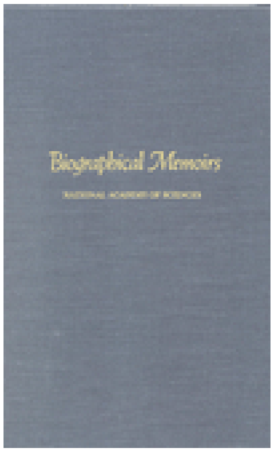

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OF THE UNITED STATES OF AMERICA

Biographical Memoirs

VOLUME 68

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PREFACE

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

The *Biographical Memoirs,* begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work of our most distinguished leaders in the sciences, as witnessed and interpreted by their colleagues and peers. They form a biographical history of science in America—an important part of our nation's contribution to the intellectual heritage of the world.

PETER H.RAVEN

Home Secretary

Biographical Memoirs

VOLUME 68

J. A. B. Bjerknes

JACOB AALL BONNEVIE BJERKNES

November 2, 1897–July 7, 1975

BY ARNT ELIASSEN

ACOB AALL BONNEVIE BJERKNES, or Jack Bjerknes as he was usually called, was one of the founders of modern meteorology. He entered the scientific scene at the age of twenty with the discovery of the structure of extratropical cyclones, which became of the greatest importance and formed the starting point of a fruitful development for theoretical meteorology as well as practical weather forecasting. It was Jack's father, the famous physicist and geophysicist Vilhelm Bjerknes, who set the stage for the research leading up to this discovery, but Jack was the principal performer.

FAMILY BACKGROUND

The name Bjerknes comes from a family farm in southeastern Norway where some of Jack's ancestors lived. Jack represented the third generation in a dynasty of scientists. His grandfather, Carl Anton Bjerknes, was professor of mathematics at the University of Christiania, as the Norwegian capital Oslo was called at the time. He showed both theoretically and by experiment that an ideal fluid would transfer Coulomb-type forces between pulsating spheres and thought that he was on the track of a hydrodynamical ether theory of electromagnetism.

Jack's father, Vilhelm Bjerknes, began his career as a physicist working with Heinrich Hertz in Bonn on electromagnetic resonance. In 1893 he married Honoria Sophia Bonnevie, a Norwegian science student in Christiania. They settled in Stockholm, where Vilhelm Bjerknes was appointed lecturer and later professor of mechanics and mathematical physics.

Jack Bjerknes was born in Stockholm and spent his childhood years there. He was named after his mother's father, Jacob Aall Bonnevie, a prominent civil servant and minister of education in Norway. Jack's aunt, Kristine Bonnevie, was Norway's first woman professor; her field was zoology. Young Jack thus grew up in an academic family.

In 1897, the year Jack was born, his father, Vilhelm Bjerknes, discovered the circulation theorem that bears his name. It generalizes Helmholtz's and Kelvin's theorem on vortex conservation in ideal fluids into a theorem on vortex formation in nonhomogeneous fluids. With this theorem, Vilhelm Bjerknes realized that he now was in possession of the complete set of hydrodynamic/thermodynamic equations that govern the motion of nonhomogeneous fluids. Encouraged by his Swedish colleagues, among them the famous chemist Svante Arrhenius and the oceanographer Otto Petterson, he set out to apply the theory to the motions in the atmosphere and the sea. He put forward the view that weather forecasting should be dealt with as an initial value problem of mathematical physics and carried out by numerical or graphical integration of the governing equations. This is nothing more than treating the atmosphere as a physical system; but at the time it was a revolutionary idea.

Vilhelm Bjerknes visited the United States in 1905. At the initiative of the renowned American meteorologist Cleveland Abbe, he gave a lecture in Washington, D.C., where he described his vision of scientific weather prediction. The lecture was enthusiastically received and resulted in a yearly grant from the Carnegie Institution of Washington, which he retained until the Second World War. The money could hardly have found a better use; it enabled Vilhelm to employ and educate a considerable number of research assistants, all of whom became well-known geophysicists.

In 1907, when Jack was nine, the family moved to Christiania, where Vilhelm was called to a chair at the university. In cooperation with his Carnegie assistants, the Swede Johan Sandström, and the Norwegians Olaf Devik and Theodor Hesselberg, he published a substantial work, *Dynamic Meteorology and Hydrography.* In Germany they were impressed and offered him a position as director of a new geophysics institute at the University of Leipzig. He accepted and with his family moved to Leipzig in 1913. Jack, however, stayed in Christiania to finish junior college and begin science studies at the Norwegian university.

IN LEIPZIG DURING WORLD WAR I

The Geophysics Institute in Leipzig started out successfully. Vilhelm Bjerknes brought with him his two Carnegie assistants, T.Hesselberg (later director of the Norwegian Meteorological Institute) and H.U.Sverdrup (later director of the Scripps Oceanographic Institution). In addition, there were several German doctoral students and research assistants, among them Robert Wenger, who followed Bjerknes as director of the Leipzig institute. Then World War I broke out, and many of the German students and staff were called to war service. Sverdrup and Hesselberg also left, and Vilhelm Bjerknes was in great need of help for his research.

In 1916 Jack Bjerknes, not yet nineteen, interrupted his studies in Norway and went to Leipzig to join his family and assist his father. With him went another Norwegian student, Halvor Solberg.

In Leipzig a German doctoral student, Herbert Petzold, had been studying convergence lines in the wind field. But Petzold was sent to the front and was killed at Verdun in 1916. Jack Bjerknes took over his research. He found that convergence lines may be thousands of kilometers long, tend to drift eastward, and are connected with clouds and precipitation. He reported these results in his first scientific paper, which appeared in print before he was twenty.

As the war went on, the situation for the Leipzig Geophysics Institute worsened, with lack of labor and food shortages. Through the intervention of the Norwegian oceanographers Fridtjof Nansen and Bjørn Helland-Hansen, a professorship was established for Vilhelm Bjerknes in Bergen in western Norway.

THE BERGEN SCHOOL

There was no university in Bergen at the time; but plans for a science faculty existed, and Bergen Museum served as a nucleus for such a development. Helland-Hansen held a position as professor of oceanography at Bergen Museum; he had for many years given international courses in ocean research. The establishment in 1917 of a new geophysics institute at Bergen Museum with a professorship for Vilhelm Bjerknes was an important step toward strengthening the academic milieu in Bergen.

Vilhelm Bjerknes left wartime Germany and arrived in Bergen in the summer of 1917 with two young assistants, Jack Bjerknes and Halvor Solberg. He realized that he would not have in Bergen the resources for a theoretical attack on the problem of weather prognosis and planned instead a push toward practical weather forecasting by offering a special summer forecasting service for agriculture. With sup port from the Norwegian government, he arranged for a nearly tenfold increase in the number of observing stations in southern Norway.

With these preparations, the forecasting started in the summer of 1918. Vilhelm Bjerknes did not himself take part in the map work but arranged to have Jack as forecaster in Bergen and Solberg in Christiania. The war was still on, and no weather data were received from France, England, or the Atlantic. From the improved data network in Norway, however, Jack could again identify convergence lines of the type he had studied in Leipzig, as they moved along the Norwegian coast. Moreover, he discovered that these convergence lines, which were later termed fronts, were connected with cyclones in characteristic manner. In a paper ("On the Structure of Moving Cyclones") written in the fall of 1918 before he was twenty-one, he presented his famous frontal cyclone model (see Figure 1). The fronts in the model were assumed to represent boundary surfaces separating cold air to the north and west of the cyclone center from warm air in the warm sector to the south and southeast. These frontal boundary surfaces were assumed to be sloping with the cold air on the underside, in accordance with a formula derived in 1903 by the Austrian meteorologist Max Margules. Furthermore, Jack stated in his paper that warm air ascends along the sloping frontal surfaces, causing bands of clouds and precipitation to form along the fronts, whereas the cold air sinks and spreads out along the ground. He noted that these vertical motions represent a reduction of the potential energy, which could account for the formation of the cyclone's kinetic energy, in agreement with Margules's theory published fifteen years earlier. Jack also mentioned in his paper the notion of cyclone series following the same path, with the trailing cold front after one cyclone serving as a warm front of a new cyclone farther west. He even discussed the role of cyclones in the general circulation of the atmosphere and ascribed to them a role as a link in the interchange of air between the polar regions and the equatorial zone.

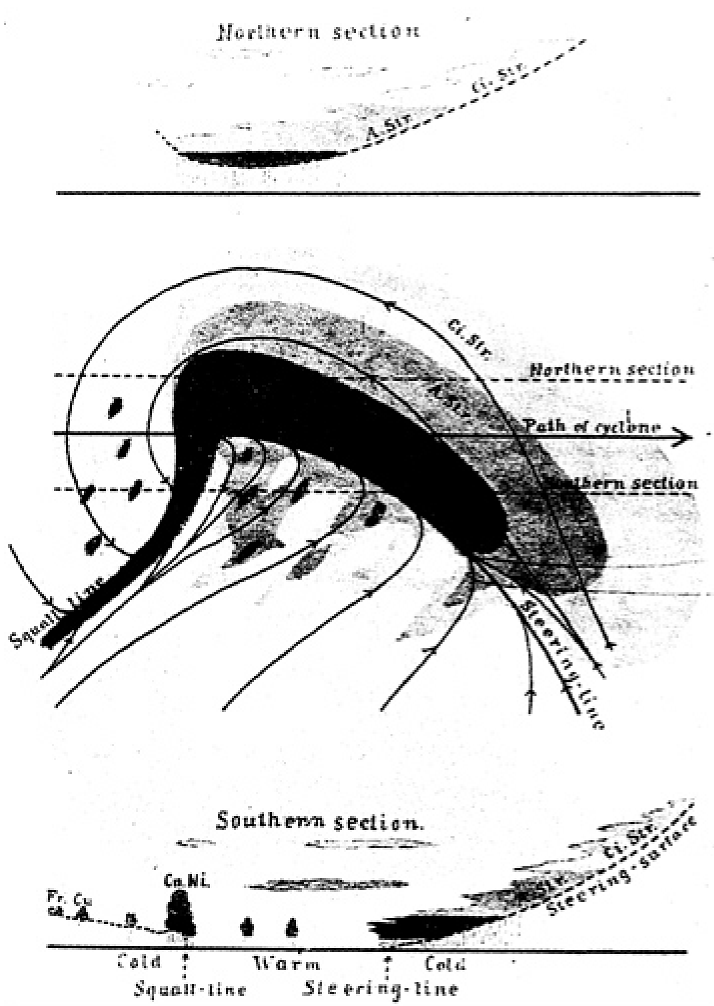

FIGURE 1 Jack Bjerknes's cyclone model: streamlines, clouds, and precipitation and vertical cross-sections north and south of the center. (From Bjerknes, 1919.)

On the whole, Jack's short paper of eight pages contained an abundance of very interesting thoughts and suggestions. It was a fantastic achievement for a twenty year old after a few months of work with weather maps from a limited part of Europe.

In the following years, Jack Bjerknes's frontal cyclone model was subject to intense study by the team consisting of Jack, Solberg, and Tor Bergeron, a Swedish student who joined the Bergen team in 1919. Solberg reanalyzed old weather maps from the Atlantic with many observations from ships. He found evidence that a polar front exists as an undulating line across most of the ocean, with new cyclones forming as growing waves on the front. Bergeron found that in a later stage of the cyclone's life, the cold front will overtake the warm front, lifting the warm sector air to higher levels, whereas the cold air spreads out along the ground. This process he called occlusion.

The Bergen team could now formulate a four-dimensional cyclone model, with a typical structure and a typical life cycle. As Theodor Hesselberg put it: "[The cyclone] is born as Solberg's initial wave on the polar front, develops into Jack Bjerknes's ideal cyclone, and finally suffers the Bergeronian occlusion death." It is noteworthy, however, that many of the features of the life cycle of cyclones were already contained in Jack's original paper, which he wrote in the fall of 1918.

In 1920 Jack Bjerknes was appointed head of the Weather Forecasting Office for western Norway. Here weather map analysis and forecasting were based on the frontal cyclone model. The frontal positions give information about winds, temperature, clouds, and precipitation; moreover, the shape of a frontal cyclone is indicative of its stage of development and can thus give information about its future behavior. The frontal cyclone model thus turned out to be an extremely useful tool in weather forecasting.

So far, the cyclone model was mainly based on observations from the ground; its vertical structure was mostly inferred from theory. In 1922 Jack Bjerknes went to Zurich in Switzerland as an invited consultant to the Swiss Meteorological Institute. By means of data from mountain peak observatories in the Alps, he could verify the existence of sloping frontal surfaces up to an altitude of 3,000 meters. For the paper he wrote about this investigation, he was awarded the degree of doctor of philosophy by the University of Oslo in 1924.

In 1928 Jack married Hedvig Borthen, daughter of a well-known ophthalmologist in Bergen. After some years, Jack and Hedvig settled into a house in Hop, south of Bergen, built on a lot bought from Hedvig's father.

THE UPPER WAVE

In the 1920s further exploration of air flow at higher elevations was hampered by lack of observations from these levels. However, during three consecutive days in December 1928 when two cyclones passed over Europe, P.Jaumotte, director of the Royal Belgian Meteorological Institute, launched thirty-one instrumented balloons at Uccle, Belgium. Of these, twenty-five were recovered. The recordings were analyzed by Jack Bjerknes, and the results were given in a paper that was one of his most brilliant. It contains probably the first description of the waves in the upper-tropospheric westerlies, which are connected with the cyclones at low levels. The wave trough was found to be located above the cold front at the ground, and the vorticity connected with this trough was correctly ascribed to vertical stretching as the warm air descends down the sloping cold front surface. This is the first attempt toward a dynamic treatment of the upper wave.

In 1931 Jack left the leadership of the Weather Forecast ing Office in Bergen to Sverre Petterssen and took over a professorship of meteorology that was established for him at the Bergen Museum.

As radiosonde data in the 1930s became sufficiently numerous to make possible a systematic diagnosis of the motions in the upper troposphere, Jack was quick to exploit these new possibilities. In a number of papers, partly in cooperation with the Finnish geophysicist Erik Palmén, he constructed cross-sections through fronts and tropopause and showed how the cold front surface of one cyclone turns into the warm front of the next. In a paper from 1937 he pointed to the meridional gradient of the Coriolis parameter as an important quantity in the dynamics of upper waves. This work inspired Carl-Gustaf Rossby to derive his celebrated wave formula.

A close collaborator of Jack's in the 1930s was Carl Ludvig Godske, Vilhelm Bjerknes's Carnegie assistant who later succeeded Jack in the chair in Bergen. Godske and Jack wrote an interesting paper together on instability of fronts; and Godske wrote the greater part of an extensive volume titled *Dynamic Meteorology and Weather Forecasting,* coauthored by Godske, Bergeron, Jack Bjerknes, and R.C.Bundgaard. The book was planned by Vilhelm Bjerknes in the 1930s but was delayed by World War II and did not appear in print until 1959.

Jack Bjerknes's work in Bergen aroused the attention of his colleagues. He received visitors from many countries and went himself on trips to Switzerland, England, the Netherlands, Germany, Canada, and the United States as an invited guest lecturer.

WAR YEARS

In July 1939 Jack Bjerknes, with his family, went on what was supposed to be an eight-month lecture tour to the United

States. But on September 1 of that year, World War II began, and later came the German invasion of Norway. For Jack Bjerknes and his family, this had the consequence that they stayed in America and became U.S. citizens.

The threatening international situation made it imperative for the United States to educate a considerable number of meteorologists for military operations. Jack was asked to organize a training school for Air Force weather officers at the University of California. His wife recalls that he chose the Los Angeles campus for this undertaking in order to be near the Scripps Oceanographic Institution in La Jolla; he was of the opinion that cooperation with oceanographers was important. In 1940 he joined the faculty of the University of California at Los Angeles as professor of meteorology and head of the Section of Meteorology in the Department of Physics. He brought with him Jørgen Holmboe, a Norwegian meteorologist from the Bergen Weather Service who had spent three years in Rossby's department at MIT. During the war, Jack visited England, Italy, Hawaii, and Guam as a consultant to the U.S. Army Air Corps.

A NEW METEOROLOGY DEPARTMENT

In 1945 a new Department of Meteorology was established at the University of California at Los Angeles, with Jack Bjerknes as chairman. The new department grew fast and soon became one of the world's leading centers of teaching and research in the atmospheric sciences.

During the war, Jack, in collaboration with Holmboe, attempted to treat theoretically the problem of the growing cyclone with its associated upper wave. One cannot say they solved the problem, but they threw new light on it, and their work inspired their first doctoral student, Jule Charney, to come out with the first mathematical solution describing growing waves on a baroclinic current.

It had been known for many years that the atmospheric westerlies in middle latitudes increased with height and usually reached a maximum near the tropopause. For instance, a calculated distribution of the mean west wind in the meridional plane is given in *Physikalische Hydrodynamik,* the volume published by V.Bjerknes, J.Bjerknes, H.Solberg, and T.Bergeron in 1933. The improved wind measurements after the war revealed that the upper westerlies—the jet stream—are often much stronger than previously assumed. How was this strong current circumventing the earth, one in each hemisphere, maintained against frictional dissipation? The English mathematician and geophysicist Harold Jeffreys had already, in 1933, proposed an answer to this pressing question by suggesting that angular momentum could be transferred from low to middle latitudes by atmospheric waves and eddies. Based on Jeffreys's theory, Jack Bjerknes started a major research project on the general circulation of the atmosphere. His principal co-worker was Yale Mintz; but invited scientists from many countries participated. They collected data from the entire northern hemisphere and calculated meridional fluxes of angular momentum and energy and many other statistics. The results verified Jeffreys's thesis and were in reasonable agreement with results from Victor Starr and his group at MIT, who ran a similar project at the same time. Our quantitative knowledge of the general circulation was greatly advanced by these two research projects.

AIR-SEA INTERACTION ON A GLOBAL SCALE

Toward the end of the 1950s, when Jack Bjerknes was around sixty, he turned his mind to a new field of research that engaged him for the rest of his life—the interaction of atmosphere and sea.

Jack credits C.G.Rossby, H.U.Sverdrup, and Bjørn

Helland-Hansen for giving him inspiration and encouragement to take up this new field. These three scientists all died in 1957, when Jack was starting his oceanographic studies. Helland-Hansen belonged to an older generation; he had made pioneering studies of the Atlantic Ocean in cooperation with Fridtjof Nansen before and during World War I. Jack was his colleague in Bergen for twenty years and was familiar with his and Nansen's work.

Jack first took up the study of the warming of the North Atlantic Ocean at the beginning of the century and found that it could be explained by an increased wind drag that speeded up the Gulf Stream. The interannual variations of the sea surface temperature in the North Atlantic were his next study subject. He discovered that these temperature variations are connected with the strength of the westerlies. Years with particularly strong westerly winds in middle latitudes would display a typical pattern of sea surface temperature anomalies, with unusually cold water south of Iceland and Greenland and warmer water in the Gulf Stream outside the Grand Banks. In a series of papers he discussed the physical processes involved and gave qualitative explanations of the observed anomalies of the sea surface temperatures.

Jack's studies of the Pacific Ocean are even more remarkable. He began with an investigation of the El Niño phenomenon. Once every two to five years the cool nutrition-rich waters off the coast of Peru are replaced in the Southern Hemisphere summer by warmer sterile water, with a catastrophic result for Peruvian fishing and production of guano fertilizer from sea birds. These episodes are known as El Niño (the Holy Child, since they usually set in at Christmas time).

Jack found that El Niño is not a local phenomenon confined to the Peruvian coast but the manifestation of an oscillatory process that affects the atmosphere and the ocean over the entire tropical Pacific. During the El Niño years, a huge area of the eastern and middle equatorial Pacific may be as much as 2 K warmer than normal. To the atmosphere, such a disturbed Pacific Ocean must represent a very strong additional source of heat and moisture. The immediate effect is increased rainfall locally in the region of warm sea surface. But Jack also looked for manifestations of a strengthened Hadley circulation, and he found an increased west wind in the northern Pacific, with distant effects on the weather in North America and possibly also in Europe. These teleconnections over large distances recurred when new El Niño episodes occurred.

Jack also established a connection between the El Niño phenomenon and the southern oscillation, an irregular pulsation of atmospheric pressure between the Pacific and the Indian Oceans discovered by Sir Gilbert Walker in the 1920s. To account for variations in rainfall, Jack envisaged a vertical air circulation along the equator in the Pacific area, which he called the Walker circulation, since its strength would vary with Walker's southern oscillation. As a result of his investigations, we now have a coherent picture of these large-scale processes in the equatorial Pacific.

Jack Bjerknes's research on air-sea interactions is particularly important because these processes play an essential role in the theory of climate. Today, when the earth's climate is being threatened by human activities, research aiming at predicting possible climate changes is carried on vigorously in many countries. Such research, which is so vitally important to humankind, can build on Jack's results and the wealth of ideas contained in his papers.

CONCLUSION

Jack Bjerknes was active as a scientist for more than fifty-five years. He was a modest and kind man, always generous with his time to listen to the problems of students and colleagues and always comforting them with his wise counsel and guidance. But he never wasted his time. His waking hours were devoted to work and studies of his problems, never in a great hurry but never stopping. He was very persistent; when he took up a problem, he would not let go of it until he had done his utmost to have it clarified.

Through the years many friends and colleagues enjoyed Jack and Hedvig's hospitality in their Santa Monica home. They also kept a house outside Bergen and often spent their holidays in Norway. Jack held contact with Norwegian colleagues, many of whom were invited to UCLA as visiting scientists.

More than any other atmospheric scientist, Jack Bjerknes managed to create order and system in a seemingly disorderly atmosphere. It is most remarkable that after seventy-five years his frontal cyclone model is still used as a principal tool in the world's weather services. Nobody knows how many lives have been saved through the years as a result of the improved methods of weather forecasting that Jack Bjerknes instituted.

IN PREPARING THIS MEMOIR I received very useful information and advice from Mrs. Hedvig Bjerknes and from Professors Morton G. Wurtele and Akio Arakawa. I also used information extracted from Robert Marc Friedman's book, *Appropriating the Weather* (Cornell University Press, Ithaca, N.Y., 1989). A very useful selection of Jack Bjerknes's papers, with a complete bibliography, was published by M.G.Wurtele (*Selected Papers of Jacob Aall Bonnevie Bjerknes,* Western Periodicals Co., North Hollywood, Calif., 1975).

HONORS AND DISTINCTIONS

Honorary Fellow, Royal Meteorological Society, 1932

Symons Medal, Royal Meteorological Society, 1940

Bowie Medal, American Geophysical Union, 1945

Meritorious Civilian Service Medal, U.S. Air Force, 1946

Royal Norwegian Order of St. Olav, 1947

Vega Medal, Swedish Society of Geography, 1958

International Meteorological Organization Prize, World Meteorological Organization, 1959

Carl-Gustaf Rossby Award, American Meteorological Society, 1960

Robert M.Losey Award, Institute of Aerospace Sciences, 1963

President, Meteorological Association, International Union of Geodesy and Geophysics, 1948–51

National Medal of Science, 1966

Honorary Member and Fellow, American Meteorological Society, 1966

Honorary Doctor of Laws, University of California, 1967

Member, Royal Norwegian Academy of Sciences, Royal Swedish Academy of Sciences, Danish Academy of Technical Sciences, Academy of Sciences (India), American Academy of Arts and Sciences, and National Academy of Sciences

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HUBERT MORSE BLALOCK, JR.

August 23, 1926–February 8, 1991

BY HERBERT L.COSTNER

N HIS EXCEPTIONALLY productive life, Hubert Blalock played a major role in shaping the field of sociology during the latter half of the twentieth century. His vision of social science inspired his students and colleagues as much as his teaching and writing instructed them. Although his life took some surprising turns in his youth, his career as a sociologist was surprising only to those who underestimated his commitment and creativity.

Hubert Morse Blalock, Jr., was born in Baltimore, Maryland, on August 23, 1926. His mother, born Dorothy Welsh, was the daughter of a prosperous hat manufacturer in Baltimore. She met her future husband at Johns Hopkins University where both were working toward a master's degree in history. Hubert Blalock, Sr., was raised by his mother, a widowed schoolteacher in North Carolina. Following completion of his master's degree in history and a degree in law, the elder Blalock accepted employment in the legal department of the casualty division of the Aetna Casualty and Surety Company of Hartford, Connecticut. He remained with Aetna for his entire career and retired as a senior officer of the company.

It was in West Hartford that Hubert Blalock, Jr., had his early schooling. Young Tad Blalock, whose childhood nickname lasted a lifetime, was a bright and active boy for whom the public school program in the elementary grades was not very challenging. As Tad was about to enter the seventh grade, his parents decided that a private school might be better able to channel his energies in constructive ways. They selected the well-respected Loomis School, which was relatively close to home and hence did not require Tad to become a boarding student.

Young Blalock blossomed at his new school, especially in mathematics. After the Loomis faculty noted how he raced through the established mathematics curriculum, they developed more advanced courses especially for him. When he graduated from the Loomis School in June 1944, World War II was under way. Tad knew that he would enter military service shortly after he turned eighteen, but he had time for one semester at Dartmouth before entering the U.S. Navy in December 1944.

Nothing in his background had prepared eighteen-year-old Tad Blalock for his two years in the Navy. Tad himself later wrote:

> I was a total misfit in the Navy, from the very first day when our Chief Petty Officer delivered a speech ending with the sentence, “Remember, youse guys, in the Navy you don’t think!” (Blalock, 1988, p. 107)

Tad’s mathematical aptitude landed him in radar training school in Chicago immediately after his induction. Although he could readily master the textbook principles, he found the hands-on applications tedious and uninteresting. He requested a transfer to sea duty and soon found himself assigned as a radio operator on a Landing Ship Tank (LST) serving in and around Shanghai.

In the Navy Blalock first encountered the full variety of American youth. He was more puzzled than pleased by his new compatriots, and he didn't fit into the common routine of drinking, carousing, and hackneyed obscenities. When their LST visited Chinese port cities, Tad was appalled to find that some of his shipmates "delighted in their nightly fisticuffs (and worse) with the so-called 'gooks'" (Blalock, 1988, p. 108). For Tad Blalock the Navy provided both an eye-opening and a heart-rending experience. It awakened in him a deep sympathy for those who were poor and subservient and a vague commitment to make their lives better.

In 1946, after completing his Navy service, Tad Blalock returned to Dartmouth, where he majored in mathematics. He was elected to Phi Beta Kappa in 1948, awarded the Thayer Mathematics Prize in 1949, and in the same year was accorded a bachelor of arts degree in mathematics, *summa cum laude*. But as a Dartmouth undergraduate he had also discovered new interests. His concern for the poor and subservient, initially kindled by his Navy experience, was reinforced by taking the "Great Issues" course required of Dartmouth seniors. He had also found an off-campus way of expanding his insights into the world of the under-privileged. He had become involved in the workshops of the American Friends Service Committee. During the summer following completion of his bachelor's degree at Dartmouth, Tad worked in a Quaker work camp in a black area. He later reported that he "had always had a concern about the treatment of blacks in America—perhaps a Myrdallian white guilt complex" (Blalock, 1988, p. 109). The Quaker summer camp provided his first experience of daily contact with black people.

In the fall of 1949 Tad started work on a master's degree in mathematics at Brown University. There, he

> discovered the meaning of "pure" mathematics, as well as the impact of

> absolutely horrible teaching. At about that time I began to realize that I did not want to spend my lifetime being quite so pure, and that there was something of an escape from reality in all of this. (Blalock, 1988, p. 109)

Tad remained at Brown for his master's degree, but in 1950 he shifted from mathematics to sociology "almost sight unseen." He had previously had only two sociology courses.

One of the friends whom Tad encountered in the workshops of the American Friends Service Committee was Ann Bonar from West Virginia. Following her graduation from Oberlin College in 1950, Ann had come to Boston as a research assistant on a Harvard University endocrinology project at Massachusetts General Hospital. She spent her weekends as a volunteer at Peabody House, a famous old West End settlement house. There in the fall of 1950 she met Tad Blalock. He was a graduate student at Brown who came to Boston about every other weekend to participate in the Quaker workcamp at Peabody House.

Ann Bonar and Tad Blalock rapidly discovered their common values and interests. They first discussed marriage while walking around Walden Pond, and Tad gave Ann an engagement ring in the spring of 1951. They were married in August 1951 in Parkersburg, West Virginia, Ann's hometown. They spent their honeymoon camping and canoeing in the Adirondacks in New York. Tad's mother never quite understood his unusual tastes, and she maligned him for taking this fragile girl into the wilderness and making her sleep on the ground for two weeks.

The newlyweds were both intent on continuing their education, Tad in sociology and Ann in social work. Having searched intensively for a university with strong programs for both, they decided on the University of North Carolina. Professors Howard Odum, Rupert Vance, and Guy Johnson were the influential elders of the North Carolina Department of Sociology at that time, but a somewhat younger group of faculty members also influenced Tad's academic development. His interest in the use of statistics in social research was fostered by Daniel Price, while Nicholas Demerath influenced his thinking about sociological theory. Guy Johnson was his principal mentor in the field of race relations.

Tad spent only three years at North Carolina, a relatively brief time for a sociologist to complete a Ph.D. By the time he left in 1954, he had pushed himself through a demanding reading program, completed a minor in mathematical statistics, and finished a dissertation. His dissertation raised some eyebrows in the sociology department, where the established practice was to undertake an empirical study for the dissertation. Contrary to the usual practice Tad's dissertation was an attempt to achieve a more systematic theoretical formulation in the field of race relations, drawing on the work of Robin Williams, E.Franklin Frazier, and others.

Although new Ph.D.s in sociology were not in high demand in 1954 Blalock was highly recommended by his mentors, and his first academic position was at the University of Michigan. The prevailing practice at that time was for new Ph.D.s to begin, not as assistant professors, but as instructors and to carry teaching loads that were considered, a decade later, inordinately burdensome. Twenty-eight-year-old Tad Blalock, instructor in sociology, was in charge of his department's statistics courses, both graduate and undergraduate. He also taught introductory sociology and the undergraduate course on research methods, and he served as an academic counselor for undergraduate majors and incoming graduate students.

Enthusiasm notwithstanding, classroom teaching did not come naturally to Instructor Blalock in his first years as a faculty member. One of the first things he had to learn about teaching was that everyone did not grasp abstract mathematical concepts as readily as he did, and the panic and tears of some of his early students in statistics courses prompted him to devise teaching procedures that went beyond the usual classroom lecture. He worked to develop improved ways of communicating with the students in his classes. But, even more, he devoted additional teaching and consulting time to his students, offering extra sessions and special assistance to those who wanted to take advantage of them. Even in his first years of teaching, Tad showed evidence of the kind of concern and the extra time commitment that were to earn for him the high respect of several generations of students.

As demanding as his teaching duties were in those early years, Blalock rapidly started to accumulate a publication record. He had tremendous energy and drive. He loved what he was doing, and he frequently worked late into the evening and all through the weekend. During the first six years following completion of his Ph.D. (1954–60), he published eleven papers in scholarly journals, including papers in *The American Sociological Review, The Journal of the American Statistical Association,* and *Social Forces.* Occasionally, Tad and Ann Blalock published jointly, beginning in 1959 with a paper in *Philosophy of Science*.

Other joint products of Tad and Ann during their years at Michigan were two daughters: Susan Lynn (1956) and Kathleen Ann (1958).

While the flurry of journal publications from 1956 to 1960 was sufficient in itself to suggest an unusually active young scholar, another publication effort was under way. In 1960 the first edition of *Social Statistics* was published. Tad later indicated that, at the time, he was unaware of the disdain that some of his senior colleagues had for textbook writing, especially in statistics rather than sociology. After completing his manuscript, he was surprised to learn that it would not be very beneficial in his tenure decision. Perhaps it is fortunate that he didn't know; otherwise he might not have written this outstanding and influential textbook. In writing this volume, Blalock drew on his own training in mathematics and statistics, but he wrote for social scientists interested in applying statistical techniques rather than for mathematicians interested in the formal theory of statistics. It was also evident that Blalock drew on his experience as a teacher of statistics, and his book was designed to clarify the fundamentals applied in social science research for students lacking an extensive mathematical background. The book was authoritative without being esoteric, and it was student oriented without being oversimplified. Adoptions for classroom use were soon sufficiently numerous to make the book a commercial as well as a pedagogical success.

In the fall of 1961 Blalock accepted an offer to become an associate professor at Yale. His stay there was brief (three years), but it was during this period that he produced a series of publications on statistical procedures relevant to causal inferences. His book titled *Causal Inferences in Nonexperimental Research,* published in 1964, included an examination of prior philosophical discussions of cause and effect, but its primary focus was the exploration of strategies for making reasonable inferences about causal processes from a combination of a priori assumptions and statistical outcomes. He also published a number of papers on the same general topic. Between 1961 and 1964 inclusive, he published twelve papers in scholarly journals, primarily *The American Journal of Sociology* and *The American Sociological Review.* His papers on causal inferences built on the foundation laid by Sewell Wright in the development of path analysis four decades earlier. He also built on structural equation models developed by econometricians since the 1930s. Blalock's papers were widely read and were among the most influential papers in sociology during the decade. His name was thereafter closely associated with causal models in the thinking of sociologists, and his reputation as a sociologist and statistical methodologist spread throughout the United States and abroad.

Along with his growing reputation as a sociologist, Tad Blalock's family was also growing at Yale. His son, James Welsh, was born in 1963, joining his sisters Susan and Kathleen.

Responding to an offer of a full professorship, Blalock moved in 1964 to the University of North Carolina, where he remained until 1971. His years as a faculty member at North Carolina were highly productive. At North Carolina he produced three books and coedited a fourth with his wife, Ann. Two of these books were especially influential. *Toward a Theory of Minority Group Relations,* published in 1967, was a continuation of the work undertaken in his Ph.D. dissertation. Citations to this work continue after twenty-five years. In 1969 he published *Theory Construction: From Verbal to Mathematical Formulations,* in which he described a mode of theory construction intended to help bridge the gap between traditional sociological theory and empirical research. This short book is appropriately seen as an extension of his earlier work on causal inferences.

The papers produced during Tad Blalock's period as a North Carolina faculty member were even more influential than his books of that period. While at North Carolina, he published twenty-one papers on a variety of substantive and methodological topics. His most influential papers of this period were concentrated in two areas. First, he presented a set of papers on methodological problems entailed in testing theories of status inconsistency. The theme in these papers was the intractable nature of certain formulations of the theory because the set of equations associated with those formulations was underidentified. The importance of these papers lay, not simply in their relevance for status inconsistency theory, as then formulated, but in making the more general point that verbal formulations of theoretical ideas frequently make it difficult to recognize logical flaws, harking back to one of the points in *Theory Construction: From Verbal to Mathematical Formulations.*

The second set of influential papers published during Tad's period as a faculty member at North Carolina pertained to conceptualization and measurement in social research. In these papers the pervasive feature is representation of the relationship between concepts and their empirical indicators in the form of a causal model. This representation allowed him to explore measurement error and its implications for multiple regression, path analysis, and structural equation models. This was to be one of the continuing themes in his work for the remainder of his life.

Tad's publications brought him increasing recognition, and this was reflected in his invitation to serve on editorial boards or as an associate editor for several journals. He had earlier (1962–64) served as an associate editor of *The American Sociological Review,* and in the late 1960s he was invited to serve in a similar role for *The American Journal of Sociology, Social Problems,* and *Sociological Methodology.* He was elected to serve on the Council of the American Statistical Association in 1970 and on the Council of the American Sociological Association in 1971.

Although Tad had a highly congenial set of departmental colleagues at the University of North Carolina, he felt that the university was not in step with the spirit of the times. Influenced by the civil rights movement and the antiwar movement, there was a spirit of moral change in the country at the end of the 1960s. Tad had long been an advocate for civil rights, and he was pleased to see that American universities were in the vanguard of change. On the other hand, the University of North Carolina seemed to him to be unduly influenced, by a faculty and administration that were intent on preserving discredited traditions. Among other things, Tad believed the university was failing to recruit black students with genuine vigor. In an unrelated matter pertaining to a young faculty member, when the most conservative elements in the university took actions that Tad considered unwarranted and unfair, it was the proverbial straw that broke the camel's back. Tad decided to seek a suitable position at another university.

After careful consideration of several options, Tad accepted an offer to join the University of Washington faculty in the fall of 1971. There he continued to play a vital role in the training of graduate students, and he was the recipient of numerous honors.

Tad's many publications and other achievements during his Washington years can probably best be summarized by considering them in two sets: those prior to 1980 and those that came in 1980 or later. In the earlier of these periods (1971–79), Tad was the author or coauthor of three books and the editor or coeditor of three additional volumes. In these books he further developed his work on familiar topics, notably quantitative research methodology and race relations. The book of this period that was most influential was probably an edited volume published in 1971 with the title *Causal Models in the Social Sciences*. In this collection Tad brought together papers by several authors, including some papers published for the first time. This collection served as a major resource for the further development and application of causal models in social science research.

In 1973 Tad received the Stouffer Award, presented by the American Sociological Association in recognition of his outstanding contributions to sociological research and re search methodology. He was made a fellow of the American Statistical Association in 1974 and a fellow of the American Academy of Arts and Sciences in 1975. He was elected to the National Academy of Sciences in 1976 and served as president of the American Sociological Association in 1978– 79. Such exalted recognition must have made an impression on Tad, but the only change in his behavior evident to colleagues and students was an increase in his energy. It was almost as if he were intent on convincing everyone around him that he was not unduly impressed by his own success and that he wasn't going to rest on his laurels.

As president of the American Sociological Association, Tad was persistent in his attempts to improve the discipline. In his presidential report to the membership (*Footnotes,* August 1979), he urged the association to give continuing attention to several important matters, three of which represented well his own long-term personal and professional commitments: improving the training of sociologists, upgrading the quality of undergraduate teaching in sociology, and enhancing the standing of sociological research as a basis for social action and public policy decisions.

Undergraduate teaching and the improved training of sociologists were matters of persistent concern to Tad. He was a superb exemplar of a committed teacher, and he was an active participant in the programs of the association devoted to improving undergraduate education. Scores of graduate students— many from disciplines other than sociology— considered Tad's courses one of the highlights of their graduate training. Tad was always emphasizing for his colleagues the need to upgrade graduate training and to upgrade skills in postdoctoral training programs. He subsequently published papers on quality training for sociology graduate students.

Tad's interest in enhancing the standing of sociological research as a basis for social action and public policy decisions was central to his own conception of sociology. He was committed to developing the kind of sociology that would be useful in application while also meeting the most exacting standards of methodological rigor and theoretical sophistication. To Tad's chagrin, many of his fellow sociologists saw these objectives as mutually incompatible. They commonly expected an applied sociologist to have limited interest in theory development and little commitment to improving research methodology. Furthermore, many expected a sociological theorist to be disdainful of applications and indifferent to empirical studies. And a widely held stereotype portrayed the empirical researcher in sociology as contemptuous of theory and lacking in concern for applied or policy concerns. But for Tad these three elements—general theory, sound empirical research, and policy relevance—constituted an integrated whole toward which the discipline should always strive. His interest in improving research methodology and his concern for the development of sound theory were primarily means to make the field more relevant to action and policy problems. The social conscience that Tad developed as a very young man and that was nurtured by the workshops of the American Friends Service Committee and his explorations in the field of race relations was still with him as president of the American Sociological Association.

Eight of the ten papers Tad published between 1971 and 1979 inclusive were devoted to issues pertaining to social science conceptualization and measurement. One of these was the published version of his 1979 presidential address to the American Sociological Association. His title was "Measurement and Conceptualization Problems: The Major Obstacle to Integrating Theory and Research." It was evident that Tad believed that conceptualization and measurement problems were serious impediments to the continuing development of sociology, and he was not optimistic about the prospects for much progress in resolving such problems in his lifetime. The pessimistic tone and the high level of abstraction of his presidential address did not make it a crowd pleaser. But pleasing the crowd and oversimplifying complex issues had never been high in Tad's priorities, and he considered it important to highlight for his sociological colleagues some difficult problems that needed resolution.

By 1980 Tad had accumulated an impressive record of achievements. He was a sociologist with an international reputation; he had been elected to membership in the nation's most prestigious scientific organization; and he had just completed a term as president of the national organization for members of his discipline. But at fifty-four he was far too energetic and vigorous to be satisfied simply being an elder statesman. He continued working.

In 1982 he was selected to present the Annual Faculty Lecture at the University of Washington. In 1983–84 he served as vice-chair of the University of Washington Faculty Senate, and in 1984–85 he served as the chair of that body. This office brought with it a host of committee and administrative responsibilities, including ex officio membership on the Board of Regents. He immersed himself in these activities, and his penetrating questions did not always endear him to the university administration. But his straightforward style, his questioning attitude, his strong commitment to fairness, and his fervent defense of scholarly values gave him an enthusiastic following among the faculty.

Even as he was heavily engaged in the activities of the University of Washington Faculty Senate, his commitment to teaching did not falter and his scholarly productivity did not decline. In the early 1980s he authored three books, edited a volume of selected papers from the 1979 meeting of the American Sociological Association, and coedited a collection of works on teaching sociology. He was also the author or coauthor of eight papers in the first half of the decade.

Few people knew in 1984—and no one who didn't know would have guessed—that Tad had a serious health problem. During a routine hernia operation, he was found to be suffering from a rare form of abdominal cancer. He was told that there was no cure. He was also told that the cancer was relatively slow growing and that the major treatment would be periodic surgery.

Tad undoubtedly understood all that the specialists told him about his condition. He probably believed them. But it was almost as if his cancer and the threat it posed to his life never seemed real enough to him to be worthy of discussion. Even with family members he declined to discuss his disease and his altered life expectancy. It was not a topic he broached with colleagues; he could always find more interesting and more productive things to talk about.

Even so, beginning about 1987, an examination of Tad's work suggests that he had made a subtle change in his scholarly agenda, in recognition of his deteriorating health. After that date, his papers appeared primarily in edited collections, as if he were fulfilling commitments to a few colleagues to prepare a paper for their special volumes. The two major works that he completed in the few years remaining before his death reach for a new level of generality. A longtime student of race relations, Tad had, of course, also been a student of social conflict and the exercise of power as exemplified in race relations. In *Power and Conflict Processes: Toward a General Theory,* Tad no longer focuses specifically on race relations; rather, he examines power and conflict more abstractly, considering the relevant processes in all contexts, including, but not limited to, the context of race relations. Similarly, in *Understanding Social Inequality: Modelling Allocation Processes,* Tad goes beyond the specific features of inequality entailed in the stratification of racial groupings to explore general processes that create and sustain social inequality. These two books are a fitting capstone for Tad's long series of publications. They embody his persistent conviction that sociology must develop systematic and general theoretical formulations with clear links to the empirical world. They illustrate his belief that common explanations for social phenomena are overly simplistic and hence lack the capacity to advance understanding. They address pressing public policy problems in ways that are intended to provide guidelines for potential social change. And his argument is presented in the form of carefully formulated causal models, a form that became prominent in sociology largely through his work.

Even as he continued to work, medical treatments periodically interrupted Tad's schedule. The treatments were risky and painful, and each required weeks of recovery. Between treatments, abdominal blockages created pain, increasingly severe dietary restrictions, and continuing weight loss. Long before his retirement in 1989, Tad's deteriorating health was evident to all who saw him.

In the spring of 1989 Tad retired from active faculty status to become professor emeritus. During that spring the Department of Sociology at the University of Washington sponsored a lecture series in Tad's honor. Eight distinguished scholars whose work was related in some way to Tad's were brought to Seattle to present public lectures on their current work and recent findings. As the 1988–89 academic year drew to a close, the Department of Sociology celebrated Tad's career with a retirement dinner, complete with reminiscing speakers and testimonial toasts. Tad was on such a heavily restricted diet that he could not enjoy the feast, but he evidently enjoyed the event.

Tad's brief period as professor emeritus was a period of continuing physical decline despite his tenacious will to live and a determination to continue his work. While recovering from his final surgery, he read proofs for his last book. He was notified by telephone that he was the 1991 recipient of the American Sociological Association's Lazarsfeld Award four days before he died on February 8, 1991. The Persian Gulf War was under way, and Tad spent his final days analyzing recent developments in the Middle East in light of the general principles he had discussed in *Power and Conflict Processes: Toward a General Theory.*

To the end of his life, Tad remained a person of great inner strength, sustained by the remarkably warm and close relationship that he and Ann maintained for nearly forty years. To many he was an inspiring figure of great personal warmth. In the words of the Lazarsfeld Award citation, "… To colleagues, friends, and scores of former students, he was known simply—and very affectionately—as 'Tad,' and his image as an internationally renowned sociologist is inextricably mixed with his image as a kind and generous human being who has enriched the lives of many" (*Footnotes,* April 1991).

I AM INDEBTED TO Ann Blalock for providing much information about Tad's life that would otherwise have been inaccessible to me. Her assistance has greatly enriched this memoir and eased the task of writing it. Tad's own partial biographical sketch, titled "Socialization to Sociology by Culture Shock," was a helpful resource more often than one might infer from the explicit citations to it.

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MIN CHUEH CHANG

October 10, 1908–June 5, 1991

BY ROY O.GREEP

HE LIFE WORK OF Min Chueh Chang centered on a discrete portion of the mammalian reproduction process, the part that begins with the existence of male and female free living gametes and ends with their successful union, fertilization. In this sphere Chang was a world leader, a giant of his time. That, however, is only part of the fame that is conjured up by the mention of his name. Actually, Chang is best known in the public mind for his work on the development of the oral contraceptive, "the Pill." The latter benefited millions of women and fomented a social/ sexual revolution. This freeing of the sexual act from the threat of conception led to major changes in the way men and women live together.

To bring these two related but very different aspects of Chang's research into perspective, it is important to note that of his forty-five years in research only five (1951 through 1956) were spent in proving the effectiveness of certain steroids in controlling fertility in laboratory mammals when administered orally. This was his greatest contribution in pragmatic terms.

This brief departure from Chang's abiding interest in eggs and sperm themselves is in keeping with his recognition of the critical need for better means of controlling human fertility. It is noteworthy that Chang's work on developing the Pill utilized fundamental information already available. Except for the mode of administering the contraceptive steroids, little was added to existing knowledge. On the contrary, Chang's monumental work on fertilization was purely an exercise in basic science for the purpose of gaining new insight into the mechanism of fertilization. That this turned out to have great practical significance was, of course, a personal satisfaction, but it was the plaudits of the scientific community that pleased him most.

His life career is a story of triumph and disappointments, perseverance and major accomplishments, accolades, international recognition, and, lastly, an element of what Peter Medawar recognized as chance. It was largely by chance that Chang often found himself the right man at the right place at the right time. This was especially true at Cambridge University (1939–45) and again at the Worcester Foundation for Experimental Biology (1951–56). Chang was aware of these favoring circumstances. In an unpublished manuscript titled "Reminiscences on the Study of Animal Reproduction and Association with Reproductive Biologists," Chang wrote extensively, forthrightly, and illuminatingly on the many preceptors and counselors to whom he was greatly indebted for their material help, guidance, and encouragement.

Lastly, note need be made of the fact that Chang grew up, as it were, with a newly founded institution that provided him with the opportunity and the facilities to carry out his extended program of research and to attain preeminence in the world of science. Chang returned this favor by leaving to the Worcester Foundation for Experimental Biology a proud legacy of prestige and renown.

Not the least of what made Chang a notable and endearing character is that he was every inch a kind, generous, fair-minded, and gentle person whose integrity was a given.

PERSONAL HISTORY

Min Chueh Chang was born in Tai Yuan (Shanxi province), China, on October 10, 1908. His father, a magistrate, was able to provide him with a quality education, including in 1933, a bachelor's degree in animal psychology from Tsing Hua University in Peking. Over the next few years of turbulent times in China, Chang stayed at the university as a teacher and made some original observations on the staining of nerve cells that gained publication in a prestigious American journal.

Chang's brilliance of mind and unbounded curiosity did not go unnoticed. In 1938 he was encouraged to compete in a national examination for a few much-prized fellowships to study abroad and he won. He opted for a year of study in agricultural science at Edinburgh University. At year's end the chilly climate and his perception of some bias against foreigners were not to his liking. An appealing invitation from Arthur Walton to join him in research on ram spermatozoa at Cambridge University was gladly accepted. This was in keeping with Chang's newfound interest in reproductive biology, a departure from his initial intent on a career in behavioral psychology. There under Walton's tutelage and association with such other greats as Sir John Hammond and F.H.A.Marshall, Chang became engrossed in research. On the basis of his multiple observations on the effect of testicular cooling and various hormonal treatments on the respiration, metabolism, and survival of sperm in rabbits and some farm animals, Chang was awarded a Ph.D. degree in animal breeding by Cambridge University in 1961.

The options available to Chang at that time included returning to China and sharing in the suffering of his parents and friends. Fortunately, counselors at Cambridge pre vailed upon him to remain there. With the exigencies imposed by World War II, the best that could be provided was maintenance support and limited opportunities for research.

At war's end Chang again was torn between returning to China or finding elsewhere an outlet for his study of fertility. He sought and was granted a one-year fellowship with Gregory Pincus to learn the technique of in vitro fertilization before returning home. At the time of Chang's arrival in the United States, Pincus was at Clark University with Hudson Hoagland, and the two of them were in the process of founding the Worcester Foundation for Experimental Biology in Shrewsbury, Massachusetts, just outside Worcester. Chang was given a room at the newly created foundation, and he often told with some delight how he served as night watchman. It was soon evident to Pincus and Chang that they were an effective team with many common interests in the broad field of reproductive biology. Moreover, they almost immediately formed a warm, personal, and enduring relationship. It was there that Chang would spend the remainder of his illustrious and rewarding career in research on matters relating to mammalian fertility.

As funds for support of research on reproduction became increasingly available after mid-century, Chang's laboratory began to attract a cadre of highly competent young investigators who today are distinguished leaders in basic and clinical research on reproduction. In Chang's laboratory they were mainly left to their own devices except that Chang was always at hand for helpful guidance and advice when needed. Among the group of approximately 100 fellows and associates, singling out any for mention risks the sin of omission, but mention of a few will illustrate the distinction of the group as a whole: J.M.Bedford, C.R. Austin, R.Yanagimachi, M.R.J.Harper, Y.Toyodo, R.H.F. Hunter, J.H.Marston, T.Iwamatsu, and H.Miyamoto. With this concentration of expertise in Chang's special field, his laboratory became an international crossroad. An unending influx of distinguished visitors was a significant factor in establishing the Worcester Foundation for Experimental Biology as an important biomedical research center.

Chang's work habits were incredible. He personified what is dubbed a workaholic. His quest for better understanding of the intricate series of sequential physiological mechanisms involved in the fertilization of mammalian ova was the dominant and consuming factor in his life. Some measure of the intensity of his labors will be evident from the fact that at the peak of his productivity he was publishing up to nineteen papers annually—all in first-rate, peer-reviewed journals and all reporting substantive findings. Chang was by his own admission a patient and persevering type of investigator. He had long-range goals toward which he planned his experimentation assiduously. The strong likelihood of gaining substantive new information from each carefully designed experiment was a contributing factor to his prolific productivity. Chang's bibliography lists 347 papers, of which he was sole author of 112 and senior author of another 38. Most scientists will agree that such prodigious effort comes at the expense of time with the family, cultural pursuits, and reflections on broader issues within and outside science.

Shortly after Chang arrived in the United States he married an American-born Chinese woman, Isabelle Chin, whom he met by chance in the Yale University library. Their three children include two daughters, Claudia Chang Tourtellotte, head of the anthropology department at Sweet Briar College in Sweet Briar, Virginia; Pamela O'Malley Chang, an architect and civil engineer in San Francisco, California; and a son, Francis Hugh Chang, director of a health center in Boston, Massachusetts.

Chang was neither a family man in the usual sense nor a doting father. In his private life he was a Confucian scholar and held to the principles of strict discipline for himself and his son and male dominance of the marital relationship. Much credit must be given to Chang's talented wife for her willing acquiescence in the role of a Confucian wife as her part in enabling Chang to develop his full potential unhindered by domestic concerns. On Chang's behalf it can be said that he followed the cultural traditions of his Asian background in a Western setting yet retained the profound respect of his family.

In his later years Chang traveled extensively to many parts of the world to participate in meetings devoted to his special field of investigation. Such attendance was almost always as an invited speaker. His distinguished accomplishments were otherwise recognized by numerous honors and awards. A partial list includes the Albert Lasker Award (1954), Ortho Medal and Award by the American Fertility Society (1961), Hartman Award by the Society for the Study of Fertility (1971), Frances Amory Prize by the American Academy of Arts and Sciences (1975), Wippman Scientific Research Award by the Planned Parenthood Federation of America (1987), and election to membership in the National Academy of Sciences in 1990.

PROFESSIONAL HISTORY

A detailed account of Chang's experimental work as depicted in nearly 350 publications is far beyond accommodation here. A look at some of his major accomplishments must suffice. Chang's life work involved a series of highly interrelated projects. The first dealt with the metabolism, motility, and fertilizing capacity of ram sperm. This was closely tied to a concurrent attempt to improve the effectiveness of artificial insemination in farm animals. It being wartime this had the prospect of increasing food production. Once the war ended and Chang had moved to the United States, he was able to take up a quest that he had had in mind for some time—namely, fertilization of ova outside the mammalian body (in vitro fertilization). To that end he sought first to understand why sperm from the epididymis or ejaculate were motile but incapable of penetrating ova.

Chang's competence in reproductive biology was occasioned by having to understand, and to manipulate, the reproductive status of the host animals from which he obtained male and female gametes. It was with this background that he was eminently qualified to meet the challenge of evaluating, on a virtually emergency basis, a wide range of steroidal compounds as potential orally active antifertility agents in the early 1950s.

In his initial studies on eggs and sperm Chang carried out a variety of experiments mainly to acquire expertise in the techniques involved and to gain a thorough knowledge of the field. He examined the motility and fertilizing capacity of sperm taken from different areas of the male reproductive tract, with special attention to sperm from various parts of the epididymis. Out of this came the finding that cooling by simply applying ice to the scrotum caused severe disintegration of sperm from the lower end of the epididymis. This is now a generally recognized phenomenon known as cold shock. It occurs in a critical range of temperatures (13°–0°C) and results in a breakdown of membrane structure and function. Chang showed that sperm subjected to deep freezing must be protected by a cryoprotective agent found in egg yolk. Chang's original observation on cooling led to a massive study of cold shock. Obversely, Chang found that exposure of unfertilized rabbit ova to elevated temperatures destroyed their fertilizability.

