well as a thermal diffusion column five stories high for concentration of this stable isotope. It was the ingenuity and hard work of graduate students such as Merton Utter that made the laboratory of C. H. Werkman, which was situated in the middle of the farm belt of Iowa, a leading center for study of microbial metabolism.

This is the environment in which Merton Utter started his research. He had a nine-month fellowship that paid $50 monthly. His wife Marjorie worked as a secretary with Dr. Theodore Schultz in the Department of Economics at Iowa State College, now Iowa State University. Interestingly, Dr.

Schultz, who by then had moved to Chicago, was a winner of the Nobel Prize in Economics in 1979.

Merton Utter's research was truly pioneering. In 1941 a paper was published in the *Journal of Bacteriology* entitled "The Occurrence of the Aldolase and Isomerase Equilibria in Bacterial Metabolism." Aldolase and isomerase are two important enzymes of carbohydrate metabolism. There were two more papers published in *the Journal of Biological Chemistry* in 1942: "Effect of Metal Ions on the Reactions of Phosphopyruvate by *Escherichia coli"* and "The Dissimilation of Phosphoglyceric Acid by *Escherichia coli."* Phosphoglyceric acid had been shown at that time to be a key compound in the metabolism of carbohydrate by yeast and mammalian tissues. In 1943 Utter published "The Role of Phosphate in the Anaerobic Dissimilation of Pyruvic Acid" and in 1944 the "Formation and Reactions of Acetylphosphate in *Escherichia coli"* and "Reversibility of the Phosphoroclastic Split of Pyruvate." (At that time, Fritz Lipmann had just discovered the role of acetylphosphate in metabolism.) Anyone who is familiar with the history of biochemistry recognizes from the titles that Merton Utter's early work was at the forefront of biochemistry, just as it has been at the forefront of carbohydrate metabolism to this day. Methods of isolation of enzymes and study of their properties were in their infancy. Utter's studies helped to show that bacteria share similar metabolic pathways with mammals and that all forms of life exist in large part by the same biochemical processes. Soon bacteria were to become the major subject for study of intermediary metabolism and molecular biology.

Studies on Fixation of CO_2. The fixation of CO_2 by heterotrophic organisms was discovered by H. G. Wood and C. H. Werkman in 1936. Later they proposed that the fixation occurred as follows:

$$^{*}CO_2 + CH_3\ CO\ COOH \rightarrow HOO^{*}C\ CH_2\ CO\ COOH$$

This reaction became known as the Wood and Werkman reaction. It was not until 1948, however, that S. Ochoa, A. H. Mehler, and A. Kornberg purified an enzyme that fixed CO_2, to form a dicarboxylic acid. Subsequently, the enzyme was named the malic enzyme and shown to catalyze the following reaction:

$$CO_2 + \text{pyruvate} + \text{NADPH} \xrightleftharpoons{\text{malic enzyme}} \text{malate} + \text{NADP}$$

Following this discovery, Ochoa and collaborators suggested that this enzyme catalyzed the primary reaction in the fixation of CO_2 and that oxalacetate is formed by coupling the following two reactions:

$$CO_2 + \text{pyruvate} + \text{NADPH} \xrightleftharpoons{\text{malic enzyme}} \text{malate} + \text{NADP}$$

$$\text{Malate} + \text{NAD} \xrightleftharpoons{\text{malic dehydrogenase}} \text{oxalacetate} + \text{NADH}$$

$$\text{Sum: } CO_2 + \text{pyruvate} + \text{NADPH} + \text{NAD} \longrightarrow \text{oxalacetate} + \text{NADP} + \text{NADH}$$

Ephraim Racker summarized the status of work in this field at a meeting on CO_2 fixation in 1950, when he proposed a toast to the "wouldn't work reaction."

Although the enzymatic basis for the Wood and Werkman reaction continued to be elusive, Utter and K. Kurahashi showed that chicken liver forms oxalacetate without the involvement of malic enzyme. They isolated a new enzyme, P-enolpyruvate carboxykinase, which catalyzes the formation of oxalacetate with fixation of CO_2, using guanosine di-and triphosphate (GDP and GTP) as high-energy intermediates:

$$\text{P-enolpyruvate} + CO_2 + GDP \xrightleftharpoons[]{\text{P-enolpyruvate carboxykinase}} \text{oxalacetate} + GTP$$

Utter's Discovery of the Mechanism of Conversion of Pyruvate to P-enolpyruvate. It was the finding of P-enolpyruvate carboxykinase that launched Utter into the studies of gluconeogenesis. He was aware that because of the high, negative free energy change it was unlikely that P-enolpyruvate was formed from pyruvate by a simple reversal of the pyruvate kinase reaction.

$$\text{P-enolpyruvate} + ADP \xrightarrow{\text{pyruvate kinase}} \text{pyruvate} + ATP$$
$$(\Delta G^{0'} = -7 \text{ kcal/mole})$$

As a possible solution, both H. A. Krebs and Utter (1954) independently proposed that pyruvate might be converted to P-enolpyruvate by the combined action of the malic enzyme and P-enolpyruvate carboxykinase by the following sequence:

$$\text{Pyruvate} + CO_2 + NADPH \xrightleftharpoons[]{\text{malic enzyme}} \text{malate} + NADP$$

$$\text{Malate} + NAD \xrightleftharpoons[]{\text{malic dehydrogenase}} \text{oxalacetate} + NADH$$

$$\text{Oxalacetate} + GTP \xrightleftharpoons[\text{carboxykinase}]{\text{P-enolpyruvate}} \text{P-enolpyruvate} + GDP + CO_2$$

$$\text{Sum: Pyruvate} + GTP + NADPH + NAD \longrightarrow \text{P-enolpyruvate} + GDP + NADP + NADH$$

The thermodynamics of this sequence are not particularly favorable, but by coupling the oxidation of NADH to other reactions it was considered possible to maintain a high ratio of NADPH/NAD, thereby favoring the synthesis of the P-enolpyruvate.

It was an investigation of the above reaction sequence that led to the discovery of the major anaplerotic enzyme, pyruvate carboxykinase. Utter and coworkers then found that mitochondria from chicken liver contained only trace amounts of either pyruvic kinase or malic enzyme, but they could still form significant amounts of P-enolpyruvate from pyruvate. These experiments provided the first clear evidence that neither of these enzymes was required for the net synthesis of P-enolpyruvate. Since Utter knew that P-enolpyruvate could be formed from oxalacetate, it was natural to look for an enzyme that could form oxalacetate from pyruvate. In 1963 Utter and D. B. Keech found such an enzyme in the mitochondria of chicken liver (later named pyruvate carboxylase), which catalyzed the direct carboxylation of pyruvate. Utter had thus found the enzymatic basis of the *"wouldn't work reaction,"* twenty-five years after it had been postulated as a possible mechanism for the formation of dicarboxylic acids by CO_2 fixation. Pyruvate carboxylase, when coupled with Penolpyruvate carboxykinase, catalyzed the formation of Penolpyruvate as illustrated below.

$$\text{Pyruvate} + CO_2 + \text{ATP} \xrightarrow{\text{pyruvate carboxylase}} \text{oxalacetate} + \text{ADP} + P_i$$

$$\text{Oxalacetate} + \text{GTP} \underset{\text{carboxykinase}}{\overset{\text{P-enolpyruvate}}{\rightleftharpoons}} \text{P-enolpyruvate} + CO_2 + \text{GDP}$$

$$\text{Sum: Pyruvate} + \text{ATP} + \text{GTP} \longrightarrow \text{P-enolpyruvate} + \text{ADP} + \text{GDP} + P_i$$

This sequence is energetically favorable because it combines cleavage of two high-energy phosphates from ATP and GTP to drive the overall synthesis of P-enolpyruvate. A beautiful summary of this research was published in a 1963 article by

Utter in the *Iowa State College Journal of Science,* which contained a compilation of papers by C. H. Werkman's students. Today this pathway of P-enolpyruvate formation from pyruvate is widely held as the key, pacesetting step in gluconeogenesis. The degree of the regulation of the two enzymes in this sequence, pyruvate carboxylase and P-enolpyruvate carboxykinase, now serves as a model for control of metabolic pathways and remains a major legacy of Merton Utter's scientific work.

Structure of Biotin Enzymes. One portion of Utter's research that had a large effect was his 1966 study, in collaboration with R. C. Valentine, N. C. Wrigley, M. C. Scrutton, and J. J. Irias, using electron microscopy to determine the structure of pyruvate carboxylase from chicken liver. This was one of the earliest applications of electron microscopy for investigation of the quaternary structure of enzymes. Negative staining techniques showed square-planar tetramers with vivid clarity. It was these studies that convinced one of us (H. G. W.) to undertake similar studies with another biotin enzyme, transcarboxylase, and no doubt induced others to adopt the procedure.

That pyruvate carboxylase was being visualized seemed compelling. Pyruvate carboxylase was known to contain four biotins, which was in accord with the observed tetrameric structure. Also, estimates from the dimensions of the profiles of the four subunits were in accord with the observed molecular weight of the enzyme. These square tetramers were observed in pyruvate carboxylase preparations from the livers of a variety of animals, including the chicken, turkey, beef cattle, and calf. In addition, Gottschalk and coworkers (*European Journal of Biochemistry,* 64 [1976]:411-21) at the University of Göttingen, Federal Republic of Germany, reported that pyruvate carboxylase of rat liver had a square tetramer shape. Finally and most convincingly, pyruvate carboxylase was known to be cold sensitive. The enzyme dissociates to its subunits in the cold, with loss of enzymatic activity; upon rewarming, the subunits reassociate with accompanying return of activity. The square planar tetramer dissociated to subunits in the cold, which were observed in the electron microscope; on rewarming, they reassociated to the square tetramer.

To Utter's amazement and chagrin, thirteen years later he and his coworkers found that the square-shaped tetramers were not pyruvate carboxylase. It was a minor, highly visible impurity present in purified pyruvate carboxylase. The pyruvate carboxylase dissociated during preparation of the grids to faintly visible material, leaving the minor contaminant highly visible. Independently and at the same time, N. H. Goss, P. Y. Dyer, D. B. Keech, and J. C. Wallace at the University of Adelaide in Australia found that the square tetramer is not pyruvate carboxylase. Utter and his former collaborator, D. B. Keech, by mutual agreement, published their new results on the correct structure of pyruvate carboxylase in the same issue of *the Journal of Biological Chemistry.*

The identity and function of the square tetramer remains a mystery. A better design of a protein could not be constructed by the Devil himself to mislead a brilliant scientist. Although there is not complete agreement, the overall configuration of pyruvate carboxylase from chicken, sheep, and rat appears to be a tetrahedron-like structure consisting of two pairs of subunits in different planes orthogonal to each other, with the opposing pairs of subunits interacting on their convex surfaces (F. Mayer, J. C. Wallace, and D. B. Keech, *European Journal of Biochemistry,* 112 [1980]:265-72).

Inborn Errors in the Enzymes of Pyruvate Metabolism. Later in his career, Merton Utter turned his attention to the causes of lactic acidosis in children. Many of the diseases in this ill-defined category of childhood disorders are thought to in volve inborn errors of enzymes in pyruvate metabolism. During the last decade of his life, Utter and his coworkers began a systematic study of these enzymes in human populations. He developed sensitive enzymatic assays for pyruvate dehydrogenase, pyruvate carboxylase, P-enolpyruvate carboxykinase, and pyruvate kinase using easily obtained tissues, such as cultured skin fibroblasts, reticulocytes, or lymphocytes. He was able to demonstrate, for example, that contrary to the prevailing opinion, Leigh's disease did not involve a deficiency in pyruvate carboxylase. His skill as an enzymologist was a major factor in the development of a standard assay for pyruvate dehydrogenase in human tissues that accurately and reproducibly measured the true basal rate of activity of this enzyme. His laboratory had, at the time of his death, become a reference point for many clinicians interested in collaboration in determining the absences of a specific enzyme in pyruvate metabolism in patients.

Personal Aspects in Merton Utter's Life

Merton Utter was a man of great personal charm and dignity. His life was dedicated to scholarship and the ideals of university education. He loved books and all his life read widely, particularly in history, biography, and politics. He was a quiet and unassuming gentleman whose advice was often sought by his colleagues and students. He was a man of unfailing charity who did not speak ill of others and whose personal qualities will be long remembered by all who knew him. Paul Berg, a student in the Department of Biochemistry at Western Reserve University in the 1950s, commented recently:

> One of the things about Mert which I will always remember is that while we learned that science was exciting and pertinent and that it required a kind of commitment, day and night preoccupation, Mert made science fun. In chatting with us, he would see the less intense side of things

> as well as the importance of what we were trying to talk about. He would chat about music, art, baseball. He made being in the lab a great joy. It was always a pleasure for me to come into the lab, morning, noon and night and find Mert there.

Merton Utter served with distinction as an associate editor of the *Journal of Biological Chemistry* and helped to guide the editorial policies of the journal during the period of its most rapid expansion. He was a member of numerous study sections and national committees, and his quiet and judicious manner made him a valued member of numerous national panels concerned with research policy.

Merton Utter spent virtually his complete university career as a member of the Department of Biochemistry at Case Western Reserve University School of Medicine. He trained numerous biochemists and medical students during his career as a research scientist and teacher. His knowledge of biochemistry was encyclopedic, and he was a superb teacher. His sudden death at the age of sixty-three was a serious blow to those who knew and loved him. A symposium was held at Case Western Reserve University School of Medicine in May of 1982 to honor the memory of Merton Utter. The sentiments of all who attended were expressed in a letter written at the time of his death by Dr. Albert S. Mildvan:

> It was with shock, and with the deepest of sadness that I read your letter today, telling of our loss of Mert Utter. His life's work can only be described as monumental. I am grateful to have had the privilege of collaborating with this great scientist and person.
>
> In thinking of Mert, I recall the pleasure of hearing his witty lectures on his research. With profound modesty he would make his great discoveries sound like the result of a series of unexpected accidents. This didactic technique greatly encouraged and inspired the next generation of scientists, indeed all of us, who try to emulate this high example.