Early in his career Chang was intrigued by the prodigality of sperm production and made several observations on the effect of the number of sperm on fertilization of ova. He once estimated that the human male produces about 1 billion sperm for every egg released by the female gonads. He found that of approximately 200 million sperm deposited in the rabbit vagina by ejaculation or artificial insemination barely 1 percent make it past the cervical barrier to the uterine cavity and only about 5,000 find their way past the utero-tubal junction. Fewer still reach the site of fertilization in the outer segment of the oviduct.

Earlier literature claimed that fertilization required the presence of what were termed swarms of sperm in contra-distinction to vanguards. The belief was that large numbers of sperm were necessary to release a lytic agent that would dispel the follicular cells surrounding the oocyte, the cumulus oophorus, and corona radiata. Chang showed that it is the physiological integrity of an individual sperm that is important for fertilization. He also found that a single sperm can penetrate the cumulus mass of cells and reach the zona pellucida, a thick mucoprotein membrane enveloping the ovum. Each sperm head carries an attached packet, the acrosome, containing hyaluronidase, which is released by the acrosome at the site of fertilization and was believed to effect the dispersal of cumulus cells. Chang found that the number of sperm at the site was far too few to accomplish this event. Adding hyaluronidase to sperm suspensions did not prove to be beneficial. Chang also disproved an alleged claim that phosphorylated hesperidin, a hyaluronidase inhibitor, had an antifertility action when administered orally.

Since large numbers of sperm are of no benefit to fertilization, their production in astronomical numbers throughout reproductive life posed a challenge to Chang. He posited that every population of sperm is comprised of some that are strong and others weak, morphologically defective, or aged. Since only the strongest of the strong reach the site of fertilization, Chang held that the more sperm entering the female reproductive tract the more this would provide for greater variation in the recombination of genes.

Chang also did a large amount of work on the local milieu of sperm. He wanted to understand the effect of the very different environmental factors to which sperm are exposed during their passage through the epididymis and ascent of the female reproductive tract. The fertilizing capacity of rabbit epididymal sperm was not benefited by suspension in rabbit seminal plasma as compared to Ringer's or Tyrode's solution. On the matter of osmolality, Chang and Thorsteinsson found that rabbit sperm could tolerate without ill effect on either motility or fertility half the strength or twice the strength of Ringer's solution at neutral pH. They also found that sperm could survive a wide range of pH at isotonicity—namely, from 5.57 to 10.94! That is fortuitous since sperm often encounter a wide range of pH in the human vagina.

Sperm deposited in the rabbit vagina on mating reach the fallopian tubes within minutes and await the arrival of ova for fertilization ten to twelve hours later. In a fateful experiment Chang deposited ejaculated sperm in the tubes to coincide with the arrival of ova. Fertilization failed. Testing his speculation that the waiting period was the crucial factor, Chang next deposited sperm in the tubes six to eight hours before the arrival of ova and obtained fertilization. This finding that sperm must undergo an incubation period in the female reproductive tract before they acquire fertilizing capacity was independently reported in 1951 by Chang and his close friend and arch rival, C.A.Austin of Australia. For both investigators this was at once a blessing by virtue of immediate confirmation and inescapably some sense of disappointment. A year later Austin named this phenomenon sperm capacitation—a term now in wide use by reproductive biologists and clinicians.

Chang and his associates proceeded to show that capacitation was a general phenomenon occurring among all mammalian species studied. They found that the duration of the waiting period varied somewhat among species: rabbit, five to six hours; rat, four to five hours; mouse, one hour; golden hamster, two to three hours; and sheep, one and one-half hours. They also found that sperm could acquire capacitation in the uterus as well as the tubes. An exciting extension of capacitation came with the discovery by Chang in 1957 that capacitated sperm exposed to either seminal plasma or blood serum from the same species or from other species lost their capacitation, an event termed decapacitation. This factor was found by Bedford and Chang to be a high-molecular-weight substance that adheres to the surface of sperm and is removable by centrifugation at 105,000 times g. Taking this one step further, they found that decapacitated sperm could be recapacitated by placing them back in the uterus or tubes.

In a 1958 study of the possible influence of the hormonal status of the female rabbit reproductive tract on the capacitation of sperm, Chang found that ejaculated sperm placed in the uteri of pseudopregnant or progesterone-treated rabbits failed to become capacitated. In striking contrast, sperm placed in the fallopian tubes of these rabbits did become capacitated. Capacitation was also achieved in the uteri of immature or ovariectomized rabbits with or without estrogen treatment.

Chang's mastery of capacitation did not prove to be the Holy Grail. He could not have been unaware that by the discovery of capacitation he was one step closer to the achievement of in vitro fertilization. Back in 1945 Chang's main purpose in coming to the Worcester Foundation was to learn the technique of in vitro fertilization from Gregory Pincus. As early as 1935 Pincus claimed to have obtained living young from rabbit eggs fertilized in vitro and returned to the doe. Doubts as to the authenticity of this report lingered, and Chang working in Pincus's laboratory was not able to repeat those findings. This opened an intense and competitive search for a solution to this important problem. In 1954 Thibault and associates reported early embryonic development in eggs fertilized in vitro. Chang's crowning achievement came in 1959 with his demonstration that eggs from a black rabbit fertilized in vitro by capacitated sperm from a black male and transferred to a white female resulted in the birth of a litter of black young. This evidence seemed beyond question, and was, but some skepticism persisted for a while.

The circumstances that allowed in vitro fertilization in rabbits proved to be species specific. For many years, Chang and his students continued to define the varying specific conditions required for in vitro fertilization in several species. To wit, in 1963, Yanagimachi and Chang reported the first successful fertilization of golden hamster eggs in vitro. They used Tyrode's solution, containing glycine and sperm capacitated in the uterus. Next came the in vitro fertilization of mouse ova incubated in bovine follicular fluid, as reported by Iwamatsu and Chang in 1969, and in the same year Pickworth and Chang succeeded in fertilizing Chinese hamster eggs in vitro. In 1973–74 Miyamoto and Chang and Toyoda and Chang reported fertilization of rat eggs in vitro, and in 1978 Hanoda et al. accomplished the same for deermouse eggs.

In an extension of this study of in vitro fertilization, Yanagimachi and Chang (1963) found penetration of rabbit ova by sperm taken from the epididymis, thus showing that capacitation had occurred in vitro. In 1973 Miyamoto and Chang observed fertilization of mouse eggs by in vitro capacitated sperm, and a year later Toyodo and Chang made similar observations on rat sperm capacitated in a chemically defined medium.

It was on the basis of this animal data that Steptoe, Bravister, and Edwards were able to achieve in vitro fertilization of human ova. Later, the landmark birth on July 25, 1978, of the world's first test-tube baby, Louise Joy Brown, in Oldham, England, was recorded by Steptoe and Edwards.

From this overview of the entire spectrum of Chang's investigational program, it is evident that the central and constant objective was understanding the detailed circumstances involved in the process of sperm penetration and fertilization of mammalian ova. Such was the fabric of his illustrious career.

In the twilight of his career Chang became disturbed by the confusion and controversy appearing in the literature as a result of disagreement as to what changes are to be considered part of the capacitation process. Capacitation as originally defined both by Austin and Chang in 1951 included all the changes that enable sperm to penetrate eggs. Over the years a variety of structural and chemical changes in the sperm during the capacitation were described, some being considered as components of the capacitation process, others not. The bulk of the controversy centered on whether the acrosome reaction was a separate feature or a part of capacitation. Among those holding that capacitation was simply a preliminary that enabled the acrosome reaction to occur were several of Chang's former colleagues, most notably Bedford, Austin, and Yanagimachi. In a review article on capacitation published in 1984, Chang argued forcefully at some length as to why the original definition, which includes all the changes that enable a sperm to pen etrate the egg, should be retained. He dealt strictly with the facts on a totally impersonal basis. It was an elegant appeal to await full understanding of what happens to sperm during that still ever so puzzling waiting period.

PERSONAL COMMENTS BY THE AUTHOR

Chang was an easily recognized figure, tall and slimly built with a copious head of dark hair tinged with gray. His twinkling eyes and ready smile were prominent features of his friendly greetings. Despite being a man of illustrious international stature, the impression he gave was that of a genuinely modest and somewhat humble man. He always seemed to look up to whomever he met on a casual basis. Albeit meeting Chang was always a welcome and delightful experience. Despite this self-effacement, Chang had a normal healthy ego and took justifiable pride in his own accomplishments.

One of Chang's most endearing attributes was his wonderful sense of humor. Before an audience his quick wit and facile repartee often had his audience in stitches. This was aided by an unusual feature in his manner of speaking, a rapid motion of the lower jaw. This made it appear that his often pithy and sometimes pungent quips were being ushered out with gnashing of teeth.

Chang was sensitive to any personal slight or any oversight of his scientific work. The aftermath of the discovery of the oral contraceptive was particularly nettlesome. As I have indicated elsewhere (*Journal of Andrology,* Nov.-Dec. 1992), "Chang sometimes figured in the series of clinical reports on these field trials but not to the extent that seemed deserving. Certainly there was no intent to downplay his role in this remarkable development and with the passage of time Chang came into his rightful dues as a co-founder of the Pill. It is much to Chang's credit that he never wa vered in his admiration and respect for his benefactor, Gregory Pincus."

In a final review summarizing his extensive studies, Chang noted rather pointedly and with some understatement that, "We have achieved a good deal towards the understanding of mammalian fertilization by simple biological experimentation." In this age of high technology and molecular probing, this may give encouragement to some who would aspire to extend the frontiers of knowledge by other means.

Chang was by any measure an extraordinary person. His mastery of the phenomenon of capacitation took six years of unrelenting effort. His demonstration of in vitro fertilization in a mammal took fourteen years. Such tenacity has become a rarity in biomedical research due to the tenuousness of financial support. Chang's rise to prominence as a Chinese scholar during a period of political unrest and social upheaval at home was also out of the ordinary by a wide, nay very wide, margin. Similarly, his contributions to human welfare are on a scale matched only by the few whose names are legend.

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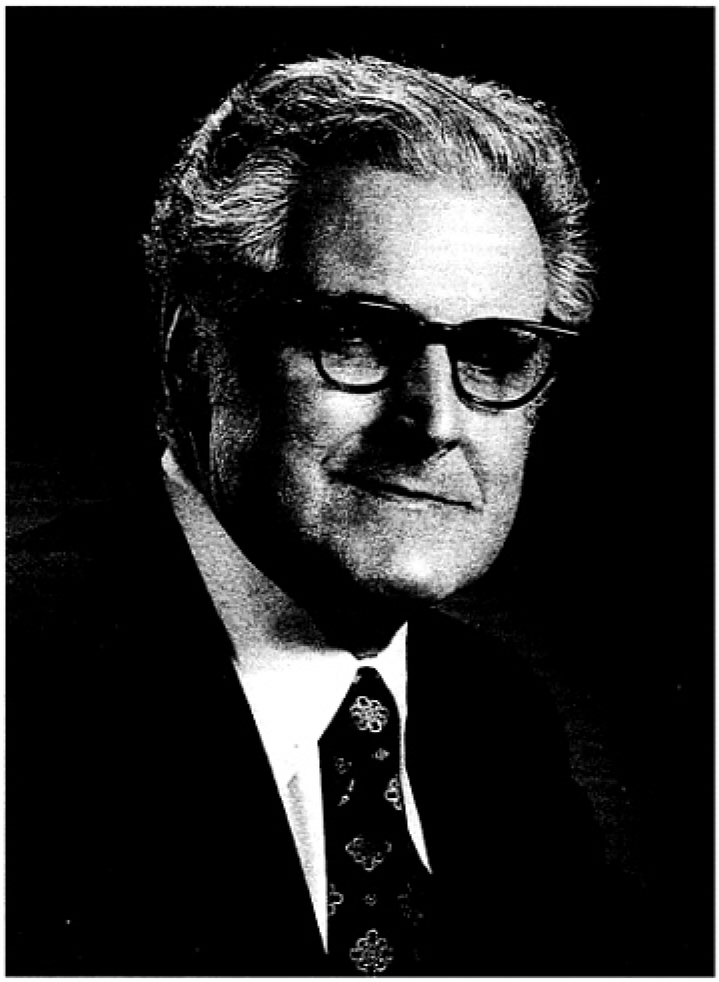

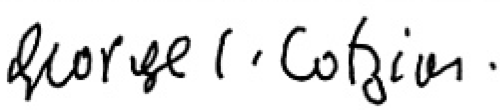

GEORGE CONSTANTIN COTZIAS

June 16, 1918–June 13, 1977

BY VINCENT P.DOLE

EORGE CONSTANTIN COTZIAS was born in the city of Chania on the island of Crete, the eldest child of Constantin and Catherine Stroumboli Cotzias and the grandson of George J.Cotzias, a wealthy merchant in Athens. His father, Constantin George Cotzias, a lawyer and journalist, had recently been banished from Athens because of political activity in support of the king. He was arrested by the fascist government of Eleftherios Venizelos and exiled to Crete to limit his political influence, but he was able to return to Athens eighteen months later. Soon after returning he established an influential newspaper and an advertising agency. Twelve years later, in 1932, he became president of the Greek chamber of commerce and in 1934 was elected mayor of Athens.

As mayor and later in the expanded role of governmental minister for all municipal activities in the region of Greater Athens, Constantin Cotzias shaped the political structure of the modern city of Athens. He reorganized its government, initiated programs of health and public works, rebuilt the municipal hospital, paved roads, created parks, supported young artists, and established a new municipal symphony orchestra.

This productive period was terminated abruptly by the invasion of Greece in 1940. After an intense but brief resistance, the Greek armies were defeated. The prime minister, Alexandros Koryzis, refusing to acknowledge defeat and collaborate, committed suicide. In this extremity, King George of Greece asked Constantin Cotzias to go to the United States as ambassador-at-large representing the Greek government in exile.

George and his family arrived in New York in August 1941, financially destitute after a desperate four-month journey through warring countries. They remained in New York until the defeat of the Axis powers in 1945 made it possible for his parents to return. To complete this account, Constantin Cotzias was reelected mayor of Athens by an overwhelming majority in the first postwar election (1951), but he died shortly afterwards of a heart attack at the age of fifty-nine while resuming his municipal duties. Cotzias Square, next to the town hall of the city of Athens, bears his name.

As the eldest son of a leading citizen of Athens prior to these events, George Cotzias had a privileged early life, attending the best schools and associating with the most stimulating intellectuals of the city. Reflecting George's mother's interest in literature, the Cotzias home was a meeting place for leading writers of prewar Athens and, of course, as the mayor's residence, it was at the epicenter of public policy.

At the age of twenty-two, within one year of graduating from medical school, George apparently was on his way to an uneventful career as a medical practitioner in Athens, specializing in surgery. He had become an assistant to the professor of surgery, Xenophon Kondiades. However, the invasion of Greece changed all plans. George immediately volunteered for military service, although as a medical stu dent he was exempt from the draft. At the request of Professor Kondiades, heading a surgical team at a hospital close to the Albanian front, he was assigned to this unit, but while he was in transit the hospital was obliterated by bombing, which killed the entire staff. Military resistance collapsed before the advancing German army. George made his way back to Athens, rejoined his family, and left Greece with them.

After arriving in New York in 1942, George applied for admission to medical school to complete his studies. The rejection by Cornell was unequivocal; not only was he found to be deficient in English, he was told that his education in Athens had provided inadequate training in basic biochemistry, pharmacology, and physiology. He would need further premedical training even to be eligible for admission to the first year of medical school. Applications to Columbia, New York University, Johns Hopkins, and Pennsylvania were equally fruitless.

Reminiscing in later years, George recalled critical advice that his father gave him at this low point. “When I ran for the office of councilman, I was defeated. So I ran for mayor and was elected. Choose the leading medical school in the country and apply there.” With this advice George applied to Harvard Medical School and had the good fortune to be interviewed by Soma Weiss, a brilliant professor of medicine and himself a refugee a decade earlier from Nazi oppression. After a long conversation in German, which George spoke fluently, Weiss recommended that he be admitted conditionally to the third year of study at the medical school. Two years later he graduated from Harvard cum laude. He then trained as an intern in pathology at Brigham Hospital, as an intern in medicine at Massachusetts General Hospital, and was a resident in neurology at Massachusetts General Hospital.

This brings the story to the point at which I first knew George Cotzias. During the four years of World War II, it was my good fortune to be assigned to a naval medical research unit based at the hospital of Rockefeller Institute (now Rockefeller University) and to work in the department of D.D.Van Slyke. With the end of the war and demobilization, I left Rockefeller and returned to Massachusetts General Hospital, where five years earlier I had trained in medicine. Shortly afterward to my great surprise and delight, the director of the Rockefeller Hospital, Thomas Rivers, invited me to return to Rockefeller. He asked me to form a new department, replacing that of Van Slyke, who planned to move to Brookhaven National Laboratory the following year.

My first act was to invite Lewis Dahl to join me; Dahl, a scholarly physician and friend, who was completing a tour as senior medical resident at Massachusetts General Hospital, agreed. Within the hour George burst into my office and announced explosively, “I’m coming, too.” This unusual application could have been counterproductive, but fortunately Dahl had worked with Cotzias and gave him an enthusiastic endorsement. Immediately I had two talented associates with whom to start the department, the only difficulty being there was no space for the new laboratory immediately available at Rockefeller. At Rivers’s suggestion, my two associates were assigned to Van Slyke’s group during his final year (an invaluable experience for them), and I started my new career as department head with a year’s sabbatical in Europe.

The five years after my return were busy ones. We studied hypertensive patients, looking for clues to the nature of this disorder in disturbances of salt metabolism and energy balance. George, in addition to loyal work as a team member in the clinical studies, became interested in the me tabolism of amines in the tissues. He reasoned that catecholamines, diamines, and histamine have powerful vasomotor effects that might be relevant to hypertensive disorders. In particular, he directed his attention to the enzymes that limit their biological activity by oxidizing them.

George thus initiated studies that led him some fifteen years later to the demonstration that l-DOPA (the amino acid l-dihydroxy phenylalanine) was effective in giving symptomatic relief to patients with disabling Parkinsonism. The most important of these studies were conducted after he left my department in 1954. Dahl left at the same time, both having outgrown the limited opportunities for advancement at Rockefeller. They transferred their work to Brookhaven National Laboratory, where Van Slyke meanwhile had established a strong medical division and was able to provide space and support for each to develop an active, independent laboratory.

The move to Brookhaven also provided George with a new resource for metabolic studies, namely a cyclotron. When activated by a beam of high-energy neutrons, trace metals in samples of tissue and blood can be determined with unprecedented sensitivity and specificity. George seized the opportunity. In a series of basic studies over the following decade, he elucidated the distribution, absorption, elimination kinetics, and probable function of manganese. At the same time he became interested in its toxicity, manifest especially in the neurological symptoms of Chilean miners excavating manganese ore. As a neurologist he was impressed by the similarity of the symptoms to those of classical Parkinson's disease (rigidity, retardation of motion, tremors, lack of coordination), and as a pathologist he was attracted by the opportunity to correlate specific structural lesions made by a known toxin to disturbances of brain function. In both disorders the main lesions found in the brain involve the substantia nigra, a region made conspicuous by a deposit of dark melanin pigment. It was known that this region of the brain is rich in the neurotransmitter dopamine and also that both melanin and dopamine are derived from common precursors. Further, it was known that this region is deficient in both substances in patients with Parkinson's disease.

A possible remedy, as was evident to several investigators at the time, is to increase the supply of dopamine to the neurones in this region. However, dopamine administered as a medication (orally or by injection) cannot reach the site in significant concentration because it does not pass the blood-brain barrier. As an alternative, one can look for precursors of dopamine that are not excluded by the barrier, administer them in large doses, and hope that when the molecules arrive at the critical site enough will be converted into dopamine to have a therapeutic effect. Other investigators had pursued this idea with little success, although the validity of the approach was shown by the transient benefit seen after injection of the precursor, dihydroxyphenylalanine (DOPA). But this effect was only of theoretical interest. It was not of practical value as a treatment because of the severe toxicity associated with the injection.

Cotzias, at this point, made a critical observation that converted the transient response into a successful, large-scale treatment. By starting with very small doses of DOPA, given orally every two hours under continued observation, and gradually increasing the dose over a period of several weeks as permitted by the development of tolerance, he was able to stabilize patients on large enough doses to cause a dramatic remission of their symptoms. He further improved the treatment by utilizing the active isomer, l-DOPA, recognizing that the inactive isomer, d-DOPA (which constitutes 50 percent of the dose in a racemic mixture) is responsible for 50 percent of the toxicity but contributes nothing to the therapeutic result.

The result was soon confirmed by other investigators and has now become the standard treatment for Parkinsonian symptoms. To his credit, Cotzias realized that this success was only one step toward a definitive treatment. After having a remission, many patients show a disturbing tendency to relapse, even with continued treatment, or to develop movement disorders. He therefore was engaged during the last decade of his life in testing supplementary treatments, especially those utilizing structural analogs of dopamine (like apomorphine) that could penetrate the blood-brain barrier and substitute for dopamine without having to be converted by local enzymes. By a remarkable coincidence, his early interest in the function of bioactive amines in tissues and his subsequent investigations of the toxicity of manganese converged on this problem. Cotzias was at work on the development of new medications when his career was terminated by lung cancer. Like his father, he died at age fifty-nine, before his work was finished.

George was a large man physically and intellectually— restless, fiercely loyal, informed, intuitive, quick in conversation with an infectious laugh that began as a furtive chuckle and grew into a roar. Basically he remained the intense young medical resident who burst into my office in 1946 announcing, “I’m coming, too.” He is survived by his widow, Betty, and a son, Constantin George Cotzias.

Among the honors and awards received by George Cotzias were election to the National Academy of Sciences (1973), election to the American Academy of Arts and Sciences (1970), the A.Cressy Morrison Award in Natural Sciences (1954), the Albert Lasker Award in Clinical Medical Research (1969), the Borden Award of the Association of American Medical Colleges (1972), and the annual award of the

American College of Physicians (1974). He received honorary degrees from Catholic University, Santiago (1969); Women's Medical College of Pennsylvania (1970); St. John's University, New York (1971); and the National University, Athens (1974).

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Photo by Joan Eggan

Fred Eggan

FREDERICK RUSSELL EGGAN

September 12, 1906–May 7, 1991

BY EVON Z.VOGT, JR.

RED EGGAN, WHO DIED IN Santa Fe, New Mexico, on May 7, 1991, in his eighty-fourth year, was universally recognized as one of the great anthropologists of the twentieth century. His pivotal contribution to anthropological science during his long, productive life consisted of a creative synthesis of American historical ethnology with the structural-functional approach of British social anthropology, especially in a series of rigorous, comparative studies of the kinship and social systems of Native Americans in the Southwest and on the Plains. In addition, he made notable contributions to our knowledge of the cultures of tribal groups in the northern Philippines.[1]

Fred Eggan was born in Seattle, Washington, on September 12, 1906, one of two children (the other a younger sister) of Alfred Julius Eggan and Olive M.Smith. His father was born into a large family in Rushford, Minnesota, a small working-class Norwegian-American community. He was a bright, restless boy who loved adventure and travel to faraway places. At the age of fifteen he enlisted in the U.S. Navy for a ten-year stint. After his discharge, Alfred tried a number of unsuccessful business ventures and eventually moved to Illinois, where he joined the U.S. Merchant Ma rine and became a petty officer serving in the engineering department of ships sailing through the Great Lakes.

Fred's mother, Olive M.Smith, of old Yankee stock, was born in Armenia, New York, where her father was a successful middle-class businessman. Olive was a well-disciplined schoolteacher who taught her cherished son to work hard and to love books.

By the time Fred was in the eighth grade, the family had moved three times —from Seattle, to Vancouver, to Rushford, to Lake Forest, a well-to-do suburb of Chicago, where they lived on the wrong side of the tracks. Fred's love of books was further enhanced when, at the age of twelve, he contracted a serious case of typhoid fever and was not permitted to attend school for a year. He promptly discovered the public library, where he happily spent most of the year.

Fred later graduated from the Deerfield Township High School, excelling in mathematics, physics, and chemistry. He enrolled in the University of Chicago in 1923, and in 1924 his parents moved again to an apartment near the university, which they occupied during Fred's undergraduate and graduate years. The family was forced to make many sacrifices to send their two children to college. Both children lived at home until they completed their graduate work, and their mother took in boarders to supplement their income.

After first contemplating a degree in business administration (in which he attended classes with James B.Griffin), young Eggan shifted to psychology as a major. But during his college days he was also exposed to geography courses, which intensified his interest in faraway places and peoples, and he stumbled by chance into an anthropology course on "Peoples and Races" taught by the newly appointed head of the department, Fay-Cooper Cole, who had been trained by Franz Boas at Columbia. As Eggan remembered in retro spect, "Cole was a dynamic and inspiring lecturer whose enthusiasm for the subject was contagious" and this course launched him on lifelong involvement with anthropology (Eggan, 1974, p. 5).

While still an undergraduate, Eggan and his classmate, Cornelius Osgood, were invited by Fay-Cooper Cole to join a graduate seminar on India taught jointly by Cole and Edward Sapir, who had been brought to the university in 1925. The two undergraduates were excited by being allowed to attend the seminar, until the topics were assigned. Eggan reported:

We protested we were neophytes, with only two or three weeks of introductory anthropology, but the faculty decreed it to be a "working seminar." I was given the topic, "The Caste System of India," and disappeared into the stacks for a month where I read all the reports on caste in the census volumes and other tomes. I survived the experience and produced a paper, but I have been happy to leave the caste system to others ever since. (Eggan, 1974, p. 6)

Even though Eggan did a year of graduate work in psychology and wrote a master's thesis in 1928, titled "An Experimental Study of Attitudes Toward Race and Nationality," under the supervision of the eminent psychometrician, L.L.Thorndike, he had already decided he wanted to be an anthropologist. Unfortunately, there was little support for graduate work, especially for a student changing fields, so he took a teaching post for two years at Wentworth Junior College and Military Academy in Lexington, Missouri, where he was assigned courses in psychology, sociology, and history and saved enough money to return to graduate work in anthropology in the summer of 1930.

By this time Fay-Cooper Cole had added Robert Redfield, who had just returned from his field study of Tepoztlan, Mexico, to the staff as an instructor and had moved to establish a separate department of anthropology. While Cole taught physical anthropology and archeology, Sapir covered linguistics and ethnology, with excursions into culture and personality, and Redfield offered courses in folk culture and peasant society. Cole also organized field expeditions to survey and dig archeological sites in Illinois, and Fred Eggan spent several summers excavating Indian mounds and village sites in the Middle West. He later participated in the archeological Awatovi Expedition of Professor J.O. Brew in Hopi country in the summers of 1939 and 1940. These early interests in archeology are reflected in his article, “The Ethnological Cultures and Their Archaeological Backgrounds” (1952), as well as in much of his other work on North American cultures. During this same period, Eggan also took courses at Chicago with visiting professor Leslie Spier, who first sparked his interest in kinship and Southwestern ethnology.

In 1931 there occurred an even more momentous happening in the career of Fred Eggan. Fay-Cooper Cole recruited A.R.Radcliffe-Brown to replace Edward Sapir, who left Chicago to become Sterling Professor of Anthropology and Linguistics at Yale. Eggan attended Radcliffe-Brown’s course on family, kin, and clan and was stimulated by the erudition and fresh theoretical orientation he brought to the department. Radcliffe-Brown vigorously attacked the ethnological work done by American anthropologists and advocated the synchronic study of social structures as functioning wholes. He also contended that a comparison of these structures could provide a set of principles of organization comparable to the principles discovered by biologists for the organization and functioning of organisms. R-B (as he was called by his colleagues) arrived at Chicago with a program for reanalyzing the social structures of the American Indian in the manner he had developed in his research with the Australian aboriginals. Eggan became R-

B's research assistant, with the task of reviewing publications on the American Indian and writing summaries of what was known and what needed to be done (Fogelson, 1979, pp. 163–64).

In the summer of 1932 Eggan was selected for a Laboratory of Anthropology (Santa Fe) fellowship for field training in ethnology, and he joined Edward Kennard (Columbia), Mischa Titiev (Harvard), Jess Spirer (Yale), and Georges Devereaux (France) in a field party that spent the summer among the Hopi under the direction of Leslie White. The experience was formative for Eggan, who subsequently had a lifelong association with the Hopi, during which he revolutionized our understanding of their social organization. Fred was now fully committed to social anthropology and clearly perceived the need for new theory to illuminate Boasian empiricism (Fenton, 1992, p. 434).

The Hopi research led to a Ph.D. dissertation on the social organization of the Western Pueblos (Hopi, Zuni, Acoma, and Laguna), which Eggan completed in 1933, later revising and publishing it (Eggan, 1950). In this landmark study, Eggan made brilliant analyses of each of the Western Pueblo social structures as functioning wholes, then compared the four, and contrasted the Western Pueblos with the Eastern Pueblos (who live along the Rio Grande). He focused especially on the contrast between the "lineage principle" he found in the kinship systems of the Zuni and Hopi with their crucial matrilineal clans and the "principle of dual organization" of the Eastern Pueblos with their "Summer People" and "Winter People," each with their own ceremonial kivas. He demonstrated how the variations currently observed in the Pueblo social structures are related to cultural adaptations to ecological niches (dry-land agriculture in the west versus irrigation agriculture in the east) and in historical experiences—heavy Spanish contact along the Rio

Grande compared to slight Spanish influence in the far western Pueblos of Zuni and Hopi.

In the summer of 1933 Eggan undertook a brief field trip among the Mississippi Choctaw and the Cheyenne and Arapaho in Oklahoma (Eggan, 1937). Armed with these data and supplemented with detailed library study, he discovered that these kinship systems were not immutable but subject to changes due to shifts in ecological settings and historical experiences. He also found that their joking relationship functioned systematically to regulate respect and avoidance relationships among kin. Eggan likewise demonstrated how a tribe like the Cheyenne could change from a lineage-type kinship system nicely adapted to a settled agricultural existence in southwestern Minnesota during the early historic period to a generation-type system when they were pushed onto the Plains by other tribes, became nomadic buffalo hunters with horses and rifles, and needed bands of "brothers" for efficient hunting and fighting on the High Plains (Eggan, 1937).

From this research emerged his classic presidential address to the American Anthropological Association on "Social Anthropology and the Method of Controlled Comparison" (1954), in which he cogently laid out the theoretical and methodological dimensions of a comparative method that has been widely admired and utilized by anthropologists during the past four decades. By "controlled comparison" Eggan meant essentially that the cases for comparative treatment are best selected when they are either (1) a small number of cases that are cultural variations set within a geographical and historical frame (such as the Southwestern Pueblos or the tribes of the American Plains) or (2) are variations on a given type of social structure (such as moiety systems).

Eggan continued to work with this method of controlled comparison during most of his professional career; two of his last publications were a brilliant review titled “Shoshone Kinship Structures and Their Significance for Anthropological Theory” (1980) and a masterful article on the Southwest entitled “Comparative Social Organization” (1983).

The other area of the world in which Fred Eggan engaged in basic field research and scientific publication was the northern Philippines. Although his anthropological data on the Philippines were never so fully analyzed and published as they were on the American Indian cultures of the Southwest and the Plains—mainly because of the interruptions of World War II, the restrictions imposed by the Marcos regime on anthropological research, and the subsequent administrative duties he undertook—Fred collected significant information and published a number of fundamental papers on the tribal cultures of northern Luzon as he further developed his structural-historical concepts (Sahlins, 1992, p. 24).

In 1934 Eggan had hoped to undertake two years of field research in the Kimberly district of Australia on an Australian National Research Council postdoctoral fellowship arranged by Radcliffe-Brown. He had just spent the winter season of 1933–34 doing field research among the Hopi. But when he returned to Chicago in March, President Roosevelt had just devalued the dollar. Since the Australian National Research Council received a large portion of its funds from the Rockefeller Foundation, it was forced to cancel Eggan’s fellowship. At this point, Fay-Cooper Cole came to the rescue with a proposal that Eggan go to the Philippines. Cole had always wanted to send a young anthropologist there to study what had happened to the Tinguian, whom he and his wife had studied in 1907 and 1908. He drafted a proposal and found the funds for Eggan. But Fred was disappointed. In his words: “It was attractive but I would have preferred going to Australia. I had always been studying other people's tribes, and it would have been fun to have a tribe of my own" (Eggan, 1974, p. 12).

Nonetheless, Eggan dutifully went to the Philippines, with a stop in Japan for a month, where he traveled around staying in rural inns and climbing Mt. Fuji with two companions he met on the ship crossing the Pacific. He arrived in Manila in the fall of 1934 and checked in with H.Otley Beyer, the one remaining anthropologist in the Philippines, who took Eggan in charge and outfitted him "in white cotton duck for Manila and brown cotton for the field."

Eggan spent the 1934–35 year in the Abra Province of Luzon, learning some of the language, collecting data on all aspects of Tinguian life, and focusing his research interests on problems of social and cultural change. His principal mentor and informant was "Dumagat, the son of a headman whom Cole had brought to Chicago to help him with setting up exhibits in the Field Museum, and who had then stayed on in America until the onset of the Depression" (Eggan, 1974, p. 15). He later worked farther up the Abra River and traveled to almost all the communities in Abra, including one journey over the Cordillera with a group of Tinguians.

The results of this field research appeared in a number of papers, the most important being "Some Aspects of Culture Change in the Northern Philippines" (1941), in which Eggan reported on the regular series of changes in social, political, economic, and religious institutions he discovered as he traveled from the interior to the coast—from the Ifugao through the Bontok, Tinguian, and Ilocano. To define these changes, he introduced the notion of cultural drift, adapted from Sapir's concept of linguistic drift.

Just as Fred Eggan was getting ready to return to the United States, he received word that he was being offered a position as instructor at the University of Chicago, with his time being divided between the Extension Program and the Department of Anthropology. After serving for five years as instructor, he served as assistant professor (1940–42), associate professor (1942–48), and professor (1948–63). He then became the Harold H.Swift Distinguished Service Professor of Anthropology until he retired in 1974. During this period he served as department chairman twice (1948–52 and 1961–63), in an era when the Department of Anthropology at Chicago was considered first in the nation.

In 1938 Fred Eggan married Dorothy Way, who visited the Hopi Reservation with him frequently and worked with him in doing field research. She became noted for her research on Hopi dreams.[2]

During World War II, Fred Eggan was called to duty as chief of research, Office of Special Services, Philippine Commonwealth Government. Later he became a captain in the army after graduating from the School for Military Government in Charlottesville and was assigned to duty in Chicago as the director of the Civil Affairs Training School for the Far East (1943–45). In 1945 he also served as a Cultural Relations Officer for the Department of State. Following the war, Eggan became the director of the Philippine Study Program at the University of Chicago, a post he held until his retirement.

Eggan finally managed to return to additional field research in the Philippines when he was appointed as a Fulbright Research Scholar at the University of the Philippines during 1949–50, where he helped train young Philippine anthropologists. His fieldwork during that Fulbright year was focused on Sagada, an Igorot community west of Bontoc. From this research flowed a number of papers on the Philippines, the most notable being his article titled “Cultural Drift and Social Change” (1963), which appeared in the Festschrift for Melville J.Herskovits. Eggan also served as the supervisor of the four-volume *Area Handbook on the Philippines* (1956) published by the Human Relations Area Files, Inc.

In the 1960s Fred Eggan became one of our most esteemed senior anthropologists. His contributions were recognized by his election to the American Philosophical Society in 1962 and to the American Academy of Arts and Sciences and the National Academy of Sciences in 1963, as well as by being invited to deliver the Lewis Henry Morgan Lecture at the University of Rochester in 1964.

The Morgan Lecture gave Eggan an opportunity to make a modern appraisal of the scientific achievements of Lewis Henry Morgan, to summarize and synthesize his own scholarly efforts to understand changes in kinship systems, and to establish a link with that first American scholar to undertake a systematic study of kinship.[3] The lectures were published as *The American Indian* (1966) and they constitute, in the wise words of one of Eggan's students: "the most thorough and readable synthesis of American Indian kinship and social organization in the literature and serve as a model comparative study" (DeMallie, 1991, p. 175).

The 1960s were also a time of personal turmoil for Fred Eggan with the long illness of his first wife, Dorothy, who died in 1965 and to whose memory he dedicated the publication of the Morgan Lecture (Eggan, 1966).

In 1969 Fred married his second wife, Joan Rosenfels, a photographer and psychotherapist, who is well known in anthropological circles for her remarkable photographs of anthropologists, some of which are in the Royal Anthropological Institute in London. Fred and Joan led busy and happy lives together in Chicago, where Joan practiced psychotherapy for over twenty-five years as a therapist with the students of the Laboratory Schools of the University of Chi cago and as a psychological consultant to school administrators and pediatricians. In Santa Fe, where they moved upon Fred's retirement, Joan served as a psychological consultant to two private schools; her present private practice is mainly limited to adults in the arts. She is likewise currently undertaking a study of Jungian dream analysis with the hope of analyzing the dreams of Don Talayesva (whose biography was published in the book *Sun Chief*) that were collected over the years by Dorothy Eggan.

In 1970 Eggan was a visiting fellow at All Souls College at Oxford. He delivered the Sir James Frazer Lecture at Cambridge University in 1971 and became a corresponding fellow of the British Academy in 1974. He also served on many boards, councils, and committees, becoming president of the American Anthropological Society in 1953 and a member of the Council of the American Academy of Arts and Sciences (1982–85) and of the Council of the American Philosophical Society (1983–86).

In his retirement in Santa Fe, Fred Eggan continued his work on the Indians of the Southwest and became a crucial researcher, consultant, and champion for the Hopis and Zunis in their land claims against the U.S. government. The deep respect these Pueblos had for Fred Eggan and his work is expressed in the following message (in part) to his widow from Vernon Masayesva, chairman, and Abbott Sekaquaptewa, past chairman, of the Hopi Tribal Council at the time of his death:

> We will miss Dr. Eggan greatly, but we realize that his contribution to understanding and documenting Hopi culture, and his involvement with our eternal struggle to recover our ancestral lands will be his everlasting legacy to the Hopi Tribe. May the Great Spirit be with you and your family and with Fred as he continues his journey to join his ancestors, (personal communication from Joan Eggan, November 2, 1991).

Fred Eggan had a great capacity for friendship and became a mentor for dozens of students and younger colleagues in anthropology (including this author)—as an "older brother" when he was younger, and as an "uncle" when he became older. He also made countless indirect contributions to science in his service on boards, panels, and committees that perform the annual decisions and tasks that must be done for our enterprise to carry on and move forward. He has indeed been described as "the model anthropologist of his generation" (Fenton, 1992, p. 435).

But Fred Eggan's greatest impact in the long run will come from his publications, which exhibit, in the thoughtful words of one of his younger colleagues at the University of Chicago: "His clarity of vision, ability to reduce complex phenomena to their essentials with minimum distortion, and capacity to demonstrate productive connections between hitherto disparate approaches and theories…" (Fogelson, 1979, p. 165).

Fred Eggan and his scholarly contributions will be long and warmly remembered by his colleagues in anthropology and other sciences throughout the world, as well as by his countless friends among the peoples he studied in North America and in the Philippines.

NOTES

1. The author has drawn on the biographical files of the National Academy of Sciences and the reminiscences and comments of Joan Rosenfels Eggan and James B.Griffin, as well as on various autobiographies, biographies, and obituaries of Fred Eggan, including Raymond J.De Mallie, "Eggan, Fred" in *International Directory of Anthropologists,* ed. C.Winter, pp. 174–75, New York: Garland, 1991; Fred Eggan, "Among the Anthropologists," *Annual Review of Anthropology* 3 (1974):1–19; William N.Fenton, "Fred Russell Eggan," *Proceedings of the American Philosophical Society* 136(1992):433–34; Raymond D.Fogelson, "Eggan, Fred," *Encyclopedia of the Social Sciences: Bio*

graphical Supplement 18(1979):163–66; Marshall Sahlins, “Fred Eggan: History and Structure,” *Anthropology Today* 8(Feb. 1992):23–25; Ernest L.Schusky, “Fred Eggan: Anthropologist Full Circle,” *American Ethnologist* 16(1989):142–57; Aram A.Yengoyan, “Fred Eggan (1906– 1991),” *Journal of Asian Studies* 50(1991):1017–19; and Mario D.Zamora, “Fred Russell Eggan 1906–1991,” *Eastern Anthropologist* 44(1991):313– 14.

2. See Dorothy Eggan, “The Significance of Dreams for Anthropological Research,” *American Anthropologist* 51(1949):177–98; “The Manifest Content of Dreams: A Challenge to Social Science,” *American Anthropologist* 54(1952):469–85; and “The Personal Use of Myth in Dreams,” *Journal of American Folklore* 68(1955):445–53.

3. See Lewis Henry Morgan, *Systems of Consanguinity and Affinity of the Human Family,* Smithsonian Contributions of Knowledge, vol. 17. Washington, D.C.: Smithsonian Institution.

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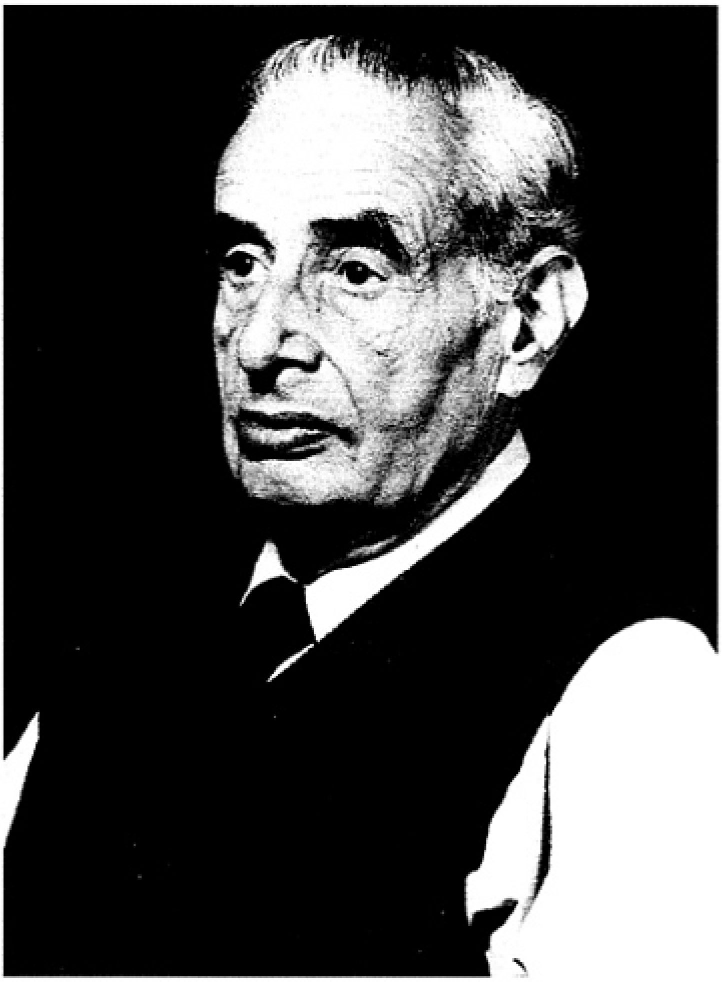

WALTER M.ELSASSER

March 20, 1904–October 14, 1991

BY HARRY RUBIN

ALTER ELSASSER WAS TRAINED as a theoretical physicist and made several important contributions to fundamental problems of atomic physics, including interpretation of the experiments on electron scattering by Davisson and Germer as an effect of de Broglie's electron waves and recognition of the shell structure of atomic nuclei. Circumstances later turned his interests to geophysics, where he had important insights about the radiative transfer of heat in the atmosphere and fathered the generally accepted dynamo theory of the earth's magnetism. He devoted a major part of the last fifty years of his life to developing a theory of organisms, concentrating on the basic features that distinguish between living and inanimate matter, and he produced four books on the subject. While his contribution to biology was not widely acknowledged, he felt it would eventually be seen as his major scientific achievement.

BACKGROUND AND YOUTH

Walter was born in Mannheim, Germany, the older of two children of Maurice and Johanna Elsasser. His sister, Maria, was three years younger than him. His grandparents were prosperous Jewish merchants, but his father was a law yer who was caught up in the great wave of assimilation and both parents became nonpracticing Protestants. Walter was confirmed in the Evangelical Church and had no idea of his Jewish ancestry until the age of fifteen when an acquaintance unexpectedly asked him about it. His father gave evasive answers when he inquired about his ancestry, and it took about a year to learn the truth. Up to this time he had no notion of Jews as a separate group, but his Jewish identity was to prove a crucial factor for him in the rising tide of antisemitism that culminated in the Hitler regime. One of the first manifestations of that antisemitism occurred in his last year of high school, when he applied to join a fraternity at the urging of his father who thought it would help turn his son from somewhat of an oddball into an ordinary good German citizen. His application was rejected on the grounds of the so-called Nürnberg articles adopted in 1919 by a national organization of fraternities which specified that persons of Jewish descent were inadmissible.

Up to the age of thirteen Walter had a congenial up-bringing, although there were severe food shortages because of World War I. His father, then in his forties, was called into the German army and because he was a lawyer was given a desk job at the headquarters of the Swiss border guard. Since he had earlier developed a severe ulcer, which was exacerbated by barracks food, he obtained permission to have his family join him. When headquarters were shifted from a small town to a Konstanz, the food shortage became more severe. His father's illness became so bad he was mustered out of the army and shortly after was promoted to a judgeship at the Superior Court of Heidelberg.

Walter attended Gymnasium, which had a nine-year curriculum, roughly equivalent to the fourth through twelfth grades in America. The emphasis was on classical subjects such as Latin, Greek, mathematics, history, and religion with only a smattering of physics and chemistry. This was in contrast to the alternative Realschule, which appeared in the nineteenth century and emphasized science, mathematics, and modern languages. He felt that the unpragmatic immersion into a past world tended to bring out introverted features that he already possessed. In any case, Germans predominantly thought of science as a philosophical enterprise, and Walter maintained a strong interest in the philosophical aspects of science throughout his career.

THE ROAD TO SCIENCE

Walter's first encounter with natural science came from a journal of popular science to which his father subscribed. The journal also issued a series of small books dealing with various subjects of scientific research, which he perused from the age of thirteen or fourteen. These were monographs covering all branches of science by carefully chosen authors who knew their fields well and had a knack for popular interpretation. He was particularly attracted to the books dealing with the mysteries of the discoveries of atoms and molecules, a curiosity that never left him. His mathematics teacher around the eighth grade took an intensive interest in him when he discovered Walter's interest in science and mathematics. They took long walks in the wooded hills around Heidelberg and discussed everything concerned with science and the nature of scientific inquiry. In the ninth grade, mathematics was chiefly concerned with solid geometry, which his teacher thought Walter could learn in a fraction of the time devoted to it. He then suggested Walter get a book of calculus problems and do them in lieu of solid geometry. Walter worked through these elaborate problems one by one and thereby acquired an extensive working knowledge of calculus long before graduating from

Gymnasium. He also had a fairly good intuitive understanding of avant garde thought in physics from the monographs on atoms and crystals, light and X rays, and stars and galaxies, which he read twice if not more often.

Walter also found books of a philosophical character in his father's library, among them Ernst Haeckel's enormously popular *The Riddles of the Universe*. He already recognized the book as a statement of a very coarse rationalism or straight materialism. Although he disliked Haeckel's crude philosophy, the shock he received from it opened his eyes to genuine problems in the philosophy of science, which occupied much of his thinking in later years. Under pressure of antisemitism he began to identify with his forebears, the authors of the Bible. These men were unanimous about one thing—that the understanding of nature, man, or God was not a wholly intellectual matter. This stood in contrast to Haeckel, whose world was nothing but Haeckel-like intellects trying to understand the world intellectually.

Stimulated by such quasi-philosophy Walter started thinking about philosophy, in particular Hegel's dictum that, when quantitative differences in some field become pronounced, they tend to turn into differences in quality. This contrasts with the view of the great philosophers of science, that the scientist in his methods has no place for qualities: they pertain to philosophy proper, usually expressed as metaphysics. An example is the notion of heat, which first appears to our perceptions as a quality but which physicists have shown is motion of molecules that can in all detail account for the properties of warm and cold bodies.

Philosophical thinking eventually led Walter to the realization that the purpose of the scientific method, which is to structure the multifarious data of experience, is neither simple nor obvious. This is apparent in the example that for thousands of years wise men studied the motions of stars and developed complicated mathematical descriptions for them. Around 300 B.C. Aristarchos proposed that the earth rotates about itself and moves in an orbit around the sun, as did the other planets. This view was ignored as idle speculation for eighteen centuries up to Copernicus. Walter grew to realize that acceptance of scientific ideas depends on whether they harmonize with the prevailing ideas of society. These ideas are controlled by unconscious tendencies and cannot be controlled by rational volition. He considered “the current unrelieved and brutal dominance of pragmatism in science, often clothed in terms such as political or other ‘relevance’,” a frightening development.

Walter tried his hand briefly at the commercial enterprise left by his grandparents. He was surprised at how much he liked the work, primarily because it provided a framework for his activities. However, he had become deeply committed to scientific activity and did not wish to abandon the sense of intellectual adventure found in any scientist of an inquisitive mind. He therefore returned to science with a renewed determination to become a physicist. He entered the University of Heidelberg, where the professor of physics was Phillip Lenard, who had received the Nobel Prize several years earlier. While still in high school Walter had occasionally skipped class to attend Lenard’s lectures with their admirable demonstration experiments. Lenard was actively involved in right-wing politics and was later to retire from his professorship to devote himself entirely to the Nazi party, ending up as president of the Nazi Academy of Sciences. But in 1922 politics was far from Walter’s mind when he entered the large lecture room for his first physics class. Every seat was taken as Lenard walked in wearing an impeccably tailored suit bearing an enormous silver swastika. This was unusual as Germany was still a place of law and order, and professors were not expected to brandish symbols of political extremism in class. But the students gave him the longest, loudest, and most dedicated ovation Walter ever witnessed either before or after. They had clearly voted for the swastika, and Walter, who knew what this meant for Jews, was deeply disturbed. A number of people advised him to leave after his first year, as he would then have to enter the laboratory where Lenard was as likely as not to grab him by the scruff of the neck and throw him out bodily. He therefore decided in the fall of 1923 to move to Munich, by far the best university in southern Germany.

There were two kingdoms of physics in Munich, one headed by the noted experimentalist Wilhelm Wien and the other by the theoretician Arnold Sommerfeld. Walter did considerable experimental work in Wien's institute and enjoyed it very much. During his third semester he worked assiduously on such complicated matters as the Millikan oil drop experiment and the electrostatic quadrant electrometer. But the chief influence on him was Sommerfeld, who was as brilliant a teacher as he was a research man. Walter considered Sommerfeld's classes the best he ever attended. In addition, he participated in Sommerfeld's weekly seminar on contemporary atomic physics attended by his assistants and a small number of students. The seminar stimulated Walter to read some scientific literature on his own. He particularly remembered a paper by James Franck of Göttingen on spectral lines that involved highly excited states of gaseous atoms. Calculations indicated orbits of the electrons that were incomprehensible from a simple mechanical point of view. This provided an unexpected glimpse into a different order of nature in the minute dimensions of the atom that would soon be expressed in the mathematical language of quantum mechanics.

In Munich Walter overlapped for one semester with Werner Heisenberg, who then obtained his Ph.D. and went on to

Göttingen. On several occasions he heard Heisenberg say that doing physics was fun. This came as a great revelation to Walter, who had grown up in the stolid environment of the German middle class and who could think of scientific research only as a matter of duty or personal ambition or just to make money. The insight of doing science for the fun of it left Walter exhilarated and deeply impressed.

Walter became fond of traveling and hiking during his years in southern Germany. While in Munich he took many weekend trips to the fore-Alps, staying in the numerous inexpensive youth hostels and hiking on the innumerable trails to the top of the mountains, often 2 kilometers high. He decided to become an experimental physicist, but early in his third semester in Munich an assistant professor approached him with the advice that every single member of the faculty of Wien's institute aside from the director and himself were card-carrying members of the Nazi party. It was, of course, in Munich in November 1923 that the abortive beer hall putsch by Hitler and Ludendorff, the former chief of staff of the German army during World War I, took place. Munich would therefore not be a favorable place for Walter to continue to work toward his Ph.D. He suggested that Göttingen was not only very good but "full of Jews." Walter asked Sommerfeld to write a letter of introduction to James Franck, whose work had so intrigued him, and he at once acceded to Walter's visit. When he arrived in Göttingen early in 1925, Franck accepted him almost immediately as one of his Ph.D. candidates.

THE WORLD OF GÖTTINGEN

Walter developed a close relationship with James Franck, whom he admired greatly. Franck kept his office open, and Walter often found himself sitting on one end of an old battered sofa in lively discussion with Franck at the other end. Franck's main interest was in the study of atoms and molecules by the simplest means possible, electrons and light. He had already shown that electrons transfer energy strictly in lumps or packages and had received the Nobel Prize for this achievement in 1925 along with his younger collaborator Gustav Hertz. Walter received for his thesis the subject of fluorescence, whereby one quantum of light is absorbed while another slightly different energy is emitted. While he was making technical preparations for this thesis work he would drop in now and then to Franck's office to question him about atomic physics, and Walter came to regard Franck as his main teacher of science.

Max Born, the theoretician, was also a professor in Göttingen and the growing international reputation of Born and Franck attracted many foreign students, among them Robert Oppenheimer, Robert Brode, H.P.Robertson, and Patrick Blackett. Paul Dirac was a frequent visitor. Walter also had close interactions with German students in the institute and developed close friendships with Fritz Houtermans and Wolfgang Harries. There were many interesting lectures in physics, especially theoretical physics. One course was a seminar titled "The Structure of Matter," which played a germinal role in the development of quantum mechanics. Although the seminar was long listed under the name of David Hubert, the famous mathematician, he was no longer active and its conduct was left to Max Born. Walter was a regular attendant at the seminar throughout his stay in Göttingen.