HONORS AND DISTINCTIONS

Degrees

B.A., Simpson College, 1939
Ph.D., Iowa State University, 1942

Professional Appointments

Instructor in Bacteriology and Research, Associate in the Agricultural Experiment Station, Iowa State University, 1942-1944

Assistant Professor in Physiological Chemistry, University of Minnesota, 1944-1946

Associate Professor of Biochemistry, Western Reserve University, 1946-1956

Professor of Biochemistry, Case Western Reserve University, 1956-1980

Director, Department of Biochemistry, Case Western Reserve University, 1965-1976

Professional Activities

Metabolic Biology Panel, National Science Foundation
Biochemistry Study Section, National Institutes of Health
Program Project Committee, AMDD Institute, National Institutes of Health
Associate Editor, *Journal of Biological Chemistry*
Editorial Board, *Journal of Biological Chemistry*
Editorial Advisory Board, *Biochemistry*
Editorial Board, *Trends in the Biochemical Sciences*
Biochemistry Panel, National Board of Medical Examiners
U.S. Representative, International Union of Biochemistry

Honors

Fulbright Senior Research Fellow (Australia), 1953-1954
Paul Lewis Award in Enzyme Chemistry, 1956
National Science Foundation Senior Research Fellow (Oxford), 1960-1961
National Science Foundation Senior Research Fellow (Leicester), 1968-1969
National Academy of Sciences, 1973
American Academy of Arts and Sciences, 1972

Professional Societies
American Society of Biological Chemist
American Association for the Advancement of Science
American Chemical Society
American Society of Microbiologists
Biochemical Society (England)
New York Academy of Sciences
Society of Experimental Biology and Medicine

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John Hasbrouck Van Vleck

March 13, 1899-October 27, 1980

By P. W. Anderson

John Hasbrouck Van Vleck was the most eminent American theoretical physicist between J. Willard Gibbs and the postwar generation. He has often been characterized as the "father of modern magnetism," but his influence was in fact much wider: He played a vital role in establishing the modern fields of solid-state physics, chemical physics, and quantum electronics. Many generations of students were influenced by his unique teaching style, and he made important administrative contributions at a crucial time in the history of Harvard University.

Family and Early Years

The Van Vleck family is of the patrician Dutch stock that has given the nation three presidents, among other eminent citizens. "Van" (as he was always known) was proud of his ancestry, which had been traced by an aunt[1] to prosperous burghers of Maestricht in the sixteenth century. One of the family, Tielman Van Vleck, was an eminent citizen of the Dutch colony of Nieuw Amsterdam and founded Jersey City.

Van's immediate family was very distinguished academi

[1] More details will be found in The Royal Society memoir by B. Bleaney.

cally. His grandfather, John Monroe Van Vleck, was professor of mathematics and astronomy at Wesleyan University in Middletown, Connecticut, from 1853 to 1904, serving as acting president on two occasions. John Monroe's brother, Joseph Van Vleck, a successful New Jersey businessman, donated an observatory to Wesleyan in his honor. At the dedication ceremony in 1916, his son, E. B. Van Vleck, spoke and the young J. H. Van Vleck unveiled the memorial plaque.

All of John M. Van Vleck's four children were mathematicians, including Van's father Edward Burr Van Vleck (1863-1943). E. B. Van Vleck took a doctorate at Göttingen in 1893, taught for two years at Wisconsin, and then went to Wesleyan (1895-1905). He married Hester Raymond of Middletown in 1893, and here John Hasbrouck Van Vleck, his only child, was born on March 13, 1899. From 1905 to 1929 Edward Burr Van Vleck was professor of mathematics at the University of Wisconsin in Madison, Wisconsin, where the mathematics building is named Van Vleck Hall in his honor. He was eminent in his field and highly respected at Wisconsin. He was a member of NAS, president of the American Mathematical Society, and recipient of four honorary degrees. His lectures were noted for their formal clarity.

The E. B. Van Vleck home was a cultivated and a prosperous one, since he inherited a portion of his uncle's fortune. He built up a notable art collection, especially of Japanese woodblock prints but also of other objects of beauty. It is said that a number of the prints were acquired from Frank Lloyd Wright. They were collected during the building of the Imperial Hotel and sold to repay debts. Edward Burr and his wife Hester read voraciously and traveled widely, so that "the galleries, churches, and mountains of Europe were equally familiar." In this atmosphere Van absorbed his interest in travel and his deep cultivation very naturally.

Except for a few periods spent during his father's sabbat icals in schools or kindergartens abroad, Van was educated in the Madison public schools and went directly on to the University of Wisconsin. He did not recall any particular partiality toward science or mathematics as a boy, nor did his father make a special effort to interest him in advanced topics of mathematics, except for advising him to be sure to take mathematics through calculus in college. Reading his own remarks about this period of his life, one has the impression of a very normal boy with a rather matter-of-fact precocity. His interest in American football began early at Wisconsin, and he claimed that the Wisconsin song "On Wisconsin" was first sung at his first game in 1909 (information on this is contained in an article of his on the history of football songs). He played in the Wisconsin marching band from 1916 until 1918—his instrument is not recorded, but it was probably the flute, which he played later as a young assistant professor. His well-known interest in railroads began early: It was when he was about seven that he first spent a period of recuperation on one of his parents' European trips learning the relevant railroad schedules, and thereafter the family never again had to consult a timetable. In college, an early interest in French was turned off by his self-confessed "miserable" accent, and in geology by the obtuseness of a professor who "required all triangles to be solved graphically."

In fact, until late in his college career, Van seems to have seen himself as a bit of a dilettante. His youthful preference had been not to go into the academic life, and he recorded in his reminiscences that "serious young men took engineering rather than math or physics, where most of the students were girls." Just as he rebelled at solving triangles graphically, he evaded the physics senior thesis—involving experimental work, including, worst of all possible fates, glassblowing—by joining a debating team and arguing successfully against the government ownership of railroads. His stated reason (in

"Reminiscences of the First Decade of Quantum Mechanics," 1971) for taking physics was the otherwise light course load and the idea that mathematics would involve him in his father's courses, which would not be "cricket." He did, however, make Phi Beta Kappa in his junior year, so one is permitted a great deal of skepticism about the rather frivolous motivations he gives for his choice of careers. I note also the inclusion of an "unlisted reading course in Molière" in the accelerated program he finally completed—surely not a common interest for a young scientist in the Middle West.

Early Career in Physics

Van's consciousness of physics seems to have been first raised by the experience of a course on kinetic theory taken from L. R. Ingersoll in his third year at Wisconsin. He was not well prepared because he had put off the calculus to this same year, but he sat near Warren Weaver who was taking the course (though on the junior faculty) and was a very vocal critic of the textbook used. This stimulated Van's interest enormously. In the next year he took a course from Professor March on dynamics that was his introduction to genuine theoretical physics and showed him very clearly the course of his future career.

A fortunate accident took him to Harvard for the last semester of the 1919-20 college year: His father was spending a sabbatical there, and urged him to finish up at Wisconsin in three-and-a-half years and come take courses at Harvard. He started out with three courses in math and physics and one in railroad administration. This latter convinced him that he "would not get on very fast in the railroad business." The courses that influenced him positively were by Bridgman and Kemble. He felt Bridgman's operational philosophy, while not explicitly stated, was very much implicit in the attitude to physics that he acquired, while Kemble was one of two or three teachers in the United States concerned about quantum theory. He was influenced negatively as well by a formal mathematical course in differential equations, a replacement for his father's course, which he had been unwilling to take; this added to his lifelong distaste for empty formalism.

Thus, almost without conscious decision, he found himself a full-fledged Ph.D. student at Harvard. He quickly completed the requirements for an M.A. (1921) and finished a Ph.D. thesis under Kemble (1922). He stayed on as instructor with Kemble for one more year, leaving for Minnesota in 1923.

Early Days of the Quantum Theory: Harvard (1920-23) and Minnesota (1923-28)

His thesis topic attacked one of the truly knotty problems of the old quantum theory: the attempt to come up with a method of quantization that would give reasonable results for the helium atom. Once the hydrogen atom was more or less dealt with, and simple harmonic motion understood with Debye's and Planck's laws, the next stage was clearly more complex atoms. At about the same time, several European physicists were attempting the problem, and Van's paper was essentially equivalent to results of Bohr, Kramers, and Kronig. In short order there followed a sequence of papers on various aspects of spectroscopy using the old quantum theory, and then a book, *Quantum Principles and Line Spectra,* finished in late 1925.

During the Harvard years, Van was in constant touch with John Slater, who finished a mostly experimental thesis with Bridgman at about the same time, and then left for Copenhagen to work for a year with Bohr. I think it is fair to say that this little American group kept in remarkably close touch with the enormous activity centered in Central Europe that was to lead to the new quantum mechanics, and it contributed at least two papers of real significance, one of which was Van's paper of 1924 on the correspondence principle for absorption. This paper comes tantalizingly close to the kind of considerations that led to Heisenberg's matrix mechanics; it was one of the few papers to attack the intensity question that was the key failure of old quantum theory. Van maintained close contact with European physics, and was fortunate enough to accompany his parents to Europe in 1923; during the trip he made time to visit Bohr and had extended discussions with Kramers in Holland.

'I'he University of Minnesota invited both Van and Gregory Breit to come as assistant professors in 1923. This was a unique opportunity. Only graduate teaching was required, and he would have someone to talk with, because Breit's interests were very close to Van's, so he left the annual instructorship available to him at Harvard with no apparent reluctance. (Slater got the only permanent faculty job!) He was at Minnesota for five years, rising to associate professor in 1926 and full professor in 1927. On June 10, 1927, he married Abigail Pearson of Minneapolis. Of his courtship he remarked that they both loved dancing and that her ignorance of his work assured him that she would never interfere in it. He joked later that he made up his mind when, at a dance, she introduced him as the professor of chemistry.

His first book was published as a Bulletin of the National Research Council. At that time NRC committees were occasionally formed with the express purpose of informing American scientists about important developments, and this book was a report of the Committee on Optical Spectra, with Paul Foote as chairman. It was well received, but by the time it was issued in early 1926 much of it had been superseded by the enormous explosion of results from the new quantum mechanics. The exercise, however, was of considerable value to the author; his remarkably clear writing style had been formed, with the acknowledged help of his father, and his very physical understanding of classical dynamics and of the workings of the correspondence principle, which were to serve him well in his later career, had been developed.

Van learned with great excitement of the new quantum mechanics through correspondence and the early published papers of Heisenberg on the matrix mechanics, which remained for a number of years his preferred form (in this, as in many other things, his style was unique). He sent in his first paper using it in early 1926, showing that the classical symmetry factor 1/3 in the magnetic susceptibility is restored in the new theory. *Nature* asked him to shorten the piece, which slowed him down to what he called a "quadruple tie" in publication. He sailed for a visit to Copenhagen that summer and completed another paper on the boat calculating mean values of inverse powers of *r* by matrix mechanics—only to learn that he had been beaten by practitioners of Schrödinger's wave mechanics. Nevertheless he wrote no less than four papers that year on the new mechanics, and for several years he continued to write papers discussing its nature and interpretation (such as [18]).

The little note in *Nature* [10] was to set the theme for much of the rest of his career: the use of the new quantum mechanics to elucidate electromagnetic properties of matter. This had been an interest of his since an early seminar on the Weiss theory of ferromagnetism, and it was implicit in his correspondence principle paper; but from now on his interest led to a stream of fundamental papers, and it became clear that he had chosen this as his particular portion of the great work of verifying and using the new quantum theory. Bohr remarked, with characteristic insight, that while the spectroscopic successes of the quantum theory were more spectacular, the macroscopic ones such as Van worked on were in some ways "more satisfying and more fundamental." The most fundamental immediate results were his demonstration of the general formula (which he always carefully called "Langevin-Debye") for the susceptibility (papers [10], [11], [15] in the accompanying bibliography) and the detailed application to O_2 and NO [12]. But at this time, he had already begun the work on molecular and other spectra in the new mechanics that was a second major theme in his life with a paper with Hill on rotational distortion [16]. He was asked by the chemists to review the new quantum mechanics [17], and in general was an important teacher and proselytizer of the new knowledge in the American context. His worldwide reputation was assured by the series of remarkable experimental verifications of his results on O_2, and NO in Leyden, MIT, and Zurich.

Wisconsin (1928-34) and Prewar Harvard: Magnetism, Molecules, Crystal Fields, and the Origin of Magnetic Relaxation

Although Minnesota was, as he later said, very congenial, with Breit as a coworker, Tate and other experimentalists encouraging him, and a number of bright auditors interested in his lectures (Tate himself, Bleakney, Brattain, and others), physics was more active at Wisconsin. Van accepted an offer of a professorship at the University of Wisconsin in 1928, remaining there until 1934. This gave him the pleasure of overlapping for one year with his father and renewing several old associations. It was at Wisconsin that he conceived and finished his book, *Theory of Electric and Magnetic Susceptibilities,* although much of it was written on a European trip in the summer of 1930. His stay in Zurich was particularly productive; arriving in vacation time, he worked until Pauli returned and, with characteristic rudeness, said: "I don't republish *my* papers into a book!" In fact, the book, aside from being ar guably the first book in the modern field of solid-state physics, contained—as Van defensively remarked—much new material aside from the contents of the several papers on Langevin-Debye, "Van Vleck paramagnetism" (the secondorder contribution of what he called "high-frequency matrix elements"), and the magnetism of the rare earth and iron group ions in salts (with Miss Frank [23]), which had already appeared. There were new discussions of the averaging process for obtaining the fields in materials, of Heisenberg's theory of ferromagnetism, of aspects of dielectric local fields and of dielectric theory in general, of Landau diamagnetism and its relationship to the classical theory, and the like. It is marked—perhaps even slightly marred, as a modern text for physicists poorly trained in classical mechanics—by careful discussion of the ways in which quantum mechanics, the old quantum theory, and classical physics differ. It just missed a number of very important developments, notably Van's own crystal field theory work and Néel and Landau's concept of antiferromagnetism, but in one sense it is not dated in the slightest: All results quoted in the book are, to my knowledge, correct to this day, needing no revision, only expansion. It was an enormously influential book and set a standard and a style for American solid-state physics that greatly influenced its development during decades to come—for the better. We have, incidentally, R. H. Fowler's suggestion that Van write for the Oxford University Press to thank for the book's existence.

During the same Guggenheim fellowship trip that took Van to Zurich in 1930, he was invited to be the only American at the Solvay Congress. In Holland, he spent a number of days talking and walking with his friend Kramers, who pointed out to him Bethe's recent work on group theory. This led to a lifelong interest in group theory, which he still taught in inimitable style—and from Wigner in the original Ger man—when I was at Harvard in 1948. It also led to a series of applications that Van seemed to feel were among his best work: to the spectroscopy and susceptibilities of magnetic ions in solids (papers [25], [36], [43], [45], [53], [54], [60], [61]; and papers by his students M. H. Hebb, R. Schlapp, and W. Penney, later Baron Penney, on most of which his name, characteristically, did not appear). In these papers he introduced the "crystal field" concept in which a magnetic ion is envisaged as behaving more or less like a free ion perturbed by the anisotropic potential of the surrounding ions and atoms. The orbital angular momentum in most iron group ions in solids is "quenched" (a typical Van concept) by such fields, because of the weakness of spin-orbit coupling relative to the crystal field splittings, while rare earth ions retain free ion character but respond to the local symmetry by the appropriate splittings of the energy levels. This concept correlates enormous masses of spectroscopic, magnetic, and even chemical data on these compounds (the chemical energetics were developed by Penney and by Orgel and Ballhausen, much later). It provides an absolutely essential starting point for the understanding of all the technically important insulating magnetic materials, such as ferrites, garnets, ruby, and the like. Van initiated both of the main branches of the theory of crystal fields, the naive electrostatic version and the "ligand field" theory (now more generally accepted) in which the emphasis is on semicovalent bonding to the neighboring "ligand" ions or groups; and with his characteristic flexibility he demonstrated (paper [43]) that both led to essentially the same experimental results. In the chemistry and spectroscopy of these ions, Van's (or Schlapp and Penney's) name for the field strength parameter, *"Dq,"* is still used. Of the crystal field theory it has been said by Moffitt and Ballhausen (perhaps a bit extravagantly, but Van liked to quote this statement): "It will be a long time before a method is developed to surpass [it] in simplicity, elegance and power."

Other applications of group theory in paramagnetism first brought out by Van in the 1930s were the importance of Kramers' degeneracy (a consequence of time-reversal symmetry) in leaving at least two degenerate levels for odd ions, and the application of the Jahn-Teller theorem to deduce small distortions from perfect symmetry in certain cases, distortions that could lead to complex mixed electronic-vibrational states (now called "vibronic") due to tunneling among symmetry-equivalent configurations ([54]).

Two experimental developments sparked his continuing interest in paramagnetism: the adiabatic demagnetization method, especially as practiced at Oxford to reach temperatures below 1°K, and the paramagnetic relaxation work going on at Leyden. The former stimulated several of the papers already quoted, as well as [50]; the latter led to Van's remarkable and prescient calculations of the paramagnetic relaxation caused by lattice modulation of the crystal field parameters ([59]), as well as his invention of the "bottleneck" concept ([66], [67]), which sparked many important experiments after the war. The sum total of his achievements in magnetism in the 1930s after his book appeared can best be appreciated by reading his 130-page Institut Henri Poincaré lectures of May 1939. These were given in French (he remarked that he had had to go to the Riviera to recover, and he did not know what had been necessary for the auditors) and because of war conditions not published until 1947 ([77]). To summarize, he left in place, as a result of this body of work, the conceptual structure from which the science and technology of quantum electronics and much of the science of magnetism arose in the next two decades.

To finish out his contributions to magnetism in this period, we mention his paper on ferromagnetic anisotropy [49], a noteworthy first discussion of this difficult subject, further treated in Harvey Brooks' thesis; and his important clarification [63] of the Néel-Landau theory of antiferromagne tism, putting the subject in vector model form. This made the theory much more suitable for experimental comparisons than the qualitative form given by Néel and Landau. As is often the case, the existence of a formal model stimulated theoretical interest, and the so-called "Heisenberg antiferromagnet," which Van first introduced, has become a favorite subject of theoretical investigation, beginning with Kramers' and my own work on the antiferromagnetic ground state.

Two other subjects, also related to his interest in group theory, constituted much of the remainder of Van's work in this period. These were molecular spectroscopy and the theory of chemical binding. A number of papers ([24], [28], [29], [38], [40], [52], [56], [57]) showed his continued interest in molecular spectra, which dates back to his early work on Λ-type doubling and was a lifelong theme of his work. There is also an important group of papers on CH_4 and on valence theory ([26], [27], [31], [32], [41], [42]), which are the only prewar attempt, to my knowledge, to understand directed valence bonding from a fundamental point of view, rather than to simply postulate it as Pauling did.

Van particularly enjoyed trying to reconcile opposing schools of thought on the important questions. As I remarked, he actually originated both schools in the case of crystal and ligand field theory. He tried, in this sequence on chemical bonding, to show that Hund-Mulliken molecular orbital theory, and Heitler-London valence band theory as adapted by Pauling (and called the Slater-Pauling theory) could lead to the same results for directed covalent bonds such as in CH_4. This work was unaccountably neglected, and it is not until very recently that a real attempt has begun to reach the level of fundamental conceptual understanding of the chemical bond that Van was seeking. Later, as we shall see, he tried to build the same kind of "Van Vleck bridge" (as Purcell termed it) in the theory of ferromagnetism.

Van picked up and contributed to whatever subject was at hand. The local field corrections in dielectric and magnetic media remained an interest ([35], [46], [47], [48], and especially [64], which is a very important clarification of the local field problem which I, at least, found very useful later). He contributed to questions of the interpretation of quantum mechanics ([18], [23], [65]), to nuclear physics and the problem of neutron diffraction ([39], [58], [68]), and to the theory of ferromagnetism ([44], [51]). As student or postdoc problems he touched on atomic energy level theory ([30]), on band theory ([55]), and on intermolecular forces ([56]).

In 1934 Van had accepted an offer to take up a full professorship at Harvard. Initially, his courses were listed jointly in the mathematics and physics departments, since his appointment was in mathematical physics. He was asked by the then president of Harvard, J. B. Conant, to foster education on the less abstract and more applied side of mathematics. Conant believed that his mathematics department had become too remote and that the courses at the graduate level were of little use then (as in my day as well) to physicists, chemists, and others who needed advanced mathematical training. This is not a tendency that even Van could stem (as experience in many other institutions attests), and twenty years later, when he took up his appointment as dean of Applied Science, he resigned sadly from the Mathematics Department, and the Division is the present home of applied mathematics at Harvard.

During this period it is interesting to see Van's interests actually moving progressively away from the abstract or mathematical aspects—the questions that had concerned him initially having been solved by the new quantum theory—toward applications and his eventually most important role as an experimental consultant. Van regretted audibly the drift of the majority of his colleagues in theoretical physics toward more abstract mathematics and toward nuclear, and then particle, physics, although he had at least one student at Wisconsin, R. Serber, who later excelled in that domain, and he was to build up Harvard's strength in that area of physics while chairman of the department.

From the first Van made a point of minimizing his role in his students' and associates' work. With some—especially of the better known, such as Serber and Brooks, as well as with Hurwitz, Jordahl, and myself—there were no joint publications at all. With others, such as Hebb, Schlapp, and Penney, the bulk of the work, though wholly inspired by Van's ideas, was not published under his name. This was even more true of postdoctoral associates; he brought John Bardeen and Nico Bloembergen to Harvard as junior fellows, as well as many visitors in other capacities, such as Broer, Van Kranendonk, Gorter, Abragam, and others with whom his name is not usually closely linked.

Harvard: War and Immediate Postwar Years Van as Consultant, Grey Eminence, and Midwife to the Birth of Quantum Electronics

Although Van continued some teaching at least through 1943, he carried out several roles, especially that of head of the theory group (after 1942) at Harvard's Radio Research Laboratory, a smaller, closely linked cousin of MIT's giant Radiation Laboratory (at which he spent some time in 1942-43). The two laboratories played a very important role in the development of microwave radar, working closely with Bell Labs and other military and industrial laboratories. (Van had early contact as an adviser with the uranium project but was much more closely associated with radar work. Perhaps some future historian of science should trace the rather marked influence that the radar laboratories had on Harvard and MI T physics, as contrasted to Chicago, Cal Tech, and Berke ley, which were heavily involved in Los Alamos and the bomb project and developed in a very different way.)

Several wartime projects strongly influenced by Van can be identified through later papers: one on the theory of WINDOW (clouds of metal foil strips used to fool radar), an unlikely collaboration with Morton Hamermesh and Felix Bloch ([73]); a classic and technologically vital pair of reports on identifying pulses in the presence of noise, carried out with David Middleton ([72]); and, particularly, his discovery that the ill-fated "K-band" radar operating at $\lambda = 1.25$ cm would be a fiasco because of atmosphere absorption by molecular lines of O_2 and, particularly, water. In order to work out this conclusion quantitatively it was necessary to revise accepted theories of collision broadening (since the relevant lines would be severely broadened) in such a way as to include induced emission as well as absorption, and the appropriate revision was carried out with V. F. Weisskopf ([70]). This paper was of the utmost importance both experimentally and theoretically in the immediate postwar years. Its experimental significance is evident and well known: It was a chief tool for interpretation of the mass of experimental data that were produced in the great outburst of radio-frequency spectroscopy after the war—in Bleaney's lab at Oxford, Wilson's and Purcell's at Harvard, Townes' at Bell and Columbia, and Gordy's at Duke, among others. But it had an even deeper significance, in that it was the precursor of my own work (itself stimulated by Van) on pressure-broadening and linebreadths in magnetic resonance spectroscopy, and of other early versions of the calculation of physical results using fluctuation-dissipation methods, and hence sparked one of the earliest lines of inquiry into what became the many-body theory.

In my opinion Van's status as a figure of major importance to the history of science rests most securely on his role in the immediate postwar years at Harvard, rather than on his prewar achievements, massive as these are. During this time, for instance, he was closely associated with no less than four Nobel prizes (those to Purcell, Townes, Bloembergen, and the joint prize in which we participated). I suppose that with his no-nonsense attitude to scientific credit and to who did what, and with his persistent belief—against all odds—that all his associates were as quick and perceptive as he was, Van himself would never have realized the important role that he played as adviser and consultant, but all of those who worked near him at that time, especially the experimentalists, gave him much credit for the very rapid progress they made.

It is hard to take one's mind back to that time and recognize the mental leap that coherent spectroscopy required. Rabi's molecular beam methods, of course, have often been cited as the original for radio-frequency spectroscopy, but the peculiar environment and detection methods made that a rather esoteric specialty. It was really Van who noticed the early measurement of Cleeton and Williams on ammonia (its importance to him he mentioned in his charming little article, not in the bibliography, entitled "Molecular Spectroscopy in Ann Arbor and Outer Space," although I never heard him refer to Rabi's method as seminal) and, during the war, single-mindedly pushed on with identifying the levels of O_2 and H_2O that could lie in the centimeter wave region and hence interfere with the proposed "K-band" radar ([74], [75] were based on this work). At the same time he maintained close rapport with the Leyden group, which was pushing relaxation spectroscopy up in frequency from the radiofrequency end, culminating in Gorter's work during the war and shortly after, and which was the most technically close (because of its emphasis on line-width and relaxation, as well as its instrumentation) to the Purcell group's discovery of nuclear magnetic resonance, and especially to electron para magnetic resonance. Most of those who later took part in the explosion of coherent spectroscopy after the war met at wartime conferences at which Van's work was discussed. (I was at one such where Bleaney, Van himself, and Townes, at least, were present.)

In any case, he was a central figure—perhaps *the* central figure—in taking the first steps in establishing the field that eventually came to be known as quantum electronics. In essence, this field can be defined as the physics of the interaction of coherent electromagnetic radiation with atomic, molecular, or solid-state systems of quantized energy levels. The first step, of course, is the mental leap of recognizing that suitable systems of energy levels exist in reasonable profusion, and with energy level breadths that do not overwhelm the quantization of the levels and leave one with a featureless classical smear. Van brought together the knowledge of molecular energy levels, the appropriate techniques (Waller's moment method as applied by Broer in Holland during the war, and then by Van [79] and the Van Vleck-Weisskopf pressure-broadening theory) for estimating line breadths, and an encyclopedic knowledge of the physics of electric and magnetic interaction that were necessary to get a start in this field. One finds his help acknowledged and his papers quoted in early papers in all the coherent spectroscopies: NMR, EPR, molecular microwave spectroscopy, and later ferromagnetic resonance spectroscopy as well. His name even appears on an experimental paper in molecular microwave spectroscopy, the discovery paper (by Dailey et al.) of hyperfine structure in this field, which also determined the quadrupole moment of N^{14} [70]. Other papers directly on the various quantum electronic spectroscopies were Van Vleck and Weisskopf (already mentioned), the discovery of exchange narrowing with Gorter [76], his beautiful overall summary of dipolar broadening and exchange narrowing [79], a paper on ferromag netic resonance [83], [87], and a pair on pressure broadening ([82], [97]). A number of other papers continued his prewar interest in calculating molecular, ionic, and atomic energy levels: [78], [80], [89], [93], [94], and [95], with some emphasis on the new spectroscopies.

There were at least two other contributions of major importance during this immediate postwar period. Tom Kuhn, later to become very well known as a philosopher and historian of science, collaborated with Van in inventing a new technique, the quantum defect method, for using spectroscopic data directly to calculate band energies in the alkali metals [84], [96], [98]. This was an important forerunner of modern pseudopotential methods, and is very close to the most recent "norm-conserving pseudopotential" method. It was refined extensively by Frank Ham, and the elegant mathematical machinery has been a source of a number of developments.