One of the earliest presentations in this seminar was by Born's student, Friedrich Hund, who later made major contributions to the theory of atomic spectra. The report was about an experiment of Davisson and Kunsman, two physicists at Bell Telephone Laboratories in New York. They shot electrons at a platinum plate and observed how they were scattered back. They found that the intensity of the distribution of the electrons varied with the angle of scattering, showing maxima and minima. This was a mysterious and quite surprising result, but the source was unimpeachable. Born tried to explain the result by the variable deflection of the extraneous electrons by shells of electrons that were of different densities. Without calculations it was impossible to know whether this suggestion was correct. One day in May 1925 Walter found in the library two recent papers by Einstein on the effect of quantum theory on gases. Einstein showed that certain gases behaved like assemblies of waves rather than particles. Twenty years earlier Einstein had noted that light, which everyone thought to be of wave motion, also had particle properties and that light was emitted and absorbed in packets called quanta. Coming from Einstein this was highly significant news. Einstein then noted a thesis of Louis de Broglie, which Walter found in the university library. The thesis contained de Broglie's basic idea that all primary components of matter have wave properties and presented a simple formula connecting the wave length with the particle's velocity. Walter wondered whether Davisson and Kunsman's maxima and minima were diffraction phenomena similar to those produced by X rays penetrating crystals but produced by a slight penetration and reflection of the electrons. He easily calculated the energy of the electrons required for the maxima, and it came out just right. Since the experiments were still crude, his surmise was only a guess but an exceptionally interesting and promising one.

Walter talked with Franck about the problem and was encouraged by his opinion that the idea was interesting though speculative. Franck suggested Walter think it through carefully and write to *Naturwissenschaften*. A few weeks later he did so and, after receiving Franck's approval, sent it off.

He later learned that the paper was reviewed by Einstein, who indicated that he was not sure how literally the idea of waves associated with electrons should be taken but thought the paper should certainly be published, which it was shortly thereafter. In print it became a note about half a page in the folio-sized volume. Heisenberg wrote to Wolfgang Pauli about the importance of Walter's note, and it was reproduced by Max von Laue in 1944 in a book on matter waves. In 1927 the decisive publication of Davisson and Germer appeared which demonstrated the wave character of electrons without a doubt. The authors referred to Schrödinger's famous papers of 1926 but not to Walter's note. Born published an article in 1926 in which he treated the collision of an electron with an atom as the scattering of a de Broglie wave and then developed a whole mathematical machinery for wave scattering. In this article Born introduced the notion of probability for the first time in quantum mechanics; he proposed that the wave function was a statistical guide for the particles in the sense that the amplitude of the wave specifies a probability for the particle to travel in certain ways. At the end of the paper Born quoted Walter's note saying he had correctly interpreted the experimental results of Davisson and Kunsman.

Publication of his note turned Walter's head and he asked Franck if he could experiment with the scattering of electrons by metal surfaces. This was quite foolish because that type of experiment is technically very difficult, and Walter's skill was not up to it. Franck agreed as long as he would do it on his own, since Franck would not allow his group to engage in highly speculative exercises. Walter tried it for three months before he realized how silly it was for an inexperienced young man to undertake such formidably difficult experiments on his own.

Among Franck's six or eight graduate students, Walter felt he was the least successful at building apparatus, a skill that was essential to an experimental physicist of those days. He also recognized that he was the most passionately interested in and most knowledgeable about physical theory. This was apparently recognized by Max Born, who, in the summer of 1926, asked Walter if he would consider becoming a theoretical physicist and doing a thesis with him. After consulting with Franck he decided to accept the offer and undertook an uncomplicated study of the collision of an electron with a hydrogen atom. This involved straightforward mathematical techniques with a large pile of formulas and offered few difficulties to Walter. He had few opportunities to discuss his work with Born, who worked at home and exhibited little interest in seeing students. Walter was fortunate at this stage to have the help of Robert Oppenheimer, who could steer him to the proper place in a mathematical book when difficulties did arise. One conversation with Born Walter remembered well. Born, who was a mathematical virtuoso, told Walter that he was not outstanding in mathematics but strong in conceptual thought, where Born felt less secure. A common idea is that conceptual thought precedes precise mathematical analysis: models and patterns emerge out of the primal chaos of data and thoughts of human experience and often cannot be predicted. Walter had already recognized that his great strength lay in conceptual thought, and his self-confidence grew stronger with age, so he feared no competition in this area.

Walter chose astronomy and mathematics as his required minors in the Ph.D. program. The astronomy, which was mainly astrophysics, confirmed the ideas of the uniformity of atomic physics and its laws. Mathematics in Göttingen involved some of the great men of the field. It had been established there by Carl Friedrich Gauss, one of the most remarkable and versatile mathematicians who ever lived. Bernard Riemann, who had developed Riemannian geometry, had been a professor of mathematics in Göttingen. David Hubert was considered the grand old man of mathematics while Walter was in Göttingen. The mathematician closest to the Franck-Born group of physicists was Richard Courant. Many mathematicians visited Göttingen, among them Norbert Wiener, who Walter met and occasionally encountered after he moved to the United States.

In his 1948 book Wiener generalized the concept of feedback, which had been discovered in neural circuits a century before. In the wake of the rise of computer technology the subject dealt with by Wiener is commonly called systems theory. It is the general mathematical theory of machines that operate in a causal manner. By the time Walter had read Wiener's book, quantum mechanics had led to a broad confidence in the uniformity of nature, and he felt that from there one could attack the central problem in the philosophy of science, the relationship of inorganic science to organic life. In the early days of science, the age of the clockwork universe, Descartes had declared that there were two substances, matter and mind; the body pertained to matter and was a machine pure and simple, an automaton. But to decide whether the body was simply an automaton, one had to know just what machines can and cannot do. This is exactly what modern systems theory claims to do, and Wiener had given the field its greatest impetus. He thus played a major role in framing Walter's approach to the problem of the relationship of inorganic science and organic life.

Another mathematician Walter met in Göttingen was John von Neumann, who later influenced his scientific thought as much as any individual, von Neumann was one of Hilbert's assistants and was charged with keeping Hubert abreast of new developments in quantum mechanics. Walter only saw von Neumann in lectures and seminars but had regular discussions with him at Princeton between 1946 and 1949 on their common interest in hydrodynamics. He learned a useful lesson from von Neumann's celebrated 1932 book *The Mathematical Foundations of Quantum Mechanics,* namely that any model of organic life based only on the existence of statistical features of physics was likely to be false. Walter studied the book carefully, and it taught him that the introduction of probabilities into physics, which is the most distinctive feature of quantum mechanics, did not "loosen up" the framework of the theory but made it even more deterministic in the mean and more suitable for reducing everything to physics than Newtonian mechanics had ever been. Although this was a valuable lesson for Walter, it deeply dissatisfied him because he distrusted reductionism, especially as applied to organisms. He struggled with von Neumann's book for years, both to absorb its technical details and to explore its philosophical implications. It took some twenty years of this struggle for him to escape the philosophical impasse of the mechanistic nature of quantum mechanics and his distrust of reductionism in organisms. The escape consisted of comparing the infinite sets of symbols underlying mathematical description with the finite sets of observations that experience offers. This later took the form of heterogeneity or individuality among organisms as a key element in setting apart organic life from the inorganic world. More will be said about this in a later section dealing with Walter's theoretical work in biology.

THE TROUBLED YEARS

Shortly after receiving his Ph.D. in 1927, Walter received an unexpected invitation from Paul Ehrenfest, a well-known theoretical physicist, to be his assistant in Leiden, Holland, for a semester and possibly longer, and he accepted. He wondered who had recommended him since he did not know Ehrenfest and concluded that it must have been Oppenheimer. Shortly before leaving for Leiden, Walter received a long letter from Ehrenfest concerned not with physics but with the latter's psychological problems. Walter worried about this strange behavior of explaining the complexities of his soul to a stranger half his age, soon to be his assistant. Ehrenfest met him at the train station in Leiden and immediately took him on a long walk on which he recounted his psychological problems and appeared to be pleading for help. Walter offered to help as much as he could, but as events turned out this was not to be.

Walter settled down in Leiden, enjoying the people, the city, and the countryside. He loved paintings and recognized scenes painted by great Dutch artists. He came to know H.A.Lorentz, the famed mathematical physicist, who brought Ehrenfest to Holland to be his successor. Walter served as Lorentz's assistant at a series of lectures. The lectures were at first utterly strange to him, as they differed radically in style from that of the German university. Lorentz started with general propositions in a pleasantly undulating voice that had a hypnotic effect and after ten minutes or so shifted to a very precise description of an electron as a little charged ball. It then became clear how it moved in an electrical field. Walter drew a parallel between this lecture style and the clair-obscure style of Rembrandt's paintings with their varying shades of brown from which there emerges an intensely illuminated face or object. This visual simile allowed Walter to understand Lorentz's presentation, and he thereafter enjoyed the lectures immensely. Walter later wondered how much of his own thinking originated in those lectures. This involved the thought that a scientist ought to imitate nature in discovering how order can be created out of chaos. He then realized that there were two forms of creation, the first being the creation of raw inorganic matter in the "big bang" or other cosmic device. But anyone who admits that living things are not just automata has to assume that there is an ongoing creative process in organic life that is much closer at hand and more readily studied than the cosmic process.

Walter developed pleasant friendships in Holland, but his relationship with Ehrenfest deteriorated as the latter changed from aggressiveness to downright hostility, for reasons Walter could not understand. One day after Walter had had a haircut with the usual barber's pomade, Ehrenfest came into his office and accused him of wearing perfume. Ehrenfest said he hated perfume, grew furious, and ordered Walter out. A few days later he told Walter to go back to Berlin. It is perhaps not unrelated to this behavior that a few years later Ehrenfest killed his retarded son and committed suicide.

Walter was, of course, very upset by Ehrenfest's rejection and returned to his parents' home then in Berlin. He realized that the entire episode would greatly reduce his chances of employment abroad and that he would most likely end up as a teacher of science and mathematics in a high school. This in itself seemed a reasonable enough career, but given the ever rising tide of antisemitism in Germany he felt he would be spending much of his time defending himself against Nazi hoodlums. His growing alienation from the majority of his fellow citizens was not compensated by a strong positive feeling of being Jewish since he had never been provided with the opportunity to develop such identification. Feeling blocked in, he became severely depressed and lacked the initiative to formulate and carry through scientific research.

Walter's parents were sympathetic and agreed to let him spend another postdoctoral seminar away from home. He decided to study with Wolfgang Pauli in Zurich. Walter attended Pauli's lectures on quantum mechanics, which he found both powerful and elegant. Although he tried to work closely with Pauli, Pauli did not seem very interested. A few years later Pauli told Walter he had seemed so weak and shaken up at the time that he was afraid he would faint if he breathed on him. Many years later in 1958 Pauli read Walter's first book on "philosophical biology" and wrote asking questions, which Walter answered. Later that year they met in Berkeley, California, and had a pleasant visit.

While in Zurich Walter also became acquainted with the famous Swiss mathematician Herman Weyl. He read Weyl's popular book on relativity that was "all the rage" among physicists at the time. At first he was very taken with the book but later realized that despite its exhibition of prodigious mathematical skill it made only tenuous contact with reality. He began to be aware that one cannot grasp reality by a commitment to one technique or procedure. He dimly perceived that anyone who depicted nature in a manner that seemed wholly comprehensible should be approached with the greatest skepticism. Many years later he learned from experience that, as soon as one exhibits skepticism about the comprehensibility of scientific description, many otherwise sound scientists become uncomfortable and freeze into dogma.

At the end of the semester, Walter returned to Berlin, where his friend Fritz Houtermans procured for him a part-time position as an auxiliary assistant in the physics laboratory at the Polytechnic School. His parents continued to provide room and board at their apartment, which gave him the wherewithal to lead at least a limited life of his own. He stayed on for two years in Berlin. One man who impressed him deeply was Max von Laue, the discoverer of

X ray diffraction by crystals, von Laue was very favorably disposed toward Walter, partly because of Walter's paper interpreting the experiments of Davisson and Kunsman as demonstrating the diffraction of de Broglie waves. It was a sign of Walter's psychological difficulties in that period that he could not utilize von Laue's good offices to start over again in theoretical physics, where he had made a small but spectacular start.

While in Leiden Walter had become acquainted with a Russian experimental physicist named Obreimov, who later became director of a new physics research institute in Kharkov, a large industrial town in the Ukraine. Late in 1929 Obreimov asked Walter to come to Kharkov as a technical specialist and he agreed. He was the first non-Russian to be associated with the institute, so it was a challenging undertaking. The challenge of adventure in a foreign land, plus the likely deterioration of the economy in Western countries following the 1929 Wall Street crash, induced Walter to accept. Russia was in the midst of a great famine, but the numerous engineers who had come from the West to work in the factories were supplied with the limited food of a special store. When he tried inviting his Russian colleagues to share in his spare meals they politely but firmly refused. He was impressed with the transformation of society from conditions of bare survival in peasant communities to a more technologically oriented one. He was convinced that the revolution was primarily cultural and educational and only secondarily political and economic. It was a desire for cleanliness, orderliness, elementary decency, and honesty by people who had been confronted with a harsh environment they could not subdue and had sunk lower and lower century after century. He met some of Russia's outstanding physicists and toured to Odessa on the Black Sea with Paul Dirac, who was attending a meeting there. Walter traveled on through the Caucasus and then from Baku to Rostov before returning to Kharkov. He therefore had an opportunity to view a great deal of the countryside and its people. These experiences did his mental state much good, but he soon developed a severe case of infectious hepatitis, which required his return to Germany.

Walter felt that the half-year spent in Russia was the most profound external experience of his life. The 1,600-kilometer displacement to the east introduced him to a new universe, where almost none of the concepts he had grown up with were applicable. It made him aware of the tremendous heterogeneity among people, as contrasted with the uniformity of the behavior of matter that allows us to predict the behavior of atoms and molecules in distant galaxies. It also convinced him that any ideas of the unification of diverse societies are just illusions.

In 1931, after his recovery, he accepted an offer to be an assistant to Professor E.Madelung in Frankfurt. An aunt who lived in Frankfurt told him that it was an ancient mercantile town where Jews had flourished and that he need not fear discrimination. Within six months, however, Walter found that the racist disease had spread there, leaving little hope for a Jew to remain in a university position.

SKETCHES FROM BERLIN

In the six years between obtaining his Ph.D. and leaving Germany, Walter spent about half his time in Berlin. It had become a cultural capital for many groups from eastern and southwestern Europe, and many talented people flocked there. The most memorable was a group of Hungarians, all entirely or partly Jewish though far from the Jewish tradition. Many appear in the history of physics, such as von Neumann, Wigner, Szilard, Orowan, and Polanyi. They met in a weekly seminar at the Kaiser Wilhelm Institute at Dahlem, and Walter frequently took the long subway trip to attend these lively and stimulating discussions. He became well acquainted with Wigner, who introduced group theory as the mathematical tool of choice for atomic physics and later, along with his student Frederick Seitz, provided the quantum mechanical basis for solid-state physics. On one occasion when Walter was "somewhat too easy with my imagination," Wigner counseled him, "One should tackle a problem only when its solution seems trivially easy, it will then turn out to be just at the limits of the manageable; when it appears more difficult, trying to solve it is usually a hopeless undertaking."

Walter also came to know Erwin Schrödinger rather well. Schrödinger was called from his native Austria to Berlin in 1927 as the successor to Max Planck, who introduced the idea of quanta in physics a quarter-century earlier. One thing that drew Walter to Schrödinger was that both were primarily visual types in a field, theoretical physics, where most of the practitioners were passionate musicians. A second factor was Schrödinger's conception of science as a natural philosophy, that is, that science was a continuation of philosophy by other means. Still another basis for common interest was the fact that Schrödinger's great papers of 1925–26 started from de Broglie's insight on the wave nature of matter, which Walter had recognized as the explanation for Davisson and Kunsman's strange results on electron scattering, as later established by Davisson and Germer. Schrödinger wrote a series of thirteen small books, several of which dealt with theoretical physics and others with philosophy. He was a traditionalist who drew his spiritual nourishment from the society in which he grew up. Walter felt that it was Schrödinger's commitment to historical continuity that led as he grew older to doubt, along with Einstein and de Broglie, the statistical interpretation of quantum theory and to suggest that it must ultimately be replaced by a strictly causal theory. Walter could not understand how one acquires these predilections since he felt that "scientific theory consists in finding all the order that can be detected by observations in nature, and in representing it by suitable means, mathematical, logical or other." He felt that the preference for traditional physical causality was patterned after the behavior of machines and was not an innate direction of man's mind. It probably dates from the age of Galileo and Descartes, and since it was born in history it can disappear in history.

Although Walter thought that Schrödinger's books on physics and philosophy were pearls of the art of exposition, he was highly critical of his famous little book *What Is Life,* which influenced many physically trained scientists to turn to biology. Walter thought that the book was a failed attempt to reconcile Schrödinger's humanistic philosophy with biochemistry, now often called molecular biology. While he acknowledged Schrödinger's superior skill in presenting his case, he believed that the basic philosophy of strict rationalism on which the book was based was a throwback to Descartes's seventeenth-century view that the world is formed of two substances, matter and mind-soul. The body belongs to matter and is basically a machine obeying causal laws. Walter thought that it was necessary to replace this paradigm with a better one before any deeper understanding of organisms was likely to occur. Basically, he believed that the dividing line between the inanimate and the living was much more powerful than that between man with his rational soul and brute beasts. He rejected vitalism as a flimsy substitute for a real criticism of the machine nature of organisms. His encounter with Schrödinger, whom he clearly admired, brought out a hitherto somewhat latent passion for natural philosophy, which inspired much of his later life.

THE CONVERSION OF A RATIONALIST

While Walter was still in high school, the family doctor, a distant relative, often referred to him as a "bundle of nerves." Such remarks and others convinced Walter that he was a high-strung individual. This appeared to him only as a deficiency, especially in the considerably brutalized environment of pre-Hitler Germany. During his troubled years after receiving his Ph.D., he became an aficionado of Marcel Proust, reading in succession all twenty-two volumes that made up the original editions of his novel. He felt Proust was an excellent introduction to depth psychology, or exploration of the unconscious, which Walter had been contemplating for some time. He was particularly impressed that Proust had succeeded in putting his oversensitivity to work for him in creating a splendid portrait of a whole society. He began to think of using his own perceived weaknesses for constructive purposes and kept this idea with him for the rest of his life.

When Walter moved to Frankfurt, he entered psychoanalysis with Dr. Karl Landauer, a man of great perception. The analysis continued every weekday for eighteen months, until one day in April 1933 when the Nazis actually carried out their long announced seizure of power. This act marked the death of the Weimar Republic and the beginning of the Nazi terror that was to last for twelve years. When he arrived that morning for his session, Dr. Landauer advised him to leave the country before the Nazis closed the Swiss border. Walter's immediate reaction was an indignant resistance to the suggestion, but when he arrived at the university he was greeted by a gang of brownshirts, each with a rifle slung over his shoulder. Their leader demanded Walter's university identification card, which he then pocketed and told Walter to go home and await further orders. Seeing the handwriting on the wall, Walter left for Zurich a few days later while his passport was still valid and the border still open. He never saw Dr. Landauer again but learned that he perished in the Holocaust.

The full measure of the analytic procedure did not dawn on Walter until after its termination. He then had the phantom feeling that someone was drilling on his innards, though he consciously knew the drilling had stopped. Through his work with Dr. Landauer, Walter learned that he was a far stronger person than he had ever perceived by self-observation. Landauer pointed out that there was a core of aggressiveness submerged in Walter's unconscious that contrasted with the timid young man cowed by Nazi bullying that he fancied himself. If this was correct, Walter realized he had the strength of developing into a scientifically creative person by the standard psychological technique of sublimation.

As a result of his analytic experience, Walter could no more question the reality of the unconscious than that of the electrons or atomic nuclei of his professional work. But the reality of psychic phenomena was of a different kind, characterized by elusiveness, irregularity, disorder, disharmony, or, in Jung's terms, irrationality. He read the literature of the psychology of the unconscious and began to appreciate its depth and breadth. The knowledge of the human unconscious appeared to him a scientific discovery of as great a radical novelty as the discovery of atoms, molecules, and nuclei. Those who spoke of the unconscious before Freud were like those who spoke of gravity before Newton, correct but irrelevant. Newton did not discover gravity but the structure of its laws. Similarly, Freud and Jung did not discover the unconscious but the structure of its modes of behavior.

Such musing filled a gap in Walter's philosophical under standing and even of his science. As a physical scientist it was hard for him to think about the human psyche as purely abstract functionalities apart from anchoring in the material substance of the brain. This led him to an encounter with the ancient philosophical problem of the dualism of body and soul, an interest that never left him. He felt the methods of theoretical science were sufficiently advanced to rescue the central problem of philosophy, the body-soul dualism, from metaphysics in order to treat it as a specifically scientific problem. This would have remained a matter of speculation had it not been for his meeting with the physiologist Theophile Kahn in Paris, about a year after leaving Frankfurt. They met frequently, and Walter was impressed with Kahn's claim that biology was first and foremost the realm of utter complexity. The idea appealed to him because it was simple, abstract, and general and therefore exactly like the notions constructed and used by theoretical physicists. He found no shortage of examples for this complexity in biology from the molecular diversity of cells to the structure of the human body. Later, when he became a leading figure in geophysics, he found that the complexity of neither the turbulence of the fluid earth or the structure of minerals and rocks in the solid earth could compare with the formidable complexity of all higher organisms, nor even that of a single cell.

Walter contrasted the complexity and individuality of organisms with the simplicity and universality of physical science. He felt that these two pairs bring out the difference, otherwise frequently hidden, between the ways of thinking needed in the biological and physical sciences. This realization had an overwhelming intellectual effect on him. He realized that the worlds of physics and biology could be reconciled only if one of the groups yielded and that it is the man who thinks in traditional terms of physics who must change his ways and learn to accommodate complexity and individuality if he is to encounter life and understand its nature. In particular, the notion of creativity that arises in depth psychology, can be converted into scientific terms and applied at all levels of the living world from the cell upward. One must show that the complexity of life is broad enough to encompass the creativity of the organism. The complex structure of the organism, including that of cells, that creates the enchainment of ever-new and unpredictable individualities not only makes creativity possible, it *is* this creativity. Walter was convinced that creativity is a basic property of all life and that the transition from simplicity and universality toward complexity and individuality is essential to the development of a true science of life. As he became successful in his later scientific activity in the United States, he became more committed to these ideas. Their articulation in books and articles led to many clashes with the biomedical establishment.

As these ideas developed, Walter began to understand Born's remark that his strength was more in conceptual thinking than mathematics. This had made no sense to him as long as he believed along extravagantly rationalistic terms that all thinking could ultimately be expressed in mathematical form. But if the utter complexity of organisms at both the structural and logical levels impeded the application of mathematical analysis, then conceptual thought could and indeed had to have a respected role in the hierarchy of human mental endeavors. If biology is the locus of utter and perhaps irreducible complexity, it can be the locus of partial irrationality, that is, of our inability to order the phenomena exhaustively in logico-mathematical schemes. Under these conditions, organic structure serves as the vehicle of this irrationality, and the study of the nature of this vehicle is strictly a scientific task, which was to occupy many of Walter's later years.

RUE PIERRE CURIE, PARIS

Upon arriving in Zurich, Walter went directly to the physics building of the Polytechnic School, where he was heartily greeted by Pauli. Without a spoken word Pauli understood that Walter was the first of many to come, escaping the grim situation in Germany. Fortunately, Pauli had just received a letter from Frederic Joliot, the son-in-law of Madame Curie, saying that the nuclear physicists in Paris needed a theoretical man, and Pauli wrote that he would propose Walter for the position. Joliot wrote back without delay accepting the proposal but indicated that it would take some time to set the necessary bureaucratic machinery in motion. Walter waited two to three months in Zurich for another letter from Joliot and then decided he might as well wait in Paris. There Joliot told him that he was encountering some obstacles to funding a position for Walter, but the matter would be cleared up. Soon thereafter Walter found himself the recipient of a fellowship from the Alliance Israélite Universelle, a Jewish cultural organization. This piece of good fortune actually annoyed him since it was essentially an act of charity, although he had never had any connection with Judaism or its organizations. After a year his support was transferred to the Centre National de la Recherche Scientifique (C.N.R.S.). He was given a small table in the library of the Institut Henri Poincaré from which he had to remove all his books and papers every night. The spare nature of the facilities and the restrictive working conditions were typical of the way scientists were treated in France at that time.

Walter got to know some of the remarkable men who directed French physical science and its instruction. Among these was the Nobel Prize-winning experimental physicist Jean Perrin, who was the first to obtain a precise value of Avogadro's constant, the number of hydrogen atoms in a gram of hydrogen. He also met Louis de Broglie, whose introduction of matter-waves in physics had provided the foundation for Walter's first published paper in Göttingen. Joliot was clearly the moving spirit in the physical sciences, and since he was the one who had brought Walter to Paris, he decided to specialize in the structure and dynamics of atomic nuclei. The transition from his previous research to the application of quantum mechanics to the nucleus was not difficult. During his stay in Paris, Walter got to know Joliot and his wife, Irene Curie, rather well, and he admired them both. He remembered Joliot as a man without affectation, who could deal with every man he encountered and could design experiments of formidable simplicity.

In the fall of 1933 many more scientists found their way from Germany to Paris. Walter was the oldest of these, and since he was on excellent terms with Perrin and Joliot, the new arrivals, if they were physicists, were sent to him. He received great support in helping these people from Jean Langevin, son of the eminent physicist and himself a physicist. Langevin had an "excellent mind and a heart of gold" and put out great effort in trying to find a niche for the refugee physicists in the Greater Paris area. Once a potential position was identified, Walter would escort the man to his destination and, if nothing was available, take him to the Alliance Israélite to keep him from starving.

Walter was intensely aware that his publication record was poor for one aiming to be a professional research scientist. He realized that he had previously had inhibitions about seeing his work in print. Paris provided him with a last chance to make his mark. The two journals available for a physicist were the *Comptes Rendus* and the *Journal de*

Physique. There was no formal scheme for review of papers; they merely had to be submitted through a well-known person. The *Comptes Rendus* limited the length of an article to two and one-half printed pages. The pages of the journal were large and had two columns, which was space enough to clearly formulate any ordinary communication of the theoretical type that Walter would submit. There was also a limit of five of these notes per year for an individual author. Walter took his first note to de Broglie, who was closest to his interests, and asked him to submit it. De Broglie told Walter that he had no desire to read it but might read it later at his leisure when it was in print. He advised Walter to hand his manuscripts to him at the same time each week before the start of the regular academy session. A week later the official printer had the proofs, which Walter immediately corrected. The next issue of the *Comptes Rendus,* which appeared the following week, had the article. Walter tried to publish the limit of five articles for each of the three years he remained in Paris. In addition, he published a total of seven somewhat larger articles in the *Journal de Physique*. All of these were on the structure and dynamics of the atomic nucleus, a subject in which Walter made his second great contribution to physics.

At that time Niels Bohr and his colleagues in Copenhagen had proposed the liquid drop model of the nucleus in which there was proposed to be a homogeneous agglomeration of protons and neutrons without further internal structure to the nucleus. Though there was considerable empirical support for this model, Walter had certain doubts that led him to think the nucleus would have a degree of internal structure. A large part of his efforts in France were devoted to following up on this idea. He discussed the problem of how the nucleus is held together with a refugee physical chemist, K.Guggenheimer, who had found a temporary post in a laboratory at the College de France. Guggenheimer had a great deal of knowledge about how molecules are held together starting from atoms. There are many analogies of the molecular forces with nuclei, but the energies of nuclear binding are 100,000 times greater than those holding molecules together. It became clear though from a study of chemical reaction kinetics that variations in binding energies of nucleons would in many cases be reflected in nuclear abundances, many of which were known. Walter proposed a joint piece of research to Guggenheimer, but they failed to agree on collaboration. Guggenheimer published two articles on binding energies of nucleons in 1935 and then left for a position in England. Walter then found a trick to obtain at least an approximation of the binding energies of individual protons and neutrons from the directly measured disintegration energies of the very heavy, naturally radioactive nuclei. He could then show how the binding energy of a nucleon decreases sharply beyond the end of a nuclear shell. He was satisfied he had established the existence of nuclear shells, though these were not analogous to the electron shells of atoms. Deeper physical understanding of the physical forces that brought about the nuclear shell structures only became available two decades later. A more detailed theory of nuclear structure was then worked out by Hans Jensen and Maria G.Mayer, who had been a Ph.D. student of Born's just after Walter left Göttingen. Both the latter workers received the Nobel Prize in 1963, which they shared with Eugene Wigner in a long-delayed recognition of his many contributions to atomic and nuclear physics. Maria Mayer quoted Walter's earlier work in a 1964 article in *Science,* where she also pointed out that the mathematical theory could not have been complete before the knowledge of nuclear interactions had advanced to the level of the 1950s. Despite the importance and originality of this pioneering work of Walter's, it received little recognition, probably because of the impact of Bohr's liquid drop model, which made its debut a short time earlier.

Walter's remarkable insight into the structure of the atomic nucleus typified other discoveries he made in the physical sciences, some of which will be described later. He would enter a field, read exhaustively about the subject, and then through calculations and reflection develop concepts appropriate for structuring the published observations long before specialists had penetrated deeply into the subject. Once he had confirmed his primary insight, he would, for various reasons, not work through the full ramifications of his ideas. The one great exception was his theory of organisms, which occupied much of the thinking of his last five decades. He had few regrets about the limited recognition of his work in physical sciences, particularly in the matter of nuclear structure, where he would have had to remain a nuclear specialist to follow up on the insights. This ultimately would have involved him in developing the atomic bomb. He of course had no idea of this possibility when he was developing his ideas about nuclear forces since atomic fission was not discovered until 1938, several years after he had left research in nuclear physics. In addition to his work in Paris on nuclear binding, he and Frank Perrin published a theoretical explanation of the exceptionally large capture cross-sections for slow neutrons that certain nuclei had shown. Others published similar analyses at the same time, including Hans Bethe, whose treatment was more extensive and general than those of anyone else. However, Walter was the first to explain the large capture cross-section as the square of the de Broglie wavelength of the neutron rather than the cross-sectional area of the nucleus. The original papers of his French sojourn were hardly noticed, and he turned to other things. He remarked to Eugene Parker that when

Shakespeare wrote Hamlet he had Walter Elsasser in mind. Parker wrote me that, “Walter had a deep aversion to asserting himself in the generally cynical scientific arena.”

PASSAGE TO THE NEW WORLD

By 1934 Walter’s small position as investigator was fairly secure, since it was financed by the budding French Science Center, and likely to last almost indefinitely. He then realized that he had to make a decision about his career. If he decided to remain in France, he would have to become a French citizen, which required two years of military service. If not a citizen, he would have to fight endlessly with a bureaucracy determined to keep him out. The alternative was to go to the United States. Since this was the time of a worldwide depression, it was hard to get a job, even in science. Having a stable position in Paris made it difficult to decide for the American option. His decision to do so was finally determined by his parents’ situation. His father in his early sixties was ill and had retired and moved to a small house in the Black Forest. By that time, however, the Nazi policies had become clear: in spite of the depression, they were going to rearm, and the property of the non-Aryans was going to finance at least part of the rearmament. That category included not only nominal Jews but those who had married into the middle and upper classes and changed their religion. His parents would be reduced to indigents, deprived of home, income, and normal medical care. A few years later in early 1939 Walter’s foresight was justified, when his parents were able to go to England for temporary residence pending their passage to the United States. In 1940 they came to America, where they later lived with his sister in Chicago.

Walter applied for an immigration visa to the United States and after the usual bureaucratic delays received it in 1935.

He then took a large German steamer from Le Havre to New York. The ambiance on board was his last closeup of the subdued nazification he encountered. On board, however, he met Margaret Trahey, an American girl of Scotch-Irish extraction, who was later to be his wife. She was reading C.P.Snow's *The Search,* about the rise of a young man from modest circumstances to academic research as a physicist. Since Walter was the first of that strange breed she had met, it helped smooth the way to a further understanding of each other.

Walter always remembered vividly his first sighting of the Statue of Liberty when the steamer entered New York harbor. He felt that no one who had not undergone years of persecution could fully appreciate what this means to an immigrant—the chance to begin life over again without the impediments that Europe throws in the way of anyone not born of a privileged social class. Although free immigration had stopped when he arrived in the United States, the country still accommodated itself to the reception of persecuted minorities. In his mind it was the spirit that counted: comparable conditions for entry existed in South America but little that corresponded to the reception of the downtrodden masses at the feet of the Statue of Liberty.

Two days after disembarkation Walter met I.I.Rabi of Columbia University, whom he knew from the latter's stay in Europe. Rabi set him up in a dormitory room on the Columbia campus and introduced him to Harold Urey. He also visited Davisson at the old Bell Telephone Laboratories, where the latter had first demonstrated electron diffraction. Walter was alert to the possibility of obtaining a position in all his meetings, but there was none to be had. He traveled on to Ann Arbor to attend a summer course by Enrico Fermi whom he came to know personally to some degree. Although Fermi was perhaps the most scientifically gifted of all the physicists Walter had encountered, he was simple and modest in his personal contacts.

During his month-long stay in Ann Arbor, Walter frequently visited with a young theoretical physicist, David Inglis, whom he had first known in Pauli's institute in Zurich. Inglis and his wife Betty introduced him to many of the nuances of American life that are not explained in guidebooks. If in France he was impressed by the sensitivity of the residents, he was impressed by the generosity of Americans. Unlike Europeans, the prospect of an uncertain economic future does not breed cynicism or despair among Americans.

Walter went on to Chicago, where he met Arthur Compton, discoverer of the Compton effect, the most spectacular demonstration that light exhibits corpuscular in addition to the well-known wave properties of light. Compton was most unsympathetic to Walter's quest for a position because he thought it unethical to give positions to Europeans when many American scientists were out of work. He had in fact sent back to Germany a young German physicist, although the latter had married an American and wished to stay in the United States. Walter knew this young man from his Frankfurt days. He was of frail constitution and died soon after being drafted into the German army from the exertions of a forced march. Thereafter Compton was always the first and most vociferous among those who helped displaced scholars from Europe.

After Chicago Walter returned to New York to take the boat back to Europe for another year in Paris. His trip had shown him that the prospects for nuclear research in the United States were slim, and he began to think seriously about another specialty. Since many of the ideas described above under "Conversion of a Rationalist" had already developed in embryonic form, he began to think that much of the science of the future would be concerned with com plex systems. Although the ideal place for such study would be biology, Walter was reluctant to commit himself to that field because it involved too much chemistry, to say nothing about the details of biology. He was later to approach biology by way of the semiphilosophical ideas described earlier. It occurred to him that in geophysics he could indulge the study of complex systems, using the tools of the theoretical physicist and some knowledge of astronomy, which had been a minor subject along with mathematics in his Ph.D. examination. He decided to cut loose from his moorings in Paris and on his next trip to the United States try his luck in California, specifically at CalTech. He wrote to Oppenheimer about his intention to go to Pasadena, where Oppenheimer spent a few months in the spring of every year as a part-time professor of physics. Oppenheimer answered promptly, suggesting that Walter first visit his wealthy father on his way through New York and then stop off at his ranch in New Mexico. Walter followed up on both invitations in 1936 after he arrived in the United States for the second time.

In a long, friendly conversation with Oppenheimer's father in New York, Walter found the man to be warm and generous. After a few days exploring the "marvelous and complex city" of New York, Walter moved on to Chicago, where he visited Margaret and they announced their engagement to her family. They also decided they could manage between their two small incomes. A year later they married and had a girl and five years later a boy. He derived the deepest happiness of his life from the seventeen years of their marriage, which ended with the loss of Margaret under extremely tragic circumstances. He did not remarry until ten years after the tragedy.

After several weeks in Chicago, Walter took the westward train headed for Albuquerque and Los Angeles and stopped off at a small station some distance from Santa Fe. He was picked up by Oppenheimer and driven to his ranch in the Sangre de Cristo mountains, where he could see through the trees the mountain range in which the Los Alamos Laboratory was later to rise. Included in his stay on the ranch was a daylong horseback trip with Oppenheimer into the surrounding mountains, which was exhilarating but tiring to one unaccustomed to riding. On their return Oppenheimer offered him a Chili drink he had prepared according to an old recipe. A mouthful of the vermilion fluid tasted like what Walter imagined sulfuric acid would taste, and he immediately spit it out. Oppenheimer told him he had passed the first but failed the second part of the initiation ceremony at the ranch.

After a few days of relaxation on the ranch, Walter took the train to Los Angeles and then on to Pasadena. There he met a physicist, Jesse DuMond, who introduced him to several people at CalTech, including Linus Pauling, who, though a young man, was already head of the chemistry department. Pauling almost at once offered him a position in his laboratory, but feeling he had no background in chemistry Walter refused. He considered this one of the wiser decisions he made in his life.

In 1936 DuMond made an appointment for Walter to see Robert Millikan, the head of CalTech, whom he was to see many times thereafter. He told Millikan that he was a theoretician who had worked in atomic and nuclear physics but was willing to work in other areas such as geophysics. Millikan told him, “If you want to do geophysics we can use you. If you want a place in nuclear physics or in astrophysics I can do absolutely nothing for you…. If you are serious about geophysics I can promise you that I will find some salary for you.” Walter considered Millikan one of the most extraordinary men he had ever met. He was, of course, famous for the oil-drop experiment of his younger years, which established the unit charge of the electron. His forte was not original thought but subtle and controlled strength in everything he did—a human powerhouse. Among his many accomplishments was to build CalTech into a major scientific institution. He once told Walter, “You are a producer; if you were put on the top of a mountain you would still produce.” Considering the eminence of the source, Walter thought of this as the highest compliment he ever received about his qualifications as a scientist.

Margaret came west in the spring of 1937 to find a job. She did find a tolerable but rather low-paying job, and they got married. Walter decided to make his break with nuclear physics gradually, so he made the acquaintance of Charles Lauritsen and his capable group of research students who represented nuclear physics at CalTech. Their work was at the forefront of studies on nuclear structure and dynamics then going on in the world. Oppenheimer was the official theoretician for the group, but he only spent two or three months each spring in Pasadena. Since Walter knew quite a bit about the subject, it was easy for him to converse with the group, which he did in the summer and fall of 1936. In return, men of the Lauritsen group alternated in teaching him one skill that was essential for a full life in the western United States, namely driving a car. Early in 1938 he and Margaret bought their first automobile, a somewhat aged Ford, in which they made many excursions. By the time Oppenheimer arrived in the spring of 1937, Walter had become deeply engrossed in the physics of the atmosphere, which seemed a weird subject to nuclear physicists, including Oppenheimer. As Walter had stopped reading the burgeoning literature of nuclear physics, he eventually lost touch with the congenial crowd of Lauritsen’s laboratory.

EARTH SCIENCE

In 1937 Walter joined the fairly new Meteorology Department at CalTech at Millikan's suggestion. Millikan had gotten a contract from the Department of Agriculture to be administered by the Weather Bureau, which was then part of Agriculture. Walter knew nothing about meteorology, but he read a few textbooks and came up with some unsolved problems in which he could use his accumulated knowledge of atomic physics. He then began to receive a small salary to supplant the loans on which he had been existing up to that point.

He undertook the study of the effect of infrared radiation on the atmosphere. The American Meteorological Society had for years been urging the American Physical Society to do research on infrared radiation in view of its importance for the atmosphere, but nothing was done. The far infrared spectra of the atmosphere have a very complicated structure that required an understanding of quantum mechanics for its full elucidation. The basic material on infrared spectroscopy had been gathered and presented by the physicist Gerhard Herzberg, another refugee from Germany then living in Canada. Walter spent the years 1937–41 analyzing the properties of far-infrared atmospheric radiation from first principles. After lengthy calculations, he ended up with tables and graphs that a practitioner could use to find the heating and cooling of the atmosphere when he knew the distribution of temperature and moisture.

This was a time of rest and recovery for Walter, despite the pinched material circumstances under which he lived. The hot breath of Hitler was off his neck for the first time in twenty years. Extreme brutalities did not exist in the California environment, and he had a congenial wife. He had plenty of time to think, which allowed him to develop the two basic ideas that dominated the scientific ideas of his later life: those that led to the theory of the earth's magnetic field and the relation between quantum mechanics and organic life on the background sketched out above under "Conversion of a Rationalist."

After he had done the atmospheric research for a few years with emphasis on calculations and drafting, Walter felt frustrated at his previous inability to do experiments. To correct this deficiency, he befriended John Strong, a very successful experimentalist. Strong taught him the procedures used by a successful experimenter, including careful advance planning and breaking down the procedure into a number of little steps, mastering each before proceeding to the next. Walter then set out to measure the far-infrared transmission of the atmosphere along paths up to 300 meters in length. This was done on an athletic field next to CalTech with equipment he built with his own hands. He monitored the temperature and moisture continuously and wrote up the results. Although he realized he would never be a first-rate experimentalist, the experience restored his self-confidence and made him realize that the American ambiance had a relaxing and stabilizing influence on him.

In 1936, before he was officially employed, Walter decided to take a meteorology course. Almost all of the twenty some students in the course were career military officers sent to Pasadena for training as weather officers. They were well aware that in a few years their new skills might be required for a war against Hitler. Much of the course was practical plotting of weather characteristics and upper-air data. Walter learned much about the peculiarities and vagaries of the earth's atmosphere, which proved useful in his later work on the earth's magnetism. He learned that the earth's atmosphere is full of unpredictable contingencies on every level of its scale, which gives the meteorologist a feeling for reality that is radically different from that of the laboratory scientist who can minimize contingencies.

The Meteorology Department at CalTech was part of Aeronautical Engineering headed by an outstanding man, Theodore von Karman, who had come to Pasadena from Europe in 1930 after a brilliant academic career. He tried to induce Walter to leave his meteorological studies and turn to hydrodynamic problems such as turbulence and thereby become an aeronautical theorist. As a result of Walter's refusal, he never got closer to von Karman. He also began to see that von Karman had only accepted meteorology into his laboratory as an accommodation to Millikan, who believed it was his patriotic duty to foster that subject. Walter saw that the smallness of his position was the result of a protracted political accommodation between two powerful men, and he began looking for other employment. His efforts were unsuccessful because positions were still rare in the wake of the great depression, and he had no money to travel around looking for opportunities. He asked Oppenheimer about a job on one of the campuses of the University of California, particularly the Los Angeles campus, but was told, "We do not wish to encourage graduate teaching outside of Berkeley."

Walter's position at CalTech came to a rather ludicrous end in 1941. He had gone to Washington on some business connected with his research. While there he encountered Carl Rossby, who was head of research for the Weather Bureau. Walter had gotten to know him well in the summer of 1938 when Rossby was head of the Department of Meteorology at MIT and had invited him to lecture there. Rossby controlled scientific meteorology in the United States, a task he found overwhelming, and decided to delegate control of the western part of the country to a deputy, that deputy being Walter. Walter thought this was some sort of fantasy that would never work out when confronted with reality but said nothing. However, von Karman got wind of this plan from Rossby, who told him that the Meteorology Department at CalTech was inadequate, since its head, a young assistant professor, was more commercially than scientifically inclined. He said that there was already an excellent man on the spot, Elsasser, and that he had already discussed with Elsasser his future functions. To emphasize his point, he declared that the contract with the Meteorology Department would not be renewed. Von Karman reported to Millikan that Elsasser had used the federal government to pressure CalTech into an appointment for himself.

When von Karman informed Walter of the situation, he saw no sense in asserting his innocence. He walked out of von Karman's office and considered himself fired. It occurred to him that the logical course of action was to move east, where, with the war coming on, there were greater opportunities. He wrote to Charles Brooks, professor of meteorology at Harvard, who was in charge of the Blue Hill Observatory in South Boston. Brooks agreed to give Walter a position compiling tables of various data with spare remuneration. He and Margaret packed their few belongings in the car and drove off to Boston. While there he wrote a monograph on atmospheric radiation, which Brooks published in a series he edited. Rossby, who by then was suffering from a bad conscience, had most of the edition bought up and distributed to meteorology students.

Walter started thinking seriously about the source of the earth's magnetism around 1940. Three months after the Japanese attacked Pearl Harbor, he received a telegram from the U.S. Signal Corps Laboratories at Fort Monmouth, New Jersey, telling him to promptly report there for duty. He spent the first part of the wartime years there and the second part working in the Empire State Building in New York for the Radio Propagation Committee of the National Defense Research Council. The chief part of his theoretical work on the earth's magnetism was done during the war. He had plenty of time on the weekends to work on this problem, especially toward the end of the war. He used this time to do calculations and write articles that appeared in a series of three papers in the *Physical Review* in 1946–47. The first systematic summary of the physics of the earth's interior was given in an article in the *Reviews of Modern Physics* in 1950.

Full exploration of the earth's body had begun in the 1890s, when the first seismological stations were set up. In the decades following, the main features of the earth's interior came into view. The most important of these was the sharp boundary about halfway between the surface and the center of the earth separating the core from the mantle. The core is made of molten iron, the mantle of conventional rock. There is by now a thorough quantitative knowledge of all the mechanical properties of the earth's interior. The one characteristic of the earth as a whole that was not included in this scheme was magnetism, a property studied scientifically since 1600. Early thinking was that the earth behaves like a bar magnet, magnetized along the earth's axis. But mysterious deviations were later discovered in the regularity of the earth's magnetic field. These irregularities occurred not only from place to place but from time to time, leading physicists to speak of the secular variation in the earth's magnetic field. In the first half of the nineteenth century, the famous Göttingen mathematician, Gauss, showed that if one knows the magnetic field over all of the earth's surface the field can be mathematically divided into a part whose sources are inside the earth and a part whose sources are outside. The overwhelming part of the field arises from sources within the earth. Toward the end of the century, it became clear that the internal source could not be of the bar magnet type, since the temperatures inside the earth are too high for this sort of magnetism to occur.

Two competing theories arose to replace the idea of a bar magnet. The first, espoused by a prominent group of theoretical physicists with Albert Einstein at the head, believed that a theory must be created in which large bodies such as the earth are magnetic by the very fact of their rotation. This involved finding a new term in Maxwell's equations to provide a connection between gravity fields and electromagnetic fields. The alternative theory was that electric currents flow in the conducting molten iron of the outer core. This idea had its origin in the observation in 1908 by the astronomer George Hale, founder of the Mount Wilson Observatory in Pasadena, that all sunspots have large magnetic fields. The British physicist J.J.Larmor attributed the sun's magnetism to a dynamo effect in analogy to the engineering term for power station machines that convert mechanical motion to electrical currents, the conventional rotating generators. Although Larmor vaguely suggested in 1919 that the earth's magnetism might be explained along these lines, not a single article appeared about it in the intervening years. The likely explanation for the total absence of papers on the subject is that the limited number of those qualified to handle the solution of problems of theoretical physics was absorbed by the two main streams of inquiry then existing in physics, namely relativity and quantum mechanics.

Walter's work was truly pioneering because there was no theoretical literature in the field. The main problem was to find a mechanism to sustain the electric currents for billions of years. The decisive step was the discovery that there could and should be a toroidal field inside the earth. Walter spent a lengthy period making calculations on the basis of different models and found that only the dynamo model gave an adequate numerical magnitude. Reassuringly for the dynamo model, the calculations came out just right. He then adapted a two-step strategy to resolve the problem. The first step was to assume a constant main magnetic field and calculate how it would be modified by eddy-type motions in the core. Then he constructed the mathematical machinery that could describe the field as it was generated by currents inside the core. Using his meteorological knowledge about the dynamics of the atmosphere, he derived rules about the character of the fluid motions needed to produce the field. He thus showed how a metallic fluid dynamo can be self-sustaining indefinitely as a result of ordinary electromagnetic induction. This involved mathematical demonstration of how the kinetic energy of fluid motion in the earth's core can be converted to electromagnetic energy. The key element is a feedback process in which energy is exchanged between two components of the magnetic field, that extending to the earth's surface and beyond (the poloidal field) and that contained within the core (the toroidal field). The convective motion of the fluid is the agent in the exchange process.

Walter was greatly aided in this work by magnetic maps consisting of lines of constant rates of secular or long-term variation, prepared under the direction of E.H.Vestine. These closely resembled ordinary weather maps. From these maps Walter calculated the average speed of fluid iron in the core, which was 0.03 cm/second. He also derived an important formula for predicting the strength of magnetic fields in celestial objects. By balancing Coriolis and Lorentz forces acting on the conducting fluid, he concluded that the earth's dynamo is characterized by a particular value of a dimensionless parameter that includes the magnetic field strength, the electrical conductivity of the core, and the rotation rate of the earth. This parameter is now called the Elsasser number and is the critical parameter in modern dynamo theories.

After the war, Walter renewed his acquaintance with John von Neumann, then at the Institute for Advanced Study in Princeton, who had gotten interested in the problems of magnetohydrodynamics during his work at Los Alamos. Von Neumann listened with interest but, like everyone else, refused to believe that magnetic fields could be created by fluid motions alone. Over many visits in the course of a year Walter convinced him that his arguments were mathematically unassailable. Stimulated by remarks of von Neumann, a distinguished British hydrodynamicist, K.G. Batchelor, carried out calculations showing that when an electrically conducting fluid is in random turbulent motion, a random stray magnetic field that happens to exist in the field will always be amplified by random shears engendered by the turbulence. When Batchelor's work appeared in 1950, the objections to dynamo models disappeared, and in a few years the dynamo idea was fully accepted, as indicated by Walter's election to membership in the National Academy of Sciences in 1957. This gave him a strong sense of personal achievement since his was mainly an individual accomplishment. It is perhaps of more than passing interest that this work was done in his spare time and had no direct connection to his war work. A similar pattern will be seen in his later biological work.

When the concept of plate tectonics revolutionized the geological sciences in the 1960s, Elsasser turned his attention to the question of the driving force for plate motions. One of his prime contributions during this period was an analysis of how stress diffuses across tectonic plates. This work explained the phenomenon of postseismic deformation observed in seismically active plate boundary regions such as the San Andreas fault zone following large earthquakes. As in other areas of physical science, Walter's central contributions to plate tectonics continue to influence modern thinking to this day.

WORLD WAR II AND THE POSTWAR YEARS

While Walter was working at the Blue Hill Observatory and before he was ordered to Fort Monmouth, he completed his monograph on infrared atmospheric radiation. When he received the orders to move, Charles Brooks, director of the observatory, offered to read the proofs, so Walter, his wife, and six-month-old daughter could depart for Fort Monmouth. There he was given a civil service classification as a meteorologist and a salary corresponding to his age, education, and experience, which lifted him to a comfortable level considerably higher than the marginal one he had before. He soon found, however, that the Signal Corps no longer had much interest in meteorology, since the subject had been taken over by the Air Corps. He was told he was to become an electronics specialist, thus ending his career as a meteorologist. Walter protested, mainly against the military way of doing things, but was told this was war and he had to obey orders.

He was assigned to the quartz crystal division, one of the largest and busiest branches of the Signal Corps Laboratories. He found himself in charge of a small group of younger people. Individualist that he was he did not relish the bureaucratic functions the job entailed. This resulted in an awkward situation one day when he was surprised while holding a soldering iron in his hand by an inspector from the Civil Service who severely rebuked him for stooping to such manual efforts, when the government could hire people to do it for half what he was being paid. Nonetheless, he rapidly learned electronics in this work and carried out technical supervision and inspection of industries that built quartz crystals into radio transmitters.

About a year later, an emergency arose that required the help of men versed in meteorology. There had been trouble with anomalous propagation, in which radio waves are bent by moisture in the air. This interferes with the ability of radar to measure distances of targets and causes artillery that is guided by radar to be directed to a wrong distance. Walter wrestled with the problem for some time without success, when he was told it was to be transferred to the Radio Wave Propagation Committee of the National Defense Research Committee. He was then released to serve with the committee, which had set up an office in one of the upper floors of the Empire State Building in New York City. He spent the rest of the war there with a half dozen other technical people. During this time he wrote a simply worded pamphlet for technical personnel explaining the meteorological origins of anomalous propagation, how to recognize it, and how to make the best use of radar sets that one could under such conditions. It was illustrated by a very capable artist, was printed in tens of thousands of copies, and was distributed to the technical branches of the Allied armed services. This constituted Walter's basic contribution to the war effort.

After the end of the war, his office was disbanded. He had lost his connections with atomic physicists many years before and had no desire to return to meteorology. Through a contact he had made on the Radio Wave Propagation Committee, Walter obtained a position in the RCA laboratories at Princeton testing antennas of new design. Since this did not appeal to him, he managed to get himself transferred to a section supervised by Dwight O.North, a theoretical physicist. The group was studying the properties of solids used in electronic solid-state devices. Walter found a very congenial group among his collaborators but decided it was too late in life for him to switch to industry.

After two years at RCA, he found a position as associate professor of physics at the University of Pennsylvania in Philadelphia. He taught a graduate course in mechanics that later became his mainstay in teaching. He wrote a review for physicists, “The Interior of the Earth and Geomagnetism,” in 1950 that made him known to his American colleagues. He had been warned by friends that there was an ingrained political constellation in Philadelphia that would make an extended stay unpleasant. He found this to be true, so when he received an offer from the University of Utah’s physics department to join the faculty and develop a graduate program he was definitely interested. Oppenheimer, who was then director of the Institute for Advanced Study in Princeton, told him that the University of Utah had a very high scholastic level judging from the excellent students he got from there when he was still in Berkeley. Considering Oppenheimer’s critical acumen, this seemed a very high recommendation and Walter accepted the offer in 1950. At Utah he gradually wound up teaching a comprehensive set of courses in theoretical physics, with the exception of quantum mechanics, which was taught by the most distinguished member of the faculty, the theoretical chemist Henry Eyring.

In an earlier discussion with John von Neumann, Walter had expressed the desire for a small inexpensive computer. He was told that the first such machine had just come out. The Air Force wanted such machines tested by competent academic people, so they bought one and loaned it to him in 1951, indifferent about the scientific problem he proposed to solve. Walter became intimately familiar with the design of the machine since it had been hastily constructed and the diodes frequently burned out and had to be re placed. As a result he was unable to carry out any lengthy calculation, but he did teach himself Boolean algebra, the formal logic of computing machines. This experience with early electronic computers gave Walter a sense of their capacities and limits, which was to prove useful in comparing them with living organisms. He gained first-hand knowledge of what was called cybernetics, now called systems theory, and he gave some thought to using the computer as a model of the brain. He decided to write a book in which the first two chapters were a survey of the basic ideas and techniques of cybernetics, followed by more philosophical notions. This book, *The Physical Foundation of Biology* (1958), was the first of four books he wrote in biology.

In 1955 after he wrote the first draft of the book, Walter decided to get an opinion about it from the university's biomedical community. When he asked several members of that community whom they recommended to read the book, they all named a young professor of biophysics who had an interest in theory. This man took it home and brought it back a few days later. He said, "I have read this. It is thought provoking, in fact extremely thought provoking—but so far as I am concerned, I do not think, I observe."