Van's student Hurwitz had written an unpublished thesis about the many-body theory of ferromagnetism in metals. What Van and he wanted to do was to strike a middle ground between the free-electron theory purists, especially Stoner, who were determined to apply the pure Bloch band theory of ferromagnetism and ignore the necessarily strong electronic interactions otherwise; and the naive "Heisenberg model" theorists who proposed that the magnetic electrons stay in purely atomic states with no itinerant character at all. Van's middle ground, expressed at a seminal meeting in 1952 and written up for *Reviews of Modern Physics* in 1953 ([91], [92], [110]), had the essential components of the correct way to treat strongly magnetic itinerant electrons. Though little regarded in the heat of battle of the time, it formed a basis for important work by Hubbard, Gutzwiller, Herring, myself, Moriya, and others that advanced this difficult problem greatly in the years to follow. This work, in which he was groping toward the same set of concepts being considered by Mott at the same time with regard to the Mott transition but not to magnetic phenomena, makes their joint Nobel prize seem a little less arbitrary.

Harvard: The Later Years Scientist-Administrator

After the great burst of creative energy that I have just described, Van was never again to be at the forefront of several active scientific areas at once, as he had been throughout the quarter century from 1925 to 1950. One may speculate that this—purely relative—slowing down coincided with his assumption of two demanding administrative jobs, chairman of the Physics Department (1945-49) and dean of Engineering and Applied Physics (1951-57). He had been, from the start, involved in the discussions that led to the combining of the Engineering School with the Department of Applied Sciences to make the School, and it was only natural to give the new entity a good start by making him its first dean; its second was his student Harvey Brooks. Also during 1952-53 he was president of the American Physical Society. He was closely associated with the work of the APS, and Bill Havens, the present secretary, remembers his help with much gratitude, both at that time and later. As chairman of the Physics Department, Van presided over the admission—and recruitment—of an extraordinary group of students in the first few years, brought back at irregular times and from the ends of the earth, mixing refugees, returning servicemen, and bright young products of the wartime accelerated courses. It surprised me to find how many of us felt that he personally intervened to get us to Harvard. At the same time he was recruiting junior and senior

faculty: Bloembergen, Pound, Schwinger, Ramsey, and Purcell, among others, were his ap pointments. Those years were a golden age of Harvard physics, and few of us who participated in them can have been unaware of that fact.

Even more demanding was the creation of the Engineering and Applied Physics School. The no-man's land between engineering and physics, or more accurately, bordering mathematics and chemistry as well, is a very sensitive region, with delicate problems of defensiveness, snobbery, and intellectual fragmentation. To get the new entity going, to keep the lines of communication open, and in particular to make the resulting unit operate well with the Physics Department, was a remarkable achievement. Van has left Harvard, as his most enduring legacy, one of the world's strongest groups in these areas, particularly in condensed-matter physics and materials science, the successor areas to his own interests. One of his stratagems in doing this was the "Van Vleck Bridge," an actual physical connection between Cruft Laboratory, where many of the applied scientists were, and the Jefferson Physics Lab.

After six years as dean, Van returned in 1957 to more or less private academic life, although first he served as vice president of the IUPAP, 1958-60, and then he took up two visiting professorships: the Lorentz Professorship at Leyden in 1960 and the Eastman Visiting Professorship in Oxford, 1961-62, where Abigail and he were the first to occupy the new residence provided. Then he returned to Harvard until his retirement at the age of seventy in 1969.

All during this period, of course, he was maintaining a level of scientific publication that would have seemed high for anyone else, especially if one includes the many review papers he was asked to give, and the increasing stream of scientific reminiscence and commentary that began to flow.

His strictly scientific output resumed its normal flow in 1957, and dealt mainly with aspects of magnetisim, a field that increasingly claimed his scientific attention. He was delighted to find his old friends, NO and O_2, captured in 1957 in a solid-state "clathrate" compound by H. Meyer, and to interpret the results of measurements on them ([102], [120], and a similar measurement by Huang, [153]). He wrote a stream of papers on various aspects of rare earth magnetism, including some basic papers on the now technically important garnet materials (used in bubble memories), the first of these in 1960 with Werner Wolf ([113], [119], [123], [127], [128], [130], [131], [132], [134], [138], [139], [140], [147], [164], [166]; the last two in 1974 and 1975, well after his retirement). Other rare earth materials discussed included europium metal [114]; Sm intermetallic compounds [118]; EuO, where he helped interpret the surprising ferromagnetic behavior discovered by his old friend Richard Bozorth and Bernd Matthias [121]; Eu_2O_3 [150], [152]; and Ho-Er alloys [159].

He retained an interest in the theory of magnetism in metals, and continued to the end to be properly skeptical of reports of progress. (I well recall being asked repeatedly: "Do you *really* think the 'U-T' model explains magnetism?"—his name for Hubbard's model based on his own nomenclature of 1953 for the relevant parameters and on his private joke about the University Theater in Harvard Square.) His own work included mostly review and discussion papers ([103] in 1957; a set of Varenna lectures, [110], [125], [141], all given at meetings, the last at a Sanibel symposium in his honor in 1966). His interest in magnetic anisotropy and other weaker effects led to a few papers: a review article at the first "Bozorth" conference in 1956 [99], and [112]; these in addition to several of those on the rare earth materials.

Finally, he continued his interest in spectroscopy and relaxation, and particularly in the new concepts being introduced by Bloembergen and Abragam of spin temperature and in his own old idea of the Van Vleck bottleneck at the phonons, which led to several reviews and some original papers: another set of Varenna lectures of 1957 [105]; [103], [108], [116], [117], [122] (a paper at a Quantum Electronics conference in 1961: the field now had a name); [128], and [135].

Reviews proliferate in his later work; in addition to those we have already mentioned, there was one on spin waves, with Van Kranendonk (who also took an interest in his old field of pressure broadening, [106]); EPR and magnetism [90]; exchange [109]; antiferromagnetism [86], (where he kindly publicized my own early work); rare earth magnetism [137]; and line breadths (with David Huber, his last contribution of some ten or more to *Reviews of Modern Physics*, [167]).

One very small group of papers commemorates a rather bizarre incident in Van's life. These are the papers on "Hidden momentum" [148] and [151]. (The first is a collaboration, delightful to behold, between one of the department's youngest and most famous particle theorists, Sidney Coleman, and Van, about to retire in 1968.) Van had been asked to referee a paper by the now notorious Nobel prize winner, Bill Shockley, proposing a new point of view on energy and momentum transport by light in the presence of polarizable media. This esoteric question had some small experimental interest in view of the availability of high-power lasers and the capability of measuring their effect on media, but on the whole the subject did not merit the extreme importance Shockley placed on it as an example of the success of his so-called "Try-simplest-cases" way of carrying on scientific investigation (I always wondered what he thought the rest of us do), and of the short-sightedness of the scientific establishment. Shockley somehow seemed to identify the physical science establishment with that primarily biological one that opposed his views on heredity and race (a subject in the complication of which the application of "Try simplest cases" can be a disastrous blunder). In any case, I presume Van had been an outspokenly opposed referee on Shockley's paper, and after a rather bitter correspondence not marked on Shockley's side even by the normal perfunctory courtesies, Van felt moved to write a refutation. The bizarre event was that Shockley then threatened both Van and the *Physical Review* with a lawsuit for libel, on the basis that this paper called his scientific reputation into question, a reputation which was of immense value to him in the great work he felt he was doing on behalf of "human quality." I believe this is the only public controversy in which J. H. Van Vleck has ever been involved, and the story does him honor.

Teaching and Other Personalia

I don't know if there exists anywhere a full list of Van's graduate students. It doesn't really matter, because some of those most influenced by him, such as John Bardeen, Walter Brattain, W. G. (Baron) Penney, and Nico Bloembergen, were not formally his students. As I have already remarked, most of his students published their work independently, so it is rather hard to trace them; a fair list is given in Bleaney's memoir. Among those who were, formally, or considered themselves, his students, are at least two eminent administrators of science and technology, one a peer of the realm in England and one his successor as dean at Harvard; one of the country's premier historians of science; an eminent and widely respected particle theorist; an eminent plasma theorist; and a Nobel prizewinner. Just as Van wrote the first American thesis on the old quantum theory, his student E. L. Hill wrote the first one on the new quantum mechanics. Van was a master of that delicate judgment that allows the student just the amount of freedom he can manage, while at the same time the student remains conscious that there is a "satety net" in case he cannot make it on his own. Van seems to have been remarkably successful at the process; there were very few failures. (I know of none, in fact, but merely suppose there must have been some.) The decision to work with him was one of the wiser choices of my life.

Van's early lectures at Minnesota, Wisconsin, and at the famous Michigan summer schools of the early 1930s where, as he said, American physics came of age, had a great influence historically. By the 1940s, however, his teaching style had become unique, and is remembered with fondness by everyone I spoke to. Most of the material was written in his inimitable scrawl on the board, and he spoke in a very personal style, using such favorite phrases as "engineering approximation" (often referring to a highly sophisticated mathematical procedure quite beyond the capability of your average engineer), "hand that over to our mathematical slave," and the like. Especially in group theory, his intuitive feeling for the subject often bewildered us as he scribbled down symmetry functions in an offhand shorthand to demonstrate what we thought were exceedingly abstruse points. He had a schoolmarmish way of interrupting himself with a "what?" to the class, usually getting a murmur that he took to be the end of his sentence, and I suppose like most lecturing tricks it served to maintain a proper pace. But he did at least once come into class and start his lecture with "A clever trick is—what?"

In all of his classes, however, he used two basic techniques of the genuinely good teacher. First, he presented a set of carefully chosen problems, which really contained the meat of the subject, often with "hints" that I usually found hopeless as helps but highly useful as explanations. Second, he supplied a "crib" for examination study, which we always thought was practically cheating, saying precisely what could be asked on the exam. It was only after the fact that you realized that it contained every significant idea of the course.

He continued to lecture at summer schools until quite late in life, often in exotic places, in keeping with his love of travel. Several of his lecture note sets—the Henri Poincaré in 1939, and Varenna in 1957, for instance—are important scientifically. As time went on he was increasingly in demand for semi-ceremonial speeches, and often these, with their gentle wit and unmistakable flavor, took the form of reminiscences or remarks on the state of his beloved science, as his articles on walking with Dirac; on his "Swiss visits of 1906, 1926 and 1930"; or his deploring of barriers between minds in his presidential address. Nonetheless, more often than not he chose to emphasize technical content, to the end of his life. His wit was ever gentle; he was never sarcastic or self-important, delighting in strange juxtapositions ("Molecules in Michigan and Outer Space"), coincidences, and in riding his own incongruous hobby-horses to extreme lengths—as in using the official or football nickname for every university in the country, as "Sooner" for a person associated with Oklahoma. He never told "jokes" in the usual sense of the word, especially avoiding off-color speech.

He loved travel and knew the cities of the world well—well enough to have a favorite hotel in Hong Kong, for instance. Abigail almost always traveled with him. They were inseparable, and her wit was an excellent foil to his, slightly more personal and acerbic, occasionally expressing the impatience with people that Van never permitted himself. Bridge was a favorite avocation of theirs, and still is for her; they were excellent players. In his younger years Van was a dedicated walker, both in the fields near Madison and in the wild places of the world—Colorado, the White Mountains, the Alps, and many other places.

Incidentally, he always managed his own investments, and

I believe very much increased the reasonably comfortable fortune his father left. The only detail I ever heard revealed was that he spotted the departure of a first-rate young crystal chemist from Bell Labs for the newly formed Texas Instruments Corporation at the right time, and invested in TI, a hundred-fold winner. In any case he had enough to give generously to Wisconsin, including the gift of his father's print collection, and to Harvard.

Last Years

In 1969 Van retired, remaining in Cambridge except for the inevitable travels (we find papers in this period from Melbourne, Rumania, Cambridge, London, and Holland) and writing an occasional scientific paper, as we have seen. He kept in mind until very late the project of updating his book, but surely that was too massive a task. The only replacement at a comparable level is the six-volume Rado-Suhl series, so some very rigorous selection would have been required. Honors continued to flow to him, such as the coveted Lorentz medal, 1974; Chevalier of the Legion d'Honneur, 1970; foreign membership in The Royal Society; and finally the Nobel prize. Aside from an operation, he remained in good health until about 1975, when he began to have a heart weakness that required a pacemaker. At this time the Van Vlecks decided to give up their lovely old house on Fayerweather Street for an apartment at 989 Memorial Drive, in which Abigail still lives.

I saw Van in London in 1968, at the time of his "signing the book" at The Royal Society, and his speech was as sparklingly witty and as full of new ideas about magnetism as ever. We attended a play of which I remember only that it was a bit modern and negative for the Vans' determinedly old-fashioned point of view with regard to literature, or music, or styles in science, for that matter. The result was some sharp discussion from Abigail.

Ten years later, in Stockholm, with his pacemaker, the wit was still there, but it seemed that there was a short "duty cycle"; he sparkled for an hour at a time or so, and then rested. His brief speech, as the eldest, on behalf of the physics laureates is a model of taste and grace, complimenting his hosts and his fellow laureates with a sure hand.

He continued to travel, but encountered medical problems again on a trip to the West Coast to honor his old friend Julian Schwinger. Finally, on October 27, 1980, his heart gave out for good, and we lost the grandest representative of what he himself called the "Coming of Age of American Science."

In speaking of his own father at the dedication of E. B. Van Vleck Hall at Madison, Van quoted his father's precepts: "Two qualities may be noticed as especially needed by the American [scientist]. The first is a broad, liberal culture. The pursuit of [science] in itself is doubtless narrowing . . . its abstract height tends to separate one from daily life. A wide liberal culture therefore is eminently desirable."

The second is "moral fiber and force, as exhibited in patience with students."