This was Walter's first encounter with a kind of mentality he found widespread among biologists who, living among the most gigantic accumulation of data the world had ever seen, proclaimed that salvation lay in more data. This was, so to speak, a shattering incident for Walter since it denied science as a creative activity. He had always considered scientific research in which observation was related by a reciprocal interaction with thought; observational results tended to modify thought, which in turn engendered suggestions for more observations until significant knowledge had come close to its boundaries. This dynamic process embodied the advance in understanding in physical science, but it seemed that in the life sciences the prevailing approach, the philosophy as it were, had assumed a quite different form. This discrepancy became increasingly clear in Walter's later years and sharpened his desire to pursue the philosophical interpretation of biological matters.

Walter decided early during his tenure in Salt Lake City that he had risen as high as he could in an academic career short of moving to a more prestigious university. To do so would require that he specialize in only one of the several scientific fields in which he had become competent, in order to remain on top in one of the fields. Instead, he decided to remain a generalist or, as he preferred to call it, a "natural philosopher." This decision was reinforced by the tragic loss in 1954 of his beloved wife Margaret. Since he did not want to become a purely speculative philosopher, he invented a technique in which he divided his time into periods of several weeks to several months in which he concentrated on only one of the subjects in which he was interested. He found periods of this size long enough to permit strong concentration and continued the scheme for the rest of his life.

The loss of his wife left Walter alone with their children. He had developed cordial relations with his neighbors, who were mainly Mormons, and his children felt at home in the neighborhood. He had become fond of the Mormons with their simple way of life combined with their appreciation of higher education, but he did not want to see himself or his children dissolved in the Mormon collectivity. Therefore, he accepted an offer from Roger Revelle to become the first professor at the newly developing University of California at San Diego, which was actually located in La Jolla.

Not long after he arrived in 1956 at La Jolla, the university committed itself to develop the San Diego branch into a major center of graduate study equivalent to that of the

Berkeley and Los Angeles campuses. A building to accommodate the nascent physics department was completed, and a number of highly competent young physicists were recruited. Walter felt that these men were completely steeped in technology and did not share his philosophical interests. He decided he did not fit into such a department, and Revelle created a niche for him at the Scripps Institution of Oceanography to do research in geophysics.

However, about this time Walter heard from Harry Hess, chairman of the Geology Department at Princeton, with an offer of a professorship in geophysics. Hess was the individual among geologists who above all took Alfred Wegener's theory of continental drift seriously. Walter's interest in geodynamics therefore fit with Hess's interest. The Geology Department was in one wing of a building built early in the century when evolution excited the academic world, with much of the research centered on paleontology. The Geology Department was in one wing of the building and biology in the other, and the libraries for both were in one large common room in the center. These seemed ideal conditions for Walter to continue his dual existence in geophysics and the philosophy of biology, and he accepted the offer in 1962.

Moving to Princeton was relatively easy for Walter since his children were now grown. Two years after his arrival he married again, this time to a cousin, Susanne, whom he had known from childhood. Although he met several distinguished biologists during his five years at Princeton, he found that his philosophical probings into the foundations of biology made them uncomfortable, as might be expected for one who aims to solve a specific question and does not want to be distracted from it. He concluded that biology had always been like what physics had become only late in its existence, during his lifetime. It was only by stripping off the practical implications and complexities that physicists as natural philosophers could develop the methods that led to the major experimental discoveries and the great unifying mathematical schemes that constitute the grand edifice of modern physical science. The situation in biology was far more difficult. There was no grand edifice, but there was evidence of a general unity of pattern in organic nature. Walter felt that the situation in our age is uniquely propitious for developing a basic theory of organisms because there was, for the first time, an altogether coherent abstract scheme, quantum mechanics, for representing the physical basis of life. It needed only the assumption that biology is the realm of the utterly complex to realize that the simple black and white world of mathematics might no longer apply, and the whole conceptual system of scientific analysis might have to be reconstructed. This task was to occupy most of the rest of Walter's life. In 1967 he left Princeton for a research professorship at the University of Maryland, which relieved him of all teaching obligations. While he lost the prestige of Princeton, he gained the freedom to do whatever research he pleased. In the summer of 1974, having reached his seventieth birthday, he was duly retired from the University of Maryland. A few months later he received an offer of a suitable postretirement affiliation as adjunct professor in the Department of Earth and Planetary Sciences at Johns Hopkins University, which entailed no formal obligations. There he wrote the memoirs that form the backbone of this article up to this point.

BIOLOGICAL WORK

Walter's specific interest in biology as a scientific discipline began in Paris in the early 1930s as a result of his discussions with Theophile Kahn, but his particular approach to the problems of biology grew out of his earlier experi ence with depth psychology in Frankfurt. He felt there was a fundamental distinction between the living and nonliving states, but it had to be one that did not violate quantum mechanics. Von Neumann's 1932 book *Mathematical Foundations of Quantum Mechanics* demonstrated that all ensemble averages of physical quantities obey differential equations of a simple kind; that is, their change in time is causally determined. Since Walter was convinced that a distinguishing characteristic of organisms was that their long-term behavior was not causally determined, he set himself the task of finding a way around von Neumann's completeness proof and found it in the concept that the members of any biological class are heterogeneous; that is, they share some but not all characteristics, while the members within any physical class such as electrons, photons, atoms, and molecules are rigorously identical to one another. The heterogeneity of biological classes arises from the unfathomable complexity of living things. His aim was to construct a formal scientific logic that is suitable for organisms. His first publication explicitly in biology was in 1951, although he lists a 1937 paper on quantum measurements and generalized complementarity among his biological publications. Altogether there were about thirty published papers in biology through 1984 plus at least as many drafts of papers extending through 1989, which did not get published but are in the Elsasser collection in the Milton Eisenhower Library at Johns Hopkins. Seven of the latter were written in 1988 and 1989, indicative of his continued interest and activity in the area. Walter also wrote four books on theoretical biology, the first appearing in 1958 and the last in 1987.

I first became aware of Walter's biological thought in the early 1960s through his first book and his articles in the *Journal of Theoretical Biology*. The book, *The Physical Foundation of Biology,* was heavy going for an experimental biolo gist, for the first half dealt with unfamiliar physical feedback control systems and information theory while the second half was more abstract and philosophical than most biologists are accustomed to. Some twenty years went by during which I became increasingly aware from my daily work that conventional ideas of causality were inadequate to deal with the often unpredictable behavior of cells in culture. One day in 1981 upon reading Volume 1 of Leslie Foulds's classic book on cancer, *Neoplastic Development,* I came upon a brief account of Walter's biological thought that seemed to anticipate the types of problem I was encountering. Rather than go back to his earlier papers, I wrote a letter to Walter asking about further development of his concepts, half expecting my request to be consigned to the wastebasket. To my delight he answered promptly in a friendly manner that seemed to invite further correspondence. That began an intense intellectual exchange that lasted through the 1980s and grew to some 500 pages. As it began, Walter was writing a draft of his last book, *Reflections on a Theory of Organisms* (1987), which he sent me in stages. The manuscript went through many revisions and several privately printed editions, paid for out of Walter's pocket before he found a one-woman publishing company in Canada to bring the book out. Through all this time he was unbelievably generous with his time, writing extensive expositions of his biological thought and general philosophy, often in response to naive questions of a pragmatically trained biologist.

While at Princeton Walter wrote a second book, *Atom and Organism* (1965). After it appeared, he decided he had fallen under the sway of the establishment. This made him feel as though he had been flying with clipped wings, a situation he was determined to correct. The third book, *The Chief Abstractions of Biology* (1975), was written while he was at the University of Maryland, where he was free to do whatever research he chose. It formalizes many of the thoughts of the earlier books in a few abstract concepts. The fourth and final book is a distillation and refinement of the concepts formulated in the previous books. The following summarizes some of the main ideas expressed in his biological *oeuvre*.

All of Walter's biological writings refer to the immense complexity of the organism based on the number and types of atoms in a cell and the number of possible bonds connecting the atoms in organic molecules. Complexity is taken as an intrinsic aspect of the living state. Another important consideration is the near reversibility of most biochemical reactions as expressed in a well-known textbook by Conant and Blatt.[1] "Biochemical reactions liberate or absorb small amounts of energy…. Apparently the necessity for reversible reactions with relatively small energy changes is characteristic of biochemical reactions." Walter recognized that the heat of biochemical reactions is close to that of thermal noise and therefore almost the direct opposite in its properties for maintaining and transmitting information from those of computing machinery, which maximizes the difference between signal and noise. The input by noise in biology is therefore removed from empirical control, and adequate results can no longer be obtained by a purely mechanistic model. The closeness of energy exchange in biochemical reactions to thermal noise is necessary for the decision-making ability of the organism, allowing it to choose between available states without need for more than a minimal supply of energy. This condition contributes to what Walter called the fragility of the living state, defined as the capacity of the system to respond with large-amplitude changes to small perturbations. In that sense fragility may also be characteristic of the processes involved in development and differentiation of the organism. At the same time, the maintenance of information is so powerful that many species do not change their species-specific characteristics for millions of generations.

These thoughts led Walter to formulate a holistic set of principles to represent the living state. These principles are not scientific laws in the usual sense since they are not derivable from the mathematics of quantum mechanics. They define that which is in the form of regularities but not determined by atomic and molecular physics. The basic assumption in his holistic interpretation is that "an organism [or a cell] is a source (or sometimes a sink) of causal chains which cannot be traced beyond a terminal point because they are lost in the unfathomable complexity of the organism [or cell]." The basic principles of organisms as listed in his 1987 book are the following:

1. **The first principle is ordered heterogeneity.** Combinatorial analysis shows that the number of structural arrangements of atoms in a cell is immense; that is, much greater that 10^{100}, a number that is itself much larger than the number of elementary particles in the universe (10^{80}). But biology shows us there is regularity in the large where there is heterogeneity in the small, hence order above heterogeneity. This concept of ordered heterogeneity was first introduced by the molecular biologist Rollin Hotchkiss, systematized by the embryologist Paul Weiss, but given quantitative definition and set in a general theory by Walter.
2. **The second principle is creative selection.** A choice is made in nature among the immense number of possible patterns inferred in the first principle. The availability of such a choice is considered the basic and irreplaceable criterion of holistic or nonmechanistic biology. The term "creative" refers to phenomena that, like everything in biology, are compatible with the laws of physics but are not uniquely

determined by them. No mechanism can be specified by whose operation those selected differ from those not selected. He points out that the number of different patterns is also immense in the physical science of statistical mechanics, but in that case the variation of structure from pattern to pattern averages out. The patterns of inorganic systems repeat themselves over and over again ad infinitum, while those of each organism are unique. The selection of a relatively small number of organisms from the immense number of possibilities allowed by quantum mechanics is a primary expression of biological order and is the scientific counterpart of the term "creativity" used in ordinary language.

3. **The third principle is holistic memory.** It provides the criterion for choice not expressed in the second principle. That criterion is information stability. The term "memory" in a generalized sense indicates stability of information in time or, as in the case of heredity, the reproduction of information in an empirical sense, that is, without our knowing the full mechanism of reproduction. The creative selection of the second principle means the organism has many more states to choose from than are actually needed. The third principle says the organism uses this freedom to create a pattern that resembles earlier patterns. Walter borrowed the term "memory without storage" from the philosopher Henri Bergson, who was considering the memory function of the brain in his book *Matter and Memory*. Walter considered holistic memory an epistemological innovation that was the touchstone of his theoretical scheme but realized that it might seem like black magic to many of his readers. However, he noted that the concept is free from internal contradiction while it obviously runs counter to habitual thought. In that formal sense it is no different from the concept of the antipodes, which would have been

inconceivable before Newton since the people in Australia should have fallen off the earth. Memory without storage is considered as transmission of morphological features through time without a material memory device, just as relativity is based on the transmission of signals through space without a material carrier.

4. Holistic memory requires a **fourth principle, operative symbolism,** to indicate that a material carrier of information is needed, namely DNA, but this acts as a releaser or operative symbol for the capacity of the whole organism to reconstruct a complete message that characterizes the adult of the next generation. Walter was sketchy and superficial about the fourth principle and considered it in the nature of a specific detail. In other words, operative symbolism is not necessary to the development of the postulational system of the first three principles that can do away with the conceptual difficulties and internal contradictions that always appear in any purely mechanistic interpretation of organic life. The informational system of organisms is therefore postulated to be dualistic; on one level it is mechanistic in the operation of the genetic code; on the other level it is holistic, involving the entire cell or organism.

Walter's epistemological revision for the life sciences has been ignored by most biologists and attacked by some. The cool and sometimes downright hostile response of the biologists is probably related to the challenge presented to the basic preconceptions, often subconscious, that underlie their present modus operandi. The most pervasive of these preconceptions is that biology is ultimately an extension of physics and chemistry and can be studied in an analogous manner. Walter's theoretical innovations require a novel experimental approach that is just beginning to take shape to deal with the holistic aspects of cell and organismic be havior. Despite the difficulties, his thought has found strong support from a few outstanding biologists such as Leslie Foulds and Paul Weiss. It has also met with approval from some notables among theoretical physicists, including Pauli and Wigner, and from the information theorist L.Brillouin. Perhaps his strongest support has come from Frederick Seitz, a student of Wigner's in the early 1930s and a founder of modern solid-state physics. Seitz spent a decade as president of the Rockefeller University, where he was in continuous contact with many of the most creatively active individuals in molecular and cell biology and was impressed with their ingenuity. However, he was struck by the comparative rigidity of their molecular concepts and their enormous confidence (or overconfidence) that reductionism would lead to an understanding of all aspects of living systems. Flying in the face of these attitudes was the fact that the picture of such systems that was evolving at the molecular level was becoming ever more complex with each new major phase of development. Seitz felt that the outlook of the molecular biologists was somewhat reminiscent of the attitude of some nineteenth-century physicists who believed that the universe was a gigantic clockwork governed by the laws of classical physics. Ironically, Seitz's own work provides the theoretical foundation for the currently fashionable field of structural biology. While musing on the situation in biology he came upon Walter's work, which he considered a "profound analysis of the status of biological systems in the physical world." He felt that the biological community had "to a substantial degree lost sight of the forest for the trees and presumably will continue to do so until it is forced to reexamine its own foundations either through the appearance of obvious paradoxes or because it becomes enmeshed in unresolvable complexity—or both." When that time comes, he is "certain that the profoundness of Walter's work will be appreciated and will form a significant part of the cornerstone of understanding of living systems by the biological community."

Walter's work has already formed the cornerstone of my own understanding of living systems through its effect on my day-to-day work with cells in culture. A major feature of the behavior of cells dissociated from the organism and from one another is their radical heterogeneity in a large variety of behavioral and physico-chemical properties. This was anticipated in Walter's principle of ordered heterogeneity but appeared experimentally at the cellular level rather than the molecular level which most concerned him. Another feature of these cells in Walter's terms is their fragility, so they change their growth behavior in a striking and enduring fashion in response to small physiological differences in their environment. These responses are foreshadowed in Walter's principle of creative selection, which I modified to progressive state selection to image cellular behavior. Paradoxically, the behavior of some cells, depending on their initial state, is extremely stable, so that both fragility and stability are subsumed in the same system, as full consideration of Walter's theory would suggest. This goes along with his insight that there are no "yes/no" or purely arithmetic answers in the behavior of living systems. All depends on the initial state of the cells and the perturbations to which they are subjected. On a personal note, his philosophical analysis liberated me from the reductionist strictures that dominate biological thought and allowed me to acknowledge and organize the actual behavior of cells as seen every day before my own eyes rather than sweep the frequently inconvenient behavior under the rug. There is no doubt in my mind that Walter was correct in the evaluation he left with his own collected papers in the Johns Hopkins library that, although he was best known for his work in geophysics, his controversial ideas in theoretical biology were what historians would want to study. I believe his ideas will play a central role in the future development of biology.

WALTER ELSASSER'S *Memoirs of a Physicist in the Atomic Age* was the major source of information used here in describing his life up to 1974. His sister, Maria Lindberg, and Eugene Parker of the University of Chicago provided some personal insights. Peter Olson of Johns Hopkins provided a description of Walter's work on geomagnetism and plate tectonics. Frederick Seitz, formerly president of Rockefeller University, contributed his thoughts on Walter's biological work. Most of the section on that work was derived from Walter's published biological writings and from his extensive correspondence with me between 1981 and 1991. My wife, Dorothy Rubin, helped in every phase of preparing this memoir.

NOTE

1. *The Chemistry of Organic Compounds,* 2nd ed., Ch. 20. New York: MacMillan, 1947.

HONORS

1932	Research Prize of the German Physical Society
1957	Member, National Academy of Sciences
1971	John A.Fleming Medal, American Geophysical Union
1972	Fellow, American Academy of Arts and Sciences
1977	Gauss Medal, Braunschweig, Germany, (200th Anniversary of Gauss's birth)
1979	Penrose Medal (USA)
1987	National Medal of Science

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EARNEST ALBERT HOOTON

November 20, 1887–May 3, 1954

BY STANLEY M.GARN AND EUGENE GILES

VER FOUR DECADES Earnest Albert Hooton became known nationally and internationally for his contributions to the study of human evolution, for his comprehensive comparisons of nonhuman primates, and for his management of mass-scale anthropometric studies both of skeletal populations and on the living. He also became well known to a generation of newspaper readers for his pithy and often irreverent comments on the human condition and for his advocacy of a woman president. As an early exponent of applied physical anthropology and human engineering, Hooton was responsible for improvements in clothing sizing, work space, and air frame and seating design. For years Earnest Hooton was the principal source of graduate students in physical anthropology and, through his students, was responsible for much of the growth and direction of the American Association of Physical Anthropologists.

EARLY LIFE AND EDUCATION

Earnest Albert Hooton was born in Clemansville, Wisconsin, on November 20, 1887, the third child and only son of an English-born Methodist minister married to a Canadian-born woman of Scotch-Irish ancestry. Both parents emphasized learning and made sure that all three Hooton children went to college, despite the meager salary accorded a clergyman. Besides, Hooton's small size and myopia made him a scholar from the start—"with my nose always stuck in a book." Hooton also demonstrated an ability for cartooning at an early age, and he enlivened both his high school and college annuals with cartoons and more serious artwork, a skill he maintained for the rest of his sixty-six years.

Earnest Hooton graduated from Lawrence College at the age of nineteen and went on to the University of Wisconsin, where he attained his Ph.D. degree in the classics, having great proficiency in Latin and more skills in ancient Greek. His 1911 Ph.D. thesis was titled "The Evolution of Literary Art in Pre-hellenic Rome." With this educational background and his outstanding academic record, he applied for and was awarded a Rhodes scholarship, electing to study at Oxford. There he moved in succession from classical archeology to iron-age and Viking-period archeology, assisting in the excavation of Viking boat burials and description of the remains. At Oxford, under R.R.Marett, Hooton turned to anthropology, taking a diploma in general anthropology in 1912. He then worked with Arthur Keith, where he developed a lifelong interest in human paleontology, especially paleoanthropic fossils from England and the continent.

With Marett's strong support, Hooton was offered a teaching position at Harvard in 1913, and he remained there for four decades. Besides teaching introductory physical anthropology and iron-age archeology, he busied himself with descriptive analyses of skeletal remains, writing many addenda or technical notes to archeological reports and lecturing to alumni and professional groups on the relevance of physical anthropology to medicine and dentistry.

Though disqualified from military service because of his nearsightedness, requiring six diopters of correction, he volunteered for training at the Civilian Military Training Center at Plattsburgh, New York, becoming a passable rifleman at 100 yards but a wild shot at greater distances. Hooton also became involved in revising military recruitment standards, a necessity given the large number of smallish immigrants who could not qualify for service under the existing dimensional requirements standards.

A RECORD OF RESEARCH

During the 1920s, Hooton moved on from his earlier descriptions of individual skeletal remains found in the course of archeological digs and isolated fossil crania (like the La Quina skull) to metric and morphological analyses of large skeletal assemblages, including the remains of the ancient inhabitants of the Canary Islands, originally collected in 1915. Studies on the remains from Pecos Pueblo, comprising over 500 individuals of all ages, marked a turning point in human skeletal biology, for the sample was large enough to allow attention to age changes in this prehistoric skeletal population, as well as a careful and detailed description of such pathological conditions as osteoarthritis and rheumatoid arthritis, accomplished in conjunction with radiologists and pathologists.

Chapter X of the Pecos report (Pathology) included a detailed analysis of the age incidence and population prevalence of antemortem fractures (some 7 percent overall), with the highest age incidence in the elderly. The Pecos report also included appendixes on the dentition (by Habib J.Rihan) and a separate chapter on the pelvis (by Edward Reynolds). The entire study was facilitated by a sizeable group of devoted laboratory and statistical assistants, including Ruth O.Sawtell, who later wrote a series of popular detective stories featuring human bones and skeletal identification.

During the 1930s, Hooton turned his attention to anthropometric surveys and anthropometric studies of living human beings, including a very large series of criminals measured in ten different states, and years later, an anthropometric survey of the Irish. Such studies represented a major management task, keeping track of workers at distant locations, a major accomplishment in data handling (thousands of completed anthropometric and observational forms), and a major accomplishment in data analysis, made possible by the use of IBM punched cards and the Hollerith card sorter.

Though his criminal study (published as *The American Criminal* in 1939) was criticized as Lombroso-like in assuming the existence of criminal types, Hooton did demonstrate that different classes of felons differed substantially in body size and proportions, pickpockets being the smallest and forgers being the tallest and best educated. Self-selection and occupational selection clearly accounted for such dimensional and proportional differences, as we have since come to know also for different groups of Olympic athletes.

Hooton also operated an anthropometric booth at the New York World's Fair, gathering novel dimensional data on the visitors, and he was involved in annual anthropometric studies on Harvard freshmen, extending investigations originally initiated by Dudley Sargent at the turn of the century.

MILITARY AND CIVILIAN APPLICATIONS

In the course of his anthropometric studies, Hooton developed a model for mass surveys and for data analysis using punched cards and card-sorting equipment located in his statistical laboratory atop the Peabody Museum. This model proved especially applicable to mass data surveys relating to equipment design, both civilian and military, which Hooton helped organize and provided direction. As a result, many of Hooton's students became involved in applied human engineering with the Air Force (previously the Army Air Corps), the Chemical Warfare Service (later the Army Chemical Corps), and the Quartermaster Corps, among others.

Gas masks, oxygen masks, aircraft seating, tank interiors, military uniforms, G-suits, and tank helmets all became more comfortable, better-fitting, and more user friendly because of Hooton's efforts and directions. It was his notion that equipment and garments should fit the user, rather than vice versa, and Hooton was a proponent of ergonomics long before the term was coined by Le Gros Clark. Many of the national and international nutrition surveys conducted well after the midcentury mark also reflect Hooton's designs and contributions, through the efforts of his students of an earlier period.

Hooton also conducted an anthropometric study of commuters in Boston's North Station in order to develop more comfortable train seats for the Heywood Wakefield Company, as described in *A Survey of Seating* (1945). (Hooton's principal assistant in that study later became the director of the Kinsey Institute.) From such endeavors Hooton was able to provide alternative employment for many of his students, at Wright-Patterson Air Force Base, for example, and at the Quartermaster Laboratories in Natick, so that academia was no longer the only source of jobs for physical anthropologists.

OTHER LITERARY CONTRIBUTIONS

Besides technical monographs and book-length research reports (one over a thousand pages in length), Hooton also wrote several introductory texts that were widely used and lasted through multiple revisions. *Up from the Ape* (1931, 1946) was his best-known work, covering the scope and range of physical anthropology and providing detailed, illustrated instructions on anthropometry. *Man's Poor Relations* (1942) was the first comprehensive treatise on primates, primate taxonomy, and primate behavior. Their titles were sufficiently catchy to attract a wide and appreciative audience, and they were written in a friendly expository style so that students found them pleasant reading despite the wealth of technical material and the polysyllabic Greco-Latin names bestowed on individual fossils and primate genera and species.

Hooton also extended his writing to popularized accounts of his own contributions (such as *Crime and the Man*), and he was called upon to write a popular description of the Grant study at Harvard University. Since the study was dedicated to a biobehavioral understanding of normal college undergraduates, Hooton titled that popular work *Young Man, You Are Normal*.

Hooton also penned doggerel that has been likened to the work of Ogden Nash. Some of these verses were included in his scholarly texts, some found their way into his popular works, and others were used to enliven his classroom lectures and the lectures he was invited to give at conventions and conferences. His *Ode to a Dental Hygienist* was especially well received by dentists, who frequently invited Hooton to serve as a dinner speaker. Some of Hooton's more notable verses have been reprinted in volumes of poetry, and a representative selection (with illustrations also by Hooton) was reprinted posthumously under the title *Subverse* (1961). Like Ogden Nash, Hooton made use of unorthodox and surprising rhyme combinations.

Hooton became an accomplished cartoonist in his high school and college days and returned to this skill in the second half of his academic career. Some cartoons enlivened his popular works, and a selection can be found in *Subverse,* including his rather hilarious drawing of a Harvard faculty meeting showing Conant at the dais and a back view of Hooton himself lounging in the front row.

HOOTON AND HIS PH.D. STUDENTS

For three decades, 1920–50 approximately, Earnest Albert Hooton was the major source of Ph.D. recipients in physical anthropology in the United States and indeed the world. This preeminence in the supply side stemmed, in equal parts, from Hooton's location in the Peabody Museum of Harvard University, from the laboratory and statistical facilities he built, from his inspiring teaching, and from his personality. The Department of Anthropology, in the Peabody Museum, was rich in archeological and ethnological holdings and had access to a remarkably complete research library, with long runs of scientific journals in many languages. The bone lab grew under Hooton and came to include extensive primate collections as well as collections of human skeletons from many parts of the world. Hooton also expanded his statistics laboratory, beginning at the time he participated in the Civilian Military Corps during World War I, and with continuing cooperation of the International Business Machines Corporation thereafter, thus providing a facility for data reduction and data analyses without parallel in the field.

Hooton excelled as a teacher, teaching all of the courses in physical anthropology himself until the postwar expansion of physical anthropology demanded additional course offerings. With continuing programs of research, with expeditions to staff, and (later) with commercial and military projects, he was able to provide work-related training and financial support at a time when fellowships were scarce and uncommon.

Though a shy man in public, Hooton had a warm relationship with his graduate students, according each in turn the feeling of being most favored. While many professors doled out bits of research as thesis topics along their own lines of interest, Hooton encouraged his graduate students to look wide in search of investigative problems and then provided advice and counsel in the course of data acquisition and thesis writing. As a result, Hooton's students were diverse in their interests, some excelling in primate comparisons; some concentrating on prehistoric and protohistoric skeletal remains and skulls; other working in population biology, demography, and the secular (generational) changes of Americans or immigrant populations; and some in human genetics and histology.

Besides hour-long student conferences of the formal sort, Hooton had regular afternoon teas (especially on Saturdays), which provided social interaction, good conversation, and the opportunity to meet visitors from around the world. Thus, along with jasmine tea and shortbread, Hooton's graduate students (and other graduate students in anthropology) became acquainted with a wider academic world. As one of his former students calculated, getting a Ph.D. degree with Hooton included twenty-three gallons of jasmine tea, sixteen pounds of Scotch shortbread, and a surprising variety of people.

Most of the doctoral-level students produced by Hooton went on to professional positions in physical anthropology, thereby changing the composition of the American Association of Physical Anthropologists, which had been largely made up of anatomists and clinicians at the time of its inception. As their numbers grew, and as they gained in academic status, Hooton's students came to dominate the

AAPA for decades, eventually providing all of the elected officers for years and the majority of the executive committee. Given this start and a long-term near-monopoly of graduate students, it is not surprising that many of Hooton's products were elected to the National Academy of Sciences, including Baker, Garn, Howells, Hulse, Shapiro, and Washburn.

HOOTON'S PLACE IN NATURE

It is difficult to evaluate Hooton or to rank him among his peers for he held a unique position in physical anthropology and was without parallel. Only Franz Boas at Columbia and Ales Hrdlicka at the Smithsonian had comparable stature and recognition in the scientific community.

Hooton's honors included membership in the National Academy of Sciences, the Viking Fund Medal in Anthropology (he was the second recipient), and an honorary degree from Lawrence College. He was one of the founding members of the American Association of Physical Anthropologists, serving as president from 1936 to 1938 and associate editor of the *American Journal of Physical Anthropology* from 1928 to 1942, working closely with Hrdlicka. Hooton was also much esteemed as a guest lecturer and dinner speaker at various professional conventions, including the NAACP. *Life* magazine devoted a six-page spread to him under the title "Hooton of Harvard" (Aug. 7, 1939, pp. 60–66).

Hooton was often quoted in daily newspapers and news magazines, for his pithy comments were highly quotable. That and some of the titles of his popular books (*Apes, Men and Morons, The Twilight of Man,* etc.) did not sit well with more conservative colleagues and publicity-averse members of the Harvard faculty, including Harvard president James Bryant Conant. Hooton's comments were much appreciated by generations of Harvard undergraduates, however, and his popular “Anthropology A” course was long well attended by premedical students, liberal arts majors, and socialites alike. Lectures, according to Hooton, “need not be the same as a sleeping pill.”

THE LAST YEARS

Though Hooton reached the official retirement age at Harvard after his sixty-fifth year, he was invited to return by a new and more favorable administration at Harvard and happily resumed teaching introductory courses that had decreased in enrollment. He was actively teaching “Anthropology 10” when he died unexpectedly of a vascular accident.

Shortly before his death, Earnest Hooton expressed a desire to visit England once again to renew his acquaintance with Sir Arthur Keith, his old mentor and friend and “hear his cheerful voice again.” This was an unusual decision on Hooton’s part, for he detested travel except to the annual meetings of the American Association of Physical Anthropologists, and his yearly treks to Pinehurst, North Carolina, to play golf.

Hooton was survived by his wife Mary Camp Hooton, whom he married in 1913, by two sons (Newton and Jay), one daughter (Emma Hooton Robbins) and two grandchildren. Though he had agreed to accept a doctor of letters degree at the University of Wisconsin-Madison, the award was made posthumously at the 1954 spring commencement. Thereafter, an Earnest Albert Hooton professorship was established at the University of Wisconsin-Milwaukee, and its first incumbent was, fittingly enough, a pupil of a pupil of Hooton’s.

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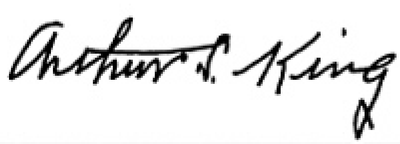

ARTHUR S.KING

January 18, 1876–April 17, 1957

BY ROBERT B.KING

RTHUR SCOTT KING was born on January 18, 1876, in Jerseyville, Illinois, and died in Pasadena, California, on April 17, 1957. He was elected to the National Academy of Sciences in 1940. His father, Robert Andrew King, born in Missouri in 1830, was a descendant of Scottish-Irish from Northern Ireland who settled in Virginia in the eighteenth century and moved to Washington County, Missouri, in 1817. Arthur's mother, Miriam Munson King, came from a New England family that had emigrated from Lincolnshire, England, in the seventeenth century. Robert Andrew King studied law and set up a successful practice in Union, Missouri. He also served in the Missouri state legislature. During the Civil War, he moved his family to Jerseyville, Illinois, where he served as circuit judge for southern Illinois, sitting in Jerseyville. Arthur's older brother, Louis, was born in Jerseyville.

Arthur was a rather frail boy and suffered from chronic asthma brought on, his parents believed, by the damp climate of southern Illinois. This was a major reason for the family's move to California in 1883. They purchased a small farm about five miles north of Santa Rosa, at the point where Mark West Creek emerges from the hills into the Santa Rosa plain.

Robert Andrew set up a law office in Santa Rosa, commuting by horse and buggy. The healthy California climate was effective, and Arthur was never again troubled with asthma; in fact, he was remarkably healthy the rest of his long life. Around 1890 the family moved to Fresno, where Arthur's father again set up an active law practice and Arthur attended the public schools. During the latter part of the nineteenth century, the great central valley of California was rapidly becoming the state's agricultural heart and Fresno the metropolis of the southern portion of the valley. A notable event in King's life here, and one that he liked to recall in later years, was a pack trip with a group of Fresno men and boys into the Kings River Canyon. At that time, this was undisturbed wild mountain country, now part of a national park. He attended the Fresno public schools, graduated from Fresno High School in 1895, and was admitted to the University of California (then only one campus) at Berkeley. During his undergraduate years at Berkeley, King became interested in science, especially physics, in which he must have done well because on graduation in 1899, he was admitted to the graduate school in physics.

At the beginning of the twentieth century, spectroscopy was becoming a major field of research in physics. No adequate theory yet existed to explain the lines observed with a spectrograph when elements were heated by flame, arc, or high-voltage spark. However, a great deal of data on measurement of wavelengths and estimated relative intensities of lines appearing in arc and spark spectra were being published. King was intrigued by this, and, fortunately, his research supervisor, Professor Percival Lewis, was interested in spectroscopy. King's first paper, published in the *Astrophysical Journal* in 1901, was concerned with the structure of the cyanogen bands (CN) as was his third paper (with Professor Lewis) in 1902. King's final examination for the Ph.D. degree took place on May 8, 1903. His thesis was titled "The Structure of Arc Spectra and Some Effects of Changes in Physical Conditions." He was awarded his doctorate, the first Ph.D. in physics given by the University of California, which since then has probably produced more Ph.D.s in physics than any other American university.

King was fortunate to receive a Whiting Fellowship for study abroad. The size of the fellowship is not known, but it was sufficient to enable him to spend two years in the physics laboratories of the universities of Bonn and Berlin, then the leaders in spectroscopic research, and to do considerable traveling in Europe as well. The majority of his time was spent in the laboratory of the leading spectroscopist of the time, Professor Heinrich Kayser, at Bonn. Kayser's *Handbuch der Spectroscopy,* continually revised, was the primary source of data on wavelengths, estimated intensities, and identification of the spectral lines of the elements. In Kayser's laboratory King designed and built his first electric furnace for excitation of the spectra of metals. It was clear to him that the furnace provided a new range of controlled excitation fitting with the existing sequence of spectroscopic sources: flames, arc, and spark, between the flame and the arc. His first paper on the electric furnace was published in the *Annalen der Physik* in 1905.

King's primary interests were in the changes in the spectra of the elements produced by different methods and degrees of excitation. At that time, excitation was pretty well limited to the classic arc, spark, and flame sources. Of course, these showed quite different sets of spectral lines, which led to the distinction between spectra of neutral and ionized atoms of the metals. Obviously, a source capable of producing spectra under conditions of thermal equilibrium with controllable temperatures would be highly desirable. King returned to Berkeley as an instructor in physics in 1905.

Two notable events in King's life occurred in 1906: one was a paper he published in the *Annalen der Physik* that described an electric furnace for spectroscopic purposes. This was the forerunner of the King furnace. The other was his marriage to Louise Burnett, the daughter of a Presbyterian minister and a descendent of early New England and New York families. The Kings had two sons, Robert, born in 1908, and Ralph, born in 1911.

In 1907 George E.Hale offered King a position on the staff of the recently established Mt. Wilson Observatory in Southern California. Hale, founder and first director of the observatory, was forming a staff of competent young men who were destined to dominate observational astronomy for many years. Hale was a strong believer in the need for an associated physical laboratory to provide data necessary for interpretation of astronomical observations, especially spectroscopic observations of the sun and stars for which the great telescopes located on Mt. Wilson were designed. Hale's remarkable enthusiasm was shared by the staff. He had the ability to pick good people, not only the scientific staff but the support people as well. He also felt that one of the main objectives of the observatory should be the undertaking of basic and extended research programs seldom undertaken by university researchers at that time because they depended on continued support over a long period of time and significant results were not expected to be forthcoming in a few months. This policy applied to both astronomical and physical laboratory programs. The large endowment (for the time) from the Carnegie Institution of Washington was able to provide continuing support for major projects such as the Mt. Wilson Observatory. At that time there was no National Science Foundation, and financial support of scientific research by the government came largely through private and state-supported universities.

A physical laboratory as part of the observatory was one of Hale's several innovative policies. He had already set up a small laboratory on the mountain that soon proved to be impractical largely due to power limitations. It was also Hale's idea that it would be best for the observatory staff to live in Pasadena rather than on Mt. Wilson. This also has proved to be a wise decision. Consequently, the laboratory was established in Pasadena in conjunction with the shops and offices of the staff. In 1908 Hale published an article in the *Astrophysical Journal* describing a new physical laboratory being constructed for the observatory in Pasadena. At the time it was probably the best-equipped physical laboratory west of Chicago.

In the same issue of the *Astrophysical Journal* containing Hale's article was a paper by King describing a newly designed electric vacuum furnace for spectroscopic observations. This was the furnace that became known as the King furnace. It remained basically unchanged during King's long series of spectroscopic observations extending over almost forty years.

The heating element of the furnace was an accurately machined graphite tube about one-half inch in diameter inside with an eighth-inch wall thickness. The tube was clamped between graphite blocks, which in turn were held tightly in water-cooled metal clamps. The whole assembly was enclosed in a vacuum chamber with quartz windows at each end. Voltages in five volt stages between 5 and 50 volts provided high currents through the tube. The current was controlled by a rheostat, while the observer could continually monitor the temperature inside the tube by means of an optical pyrometer. Temperatures up to 3000°C could be achieved, and almost any solid substance could be melted or vaporized and most of its molecules dissociated. The incandescent vapor, when focused on the slit of a spectrograph, produced emission lines, mainly those of the neutral atom. At high furnace temperatures, the molecular bands of C_2 and CN became prominent. Absorption spectra could be observed by placing a graphite plug in the center of the furnace tube or by focusing the beam of light from a source of continuous spectrum such as a tungsten filament at the center of the heated tube, then onto the slit of the spectrograph. The photographic plate holder was mounted on a horizontal track above floor level.

The Pasadena laboratory was equipped with two large electrograting spectrographs. One had a plane grating in a 30-foot-focus, Litrow-type mounting. The second used a concave grating of 15-feet focus in a Rowland-type mounting. Both were mounted vertically in a pit. This arrangement provided more free floor space and excellent temperature stability. Most of King's work was done with the concave grating spectrograph.

Ever since his European sojourn King had loved to travel. In 1914 he purchased a new Overland touring car, a powerful medium-size four-cylinder automobile, and began to explore the local country. Almost every Sunday he took his family for an afternoon ride: to the beaches, the mountains, or just through the orange groves to a nearby town. He was the first Mt. Wilson staff member to drive his family up Mt. Wilson. The old toll road, while well maintained, was only one car wide, except for frequent "turnouts' and for the inexperienced it was a real adventure. In 1915 King drove his family to Berkeley, where they spent most of the summer, and frequently visited the Pan American International Exposition in San Francisco. He also took them to Lake Tahoe and to Sequoia National Park.

World War I did not seriously disrupt the observatory's research programs, although King and Harold D.Babcock spent some time developing an acoustical submarine-detecting device that seems to have been a forerunner of the sophisticated and very effective detection devices developed during World War II. By 1918 the war had created a shortage of physics instructors at Berkeley, and King was asked to come and help teach. He obtained a leave of absence from the observatory and took his family with him to Berkeley for several months.

King lived most of his adult life during what this writer considers the golden age of Southern California, say, between 1900 and the beginning of World War II. The countryside, originally semidesert, was beautiful, with the alluvial slopes at the base of the mountains covered with citrus groves interspersed with attractive and prosperous small towns. The lower-lying portions of the river valleys were devoted to alfalfa fields and dairy and vegetable farms. Smog was not yet a problem, nor was overpopulation or traffic. In fact, Southern California was a delightful place to live and was appreciated by easterners and midwesterners, who moved there in large numbers. Industry was mainly engaged in services and construction. Manufacturing was small scale and located mainly in east Los Angeles. It was a favorable atmosphere for scientific research; the observatory thrived and its neighbor, the California Institute of Technology, began its development into a great research institution, due in large part to the efforts of George E.Hale.

Beginning in 1909 King developed his well-known temperature classification of spectral lines of the elements. His earliest publications indicate he had always been interested in the differences in the relative strengths of lines in a given spectrum with the degree of excitation. The graphite tube electric furnace made it possible to sort out lines that appeared at different known temperatures and strengths with increasing temperature and were therefore presumably associated with different energy levels in the atom. He thus proceeded to adopt an arbitrary scale of estimates of relative intensity of lines in a given spectrum and associate it with the temperature at which the lines appeared. The lines that appeared at the lowest temperatures usually below 2000° and strengthened rapidly with increasing temperature were designated Class I. Classes II, III, and IV designated lines appearing at increasingly higher temperatures. Class V included lines that first appeared at high temperatures but were much stronger in the arc spectrum. King's intensity estimates were meticulous and very consistent. These intensity estimates and the temperature classifications proved to be key ingredients in the later-term analyses of the complex spectra of the metals and rare earths. In nearly all of his observations over a period of almost forty years King used a concave grating spectrograph of 15-foot focus in a Rowland mounting in a vertical rather than the usual horizontal arrangement. The grating was located in a pit that ensured quite constant temperature, and the plate holder was mounted on a convenient horizontal track above floor level.

Because of Hale's interest in the magnetic splitting of the many lines observed in sunspot spectra, the laboratory acquired a large Weiss electromagnet capable of producing fields up to 30,000 gauss. King and others used this to record spark spectra of several metals and to measure the Zeeman splitting of many lines. In later years these data also proved useful and often conclusive evidence in the identification of the terms involved in transitions. However, of greater importance in the 1920s and 1930s to those involved in the term analysis of complex spectra were King's estimated intensities and temperature classifications of lines. They fur nished clues and often unique evidence about the ordering of the electronic energy levels in the atom. King continued this work throughout the 1920s and 1930s, in fact until his retirement. In the late 1920s King became interested in the spectra of the rare earth elements; accurate wavelength measurements had not been done on many of them. The main reason for this was the difficulty in obtaining pure samples of these elements. Usually samples contained mixtures of two or more rare earth elements, and consequently spectroscopic observations were discouraging.

King contacted a retired industrial chemist named McCoy who was making a hobby of purifying rare earths. McCoy's samples enabled King to make wavelength measurements, furnace arc, and spark intensity estimates as well as temperature classifications of their spectral lines. These data were basic to the term analysis of these very complex spectra. It is not too much to say that probably little would have been accomplished for a long time in the term analysis of rare earth spectra without King's data. King continued his observations of rare earth spectra until his retirement in 1943.

The graphite tube furnace also led to the discovery of an important isotope. The furnace at higher temperatures brought out the band spectra of the characteristic molecules CN and C_2 (CN because of residual air in the furnace enclosure). King had often recorded spectra of the CN bands and also the carbon bands called the Swan bands whose primary head lay in 4737 angstroms. This band had been identified as belonging to the diatomic molecule C_2. Furnace spectra at high temperature and long exposure showed a much fainter but almost identical band head at λ4744.5 with the resolved components degrading in intensity toward the violet as did those associated with the strong λ4737 band. The faint 4744.5 band had also been observed in the spectra of N-type stars, the co-called carbon stars. King had suspected for some time that these faint bands were probably due to the presence of the isotope C^{13} forming with the dominant isotope C^{12} the molecule $C^{12}C^{13}$. Since the presence and relative strength of the 4744.5 band was consistent in the furnace spectra at high temperature, the isotope was probably stable (unlike the later-discovered isotope C^{14}). The theory of diatomic molecular spectra or band spectra was still in the process of development, and King invited one of those who was engaged in this work, R.T. Birge of the University of California, to collaborate. They published a paper in *Nature* announcing the discovery of the carbon isotope of mass 13. The comprehensive paper by King and Birge was published in 1930 in the *Astrophysical Journal* The position of the band head and the wavelength of the secondary lines matched exactly those predicted by theory for the molecule $C^{12}C^{13}$. This identification also, of course, solved the mystery of the presence of the faint band head in spectra of carbon stars mentioned above.

During the 1930s King continued his observations of furnace spectra, particularly of the rare earths; in fact, he did so until his retirement. In 1936 he collaborated with his son, Robert, in developing a method using the graphite tube furnace for the quantitative measurement of the true relative strengths of lines in complex spectra. These data were desired by astronomers studying high-dispersion stellar and solar spectra. Thus, it can be said that the majority of King's work in the team was directed toward providing data for astrophysical applications; his wavelengths, line intensity, and temperature classifications were essential ingredients to the term analyses of complex spectra, which were necessary for interpretation of solar and stellar spectra.

In 1941 King served as president of the Astronomical Society of the Pacific, one of the oldest astronomical societ les. He also served a term as president of the American Meteorical Society in the 1930s.

King retired in 1943 and soon became involved with war work at CalTech, where he joined a project concerned with the underwater ballistics of aircraft torpedoes. He continued with this work for several years after the end of the war when the project was taken over by the U.S. Navy but remained in Pasadena.

Good health had been as asset to King since his early youth and permitted him to remain active long after retirement. However, in the mid-1950s he overextended himself on an automobile trip with his wife to Oregon and never really recovered his full strength. His health failed rapidly in 1957, and he passed away in his sleep on April 25th in Pasadena. He left his wife, two sons, and four granddaughters.

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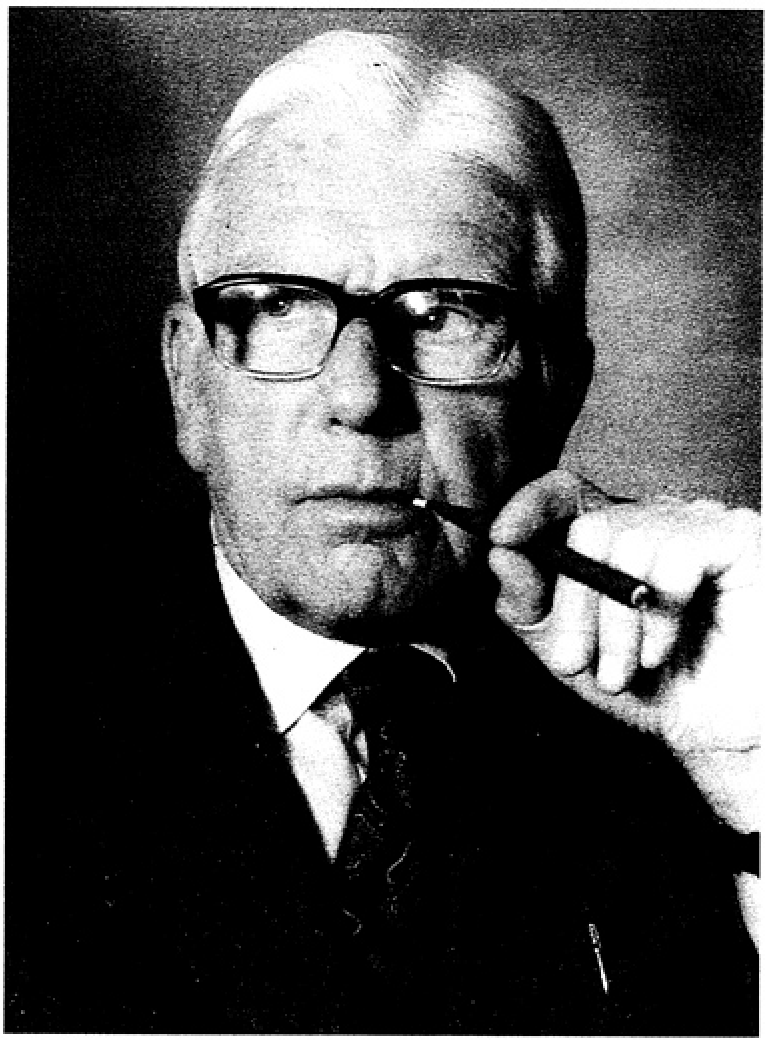

H. F. Mark

HERMAN FRANCIS MARK

May 3, 1895–April 6, 1992

BY HERBERT MORAWETZ

ERMAN MARK WAS WIDELY KNOWN as the father of polymer science and the contribution he made to his chosen field was crucial in many ways. His research and inspiring teaching and lecturing were only part of his activities. Being completely devoid of academic snobbery, Mark was equally at home at universities and industrial laboratories and was most influential in the phenomenal growth of the polymer industry. Deeply concerned with establishing the study of polymers as a discrete branch of chemistry, he designed the first graduate curricula in that discipline, founded a polymer journal and monograph series, and was one of the chief architects of the Polymer Section of the International Union of Pure and Applied Chemistry.

Because of the informality of his nature, Mark was affectionately called by the nickname “Geheimrat” to stress his extreme difference from the pompous professors who had been adorned with this “Secret Councillor” title. His constant cheerfulness reflected his enviable ability to recall the good part of his life’s experience while choosing to forget all the unpleasantness.

EARLY LIFE AND EDUCATION

Herman grew up in Vienna as the second son of Herman Carl Mark and Lily Mueller. His father was a physician, and Herman was early impressed with the conversation around the family dinner table with guests such as the psychoanalyst Sigmund Freud, the dramatist Arthur Schnitzler, and the founder of zionism Theodore Herzl. The musical life of the period, when Gustav Mahler was conductor of the Vienna Philharmonic, made a great impression. Mark was also enthusiastic about sports, particularly skiing and soccer, and on one occasion he was a member of the Austrian national soccer team.

This idyllic life came to a sudden end with the outbreak of the First World War. Mark fought on all fronts, was wounded several times, and was awarded fourteen medals for bravery. On one occasion, when the Italian army captured Monte Ortigora and the Austrians ordered a retreat, Mark persuaded his superior officer to allow him to lead a counterattack in which the strategic peak was retaken in spite of a heavy loss of life. During the final months of the war Mark was captured. He spent almost a year as a prisoner of war in a convent near Bari; his account of the humane treatment of the prisoners in those days reads today almost like a fairy tale. To relieve the boredom of prison life, Mark studied languages and resumed his study of chemistry, which he had started two years earlier while recuperating from a battle wound. In October 1919 he learned that his father was sick. He bribed a prison guard and took a train to Vienna.

He worked on a doctoral thesis dealing with the synthesis and characterization of the pentaphenyl ethyl free radical under the direction of Wilhelm Schlenk. Many years later, Mark recalled that Schlenk repeated his elemental analysis before he allowed him to write up the dissertation. Mark worshipped his thesis advisor and Schlenk was clearly most impressed with his student, since he invited Mark to come with him to the University of Berlin, where, in 1921, Schlenk was offered the chair previously occupied by Emil Fischer.

KAISER WILHELM INSTITUTE (1922–26)

As it turned out, the Schlenk-Mark collaboration did not last long. A year after Mark arrived in Berlin with his young wife, the former Mimi Schramek, Fritz Haber, director of the Kaiser Wilhelm Institute (KWI), asked Schlenk for a modern organic chemist for a new Institute of Fiber Research to be organized at KWI. Schlenk unhesitatingly recommended Mark. Thus, a period of almost unbelievable productivity started for Mark at what was then one of the leading scientific centers in the world. Michael Polanyi had recently joined KWI and under his inspired leadership a broad program of X-ray crystallographic studies was initiated.

Polanyi found that the X-ray diffraction from cellulose fibers indicated the presence of crystallites oriented in the direction of the fiber axis and that an analogous crystal orientation existed in metal wires. A full structure analysis of cellulose seemed beyond the experimental possibilities of the time, but Mark and Polanyi noted that the increase in the modulus of cellulose fibers on stretching seemed similar to the reinforcement of metal wires during cold-drawing. They embarked, therefore, on a detailed analysis of the changes accompanying the cold-drawing of a zinc wire.

It is interesting to list some of the large number of materials Mark selected for his later crystallographic studies. The determination of the hexamethylene tetramine structure in 1923 was one of the first for a comparatively complex or ganic molecule. The graphite structure (1924) later proved important in convincing Mark (in opposition to many crystallographers of that time) that covalently bonded structures may extend beyond the crystallographic unit cell. The oxalic acid study (1924) led him to conclude that "one may assume that the carboxyl group of one molecule may attract as well the OH of water as the hydroxyl of another oxalic acid molecule." In his study of calomel (1926) he related the birefringence to the crystal structure. The crystallographic study of carbon dioxide (1925, 1926) was aimed at the determination of the carbon-oxygen bond length. A comparison of the ethane and diborane crystal structure (1925) showed that the two molecules had similar geometries—a result that was then quite surprising, since the manner in which the trivalent boron could form B_2H_6 molecules was something of a mystery. (The discovery of the similarity of the ethane and diborane structures is generally credited to S.H.Bauer's 1937 electron diffraction study, ignoring Mark's earlier work.)

Considering that Mark was originally trained as an organic chemist, it is striking how much of his effort during his years at KWI was directed at problems in physics, such as the natural width, the refractive index and the polarization of X rays, the Stark effect, and the Compton effect.

An important milestone in Mark's career was the meeting of the Gesellschaft Deutscher Naturforscher und Ärzte (Society of German Natural Scientists and Physicians) held in Düsseldorf in 1926. At this meeting Herman Staudinger, the champion of the concept of long chain molecules, was confronted with some of Germany's most distinguished chemists who viewed this idea with utmost skepticism, insisting that all effects ascribed by Staudinger to macromolecules could be explained by a colloid association of small molecules. Mark's lecture was titled "Roentgenographic Deter mination of the Structure of Organic, especially Highmolecular Substances." It is important to note that the term "highmolecular" (hochmolekular) at that time carried no implication whether such substances were composed of Staudinger's macromolecules or of his opponents' colloidal aggregates. Mark reviewed his crystallographic work, stressing that important information may be gained from the unit cells and space groups, even if detailed molecular structures are not available. As for highmolecular substances, he was not yet ready to commit himself to long chains but suggested that the failure of such substances to break up on dissolution into small particles "indicates that lattice forces are quantitatively and qualitatively comparable to intramolecular forces: The entire crystallite behaves like a large molecule."

I.G.FARBENINDUSTRIE

In 1926 K.H.Meyer, a director of Germany's largest chemical corporation, the I.G.Farbenindustrie, invited Mark to become the director of a research laboratory of highmolecular compounds in Ludwigshafen. I.G. was a prominent producer of rayon and cellulose acetate fibers, and Mark was given full freedom to pursue fundamental studies as well as studies of spinning technology. His years at KWI had prepared him well for a consideration of the cellulose structure. As far back as 1921, Polanyi had pointed out that the unit cell of cellulose contains four glucose residues, but although he remarked that the diffraction pattern might be consistent with long chains composed of glucose residues, he attached no importance to that possibility. Mark made later the suggestive observation that the identity period in the fiber direction remained unchanged when cellulose was converted to its ethers or esters. Then, in 1926, Sponsler and Dore proposed in the United States a solu tion of the X-ray structure in which cellulose consisted of long chains of glucose residues, but this structure was inconsistent with chemical evidence that cellulose could be degraded to cellobiose. In a pioneering 1928 paper, Meyer and Mark solved the diffraction pattern to yield a structure in agreement with the chemical evidence—the first polymer crystal structure that has survived the test of time.

Another important investigation concerned Hevea rubber. While Mark was at KWI, one of his colleagues, J.R. Katz, made the surprising discovery that natural rubber, amorphous in the relaxed state, exhibits a sharp X-ray diffraction pattern when stretched. In his I.G. laboratory Mark followed up this discovery by solving with G.V.Susich the Hevea rubber crystal structure. This result was particularly important since it settled, for the first time, a question of *chemical* constitution: It had been known that natural rubber is poly (1,4-isoprene), but only the crystal structure proved that it had the *cis* configuration around the carbon-carbon double bond.

Mark was keenly interested in the relationship between the molecular characteristics of polymers and their technologically useful properties. This led him to calculate, on the basis of the cellulose crystal structure and the energy required to break its covalent bonds, estimated on the basis of spectroscopic data, the ultimate strength of an ideal cellulose fiber. In this approach he was far ahead of his time. He showed that the best industrial fiber was only about 10 percent as strong as the ideal fiber—clearly owing to various defects.

With Meyer's encouragement, Mark was free to pursue during his years in Ludwigshafen a number of his scientific interests, which were not likely to translate into financial profit for the company but which gained academic prestige for the laboratory. Thus, he carried out the first electron diffraction studies of gases, determining the bond length and bond angles for molecules such as carbon tetrachloride, germanium tetrachloride, benzene, cyclohexane, *cis*-1,2-dichloroethylene and *trans*-1,2-dichloroethylene. It is of special interest that he concluded, as early as 1930, that the data for 1,2-dichloroethane are incompatible with free rotation (i.e., that some values of the internal angle of rotation must be favored). The young Linus Pauling visited Mark's laboratory and was greatly impressed with these studies, as he frequently recalled in later years.

One of the most important contributions of the Ludwigshafen years was the writing, with Meyer in 1930, of the first monograph on polymeric compounds, *"Der Aufbau der hochpolymeren organischen Naturstoffe"* ("The Structure of High Molecular Organic Natural Substances"). It dealt with cellulose, Hevea rubber, gutta percha, starch, silk fibroin, and collagen on the basis of their crystallographic and solution properties. Another influential book of this period was Mark's *Physik und Chemie der Zellulose,* published in 1932.

However, the description of this period of Mark's career would be incomplete without mention of the bitter attacks to which he was subjected on the part of Herman Staudinger. Staudinger's highly charged emotional reaction was undoubtedly due to his conviction that, having first proposed the existence of long chain molecules, he had created a new fertile field of organic chemistry and could regard physical chemists and physicists who studied polymers as interlopers whom he felt free of accusing of plagiarizing his ideas. The central controversy involved Staudinger's insistence that polymer molecules are stiff rods, whereas Mark and Meyer realized that because of the hindered rotation around the bonds in the polymer backbone they must be thought of as flexible coils.

By 1932 the management of I.G. concluded that, with the probable takeover of the German government by Hitler's party, Mark, as the son of a Jewish father, could not continue to hold a prominent position in the company. It was characteristic of Mark that he always depicted the interview in which he was told that he would have to leave in the most innocuous colors.

VIENNA (1932–38)

In 1932 Mark was appointed professor of chemistry at the University of Vienna. He embarked immediately on the design of a curriculum in polymer science, the first of its kind. The research of his students dealt with the mechanism of polymerization and the viscosity of polymer solutions, but the most important achievement of the Vienna years was the formulation, with E.Guth, of a statistical theory of the elasticity of a rubber molecule. This provided the basic ideas that led later to the theory of crosslinked rubber elasticity.

In 1935 Mark conceived an ingenious idea for combining his scientific interests with his love for mountaineering. He persuaded his friends at the Soviet Academy of Sciences to organize an expedition to the highest peak of the Caucasus to determine whether deuterium was concentrated in its ancient glaciers. The results were inconclusive but all members of the expedition had a wonderful time.

Once again a political upheaval led to a painful change in Mark's life. In March 1938 Hitler's army occupied Austria, Mark was dismissed from his professorship and arrested because of his friendship with Chancellor Dollfuss who had tried to keep Austria independent and had been murdered by Nazi conspirators during an attempted coup in 1934. Mark had to use the influence of a high school classmate, now an influential man in the new regime, to be released from jail and to have his passport returned. He told me that the fee demanded by the "friend" for this service was his year's salary at the university.

Fortunately, Mark had been contacted during the previous year by the director of the International Paper Company plant in Hawkesbury, Ontario, who had offered him the position of research director. This offer was now repeated. Mark, his wife, and two young sons left Austria for Switzerland, ostensibly on a skiing vacation, and proceeded to England. In September Mark arrived in Canada, where he was joined by his family a few weeks later.

THE AMERICAN YEARS (1938–92)

Mark stayed in Hawkesbury for only two years, concentrating on the improvement of the manufacturing of wood pulp, cellulose acetate, and viscose. The work on viscose was particularly challenging because of its recent use in tire cord, which required fibers of new exacting quality standards. This led also to a close relationship with the Du Pont company, which proved important when Mark decided that Hawkesbury did not offer a sufficiently broad scope for his scientific interests. One of the Du Pont officers with whom Mark had dealt on the tire cord project was a board member of the Polytechnic Institute of Brooklyn and he proposed that Mark combine a Du Pont consultantship with an academic appointment at Poly.

In retrospect, the situation at Poly seemed far from auspicious when Mark joined its faculty as an adjunct professor in September 1940. This was a time when a flood of refugees from Hitler's Europe found it next to impossible to obtain academic positions appropriate to their qualifications and experience. Mark was assigned to the Shellac Bureau, whose function was the testing and chemical characterization of shellac. However, since this material was imported from Asia, the war stimulated a search for a synthetic substi tute and Mark's past experience was most valuable in that effort.

His activities at Poly broadened substantially as a result of research contracts with the wartime Office of Scientific Research and Development, which allowed him to hire A. V.Tobolsky, P.M.Doty, and B.H.Zimm, none of whom had experience with polymers but who later became leading figures in polymer research. Mark's influence induced a number of gifted students, such as S.Krimm and R.S. Stein, to make their career in this academically unfashionable area.

Mark also became involved in a number of rather exotic wartime projects. The most intriguing, perhaps, resulted from Mark's observation that the brittleness of ice could be largely eliminated by the incorporation of a few percent of sawdust. The British Military Mission in Washington, concerned about the shortage of landing facilities, which limited the scope of airplane attacks against German submarines, hoped that a flat iceberg made from this composition could be used as "an unsinkable landing field." A prototype was in construction when a dramatic improvement in the fortunes of war led to the termination of this effort in September 1942.

The polymer research activities at Poly led in 1947 to the foundation of the Institute of Polymer Research, the first graduate program of its kind in America. It is hard to recall today, when a number of distinguished programs of this kind are active at American universities, that this was highly controversial half a century ago. Mark's enthusiastic leadership was essential, as were his innumerable contacts all over the world, which enabled Poly's students to meet all the leading polymer scientists of that time. On Saturday mornings, symposia on subjects related to the rapidly advancing polymer research were held in Brooklyn and were attended by people from a wide area.

At the same time, Mark's friendship with polymer scientists in all countries where polymer research was active made the polymer community an exceptionally close-knit group. Because of his inability to bear grudges, he was most helpful to German and Austrian colleagues at a time when they were frequently ostracized. He also did a great deal to bridge the gulf between the area dominated by the Soviet Union and the rest of the scientific world. He was particularly devoted to the Weizmann Institute, created in Palestine in 1944, before the end of World War II, and under his leadership Poly was used to procure equipment for what became in time one of the world's outstanding scientific centers.

Advancing age did not seem to slow Mark's activities. Although he gave up his lecture course at Poly when he turned seventy, he continued for many years to delight students and faculty by his yearly lecture on "What is new in polymers," in which he related what he had heard during his many trips overseas and in America. By his count he made about 500 overseas visits, using his native Vienna as a base. He lectured extensively at universities and industrial laboratories, acted as editor of the *Journal of Polymer Science,* and was a consultant to the polymer industry and the U.S. government. He was also a most effective expert witness at a number of important patent litigations. Two experiences of his travels were specially memorable: In 1962 he was invited to present a lecture to the Japanese Emperor. I was told that such invitations were customary when a Nobel Prize laureate visited Japan, but that it was a unique honor in Mark's case since he did not fall into that category. Ten years later, Mark was one of the first two American scientists to visit China after the communist government seized power in 1949.

Over the years Mark became the recipient of many honors. Among these were honorary degrees from the universities of Liège, Uppsala, Berlin, Vienna, Madrid, Prague, and the Technion in Haifa and memberships in the Royal Institute of Great Britain, the National Academy of Sciences, and the Soviet Academy of Sciences. He received the Hertz Medal in 1928, the Nichols Medal in 1960, the Gibbs Medal in 1975, the Humbold Award in 1978, the Wolf Prize in 1979, the Perkin Medal and the National Medal of Science in 1980, and the Michelson-Morley Award in 1989.

His personal life was deeply affected by the death, after a long struggle with angina pectoris, of his wife Mimi in 1970. Another blow was the death of his son Peter in 1979. During the last two years of his long life Mark lived with his son Hans, who was chancellor of the University of Texas. There I visited him in the spring of 1991. The Geheimrat was in a wheelchair, but there seemed little change in his spirit as he told me about preparations for a lecture on conducting polymers.

NOTE

FOUR YEARS BEFORE Herman Mark's death, Jeffrey I.Seeman asked him to contribute an autobiographical sketch to the series of such memoirs of outstanding organic chemists published by the American Chemical Society under the title "Profiles, Pathways and Dreams." Mark accepted with enthusiasm, and I had the unique pleasure to work with him on the editing of that booklet, which was published in 1993. This memoir presents a picture of Mark's vivid character that cannot be conveyed second hand.

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Courtesy of Cold Spring Harbor Laboratory

BARBARA MCCLINTOCK

June 16, 1902–September 2, 1992

BY NINA V.FEDOROFF

ARBARA MCCLINTOCK'S remarkable life spanned the history of genetics in the twentieth century. Though technically rooted in Mendel's experiments carried out decades earlier, the science of genetics began with the rediscovery of his work at the turn of the century. In 1902, the year of McClintock's birth, William Bateson wrote prophetically that "an exact determination of the laws of heredity will probably work more change in man's outlook on the world, and in his power over nature, than any other advance in natural knowledge that can be clearly foreseen." And indeed, the science of genetics, to which McClintock made seminal contributions both experimental and conceptual, has come to dominate all of the biological sciences, from molecular biology, through cell and developmental biology, to medicine and agriculture. Bateson's immodest guess was arguably an underestimate of the impact of genetic knowledge on humankind.

The chromosomal basis of heredity was already well established by the time McClintock began her graduate training in the Botany Department at Cornell University. McClintock made her first significant contribution as a graduate student, developing cytological techniques that allowed her to identify each of the ten maize chromosomes. These early experiments laid the groundwork for a remarkable series of cytogenetic discoveries by the Cornell maize genetics group between 1929 and 1935. By all accounts, McClintock was the intellectual driving force of this talented group and either contributed substantially to or was exclusively responsible for many of the discoveries. These include identification of maize linkage groups with individual chromosomes, the well-known cytological proof of genetic crossing-over, evidence of chromatid crossing-over, cytological determination of the physical location of genes within chromosomes, identification of the genetic consequences of nonhomologous pairing, establishment of the causal relationship between the instability of ring-shaped chromosomes and phenotypic variegation, discovery that the centromere is divisible, and identification of a chromosomal site essential for the formation of the nucleolus.

In the years following completion of her doctoral work, McClintock continued her maize cytogenetic studies, eventually becoming interested in chromosome breakage, making important observations on the behavior of chromosomes lacking telomeres. Using knowledge gained from these studies, McClintock developed a method for using broken chromosomes to generate new mutations. Among the progeny of plants that had received a broken chromosome from each parent, she observed unstable mutations at an unexpectedly high frequency, as well as a unique mutation that defined a regular site of chromosome breakage. These observations so intrigued her that she began an intensive investigation of the chromosome-breaking locus. Within several years she had learned enough to reach the conclusion, published in 1948, that the chromosome-breaking locus did something hitherto unknown for any genetic locus: it moved from one chromosomal location to another, a phenomenon she called transposition. The study of transposable genetic elements and transposition became the central theme of her genetic experiments from the mid-1940s until the end of her active research career.

As with Mendel's experiments, it took decades for the generality and significance of McClintock's discovery of transposition to be appreciated. McClintock's extraordinary scientific talent and the importance of her early cytogenetic work were quickly recognized. She became a member of the National Academy of Sciences in 1944 at the young age of forty-two, only the third woman ever to have been elected. But her subsequent work on transposition led to a period of intellectual adumbration. While no one doubted her reputation for impeccable experimentation, the concept that genes could move was so at variance with the regularities of genetic transmission that permit the construction of genetic maps that its generality was doubted. But in the late 1960s evidence began to accumulate that bacteriophages and bacteria contain mobile DNA sequences. During the following two decades, it became clear that transposable elements are not only ubiquitous but are extraordinarily abundant in the genomes of many organisms. As awareness of the importance of her discovery grew, so did public recognition. Commencing with the National Medal of Science in 1970, McClintock received a number of prestigious awards, culminating in the award of an unshared Nobel Prize in Physiology or Medicine in 1983 for her discovery of transposition almost forty years earlier.

EARLY LIFE AND EDUCATION

Barbara McClintock was born in Hartford, Connecticut, to Sara Handy McClintock and Thomas Henry McClintock. Her mother was an accomplished pianist as well as a poet and painter, and her father was a physician. Barbara was the third of four children born while Dr. McClintock was struggling to establish his medical practice. By her own account, McClintock was an odd child and her relationship with her mother was difficult from the beginning. From about the age of three until she began school, Barbara lived in Massachusetts with an aunt and uncle. She accompanied her uncle, who was a fish dealer, first in a horse-drawn cart and later in his first motor truck. She reported enjoying this time and attributed her later interest in cars to watching her uncle struggle with his vehicle's frequent malfunctions.

McClintock returned home to attend school, and in 1908 the family moved to Brooklyn, New York. McClintock described herself as self-contained from a very early age, re-counting her mother's report that she could entertain herself for unusually long periods even in infancy. Later, she preferred sports, as well as solitary occupations such as reading or just sitting still and thinking. Both parents were quite unconventional in their attitudes toward child rearing: they were interested in what the children would and could be, rather than what they should be. They believed that formal schooling was only a part of a child's education, of equal importance with other experiences. When, for example, Barbara showed an interest in ice skating, her parents bought her the best equipment available and let her skip school to skate when the weather was right for it.

Barbara had a very special relationship with her father, who was extremely perceptive of and responsive to her as a human being. Even as a child, McClintock had an uncanny sensitivity toward people. She recounted having a teacher who disturbed her intensely because of her perception that the teacher was spiritually repulsive. Rather than make light of her reaction to the teacher, McClintock's father took her out of school and provided her with a private tutor. And despite the strained relationship between them, McClintock's mother fully supported her daughter's unconventional life style. Barbara described an incident from childhood in which a neighbor chided her for playing boys' games in the street, telling her it was time for her to learn to do the things that girls do. Upon hearing of the incident, Barbara's mother telephoned the neighbor and firmly told her never again to speak to her daughter in that fashion.

McClintock attended Erasmus Hall High School in Brooklyn, and during her high school years it became increasingly obvious that she would not outgrow her childhood oddities and become a conventional young woman. She discovered science; she loved to learn, and most of all, to figure things out. Barbara recalled her mother's deep concern that she might become a female college professor, whom her mother viewed as creatures that really didn't belong to society and had a difficult life. During this period, Barbara too became increasingly aware that doing what she wanted to do would have painful consequences. But she knew, as well, that she had to follow her own inclinations, whatever the consequences.

At the time McClintock graduated from high school in 1918, the family situation was difficult. Although Barbara had set her heart on attending Cornell University, there was very little money and her mother was firmly opposed to further education for her daughters, believing that it made them unmarriageable. Barbara took a job at an employment agency and spent evenings continuing her education by reading in the library. Just days before the semester started and with the intervention of her father, the decision was reversed. Barbara took a train to Ithaca and began her studies at Cornell, where she would stay to earn her doctor of philosophy degree.

PROFESSIONAL HISTORY

McClintock flourished at Cornell, both socially and intellectually. She loved learning and she was well liked—so much so that she was elected president of the women's freshmen class. But the decisions she made during her university years were consistent with her adamant individuality and self-containment. She enjoyed her social life, but she knew that none of her relationships would last. Her comfort with solitude and the tremendous joy that she experienced in knowing, learning, and understanding were to be the defining themes of her life. In her junior year, after a particularly exciting course in genetics, her professor invited her to take a graduate course in genetics. After that, she was treated much like a graduate student, and by the time she had finished her undergraduate coursework, there was no question in her mind: she had to continue her studies of genetics.

But while Cornell had a group of outstanding geneticists, genetics was taught in the plant breeding department, which did not take female graduate students. So McClintock registered in the botany department with a major in cytology and a minor in genetics and zoology. She began to work as a paid assistant to Lowell Randolph, a cytologist who had been appointed to a position at Cornell supported by the U.S. Department of Agriculture to complement the work of the maize geneticists and, it was hoped, strengthen the maize plant breeding efforts. McClintock and Randolph did not get along well and soon dissolved their working relationship, but as her colleague and lifelong friend Marcus Rhoades later wrote: "Their brief association was momentous because it led to the birth of maize cytogenetics." The initial task of reliably identifying each of the ten maize chromosomes had not yet been accomplished. Progress was limited by the in adequacy of the existing staining techniques, as well as the fact that the chromosomes in the root tip material generally used for such studies could not be reliably distinguished. McClintock solved both problems. As Rhoades related it:

> It was McClintock who capitalized on the use of Belling's new acetocarmine smear technique. In the course of her triploid studies, she had discovered that the metaphase or late prophase chromosomes in the first microspore mitosis were far better for cytological discrimination than were root tip chromosomes in paraffin sections. In a few weeks' time she had prepared an idiogram of the maize chromosomes, which she published in *Science*.

This was McClintock's first major contribution to maize genetics and laid the groundwork for a veritable explosion of discoveries that connected the behavior of chromosomes with the genetic properties of the organism, defining the new field of cytogenetics. McClintock was awarded the doctor of philosophy degree in 1927 and appointed an instructor. She had no thought of leaving Cornell and she knew exactly what needed to be done next: the maize genetic linkage groups had to be assigned to chromosomes. Again in Rhoades's words: "The years at Cornell from 1928 to 1935 were ones of intense cytogenetical activity. Progress was rapid, the air electric." The group was small, including Professor R.A.Emerson, the founder of maize genetics, McClintock, Beadle, Burnham, Rhoades, and Randolph, together with a few graduate students. McClintock had by then discovered that the pachytene chromosomes in microsporocytes were far superior to those of microspores for cytogenetic work, and the discoveries followed each other in rapid succession. Each linkage group was soon assigned to a chromosome, and the physical correlates of their genetic behavior became the primary focus of investigation.

A new graduate student, Harriet Creighton, joined the group in 1929. McClintock took charge of organizing her program of graduate study, persuading her to major in cytology and genetics. In the spring of the following year, McClintock suggested that Creighton take on the work of establishing a correlation between genetic recombination and the chromosomal crossovers that could be observed cytologically. McClintock provided stocks that had the appropriate genetic and cytological markers and guided the work, which showed for the first time that the genetic recombination was a reflection of the physical exchange of chromosome segments. The work, authored by Creighton and McClintock, was published in the *Proceedings of the National Academy of Sciences* in 1931 and was perhaps McClintock's first seminal contribution to the science of genetics, many more of which were to follow. Among the most important of her discoveries during the next few years, sometimes made alone, sometimes together with others, were that sister chromatids also exhibit genetic and cytological crossing-over, that genes can be physically localized on the chromosomes, that nonhomologous chromosome pairing has genetic consequences, that the formation of ring-shaped chromosomes accounts for certain types of phenotypic variegation, that the centromere is divisible, that broken chromosomes can undergo repeated cycles of fusion and breakage, and that a particular chromosomal site, the nucleolus organizer region (NOR), is essential to the development of the nucleolus.

Although McClintock's fame was growing, she had no permanent position. Cornell was hospitable to women students, but it had no women professors in fields other than home economics. Between 1931 and 1933, McClintock was supported by a fellowship from the National Research Council and worked at the California Institute of Technology and the University of Missouri, as well as Cornell. Lewis Stadler invited her to examine the chromosomes of X-irradiated plants that showed various abnormalities. She found that the irradiation had caused a variety of structural changes in the chromosomes, including translocation, inversions, deletions, and the formation of ring chromosomes. Coming to Cal Tech at T.H.Morgan's invitation, McClintock began to study the point at which the nucleolus attached to the chromosome. This led to her identification of the NOR (McClintock rued the grammatical error she made initially in naming this site the "nucleolar organizing body") and a description of its properties. She used stocks in which a translocation had broken the NOR into two segments, and her main conclusion was that each part of the NOR could organize an independent nucleolus and thus the NOR was genetically subdivisible. Describing the effect of McClintock's NOR publication, cell biologist Joseph Gall has written:

> Out of the hundreds of papers we have each read, a half dozen or so stick in our minds because of their beautiful logic, their clarification of an otherwise obscure set of data, or simply their technical elegance…. For me, one of Barbara McClintock's early cytogenetic papers falls in this category— her analysis of the nucleolus of maize published in 1934 in the *Zeitschrift für Zellforschung und Mikroskopische Anatomie* under the title, "The relation of a particular chromosomal element to the development of the nucleoli in *Zea mays*."

In 1933 McClintock received a Guggenheim Fellowship to go to Germany. McClintock was utterly unprepared for what she encountered in prewar Germany, and she returned to Cornell before the year had elapsed. Her prospects were dismal. She had completed graduate school seven years earlier and had already attained international recognition, but as a woman she had little hope of securing a permanent academic position at a major research university. Emerson obtained a grant from the Rockefeller Foundation to support her work for two years. Nominally paid as Emerson's assistant, she continued to work independently. McClintock was discouraged and resentful of the disparity between her prospects and those of her male counterparts. Her extraordinary talents and accomplishments were widely appreciated, but she was also seen as "difficult" by many of her colleagues, in large part because of her quick mind and intolerance of second-rate work and thinking. And while a number of prominent colleagues sought to help secure her an appropriate academic position, the fact remained that few positions commensurate with her accomplishments were open to women.

Finally, in 1936 Lewis Stadler was able to convince the University of Missouri to offer her an assistant professorship. She accepted the position and began to follow the behavior of maize chromosomes that had been broken by X-irradiation. She learned that the ends of newly broken chromosomes tend to fuse with each other, creating dicentric chromosomes that break again when a cell divides and chromosomes are distributed to the daughter cells. She also described conditions under which broken chromosomes "healed" or were repaired in some way so that they could function normally. She reported briefly in a paper published in *Genetics* in 1944 that in a certain stock a broken chromosome end that would normally "heal" during development of the embryo failed to do so. This implied that the addition of chromosome ends, termed telomeres, was an active genetic process and that the responsible gene in the stock had been inactivated by mutation. Elizabeth Blackburn, who discovered the enzyme that adds telomeres to chromosomes, wrote that "this information was in my mind when I made the decision to look for an enzymatic activity that adds telomeric DNA to DNA ends."

Though McClintock's reputation continued to grow (she was elected vice-president of the Genetics Society in 1939), her position at Missouri remained tenuous. She understood soon after her arrival that hers was a special appointment. She found herself excluded from regular academic activities, including faculty meetings, and eventually came to the realization that she was not only unlikely to be promoted but that her continued employment depended on Stadler's presence. In 1941 she took a leave of absence from Missouri and departed with no intention of returning. She wrote her friend Marcus Rhoades, who had just taken a position at Columbia University, asking where he was going to grow his corn. He was planning to go to Cold Spring Harbor for the summer. An invitation for McClintock was arranged through Milislav Demerec, who was a member of the Genetics Department of the Carnegie Institution of Washington, then the dominant research laboratory at Cold Spring Harbor. Demerec became the department's director late that year and offered McClintock a year's research appointment. Though hesitant to commit herself, McClintock accepted. When Demerec proposed making the appointment permanent, McClintock was quite reluctant but agreed to fly to Washington to speak with Vannevar Bush, then president of the Carnegie Institution. McClintock recalled that they took to each other immediately and that both enjoyed the visit immensely. Bush supported Demerec's wish to appoint McClintock as a permanent member of the research staff. McClintock accepted, still unsure whether she would stay.

McClintock did stay. She was a staff member of the Carnegie Institution of Washington's Genetics Department until 1967, whereupon she became distinguished service member of the Carnegie Institution, remaining at Cold Spring Harbor until her death in 1992. Carnegie gave her the freedom to do her work unfettered by teaching and other academic duties. McClintock's dislike of making commitments was a given: she always wanted to be free—free to do exactly what she wanted to do, when she wanted to do it. Indeed, she insisted that she would never have become a scientist in today's world of grants because she could not have committed herself to a written research plan. It was the unexpected that fascinated her, and she was always ready to pursue an observation that didn't fit.

Settling in at Carnegie, McClintock continued her studies on the behavior of broken chromosomes, devising a method of using them to produce mutations on the short arm of chromosome 9. In 1944 and 1945, the years she was elected to the National Academy of Sciences and the presidency of the Genetics Society, respectively, McClintock reported in the *Yearbook* of the Carnegie Institution of Washington on her analysis of progeny grown from self-pollinated plants obtained by crossing parents, each of which bore a broken chromosome 9. She detected many mutations among these progeny, including the expected terminal deficiencies, some internal deficiencies of various sizes, and some "provocative" mutants that showed variegation from the recessive to the dominant phenotype. She further reported observing "an interesting type of chromosomal behavior" involving the repeated loss of one of the broken chromosomes from cells during development. What struck her as odd in the light of her previous studies on broken chromosomes was that in this particular stock it was always chromosome 9 that broke and it always broke at the same place. McClintock called the labile chromosome site *Dissociation* or *Ds* because "the most readily recognizable consequence of its actions is this dissociation." She quickly established that the *Ds* locus would "undergo dissociation mutations only when a particular dominant factor is present." She named this factor *Activator (Ac)* because it activated chromosome breakage at *Ds*. By the time she wrote her report for the Carnegie *Yearbook* published in 1948, she had reached some extraordinary conclusions about these loci. *Ac* was not only required for *Ds*-mediated chromosome breakage but could destabilize previously stable mutations, much as her friend Marcus Rhoades had described several years earlier for a pair of interacting loci, one of which was an allele of the maize *a* locus. But more than that, and unprecedented, the chromosome-breaking *Ds* locus could "change its position in the chromosome"; it could *transpose*. Moreover, she had evidence that the *Ac* locus was required for transposition of *Ds* and that, like the *Ds* locus, the *Ac* locus was also mobile.

Within several years, McClintock had established beyond any doubt that both the *Ac* and *Ds* loci were not only capable of changing their positions on the genetic map but also of inserting into loci to cause unstable mutations of a type initially studied by R.A.Emerson at the P locus of maize. By the time she prepared her paper for the Cold Spring Harbor Symposium of 1951, McClintock had isolated unstable alleles of at least four different genes. Some were caused by the insertion of the *Ds* element and so required the presence of *Ac* for instability. Others were caused by insertion of the *Ac* element itself and were inherently unstable. She had determined that the instability of such mutations, which had long fascinated geneticists and horticulturists, was attributable to the frequent departure of the inserted genetic element from the gene during development, restoring normal function and, concomitantly, the wildtype phenotype. She had also identified different noninteracting "systems" of mutability, later renamed transposable element "families."

McClintock recounted that the reaction to her symposium presentation ranged from perplexed to hostile. Later, she published several papers in refereed journals and from the paucity of reprint requests, inferred an equally cool reaction on the part of the larger biological community to the astonishing news that genes could move. After that, McClintock tended to write up her results *as if* for publication and file them, publishing little more than concise summaries of her results in the annual *Yearbook* of the Carnegie Institution and occasional overviews for symposia. McClintock continued her analysis of the *Ac-Ds* transposable element family and began the study of a new element that she called *Suppressor-mutator* or *Spm*. This element, which also came in versions that could transpose autonomously and versions that could not, had many of the characteristics of the *Ac-Ds* family but exhibited an even more complex behavior. Some insertion mutations, for example, did not completely suppress expression of the affected gene, except when the fully functional *Spm* element was present in the same genome, implying that the element could produce a substance that affected expression of the mutant gene.

These descriptions of McClintock's of what proved to be the first example of an interaction between a trans-acting regulatory factor and its DNA binding site, were published well before Jacob and Monod's seminal work on the regulation of the *lac* operon in *E. coli.* McClintock immediately saw and attempted to draw attention to the parallels between these regulatory phenomena by adopting Jacob and Monod's terminology to the regulation of maize gene expression mediated by transposable elements. More fascinating yet, McClintock found that the *Spm* element could become heritably inactivated by a genetic mechanism that differs strikingly from conventional mutation by its reversibility. Indeed, although the element could be transmitted in an extremely inactive form through many plant generations, it remained capable of both transient and heritable reactivation. In particular, McClintock came to the conclusion that an active element could activate an inactive one so long as both were present in the same genome. This suggested that an active element provides a substance that activates the element, either directly or by interfering with the genetic mechanism that is responsible for inactivation.

By this time, McClintock's work had taken her far outside the scientific mainstream and in a profound sense she had lost her ability to communicate with her colleagues. There have been many attempts at explanations, all of which undoubtedly contain a measure of truth. By her own admission, McClintock had neither a gift for written exposition nor a talent for explaining complex phenomena in simple terms. But perhaps there are more important factors, since patient readers have found both her early and her later papers not only comprehensible but indeed intellectually elegant. First, the very notion that genes can move was in deep contradiction to the regular relationships among genes that underlie the construction of linkage maps and the physical mapping of genes onto chromosomes. The evidence that genes maintain their positions relative to each other was overwhelming: the concept that genetic elements can move would undoubtedly have met with resistance regardless of author and presentation. Indeed, even twenty years after McClintock's initial report, emerging evidence that mobile elements exist in bacteria was met with skepticism.

And more than that, by the time McClintock took up the study of transposition, she was not just a brilliant beginner but an accomplished, experienced, mature cytogeneticist. Her experiments were very complex and difficult to communicate even to the quickest of minds. Mel Green recounts that shortly after the 1951 Cold Spring Harbor Symposium, he and several other geneticists queried Sturtevant, arguably one of the century's leading geneticists, about what McClintock had said. Green quotes Sturtevant as saying: “I didn't understand one word she said, but if she says it is so, it must be so!" Such was the intellectual respect that McClintock commanded—and such was the strangeness of concept and complexity of her experimentation.

McClintock was deeply frustrated by her failure to communicate, but her fascination with the unfolding story of transposition was sufficient to keep her working at the highest level of physical and mental intensity she could sustain. Her work on transposition was interrupted only twice. The first interruption was a visit to Stanford in 1944 in response to an invitation from George Beadle, who thought she was precisely the person to work out the problem of identifying the chromosomes of the mold *Neurospora,* which had become a popular organism for molecular geneticists. The second occurred in the late 1950s when the National Academy of Sciences established a committee to identify and collect indigenous races of maize in Central and South America out of concern that the introduction of high-yielding agricultural hybrids would result in their disappearance. McClintock was asked to help train local cytologists to carry out the work of classifying the maize races by chromosome morphology. McClintock spent the winters of 1958 and 1960 in Central and South America, fascinated by the emerging realization that the spread of maize through the region could be tracked by the chromosome constitution of the indigenous populations. The work was summarized briefly in the *Yearbooks* of the Carnegie Institution, appearing as a full monograph in 1978.

But transposition remained McClintock's central passion. By the time of her formal retirement, she had accumulated a rich store of knowledge about the genetic behavior of two markedly different transposable element families. She was sufficiently confident of the importance of her work to carefully preserve all of the stocks with mutant elements that she accumulated along the way, perhaps in unconscious prepa ration for the new generation of molecular geneticists. And indeed, beginning at about the time her active fieldwork ended, transposable genetic elements began to surface in one experimental organism after another. These discoveries began in an altogether different age. In the two decades between McClintock's original genetic discovery of transposition and its rediscovery, genetics had undergone as profound a change as the cytogenetic revolution that had occurred in the second and third decades of the century. The genetic material had been identified as DNA, the manner in which information was encoded in the genes had been deciphered, and methods had been devised to isolate and study individual genes. Genes were no longer abstract entities known only by the consequences of their alteration or loss: they were real bits of nucleic acid that could be isolated, visualized, subtly altered, and reintroduced into living organisms.

Thus, soon after the initial realization that mutations of a certain type that occurred in bacterial viruses might be attributable to the insertion of a foreign DNA sequence, visual evidence was obtained by electron microscopic analysis of heteroduplexes between homologous DNA sequences having and lacking the insertion. The newly inserted mobile elements appeared as unpaired loops of DNA extending from the DNA duplex. Mobile genetic elements were no longer abstract concepts. Although the study of maize transposable elements had been an active and productive field of research since Emerson's original studies on variegation at the P locus long before McClintock explicated the underlying genetic mechanisms, the recognition that mobile elements are ubiquitous and in fact extraordinarily abundant components of the genomes of many, if not all, organisms grew slowly during the 1970s and 1980s.

My first encounter with McClintock, which was to lead eventually to the molecular cloning and characterization of the maize elements, took place during a visit to the Cold Spring Harbor Laboratory in 1978. The laboratory itself was no longer the same institution that McClintock had joined almost four decades earlier. The Genetics Department had been closed by the Carnegie Institution of Washington, although a Genetics Unit consisting of McClintock and A.Hershey, both retired, had been maintained. J.D. Watson was by then the director of a vastly larger complex of laboratories at Cold Spring Harbor, all engaged in molecular biological investigations. I had been asked to give a seminar at the Cold Spring Harbor Laboratory on my postdoctoral work in Don Brown's laboratory at the Carnegie Institution of Washington's Department of Embryology in Baltimore. Although McClintock was unable to attend the lecture, I encountered her by chance in a hallway of the Demerec Laboratory, and she invited me to her spacious laboratory for a chat. We talked for several hours, and I was drawn to the clarity and depth of her discourse, no matter the subject. It was so at variance with her reputation for obscurity that I was prompted to read her papers from beginning to end upon my return to Baltimore. I was intrigued with what I found to be a marvelous genetic detective story, and when I received an unexpected offer of a permanent staff position at Carnegie's Embryology Department, I immediately decided to tackle the molecular analysis of the maize elements.

The task I had taken on proved daunting, as much because of the distance between McClintock's classical genetic approach and that of the molecular biologist as because plant molecular biology simply didn't exist yet. Our relationship began in earnest when I grew my first corn crop consisting of McClintock's transposable element stocks during the summer of 1979 at the Brookhaven National Labo ratory, where we were kindly offered space and help by Ben and Frances Burr. Although McClintock was highly critical of my first efforts at maize genetics, enough of the right crosses got done despite my ignorance, so that I had the material I needed to begin the molecular cloning of first the *Ac* and *Ds* elements and, later, the *Spm* element. Our first interactions were difficult, and it took several years before we were comfortable with each other's way of thinking. But in time we both came to value deeply the intellectual as well as the personal side of our relationship.

By the time the maize elements were cloned and their molecular analysis began, the importance of McClintock's discovery of transposition was widely recognized. She received the Kimber Genetics Award in 1967, the National Medal of Science in 1970, and the Lewis S.Rosensteil Award and the Louis and Bert Freedman Foundation Award in 1978. In 1981 she was named prize fellow laureate of the MacArthur Foundation and received the Wolf Prize and the Lasker Basic Medical Research Award. In 1982 she shared the Horwitz Prize. Finally, in 1983, thirty-five years after publication of the first evidence for transposition, McClintock was awarded the Nobel Prize for Physiology or Medicine. Yet while the money attached to these prizes increased her financial security, something to which she'd given little thought in earlier years, she found the ceremonies arduous and the attendant publicity and adulation utterly repugnant. She longed for her privacy, and she was exhausted and disturbed by the endless stream of requests that only seemed to grow in volume with each award. Suddenly everyone wanted her: there were honorary degrees, keynote speeches, lectures, interviews—even autograph hunters.

And still, through it all, McClintock never lost her connection with science—she never retired. She continued to live at Cold Spring Harbor, spending her last years in a spartan apartment on the ground floor of Hooper House, a women's dormitory heavily used during the summer meetings season at the laboratory. She attended every session of the annual Cold Spring Harbor Symposium, as well as seminars, the year around. She read voraciously, lamenting her failing vision. Her laboratory was filled with books on all subjects, and the tables were covered with stacks of articles copied from current journals, many with sentences carefully underlined here and there, giving evidence of careful attention. She was keenly aware of every development in the molecular and genetic analysis of the maize transposable elements as it unfolded in my laboratory and elsewhere. She took special interest in the analysis of the complex and elegant *Spm* family of elements, my own particular favorite. Not until the last few years of her life did the molecular and genetic studies on this family of elements become so complex that she began to find it difficult to follow and remember the details. Even when I visited Cold Spring Harbor in 1991 to give a course lecture on the molecular genetics of the maize transposable elements, McClintock sat through the entire session, which lasted late into the evening. Her questions were penetrating and her observations invariably widened the discussion: the students were amazed.

It was during this visit that I was approached by Jim Inglis of the Cold Spring Harbor Press to assemble a volume in honor of McClintock's ninetieth birthday the following year. I took on the project, despite qualms that Barbara would find this not a gift but another burden. David Botstein, who joined me in this effort, and I approached a number of individuals whose lives had intersected with McClintock's to write for this volume. What emerged was *The Dynamic Genome,* a collection of varied essays each reflecting the pursuits and passions ignited by the sparks and embers scat tered from the fierce blaze of McClintock's intellect through the decades of this century of genetics. Many of the authors joined in the celebration of her ninetieth birthday at the home of Jim Watson, not far from her modest apartment on the laboratory grounds. She knew nothing of the book but recognized her friends—even Harriet Creighton, her first "unofficial" graduate student, had made the trek to Cold Spring Harbor. We settled Barbara on Jim's front porch and I began to read aloud the introduction and the list of authors and their essays. At first she joked a bit, discomfited by the attention. But soon her face began to glow as she perceived the depth of understanding and respect gathered around her, lovingly collected between the covers of the book. She said later it was the best party ever for her, though she admitted that it had taken a week to recover at her age. She was sure that she would die at ninety and a few months later she was gone, drifting away from life gently, as a leaf separates from an autumn tree. What Barbara McClintock was and what she left behind are eloquently expressed in a few short lines written many years earlier by her friend and champion Marcus Rhoades, whose death preceded hers by a few short months:

> One of the remarkable things about Barbara McClintock's surpassingly beautiful investigations is that they came solely from her own labors. Without technical help of any kind she has by virtue of her boundless energy, her complete devotion to science, her originality and ingenuity, and her quick and high intelligence made a series of significant discoveries unparalleled in the history of cytogenetics. A skilled experimentalist, a master at interpreting cytological detail, a brilliant theoretician, she has had an illuminating and pervasive role in the development of cytology and genetics.

THE QUOTATIONS ATTRIBUTED to McClintock are from her publications on transposition, primarily the annual reports appearing in the *Yearbooks* of the Carnegie Institution of Washington; all of these are reproduced in *The Discovery and Characterization of Transposable Elements: The Collected Papers of Barbara McClintock* (New York: Garland Publishing, 1987). All other quotations, with the exception of the first and last (Bateson and Rhoades), appear in the chapters by the individuals to whom they are attributed in *The Dynamic Genome: Barbara McClintock's Ideas in the Century of Genetics* (ed. N.Fedoroff and D.Botstein; Cold Spring Harbor: Cold Spring Harbor Press, 1992). The Bateson quotation appears in E.A.Carlson's, *The Gene: A Critical History* (Philadelphia: W.B.Saunders). The final quotation of M.M.Rhoades was taken from an undated document in the files of the Carnegie Institution of Washington titled "Barbara McClintock: Statement of Achievements," possibly prepared in support of her nomination for an award. Other than my own recollections of conversations with McClintock, my principal source of information about her early life and the chronology of later events was E.F.Keller's, *A Feeling for the Organism: The Life and Work of Barbara McClintock* (San Francisco: Freeman, 1983), as well as a copy of McClintock's curriculum vitae, given by her to me in about 1980 together with one of her two complete collections of her reprints.

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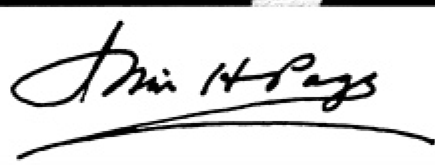

IRVINE HEINLY PAGE

January 7, 1901–June 10, 1991

BY HARRIET P.DUSTAN

OR OVER FIFTY YEARS, from his first scientific paper on hypertension (high blood pressure) in 1935 to the publication of a massive text (1,102 pages), *Hypertension Mechanisms,* in 1987, Irvine Page was a dominant figure in the field of hypertension research. In addition to his scientific contributions, which were many and seminal, his unflagging advocacy of hypertension as a major public health problem did much to focus the attention of patients, physicians, investigators, and politicians on the need for its control. Largely forgotten is the fact that Page initiated the negotiations that eventually led to the establishment of the Institute of Medicine of the National Academy of Sciences.

Irvine Heinly Page was born on January 7, 1901, in Indianapolis, Indiana. His father was Lafayette Page, a physician of considerable local repute. Irvine Page was one of three children; his brother was a lawyer, and his sister, Ruth Page, became a famous dancer. She died in 1991 and was eulogized by the *Chicago Tribune* as “a world renowned choreographer who reigned as the grand dame of dance of Chicago.” Page’s early schooling was in Indianapolis, and his summers were spent on Cape Cod at Hyannis Port. He attended Cornell University, majored in chemistry, and di rected a dance band that nicely supplemented his family's financial support. Page liked chemistry, so after graduating in 1921 he worked for a year on the recently discovered insulin with George Clowes and Elliott Joslin. Then he enrolled in Cornell Medical College, attracted by biochemist James Sumner who won a Nobel Prize for crystallizing urease. Page found he liked medicine also but not to the exclusion of chemistry, because after a two-year internship at Presbyterian Hospital he was recruited by Geheimrat Richard Wilstätter to establish a department of brain chemistry at the Kaiser Wilhelm Institute of Psychiatry in Munich, Germany. Page in his memoir, *Hypertension Research,* recounts that in the three years he spent there his accomplishments were to set up a laboratory of neurochemistry and conduct research that gave "fats and sterols a better name." During that time he also accomplished a marriage to Beatrice Allen, a dancer with the Denishawn Company. Page returned to the United States in 1931, having been warned by a German army officer of the likelihood of war. When he made the decision to return he had no position to go to; nobody wanted or needed a brain chemist Then good fortune struck. Donald Van Slyke of the Rockefeller Institute happened to be in Munich with his family and needed a doctor for his daughter who had an infected finger. Page was the only American physician there, so he was consulted. Not only was Van Slyke one of Page's heroes but, as it turned out, he held the key to Page's immediate future. The results were that the finger infection was treated successfully and Page was offered a position at the Rockefeller Institute.

Before leaving Europe, Page went to Frankfurt for two months of study with Frans Volhard, a professor of medicine particularly interested in hypertension who was responsible for some of the early descriptions of its effect on the kidney. There Page met two of Volhard's assistants, who played important roles in his future. One of them, Bohn, claimed that he could extract a blood pressure raising (pressor) substance from the blood of malignant hypertensives. (Malignant hypertension is a type of elevated blood pressure that is uniformly fatal if untreated. The term "malignant" refers to its clinical course and not to any specific causation.) The other Volhard associate was Hessel, who was working on renin, a pressor substance found in saline extracts of kidney that had been described forty years earlier by Tigerstedt and Bergman.

Page spent six years at the Rockefeller Institute as an associate member, and it was there that he began his work in hypertension. First, he tried to reproduce Bohn's finding of circulating pressor substances in malignant hypertension but to no avail; none was found. He also tried to purify renin and isolate it from blood; that was not successful either. He did make an important observation, however, when he lowered blood pressure with colloidal sulfur injections and found that kidney function was well maintained. Prior to that observation, conventional wisdom held that elevated blood pressure was essential for blood to circulate through narrowed renal arteries.

Toward the end of Page's stay at the Rockefeller Institute, Arthur Corcoran joined him, and in 1937 the two men moved their research activity to the Laboratory for Clinical Research at the Indianapolis City Hospital, which was supported in part by the Eli Lilly Company. Already there were Kenneth Kohlstaedt and Oscar Helmer. This team had great success, and in a 1940 publication they described renin as an enzyme that produces a pressor compound they called angiotonin. Braun-Menendez and his colleagues in Buenos Aires had similar success, and the two discoveries were published almost simultaneously. The Argentineans, however, had a different name for the pressor compound; they called it hypertensin.

In another first, Page, in collaboration with a young clinician, Robert Taylor, was able to reduce blood pressure by kidney extracts and later by pyrogen therapy. Those were the only nonsurgical treatments available in the early 1940s; low-sodium diets were under study but at that time were not widely used.

In 1945 Page was invited to set up a hypertension research program at Cleveland Clinic. Corcoran and Taylor went with him. Page was director of the Research Division at Cleveland Clinic from 1945 until his retirement in 1966. Much was accomplished by him and his co-workers: serotonin was isolated and its pharmacology carefully detailed, the mosaic theory of hypertension was introduced and refined, the importance of the autonomic nervous system as the controlling mechanism in hypertension was firmly established, angiotensin was crystallized, treatment of hypertension was a constant and successful focus, the National Foundation for High Blood Pressure was begun, and the Institute of Medicine had its origins there.

Retirement for Irvine Page did not mean a retirement from hypertension activities; he only changed his venue by moving to Hyannis Port. Separated from a laboratory and research administration he did the next best thing; he wrote. From that period came four important scientific texts: *Renal Hypertension* in 1968, edited with J.W.McCubbin; *Serotonin* in 1968; *Angiotensin* in 1973, edited with F.M.Bumpus; and *Hypertension Mechanisms* in 1987. Equally informative but in a different vein was his last book, *Hypertension Research, A Memoir, 1920–1960,* published in 1988.

For almost three decades Page was associated with *Modern Medicine,* and for at least half that time he was the editor. The journal was published biweekly, and this meant writing two editorials a month for practicing physicians, a task in which Page was aided immeasurably by his wife, who is herself a writer and was his justifiably trusted critic and copy editor. Although Page was not a practitioner in the usual sense, nor had he ever been, he used his editorial position to inform, cajole, teach, and exhort the country's practitioners, who were the readers of that journal. In the 1992 presidential campaign lexicon, it was a bully pulpit for him and it worked; thousands of physicians came to know more science, politics, and sociology through those editorials than they would have otherwise. In 1972 a selection of them was published under the title of "Speaking to the Doctor."

Page was injured in an auto accident in March 1990 and was in poor health from then until his sudden and unexpected death on June 10, 1991. He is survived by his wife and two sons, Christopher and Nicholas, and their wives and children.

The honor of election to the National Academy of Sciences came to Page in 1971, some time after his major scientific contributions. Having known him as I did through an association of forty-three years I can hear him say "about time." Of course, he was immensely pleased but being a bit of a curmudgeon was a necessary part of his public persona.

This brief biographical sketch in no way details the quality of Page's contributions and their impact on biology and medicine as we understand them today. One of Page's major scientific contributions was his description of the enzymatic nature of renin and its production of a potent pressor compound, angiotonin, from a plasma protein substrate. Although his first efforts at isolating renin while at the Rockefeller Institute were unsuccessful, he hit pay dirt when he resumed his studies upon moving to Indianapolis. There he had the manpower needed for the task. Oscar Helmer prepared protein fractions of kidneys, which were tested for their ability to raise blood pressure in the dog by Page; to cause vasoconstriction in isolated vascular beds by Kohlstaedt; and to decrease blood flow to the dog kidney by Corcoran. They found that, as purification proceeded, activity decreased but could be returned if plasma was added to the injectate. They called that plasma substance *renin activator* and the pressor compound that it produced *angiotonin*. A little later they realized the nature of the activator and called it *renin substrate*.

At about the same time, a group in Argentina headed by Eduardo Braun-Menendez also found a pressor substance of kidney origin. It too was the product of the enzymatic action of renin, but they called the pressor substance so produced *hypertensin* and the substrate *hypertensinogen*. Some years later Page and Braun-Menendez agreed on the names *angiotensin* and *angiotensinogen*.

That was only the beginning of the story. Page was set on crystallizing renin, and after moving to Cleveland he enlisted the collaboration of Arda Green, a protein chemist, but years of effort, many kilos of kidneys, and tons of ammonium sulfate produced nothing. However, work on the renin-angiotensin system was proceeding. First, it was learned that angiotensin was more complex than first thought when Skeggs and colleagues (1954) showed that the product of renin's action on its substrate is a peptide that is without effect on blood vessels and must be converted to an active form by what they called *converting enzyme*. These angiotensins came to be known as angiotensin I and II. Very soon (1956) Peart described the amino acid composition of angiotensin I, and within a year Bumpus, Schwarz, and Page reported the synthesis of angiotensin II, the active compound, and confirmed that it is an octapeptide. Almost simultaneously Schwyzer and colleagues reported the synthesis of L-arginine angiotensin II. Some years later renin was finally isolated and characterized in the laboratories of Corval and Menard, of Inagami, and of Haber.

The renin-angiotensin system is widely distributed and has been identified in blood vessels, brain, salivary glands, uterus, placenta, adrenal, and, of course, the kidney, where it was originally found. It is one of the most important systems of the body: it regulates blood pressure by directly affecting the smooth muscle of arteries, it is the primary factor in aldosterone release, it has an independent effect on salt excretion by the kidney, and it influences brain function. It plays a major role in hypertension and heart failure, as witnessed by the beneficial effects of angiotensin-converting enzyme inhibitors. Page's description of the renin-angiotensin system was a major contribution.

Another major contribution was the isolation and characterization of serotonin. It had been known for eighty years that when blood clots, the serum contains a vasoconstrictor that, as later work showed, is absent when sodium citrate is used to prevent clotting. Also, the appearance of this vasoconstrictor was found to have a quantitative relationship to the platelet count, and other work found a vasoconstrictor in platelet extracts. The isolation of this substance Page assigned in 1946 to a young postdoctoral research fellow, Maurice Rapport. This Page considered a necessary step before undertaking a search for substances in the blood of hypertensives that could be responsible for raised arterial pressure. Success came shortly, and in 1948 Rapport, Green, and Page reported the isolation, identification, and crystallization of that vasoconstrictor. It is 5-hydroxytryptamine that they called serotonin because it was isolated from serum and had a tonic effect on arteries. In 1953 Twarog and Page showed that the brain contains serotonin, and now it is recognized as a neurotransmitter. Subsequently, Page and his colleague McCubbin carried out an extensive investigation of the cardiovascular pharmacology of serotonin, and they showed, among many other effects, that the blood pressure response is strongly influenced by the activity of the sympathetic nervous system. Now, almost fifty years later, we still do not know the true scope of serotonin's activity in human biology.

Page influenced the conceptualization of hypertension in a unique fashion. In the 1940s and early 1950s *the* cause of essential hypertension was searched for. By 1950 Page had concluded, however, that hypertension results from an interaction of many mechanisms. This he called the mosaic theory. It was first suggested as a combination of five mechanisms and subsequently refined, so that by 1960 it was made up of eight factors, all interrelated. Because this theory (as he called it, although it was actually a schema) presaged a large volume of evidence for the interlocking of multiple mechanisms of hypertension control, the mosaic "theory" is now the dominant concept and no longer are investigators looking for a single cause of hypertension.

Although Page was not a practicing physician, he had a keen appreciation of the public health importance of hypertension and what could be achieved by blood pressure control. From 1951, when antihypertensive drugs of long-term effectiveness were first introduced, until his retirement in 1966, Page was actively concerned with the treatment of hypertension: he tested every new drug in dogs, so that he and those of us involved in the care of patients knew the pharmacology and what we were dealing with. He was insistent on the importance of treating hypertension, and we were among the first to demonstrate a causal relationship between high blood pressure and the lethal conse quences of malignant hypertension by showing the life-saving effects of antihypertensive drug therapy.

In addition to his advocacy for the treatment of hypertension, Page was a strong, fervent, and vocal supporter of research and specifically research on hypertension. Early on he maintained that industry and business should contribute directly to that support because hypertension and other cardiovascular diseases ravaged middle-aged men, causing businesses to suffer accordingly. Thus, in 1945 he organized the National Foundation for High Blood Pressure, whose members were businessmen from Cleveland and hypertension researchers from across the country. The businessmen raised money for research support, which was competed for by all scientists involved in hypertension research, not just those who were members of the foundation. Page hoped that other cities would follow the lead of these Cleveland businessmen, so that a network of support for hypertension research could be established in this country. But other events overtook these aspirations; the network never materialized, the National Institutes of Health became the major funding source of biomedical research in this country, and the foundation gave up its independent status and joined the American Heart Association as the Council for High Blood Pressure Research. The annual meetings of the council represent the best of contemporary hypertension research and are a continuing tribute to Page's advocacy of research.

In the early 1960s Page became convinced that establishment of a National Academy of Medicine would benefit by bridging "the wide gaps among government, the American Medical Association, specialty societies, academia and industry—an ecumenical movement." He used his *Modern Medicine* editorials to test the waters. Finding considerable support for this idea he approached the president of the

National Academy of Sciences, who also was interested. Page then obtained a planning grant from the Cleveland Foundation, and in January 1967 the first organizational meeting was held at Cleveland Clinic. The deliberations took a long time, and in the end a National Academy of Medicine was not established. In its place was the Institute of Medicine, which came into being in 1971.

This then is a brief look at the life of a man who made significant scientific contributions; altered the course of the investigation and treatment of high blood pressure; and, in a more general sense, influenced medicine in the United States.

MATERIALS USED IN WRITING this memoir came from my forty-three-year friendship with Irvine Page, twenty-four years of which I worked closely with him; from his book *Hypertension Research, A Memoir, 1920–1960;* and from a rereading of his major scientific papers.

HONORS

In addition to his election to the National Academy of Sciences, Page received many other honors. He was president of the American Heart Association (1956–57); he received ten honorary degrees and a number of prestigious awards—the Ida B.Gould Memorial Award of the American Association for the Advancement of Science (1957); Albert Lasker Award (1958); Gairdner Foundation Award (1963); Distinguished Award of the American Medical Association (1964); Oscar B.Hunter Award (1966); Passano Foundation Award (1967); and the Stouffer Prize for Hypertension Research (1970).