No one can have better satisfied these goals than Van

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First and foremost, I have freely borrowed material from Brebis Bleaney's admirable memoir for The Royal Society. For the opportunity to read this memoir, I am very grateful. Among other unpublished material I used for background were interviews by T. S. Kuhn from the AIP Center for the History of Physics. The speeches given in memorial services at Harvard and at Wisconsin were quite useful, as well as those at the dedication of E. B. Van Vleck Hall at Wisconsin, including Van's own. Finally, his fairly extensive reminiscent articles were very helpful, as were some insights from conversations with Abigail Van Vleck, Nico Bloembergen, and others.

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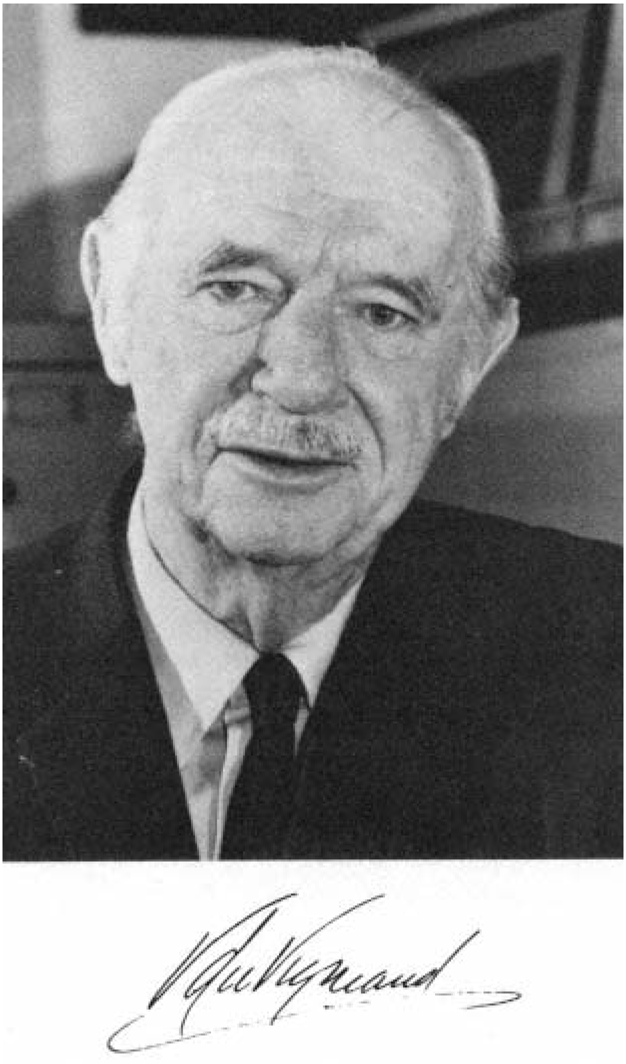

Vincent du Vigneaud

May 18, 1901-December 11, 1978

By Klaus Hofmann

Vincent du Vigneaud was born in Chicago in 1901. He was of French ancestry, the son of Alfred du Vigneaud, an inventor and machine designer, and Mary Theresa du Vigneaud. He attended Carl Schurz High School in Chicago, from which he graduated in 1918. When he was a freshman in high school, two friends, who had a chemical laboratory at home, invited him to join them in chemical experimentation. They obtained chemicals from a pharmacist and conducted experiments that involved the fabrication of explosives containing sulfur. This was his first contact with science.

World War I was under way, and young people were needed on the farms. Seniors in high school were offered the opportunity of working on the farms in spring and receiving their diplomas in June. Young Vincent worked through spring and summer on a farm near Caledonia, Illinois. He was very proud of the fact that he could milk twenty cows by hand, and he decided to become a farmer. His older sister, Beatrice, changed his mind and suggested that he go to the University of Illinois at Urbana-Champaign to study chemistry. He followed her advice and registered in chemical engineering. He later recalled:

> I found during the first year that it was chemistry rather than engineering that appealed to me most. I switched to a major in chemistry since I was deeply impressed by the senior student's work, especially in organic chemistry. I also found that I was most interested in those aspects of organic chemistry that had to do with medical substances and began to develop an interest in biochemistry.

Young du Vigneaud had no money and had to put himself through college and graduate school. Tearing down boilers, picking apples, working in the library, and jerking sodas were some of his occupations. But the job that helped most financially was that of headwaiter. The next most remunerative job turned out to be the teaching of cavalry tactics and equitation as a reserve second lieutenant in the cavalry.

One day, while working as a waiter, Vincent saw a pretty redhead and said to one of his colleagues, "That's the woman I am going to marry"—and he did. The young woman was Zella Zon Ford. She was an English major, but as she and Vincent became better acquainted he saw to it that she took classes in mathematics and chemistry. Although she graduated as an English major, she knew sufficient chemistry so that after their marriage on June 12, 1924, she was able to teach chemistry in high school.

One of the professors at Illinois who exerted a significant influence on young du Vigneaud was Carl Shipp Marvel, known as "Speed." Du Vigneaud was much impressed with Marvel's lectures and research program, and he decided to do his senior thesis with him. Later he selected Speed to become his master's degree adviser. As he progressed with his studies, he became more and more interested in the relations between biochemistry and organic chemistry. He took advanced courses in biochemistry from H. B. Lewis and the nutritionist W. C. Rose, whose studies on nutrition of the white rat later became role models for some of du Vigneaud's metabolic investigations. He was particularly taken with a lec ture by Professor Rose in which the work of Banting and Best on insulin was discussed.

Du Vigneaud earned his master's degree in February of 1924 and accepted a position with the Du Pont Company; he later worked for some time with Dr. Walter Karr at the Philadelphia General Hospital. Nevertheless, his mind was set on graduate study and the earning of a Ph.D. degree. Professor Marvel recalls the following episode:

> When du Vigneaud received his master's degree he was offered a job with Walter Karr in Philadelphia, but he was too poor and had no money to pay his way to Philadelphia. To help him out I gave him an assignment to make 10 pounds of cupferron for our organic preparations laboratory and told him I would pay him, as a wage, whatever amount he could produce in material for under the price which we would sell it. He did not ask for any hourly work or time, but we generally agreed that way. In producing the 10 pounds, he'd accumulated enough money to get to his Philadelphia job.

In the spring of 1925 du Vigneaud received an invitation from Professor John R. Murlin to join the Department of Vital Economics at the University of Rochester, New York, to undertake graduate work on the chemistry of insulin. Professor Murlin was a physiologist and not a chemist, and du Vigneaud was eager to discuss his chemical problems with other chemists. In this connection, he became acquainted with Hans Clarke, who at the time was working for Eastman Kodak. Later, Clarke became professor and chairman of the Biochemistry Department at the College of Physicians and Surgeons in New York, and the two men struck up a lifelong friendship.

In 1927 du Vigneaud graduated with a Ph.D. degree; the thesis title was "The Sulfur in Insulin." During his last year in Rochester he was awarded a National Research Council Fellowship, which enabled him to pursue postdoctoral studies with John Jacob Abel, professor of pharmacology at the

Johns Hopkins University Medical School. Here, in collaboration with Oscar Wintersteiner and Hans Jensen, the insulin studies were continued. A second fellowship enabled the young du Vigneaud to do some traveling abroad. He became acquainted with the synthesis of peptides in the laboratory of Max Bergmann at the Kaiser Wilhelm Institut in Dresden and spent some time with Professor George Barger in Edinburgh, Scotland. Bergmann, a former student of Emil Fischer, was a pioneer in the peptide field who later became a member of the Rockefeller Institute for Medical Research (now Rockefeller University) in New York.

Broadly equipped to engage in independent scientific pursuits, du Vigneaud accepted a position in the Department of Physiological Chemistry at his alma mater in Illinois. Biochemistry had become his chosen field, and the opportunity presented itself to have graduate students. He spent three happy years in Illinois; at age thirty-one he became professor and chairman of biochemistry at George Washington University Medical School in Washington, D.C. He was saddened to leave the outstanding department at Urbana with such great professors as Adams, Marvel, Shriner, and Fuson in organic chemistry and Professor Rose in biochemistry, but the opportunity for greater independence was decisive.

He stayed at George Washington from 1932 to 1938, when he was invited to head the Department of Biochemistry at Cornell Medical College in New York City, a chair that had been occupied by Stanley R. Benedict. In connection with this move he stated:

> When I came to Cornell Medical College, I brought along five members of my research group, Mildred Cohn, George W. Irving, Theodore Loring, Gail Miller, and John Wood. As in the transfer from Illinois to George Washington I thus had continuity, people with whom I had already been working. This I regard as very important when moving from one place to another. Just as in transplanting a tree with some soil around it, if possible it is well to move a man with some of his environment.

The awarding of the 1955 Nobel Prize in Chemistry constituted an unquestionable triumph for du Vigneaud, but he expressed definite opinions pertaining to the awarding of prizes for scientific achievement. He said to a reporter, "I am expecting to stay in the research field, in the academic world, but I want to tell you I will never work for any prize. I refuse to let my rewards rest in the hands of any committee."

In answer to a congratulatory note I sent him on the occasion of his award, he answered: "The real thrill of such an award is sharing the pleasure with one's friends, and particularly with those who have been associated with me on the trail."

The highly productive career at Cornell Medical College came to an end with his assumption of emeritus status in 1967. But a generous invitation from Professor Harold A. Scheraga, then head of the Chemistry Department at Cornell University in Ithaca, made it possible for du Vigneaud to continue his investigations as professor of chemistry. He was very happy and productive in Ithaca and enjoyed his new surroundings. He wrote to his former collaborators and students: "Those of you who know the Ithaca area will appreciate that I have a fantastic view from my office on the sixth floor of the Chemistry Research Building overlooking Cayuga Lake to the northwest and Beebe Lake, waterfalls and the Fall Creek gorge down below."

In addition to his outstanding contributions to science, du Vigneaud was a great teacher and lecturer. His lectures to students were interesting and well prepared. He emphasized the importance of teaching and his advice to the faculty was: "Remember your first obligation is teaching; when you are teaching it takes precedence over research." His presentations at home and abroad were masterpieces of staging. He would go over his slides with the projectionist in the greatest detail so that the presentation would proceed flawlessly. He was a showman, an artist in communicating research find ings. It was a genuine pleasure to listen to his presentations, which were as meticulously prepared and rehearsed as were his scientific papers.

Professor du Vigneaud's scientific career was abruptly terminated when he suffered a stroke in 1974. He died on December 11, 1978. His wife, Zella, had passed away one year earlier. Professor du Vigneaud is survived by a son, Vincent, Jr., and a daughter, Marilyn Renee Brown. Both are physicians.

If one views the totality of du Vigneaud's contributions to science, one recognizes a thread of continuity connecting sulfur-containing, biologically important compounds. This thread extends from insulin to cysteine, homocysteine, methionine, cystathionine, biotin, penicillin, oxytocin, and vasopressin. In the Messenger lectures, delivered at Cornell University in Ithaca in 1950, he likened his scientific work to a trail in research; he wrote:

> An attempt was made to retrace the research trails originating from a study of insulin that I have had the pleasure of working out in association with various collaborators over a period of twenty-five years. I attempted to present not only the findings encountered, but also in many instances the stepwise evolution of these findings, including the accidents of fate that played a part.

Some of du Vigneaud's earliest researches dealt with the chemical nature of insulin. Abel crystallized insulin in 1926, and Jensen, Wintersteiner, and du Vigneaud investigated the composition of acid hydrolysates of the crystalline hormone. With the rather primitive methods available at the time, the presence in such hydrolysates of cystine and various other amino acids was established. Based on this evidence, it was concluded that insulin was a protein. Du Vigneaud commented later: "It may seem strange to speak of work establishing insulin as a protein because it is now a generally ac cepted fact that a hormone can be a protein or that a protein can be a hormone, yet at that time (1928) there was great reluctance in accepting this viewpoint." The thinking at that time was strongly influenced by the concepts of Willstätter regarding the chemical nature of enzymes that were assumed to be composed of a small functional coenzyme and a protein carrier. Insulin was believed to be a small molecule that was attached or absorbed to a high molecular weight carrier.

In 1930 du Vigneaud became acquainted with L. F. Audrieth, a faculty colleague at the University of Illinois, who was an expert in the liquid ammonia field. He was impressed with liquid ammonia as a solvent for insulin and the sparingly soluble cystine. Audrieth's use of metallic sodium as a reducing agent in liquid ammonia prompted du Vigneaud to apply this method to the conversion of cystine to cysteine. He devised the technique of S-benzylation of cysteine by adding benzyl chloride to sodium in liquid ammonia-reduced cystine. The observation that the S-benzyl group was removed from S-benzylcysteine by reduction with sodium in liquid ammonia represented a significant contribution to peptide chemistry; it made possible the transient protection of the thiol group of cysteine during peptide syntheses.

In 1932 Bergmann and Zervas introduced the benzyloxycarbonyl group (carbobenzoxy group) into amino acids and peptides, and with the discovery that this protecting group could be cleaved by catalytic hydrogenolysis they laid the groundwork for the development of modern peptide synthesis. Du Vigneaud became interested in this method and embarked on synthesizing carbobenzoxy derivatives of amino acids. The story has it that in his laboratory the carbobenzoxyamino acids failed to crystallize. One day, Max Bergmann came to visit the laboratory and, lo and behold, from that time on the carbobenzoxyamino acids crystallized beautifully. Did Bergmann carry seed crystals in his pockets? The discovery that benzyloxycarbonyl groups can be removed from cysteine and cysteine-containing peptides by sodium in liquid ammonia broadened the scope of the carbobenzoxy method and opened its applicability to peptides containing sulfur.

As we proceed with this discussion, it will become apparent that the techniques du Vigneaud developed early in his career provided answers to problems he encountered at a later time (oxytocin and vasopressin). Much of du Vigneaud's work in intermediary metabolism concerned the formation of cysteine in the animal organism and the metabolic relationships among methionine, cysteine, homocysteine, cystathionine, and choline. He called the underlying reactions "transulfuration" and "transmethylation." It was known that methionine could support growth of laboratory rats on a cysteine-free diet, and Rose had shown that methionine was an essential dietary constituent for the rat. In short, the rat is capable of synthesizing cysteine but not methionine. In 1931 du Vigneaud discovered a new sulfur-containing amino acid while exposing methionine to strong sulfuric acid. This compound was the next higher symmetrical homolog of cystine and he named it "homocystine." Later, he discovered that the reduced form of this amino acid, homocysteine, was a metabolically important compound. Du Vigneaud observed that homocysteine, like methionine, could support the growth of rats on diets deficient in cystine.

These observations pointed to a metabolic relationship between methionine and homocysteine and suggested that demethylation of methionine could be involved in cysteine biosynthesis. Du Vigneaud synthesized L-cystathionine, a thioether in which the carbon chains of cysteine and homocysteine are connected by a single sulfur atom, and found that this compound sustained growth of rats on a cysteine deficient diet. This observation indicated that the rat was capable of cleaving the thioether linkage with formation of cys teine. It was observed further that cystathionine did not give rise to homocysteine, an observation that was supported by in vitro studies with liver slices. The addition to liver slices of a mixture of homocysteine and serine resulted in a 60 percent conversion of homocysteine sulfur into cysteine, providing strong evidence for the hypothesis that homocysteine was indeed an intermediate in the formation of cystathionine. The importance of serine as a precursor of cysteine had been demonstrated earlier by Dewitt Stetten.

Before continuing the discussion of du Vigneaud's work on the intermediary metabolism of sulfur compounds, it seems fitting to have a short synopsis of the status of biochemistry in the 1930s. At that time, the Biochemistry Department of the College of Physicians and Surgeons at Columbia University, under the leadership of Hans Clarke, had developed into one of the outstanding departments in the country and one that made scientific history. It was in this department that Rudolf Schönheimer and his colleagues performed the classical tracer experiments pointing to "the dynamic state of the body constituents." The application of isotopes to the solution of biochemical problems provided the key for these developments, which revolutionized biochemical thinking. Harold C. Urey, also of Columbia University, had developed the methodology for the preparation of deuterium oxide and other elements enriched with respect to stable isotopes, and the availability of these compounds opened far-reaching biochemical frontiers. Because growth experiments had severe limitations, du Vigneaud applied the new tracer techniques to the study of the conversion of methionine to cysteine. He synthesized DL-methionine labeled in the β and γ positions with ^{13}C and containing ^{34}S and fed this compound to rats. The rats were shaved at the beginning of the experiment and received another haircut after thirty-eight days in the experiment. The cystine isolated from the hair contained ^{34}S, but no ^{13}C. From the results of this ex periment it was concluded that only the sulfur, not the carbon chain of methionine, was utilized for cysteine biosynthesis. This provided final proof that, in the rat, cysteine synthesis from methionine involves demethylation with formation of homocysteine followed by condensation of the homocysteine with serine to form cystathionine. The latter is cleaved with formation of cysteine and α-ketobutyric acid. In essence, the conversion of methionine to cysteine involves a transfer of the methionine sulfur to serine. This, according to du Vigneaud, became known as "transulfuration."

An interesting coincidence led to the discovery of the concept of transmethylation or methyl transfer. Here again the crucial evidence was derived from rat feeding experiments. Rose observed good growth of rats fed a methioninecysteine-free diet that was supplemented with homocysteine. Similar experiments carried out in du Vigneaud's laboratory produced negative results; the rats failed to grow. The animals in both laboratories grew well when the diet was fortified with methionine. The difference in the results was traced to the vitamin supplements used. Du Vigneaud employed crystalline B complex vitamins, but Rose used rice bran extract (Tikitiki) as the vitamin source. Du Vigneaud noted that his rats developed fatty livers while on experiment. It was known from the work of Best that choline inhibited fatty infiltration of the liver, and Du Vigneaud reasoned that this pathology could be the result of a choline deficiency. He decided that the factor missing in his diet could be choline, and this proved to be correct. Accordingly, diets containing both homocysteine and choline were fed, and such a regimen supported growth as well as did methionine.

On the basis of these findings du Vigneaud speculated that choline, a compound rich in methyl groups, could act as a methyl donor for the conversion of homocysteine to methionine. These early findings led to the concept of transmethylation and that of "labile" methyl groups. The obser vation that rats grew well and failed to develop fatty livers on a choline-free diet supplemented with methionine suggested to du Vigneaud that the methionine could serve as a methyl source for choline synthesis. These concepts were amply confirmed by tracer experiments. Methionine in which the methyl group was enriched with respect to deuterium was fed to rats, and choline was isolated from the tissues. This choline contained deuterium in its methyl groups, demonstrating transmethylation from methionine. Conversely, choline containing deuterium was given to rats, and deuterium was present in the methyl group of methionine. Thus the hypothesis that methionine was biosynthesized from homocysteine by a methyl transfer from choline was substantiated. It was also observed that the transfer of methyl groups was a continuous process. In addition, du Vigneaud found that the methyl group of creatine was derived from the methyl group of methionine. The transfer of methyl groups from methionine to choline and from choline to methionine is a reversible process, but the methyl group of creatine does not serve as a methyl donor for the conversion of homocysteine to methionine. Based on these studies, du Vigneaud concluded:

> From our study, we know only that the methyl group of methionine and choline can be transferred, but we do not know whether methionine or choline react directly or whether they are precursors of derivatives from which the methyl groups are released. Although methionine can be demethylated *in vitro,* the conditions required are drasti c. Attention must therefore be directed to any possibility whereby the bond between the methyl group and the sulfur atom may be weakened. The formation of a sulfonium ion would be expected to effect such a labilization.

It remained for Cantoni to identify the methyl donor in biological systems as the sulfonium ion S-adenosylmethionine.

The work on biotin resulted from an invitation to du Vigneaud from Paul György to collaborate in establishing the chemical nature of the anti-egg-white injury factor in liver, which György had designated as vitamin H. Rats receiving diets containing large proportions of raw egg white as the source of protein develop severe dermatitis and nervous disorders and die if the condition is not relieved. Certain foodstuffs, such as liver and yeast, contain a substance capable of preventing and curing this disorder. The curative factor was named vitamin H by György (H being derived from the German word *Haut,* meaning skin). Biotin, a yeast growth factor, had been isolated from egg yolks by Kögl and Tönnis. Du Vigneaud, György, and collaborators were able to cure eggwhite injury with Kögl's pure biotin, demonstrating that vitamin H and biotin were one and the same compound. Biotin was isolated from liver extracts and milk in the Cornell laboratories, and the chemical structure of the compound was established. The structure worked out by du Vigneaud and collaborators was verified by chemical synthesis in the Merck laboratories. Biotin, first discovered as a yeast growth factor, turned out to be a mammalian vitamin.

The Second World War interrupted the operations of the laboratory, and du Vigneaud was invited by the wartime Committee on Medical Research, OSRD, to join the great effort being organized in this country and in England to work on the chemistry of penicillin. Many contributions to penicillin chemistry emanated from the Cornell laboratory. Perhaps the most outstanding were those dealing with the synthesis of minute quantities of the antibiotic and its identification with the natural compound.

One amusing sidelight to the penicillin story comes from Sofia Simmonds. During a discussion with Hans Clarke, she remarked that penicillin must contain sulfur. Hans Clarke, who was in charge of the U.S. part of the super-secret penicillin project, was shocked to hear this and he wanted to know how she'd found out. She said we all could tell; the labs on the second floor, where the work was being done, leaked ben zylmercaptan into the hallway—any V du V person knew what that meant.

Du Vigneaud's work on the posterior pituitary principles oxytocin and vasopressin was started in 1932 and continued until 1940, when it was interrupted by the Second World War. During this time, however, the emphasis of the laboratory was on the metabolic aspects, transulfuration and transmethylation, and du Vigneaud referred to his work with the posterior pituitary hormones as his hobby. Some progress had been made in purification of these principles, mainly by the use of precipitation and electrophoretic techniques, but of prime importance were some preliminary observations suggesting that oxytocin and vasopressin were derivatives of cystine. During the war, new techniques became available that had a critical effect on the progress of the posterior pituitary hormone project. Of immediate importance were the Craig countercurrent distribution published in 1944 and the starch column technique of Moore and Stein for the quantitative separation of mixtures of amino acids in acid hydrolysates of proteins on a micro scale.

Du Vigneaud returned to the study of the posterior pituitary principles in 1947. A concentrate he had received from Parke-Davis in 1940 that was stored during the war years was reassayed in 1947 and had retained 50 percent of its original oxytocic potency. Homogeneous oxytocin exhibiting a high level of biological activity was isolated from this material by the Craig countercurrent technique. The amino acid composition of an acid hydrolysate of this material, determined by the Moore-Stein technique, showed the presence of cystine, glutamic acid, aspartic acid, glycine, isoleucine, leucine, proline, and tyrosine in molar ratios of 1:1. In addition to these amino acid residues, the hydrolysate contained three moles of ammonia. The amino acid residues plus ammonia accounted for 97 percent of the hydrolyzed material. Molecular weight determinations were in agreement with a monomer. Oxytocin was oxidized with performic acid, and the amino acid composition of the oxidized material was determined. The composition was identical to that of the unoxidized material, except that in lieu of cystine two molecules of cysteic acid were present.

It followed from this and other results that oxytocin was a cyclic peptide. Using the dinitrofluorobenzene technique of Sanger, it was shown that oxytocin contained a free amino group that was derived from one of the two cysteine residues; a free carboxyl group was not present. By a combination of the Edman technique and analysis of partial acid hydrolysates, the amino acid sequence of oxytocin was established as H-Cys-Tyr-Ile-Glu-Asp-Cys-Pro-Leu-Gly-OH. The final, as yet unanswered, question related to the sources of the three ammonia molecules. These were shown to be asparagine, glutamine, and glycinamide. Thus the structure of oxytocin was established as H-Cys-Tyr-Ile-Gln-Asn-Cys-Pro-Leu-Gly-NH_2. In his characteristically cautious approach, du Vigneaud commented as follows: "It is obvious that, in spite of the fact that this was the only structure we could arrive at through the realization of the results from our degradative work, synthetic proof of structure was mandatory."

The crucial steps in du Vigneaud's oxytocin synthesis were based on reactions in liquid ammonia he had investigated many years earlier. When subjected to reduction with sodium in liquid ammonia, oxytocin was converted to the open-chain oxytoceine, and this on air oxidation reconstituted biologically active oxytocin. This behavior of oxytocin was the key for its successful synthesis. Another model study, using the natural hormone, was performed that suggested the strategy for a successful synthesis. Oxytocin was reduced to the openchain oxytoceine, and benzyl chloride was added to the reaction mixture, affording S,S'-dibenzyloxytoceine. Deben zylation of this material with sodium in liquid ammonia followed by air oxidation regenerated biologically active oxytocin.

The facile ring closure to the 20-membered ring structure of oxytocin from the open-chain peptide oxytoceine indicated that the open-chain structure had a preferred conformation in which the two SH groups are located in close proximity for cyclization to occur. This appears to constitute the first example of the now well-established principle that the amino acid sequence of a protein endows it with the thermodynamic information necessary for folding into a specific conformation. Based on these model reactions, du Vigneaud synthesized N-benzyloxycarbonyl-S,S'-dibenzyloxytoceine and converted the synthetic material into active oxytocin in the manner discussed above. The synthetic oxytocin was identical with the natural hormone. The first oxytocin synthesis was communicated to the *Journal of the American Chemical Society* on July 13, 1953. The paper concluded with the following statement:

> If the synthetic product truly represents oxytocin, which it does so far as we are concerned, this would constitute the first synthesis of a polypeptide hormone. What effect slight changes in the structure of a compound of such complexity might have on chemical, physical, and biological properties must be investigated.

While the work on oxytocin was under way, the structure of vasopressin was also determined. With a wealth of data based on degradation studies, paralleling those outlined for oxytocin, a structure for arginine vasopressin was arrived at that was very similar to that of oxytocin. This hormone embodies the same ring structure as oxytocin but contains two amino acid exchanges. Isoleucine is replaced by phenylalanine and leucine is substituted by arginine. Lysine vasopressin contains lysine in lieu of arginine.

A synthesis of lysine vasopressin was completed in 1960. The observation by du Vigneaud that certain combinations of amino acids into peptides can result in compounds exhibiting potent physiological activities opened a vast field of biological and chemical research. He stated, "It is a little startling to think that the amino acids when put together in a certain way, in a particular architecture, can lead to such an array of compounds exhibiting such a variety of physiological and pharmacological properties."

Du Vigneaud's findings with oxytocin and vasopressin were of great fundamental importance: They demonstrated for the first time that replacement of certain amino acid residues in the sequence of a physiologically active peptide can bring about significant changes in biological action. The exchange in oxytocin of isoleucine for phenylalanine and of leucine for arginine (or lysine) alters the physiological activity of the molecule from one of mainly oxytocic to one with mainly vasopressor potency. These discoveries have stimulated much research into the relations between peptide structure and physiological function. Hundreds of analogs of the posterior pituitary hormones have been prepared as a consequence of du Vigneaud's work, and his pioneering studies have spawned the recent explosive activity in the peptide field.

Thus far we have been concerned with the story of du Vigneaud's life and with his many scientific accomplishments. We may now ask: Who was this man and what was the atmosphere in his laboratory that promoted such a wealth of fundamental work? His laboratory was extremely well organized. Since he was a very busy man who was not always available for consultations, he initiated a system of colored slips for communicating with him. There was a pink slip for suggesting new ideas and new research approaches, there was a green slip for reporting research results, and, finally, a white slip for requesting microanalytical services. The "greens" were du Vigneaud's favorite. He wanted them at least weekly from every researcher in the group, and he read them with extreme care. To those who were reluctant to write up half-finished experiments, he insisted that that was the fun of the research to him. He couldn't remember (he said) the results presented to him in a neat package at the end nearly as well as if he had been in on them as they developed day by day. Many a collaborator was awed by his memory for details in someone else's research reports, from months or years gone by, which he could bring to bear on the problem at hand. The potential aid thus available, once appreciated, did a lot to lighten the task of grinding out the green slips! But, besides all of this red tape, there was ample opportunity to have a private audience with the chief.