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Wm. C. Rose

WILLIAM CUMMING ROSE

April 4, 1887–September 25, 1985

BY HERBERT E.CARTER AND MINOR J.COON

ILLIAM CUMMING ROSE, a member of the National Academy of Sciences from 1936, died in Urbana, Illinois, at the age of ninety-eight. Thus ended the career of a dedicated and inspiring teacher and an outstanding pioneer in biochemistry and nutritional science. He rendered the University of Illinois distinguished service until his retirement in 1955. In research he devoted his attention to the intermediary metabolism of amino acids, creatine, uric acid, and chemically related compounds and was renowned for the discovery, isolation, and identification of threonine. The characterization of the last of the amino acids to be found as universal components of proteins led to his determination of the complete essential amino acid requirements of the laboratory rat and culminated in the establishment of the amino acid requirements of humans.

EDUCATION AND EARLY LIFE

William C.Rose was born in Greenville, South Carolina, and spent his childhood in small communities in the Carolinas, where his father, John M.Rose, served as a Presbyterian minister. Money was scarce, particularly since the cleric's small salary was reduced by generous gifts to religious and humanitarian causes, but somehow the necessities were provided and the best possible education was afforded the children in the family. Young Will attended an assortment of local schools until the age of fourteen, when the inadequacy of the education caused his father to remove him from school and tutor him at home. He was well prepared by the time he entered college and had already been introduced to Latin, Greek, and Hebrew and had acquired an interest in chemistry from reading Remsen's *An Introduction to the Study of Chemistry,* a college text his older sister had used. Will wished to attend a large university, but his father thought his son, at age sixteen, was too young and so convinced him to attend Davidson College in North Carolina, a school for which Will developed a lifelong affection.

While in graduate school at Yale University, Rose decided on the branch of chemistry he would pursue. During his initial interview with Russell Chittenden, director of the Sheffield Scientific School, Rose mentioned an interest in food chemistry. This suggestion brought an introduction to Lafayette Mendel, which not only guided Rose's interest and work toward biochemistry but also led to a strong friendship that endured until Mendel's death. In 1911 Rose left Yale for an instructorship in physiological chemistry at the University of Pennsylvania, a department then headed by Alonzo Taylor, which was followed by a short period of advanced study with Professor Franz Knoop at the University of Freiburg. While in Germany, Rose received a cablegram from Galveston asking him to come to the College of Medicine to organize a department of biochemistry. At first Rose was hesitant about the offer because he felt obliged to go back to the University of Pennsylvania, but he was fully reassured by a cable from Taylor containing the terse single sentence, "You darned fool, I recommended you for that job." This message came in response to his own cable in which he had turned down the offer. He then accepted the position of associate professor of biological chemistry at the University of Texas and quickly rose to the rank of professor and head of the department. In 1922 Rose was persuaded to move to the University of Illinois, where he was appointed professor and head of the Division of Physiological Chemistry (the name was later changed to Biochemistry) in the Chemistry Department and found a permanent and very supportive home for his scientific career. He provided dedicated service to that institution for the next thirty-three years.

RESEARCH ACCOMPLISHMENTS

In research Rose displayed a gift for meticulous experimentation and for thoroughness and clarity in preparing his results for publication. His early studies on creatine and its dehydration product, creatinine, carried out with Mendel at Yale and published in 1911, dealt with the role of carbohydrates in the metabolism of these compounds and with the effect of inanition on the creatine content of muscle. In subsequent years Rose and his associates made excellent use of the experimental methods available at the time, chiefly nutritional studies, to explore the metabolic relationship of creatine to creatinine and of both to other nitrogenous substances.

They observed that the ingestion of diets high in protein failed to induce the excretion of creatine in normal men and women and concluded that there was no exogenous source of urinary creatine in the absence of creatine in the diet. In addition, no relationship was observed in growing rats between the arginine content of the diet and total creatinine elimination in the urine. Furthermore, in human subjects no evidence could be obtained that exogenous arginine was catabolized to creatine or creatinine. In light of present knowledge, such studies did not reveal the involvement of this amino acid in amidine transfer in creatine biosynthesis, but they are still of fundamental interest. We will have much to say in what follows about the pioneering studies in Rose's laboratory on essential amino acids; suffice it to say here that creatine and creatinine were found to be incapable of replacing dietary histidine for the growth of rats. The knowledge available at that time on the metabolism of creatine and creatinine was ably summarized by Rose in 1933 and 1935 in insightful articles in the *Annual Review of Biochemistry*.

From his own experimental studies and a summary of the literature he published in 1923 in *Physiological Reviews,* Rose concluded that endogenous purines may have their ultimate origin in arginine and histidine but that the extent of their synthesis might be limited to the anabolic needs of the organism. And in studies similar to those referred to above involving creatine and creatinine, he established that neither adenine nor guanine (nor, for that matter, a mixture of all four of these compounds) was capable of replacing dietary histidine for the growth of rats. In addition to his interest in nitrogenous compounds as already summarized, Rose made useful and scholarly contributions to knowledge in several other quite diverse areas, including analytical methods, the nephropathicity of dicarboxylic acids and their derivatives, the occurrence of copper in marine organisms, and digestive enzymes in coelenterates, elasmobranchs, and teleosts. However, after he developed an interest in amino acid metabolism and nutrition at the University of Illinois, Rose devoted himself wholeheartedly to this subject, which soon brought him international recognition.

Many years later, in an article titled “How Did It Happen?”, Rose described the events leading up to his first report, in 1924, on an indispensable amino acid. He was aware of the much earlier work of Osborne and Mendel in which zein of corn served as a dietary protein on which rats failed to grow until tryptophan and lysine were incorporated into the ration. That was the first clear-cut evidence that individual amino acids might be required by animals. While at the University of Texas, Rose continued his work on creatine-creatinine metabolism, as already mentioned, and, in order to study arginine as a possible precursor of these compounds, decided to prepare a casein hydrolysate and remove this amino acid as completely as possible. In view of work in another laboratory indicating that arginine and histidine were mutually interchangeable in metabolism, he changed the plan, and after preparing the hydrolysate by enzymatic action, followed by mild acid treatment, he took it to dryness, shipped it to his new scientific home in Illinois, and there removed the arginine and histidine. Rats lost weight on the treated hydrolysate but responded impressively when histidine was included in their food. However, arginine was totally incapable of replacing histidine for growth. Rose rightly considered the difference in response of the animals to the two amino acids to be little short of sensational. Having discovered that histidine is a dietary essential, he decided to investigate the nutritional role of all of the other amino acids as well. Rose realized that work with protein hydrolysates had limitations but used them in place of proteins to show that a variety of closely related synthetic compounds could not be substituted for cystine, histidine (with the exception of imidazolelactic acid), and tryptophan (with the exception of the N-acetyl compound, the ethyl ester, and indolelactic and indolepyruvic acids).

Rose then undertook investigations using mixtures of pure amino acids as the source of dietary nitrogen. Following the extremely important observation that diets containing nineteen highly purified amino acids would not support growth, his laboratory made a painstaking effort to identify the missing growth essential in proteins. After several years they finally succeeded in obtaining the unknown compound in pure crystalline form, as described in the 1935 landmark paper by McCoy, Meyer, and Rose. The structure was established as α-amino-ß-hydroxy-n-butyric acid, and the purified amino acid was demonstrated to induce maximum growth, thus constituting the first successful attempt to rear animals on a ration containing purified amino acids as the sole source of nitrogen.

In a 1979 symposium on earlier nutritional discoveries, H.E.Carter described how he became a faculty member in biochemistry at Illinois and was given what he considered a wonderful opportunity of participating in the threonine story.[1] He accomplished the chemical synthesis of the four isomers of α-amino-ß-hydroxy-*n*-butyric acid and demonstrated that only one form would support the growth of rats. This isomer, analogous in structure to D-threose and having a steric relationship to that of the other naturally occurring *L*-amino acids, was designated *L*-threonine.

Thus, the way was paved to classify other amino acids in the essential or nonessential category as judged by the maintenance of normal growth in the rat. Thorough experiments extending over the next two decades led to many important conclusions. Only ten of the twenty-two amino acids known to exist in proteins are indispensable dietary components. These are histidine, isoleucine, leucine, threonine, lysine, methionine, phenylalanine, tryptophan, valine, and arginine. With the exception of arginine, the removal of any one of these from the food of growing rats leads to profound nutritive failure, accompanied by a rapid decline in weight, loss of appetite, and eventual death. However, without arginine the animals continue to gain weight but at a suboptimal rate, thus indicating that this amino acid can be manufactured by the body but only slowly. In a study of the significance of the amino acids in canine nutrition, it was found that those amino acids that are dispensable for the growing rat are also dispensable for the adult dog, as judged by the maintenance of nitrogen equilibrium. Not unexpectedly, arginine is not needed, presumably because the rate of synthesis is adequate in the adult animal. Rose and his associates then determined the quantitative amino acid requirements by establishing the minimum amount needed to support optimal growth in the laboratory rat. In addition, many other interesting findings were made with this species using diets containing purified amino acids. For example, cystine was found to stimulate growth only when methionine was fed in suboptimal amounts and, similarly, the phenylalanine requirement could be partially replaced by tyrosine. In other attempts at substitution by related compounds, argininic acid was shown to be a poor substitute for arginine, and the *D*-isomers of phenylalanine and methionine were found to be active, whereas those of tryptophan and valine were only partly effective. Glycine, glutamate, urea, or ammonium salts were found to serve as a source of nitrogen for synthesis of the nonessential amino acids, and the effect of urea was confirmed with the ^{15}N-labeled compound. In other studies that contributed significantly to knowledge of amino acid metabolism, 5^{-14}C-labeled glutamate was observed to lead to labeled proline and arginine, thus establishing the reversibility of the known reactions in which glutamate is formed from the other two compounds. In addition, the fate of valine was investigated in the phlorhizinized dog, and three of the carbon atoms were found to yield glucose.

The experiments described briefly above provided highly important information about synthetic reactions that animals could and could not accomplish, but the ultimate objective of Rose's investigations was establishment of the amino acid requirements of the human species. In 1942 he took on this research challenge, with healthy male graduate students as the experimental subjects.[2] The diets consisted of corn starch, sucrose, butter fat from which the protein had been removed, corn oil, inorganic salts, the known vitamins, and mixtures of highly purified amino acids. The only variables allowed, other than when changes were purposely made in the amino acids consumed, were distilled water and celluflour, a product that provided bulk but had no nutritive value, nitrogen, or flavor. The only unusual component of this otherwise bland diet was a large brown candy containing a concentrated liver extract as a possible source of unknown vitamins, sweetened with sugar and flavored with peppermint oil, which provided a never-to-be-forgotten taste. Total urinary and fecal nitrogen were determined, and by the criterion of nitrogen equilibrium it was established that the twelve amino acids previously shown to be dispensable for animals were also dispensable for humans. The remaining ten amino acids were then removed from the diet one at a time; a pronounced negative nitrogen balance ensued in the case of isoleucine, leucine, tryptophan, lysine, methionine, phenylalanine, threonine, and valine. In contrast, the removal of arginine had no effect, a finding that was not surprising inasmuch as animals have a limited ability to synthesize this compound, as already stated. The results obtained with histidine, however, were most unexpected, since on a diet lacking this amino acid the subjects all maintained normal nitrogen equilibrium. Thus, only eight amino acids are essential dietary components for the adult human. However, as Rose was careful to point out, certain amino acids not necessary for nitrogen equilibrium under ordinary circumstances might become indispensable during disease or for special needs in detoxification, reproduction, or lactation.

These investigations continued into the early 1950s, resulting in sixteen papers in *The Journal of Biological Chemistry,* which C.Glen King, trustee of the Nutrition Foundation, described as a series that stands as a classic in the history of nutrition and for the benefit of humans. Importantly, the studies established the quantitative as well as the qualitative amino acid needs. Levels ranging from as low as 0.25 grams per day of tryptophan to as high as 1.1 grams per day of several other amino acids were proposed as minimal levels, with twice as much providing what was considered to be a safe margin.

In further investigations with Illinois graduate students as subjects, who were grateful in those days for the free rations, the dollar a day they were paid, and the prospect of seeing their initials in print in Rose's widely read publications, Rose made many other significant findings. A higher caloric intake is needed to maintain nitrogen equilibrium on diets containing mixtures of purified amino acids as compared to casein, for reasons that are not yet well understood. Cystine spares part of the methionine requirement, which is of significance in those parts of the world in which the latter appears to be the limiting amino acid in native diets. Similarly, tyrosine spares part of the phenylalanine requirement. In studies involving the *D* isomers of the essential amino acids, only that of methionine was found to be well utilized by the human organism. Of related interest, acetyl-*L*-tryptophan is effective but not the acetyl-*D* form, a matter of metabolic and also practical importance in view of reports by others in the literature that the acetyl-*DL* preparation might be fully utilized and thus be less costly as a dietary supplement than racemic unsubstituted tryptophan.

Another report by other investigators that could not be confirmed in the Rose laboratory, much to his relief and that of his subjects, was the claim that arginine deficiency adversely affects spermatogenesis. Thus, the only changes observed in these relatively short term studies when an essential amino acid was removed from the diet were negative nitrogen balance, a loss of appetite, and a sense of fatigue.

The above account, though necessarily brief, may give the reader an idea of how an unexpected finding made in a study on the possible relationship of amino acids to creatine synthesis eventually led to discovery of the last of the amino acids occurring in proteins and to establishment of the qualitative and quantitative amino acid requirements of animals and of the human species. Indeed, no other scientist has had a comparable record in identifying and establishing the quantitative requirements for so many essential nutrients.

Rose's findings have had many useful applications in addition to their contribution to basic knowledge. For example, they made it possible to predict the nutritional quality of a protein for human diets from the amino acid composition, rather than from animal tests, and to devise highly effective mixtures of amino acids for the intravenous feeding of surgical and pediatric patients. In 1942, partly in response to wartime nutritional problems and to the change in emphasis from acute to chronic dietary deficiencies, two important institutions were created—the Nutrition Foundation and the Food and Nutrition Board of the National Research Council. Rose played a significant role in the development and ongoing activities of both of these nongovernmental organizations, serving as a member of the Scientific Advisory Board of the former from 1943 to 1956 and as a member of the latter from 1940 to 1947. With the Food and Nutrition Board, he was instrumental in advising gov ernmental agencies on the implications of meat rationing and on minimum desirable daily allowances, as well as on the dietary usefulness of vegetable proteins. As chairman of its Committee on Protein Foods, Rose dealt with problems of supply and nutritional quality of protein foods. The results were issued in two comprehensive publications, titled *The Evaluation of Protein Nutrition with Emphasis on Amino Acid Proportionalities* and *The Evaluation of Protein Nutrition.*

PERSONAL TRAITS

Despite his many professional duties and dedication to his research and his students, Rose found time for other interests. In 1913, upon his return from Freiburg, he married Zula Franklin Hedrick, a North Carolinian. She was at his side for many happy years, and the "two Roses" exerted a wonderfully positive influence on all who knew them. They had no children of their own but instead a large "family" in which they took a personal interest—the ninety graduate students who studied under Will Rose, of whom fifty-six received the Ph.D. degree. In later years he often commented on his extraordinarily happy family life until Zula's death in 1965, his exciting professional life, and the thrill of watching his students grow into professional stature.

When asked what accounted for his longevity, Rose simply commented that he had been interested in everything all his life. He particularly enjoyed birdwatching, amateur photography, travels by automobile, and the history of science. He and his wife made numerous tours of the country by car, and until he was ninety-five he drove annually to Davidson, North Carolina, where he spoke to chemistry classes on the campus. Because of his extreme caution as a driver, it was a source of some amusement to his friends at Davidson College that at age ninety-three Rose got a speeding ticket during the drive down from Illinois. On another such visit he had to have a heart pacemaker installed, after which he got in his car and drove himself home to Illinois. Rose was the class historian at his undergraduate college, and he took a strong interest in the origins of Davidson College as well as of all the other institutions he was associated with over the years and of science in this country in general. After painstaking verification of the historical facts, he wrote fascinating and insightful accounts of the early days of American biochemistry. In "John R.Young, First American Biochemist," an introductory essay to a monograph by Young, originally published in 1803 and titled *An Experimental Inquiry into the Principles of Nutrition and the Digestive Process,*[3] Rose described a remarkable thesis submitted to the University of Pennsylvania for the degree of doctor of medicine. Considerable space is devoted to the careers of Young's teachers, including the famous Benjamin Rush, and how the youthful author "takes issue with the most revered authorities of his era—Spallanzini, Cullen, and Rush—and then proceeds to prove that each was guilty of erroneous conclusions." In "Recollections of Personalities Involved in the Early History of American Biochemistry,"[4] Rose describes his association with scientists responsible for the early development of this field and provides warm insight into their contributions and personal characteristics. The final paragraph of that article is quoted here because of the remarkable insight it provides:

> Because of the early start of the Yale laboratories, and the superior genius of Samuel W.Johnson, Russell H.Chittenden, and Lafayette B.Mendel, it is not surprising that such a large proportion of the biochemists produced in this country until approximately 1915 had their training at Yale. This would have occurred wherever Johnson, Chittenden, and Mendel happened to be located—at Harvard, Chicago, here, or anywhere. Intellects of their caliber would have found a way to do what they did regardless of the place in which fate cast their lot. In the development of a university, as in the life

> and growth of an individual, progress and ultimate attainment depend so largely upon the vision, enthusiasm, and determination of the individual participants. Perhaps, this is a truth that each one needs to remember as he carves his future out of the events and experiences of the present.

For many years Rose taught the two core biochemistry courses at Illinois and exhibited a rare talent at imparting enthusiasm about biochemistry to the undergraduate and graduate students who attended his meticulously prepared lectures. The subject came alive with his engrossing stories about the early history of the field and the personalities involved. No mention of his remarkable ability as a teacher would be complete without reference to the weekly graduate student seminars and teas at which he presided, imparting scientific knowledge and on some occasions entertaining his audience as an incomparable raconteur.

His students were somewhat in awe of the professor, perhaps wondering whether they could meet his exacting standards or could hope to emulate the seeming ease with which he succeeded in all of his professional endeavors. They learned in time that behind his somewhat reserved and formal manner was a genuine warmth and an understanding that young scientists develop their full potential only by profiting from their mistakes. His faculty colleagues also admired his many talents and sterling personal characteristics. Herbert E.Carter, who became the second member of the faculty of the Biochemistry Division in 1932, has commented that he became interested in biochemistry upon hearing Rose's lectures and states, "I recall with deep gratitude that following my graduate work in organic chemistry Dr. Rose invited me to join the Biochemistry Division. His only request of me was that I undertake the chemical synthesis of the newly discovered threonine, a project which was very fruitful in leading to my own areas of research. He enriched my life as mentor, colleague, and friend for fifty years." Carl S.Vestling, who became the third faculty member of the division, made the following remarks when he served in 1981 as moderator of "Conversations with William C.Rose," a tape-recorded group discussion:

> Absolutely uncompromising in all matters involving integrity and sincerity, he has personified many of those qualities of loyalty, unselfishness, and friendliness which mark the unusual individual. He has shown a unique blend of decisiveness and unpretentiousness in his relationships to his associates.

RECOGNITION AND AWARDS

Rose's research achievements and reputation as a stimulating and inspiring teacher brought him wide recognition and many honors. These included numerous invitations to lecture and serve as a consultant on biochemical aspects of nutrition. In addition to his work on behalf of the Nutrition Foundation and the National Research Council, as already described, he served on the Council on Pharmacy and Chemistry of the American Medical Association, the Advisory Board of the Wistar Institute, and the National Advisory Health Council of the U.S. Public Health Service. He received honorary doctor of science degrees from Davidson College, Yale University, the University of Chicago, and the University of Illinois and was elected to membership in the National Academy of Sciences. As an indication of his leadership qualities and the respect of his colleagues nationally, he was elected to serve as president of the American Society of Biological Chemists from 1939 to 1941 and president of the American Institute of Nutrition from 1945 to 1946. Other major honors included the Osborne and Mendel Award of the Institute of Nutrition, of which he was the first recipient (1949); the Willard Gibbs Medal of the American Chemical Society (1952); the Charles F.Spencer Medal of the American Chemical Society (1957);

the Twentieth Anniversary Award of the Nutrition Foundation (1961); and the National Medal of Science for 1966, conferred by the President at the White House.

On the occasion of his ninetieth birthday, Rose's former students, colleagues, and friends assembled in Urbana to join him in the celebration. He was much surprised when presented with a handsome bronze plaque announcing the establishment of the William C.Rose Lectureship in Biochemistry and Nutrition "on the occasion of his 90th birthday and presented with love, admiration and gratitude by his family of former students and colleagues." The plaque showed, in addition to his likeness and a sketch of the Noyes Laboratory, the structures of the essential amino acids and the stereochemistry and crystal structure of threonine, with a quotation and chart from his classical 1935 paper published in *The Journal of Biological Chemistry:*

> The data demonstrate conclusively that the crystalline compound is the new essential we have been endeavoring to isolate for several years. Furthermore, the experiments shown in Chart 1 represent the first successful efforts to induce growth in animals upon diets carrying synthetic mixtures of highly purified amino acids in place of proteins.

It may be noted that this prestigious national award, now administered by the American Society for Biochemistry and Molecular Biology, has been presented annually since 1978, and the awardees have all received a duplicate of the same plaque. The lectures of the recipients were originally given in Urbana to allow Rose to attend but are now presented at the society's national meetings. William J.Haines, a former student, made the following closing remarks at the celebration:

> Dr. Rose enhanced the quality of life for his students—by encouraging and supporting those things which enriched the mind and spirit. Good character was the essential raw material—good taste was the product. His per

> sonal dedication to the highest quality of performance was projected in his wise counsel. For all this, he demanded nothing in return, except excellence in performance (and behavior) of his academic children and their children, the latter whom he considers to be his academic grandchildren.

No scientist could ask for a finer memorial than having made major discoveries that contributed to the welfare of the human race and having received the respect, admiration, and gratitude of a family of former students and colleagues.

THE AUTHORS ARE GRATEFUL to Leland M.Park for material from the Davidson College Library Archives, Davidson, North Carolina, that deals with Rose's early days there and his longstanding relationship with the college; to Ellen Handler and Robert T.Chapel for useful information from the University of Illinois Archives; and to the National Academy of Sciences for a brief tribute written by Caroline K.McEuen after Rose's death.

NOTES

1. H.E.Carter. Identification and synthesis of threonine. *Fed. Proc.* 38(1979):2684–86.

2. Fifty years later the first two human subjects recalled the early experiments. J.E.Johnson and W.J.Haines. Role of amino acids in human nutrition. *FASEB Journal* 6(1992):2361–62.

3. Published in 1959 by the University of Illinois Press, Urbana.

4. *Journal of Chemical Education* 46(1969):759–63.

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Carl F. Schmidt

CARL FREDERIC SCHMIDT

July 29, 1893–April4, 1988

BY GEORGE B.KOELLE

ARL FREDERIC SCHMIDT was born on July 29, 1893, in Lebanon, Pa. Less than fifty years earlier, Rudolf Bucheim (1820–79) set up in Dorpat, Estonia, the world's first laboratory devoted to the study of the actions of drugs. Bucheim's successor, Oswald Schmiedeberg (1838–1921), subsequently moved to Strassburg where he eventually became universally regarded as the father of pharmacology; in all, he helped to train approximately 120 students from twenty or so countries. Thus, the birth of Carl F.Schmidt and that of the science of pharmacology were nearly synchronous.

Carl Schmidt's paternal grandfather, Jacob, came to the United States from Germany in 1870 for the same reason as mine: to avoid the Kaiser's draft. He settled in Philadelphia, where his dual trades were violinist and repairer of pipe organs. His son, Jacob Carl, was apprenticed to a jeweler in Reading, Pa. The son was approximately six and half feet tall, weighed 300 pounds, and was quite an athlete; he pitched for a short time for the Baltimore Orioles, and for several years thereafter was a "scorcher," or demon rider of penny-farthing bicycles. After establishing his own jewelry store in Reading, where Jacob met his wife, Ella Greth of Scotch-Irish ancestry, the family and business moved to Lebanon, Pa., where Carl was born.

Carl enjoyed the standard boyhood offered by a small town at the turn of the century. He and his elder brother, Edward, were active in the usual sports, particularly baseball, football, bicycling, fishing, and hiking. He recalled that at one time they constructed a crystal set radio receiver, but they never learned whether it functioned because there was no broadcasting station in the vicinity.

Throughout high school Carl received good grades and finished first in his class. He was then faced with the decision of what career to choose. He decided against taking up his father's trade as a jeweler as well as that of an automobile mechanic at which he had dabbled for several years. Two more plausible choices remained: he was offered a two-year scholarship at Lebanon Valley College, on the basis of his high school grades, and an appointment to West Point. He opted for the latter, but failed the physical examination (because of his limited chest expansion); therefore, he accepted the college scholarship. This was subsequently renewed. Halfway through college he met Elizabeth (Betsy) Viola Gruber, the middle member of a family of seven sisters plus one brother. They went steady for six years, then became engaged, and eventually, when Carl completed his internship, were married. They enjoyed an ideal marriage for sixty-two years until the death of Mrs. Schmidt. I remember her as a lovely person.

Just before Carl's graduation from college, the Schmidt family physician was elected state senator, which permitted him to offer Carl a scholarship to any school at the University of Pennsylvania. He accepted with alacrity and chose the medical school because of its outstanding reputation. He remained there most of the time from then on as a student, intern, faculty member, department chairman, and professor emeritus until his death at the age of ninety-four.

During his preclinical two years at Penn, Schmidt was particularly impressed by the courses of Allen J.Smith in pathology and Alfred Newton Richards in pharmacology. The latter was the youngest chairman, having arrived at Penn jut half a dozen years earlier in 1910.

Carl graduated in 1918, at the top of his class. He received a commission in the Army Medical Corps and entered a rotating internship at the Hospital of the University of Pennsylvania (HUP). There, his first assignment was to complete the blood counts of members of a crew of British and Indian seamen from a ship that had put out from Bombay a month earlier, had been refused landing at Liverpool because of an undiagnosed illness aboard, and had gone on to Philadelphia. They were found to be suffering from a vague but deadly type of respiratory infection and were wisely held in a quarantined ward at HUP. These patients proved to be the first seen in the United States in the three-wave great influenza pandemic. Before it had run its course it was responsible for several million deaths. In Philadelphia, it was the greatest medical crisis since the yellow fever epidemic of 1793. The disease was found eventually to be caused by a virus (at the time, these were only theoretical entities) with a secondary streptococcal pulmonary infection; no treatment other than palliative was available. The course of the pandemic extended well beyond the armistice of World War I on November 11, 1918. Consequently, Carl Schmidt's internship was a grim, exhaustive experience. When it ended, in August 1919, Carl considered a number of possibilities and settled for the offer of an instructorship in pharmacology from Alfred Newton Richards. He never regretted the decision.

With Carl's addition to the department, the faculty now numbered three, the other member being Oscar Plant who was an active participant in Richards's early memorable studies of glomerular filtration. In addition to his teaching duties, which he entered with enthusiasm, Schmidt initiated his own research program in collaboration with W.Benson Harrer (1923), which focused on the interrelationships between carbon dioxide, and morphine, heroin, and several other drugs, in their modification of respiration of the dog. For a short time Carl participated with Richards in the latter's renal program and succeeded in developing a method for visualizing the circulation of the kidney by means of a coverslip and overhead illumination (1924,1). However, these microscopic studies proved harmful to his eyes and he abandoned them. While casting about to decide on his next research program, an opportunity presented itself that, after some deliberation, he and Mrs. Schmidt seized upon with enthusiasm. Through Richards, he was invited to spend two years teaching at the Peking Union Medical College with the support of the Rockefeller Foundation.

Early in the summer of 1922 the Schmidts set out to China by way of Vancouver, the Great Circle route near the Aleutian Islands, Yokohama, Tokyo, Kobe, Nagasaki, and Shanghai, and from there by an exhausting train trip to Peking, or as it is now called, Beijing. They were surprised and delighted by the spectacle of the medical college itself and of the comfortable cottage on the grounds which they were to occupy for the next two years. During that time the two outstanding events that occurred in their lives were the birth of Carl F.Schmidt, Jr., on June 16, 1923, and the discovery or rediscovery along with K.K.Chen, of the sympathomimetic alkaloid ephedrine (discussed below). It is notable that a remarkable number of outstanding American medical educators and investigators spent varying periods at Peking Union Medical College during those years.[1] In spite of the many changes in government, the medical school still functions as such; my wife Win and I visited Schmidt's old laboratory there ten years ago and were de lighted to see the medical students still conducting pharmacological exercises. While a number of battles were occurring during the early twenties among the major warlords, particularly in Manchuria to the north, fortunately for the Schmidts Peking did not become involved until after they left.

Following the Schmidts' return to Philadelphia in 1924, Carl resumed his former post at Penn as assistant professor of pharmacology, then associate professor (1929), professor (1931), chairman (1939), and professor emeritus (1959– 88). In 1927 their daughter Barbara Elizabeth (deLong) was born. Just before and after his retirement from the University of Pennsylvania, Carl served as the editor of *Circulation Research* (1958–62), and was then appointed research director of the Naval Air Developmental Center at Johnsville, Pa. (1962–69), from which he finally retired at age seventy-five. He and Mrs. Schmidt moved temporarily to Florida, where he served as clinical professor of pharmacology at the University of South Florida College of Medicine in Tampa (1970–82). His research, additional activities, and honors are related in the sections that follow.

RESEARCH

Fortunately for the writer, Schmidt separated his publications into appropriate categories; here, the sequence and titles are modified slightly to allow a more chronological presentation.

CHINESE DRUGS

When Carl Schmidt embarked on his Chinese adventure, it had been suggested that he investigate the drugs in the Chinese pharmacopoeia to find any that might be of particular value. There were approximately 4,000. As a basis for starting this awesome project, he and his young Chinese colleague, K.K.Chen, selected the drugs, mostly of botanical origin, that were prescribed most frequently. (This was long before the advent of the computer, which would have simplified the task somewhat.) The results obtained with the first half-dozen drugs that were tested were discouraging. Then Chen discussed the project with his uncle, a Chinese pharmacist. The latter suggested that they investigate *ma huang,* a plant that had been tasted by the Emperor Shen Nung more than 5,000 years earlier and placed in the "medium class." Chen prepared an aqueous extract of the crude drug which was injected into a dog at the conclusion of a student practice experiment. To their amazement it produced a prolonged rise in blood pressure (all other bo-tanical extracts produced only hypotension or nothing.) Within a few days Chen isolated the active principle; a search of the literature revealed that it had been found nearly fifty years earlier by the Japanese investigator N.Nagai and named ephedrine. However, only a few short notes had been published on its actions. Chen and Schmidt conducted a thorough investigation of ephedrine, which showed it to be an orally active, long-acting sympathomimetic amine of a structure somewhat similar to that of epinephrine (1924,2). As a result, ephedrine and similar synthetic compounds were introduced into Western medicine and became widely employed for their cardiovascular, bronchodilator, and other actions (1930).

RESPIRATORY AND CIRCULATORY REFLEXES

After his return from China, along with excursions into other areas, Schmidt's major investigative work was divided between studies of respiratory and circulatory reflexes and the cerebral circulation.

In 1867 von Bezold and Hirt[2] reported that the injection of veratrine in rabbits caused bradycardia, hypotension, and apnea. This observation was practically overlooked until it was rediscovered by Jarisch in 1940,[3] when it was named the Bezold-Jarisch reflex. Further investigation of this phenomenon by Corneille F.Heymans of Ghent and his collaborators[4] led to the discovery of both chemoreceptors and pressoreceptors in the region of the internal carotid artery that produced marked reflexes in the respiratory and circulatory systems.

Carl Schmidt and his colleagues became involved in the same field. The latter group included principally Julius H. Comroe, Jr., and Domingo M.Aviado, Jr., as well as Aurilio Cerletti, Robert D.Dripps, Jr., Paul R.Dumke, Marilyn E. Hess, Orville Horwitz, Werner Kalow, T.H.Li, and others. The laboratories of Heymans and Schmidt became the preeminent ones in the world for their work in unraveling these complicated systems.

To summarize this research, it was shown that nicotine and other drugs also elicit the same reflexes. They were found to arise from one or both types of receptor not only in the carotid area but from the aortic body (the equivalent of the chemoreceptors of the carotid body) and aortic arch (which resembles the pressoreceptors of the carotid sinus), as well as from the upper respiratory tract, pulmonary venous area, coronary artery, and cardiac muscle and surface. Much of the early work was reviewed by Schmidt (1945,1) and by Aviado and Schmidt (1955); it has recently been updated by Aviado.[5] These studies represented not only a major contribution to basic physiology, but were of great importance in the work of Schmidt and his colleagues (1943,1) in aviation medicine during World War II and in Schmidt and Lambertsen's (1965) later involvement in the physiological aspects of space travel as discussed below.

CEREBRAL BLOOD FLOW

Carl Schmidt's interest in the control of respiration directed him to studies of the influence of cerebral blood flow on respiration (1923, 1928), in which measurement of the perfusion function was initially based on venous outflow, giving way eventually to the use of a cooled or heated thermocouple (1934). In 1943, with Paul Dumke, he made the first quantitative and reliable measurements of cerebral blood flow in the macaque monkey by intercalating into the arterial input a bubble flow meter (1943,2). Two years later he extended his measurements to cerebral oxygen consumption under a variety of experimental conditions (1945,2). While participating in those studies, Seymour Kety, who had joined the laboratory in 1943, devised a technique for measuring the blood flow through the human brain, utilizing the exchange of an inert gas between the perfusing blood and the brain. With Carl Schmidt he tested the technique in the monkey and calibrated it against the bubble flow meter (1945,3). The Kety-Schmidt technique was applied in 1948 to normal volunteers for the first measurements of blood flow and energy metabolism in the human brain (1948,7) and then to a number of physiological and pathological states (1948,2–5).

RENAL CIRCULATION

In addition to his early work with A.N.Richards, cited above, Carl Schmidt subsequently investigated the renal circulation in collaboration with J.M.Hayman, Jr. (1929), Arthur M.Walker (1937), and John H.Moyer, Hadley Conn, and others (1950).

MISCELLANEOUS

In collaboration with Alfred E.Livingston, Schmidt published a number of studies on the actions of morphine and related drugs (1933). He and Harry D.Bruner (1947) studied the bloodflow in the bronchial artery. With Christian J. Lambertsen, P.L.Bunce, and David L.Drabkin (1952) he measured the relationship between oxygen tension and hemoglobin oxygen saturation.

Although I was Schmidt's first graduate student, I never had the privilege of working in his laboratory. He graciously allowed me to submit some work I had completed during World War II in collaboration with Alfred Gilman at the Medical Research Laboratory, Edgewood Arsenal, as a thesis.

AVIATION AND SPACE MEDICINE

With the advent of World War II, Schmidt became deeply involved in aviation medicine. Here his aim was the development of means for improving the performance of pilots under extremes of gravitational and anoxic stresses (1943). He returned to the same area many years later at another level, after he became research director of the Naval Air Development Center, which contained the world's largest human centrifuge. He worked closely with John Glenn, Scott Carpenter, Wally Schirra, L.Gordon Cooper, Virgil Grissom, Frank Borman, Neil Armstrong, and the other pioneer astronauts in showing them how to deal with the tremendous G-forces they would encounter (1965).

HONORS AND MEMBERSHIPS

The list of Schmidt's societies and offices, editorships, committees, lectureships, and honors is a long one. Pharmacology on the international scene began as the Section on Pharmacology (SEPHAR) of the International Union of Physiological Sciences (IUPS), which in turn belonged to the International Council of Scientific Unions (ICSU). (At the annual meetings of the American Pharmacological So ciety, Schmidt used to delight us all with accounts of the activities of these acronymical associations.) Among all the pharmacologists of the world, Schmidt was elected the first president of SEPHAR, in 1959. When pharmacology achieved independent status in 1965 as IUPHAR—the reader can work out that one—Schmidt, by then retired, was elected its honorary president.

Schmidt belonged to the American Society for Pharmacology and Experimental Therapeutics (vice-president, 1940– 42; president, 1948–50). He was also a member of the National Academy of Sciences (elected 1949); the American Academy of Arts and Sciences; American Heart Association; American Physiological Society; Society for Experimental Biology and Medicine; Association of American Physicians; American Association for the Advancement of Science (fellow); Physiological Society of Philadelphia; College of Physicians of Philadelphia (fellow); Penn Valley Association (president, 1944–46); Medical Association of the Argentine; Merion Cricket Club; and others. Schmidt was managing editor of the *Journal of Pharmacology and Experimental Therapeutics* (1940– 42) and *Circulation Research* (1958–62) and associate editor of *Chemical Abstracts* (1942–48). The list of his memberships on important international and national committees is extensive.

Among Carl Schmidt's many honors were honorary degrees from his alma maters, Lebanon Valley College and the University of Pennsylvania, and from Charles University in Prague. In 1984 he received the Distinguished Graduate Award of the School of Medicine of the University of Pennsylvania. In 1962 he was the sixth recipient over many years of the Schmiedeberg Plakette of the German Pharmacological Society. The citation that accompanied the award captured the man: "for magnificent contributions to ser vice for pharmacology and in recognition of his spirit of magnanimity and great humanity."

GENERAL

The designation "clinical pharmacology" refers to a relatively young branch of the science that concerns the investigation of new drugs in human subjects after their efficacy and safety have been established by animal studies. Although I never heard either of them use the term, Schmidt and his predecessor Richards (1971) were among the pioneers in this field. Most of their junior associates were residents in internal medicine, anesthesiology, and other medical specialties who spent one or more years in the Department of Pharmacology learning the principles of pharmacological research. They then returned to their clinical departments to put these principles into practice.

In addition to their extended stay in China early in Carl Schmidt's career, the Schmidts did considerable traveling for his participation in international meetings, lecturing, and service with the Unitarian Medical Mission to Germany in 1948. Their most extensive journeys were taken shortly before his retirement from Penn; between July 1955 and February 1956, they took four trips covering some 50,000 miles. On those occasions their destinations included the Philippines, Japan, Taiwan, Hong Kong, Australia, New Zealand, Indonesia, Singapore, Thailand, Vietnam, Burma, India, Pakistan, Sudan, Rhodesia, Uganda, Kenya, Lebanon, Syria, Israel, Jordan, Egypt, Madagascar, Turkey, Italy, Spain, France, Greece, and Monaco.

In spite of his numerous other activities related above, Schmidt always considered his primary responsibility to be teaching. His lectures were models of clarity, precision, and enthusiasm, and inspired his younger colleagues to follow suit. As a result, pharmacology was generally acknowledged by the students to be the best course in the medical school.

In 1976 his old department established the Carl F.Schmidt Honorary Lectureship. Schmidt gave the first annual lecture of the series. Subsequent lectures, which are followed by dinner and an evening of good fellowship, were presented by his former associates and by distinguished pharmacologists from throughout the world. He and Mrs. Schmidt attended all up to the time of their deaths. The Schmidt lectures will continue in perpetuity.

After the death of Mrs. Schmidt in 1982, Carl Schmidt lived with his son, Carl, Jr., and his wife Betty. Shortly after Schmidt's death, at the age of ninety-four, the tragic premature death of his son followed. Betty ministered to them both, with deep affection and devoted care to the end. In addition to his daughter, Barbara Elizabeth deLong, Schmidt left five grandchildren and two great-grandchildren.

I AM ESPECIALLY GRATEFUL to Mrs. Carl F. (Betty) Schmidt, Jr., for her time and expertise in assembling the considerable volume of her father-in-law's unpublished writings and correspondence, upon which most of this memoir is based. Portions of this memoir are reproduced from a shorter one published by the author in the *Transactions of the College of Physicians of Philadelphia* (ser. 5, vol. 11, no. 3, pp. 261–65, 1989), for which permission has been granted. Mrs. Ava M.Evans typed the manuscript.

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John C Sheehan

JOHN CLARK SHEEHAN

September 23, 1915–March 21, 1992

BY E.J.COREY AND JOHN D.ROBERTS

OHN C.SHEEHAN WILL long be remembered for having solved one of the most formidable and prominent problems in synthetic chemistry of the twentieth century, the chemical synthesis of the penicillins, and for helping to lead organic chemistry to new heights in the post-World War II era. He made major contributions to the Massachusetts Institute of Technology, his academic home for four decades. His teaching and research were instrumental in rejuvenating chemistry and maintaining its excellence at the institute, which also received enormous financial returns from his successful work on synthetic penicillins. His fundamental research provided the chemical base for the development of modern semisynthetic penicillins, which have saved countless human lives.

John C.Sheehan was born on September 23, 1915, in Battle Creek, Michigan. His father, Leo C.Sheehan, then sports editor and police reporter for *The Battle Creek Enquirer,* and his mother, Florence, were described in the news article marking the birth as “prominent in the younger society circles of Battle Creek.” In addition to Irish forebears, the family had a substantial Yankee background; some sixteen known ancestors dated from revolutionary times. Florence Sheehan was a brilliant woman and skilled genealo gist who did professional work in the field and later became the Michigan registrar for the Daughters of the American Revolution. John's father left home at fifteen with only an eighth-grade education to "see the world" and found work as a reporter in San Francisco, where he witnessed the 1906 earthquake. Leo Sheehan was a skilled writer and progressed with *The Battle Creek Enquirer* to city editor and then to managing editor. He later functioned as a ghostwriter for Frank Murphy, once governor of Michigan and a Supreme Court justice.

John's grandfather, John W.Sheehan, was an outstanding lawyer who had business dealings with William Jennings Bryan, the famous political leader and orator. His maternal grandfather, Nathaniel Y.Green, was a bank manager and maintained an interest in nature and learning. A skilled amateur taxidermist, he had a large collection of birds. He greatly stimulated John's interest in science by giving him a Zeiss microscope with an oil-immersion lens and also introduced John to the curator of the local museum who helped him with several small science projects. John's grandfather had a telescope for astronomical observations and took John to meetings with the local group of astronomy buffs.

As with many future chemists in their early years, John progressed from a chemistry set to a basement laboratory and was fascinated by explosives and rocketry. He was a natural experimentalist with skillful hands. He also built model airplanes, and one with a delta wing won a first prize for longest flight time in the self-design class. He was also a zealous competitor in other activities. The Battle Creek newspaper reported him as the premier marble shooter of his grade school, representing the school in the city championships; as winner of the city yo-yo championship with a perfect score, as judged by the world's champion yo-yo player of the time; as a finalist in a Boy Scout election picking a mayor for a day; as having been injured in a high school football game; and as Battle Creek College's No. 1 tennis player, as well as a participant with his brother Joseph in numerous tennis doubles tournaments. However, not all was rosy in John's younger years. His father had a long struggle with cancer and died at age fifty.

John's brother, Joseph, most often known as Joe, later had a distinguished career as a professor of psychology at the University of California. As an adolescent and young adult, Joe suffered intensely from stuttering and with his wife, Vivian, herself an eminent speech pathologist, devoted their lives to developing and implementing very successful training programs for relieving speech defects. John and Joe's younger brother, David, now retired, was engaged in manufacturing in Battle Creek.

John was raised as a Catholic and attended Catholic grade schools. However, in later life, both he and Joe were not particularly religious. John attended Battle Creek College with a double major in chemistry and political science. He graduated with honors as valedictorian of his class and won a state college scholarship for graduate work in any field of his choice. He elected to study chemistry at the University of Michigan.

John received the Ph.D. degree in 1941. His thesis supervisor was Werner E.Bachmann, then engaged in the historic first total syntheses of the steroid hormones equilenin and estrone. John's research, on the synthesis of phenanthrene derivatives, was in Bachmann's other major field of this period, the investigation of potential carcinogenic hydrocarbons following along the lines of J.W.Cook, with whom Bachmann had worked earlier. John became a superbly trained experimentalist in the grand tradition of Bachmann and Bachmann's illustrious teacher, Moses Gomberg, the founder of the field of stable carbon free radicals.

Shortly after receiving his Ph.D., John married Marion

Jennings, who had graduated with him from St. Philips High School in Battle Creek. Earlier in 1941, Bachmann asked John to work with him as a postdoctoral fellow but did not specify the area of research. After John finished writing his thesis, Bachmann informed him that it would be national defense research on the synthesis of cyclotrimethylenetrinitroamine, code named RDX. This substance was known to be a very brisant explosive, but no commercial or large-scale synthesis was available for its preparation. Bachmann and Sheehan developed a procedure for synthesis of RDX by nitration of hexamethylenetetramine that they ran in the Michigan laboratories on a scale of more than a kilogram. One wonders whether the university administrators were aware of the substantial hazard involved in this project. John displayed a mixture of courage and prudence, wearing not only the usual safety glasses and laboratory coat but also a heavy towel wrapped around his neck as protection from flying glass.

While purifying the reaction product, John isolated cyclotetramethylenetetranitroamine, an excellent explosive in its own right. The Bachmann-Sheehan process was scaled up by Tennessee Eastman, and the RDX so produced was used with great success by the United States for the remainder of the war (often in mixtures with TNT). The rapid completion of his part of the RDX project enabled Sheehan to accept a position as a research chemist at Merck and Co. in Rahway, New Jersey, starting in October 1941 under the direction of Max Tishler. John participated in several key synthetic projects, where research was needed for scale-up to the pilot plant and beyond. One was a new preparation of calcium pantothenate; another was removal of immunogenic materials formed as by-products in fermentation broths for the production of streptomycin; and a third was isolation and purification of penicillin. Out of the latter came processes for the separation of penicillin G from other penicillins by formation of a crystalline salt with N-ethylpiperidine and subsequent exchange to the sodium salt with sodium 2-ethylhexanoate. One of his laboratory associates at Merck was Donald J.Cram, who later shared with C.J.Pedersen and Jean-Marie Lehn a Nobel Prize in chemistry for work on inclusion compounds.

John's work at Merck drew very favorable attention from Homer Adkins, a consultant to the company and renowned professor of chemistry at the University of Wisconsin. Adkins recommended John to Arthur C.Cope, who had been appointed head of the Department of Chemistry in 1945 by the president of MIT, Karl T.Compton, on the advice of a friend and wartime associate, Roger Adams of the University of Illinois. John joined the MIT faculty as an assistant professor in 1946, at a salary he said was half his compensation at Merck. At the same time, John D.Roberts and C. Gardner Swain were brought on board by Cope, and in the next few years MIT under Cope's vigorous leadership was propelled into the front ranks of U.S. chemistry.[1]

Within just four years at MIT John Sheehan became known as one of the most creative and dynamic synthetic organic chemists in the world by his development of new methods of synthesis of peptides (carbodiimide coupling and phthaloyl *N*-protection), three new syntheses of ß-lactams, the first synthesis of the penicillin ring system, and isolation and identification of a number of important new natural products.

His research on penicillins, initiated in 1948, was remarkable for several reasons. It came on the heels of the large wartime U.S.-British project of research on penicillins (involving more than a thousand chemists), which failed to develop a chemical synthesis and produced instead an ominous summary of a great many failed attempts. By 1948 penicillin G was produced in abundance commercially by fermentation, and no other leading chemist saw any reason to take on the apparently hopeless task of synthesizing such an unstable molecule. In John's own colorful language, the chemical synthesis of penicillin was like "placing an anvil on top of a house of cards." Years of determined and skillful effort were rewarded by success in 1957 when John and his group completed the first synthesis of penicillin V. One of the intermediates in the synthesis was 6-aminopenicillanic acid, a substance that Sheehan recognized could be used to prepare a variety of penicillins other than naturally occurring ones. This prescient conception turned out to have great medical value because it made possible variations in the penicillin structure that could be used to combat the tolerance developed by bacteria to particular forms of the antibiotic. The Sheehan synthesis of 6-aminopenicillanic acid is impractical for making these superpenicillins, but the amino acid is available in quantity by fermentation. John later told of his involvement with penicillins in his book, *The Enchanted Ring—The Untold Story of Penicillin,*[2] which also includes an account of the complex legal skirmish over the Sheehan-MIT patents on penicillin synthesis. Although the legal battle was protracted MIT eventually received almost $30 million in royalties from the Sheehan patent. MIT established the John C.Sheehan Professorship of Chemistry in October 1992.

Sheehan retired in 1977 and was named professor of chemistry emeritus and senior lecturer.

John Sheehan's major research achievements are described in some 150 synthetic papers and forty patents that cover not only penicillin but also peptides, antibiotics, alkaloids, and steroids. For his scientific contributions, John received several high honors, including the American Chemical Society Award in Pure Chemistry (1951), election to the Na tional Academy of Sciences (1957), the American Chemical Society Award for Creative Work in Synthetic Organic Chemistry (1959), the John Scott Award for inventors benefiting mankind (1964), the Outstanding Achievement Award of the University of Michigan (1971), and honorary doctorates from Notre Dame (1953) and the Stevens Institute of Technology (1980).

Sheehan spent 1953–54 in London as scientific liaison officer for the Office of Naval Research. From 1961 to 1965, he served the President's Science Advisory Committee as consultant, member of the limited war panel, and chairman of the committee on chemistry and biology. In the latter capacity he was involved in technology transfer negotiations with the Japanese government. He later had a close association with H.Umezawa, director of the Institute of Microbial Chemistry, and made many trips to Japan in connection with his interests in antibiotics and other pharmaceuticals. Sheehan played an active role in Organic Syntheses, Inc., by serving as editor-in-chief of volume 38 and then for many years as a member of the Advisory Board and Board of Directors. He was also engaged in the affairs of the American Chemical Society and, among other activities, served on its Board of Directors for eight years. Sheehan was a member of the National Research Council's Committee on Protection Against Mycotoxins (1982–84) and the Committee on Commercial Airport Security (1988–92). Ironically, in the latter activity he was concerned with the detection of explosives, such as RDX, on airplanes and in luggage and shipments.

Besides his work at MIT, Sheehan was involved in two rather unusual research activities of possible interest to those seeking alternative research support mechanisms. Thus, in 1958, the Schering Corporation set up the Research Institute for Medicine and Chemistry in Cambridge close to

MIT in appreciation of the contributions of M.M.Pechet to the company's clinical work. As director, Pechet invited D.H.R.Barton, then at Imperial College, London, to supervise a small research group. At Barton's suggestion, Sheehan also spent several years in a similar capacity. The first project of the institute was to achieve a synthesis of aldosterone, a goal that Sheehan hoped to reach by degradation of a steroidal alkaloid, but was better prepared by a nitrite-photolysis procedure developed by Barton. Subsequently, Sheehan extended his research on water-soluble carbodiimides at the institute for several years. Later in 1970 he was able to build on the results of a government-supported research program at Arthur D.Little Co. on cannabinoid derivatives as potential chemical warfare agents to set up a program aimed at the use of such derivatives in the treatment of nausea resulting from cancer chemotherapy. This work was carried out at a for-profit company called SHARPS Associates and the nonprofit John C.Sheehan Research Institute. The former was supported by contracts with pharmaceutical companies and the latter by research grants, as from the National Institutes of Health. The combined operation got off to an excellent start, but Sheehan was later greatly disappointed by subsequent management problems.

The present authors, one as a graduate student in the Sheehan research group and the other a professorial colleague, were greatly impressed by John's ingrained cheerfulness, optimism, and humor, as well as his broad chemical expertise. His 1948–50 research group included Gerald D. Laubach (later president of Pfizer, Inc.), Robert T.O'Neill (later a successful research chemist at Merck and a private businessman), Barry M.Bloom (president of Pfizer Research), Ajay K.Bose (professor of chemistry at Stevens Institute of Technology), David Johnson (research director at Bristol

Myers), E.J.Corey (later professor of chemistry at Harvard), and Kenneth Henery-Logan (who later participated in the successful synthesis of penicillin). This was no collection of shrinking violets, and they all found as much enjoyment in exchanges with John as in the research adventure itself.

John Sheehan was a man who made friends easily and had many close friends at home as well as abroad—the results of his extensive international travels. He was an avid competitor in all things, a trait that was particularly evident to those who played tennis with him. He enjoyed boating, was a close follower of politics and sports, a marvelous raconteur, and a lover of good stories told by others, as well as an entertaining dinner companion. John is survived by Marion, his lovely and devoted wife of more than fifty years; a brother, David Sheehan of Battle Creek, Michigan; three children, John C., Jr., of Denver, David E. of Key Biscayne, and Elizabeth (Betsy) S.Watkins of Sauderstown, Rhode Island; and six grandchildren.

Sheehan's career was multifaceted, with achievements that demonstrated an unusual ability to focus on chemical problems of great practical importance, the courage to pioneer against strong odds, and an unflagging determination to succeed.

THE AUTHORS ARE VERY GRATEFUL to Marion Sheehan; her sister-in-law, Vivian; Professor Ajay K.Bose; and Sir Derek Barton for providing valuable background material for this biography.

NOTES

1. For more on the Cope era at MIT, see J.D.Roberts, *The Right Place at the Eight Time,* pp. 53–59. (Washington, D.C.: American Chemical Society, 1990), and J.D.Roberts and J.C.Sheehan, "Arthur C. Cope," *Biographical Memoirs,* vol. 60, pp. 17–30 (Washington, D.C.: National Academy Press, 1991).

2. J.C.Sheehan, *The Enchanted Ring—The Untold Story of Penicillin* (Cambridge, Mass.: MIT Press, 1982).

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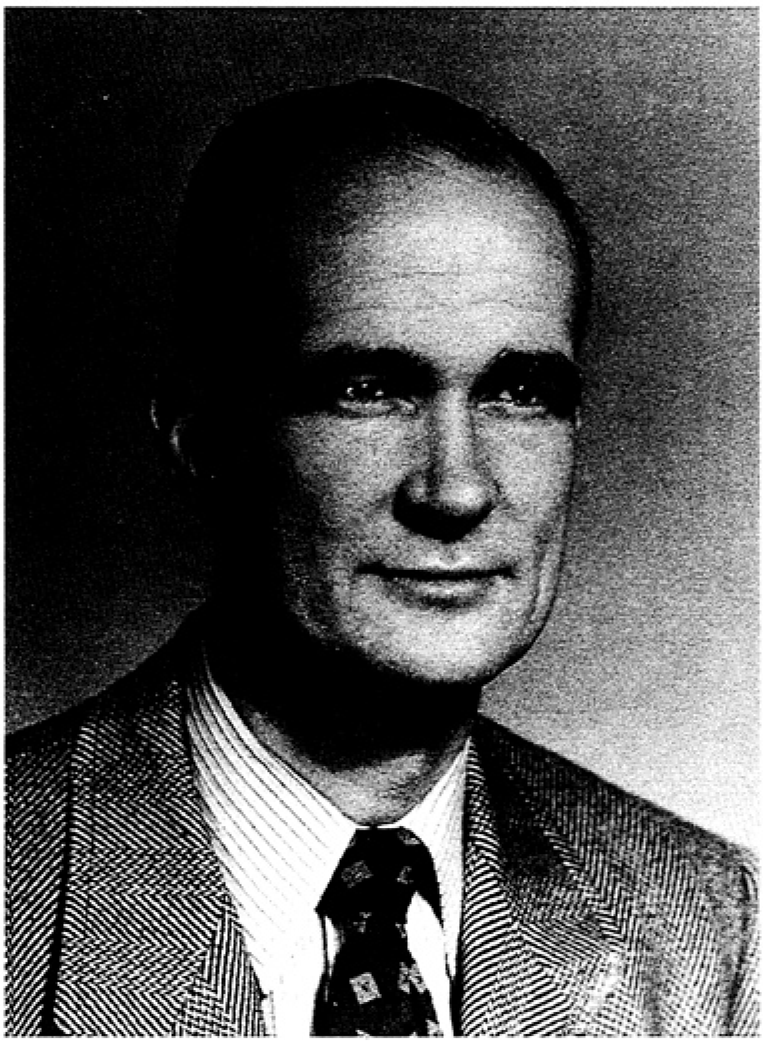


W. Shockley

WILLIAM BRADFORD SHOCKLEY

February 13, 1910–August 12, 1989

BY JOHN L.MOLL

ILLIAM BRADFORD SHOCKLEY WAS A major participant in the physical discoveries and inventions that are the basis of the transistor era and the twentieth-century electronics industrial revolution. Transistor circuits are basic to almost all of our technological advances.