The laboratory was a busy place indeed, and hard work was the order of the day. Graduate students were expected to spend several evenings a week in the laboratory, as well as part of Saturday, and papers were frequently written late into the night. Professor du Vigneaud lived in the suburbs of New York, but he maintained a beautifully furnished room in the department where he spent many a night during the week. These were the evenings when he came to visit with his collaborators. Smoking a White Owl cigar, which he gracefully waved poised between his strong fingers, he shared a cold soft drink with us and discussed the latest research results. Speaking quietly and easily, he used such words as "exciting," "surprising," "intriguing"—all suggesting great pleasure in the stepwise evolution of the research. He was always highly interested in the day's results and was truly devoted to his scientific work. He felt very secure and loved his work. To a reporter he said:

> I have had the privilege and the thrill of following those researches that I've always wanted to do. I've always had the privilege of working oil what I've wanted to work on. I have been accompanied in the various stages of these exploratory researches by a group of fine and loyal associates. I've also been fortunate throughout the years in the generous research support I've received from various sources.

He had a highly critical attitude toward laboratory results and this permeates his writings. Every possible angle of a project was discussed at great length, and new approaches and ideas that could clarify an issue were explored in depth. Papers were written in collaboration with those who did the work; a secretary was present, and while discussions went on she was typing the latest version of a draft. A great many versions were hammered out before the chief was satisfied.

Unquestionably, du Vigneaud was in command, and he was highly respected by his collaborators. He had a jovial manner with people, and every year he invited his entire crew to his home in Scarsdale for a picnic with softball and other entertainment. "Dee," as he was known by his colleagues over the years, associated with a great number of graduate students, postdoctoral fellows, and visiting professors. All the people who ever worked in Dee's laboratory belonged automatically to the V du V Club. He kept in constant touch with us, and every year during the Federation meetings we all got together for beer and pretzels to share time with former colleagues, the chief, and his charming wife, Zella.

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HONORS AND DISTINCTIONS

Degrees

B.S., University of Illinois, 1923
M.S., University of Illinois, 1924
Ph.D. (Biochemistry), University of Rochester, 1927

Honorary Degrees

D.Sc., New York University, 1955
D.Sc., Yale University, 1955
D.Sc., University of Illinois, 1960
D.Sc., University of Rochester and St. Louis University, 1965

University Appointments

National Research Council Fellow, Johns Hopkins University, 1927-28

National Research Council Fellow, Kaiser Wilhelm Institute, Dresden, Germany, and University of Edinburgh Medical School, 1928-29

Associate, University of Illinois, 1927-30

Professor and Head of Department of Biochemistry, School of Medicine, George Washington University, 1932-38

Professor and Head of Department of Biochemistry, Cornell University Medical College, 1938-64

Professor Emeritus of Chemistry, Department of Chemistry, Cornell University, 1964-74.

Memberships

National Academy of Sciences, 1944
American Philosophical Society, 1944
Royal Society of Sciences of Uppsala, 1950
Honorary Fellow of the Royal Society of Edinburgh, 1954
Honorary Fellow, Royal Institute of London, 1959

Awards and Lectureships

Hillebrand Award, Washington Chemical Society, 1936
Foster Lecturer, University of Buffalo, 1939
Harvey Society Lecturer, 1942

Meade-Johnson Vitamin B Complex Award, American Institute of Nutrition, 1942

Hitchcock Lecturer, University of California, 1944

Nichols Medal, New York Section, American Chemical Society, 1945

Borden Award, Association of American Medical Colleges, 1947

Visiting Lecturer, American Swiss Foundation for Scientific Exchange, Switzerland, 1947

Award of Merit for War Research, United States Government, 1948

Lasker Award, American Public Health Association, 1948

Stieglitz Memorial Lecturer, University of Chicago, 1948

Eastman Lecturer, University of Rochester, 1949

Liversidge Lecturer, University of Cambridge, 1949

Special Lecturer, University of London, 1949

Messenger Lecturer, Cornell University, 1950

Herter Lecturer, New York University, 1952

Edsel B. Ford Lecture, Henry Ford Hospital, 1954

Goldforb Lecturer, 1954

Harvey Society Lecturer, 1954

Osborne and Mendel Award, 1954

John Scott Award, Philadelphia, 1954

Remsen Memorial Lecturer, Johns Hopkins University, 1954

Scientific Award, American Pharmaceutical Manufacturers' Association, 1954

Chandler Award, Columbia University, 1955

Annual Hanna Lecturer, Western Reserve University, 1955

Nobel Prize in Chemistry, Nobel Foundation, 1955

Passano Award, Passano Foundation, 1955

Dakin Memorial Lecturer, Adelphi College, 1956

Willard Gibbs Medal, Chicago Section, American Chemical Society, 1956

Nieuwland Lecturer, University of Notre Dame, 1956

Edgar Fahs Smith Lecturer, University of Pennsylvania, 1958

Alumni Achievement Award, University of Illinois, 1959

Martland Memorial Lecturer, 1959

Nutrition Foundation's 20th Anniversary Award, 1961

Pirquet Society of Clinical Medicine Medalist, 1964

American College of Physicians Award, 1965

The Eli Lilly Lecture Award, Endocrine Society, 1967

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Alexander Wetmore

June 18, 1886-December 7, 1978

By S. Dillon Ripley and James A. Steed

Alexander Wetmore—destined to become the most distinguished American ornithologist of the nineteen thirties and forties, to serve as the sixth secretary of the Smithsonian, to be a member of the Academy from 1946 to 1978, and its home secretary from 1951 to 1955—was born in North Freedom, Wisconsin, on June 18, 1886. He died at his home in Glen Echo, Maryland, near Washington, on December 7, 1978, of congestive heart failure. He is survived by his second wife, Annie Beatrice Thielen, of Glen Echo, and a daughter, Margaret Fenwick Holland. In his ninety two years he compiled a remarkable record of service to science, both as an investigator and an administrator. We should like to sum up his career as a scientific administrator, which is less appreciated than it should be; to sketch an outline of his scientific work; and to say something of Wetmore's personal, human side, which was perhaps not well understood even by many who knew him.

Part I

Wetmore spent his early childhood in the small town of North Freedom, the son of Nelson Franklin and Emma Amelia (Woodworth) Wetmore. His father was a physician in the tradition of the country doctor, traveling the countryside in his horse and buggy. His mother kept the home and indulged her bent for reading and study. His parents made his home a place of books and ideas. On graduating from high school, he chose to work his way through the University of Kansas.[1]

Wetmore attributed his interest in ornithology to Chapman's *Handbook of Birds in Eastern North America,* which his mother gave him at age five. His first field entry, made in Florida three years later, was an observation of the pelican: "a great big bird that eats fish." [2] More serious study followed, and in 1900 Wetmore published his first note in *Bird Lore,* [3] recording his observation of a red-headed woodpecker. At first, Wetmore had planned to develop his interest in science as a doctor, but once he found he could make a living as a scientist, he changed his plans and concentrated on science directly. Wetmore first worked in the University of Kansas Museum. Then, in 1911, he took leave to serve as an aide to Arthur Cleveland Bent on a trip to the Aleutian Islands for the U.S. Biological Survey. In 1912 Wetmore received his B.A. from Kansas; he later earned an M.A. (1916) and Ph.D. (1920) at George Washington University.[4]

Following graduation from Kansas, Wetmore rejoined the U.S. Biological Survey as a field agent, rising to the posts of assistant biologist in 1913 and biologist in 1924. He studied the food habits of North American birds and had a chance to meet many of the noted biologists of the day, especially those about the Biological Survey and the National Museum of the Smithsonian Institution. Among others, they included

[1] Alexander Wetmore, "Autobiographical Statement." (Washington, D.C.: National Academy of Sciences, 1945), p. 1.

[2] John Sherwood, "The Museum Life," *The Washington Star;* January 13, 1977, pp. 82-83.

[3] Alexander Wetmore, "My Experience With a Red-headed Woodpecker," *Bird Lore,* II (October 1900): 155-56.

[4] "Alexander Wetmore Oral History Transcript" (Washington, D.C.: Smithsonian Institution Archives, April 18, 1974), p. 5.

C. Hart Merriam, Leonhard Stejneger, Robert Ridgway, Frederick C. Lincoln, Remington Kellogg, and Hartley H. T. Jackson. In 1911 Wetmore spent nearly a year studying the avifauna of Puerto Rico and nearby islands. In 1920 the United States signed a migratory bird treaty with Canada. This took Wetmore to South America, where he spent a year roaming from the Chaco in Paraguay to northern Patagonia, surveying wintering grounds of North American migrants. Nineteen twenty-three found Wetmore leading the *Tanager* expedition to the mid-Pacific, sponsored jointly by the Biological Survey and the Bernice P. Bishop Museum.

In 1924 Wetmore moved from the Biological Survey to the Smithsonian in order to become superintendent of its National Zoological Park. His stay at the National Zoo was brief, for he became the assistant secretary in charge of the National Museum in that same year. In that capacity he was responsible for overseeing the research and museum programs of the Institution in every field except solar radiation and astronomy, which were assigned to Charles G. Abbot. This change was significant for him and for the Smithsonian. Wetmore never pretended to enjoy administration. He admitted afterwards that he had always avoided administrative duty at the Biological Survey, either by leaving for the field or by sponsoring someone else for the post at issue. There are those who come to find administrative work interesting, an end in itself; to this group Wetmore clearly did not belong. Others become administrators reluctantly, never reconcile themselves to the work, and do it badly. Some, like Wetmore, do become reconciled and perform well. Wetmore's moment of choice, which he saw clearly as such, came with his 1924 shift to the Smithsonian. He believed the Smithsonian's way of doing business seemed least likely to hamper his research. Considering his continued scientific output, he judged rightly. Wetmore remained there for twenty-eight busy years, retiring as its sixth secretary in 1952. During that time he accomplished a great deal, both as an administrator and as a researcher.[5]

Those of us who are responsible for administering scientific programs often feel confined by less than adequate resources to support research. Yet many of us, now accustomed to years of relative largesse from foundations and government, forget—if we ever knew—just how limited support for science was before World War II. Wetmore, on the other hand, lived his professional life with that reality. The work he took up in 1924 must have been exceptionally discouraging many times. Soon after his arrival the Institution began to plan for an increase in its capital funds, only to see that effort frustrated, first, by the death of Secretary Charles D. Walcott in 1927 and, second, by the onset of the Depression. To make matters worse, the Smithsonian was losing an older generation of able staff members like Ales Hrdlicka and Leonhard Stejneger, and it often lacked the means to compete effectively to replace them. Nor could it provide the level of support from technical and clerical staff that the Institution's programs required. Salaries were low, even in comparison with government departments like the Biological Survey. Physical facilities were also a problem, one with which Wetmore struggled throughout his career. Thanks to the Smithsonian's reputation and the support of its friends, its collections grew steadily. Thus the Institution, and especially the National Museum, faced a dilemma. On the one hand were its collections, which increased at a great rate over the years. On the other was a museum understaffed by underpaid workers, housed in inadequate space, and lacking properly funded support functions.[6]

Wetmore dealt with all these difficulties carefully and me

5 "Wetmore oral history," pp. 3, 4, 9, 12.
6 "Wetmore oral history," pp. 24-25, 35.

thodically. Some may have felt he was too careful. A fairer evaluation reveals that he was agreeable to new ideas, but also mindful of the practical realities of the times. It was important to him that changes, when they occurred, should not violate the Smithsonian's reputation or appear frivolous. The first order of business was to obtain more money. The Institution, which had always stood in a special relation to the government because of its status as a trust establishment, had collected an assortment of functions that it performed, as reflected in the seven different annual federal appropriations. These lent a miscellaneous and unimpressive air to its presentations. Gradually, the Smithsonian developed a unified presentation aimed at a single appropriation from the government.[7]

At the same time the Institution took steps to revamp its administrative practices. The nineteenth-century system of chief clerks had lingered at the Smithsonian while other government offices reorganized themselves along more efficient lines. Wetmore brought in a specialist in federal budgetary procedures from the U.S. Bureau of the Budget, John Keddy. Keddy used his experience to make a more cogent case for funds and programs to the administration and the Congress. Subsequently Wetmore also brought in John Graf to aid in these reforms. Both later became assistant secretaries; but they are significant here for marking the Institution's turn, under Wetmore's direction, toward more professional management. Wetmore recognized the changing realities with which the Smithsonian must cope.

A special aspect of Wetmore's duties lay in the area of museum exhibits, and they presented something of a problem. The Smithsonian has always had a double function; that

[7] "Wetmore oral history," pp. 25-26. Frank A. Taylor oral history transcript. (Washington, D.C.: Smithsonian Institution Archives, February 27, 1974), pp. 8385.

is, it supports original research and carries out an educational mission to the public, largely through its exhibits. It is not possible that all staff members will be equally interested or capable in both areas. Particularly in Wetmore's tenure, when resources were so thinly spread, the exhibit function suffered. Good exhibits that communicate effectively with viewers are astonishingly expensive, and there was little money to spend. In consequence, Smithsonian exhibits changed very slowly, and the Institution sometimes found itself outdistanced by other organizations' efforts. Finally, after World War II Wetmore, now secretary, approved plans for study and consultation on ways to improve and update the Institution's offerings. The process was necessarily a slow one, and major results did not appear until his successor's tenure, but the beginnings were made under Secretary Wetmore, whose insistence on quality was well repaid by the outcome. [8]

In 1946 Alexander Wetmore had been secretary for two years, and a member of the Smithsonian staff for twenty-two. While he had spent much time dealing with the Institution's problems, that year brought the Smithsonian a new bureau, the Canal Zone Biological Area, located in Panama (now called the Smithsonian Tropical Research Institute). It was a great satisfaction to Wetmore to see this important center established under the Institution's banner; in a world now beginning to be aware of the importance of tropical ecosystems, his efforts must seem almost prescient. Still, old problems did not disappear for the Smithsonian. In 1945 Wetmore had urged introduction of a bill in Congress authorizing construction of a separate building for historical items; a building for engineering and industrial collections, including aviation; and more buildings for the National Zoo. None were approved at that time, but the need was clear to him all the same. In his annual report that year on the condition of

[8] "Taylor oral history," pp. 85-87. "Wetmore oral history," pp. 46-48.

the Institution, he identified several other pressing needs: adequate space for staff, greater support for research, and a program to modernize exhibits. His slow, patient work, starting in circumstances more meager than most of us can recall, was to bear fruit in later years; but Wetmore's diligent, painful spadework lent an important impetus to the hopes of many for the Smithsonian's future. He took the measure of the times and exerted himself with patience and skill in the Smithsonian's behalf.[9]

Part II

From what we have already said of Wetmore's administrative activities, we might expect to find that his scholarly output had fallen off to accommodate the demands of managing the U.S. National Museum. Far from it! By 1964 his bibliography contained 708 entries. Of these, only 107 appeared before 1924, when he began his administrative labors. He began work on his magnum opus, *The Birds of Panama,* in 1944, the year of his appointment as secretary, and had produced three of its volumes by 1972, when failing health caused him to set it aside. Altogether, quite a remarkable record!