Shockley was born in London, England, on February 13, 1910. His parents were Americans. His father, William Hillman Shockley, was a mining engineer, and his mother, the former May Bradford, had been a federal deputy surveyor of mineral lands. In 1933 Shockley married Jean Alberta Bailey. They had two sons, William and Richard, and a daughter, Alison Lanelli. They divorced in 1955, and in the same year Shockley married Emmy Lanning.

When Shockley was three years old, the family returned to the United States and settled in Palo Alto, California. His parents considered that they could give their son a better education at home than in the public schools. They therefore kept him out of school until he was eight years old. His mother taught him mathematics, and both parents encouraged his scientific interests. Professor Perley A.Ross, a Stanford physicist and neighbor in Palo Alto, exerted an especially important influence in stimulating his interest in science. Shockley was a frequent visitor at the Ross home, playing with the professor's two daughters and becoming a substitute son.

When he entered high school, Shockley spent two years at the Palo Alto Military Academy. He then enrolled for a brief time in the Los Angeles Coaching School to study physics. He finished his high school education at Hollywood High, graduating in 1927.

He started his college education the same year at the University of California at Los Angeles. After a year at UCLA, he entered the California Institute of Technology in Pasadena. He had a number of outstanding teachers at CalTech. William V.Houston taught the introductory theoretical physics course. In addition, Richard C.Tolman and Linus Pauling were professors. Shockley earned his bachelor of science degree in physics in 1932.

Shockley went to MIT on a teaching fellowship. He obtained his Ph.D. degree in 1936. His thesis title was "Calculation of Electron Wave Functions in Sodium Chloride Crystals." The solid-state physics he learned at MIT proved to be the basis of his contributions to physics and electronics.

An account of an acquaintanceship between Fred Seitz and Shockley has kindly been made available by Professor Seitz. This account covers their college days and includes items up to Shockley's later years. I am including it with his permission in an almost unedited form:

> One of Slater's students was William Shockley whom I had known since [my] undergraduate days. Among other things, Shockley demonstrated, with the use of an empty lattice model, that the precise determination of band gaps for real crystals with the use of the cellular method would require highly evolved techniques. Shockley went on to join the Bell Telephone Laboratories where, along with Bardeen and Brattain, he played such an important role in the invention of the transistor after World War II.

While on the Stanford campus in August of 1932 and in the initial phases of making plans to return to New Jersey, Mrs. Ross approached me to say that Mrs. Shockley, the mother of William Shockley, had called her from Hollywood to say that her son had received an appointment at the Massachusetts Institute of Technology and was planning to drive east with his De Soto convertible. Would I care to drive with him and share the costs? I agreed. Thus began one of the most carefree two-week periods I have enjoyed in the intervening decades.

Shockley, I soon realized, was then strongly influenced by the Hollywood culture of the time, fancying himself to be a cross between Douglas Fairbanks, Sr. and Bulldog Drummond with perhaps a dash of Ronald Coleman. Moreover, he accepted pronouncements of the Hollywood stars on political, social and economic issues with the same degree of seriousness that I would have taken of those of Governor Rolph or President Hoover. Moreover, he had a loaded pistol in the glove compartment. I was then handy with a rifle but looked askance at the pistol. Shockley's special air and the pistol eventually brought him to grief as he drove through Newark, New Jersey on the very ancient Route 1 of that day, after leaving me off at Princeton where he spent a day. He was spotted by the Newark police who took him to be a suspicious character. The pistol clinched the matter. He never gave me the details but he evidently had a difficult interview with the Newark judge who [made] very imaginative use of the English language.

We selected the southern route through Arizona, New Mexico, Texas and Arkansas, eventually reaching the Lincoln Highway in Ohio. Along the way, we visited Carlsbad Caves and the Kentucky Caves including the then famous Floyd Collins Crystal Cave in Kentucky. This had been discovered by Collins who, somewhat later, had been entrapped and killed in a further attempt at cave exploration—one that was featured for days in the national press as rescuers tried in vain to reach him before he died. His presumed body was on exhibit in a glass covered coffin, although I must admit that the object we witnessed looked surprisingly like a dummy in a very inexpensive clothing store. The guide, however, assured us on its authenticity declaring, “He was, as you can see, a very handsome man.”

We also encountered torrential rains in Texas after leaving Carlsbad Caverns in New Mexico. The rains were so heavy that the highway was obliterated. We parked the car off the highway at a rise and spent the night wandering through the desert. This gave Shockley an opportunity to fire his pistol on several occasions when he decided to ward off the possibility of meeting serious danger from a group of coyotes howling in the distance. It also caused a gasoline station attendant at a nearby store to declare to us the next morning that the local police had been alerted to the fact that two desperadoes were loose in the area. He suggested we keep a lookout.

Both of our careers were almost cut short by an incident that occurred as we were traversing the hills of Kentucky in the early evening on a narrow, two-lane road, one lane in each direction. I was driving and had a drop-off on my right. As we rounded a curve we found speeding down toward us two trucks which were racing one another and taking up both lanes. This race was evidently being run in a playful manner, typical of the spirited, young hill folks. By the grace of the Lord, I had just enough shoulder to squeeze by the oncoming truck with perhaps an inch to spare. To the best of my knowledge I have never been closer to instant death than in those few seconds.

One might ask which, if any, of the most prominent attributes which would characterize Shockley later in life, when he was a famous scientist, were evident at this early stage of his career. It was clear from the start, of course, that he was unusually intelligent. His later fame, and indeed notoriety, rested upon two characteristics. First and foremost was the ability to seek out the core issues in a scientific problem and bring them to the surface in a dramatically clear way with the use of either theoretical or experimental measures—an ability which in some ways matched those of Enrico Fermi although in a different area of physics. In this respect, his most creative period occurred when he was at the Bell Laboratories and between about 1940 and 1955. Having known him quite early in his career, I was never surprised at this aspect of his creativity.

Later on he attempted to apply his ability to the study of differences in the characteristics of ethnic groups, particularly differences in intelligence as measured in various ways. While objective studies of physiological or other differences in such groups clearly have a place in science, it is quite a different matter to advocate at the same time that any conclusions drawn from such work be used as a basis for actions, forceful or otherwise, with respect to eugenics. Here, unfortunately, Shockley became mired in a morass of his own making because of his second characteristic. He apparently was unable to place himself in the shoes of others and thereby understand that advocating strong eugenic measures in a highly diverse society is bound to be highly disruptive. Yet he advocated that such a course be followed to the very end of his life. Along with this was an unwillingness to admit that methods of analysis which work so well in relatively clear-cut physical systems may become highly controversial and indeed counterproductive in other circumstances.

I saw an inkling of this second characteristic at an early period but did not take it seriously then. He was inclined to believe that society should be governed by what one might regard as an intellectually elite group, not very well defined at this early stage, rather than by majority decisions as in a democratic society. Unlike many other intellectuals, he never coupled this belief to any ongoing political system, Marxist or Fascist. He was guided entirely by his own internal sense of logic.

Early in 1933 William Hansen received an appointment at the Massachusetts Institute of Technology. Since Shockley wanted to drive west and Hansen wanted to participate in the advanced summer physics lecture series at the University of Michigan, the three of us started west together. The school at Michigan had not yet begun when we arrived but Robert Bacher, who held a postdoctoral position and was widely known for the book he and Samuel Goudsmit had published on atomic spectra, took us in hand and gave us an excellent tour of the department. At that time, he was in the midst of studying the hyperfine structure of atomic spectra derived from nuclear magnetic moments. Bacher was destined to play major roles in the future of American science. Five years later, then at Cornell University, he would, with Hans Bethe, prepare a series of excellent overviews of the status of nuclear physics which contained much original material. Ten years later he would play one of the central roles in the development of the bomb at Los Alamos followed by a period as a member of the Atomic Energy Commission. His career would be climaxed by an appointment to the California Institute of Technology in which he would serve as faculty member and one of President Lee DuBridge's principal colleagues.

Our return trip to the West Coast was routine with one notable exception. Shockley had switched the car registration from California to Massachusetts during the winter. Whereas we had been greeted warmly wherever we went on the way east with California plates, there was conspicuous hostility west of the Mississippi River. The people there were prepared to blame the irresponsible easterners for the great economic depression they were experiencing.

Throughout his life Shockley maintained an interesting set of hobbies. Before I knew him he had been interested in slight of hand parlor tricks and maintained a great deal of skill over the years. He added a great deal of side interest to his student years at MIT by using his imagination and sense of fun to keep the staff there on edge with subtle or not too subtle tricks.

Many of my friends in industrial laboratories, being free of the academic responsibilities for teaching, committee assignments and the like, managed to find time for hobbies along with their creative work in the laboratory. Shockley was no exception. At one point this involved hand over hand rock climbing, at another far more sophisticated rope climbing including semi-professional assaults on some of the more difficult peaks in the vicinity of Mont Blanc.

In addition, he had one highly solitary hobby that displayed a special side of his makeup. He enjoyed establishing confined ant colonies in large glass containers. Part of the art he cultivated was to train the ants to take circuitous routes in seeking food and returning to their storage base. This frequently involved the construction of delicately balanced seesaws of straws which would tilt under the weight of an ant. The ant, near its home base, would climb on the lowered end of such a straw and, in moving past the fulcrum, would cause the straw to tilt so that the ant could reach the food supply. Once the ant left the straw, the latter would return to its original position. This would compel the ant to find an alternate path back. The return path usually involved one or more such challenging seesaws. Shockley could spend hours at the game.

In the latter part of the 1980s, a dean of engineering at one of the large South African universities, who frequently visited Stanford University where Shockley spent his later years, invited the latter to visit South Africa in order to give a speech commemorating the invention of the transistor. Knowing of Shockley's controversial interest in studies of differences in ethnic groups, the dean emphasized that any presentation of his views on such matters would be completely inappropriate because the South African government was trying to find a way out of the morass it had entered into in setting up the laws concerning apartheid. Alas, when Shockley came to give his lecture he focused not at all on the transistor but on his personal views of the relative merits or demerits of various ethnic groups much to the great embarrassment of the audience and the dean. Shockley used a substantial part of his time in South Africa studying the trainability of local ants.

When Shockley graduated from MIT he took a job at Bell Telephone Laboratories to work with Clinton J.Davisson.

Shockley's first project involved the design of an electron multiplier tube.

He quickly became involved in solid-state physics research. In 1939 he proposed a kind of "field effect transistor" that used wires imbedded in CuO_2. The device as proposed has never worked, but a field effect device (invented in about 1960 by other people) has become the mainstay of the ultra-large-scale integrated circuit. The proposal that Shockley made in 1939 coincided with the laboratories' goal of replacing the mechanical relays and vacuum tubes in the telephone exchange.

Shockley turned to military projects during World War II. He was first employed on the electronic design of radar equipment at Bell Labs. He then became research director of the Antisubmarine Warfare Operations Research Group set up by the Navy Department at Columbia University. He was in the Naval Operations Research position from 1942 until 1944. The new field of operations research treated military objectives, such as optimum patterns for dropping depth charges against submarines and the time of aerial bombardments, as problems subject to scientific methods of analysis. From 1944 until 1945 he was an expert consultant to the office of the Secretary of War.

In 1945 Shockley returned to Bell Labs. Mervin Kelly, president of the labs, had decided to set up a research group to understand semiconductors from a basic physical viewpoint. There seemed to be a real possibility that semiconductors could be used as electronic elements. Russell Ohl had a small laboratory at Bell Labs in Holmdel, New Jersey, where he "manufactured" point contact detectors for radar purposes during World War II. Ohl had an insatiable curiosity, and, in addition to supplying the radar detectors, he discovered numerous unique properties of the silicon crystals that were available to him at the time. He demonstrated the photoelectric effect at a p-n junction, as well as other properties of crystals in relation to point contact detectors. These properties were not understood, except in an empirical sense. A research group was formed under the supervision of Shockley and Stanley Morgan, a chemist. Shockley's job included the task of recruiting from inside as well as outside the labs. He was able to assemble a very competent group of researchers, including John Bardeen, Walter Brattain, Gerald Pearson, Morgan Sparks, and others.

In 1946 Bell Labs was engaged in a 30 percent staff reduction from its wartime peak while simultaneously resuming its prewar research activity with new insights. This reduction was occurring at the same time that Shockley was starting his physical research on semiconductors.

The application of quantum theory to solid-state physics in the decade of the 1930s had greatly advanced the knowledge of semiconductor properties, but much of the theory lacked confirmation by quantitative experiments. The radar systems used germanium and silicon point contact detectors during World War II. The material quality was greatly advanced in support of this application. Thus, the time was ripe for the task at hand. Although some members approached their work as pure research, from the beginning it was clearly Shockley's goal to discover a solid-state amplifier as a replacement for the vacuum tube.

Shockley returned to the idea of the field effect transistor, in which an externally applied electric field should, according to his calculations, modulate the current in a germanium filament, much as the grid in a vacuum tube controls the anode current. The experiments done to achieve this effect were never successful. John Bardeen suggested that electrons were trapped in surface states and thus prevented the electric field from penetrating the crystal. This insight led to a series of experiments on surface effects, including the discovery of minority carrier injection by the point contact emitter by Bardeen and Brattain in 1947.[1] The point contact transistor effect was demonstrated for Bell Labs management by Brattain and Bardeen on Christmas Eve 1947.

The discovery of the first semiconductor amplifier by Bardeen and Brattain in Shockley's department at Bell Labs, but without his participation, drove him to furious activity. Bardeen described the transistor action as minority carrier injection, but there was no clear proof that this was correct. In the process of devising an experimental test of the transistor action, Shockley invented the junction transistor. He reported on this device in a paper in the *Bell System Technical Journal*[2] and gave a comprehensive review of the electronic behavior of semiconductors in a book in 1950.[3] The junction transistor was more difficult to achieve than the point contact, and it was not until 1951 that it was first built. This series of events started the electronic revolution that is arguably the most important development of the twentieth century.

There had been relevant theoretical work in England,[4] Germany,[5] and Russia[6] on the study of band structure and the theory of rectifiers, and the American war effort had supplied some theory as well as experiments to the overall picture.[7] There were still many missing pieces to the puzzle, and the next few years involved many researchers of various disciplines trying to unravel the essence of the behavior of semiconductors. For example, the properties of both the negative electrons and positive holes had to be clarified in the fourth-column elements: germanium, silicon, and diamond. The facts are detailed in *The History of Engineering & Science in the Bell System Electronics Technology, 1925–1975*. The publication of *Electrons and Holes in Semiconductors* by Shockley in 1950 was a bible to a generation of researchers and academicians.

Shockley had always been a fast and unconventional thinker. His solutions to physical and mathematical problems were simultaneously unconventional, quick, and usually correct. He simply spun off new ideas that occupied experimenters for years.

He organized a weekly meeting in the auditorium for the presentation of new results; there were so many that the time was always filled. This was a period of high excitement and intellectual achievement; Shockley was the keystone. His example spurred his fellow workers on. His ability to approach a difficult problem in a remarkably effective manner, to break the problem into its fundamental components, and to find an elegant solution was a strong factor in his approach to the general problem of achieving a more basic understanding of semiconductors.

Shockley was adventuresome, professionally and as an individual. He published without waiting for experimental confirmation—and was usually proved correct. He was an enthusiastic amateur mountain climber. The Bell Labs cafeteria had a stone facade; at lunch time he would demonstrate his abilities by scaling the wall, gripping by his finger-tips. His enthusiasm for high-speed driving put fear into his passengers. As an amateur magician, he once challenged the protocols of the august American Physical Society, finishing a speech at the annual meeting by "miraculously" producing and flaunting a full bouquet of roses.

Unfortunately, his technical insights were counterbalanced by his lack of insight into human relations. This led to a major division within his own group, and ultimately he took paths that he should have avoided. It also accounts for some of the widely divergent views of Shockley that have been expressed by otherwise intelligent individuals.

Shockley maintained activities outside Bell Labs throughout most of his career. He was a visiting lecturer at Princeton in 1946 and at CalTech in 1954 and 1955. He also continued to serve the government, as scientific adviser for the Joint Research and Development Board from 1947 to 1949. He was deputy director of the Weapons Systems Evaluation Group of the Department of Defense in 1954 and 1955. In 1962 he became a member of the President's Science Advisory Committee on Scientific and Technical Manpower.

Shockley started the Shockley Semiconductor Laboratories in the Stanford industrial park in 1955 with help from the Beckman Instruments Company. This was the first semiconductor company in what is now Silicon Valley. The intent was to do research, development, and production of silicon switching devices. Shockley was a better scientist than businessman or manager. The Shockley labs were not a financial success. Shockley lacked the business acumen and market sense that was possessed by some of his employees; Bob Noyce, Gordon Moore, and a group of six other employees left Shockley to form Fairchild Semiconductor in 1957. Clevite Transistor purchased the operation in 1960. Shockley remained as a consultant. The company eventually closed in 1969.

Fred Terman, then provost of Stanford University, was eager to see new industry in the new electronics technology started near Stanford and was starting a parallel effort in the Stanford electrical engineering department. There were numerous informal connections between Shockley's labs and the people who were starting Stanford's semiconductor program. An arrangement was made whereby a new faculty member in electrical engineering spent an extended period working half time at the Shockley labs.

Shockley had no official connection to the university when these half-time positions were set up. There was, neverthe less, an informal connection, with many professors and students meeting from time to time to discuss their ideas. Dealing with his personality was daunting to the typical doctoral candidate, but the strongest and most independent students derived great benefit from their interactions. Shockley's association with Stanford became official in 1963, when he was appointed to be the first Alexander M.Poniatoff Professor of Engineering and Applied Science. He retired from Stanford in 1972.

Shockley was in a serious automobile accident in July 1961. He said that while he was lying immobilized from the accident he read about a teenager with an IQ of 70 who had blinded a delicatessen owner with acid. This incident made him determined to expose the "dysgenics" that was occurring in our society.

His approach to this exposition had many of the appearances of his approach to earlier scientific problems but on close examination lacked the scientific method. His methods and conclusions were highly controversial. The subject itself is charged with political and racial overtones. A great deal of data on the subject may have been fabricated or at least modified to suit a preconception. Shockley proposed some action on the topic by the National Academy of Sciences and was rejected. I believe that the combination of his strong personality and the rejection of action or support from fellow scientists made him determined to prove that his conclusions were right.

In 1965 Shockley renewed his association with Bell Labs in the capacity of executive consultant. His interests expanded into new areas, particularly domain wall motion in ferrites. He worked first with H.J.Williams and then with Andrew Bobeck and his group. Shockley helped establish a new memory technology based on the controlled motion of small domains called magnetic bubbles. It was a complete memory system but was unable to compete with the evolving semiconductor memories. In an interview on the eve of his retirement from Bell Labs in 1975 he was asked what recent technical developments he considered most important. There was no hesitation in his reply:

> One of the most striking things I've seen is the possibility of using gallium arsenide lasers and optical fibers in new transmission systems. Now you may observe that lasers and fibers will accomplish the same sorts of things as existing technology. But that's exactly what the transistor did: replaced the vacuum tube but at tremendous advantages in cost, power, space, and reliability.

In 1980 Shockley brought a $1.25 million libel suit against the *Atlanta Constitution* for an article it published about his ideas on race and intelligence. He accused the newspaper of "falsely and maliciously" likening his ideas to the Nazi genetics experiments in World War II. A token $1 in damages was awarded to him.

He entered the 1982 Republican primary in California for the seat of retiring U.S. Senator S.I.Hayakawa. He was a single-issue candidate, warning of the threat of dysgenics. He came in eighth. In his later years Shockley was far more eager to talk about his theories on race and intelligence than his contributions to science.

He died in 1989 at the age of seventy-nine. Shockley divided his life between creative science and engineering and his crusade against dysgenics. He devoted the last fifteen years of his life almost exclusively to dysgenics. The latter period increased the controversy that surrounded him. The topic of ethnic intelligence is controversial. Bill Shockley made no effort to calm the waters. His own behavior enhanced a tendency to judge him by his later years, where he was definitely dealing with a subject that would not yield to his method of attack.

Shockley led the effort in the Physical Research Department at Bell Labs for about ten years and many critical advances were accomplished during that period. He spent almost forty years making contributions to solid-state physics, was awarded over ninety patents for his inventions, and made many notable contributions to the scientific literature. His patents and publications alone do not measure his contribution to the advancement of technology. He certainly inspired a generation of scientists to great achievements. There are many testimonials to his capability of breaking a problem down to its fundamental components and finding unique solutions. A few public recognitions of his accomplishments are the Medal for Merit (1946); election to the National Academy of Sciences (1951); Air Force Association Citation of Honor for Outstanding Public Service (1951); Morris Liebmann Award (1953); Oliver Buckley Solid State Physics Prize (1953); Certificate of Appreciation from the Department of the Army (1953); and, with Bardeen and Brattain, the Nobel Prize in physics for inventing the transistor (1956).

A memoir of Shockley is incomplete without discussion of the transistor invention. This purely scientific endeavor is surrounded by a certain amount of controversy. The importance of the transistor to our industry and to our continuing advancement keeps the controversy alive. The three Nobel Prize winners (Bardeen, Brattain, and Shockley) all made significant contributions to the invention. Discussions of this period of scientific history tend to raise a question of precedence between Bardeen and Shockley concerning the discovery of minority carrier injection. It is my considered conclusion that the events as I have given them are correct. The laboratory group that Shockley recruited was seeking to invent a solid-state amplifier. There was a possible field effect device that did not work. A series of ex periments were done by Brattain and Bardeen, very much with the knowledge and approval of Shockley, to find clues as to why the field effect device did not work. Experiments with gold dots evaporated on germanium led Bardeen to suspect that minority carrier injection was a factor in the surface experiments. These experiments led to the Bardeen-Brattain experiment, which was the point contact invention. The point contact device was the first carrier injection amplifier. Shockley's activity in designing experiments to elucidate the physical processes in the point contact device led to the invention of the junction transistor. Both of these activities completely justified his receipt of the Nobel Prize.

I CONSULTED MANY PEOPLE with intimate knowledge of the activities at Bell Laboratories in the late 1940s. Ken McKay was most helpful with his account of the air of excitement generated during this period of invention and discovery. Professor Fred Seitz filled in a crucial segment of Bill Shockley's life. In addition, Morgan Sparks was very helpful in trying to recapture the early atmosphere. All of these people knew Bill Shockley before I did. I went to Bell Labs when I finished my Ph.D. at Ohio State in 1952, and most of the excitement of invention occurred between 1945 and 1950.

NOTES

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2. See Shockley (1949,2).

3. See Shockley (1950,1).

4. A.H.Wilson. *Proc. Roy. Soc.* 133A(1931):458.

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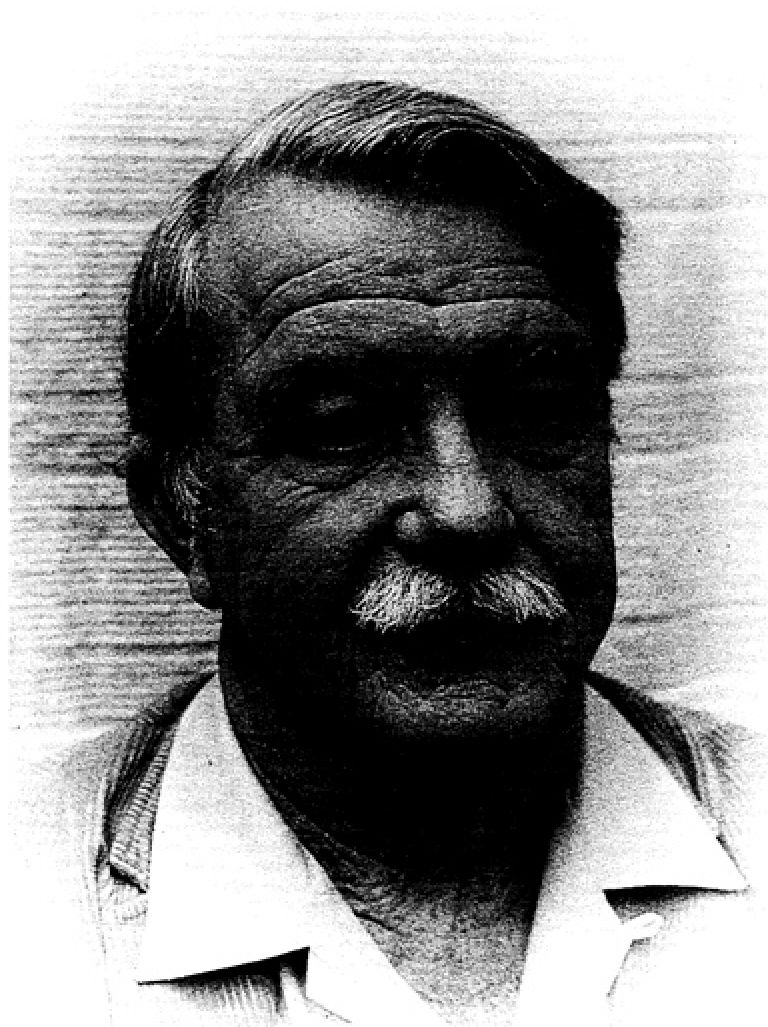

Photo by Rosamond B. Spicer, Tucson, Arizona

Edward H. Spicer

EDWARD HOLLAND SPICER

November 29, 1906–April 5, 1983

BY JAMES E.OFFICER

HE ROBERT BARCLAY SPICER FAMILY of Cheltenham, Pennsylvania, had a double cause for celebration on November 29, 1906. Not only was it Thanksgiving Day, but it was also the day Margaret Jones Spicer gave birth to her youngest son, Edward Holland. Her first-born child had died several years earlier, but a second son, Bill, was on hand to greet his new sibling. The elder Spicer was of Quaker persuasion, and in 1908 he took his family to Arden, Delaware, a single-tax community based on the principles of Henry George.

The Spicers fitted nicely into the liberal intellectual atmosphere of Arden, which lay in a setting of fields and woods along Naaman's Creek just north of Wilmington. Here, Ned and Bill were exposed to stimulating discussions of politics and economics. They also took part in the annual Shakespearean plays that provided entertainment for both local residents and summer visitors.

At the time the family moved to Arden, the elder Spicer was editor of a Quaker journal called *The Friends Intelligencer*. His ultraliberal views soon cost him his job, and he turned to truck farming, a vocational choice that introduced his sons to the world of work. Both Ned and Bill spent portions of each day hoeing and weeding the gardens, filling the woodbox, carrying water from the town pump, and looking after animals such as goats and rabbits.

Until he was thirteen, Ned obtained all his education in Arden. The children went to school in each other's homes, the mothers taking them for a month in turn. Later in life, Ned stated that he could not remember when he learned to read, but it was undoubtedly at the knee of his mother, who instilled in him a love of books and writing. From his father he gained a knowledge of philology and by the age of twelve was copying words and texts of the Algonquin Indian language. On his own he sought and absorbed knowledge about the natural environment. Memorizing the scientific names of plants and animals was a favorite pastime.

Ned's formal education began in 1919 when his parents enrolled him in the Friends School in nearby Wilmington. Traveling by train from his home in Arden, he studied there for three years. In 1922 the family moved to Louisville, Kentucky, where Ned's father began working with the Society for the Prevention of Tuberculosis. Ned graduated from Louisville Male High School in February 1924.

During the two years he lived in Louisville, Ned indulged a long-held interest in sailing. He constructed a canoe, which he outfitted with a sail, and cruised the waters of the Ohio River. After graduating from high school, he left home and enrolled at Commonwealth College, a new progressive school at Newllano, Louisiana, where he remained less than two months. In April 1924 he and a friend went to New Orleans to seek employment as merchant sailors. Ned found a job as an ordinary seaman on a ship called the *Aquarius,* which carried him to Germany. After short stops at the ports of Bremerhaven, Stettin, and Hamburg, the *Aquarius* returned to New Orleans and Ned went home to visit his parents.

There he learned that his father was dying of cancer at the age of fifty-five.

Following his father's death, Ned returned to the Wilmington area where he worked briefly, alongside his mother, at the Greenwood Bookshop. Fascinated though he was with books, Ned found he could not settle down; so he headed again to sea. Early in 1925 he sailed on a banana boat to Puerto Barrios, Guatemala; then, after returning from Central America, he joined the crew of a vessel hauling ore on the Great Lakes. A seamen's strike ended his career as a merchant sailor, and in the fall of 1925 Ned enrolled at the University of Delaware, planning to major in chemistry.

A FIRST TRY AT COLLEGE

Quickly disappointed with a required introductory course in chemistry at Delaware, Spicer turned to literature and drama. He became a member of the Footlights Club and acted in several plays. He also joined the staff of *The Delaware Review* and became its assistant editor. During his sophomore year, he took his first course in economics and decided to transfer to Johns Hopkins University, where he could obtain additional instruction in that subject.

At the time of Spicer's enrollment, Johns Hopkins was experimenting with what its administrators called the "New Plan" under which a student could take graduate courses without first earning a baccalaureate degree. Ned chose some of the more advanced classes, including one for which he wrote a paper titled "Theory of Hours and Production," which he read at a graduate seminar. He also helped to found and served as president of a student club called "The Radicals" whose members felt that socialism provided a better response than capitalism to the world's economic problems.

In 1928 Spicer learned that he had the symptoms of pulmonary tuberculosis and entered a sanitorium where he remained for most of the following year. Free from prescribed assignments, he spent his time reading, assisting with work in the laboratory, and pursuing a strong avocational interest in astronomy.

Ned returned to Johns Hopkins in the fall of 1929 but soon dropped out. Economics no longer held his interest; he felt he had to get away from Baltimore and see more of the world. With financial help from his mother, he purchased a bus ticket to Phoenix. The decision to go to Arizona would turn out to be one of the most important of his life.

Greatly stimulated by his new surroundings, Ned resolved to maintain himself and sought employment wherever he could find it. He washed windows at a resort hotel in Phoenix, picked oranges, and worked at an agricultural inspection station. He also spent many weekends exploring southwestern Arizona, where he found prehistoric ruins and traces of precious minerals. Even a bout with smallpox could not diminish his enthusiasm for the desert and mountains.

Spicer saved his money so that he could enroll at the University of Arizona and take whatever classes he might need to qualify him for a bachelor’s degree. He hoped then to pursue graduate work in either geology or archeology. He was delayed a year in carrying through on these plans because the advent of the Great Depression led to failure of the bank in which he had deposited his funds. Fortunately, he still had his job with the inspection service and was able to replace the money he had lost.

ARCHEOLOGY TAKES OVER

In the fall of 1931 Spicer went to Tucson, where he shared accommodations with several other students in similar eco nomic circumstances and began attending the University of Arizona. One course in advanced economic theory proved enough to earn him a B.A. in economics, as well as senior honors. While completing his degree, he enrolled in a class about southwestern Indians taught by Clara Lee Frapps (Tanner). He also came to know Dean Byron Cummings, who found him an eager volunteer for field trips to nearby Indian ruins. Ned would later comment that at this time he developed an interest in archeology that was "unflagging." In 1933 he completed work for his master's degree in that subject with a thesis on Prescott black-on-gray pottery and the American Indian society that produced it.

During the summer of 1932, when Spicer was working at the Kinishba Indian Ruin with Cummings, he came to know John H.Provinse, recently arrived in Arizona from the University of Chicago, where he was studying with A.R.Radcliffe-Browne and Robert Redfield. It was Provinse who first sparked Ned's interest in social anthropology.

After receiving his master's degree in the spring of 1933, Ned headed for northern Arizona to take part in an archeology project that resulted in the partial excavation of Tuzigoot Ruin, now a national monument. Associated with Spicer at Tuzigoot were Louis R.Caywood and Harry T. Getty. Funding came from the Federal Emergency Relief Administration—forerunner of the Civil Works Administration and the Works Progress Administration. One aim of the project was to provide jobs for unemployed copper miners and smelter workers in the area; and more than 100 men with picks and shovels greeted the archeologists on their first day at the site. After choosing several crews, they set to work, and early in 1934—ten months after starting—they finished both the digging and a report on what they had accomplished.

During the late spring and early summer of 1934, Spicer did additional archeology work for the Museum of Northern Arizona. He had not to this point considered seeking a higher degree. At the urging of John Provinse, however, he agreed to visit the University of Chicago campus at the beginning of the fall term to speak with the renowned anthropologists whom Provinse had recommended so highly. Impressed with Redfield and Radcliffe-Browne, Ned decided to continue graduate studies leading to a doctorate in social anthropology. Fay-Cooper Cole, department head at Chicago, suggested that Spicer apply for a full scholarship, which he did and which had positive results.

STUDYING SOCIAL ANTHROPOLOGY AT CHICAGO

In return for his scholarship Spicer assumed responsibility for cataloging and taking care of the extensive office library maintained by Redfield. Even with the scholarship, he did not have all the money he needed to pay for his room and food. Aware of this problem, one of his fellow students, Rosamond P.Brown, proposed that she and her roommate, along with Ned and another student, share supper each evening at the apartment rented by the women. This arrangement, plus many study hours spent together, brought Ned and Rosamond into a relationship they would share for the rest of Ned's life.

The long study hours and the cold Lake Michigan winter took their toll on Ned, who, early in the spring, suffered a hemorrhage that required hospitalization. Cole and other members of the department, aware of Ned's precarious financial situation, arranged for payment of his medical bills. They also suggested to Rosamond that she share her class notes with him so that he might obtain credit for certain of the third-quarter courses that he needed for his degree program. While in the hospital, Ned not only studied the notes Roz provided him but also read extensively, being particularly attracted to the writings of French sociologist Emile Durkheim.

When he was finally able to leave the hospital, Spicer returned to Arizona, where, thanks to further help from his professors at Chicago, he obtained a temporary job at the Arizona State Museum. In June 1936 he married Rosamond in a ceremony conducted by her father, a Swedenborgian minister. They spent their honeymoon at the Yaqui village of Pascua in northwest Tucson, which would be their residence for the next year and where they would conduct a community study. They then returned to Chicago, and Ned worked on his dissertation while Roz finished her master's thesis, both based on the Pascua research.

Ned gained his first teaching experience while a faculty member at Dillard University in New Orleans, where the Spicers lived from 1937 to 1939. During the summers, when school was not in session, they participated in an archeology project at Kincaid, Illinois, sponsored by the University of Chicago. Ned was field director in 1939.

Their affiliation with Dillard University provided the Spicers an unusual opportunity to meet and develop friendships with African-Americans. From these associations they acquired much knowledge about race relations, which Ned later shared with his students at the University of Arizona and which would be important to both Ned and Roz during the many years they worked with the Tucson and Arizona Councils for Civic Unity.

ARIZONA, WASHINGTON, AND ARIZONA AGAIN

The fall of 1939 found the Spicers back in Tucson, where Ned had a two-year appointment as an instructor in the University of Arizona Department of Anthropology. He was replacing Harry T.Getty, who had gone to the University of Chicago to complete work for his doctoral degree. While in

Tucson during the remainder of 1939 and 1940, Ned conducted additional research among the Tucson Yaquis and finished a manuscript called "People of Pascua" to go along with the book based on his dissertation. Roz was pregnant during part of this period and in 1940 gave birth to their first child whom they named Robert Barclay but who soon became "Barry" to one and all.

A Guggenheim Fellowship made it possible for the Spicers to spend the final months of 1941 and the early part of 1942 in southern Sonora, Mexico, studying Yaqui Indian culture on its home grounds. Following America's entry into the war, the Mexican authorities forced the Spicers to leave, and they came back to Arizona earlier than they had planned. Ned began work as a community analyst at the Poston Relocation Center for Japanese-Americans, while Roz did fieldwork on the Tohono O'Odham Indian Reservation. In 1943 Ned became head of the Community Analysis Section of the War Relocation Authority (WRA), and the Spicers moved to Washington, D.C., where they remained until the WRA discontinued operations. Their second child, Margaret Pendleton (Penny), was born in Washington in 1945.

Shortly after going to the east coast, Ned wrote in his diary that he had come to the conclusion that he wanted to spend the remainder of his life in Arizona, which he described as "my land." In the fall of 1945 Emil W.Haury, head of the Department of Anthropology at the University of Arizona, invited him to rejoin the faculty there, and he and Roz were delighted to accept the offer.

Ned's responsibilities with the WRA ended on June 30, 1946. and the Spicers went back to Tucson. Sensing they would be in Arizona for many years, they started building a home near the ruins of old Fort Lowell on the outskirts of town and, except for brief intervals of research and travel, remained in Tucson thereafter.

By the time the Spicers returned to Arizona in 1946, Ned was already well known in anthropology circles. The University of Chicago had published his revised dissertation in 1940 under the title *Pascua: A Yaqui Village in Arizona,* and the book received favorable notice from colleagues throughout the country. He also contributed—along with Fay-Cooper Cole, Fred Eggan, and Henry Hoijer—to the preface for Grenville Goodwin's classic work *Social Organization of the Western Apache,* which became available in 1942. Additionally, he wrote or collaborated in the writing of several articles on the relocation of Japanese-Americans that appeared in social science journals, and he contributed to *The Governing of Men,* a book written by Alexander H.Leighton and published by the Princeton University Press in 1945.

From his base in Tucson, Spicer continued to reflect on his experience with the WRA and to publish articles dealing with various aspects of that experience. He also returned to his research on Yaqui history and culture and wrote many additional articles, books, and chapters of books concerned with these Mexican Indians.

Serving with the WRA convinced Ned that anthropologists had much to contribute to decision making within governmental agencies. It was this conviction that led him to accept office in the Society for Applied Anthropology, which he had helped to found. He became vice-president of the organization in 1947, not long after he contributed his first article to the society's journal. Thirty years later the organization accorded him its highest honor—the Bronislaw Malinowski Award.

THE FRUITFUL FIFTIES

For Ned Spicer the 1950s proved to be one of the busiest and most satisfying decades of his life. Not only did he carry forward his involvement with applied anthropology, he also pursued an interest in culture change that in the early 1960s yielded significant contributions to acculturation theory. Additionally, he completed research for a major book concerned with the impact of European civilization on the Indian population of northwest Mexico and the southwestern part of the United States. He also expanded his professional relationships through service from 1951 through 1953 on the Board of Directors of the American Anthropological Association.

At the beginning of the decade, Spicer accepted an invitation from Alexander Leighton to work with John J.Adair of Cornell University in organizing and conducting summer seminars for administrators of overseas agricultural and social science programs, as well as Cornell graduate students in anthropology. The seminars exposed students to the cultures of Indian and Hispanic communities in northern Arizona and New Mexico. Funding for the program came from the Carnegie Corporation.

At Leighton's suggestion, Spicer edited a casebook for use by faculty members and students of the seminar. The Russell Sage Foundation sponsored publication of the casebook under the title *Human Problems in Technological Change*. It came out in 1952, the same year the last Spicer child, Lawson Alan, was born. By the mid-1960s *Human Problems* would become a standard text for Peace Corps and Vista volunteers, as well as others working in domestic and international community development programs.

Although Spicer had been interested in culture history and the processes of social and cultural change while doing research in archeology, *Human Problems* was the first publication after his conversion to social anthropology wherein he gave significant attention to such subjects. In 1941 in a complimentary review of *Pascua: A Yaqui Village in Arizona* prepared for the *American Anthropologist,* Ralph L.Beals chided Ned for not doing more historical and comparative research that might have strengthened the study and spared the author certain errors. Whether Beals's criticism had anything to do with a shift in Spicer's orientation will never be known, but after Ned's return to academic life in 1946 his research always included an important historical dimension.

Well before the beginning of World War II, anthropologists had become interested in learning more about social and cultural change. Ralph Linton's 1940 work titled *Acculturation in Seven American Indian Tribes* gave added stimulus to this research trend. Following a 1953 summer seminar on acculturation, Spicer and several colleagues decided to organize an additional conference on this theme with the aim of designing a research project to explore in greater depth the theoretical and practical aspects of culture change.

The second acculturation seminar, sponsored by the Social Science Research Council, took place on the campus of the University of New Mexico in the summer of 1956. Spicer and five colleagues agreed on a format for a joint study that would describe culture change in six Indian tribes, identify periods when particular change factors prevailed, and characterize the strategies employed by the agents of change as well as those used by tribes in responding to change. From this collaboration came the book *Perspectives in American Indian Culture Change,* published in 1961.

Spicer provided the introduction to *Perspectives* as well as a section on Yaqui culture and a final chapter titled "Types of Contact and Processes of Change." Harvard anthropologist Evon Z.Vogt—one of Spicer's associates in preparing the book—commented later that Ned made two important contributions to acculturation theory in *Perspectives* and his later writings. One was to sharpen the concepts of directed and nondirected culture change, and the other was to focus attention on the social structure of the "contact community" as a major acculturation determinant.

While engaged in research for *Perspectives,* Spicer continued to collect material for a book he had started on the comparative effects of Spanish, Mexican, and American rule on the Indian cultures of Arizona, New Mexico, and the Mexican states of Sonora and Chihuahua, a region he referred to as the Southwest. His ideas for this volume, as well as the theoretical aspects of *Perspectives,* provided exciting material for discussion among the graduate students who attended Ned's seminars on culture change, some of which he held in his home.

EDITING AND OTHER ACCOMPLISHMENTS

Despite the heavy load of teaching, research, and service to the profession that kept him busy during the 1950s, Ned, at the end of the decade, yielded to the entreaties of his colleagues and accepted responsibility for editing the *American Anthropologist*. During the three years he was engaged at this work, he somehow managed to finish his writing for *Perspectives* and also to complete the manuscript for a gigantic tome that the University of Arizona Press published in 1962 as *Cycles of Conquest: The Impact of Spain, Mexico, and the United States on the Indians of the Southwest, 1533–1960.* This volume, running to nearly 600 pages, was highly lauded by both anthropologists and historians and has since attained the status of an ethnohistorical masterpiece. With support from his second Guggenheim award, Ned began work on *Cycles* in 1955 while living in Oaxaca, Mexico.

Following his service as editor of the *American Anthropologist,* Ned took sabbatical leave in the fall of 1963. A National Science Foundation senior fellowship enabled him to do a comparative study of programs for Indian better ment in Mexico, Peru, and Ecuador. He returned from Latin America in 1964 and shortly thereafter turned down the opportunity to become a nominee for the presidency of the American Anthropological Association.

Spicer continued to do research and to write after concluding his sabbatical, but in the late 1960s he gave extra attention to teaching, which he always considered one of his most important and beloved responsibilities. He and a colleague from the sociology department expanded a seminar in community development they had organized, and both designed and implemented short training programs for Vista workers and others engaged in similar work. In 1968 Spicer accepted an invitation from William S.King, an official of the Bureau of Indian Affairs and former student of his, to help plan, and take part in, a series of regional community development seminars for BIA personnel.

During his extensive studies of culture contact and change, Spicer became fascinated with the abilities of particular ethnic groups, such as the Yaqui, to retain separate identities within the nation-states of which they formed a part. In 1969 he obtained a grant from the National Endowment for the Humanities to visit Europe, where he might investigate firsthand such groups as the Basques, Catalans, Irish, and Welsh, who he felt shared with the Yaquis this quality of endurance. In the summer of 1970 he and Roz began their travels and remained away from Tucson for a full year. Not long after they returned, Ned published a short article in *Science* titled “Persistent Cultural Systems.” Despite its brevity, the article attracted international attention and helped stimulate a new research interest in the social sciences.

Ned was already past normal retirement age when he returned from Europe, but he had two major writing projects he hoped to finish during the years that remained to him.

One, already begun, was a lengthy manuscript on Yaqui culture that incorporated many of the ideas and research techniques he felt most important. The other was a book on "enduring peoples" throughout the world. He lived to complete the first of these objectives but not the second.

In 1972 Ned returned to the board of the American Anthropological Association as president-elect. Shortly afterward, he learned that he was suffering from cancer of the lower jaw, a condition similar to that which had cost the life of his father. Doctors began radiotherapy treatments and for the next several years would continue their attempts to control the disease in this fashion. Spicer went on working as before and in the fall of 1973 took office as president of the American Anthropological Association. He spent much of the next year assisting the program chairman with arrangements for the group's annual meeting in Mexico City; and he invited his old friend, historian Miguel León-Portilla, to give a major address at that event.

Shortly before Ned stepped down from the presidency of the American Anthropological Association in the fall of 1974, he was honored with membership in the American Philosophical Society and received similar acclaim from the National Academy of Sciences the following year. Despite his cancer, he continued to teach and to advise students, as well as to work on his Yaqui manuscript. He also was deeply committed to several community projects involving the Tucson Yaquis and his beloved Fort Lowell neighborhood.

The Society for Applied Anthropology voted Spicer its coveted Bronislaw Malinowsky Award in 1976. Over the years he seized many opportunities to apply his anthropological knowledge to designing, administering, and assessing government programs for improving the lives of people. In addition to his work with the War Relocation Authority, the Cornell and Indian Affairs seminars, and his evaluations of

Indian programs in Central and South America, Spicer assisted the Stanford Research Institute with preparing an inventory of resources on the San Carlos Apache Reservation, took part in a national study of Indian education, and advised the Yaquis on public housing. He often reminded his students that anthropologists had much to contribute to administration and encouraged them to seek employment outside the academic setting.

In 1978, after completing his Yaqui manuscript, Spicer retired from the University of Arizona, where he enjoyed a joint appointment in anthropology and sociology and at one time also directed the Bureau of Ethnic Research. During his final year on campus, the Arizona chapter of Phi Beta Kappa elected him to membership. He had belonged to Sigma Xi, the national science fraternity, since his Chicago days.

Radiotherapy treatments slowed the development of Ned's cancer but did not cure it, and a year after his retirement surgeons operated on his jaw. During his recuperation, he returned to work on the "enduring people" manuscript, and in the fall of 1979 the American Anthropological Association presented him its Distinguished Service Award. Shortly afterward, the Southwestern Anthropological Association honored Ned with its Outstanding Scholarship Award for his book *The Yaquis: A Cultural History,* which the University of Arizona Press published early in 1980.

Evon Z.Vogt, who worked closely with Ned on the *Perspectives* project, predicted *The Yaquis* would be Ned's most enduring contribution to anthropology. "Here," Vogt remarked, "Spicer used what I would describe as [Clifford] Geertz's method of 'thick description' applied to the analysis of Yaqui cultural history and their processes of change and persistence. The story is masterfully placed in full materialist, structural, and ideological context and provides an unusually perceptive understanding of this distinctive case. It deserves to become one of the 'great classics' of anthropology."

Ned's cancer reasserted itself in 1982, and he found it too stressful to continue work on the manuscript he so much wanted to finish. He laid it aside and began spending a few minutes each day on the preparation of an autobiography. A second operation proved unsuccessful, and on April 5, 1983, he died at his Fort Lowell home. The memorial service held at the Arizona State Museum a few days later attracted a large audience of people who had known, admired, and loved Ned, among them many Yaqui, O'Odham, and other Indian friends.

Two months after Ned's death the International Astronomical Union approved the naming of one of the minor planets "(2065) Spicer." Recommending this action was Professor Frank K.Edmondson, director of the asteroid project at Indiana University and a member of the original board of the Association of Universities for Research in Astronomy (AURA). Twenty-eight years earlier, in 1955, Ned had aided astronomers in their efforts to obtain a site for a national observatory atop Kitt Peak on the Tohono O'Odham Indian Reservation.

In the fall of 1985 Spicer's colleagues and students organized an all-day tribute to him at the annual meeting of the American Anthropological Association in Washington, D.C. Speaker after speaker extolled the depth and breadth of Ned's interests, his contributions to anthropology and to humankind, his inspirational teaching, and, above all, his remarkable qualities of love and understanding.

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GEORGE STREISINGER

December 27, 1927–September 5, 1984

BY FRANKLIN W.STAHL

EORGE STREISINGER WAS a leading contributor to the genetics of the T-even bacterial viruses, culminating in his demonstration and rationalization of the circular linkage map. During the infancy of molecular biology, he provided crucial links between genetics and biochemistry with his demonstration of the consequences of frameshift mutations. He identified and developed zebra fish as a system for the genetic analysis of vertebrate development.

PERSONAL HISTORY

George Streisinger was born in Budapest, Hungary, on December 27, 1927. To escape Nazi persecution, he and his parents left Budapest for New York when he was ten years old. He attended public schools, graduating from the Bronx High School of Science in 1944. During his high school days, George filled his home with salamanders, spiders, and snakes and collaborated with Theodosius Dhobzansky on studies of courtship in *Drosophila*. George's first three scientific papers (1944, 1946, 1948), two of them sole authored, resulted from that precocious enterprise.

George obtained a B.S. degree in genetics from Cornell University in 1950 and a Ph.D. in genetics from the Univer sity of Illinois in 1953. His postdoctoral studies were at the California Institute of Technology from 1953 to 1956.

In 1949 George married Lotte Sielman. They had two children, Lisa and Cory, born in 1955 and 1956, respectively.

George's career was influenced by Salvadore E.Luria, with whom he took his Ph.D., and, more so, by Max Delbrück and Jean Weigle, with whom he studied as a postdoctoral fellow. George influenced a number of other collaborators and friends, and it is plausible that they influenced him, too. They include Sidney Brenner, Seymour Benzer, Matt Meselson, Robert Edgar, Jan Drake, and myself.

PROFESSIONAL HISTORY

George was one of many who used the low-cost School of Agriculture at Ithaca to gain access to a high-quality Cornell University undergraduate education. As always, he put his efforts where his interests were, For instance, while at Cornell, he met and married Lotte Sielman, a refugee from Munich. Furthermore, he spent an academic year on a farm working off a provisional status earned by his neglect of required ROTC and/or physical education courses. (This requirement could have been worked off in the summer, but George spent all his summers at Cold Spring Harbor.)

Following his graduation from Cornell, George undertook graduate studies in the genetics of T-even coliphage with S.E.Luria in the Bacteriology Department of the University of Illinois. The phages T2 and T4, while distinguishable, are obviously related and had been shown by Delbrück to recombine with each other to give hybrid phages of varying degrees of viability. George identified single genes responsible for the differences between T2 and T4 in UV sensitivity (1956,1), host range, and serological specificity (1956,2), respectively. These studies revealed phenotypic mixing, in which a phage with the host-range genotype of one phage type was found in a particle whose phenotype was that of the other (1956,2). George based his Ph.D. thesis on these experiments. When they were published in 1956, they had a profound impact on viral biology.

Upon obtaining his degree, George went off to CalTech to pioneer the study of plant somatic cell genetics. He couldn't make those studies fly, however, and while still at CalTech he returned to the genetics of phage. With Jean Weigle, he undertook further studies on T2×T4 hybrids, which led to the discovery of DNA modification (by glucosylation).

With Naomi Franklin, George undertook a fine-structure recombination study of the host-range *(h)* locus of T2, after the fashion of Benzer's studies on the *r*II locus of T4. However, whereas, the rII protein escaped detection, it was probable that the protein product of the *h* gene, T2's tail fibers, would be tractable. Although the connection between gene and protein was not made, these studies foreshadowed George's later work (described below) on the T4 lysozyme gene.

With Victor Bruce (1960), George showed that the known genetic markers of T4 could be arrayed on a single linkage group. This simplification of the previously held view of three linkage groups was an essential step in the coalescence of genetics with emerging physical data on T4 DNA.

After his postdoc at CalTech, George took a position at Cold Spring Harbor. (His science knew no boundaries and his publications no timetable. Add to that the free flow of personnel and ideas among phage labs in those days, and the chronology of my recitation occasionally becomes a bit vague.)

George took leave from Cold Spring Harbor to spend a year at the MRC in Cambridge, England, with Sidney Brenner.

In collaboration with other visiting Americans, they initiated studies on T4 proteins with an eye to decoding the relationship between DNA and protein (1959). One product of that work was the identification of T4's endolysin (lysozyme) as a promising object for such studies.

At Cold Spring Harbor and the University of Oregon's Institute of Molecular Biology, where George took a position in 1960, he developed the methods for selecting mutants, revertants, and recombinants in the *T4e* gene, which encodes the phage endolysin. His first applications of this know-how was in a demonstration that phage containing 5-bromouracil have a high mutation rate when they are allowed to grow in medium that is free of 5-bromouracil. The second application was the demonstration, at the level of amino acid sequence, that frameshift mutations in the *e* gene really do shift the translation reading frame, as predicted by Francis Crick's hypothesis of a commaless triplet code (1966,1,2). The first in vivo codon assignments resulted from this work. The third application of George's mastery of the *e* gene was an analysis of the roles played by amino acid sequence in determining protein stability. In 1992, impelled by the methods of modern genetics, this work remains a major activity of several chemists and physicists at Eugene who study protein folding as well as stability.

The physical studies on phage T4 seemed to indicate that each T4 particle contained one Watson-Crick duplex DNA molecule. Contemporaneous genetic studies, however, argued that regions of heterozygosity in T4 were 4-stranded and that these regions were variable in position. This paradox was resolved by Meselson and Streisinger's suggestion that the chromosomes of T4 are circularly permuted and terminally redundant. After George moved to Eugene, he tested predictions of that notion, most notably the prediction that the unitary linkage map of T4 be circular. Since *E.*

coli was the only creature previously known to have a circular map, George's demonstration with Bob Edgar (1964) was important in establishing the concept of widespread circularity among microbes. Subsequent papers support George's notions of terminal redundancy of a permuted chromosome whose length is determined by the amount of DNA that can be fit into a phage head (1965, 1967).