Alexander Wetmore was widely regarded as the dean of American ornithologists. He worked extensively in the field of avian paleontology and as a systematic specialist. For some sixty years Wetmore produced a stream of papers on fossil birds. With over 150 such entries, and almost as many new fossil taxa to his credit, he can certainly be said to have contributed more to his field than any other single person.[10]

Wetmore's most intensive work on fossil birds was done

9 *Annual Report of the Smithsonian Institution for 1946* (Washington, D.C.: Smithsonian Institution, 1947), pp. 9-13.

10 S. Dillon Ripley, "Appreciation," in *Collected Papers in Avian Paleontology Honoring the Ninetieth Birthday of Alexander Wetmore, Smithsonian Contributions to Paleobiology,* vol. 27, ed. Storrs L. Olson (Washington, D.C.: Smithsonian Institution Press, 1976), pp. vii, xi.

after excitement over the spectacular nineteenth-century discoveries of Mesozoic birds had faded, but before the rise of much modern interest in avian paleontology. Apart from the California school, he was for years almost the only student engaged in research on fossil birds. For this reason bird fossils from all parts of the United States, as well as such widely separated locales as Inner Mongolia and Bermuda, were continually referred to Wetmore's notice.[11]

For years Wetmore diligently maintained a card catalogue of references from which he prepared three separate editions of a checklist of fossil birds of North America. He also prepared addresses, lectures, and entertaining synoptic papers designed to keep colleagues posted on current developments in avian paleontology. All this he did in addition to regularly producing many basic detailed descriptions and diagnoses of new forms.

Wetmore's first paper on fossil birds involved removing *Paleochenoides miocaenus* from the Anseriformes to the Pelecaniformes. R. W. Schufeldt, who had first described *Paleochenoides,* was not pleased by the younger man's action, but Wetmore's judgment was sound. Just recently the National Museum received specimens possibly representing two new species of *Paleochenoides,* and it seems that they may provide a breakthrough in our understanding of these seabirds. Wetmore's recognition of the true affinities of *Paleochenoides* was a first step in this undertaking. [12]

My own (S.D.R.) interests have long included the family of rails; and it was a large flightless rail, *Nesotrochis debooyi,* found in a Virgin Islands Indian midden, that provided Wetmore's first description of a new bird from osteological remains.[13]

11 *Ibid.,* p. xi.
12 *Ibid.,* p. xii.
13 *Ibid., p.* xii.

Wetmore continued to give attention to the extinct Pleistocene birds of the West Indies, analyzing fossil avifaunas from Puerto Rico, Haiti, Cuba, and the Bahamas. Among his more striking discoveries was the giant Barn owl, *Tyto ostologa,* of Haiti, which he had diagnosed from a fragment of tarsometatarsus. As late as 1959, Pierce Brodkorb, in dedicating a new fossil species of crow from New Providence Island to Alexander Wetmore, remarked that he was "responsible for all previous knowledge of fossil birds of the West Indies."[14]

Wetmore's chief paleontological efforts probably concerned the identification and description of Tertiary birds from North America, especially from the Eocene, Oligocene, and Miocene terrestrial deposits of the western states and the marine Miocene of the east coast. In these areas he laid the groundwork for all future research.

At one time Wetmore's work on the extensive Oligocene deposits of western North America stood almost alone and was correspondingly important to students of that material. But for Wetmore, some of the most interesting fossil deposits were those found nearest home—the Miocene marine beds of the Chesapeake Group. Most of what we know of the birds of these deposits is found in Wetmore's publications.

In 1939 Wetmore published a major paper concerning the Pleistocene avifauna of Florida that established the presence of several birds—like the California condor and the huge vulture *Teratornis*—then known only from the west, especially the Rancho La Brea Tarpits, in Florida. This paper opened a fertile field of investigation in which Wetmore has been followed by other scholars. In his many years of study of paleornithology, Wetmore was often asked to identify material from Pleistocene caves and from Indian middens, an often unrewarding study, but one he pursued steadily all the

[14] *Ibid.,* p. xii.

same. From such studies he published numerous notes showing that the distribution of many modern North American species was once far different than it is now. Together, these studies have made a significant contribution to our knowledge of the effects of Pleistocene climatic changes on avian distribution. [15]

These brief notes touch upon only a few of Alexander Wetmore's contributions to avian paleontology and their significance for present and future research; but perhaps they at least serve to suggest the skill and devotion he spent on this field across a professional lifetime.

Impressive as Alexander Wetmore's fossil studies are, his work as a systematic specialist is yet more so. His arrangement of the sequence of higher taxa of birds, "A Classification for the Birds of the World" (*Smithsonian Miscellaneous Collections,* 139[11]:1-37, 1960), still stands virtually unchallenged. At the time of his death he was trying to complete the fourth volume of his monographic study, "The Birds of the Republic of Panama" (*Smithsonian Miscellaneous Collections,* 150). The volume of material he contributed to the National Museum is immense: some 26,058 skins from North America, Puerto Rico, Hispaniola, the Hawaiian Islands, Uruguay, Paraguay, Argentina, Chile, Venezuela, and Central America. Of skeletal and anatomical specimens, Wetmore prepared and contributed 4,363. The majority came from North America and Puerto Rico; but many are from Central and South America, especially Panama. Of eggs, Wetmore collected 201 clutches from North, Central, and South America. These collections may seem too large in retrospect, yet they form part of the basic resource on which present and future work depends. Today, specialists in taxonomic studies can appreciate the efforts of meticulous collectors like Wetmore, who gathered

[15] *Ibid*., pp. xv, xvi.

sufficient representative material to require only highly specific additional collection.[16]

The number of species and subspecies Wetmore described is equally remarkable. From 1914 he described some 189 species and subspecies of recent birds new to science. Many of these are from central and northern southern America; but one finds significant representation from work in the Caribbean and in some of the Pacific islands. In the midst of all his collecting, Wetmore was wise enough to read the future, too, and became an early supporter of the PanAmerican Section of the International Council for Bird Preservation. With T. Gilbert Pearson, Robert Cushman Murphy, Marshall McLean, William Vogt, Hoyes Lloyd, and Latin American colleagues, he joined in setting up the original organization.[17]

Over the years Wetmore's work involved him in many organizations. In addition to his membership in the Academy, he was a member of the American Philosophical Society; a director of the Gorgas Memorial Institute of Tropical and Preventive Medicine; a trustee of George Washington University; a trustee of the National Geographic Society, and a member and vice-chairman of its Committee for Research and Exploration. Wetmore was an active member of the American Ornithologists Union (Life Fellow, 1919) and served as its president (1926-1929) and its honorary president (1975-1978), the only person thus honored. In addition, he belonged to a number of clubs and social organizations, including the Washington Biologists' Field Club (1915-1978), the Cosmos Club (1925-1978), and the Explorers Club (1927-1978).[18]

Honors and awards came to Wetmore in great numbers.

[16] *Ibid.,* pp. vii-viii.
[17] *Ibid.,* p. viii.
[18] *Ibid.,* p. viii.

They included the Isidore Geoffroy St. Hilaire Medal of the *Societe Nationale d'Acclimitation de France* (1927), the Otto Herman Medal of the Hungarian Ornithological Society (1931), the Brewster Medal (1959) and the Elliott Coues Award (1972) of the American Ornithologists Union, the Explorers Club Medal (1962), the Bartsch Award of the Audubon Naturalist Society (1964), and the Arthur A. Allen Medal of the Cornell Laboratory of Ornithology (1970).

Gratifying as these honors were, it may be that Wetmore most appreciated the esteem of colleagues who remembered him in their own work—the colleagues who created for him what he called "my zoo." Some 56 new genera, species, and subspecies of recent and fossil birds, insects, mammals, mollusks, and amphibians bear his name. The latest was a 1976 description of a new genus and species of fossil bird from Baja California, described by Pierce Brodkorb and referred to a new family (*Alexornithidae*) and new order (*Alexornithiformes*).[19]

Part III

Wetmore outlived most of his contemporaries, and there are now comparatively few who knew him except as a professional colleague. It seems only right that we should record something of his character here to color the portrait of the scientist-administrator.

Wetmore had married Fay Holloway in 1912, and they had one daughter, Margaret Fenwick Holland. Sadly, Mrs. Wetmore was very ill for many years before her death in 1953; this unhappy circumstance affected Wetmore deeply. This sadness probably added to a certain aloofness in his public manner. Nevertheless, to those who knew him well, Wetmore displayed a warmer, more human side. He was very

[19] Paul H. Oehser, "In Memoriam: Alexander Wetmore," *The Auk,* 97 (July 1980): 612.

fond of talking with the country people he met during his field trips. His amiability also proved useful when Wetmore wanted entry to private land to collect specimens. Landowners are often reluctant to permit strangers with guns on their property, especially if they own cattle. Wetmore was quite scrupulous in asking consent. Such was his skill, that he met with few refusals on his field trips. Of course, he was suitably discreet if he suspected he was on touchy ground —say, near a moonshiner's still. Yet his shrewd judgment was not always perfect. Watson Perrygo, an old friend and field colleague, reports that Wetmore was a perennial soft touch for loans at the Smithsonian, but not always a good judge of who would repay him.[20]

Wetmore was quite meticulous in everything he did, and he expected the same of his colleagues. In the field he always wore a khaki uniform, complete with khaki tie. His friend Perrygo recalled that Wetmore expected the camp kit to be laid out with geometric precision, so much so that Wetmore preferred doing it himself. Perhaps his concern for order and efficiency helped make Wetmore the crack shot he was. Once, in dense cover, Wetmore and Perrygo sighted a bird they wanted for a specimen. The shot seemed doubtful to Perrygo and he called to Wetmore, "Did you hit him?," to which Wetmore replied, "I shot didn't I?"[21]

Wetmore was also a calm, matter-of-fact man. In 1920 his work on the then-new migratory bird treaty with Canada required him to spend some time in Argentina. Argentina had sympathized with the Central Powers in the war just ended. Feelings were still tense, and Wetmore was unsure of his reception. His approach was simply to appear and say, "Here are my papers; this is what I'm doing. What do you expect

[20] Watson M. Perrygo oral history transcript (Washington, D.C.: Smithsonian Institution Archives, October 30, 1978), pp. 417, 427, 429.
[21] "Perrygo oral history," pp. 407-9, 417, 441, 446, 517.

to do about it?" To this his hosts for the time being replied, "Nothing." So Wetmore replied, "Thank you very much," and lived more or less as he pleased.[22] His calm appraisal of opportunities for study and learning became even more evident during one of his trips to Panama. Wetmore found himself the unwilling host to screw worm larvae, which had entered his leg, causing considerable pain. Yet, when polticing had finally extracted the larvae, he was careful to preserve them for use in our entomology collections.[23]

Perhaps Secretary Wetmore's finest attribute was his absence of self-importance. His experience as an administrator had led him to believe that everyone reached a point in his career when he should step aside for other, fresher men and women to take up the work. He commented that he had seen the unfortunate results of clinging to a position too long, and he was resolved not to make that error. So it was that he resigned as our sixth secretary in 1952, ending a career of twenty-eight years at the Smithsonian. It is a real satisfaction for us to record this tribute in his memory.[24]

A modest and gentle man of whom it could be said that he served all his responsibilities with conscience and humanity, Dr. Wetmore was never to lose his love of birds, and continued his active research program until his death at the age of ninety-two. A number of his uncompleted works, the fourth volume of his monumental *The Birds of the Republic of Panama* (Vol. I, 1965; Vol. II, Vol. III, 1972), for example, has been completed by Natural History Museum staff; also forthcoming are a number of uncompleted papers, which will be published collectively under the title *Wetmorea.*

[22] "Wetmore oral history," p. 8.
[23] John Sherwood, "The Museum Life," *The Washington Star,* January 13, 1977, p. 82.
[24] "Wetmore oral history," p. 45.

It is worth recording that the Smithsonian held a memorial service for Dr. Wetmore on December 18, 1978, in the Castle building. This service was in continuation of a tradition established with previous secretaries who had died in office. The service was attended by members of the Regents, the President of the National Academy of Sciences, and the Chairman of the National Geographic Society Board, with which cognate organizations Dr. Wetmore had been so closely associated. In connection with this service, it might be worth quoting part of the eulogy that was delivered at that time: "Through Dr. Wetmore's intervention in the Public Buildings Act of 1945, calling for museums of History, as well as Engineering, Industry, and Aviation to be built, the germs of two immensely popular buildings on the Mall at the present time were inculcated in the public mind. These were the Museums of History and Technology, completed in January 1964, and the National Air and Space Museum, completed in 1976. Such episodes remind one of the legislative birth pangs of what subsequently may come to fruition many years later"—in this case, more than twenty years. These episodes are characteristic of what has happened in the history of the Institution itself where the idea of creation often precedes by many years the fruition in the form of a new structure or a new discipline within the Smithsonian. We quote finally from the conclusion of that eulogy: "Alexander Wetmore will always be respected for his scientific contributions and loved for himself. He lived in the 'traditions of civility'. He has gone from us in the accomplishment of grace."

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