Frameshift mutations are proflavin inducible, and George's interests extended to the mechanism of that mutation induction. He offered a proposal that has played a central role in our understanding of the origin of duplications and deletions (1972).

Like many phage workers, George eventually set his sights on more complex systems. Working initially without students (because it would not be fair to risk their careers), George developed the methods for the mutation and genetic analysis of zebra fish (1981;1983,1,2). It was his aim to make the fish as tractable as phage so that it could be used for a genetic analysis of the vertebrate nervous system. The degree to which he succeeded can be judged by his masterpiece on the development of the pigmented retina (1989) and by the many laboratories that are now exploiting this little Indian import to unravel other mysteries of vertebrate development. This work, better even than his work on the code, illustrates his imagination and courage. His career was reaching its zenith when he died of a heart attack during his final exam in a scuba diving class.

George's research contributions ensured his position in world science. His position in Oregon was ensured equally by his extraordinary contributions as a teacher, a politically involved citizen, a chef, and a warmly sociable friend and colleague.

As a teacher, George was unbelievably dedicated to the students. His dedication was backed by energy (he was al ways available), imagination (he conscripted a dance class to illustrate protein synthesis), and, of course, deep understanding. The University of Oregon recognized his teaching with a prestigious award.

George was politically active both on and off campus. He spent a major part of his first two years in Eugene organizing grass-roots resistance to the Vietnam War and legislative opposition to John Kennedy's civil defense program. He played a central role in the successful effort to restrict the use of potentially mutagenic herbicides in Douglas fir reforestation. This extracurricular activity informed his published work (1983,3). He led and won a battle to exclude secret war department research from the University of Oregon campus.

An invitation to dinner at the Streisinger home was never refused, because in the 1960s there were no restaurants in Eugene that could come close to the cuisine offered there. A barbecue was sometimes a suckling pig, at other times a giant Chinook salmon. Memorable winter meals were traditional Hungarian. Breakfast was for children. It featured crepes poured to resemble animals and served with chocolate syrup. Magic tricks sometimes followed.

When George was chairman of our Biology Department, he combatted the paperwork blues by unsuccessfully breeding pheasants (the foxes got them) and by successfully training to be a goat judge (he was in great demand at county fairs throughout the West).

George's family continues to have its impact on Eugene and Oregon. Eugene's well-known Saturday Market was founded in the early 1960s by George's wife, Lotte, a potter, who currently serves the community as an art administrator. His daughter, Lisa, founded a company in Portland that administers health care systems. Cory, his younger daugh ter, served as lawyer for Governor Neil Goldschmidt and is currently lawyer for the Port of Portland.

George's impact on the University of Oregon has been symbolized by the naming of a beautiful research building for cell and molecular biology. George's impact on his colleagues in the Institute of Molecular Biology has been marked by an annual lecture, quickly recognized as both a scientific and a social highlight of our community.

LOTTE STREISINGER AND AARON NOVICK made important contributions to this memorial.

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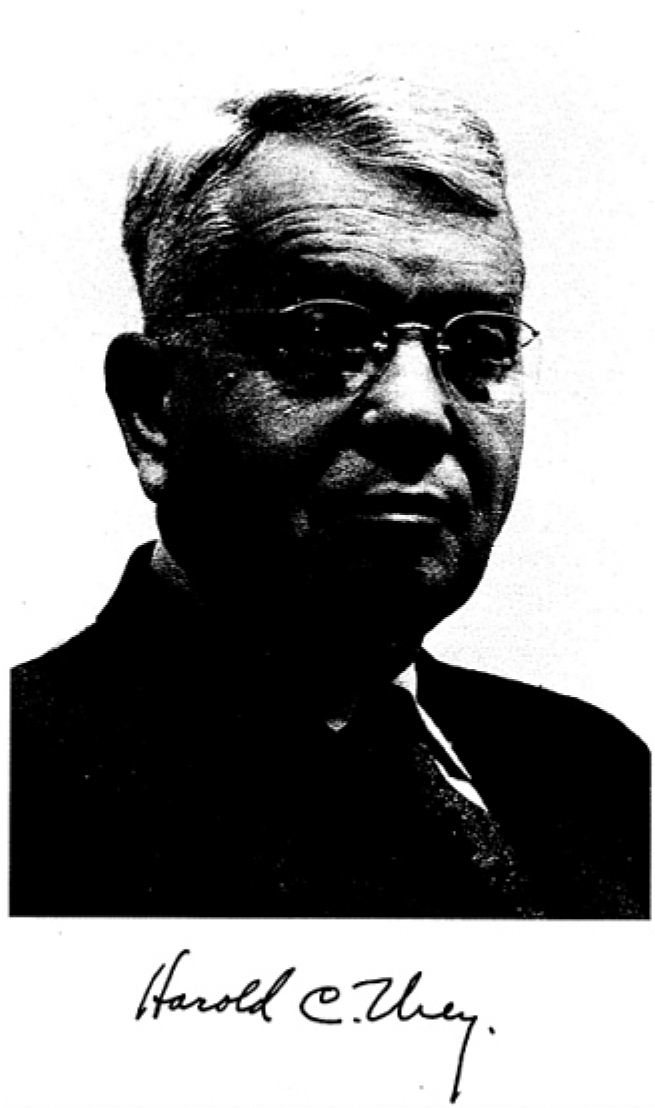

HAROLD CLAYTON UREY

April 29, 1893–January 5, 1981

BY JAMES R.ARNOLD, JACOB BIGELEISEN, AND CLYDE A.HUTCHISON JR.

AROLD UREY WAS A SCIENTIST whose interests, accomplishments, and influence spanned the disciplines of chemistry, astronomy, astrophysics, geology, geophysics, and biology. Although he was meticulous in his attention to detail, his sights were always on broad questions at the forefront of knowledge. His unusual powers of concentration and capacity for hard work accounted for much of his success in exploring and opening up major new fields of research, including his discovery of deuterium and work on isotope chemistry, isotope separation, isotope geology, and cosmochemistry. Urey's approach to a new area began with his becoming thoroughly familiar with what was known about the subject of his curiosity and then the formulation of a theory to explain a large amount of uncorrelated material, which was then followed by carefully planned experiments. The latter frequently involved the design of new experimental equipment beyond the state of the art.

As a graduate student in physical chemistry in the early

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1920s, Urey realized that future progress in that discipline would require a knowledge of the quantum theory of atomic and molecular systems, which was undergoing a revolution in Europe. He supplemented his command of mathematics and physics by formal coursework prior to going to the Bohr Institute in Copenhagen in 1923. His exposure there led to his formulation of the concept of the electron spin concurrent with but less complete than the Goudsmit-Uhlenbeck discovery. After completion of his text with Arthur Ruark, *Atoms, Quanta and Molecules,* one of the first English texts on quantum mechanics and its applications to atomic and molecular systems, Urey became interested in nuclear systematics. This led to his discovery of deuterium. The conception of this search, the design of the experiment, the actual discovery, and its publication are a model for the planning and execution of scientific research. His discovery of the differences in the chemical and physical properties of deuterium compounds led to his broader interest in isotope chemistry and isotope separation. Here again he developed the theory that led to the prediction of the magnitude of isotope effects in the light elements. He followed this up with experiments to confirm the theory, and this led to his pilot plants that achieved the first concentration of ^{15}N, ^{13}C, and ^{34}S.

Urey's interest in democratic government and world affairs led to the sense of urgency that developed in the Manhattan Project late in 1941. His major contributions and dedication to the success of the program through his work on uranium isotope separation, heavy water production, and ^{10}B enrichment and his service on the various NRDC and OSRD committees related to the development of the atomic bomb have never been fully appreciated.

With the war behind him Urey conceived the isotope thermometer and its application to geochemistry. From there he became interested in the moon, formation of the planets, meteorites, the abundances of the elements, and finally, the origin of life. He was a major supporter of the manned mission to the moon and was an active investigator in the program.

Harold Urey was a warm and generous person. He was warm in all his personal relations and generous with his time, attention, and resources. To have known him and worked with him were unequaled experiences for each of the authors of this memoir. None of us could have prepared this memoir alone.

UREY'S EARLY LIFE UP TO HIS ENTRANCE TO GRADUATE SCHOOL IN BERKELEY

Harold Clayton Urey was born in Walkerton, a small town in Indiana, on April 29, 1893. His father, a school teacher and a minister in the Church of the Brethren, died at the time Harold was just starting his elementary schooling. Upon graduation from grade school at age fourteen, Urey barely managed to pass the entrance exams for high school. But in high school he became interested in all aspects of his work, due, he said, to the excellent teachers he had there, and he immediately became the leader of his class in all subjects, a position he maintained throughout his high school years and in college.

When in 1911 at age eighteen he graduated from high school, Urey became a teacher in a small country school in Indiana with some twenty-five children in various grades. After one year he went to Montana, where his mother, stepfather, brother, and sisters had already gone, and taught in small elementary schools.

It was while teaching in a mining camp that the son of the family with which he was living decided to attend college, and this influenced Harold to do the same. He en tered the University of Montana in Missoula in the autumn of 1914. By carrying a heavy schedule of courses he was able to complete his college education in three years with a straight A record, except in athletics. He did this in spite of being required by his financial situation to wait on tables in the girls' dormitory and work one summer on the railroad being built there. Many years later in his Willard Gibbs Medal address he spoke warmly of the inspiration he received from the professors at the University of Montana and of the beginning of his interest in science due to their counseling advice, in particular the influence of A.W.Bray, professor of biology. Under Bray's guidance Harold majored in biology, and his first research effort was a study of the protozoa in a backwater of the Missoula River. His interest in the origins of life, a field in which he was to make a major contribution much later at the University of Chicago, originated with that earliest research. Bray also encouraged him to study chemistry, and he obtained a second major in that subject.

World War I began as Urey entered the university, and at the time he completed his work there in 1917 the United States declared war. He was urged by his professors to work in a chemical plant, chemists being badly needed at that time. During the rest of the war he worked at the Barrett Chemical Company in Philadelphia. In 1919 after the end of the war he returned to the University of Montana as instructor in chemistry.

After two years of teaching he realized that if he was to advance academically he would need to obtain a Ph.D. degree. The head of the Chemistry Department at Montana sent a letter of recommendation to Professor Gilbert N. Lewis of the Chemistry Department of the University of California, Berkeley. A fellowship was offered to Harold, and so in 1921, at the age of twenty-eight, he entered the University of California as a graduate student.

FROM CHEMICAL PHYSICS TO ISOTOPE GEOLOGY

The educational facilities, opportunities, and philosophy of Berkeley's Chemistry Department matched Urey's interests. The department stressed exploration of new ideas through original research and its weekly seminars. There were a minimum of formal requirements. Urey, nevertheless, took the opportunity to enroll in courses in mathematics and physics, which he deemed essential for his education as a chemist. In an unpublished autobiography (ca. 1969), Urey described his two years as a graduate student as "among the most inspiring of any of my entire life." His thesis was self-generated. The first part was an outgrowth of his unsuccessful attempt to measure the thermal ionization of cesium vapor. Bohr, Herzfeld, and Fowler had shown earlier that the ideal gas approximation leads to a dissociation instability for an atom with an infinite number of states below the dissociation or ionization limit. Its partition function is infinite at all temperatures. Urey and later Fermi showed that the correction of the ideal gas approximation for the excluded volume of the dissociating species leads to a convergence of the partition function of the atom or molecule. Urey's result was published in the *Astrophysical Journal*. When he became interested in the moon and planets, Urey was wont to tell his younger astronomy colleagues that he published a paper in the *Astrophysical Journal* before they entered the field. The second part of his thesis was of lesser long-term significance. He attempted to calculate the heat capacities and entropies of polyatomic gases before the correct description of the rotational energy states of molecules had been established by quantum mechanics.

When Urey received his doctorate in 1923, he realized that there was much he needed to learn about the structure of atoms and molecules. He received a fellowship from the American Scandinavian Foundation and went to the Institute of Theoretical Physics, Bohr Institute, in Copenhagen. The institute under Bohr's leadership was a major center in theoretical physics, particularly the development of the new quantum mechanics and its application to atomic and molecular structure. There Urey became acquainted with Heisenberg, Kramers, Pauli, and Slater and the biochemist Hevesy. Before Urey returned to the United States in 1924, he attended a meeting of the German Physical Society where he met Einstein and James Franck, who later became lifelong friends.

On his return to the United States Urey took a position as associate in chemistry at Johns Hopkins University. There he continued his association with physicists, including Ames, Herzfeld, and Wood of Hopkins; Brickwedde, Foote, and Meggers of the Bureau of Standards; and Tuve of the Carnegie Institution. His research at Hopkins ranged from speculations on the spin of the electron to cooperative experiments with F.O.Rice on the disproof of the radiation hypothesis of unimolecular reactions. With Arthur Ruark, Urey wrote *Atoms, Quanta and Molecules*. He had established himself as one of the new generation of chemists who applied the new quantum mechanics of Heisenberg and Schrödinger to chemistry.

In the fall of 1929 Urey joined the Columbia faculty as associate professor of chemistry. He initiated both experimental and theoretical research. In the former area his work was mainly in spectroscopy—ultraviolet spectra of triatomic molecules and vibrational spectroscopy. He and his student Charles Bradley measured the Raman spectrum of silico-chloroform, a tetrahedral molecule. They found that none of the molecular force fields in use at the time could reproduce the spectra of tetrahedral molecules. They introduced a new force field, the Urey-Bradley field, which is an admixture of valence bond and central force fields. The Urey-Bradley field remains in use in the analysis of the vibrational spectra of tetrahedral molecules. Urey's theoretical work at that time was directed to nuclear stability and the classification of atomic nuclei.

In 1931 Urey had on the wall of his office a chart of atomic nuclei. On the ordinate his chart was labeled "protons"; on the abscissa he plotted "nuclear electrons." This was prior to the discovery of the neutron. The number of nuclear electrons is the number of neutrons in the nucleus. The atomic number or nuclear charge is the number of protons minus the number of nuclear electrons. For the light elements Urey's chart showed the stable nuclei ${}^{1}_{1}H$, ${}^{4}_{2}He$, ${}^{6}_{3}Li$, ${}^{7}_{3}Li$, ${}^{9}_{4}Be$, ${}^{10}_{5}B$, and ${}^{11}_{5}B$. From nuclear systematics, Urey and others postulated the existence of ${}^{2}_{1}H$, ${}^{3}_{1}H$, and ${}^{5}_{2}He$. No isotopes of hydrogen or helium other than ${}^{1}_{1}H$ and ${}^{4}_{2}He$ were known in 1931. From atomic weight considerations, to be discussed below, it was estimated that, if a stable isotope of hydrogen of mass 2 existed, its natural abundance would be less than 1:30,000 parts of ${}^{1}_{1}H$.

DISCOVERY OF DEUTERIUM

As early as 1919 Otto Stern reported an unsuccessful search for isotopes of hydrogen and oxygen, other than the ones of masses 1 and 16, respectively. In 1929 two Berkeley chemists, W.F.Giauque (who had been a graduate student contemporary of Urey) and H.L.Johnston, discovered the stable isotopes of oxygen, ^{17}O and ^{18}O. Their natural abundances are 0.04 and 0.2 percent, respectively. The chemical atomic weight scale was based on the assumption that oxygen had only one isotope, mass 16. The atomic weight of hydrogen was based on the relative densities of hydrogen and oxygen gases and the atomic weight of natural oxygen equal to 16. Aston had determined the atomic weight of hydrogen based on ${}^{16}_{8}O = 16.$ The chemical value of the atomic weight of hydrogen was 1.00777 ± 0.00002. Aston's mass spectrograph value, 1.00778 ± 0.00015, reduced to the chemical scale using the 1931 values for the abundances of ${}^{17}O$ and ${}^{18}O$ was 1.00756. To reconcile the physical and chemical atomic weights of hydrogen, Birge and Menzel postulated the existence of a stable isotope of hydrogen of mass 2 with a natural abundance of 1:4500.

Urey read Birge and Menzel's communication in *Physical Review* in August 1931. Within days he decided to look for an isotope of hydrogen of mass 2 and outlined his plan of attack. He would need a method of detection, and it would be desirable to prepare samples enriched in this isotope. The design of the experiment was a model of how one should conduct a search for a small effect. It was the prototype of the characteristics of Urey's work for the next two decades. As a method of detection, Urey and his assistant George Murphy chose the atomic spectrum of hydrogen. An isotope of hydrogen of mass 2 should have red shifted lines in the Balmer series. The shifts could be calculated from the Rydberg formula for the energy levels in the hydrogen atom after taking into account the relative masses of the electron and nucleus. They amounted to 1.1 to 1.8 Å in four lines in the visible part of the spectrum. These could readily be resolved with the 21-foot grating spectrograph that had just been installed at the Pupin Laboratory of Columbia University. The latter had a dispersion of 1.2 Å/ millimeter in the second order. To enrich the heavy isotope, Urey and Murphy chose the distillation of liquid hydrogen. They estimated the fractionation factor for ${}^{1}_{1}H^{2}_{1}H$ from ${}^{1}_{1}H_{2}$ in the range between the freezing and boiling points from a Debye model for liquid hydrogen. Their esti mated fractionation factor was 2.5. To achieve an overall enrichment of 100 to 200 above natural abundance would require evaporating 5 liters of liquid hydrogen to 1 ml. The heavy hydrogen should be in this 1-ml residue. There were but two places in the United States capable of producing 5 liters of liquid hydrogen in 1931. They were Giauque's laboratory at the University of California and the low-temperature laboratory at the National Bureau of Standards in Washington, D.C. The NBS cryogenic laboratory had been established by Hopkins physics graduate F.G.Brickwedde, who overlapped with Urey at Hopkins. It is not difficult to understand why Urey chose to collaborate with Brickwedde.

During the period when Brickwedde was preparing the enriched sample, Urey and Murphy determined the optimum conditions for excitation of the atomic spectrum of hydrogen and suppression of the molecular spectrum. They did in fact find the lines to be expected for ${}^{2}_{1}H$ in the atomic spectrum of natural hydrogen. They delayed publication until these lines could be shown to increase in intensity in an enriched sample. In particular, it was necessary to rule out any possibility that the ${}^{2}_{1}H$ lines were artifacts (e.g., "ghost" lines from periodic errors in the ruling of the grating or lines from the molecular spectrum). The first of Brickwedde's samples showed no increase in the intensities of the lines attributed to ${}^{2}_{1}H$. A less persistent person than Urey would have dropped the search. Brickwedde then prepared two more samples each by evaporation of a 4-liter batch of liquid hydrogen, this time close to the triple point, where the enrichment factor is somewhat larger than at the normal boiling point. Spectroscopic examination of these samples on Thanksgiving Day of 1931 confirmed the discovery of hydrogen isotope of mass 2, subsequently named deuterium. Urey reported his success to his wife, Frieda, when he returned to his home in Leonia, New Jersey, hours late for Thanksgiving dinner.

For the discovery of deuterium, Harold Urey received the Nobel Prize in chemistry in 1934. Urey was the third American to receive a Nobel Prize in chemistry. He was young in comparison with most Nobel laureates in chemistry prior to or since 1934. He was the first of the California school to receive a Nobel Prize. He valued the contributions that his associates made to the discovery for which he received the recognition and shared one-quarter of the prize money with F.G.Brickwedde and G.M.Murphy.

In Urey's Nobel lecture, delivered on February 14, 1935, he called attention to the fact that Aston had just redetermined the physical atomic weight of hydrogen to be 1.0081. This value, if correct, would have brought the physical and chemical atomic weights of hydrogen into exact agreement and invalidated the basis of Birge and Menzel's prediction. Urey would not have undertaken the search for deuterium in 1931 and its discovery would have been delayed, perhaps for years. In 1932 Washburn and Urey discovered the electrolytic separation of deuterium from hydrogen. Dihydrogen gas generated by the electrolysis of water is depleted in deuterium. This fractionation explains the failure of Urey and Murphy to find any significant enrichment in deuterium in Brickwedde's first sample. Brickwedde took special precautions before he undertook preparation of the enriched samples. He took all of his equipment apart and cleaned it thoroughly to eliminate artifacts from impurities. Most significantly, the electrolyte in the cell used to generate the hydrogen to be liquefied was replaced by fresh alkaline solution. Brickwedde literally threw the baby out with the bath water. The dihydrogen produced from fresh alkaline solution is depleted in deuterium. The Raleigh distillation of this liquid hydrogen brought the deuterium con tent back to about natural abundance. As more and more water is added to the electrolytic cell to replace that electrolyzed, the deuterium abundances rise to the natural abundance level.

THERMODYNAMIC PROPERTIES OF ISOTOPIC SUBSTANCES

Urey's Nobel address was titled "Some Thermodynamic Properties of Hydrogen and Deuterium." The first part covered the discovery of deuterium. Two-thirds of the address dealt with the differences in the thermodynamic properties of isotopes and the feasibility of isotope separation based on these differences. By the time Urey initiated his work on deuterium, calculation of the thermodynamic properties of ideal gases from spectroscopic data had been placed on a firm foundation. Such calculations are particularly simple when one compares the differences in behavior of isotopic substances. Under the assumption of the Born-Oppenheimer approximation, the large enthalpy changes from the difference in the minima of the potential energies of products and reactants in a chemical reaction vanish for isotopic exchange reactions. Thus, Urey and Rittenberg calculated the differences in the degrees of dissociation of HCl(g) and DCl(g) and HI(g) and DI(g), respectively. They confirmed their calculations with experiments on HI(g) and DI(g). Gould, Bleakney, and H.S.Taylor confirmed the Urey-Rittenberg calculations on the disproportionation of HD into H_2 and D_2. The success of statistical mechanics to predict differences in the chemical properties of hydrogen and deuterium led Urey and Greiff to extend the method to isotopomers of polyatomic molecules of carbon, nitrogen, oxygen, and sulfur. For each of these elements, Urey and Greiff found exchange reactions with enrichment factors in the range from 1 to 4 percent at room temperature.

The predicted enrichment factors led Urey and Greiff to suggest the chemical exchange method for the separation of isotopes of the light elements. The small elementary effect was to be multiplied by countercurrent flow of two-phase systems. Each phase is to consist principally of one chemical species. When one phase is a liquid or liquid solution and the other is vapor, the process is entirely analogous to distillation. In fact, distillation technology can be readily adapted to chemical isotope separation. To replace the boiler and condenser of a distillation tower, one requires chemical reactors that convert one chemical species to the other quantitatively. The exchange reaction must be rapid and reversible. These principles led Urey and co-workers to develop the $NH_3(g)$-NH_4^+(sol'n.), $HCN(g)$-CN^-(sol'n.), and $SO_2(g)$-HSO_3^-(sol'n.) reactions for the enrichment of ^{15}N, ^{13}C, and ^{34}S, respectively, during the 1930s. Each of the reactions developed by the Urey school involved acid-base exchange reactions in aqueous solution. These are the fastest chemical reactions. Reflux was achieved by cheap reagents—acids and alkali. Compared with other isotope separation processes, centrifuges, and diffusion, the chemical exchange process and the related liquid-vapor distillation have large throughput per unit volume of separating equipment. Urey and T.I.Taylor also achieved a small enrichment of the lithium isotopes on zeolites, the forerunner of the ion exchange version of chemical isotope separation.

Most of the people who worked with Urey on isotope separation in the 1930s were postdoctoral fellows. This was rather unusual for the time in American universities. These were talented people interested in academic careers, for which there were few openings. In addition, there were professionals, a chemical engineer with expertise in distillation, and a recent Ph.D. in physics who built a Bleakney-type mass spectrometer for Urey's program. Urey had no difficulty getting support from foundations after he discov ered deuterium and received the Nobel Prize. In fact, he chose to share an award from the Carnegie Institution of Washington with a member of the Physics Department, I.I. Rabi. Rabi never forgot Urey's generosity and the impact it had on his program on molecular beam research. Urey was a good judge of talent; his investment in Rabi paid off handsomely for science, the Carnegie Institution, and Columbia University. Today, bureaucratic restrictions would make it impossible for someone like Urey to give part of a grant to another investigator, no matter how qualified or promising.

During the 1930s, Urey and his co-workers measured the vapor pressures of compounds enriched in D, ^{15}N, and ^{18}O. The values obtained for ^{18}O were utilized in the partial enrichment of ^{18}O by the distillation of water.

ISOTOPES AS TRACERS

Enriched stable isotopes of H, C, N, O, and S have found wide application in agriculture, biology, chemistry, geology, and medicine. Urey used some of his enriched isotopes, particularly ^{18}O, to carry out tracer studies. He and Cohn measured the acid and base catalyzed exchange between water and acetaldehyde and acetone. They showed that acids and alcohols do not exchange oxygen with water. They provided the basis for Roberts and Urey to show unequivocally that it is the carbon-oxygen bond in the acid that is broken in esterification reactions. Their result has been of major importance in the elucidation of the mechanism of this important class of reactions. The use of ^{15}N as a tracer in biochemistry was initiated by Rittenberg and Schoenheimer with enriched samples supplied by Urey.

PALEOTEMPERATURES

When Urey moved from Columbia to the University of Chicago at the end of World War II, he decided not to continue his interest in isotope separation or to undertake any research with direct military application. His first priority was to fill a prewar commitment to deliver the Liversidge lecture before the Chemical Society (London). For this purpose he decided to update and expand the earlier calculations of Urey and Greiff on isotope exchange equilibria using the advance method developed by Bigeleisen and Goeppert-Mayer at Columbia (SAM project) in 1943. The method afforded the possibility of calculating the temperature coefficient of an isotope exchange equilibrium constant in addition to the logarithm of the constant with confidence. In the course of these calculations Urey noticed that the fractionation factor for $^{18}O/^{16}O$ exchange between CO_3^{-2} and $H_2O(1)$ would decrease by 1.004 between O° and 25°C. Urey recognized the potential of utilizing this temperature coefficient to measure paleotemperatures.

The method depended on the development of isotopic assay methods with a precision of better than 0.1 percent in the $^{18}O/^{16}O$ ratio at the natural abundance level, which is 0.2 percent. Nier and Thode had each developed the dual collector method of measuring isotope ratios with a precision of 0.1 percent of the ratio. There were additional requirements to be met if the method were to be useful. Isotope exchange equilibrium would have to be established in the precipitation of $CaCO_3$ from H_2O. The record would have to be preserved over millions of years. It would be necessary to know the isotopic composition of the marine water in equilibrium with the $CaCO_3$. Urey assembled a research group that included graduate students, postdoctoral fellows, a paleogeologist, and an expert in electronics to attack these questions in a systematic way. They advanced the precision of measurements of isotope ratios by almost an order of magnitude and routinely obtained a precision of 0.02 percent in the $^{18}O/^{16}O$ ratio in CO_2. In the applica tion of their method, an unknown sample was intercompared with a standard using a dual inlet system. The results were expressed in δ o/oo=[($R_{sample}/R_{standard}$)−1]×1,000. The paleotemperature scale was calibrated by the isotopic analysis of $CaCO_3$ samples precipitated from water at known temperatures. The latter yielded a thermometric scale in terms of δ in good agreement with Urey's calculation and subsequent refined calculations by McCrea, a Ph.D. student. The final proof of the paleotemperature-scale concept came with the 1951 publication by Urey, Lowenstam, Epstein, and McKinney. They analyzed the $CaCO_3$ of a 100-million-year-old belemnite collected on the Isle of Skye by Cyril S.Smith. Samples of $CaCO_3$ at various distances from the axis of the belemnite core were analyzed for $^{18}O/^{16}O$. They found the fossil had a life history of four winters and three summers. The winter temperatures were 15°C; the summer temperatures were 21°C. The winters grew progressively colder during the lifetime of the belemnite.

Urey and his group founded a new branch of geology, which has flourished under the leadership of his associates and students and their students. For this achievement he received the Arthur L.Day Medal of the Geological Society of America and the Goldschmidt Medal of the Geochemical Society.

THE WAR YEARS, 1939–44: THE ATOMIC BOMB

Inasmuch as Harold Urey had studied with Bohr during his year in Copenhagen it was natural for him to attend the Fifth Washington Conference on Theoretical Physics in January 1939. It was at this conference that Bohr postulated that ^{235}U was the fissionable isotope. The possible need for separating the uranium isotopes was obvious. As the recognized world leader in isotope separation, Urey's main potential contribution to fission research was clearly in that field. He thus became one of the members of the dedicated group of scientists, centered at Columbia University, who investigated nuclear fission before government contracts were available and who solicited and ultimately obtained government backing.

Two papers written by Urey in 1938, “The Separation of Isotopes” (1939,1) and “Separation of Isotopes” (1939,5), throw light on the status of isotope separation at that time and on Urey’s speculations about methods for separating isotopes of the heavy elements. He proposed a countercurrent flow centrifuge, designed to attain a number of stages of separation in a single machine, thus reducing the number of machines required in a cascade and the amount of material circulated between machines. Countercurrent flow in a machine was to be established by continuous distillation of a liquid (uranium hexafluoride in the case of the uranium isotopes) from the bottom cap of the rotor and condensation on the top cap. The liquid would then be thrown to the periphery and flow down the walls, countercurrent to the vapor flow.

In a third paper, “Separation of Isotopes by Chemical Means” (1940,2), Urey concluded that separation of the uranium isotopes would lead to most interesting progress in the study of the fission process and discussed the centrifugal fractionation column (countercurrent flow centrifuge) as affording the separation method most likely to succeed. In early 1940 it was definitely established that ^{235}U was the isotope fissionable by thermal neutrons. Urey, together with a group of Columbia University faculty members, began work on uranium isotope separation in May 1940, and a contract with President Roosevelt’s Committee for Uranium for this work was executed in August. At approximately the same time, Urey was appointed chairman of an Advisory Committee on Nuclear Research to give tech nical advice to the Committee for Uranium. He coordinated experimental centrifuge studies at the University of Virginia; gaseous diffusion separation research at Harvard; and thermal diffusion, chemical separation, and centrifugal fractionation at Columbia.

Urey undertook personal direction of research on chemical separation of the uranium isotopes and on separation by the countercurrent centrifuge. The chemical separation involving uranium salts in immiscible solvents was not successful. The distilling centrifuge mentioned above was abandoned in favor of an all-gas countercurrent centrifuge, the theory for which was developed by Karl P.Cohen and the design was developed by Urey together with C.Skarstrom. Because some doubts had been raised by opponents of the centrifuge project about the stability of countercurrent gaseous flow, Cohen devised the theory and C.Skarstrom and Urey developed the design for a single-stage flow-through centrifuge. In early 1941 Westinghouse undertook to build a prototype of the flow-through design, a choice that had fatal consequences for the centrifuge project.

The Advisory Committee on Nuclear Research was soon reorganized under Vannevar Bush's National Defense Research Committee, and Urey and Dean George Pegram of Columbia University became members of a new parent Committee on Uranium. Urey had broad responsibilities for formulating the whole research program.

In view of some uncertainty in 1940 with respect to the feasibility of a divergent chain reaction using natural uranium with graphite moderator, Urey became interested in the use of heavy water as an alternative moderator because of its greater efficiency and its practically zero neutron absorption cross-section. Urey proposed using catalytic exchange between hydrogen and water to produce heavy water in quantity. He invited professor H.S.Taylor of Princeton to study this process.

Centrifuge work was undertaken in early 1941 by Westinghouse in Pittsburgh and also was continuing at the University of Virginia. Urey turned his attention to the gaseous diffusion process. He reported in November 1940 an initial appraisal of separation by diffusion through porous barriers. K.P.Cohen measured the first actual separation by barriers using CO_2/H_2 mixtures. Estimates of plant size based on these observations showed that a diffusion separation plant would involve as large an undertaking as the centrifuge plant.

The last half of 1941 found the uranium program in a stage of constant ferment. Plutonium had been shown to be fissionable. The English gaseous diffusion process seemed likely to succeed, and the Columbia diffusion system was not far enough along to evaluate properly. Work in Britain indicated metallic uranium, and heavy water provided the best route to a chain reaction. The British were convinced that weapons could be made from reasonably small quantities of ^{235}U. The Uranium Committee was reorganized. A new Office of Scientific Research and Development was created in the Executive Office of the President as the center for the application of science to national defense, and Urey was a member of its Section on Uranium. He was given responsibility for uranium isotope separation by exchange methods and for heavy water production. V.Bush and J.B. Conant had overall responsibility for the uranium program. The chain reaction program was reoriented to plutonium production and weapons production. An electromagnetic separation project had been initiated.

By the end of 1941 and early 1942 the program moved from the research to the engineering and construction phases. The attempt to arouse the government to the military po tential of uranium fission had finally succeeded. The chain reaction group at Columbia, headed by Fermi and Szilard, was moved to the University of Chicago. The scope of Urey's direct responsibilities in 1942 included the English diffusion separation method, the American diffusion method, and the centrifugation method. A decision had been made to transfer all uranium work to the United States, and Urey took special pains to see that the British diffusion ideas were seriously considered.

In May 1942 the Section on Uranium's Executive Committee, of which Urey was a member, was asked by J.B. Conant to recommend a program to build atomic weapons. Their proposal to Conant showed strong input from Urey in that it placed great emphasis on centrifuges and heavy water production. This program included construction of a centrifuge plant, a gaseous diffusion pilot and production plant, an electromagnetic plant, pile production of element 94, and a plant for heavy water production. It was estimated that the proposed plans would result in the production of a few atomic bombs by July 1944.

In the summer of 1942 the reported experimental results on flow-through centrifuges were disappointing, showing only 36 percent of theoretical efficiency. Urey's protestations that countercurrent centrifuges would be easier to build and were more efficient were to no avail. Centrifuge work remained at a low level. It is an irony of history that subsequent experiments in 1943 and 1944 proved that countercurrent machines could operate close to theoretical efficiency. At least six nations have at the present time operated countercurrent centrifuges with UF_6, and it is the uranium isotope separation method of choice for five of them.

In November and December of 1942 there was a commitment to a full-scale diffusion plant, a smaller electromag netic plant convertible later to full size, and heavy water plants.

The research organization at Columbia University under Harold Urey's direct supervision had been growing rapidly. In 1942 and 1943 Urey attracted many eminent scientists from academia and industry to assist in the development of components of the diffusion plant and in his other activities. By the end of 1943 Urey had more than 700 people working on gaseous diffusion alone and several hundred more, including those at other universities and industrial laboratories, working on various other researches. He had little taste for administration, and the burden weighed heavily on him.

This effort produced some notable successes. A new process was devised for producing heavy water, based on dual-temperature exchange between hydrogen sulfide and water. A successful method for separating the boron isotopes was developed for production of the crystalline ^{10}B needed at Los Alamos. Low-leakage seals for rotating equipment and mass spectrometers for process analysis, and leak detectors, needed for both laboratory research and plant construction, were devised and produced. Progress was made on fundamental theory of separation by diffusion barriers.

However, the barrier remained recalcitrant. Copper barriers were abandoned, and efforts were concentrated on nickel barriers. Both electrodeposited (Norris-Adler) and compressed powder barriers were tried. As 1943 wore on, it was realized that, despite heroic efforts, barriers with the properties, uniformity and ruggedness necessary for manufacture were not available. Nevertheless, a pilot plant for manufacture of the electrodeposited barrier was being completed.

Difficulties were also becoming apparent in the other production projects. The electromagnetic separators that had been installed in Oak Ridge, Tennessee, were experiencing severe operational problems. The laboratory in Chicago was openly critical of time schedules and of the graphite pile design that had been developed by the Dupont Company.

Urey saw his hopes for a contribution from the uranium program to the imminent 1944 war crisis fade, even as his fears of a German atom bomb remained lively. He renewed his efforts to realize a homogeneous heavy water uranium slurry reactor, proposed by Halban, for plutonium production. This led to the research piles in Chicago and later in the 1950s to the Savannah River plutonium production reactors. Urey championed P.Abelson's liquid thermal diffusion process, which seemed a last hope to achieve timely weapons production. A plant was hastily authorized in Oak Ridge in the second half of 1944 and was used to enrich the feed to the electromagnetic plant.

In the autumn of 1943 a new type of diffusion barrier, combining features of both the electrodeposited and compressed powder barriers that had been previously developed, was proposed by the Kellex Corporation. In the spring of 1944 a plant began producing acceptable barrier material of the previously developed electrodeposited type, whose production had been urged by Urey. Ten thousand workers had been building a huge diffusion plant at Oak Ridge. Early in 1944 the Army (General L.R.Groves commanding) made the decision to rely on the barrier developed at Kellex. Fortunately, both types of barrier eventually proved satisfactory. The first production from the gaseous diffusion plant occurred in March 1945. The plant operated with unprecedented reliability and economy during the postwar period, superseding all other methods, but most of the ^{235}U for the Hiroshima bomb was produced by the electromagnetic separation plant.

Early in 1944 when the decision was made to rely on the new barrier being developed by Kellex, it was clear to Urey that the diffusion plant would have little relevance to the war effort. He relinquished barrier development to his associate directors. Urey remained nominal head of the Columbia laboratories until 1945, but his heart was not in it. From that time forward his energies were directed to the control of atomic energy, not its application.

COSMOCHEMISTRY

Urey moved from Columbia to Chicago in 1945. Shortly thereafter he read a book by Ralph Baldwin, *The Face of the Moon,* which started him on a love affair with that object, which continued for the rest of his career. Colleagues at Chicago, or any available listeners, would be treated to monologues, sprinkled with the names of craters and other technical terms, which were impressive though bewildering. Urey came to regard study of the moon as a key to understanding the origin of the solar system. This led to a sustained, audacious attack on the broader problem.

His 1952 book, *The Planets,* is generally agreed to have begun the modern science of the solar system; it brought the term “cosmochemistry,” as distinguished from geochemistry, into the language. The work systematized the state of our knowledge at that time and set forth a research agenda emphasizing physicochemical and chronological study of meteorites, the oldest and least altered materials in our possession.

Two papers out of many in the following years were especially influential. Craig and Urey (1953,1) reclassified the meteorites using chemical criteria and set the stage for detailed comparison between meteorite (chrondite) chemical abundances and those of nonvolatile elements in the sun and other stars. Suess and Urey (1956,2) used meteoritic and solar abundances to make an improved table of abundances of the elements, showing clearly the influence of nuclear shell closure and other specific nuclear effects on elemental and isotopic abundances. This paper was the basis for the first successful account of the origin of the chemical elements in stars by Burbidge, Burbidge, Fowler, and Hoyle (1957). The intimate interplay between chemical and astrophysical problems became widely understood for the first time.

It is sobering to realize that when Urey wrote *The Planets* even so basic a fact as the age of the earth was not yet settled. He began to look for experimental areas beyond the $^{18}O/^{16}O$ system where his mass spectrometric skills could be applied. A young graduate student named Jerry Wasserburg turned up at Chicago, and Urey put him to work on the $^{40}K/^{40}Ar$ isotopic dating system. His thesis, which involved collaboration with R.J.Hayden of the Argonne National Laboratory and Professor Mark Inghram, was a first step on the path by which Wasserburg made a number of fundamental contributions to geochronology in subsequent decades.

Urey took great interest in the existence of diamonds in two classes of meteorites: stony objects called ureilites (not named for him) and big metallic meteorites like the one that made Meteor Crater in Arizona. He hoped that the diamonds were formed in thermodynamic equilibrium at high pressures. We know now that in the iron objects they were formed by shock; the situation in the ureilites is not so clear.

It was a natural step for a chemist thinking about the origin of the planets to think about the origin of life on this particular one and perhaps on Mars or elsewhere. Starting from the abundance of hydrogen in the sun and other stars, and the abundance of methane in the outer planets, Urey concluded that it was likely that the earth's atmosphere was originally reducing, rich in CH_4 and NH_3 rather than CO_2 and N_2. He suggested that thermodynamics favored the formation of organic compounds in such an atmosphere.

Not long after this a graduate student named Stanley Miller presented himself to Urey and proposed to do an experimental thesis testing this hypothesis in the laboratory. Urey told him it was too difficult for a thesis problem, but Miller won a grudging permission to try. Within a month he was exhibiting organic muck in a flask containing methane, ammonia, and water, excited by an electric discharge as a model for lightning discharges in such an atmosphere. Miller showed that the solution products contained amino acids and other possible precursor compounds for life.

Some of us were present at a crowded seminar in which Miller presented his results, with Urey in the front row. By the end of the presentation it was obvious to all that this was an important milestone. In the question period Enrico Fermi turned to Urey and said, "I understand that you and Miller have demonstrated that this is one path by which life might have originated. Harold, do you think it was *the* way?" Urey replied, "Let me put it this way, Enrico. If God didn't do it this way, he overlooked a good bet!" Today the assumed early reducing atmosphere is no longer widely accepted, but the impetus given by Urey still remains.

THE LA JOLLA YEARS

In 1958 Urey passed a milestone—his sixty-fifth birthday. When it became clear that he would then become emeritus at the University of Chicago, his friends at the newly forming University of California, San Diego, led by Roger Revelle, offered him an appointment there, and he accepted. That was the year after the Soviet launch of Sputnik I, and national attention was focused on space. The National Aero nautics and Space Administration also was new, and the first U.S. satellites were reaching orbit after some embarrassing failures.

At UCSD Urey joined a small number of younger faculty members who were planning a major university with a strong science and engineering side. He immediately started up a vigorous research program, involving both carbon and oxygen isotopic measurements (for paleotemperatures and other purposes) and comparable data for heavier solid and gaseous elements for dating. At the same time, his presence on campus gave a mark of quality to the place that other new foundations could not match. While professing himself unsuitable for any administrative functions, Urey was flying to Washington frequently to press advice on the new space agency. According to Robert Jastrow, it was Urey who persuaded NASA to make unmanned missions to the moon an early focus of its space efforts.

In 1960 UCSD formed its Department of Chemistry, with Urey as one of its founding members. Others were old friends and disciples—Joe Mayer, Jim Arnold, Hans Suess, and Stanley Miller. Urey was particularly emphatic about the importance of biochemistry, which became a major component of the developing department. In the following years he influenced the department and university mainly by example.

Urey was very active at the time of Apollo 11, when the first lunar samples were returned and the first data were appearing. Though he (unfortunately) almost never talked about the past, he told his colleagues one story that time. He told us that it was only in 1910, when he was seventeen years old, that he saw his first automobile in rural Montana. Less than sixty years later his friends showed him the first rock returned from the moon, an achievement in which he had played a significant role.

His powers of concentration, even into his eighties, were remarkable. He could think intensively about one problem for long periods; his well-known absentmindedness was the inverse of his sharp focus on one important problem at a time. He loved science.

One scene may give the flavor of the man at the end of his career. On any given morning he might burst into the office of a colleague eager to talk. Seeing the colleague perhaps discussing current research with students, he would apologize and begin to back out. Of course, he was invited in. He would then rush to the blackboard and begin "I've finally figured out…." He would soon be pouring out words faster than even close associates could assimilate them. "Does that seem right?" he would say at the end. Maybe one question or comment would emerge. He'd thank the group warmly, again apologize, and rush out. The effect of this display on young graduate students was remarkable.

Urey's last two scientific papers were written and published in 1977, when he was eighty-four years old. Years earlier the largest research building on campus, housing chemists and engineers, had been christened the Harold and Frieda Urey Hall, to recognize the role they both played in the founding and early development of UCSD.

UREY'S PERSONAL LIFE AND HIS POLITICAL AND EDUCATIONAL ACTIVITIES

Thus far we have been concerned with the scientific achievements of Harold Urey. He is also well remembered by all who knew him as a person intensely interested in the well-being of his fellow man. This concern was displayed not only for his students, research associates, and faculty colleagues but also with respect to social and political problems of national and international importance. He had a great interest in such problems, some of them closely related to the wartime work with which he had been involved.

He devoted the same concentrated effort and careful thought to their possible solutions as he did to the solving of the problems of his scientific researches. Having concluded that certain actions were required, he then, with the same vigor and determination that were characteristic of his scientific work, would bend every effort toward furthering these actions.

While at Columbia University he had been chairman of the University Federation for Democracy and Intellectual Freedom and a champion of loyalist Spain. As early as 1932 he espoused Clarence Streit's Atlantic Union plan for a world governmental federation. He became greatly disturbed by the rise of Hitler and the progress of Nazism. He was active in securing posts for refugee scientists and in extending his hospitality to them when they arrived in this country. In her book *Atoms in the Family,* Laura Fermi recounts how Harold and his wife Frieda helped her and her husband Enrico become their neighbors in Leonia, New Jersey, when the Fermis arrived at Columbia University from Italy.

As World War II ended, Urey, then at the University of Chicago, became concerned and worried about the potential of atomic bombs, in whose creation he had played such an important role. His interest in world government, begun at Columbia University, returned with renewed vigor. He worked diligently on the public speaker's platform and, through his writings presented in the press, toward the creation of a world free of the dangers and dread of war.

During this period of lecturing and writing on the problems created by nuclear energy developments, Urey actively opposed congressional passage of the May-Johnson bill, which he feared would permit military control of peacetime activities in the field of nuclear energy. He strongly supported the eventual McMahon bill in its final form and was a leader in the fight for its passage. His doubts concerning the jus tice of the executions of Ethel and Julius Rosenberg for atomic energy secrecy violations received national attention. His views on this matter were not ones that were popular with large sections of the American public. He was called before the House Un-American Activities Committee. He wrote, "I doubted seriously if justice had been done. I was only interested in one question. Had they indeed violated the laws of the United States and had justice been done? It is my firm conviction that justice was not done in that case."

Harold Urey was an educator, in the undergraduate and graduate classroom, in the research laboratory, and on the public platform. At the end of World War II, he returned to teaching at a time when many, including himself, felt that this country had, in the course of intensive war research, temporarily abandoned both basic research and the training of a new generation of scientists. At this time, when many were worried about how to keep the "secret" of the atomic bomb and how to prevent dominance by the Soviet Union, he wrote, "The real problem that faces this country is a long-term one. It is a problem of the proper education and inspiration of our youth." He approached with great zest the teaching not only of graduate students but also of first-year undergraduate chemistry courses. His interest in the "inspiration of our youth" even extended to public grade schools. His wife Frieda wrote, "He enjoyed nothing so much as taking his moon-globe to the fifth grade class in the La Jolla schools and telling the students about the moon and the planets."

Harold was fond of and proud of his family. While at Johns Hopkins University, he visited his mother, then living in Seattle. While on that visit he renewed his acquaintance with a friend from his University of Montana days, and she introduced him to her younger sister Frieda Daum, who was working as a bacteriologist. As Roger Revelle of La Jolla described it, "Harold Urey was never thought of as an outdoor man but he spent the next two weeks hiking in the Cascade Mountains with Frieda. Within a year they were married and their careers as mountaineers were ended. After that Harold's outdoor activity was confined to his garden." Frieda and Harold had three daughters (Elizabeth, Frieda, and Mary Alice) and one son (John). At the time Harold was notified of the award of the Nobel Prize, Frieda was expecting their third child. In order to be with Frieda he did not attend the December 10 ceremonies in Stockholm. Mary Alice was born on December 2, and Frieda and Harold sailed for Stockholm the following February and attended a special award ceremony.

The friendliness and hospitality of Harold and Frieda and their family brought people together socially in a way that created a most pleasant academic atmosphere and added greatly to the enjoyment of life on the part of the families of Harold's fellow faculty members and of the research associates and students in the universities of which he was a member.

Harold was greatly concerned with the welfare of his scientific colleagues, students, and postdoctoral research associates. He was always interested in his students' development of their own independent scientific careers. He was concerned that they be established in suitable posts upon completion of their researches in his laboratory and was active in locating suitable academic and other positions for them. He was diligent in seeing that they received appropriate credit for their work under his supervision. The first paper on the establishment of the Urey paleotemperature scale was published under the sole authorship of his student John McCrea. Likewise at Urey's insistence, the sole author of the first article on the Urey-Miller theory and experiment on the origin of terrestrial life was his student Stanley Miller.

Abolition of the boundaries between scientific disciplines was a basic tenet of Urey's philosophy. His own academic career, described above, was exemplary in this respect. Concerning his stay at Bohr's Institute for Theoretical Physics he said, "Bohr didn't know I was a chemist. He thought I was a physicist." He claimed he had learned most of his physics in Copenhagen restaurants while dining with Professor H.A.Kramers. In 1933 Harold founded the interdisciplinary *Journal of Chemical Physics,* which provided an appropriate medium for publication of the already-large body of work that bridged the traditional fields of chemistry and physics. He became the first editor of that journal, a position he held until 1940, by which time it had become a leading scientific journal.

Urey may be considered to have established at least four fields of scientific research: stable isotope chemistry, including isotope geochemistry, geochronology, and isotope separation; paleotemperature measurement; cosmochemistry; and the origin of terrestrial life. The scope of his interests and influence are reflected in the thirty-two chapters of the monograph *Isotopic and Cosmic Chemistry* contributed by former students, postdoctoral associates, and colleagues on the occasion of his seventieth birthday. Karl Cohen, in an obituary in the *Bulletin of the Atomic Scientists,* said, "Urey's pioneering work underlies every method of isotope separation successfully employed on a large scale, for every element from hydrogen to uranium." Craig, Miller, and Wasserburg, in their introduction to *Isotopic and Cosmic Chemistry,* wrote, "The measurement of the paleotemperatures of the ancient oceans stands as one of the great developments of the earth sciences; a truly remarkable scientific and intellectual achievement." Cohen, Runcorn, Suess, and Thode in the *Biographi* *cal Memoirs of the Royal Society of London,* speaking of Harold as "the founder of the field of cosmochemistry" wrote, "Urey, undoubtedly, was the first who rigorously defined this field by its problems and by asking precise questions." His ideas concerning the primordial atmosphere and the beginning of life on earth opened up a completely new approach to the study of the origin of life on this planet.

Urey's vigorous and concentrated pursuit of these researches and his enthusiastic interactions with those around him concerning his latest ideas continued to the end of his life. After his retirement at age sixty-five from the University of Chicago and his arrival at the University of California, his work continued with unabated intensity, and he published 105 scientific papers, 47 of them concerned with his study of the moon, in the remaining twenty-three years of his life. Some ten years after his retirement from Chicago he was asked by Professor James Arnold in La Jolla, "Harold, why do you put in so many hours at work?" Urey replied, "Well, you know I'm not on tenure anymore."

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HAROLD CLAYTON UREY

Born:	Walkerton, Indiana/April 29, 1893
Married:	June 12, 1926 to Frieda Daum
Children:	Gertrude Elizabeth Baranger Frieda Rebecca Brown Mary Alice Lorey John Clayton Urey
Hobbies:	Gardening and raising orchids (cattleya, cymbidium, and others)

EDUCATION

University of Montana, Missoula, 1914–17, B.S. in biology with a minor in chemistry

University of California, Berkeley, 1921–23, Ph.D. in chemistry with a minor in physics

American-Scandinavian Foundation Fellow, Niels Bohr Institute for Theoretical Physics, Copenhagen, 1923–24

PROFESSIONAL EMPLOYMENT

1911–14	Teacher in rural schools in Indiana, 1911–12; Montana, 1912–14
1918–19	Barrett Chemical Co., Baltimore, research chemist
1919–21	University of Montana, instructor in chemistry
1924–29	Johns Hopkins University, associate in chemistry
1929–36	Columbia University, associate professor of chemistry, 1929–34; Ernest Kempton Adams Fellow, 1933–36; professor of chemistry, 1934–45; executive officer, Department of Chemistry, 1939–42; director of war research, SAM Laboratories, 1940–45
1933–40	*Journal of Chemical Physics,* editor
1945–58	University of Chicago, Institute for Nuclear Studies: Distinguished Service Professor of Chemistry, 1945–52; Martin A.Ryerson Distinguished Service Professor of Chemistry, 1952–58
1956–57	Oxford University, George Eastman Visiting Professor
1958–70	University of California, San Diego, professor of chemistry-at-large

HONORS, PRIZES, AND AWARDS

1934	Nobel Prize, Chemistry Willard Gibbs Medal, American Chemical Society
1935	Silver Medal, Research Institute of America
1940	Davy Medal, Royal Society, London
1943	Franklin Medal, Franklin Institute
1945	Manhattan Project Certificate of Award for Service, U.S. War Department
1946	Medal of Merit, President Harry S.Truman
1950	Distinguished Service Award, Phi Beta Kappa Centennial Award, Northwestern University
1957	Jesuit Centennial Citation, Chicago
1960	Silver Medal, Research Institute of America Cordoza Award, Tau Epsilon Rho Law Fraternity
1961	Alexander Hamilton Award, Columbia University
1962	J.Lawrence Smith Medal, National Academy of Sciences
1963	Remsen Memorial Award, American Chemical Society, Baltimore Section
1964	University of Paris Medal National Medal of Science
1966	Gold Medal, Royal Astronomy Society, London American Academy of Achievement Award, Golden Plate Award
1967	Man of Distinction Award, Women's Guild of Temple Emanu-El, San Diego
1969	Chemical Pioneer Award, American Institute of Chemists Arthur L.Day Medal, Geological Society of America Leonard Medal, Meteoritic Society
1970	Linus Pauling Award, Oregon State University
1971	400th Anniversary of Johann Kepler Medal and Citation, American Academy of Arts and Sciences
1972	Gold Medal Award, American Institute of Chemists
1973	Honorary Council and Medal, Higher Council of Scientific Research, Barcelona Silver Medal, 50th Anniversary of International Fair of Barcelona Knights of Malta Award Priestley Medal, American Chemical Society

	NASA Medal for Exceptional Scientific Achievement
1974	Headliner Award, San Diego Press Club Medallion, Honorary Member of Indiana Academy, Indianapolis Dedication of the Harold C.Urey Laboratory for Isotopic Paleotemperature Research, University of Miami, Coral Gables, Florida.
1975	V.M.Goldschmidt Medal, Geochemical Society
1976	NASA Group Achievement Award, U.S. Members of Joint Editorial Board for Foundations of Space Biology and Medicine, for joint US/USSR treatise
1978	Honorary member UCSD chapter of Phi Beta Kappa

HONORARY DEGREES

1935	Princeton University, D.Sc. University of Montana, D.Sc.
1939	Rutgers University, D.Sc.
1946	Columbia University, D.Sc. Oxford University, D.Sc.
1948	Washington & Lee University, D.Sc.
1951	Yale University, D.Sc. University of Athens McMaster University, D.Sc.
1953	Indiana University, D.Sc.
1955	University of California, LL.D.
1957	University of Birmingham, D.Sc. University of Durham, D.Sc.
1958	Wayne State University, LL.D.
1959	Hebrew Union College, Jewish Institute of Religion, D.H.L.
1960	University of Saskatchewan, D.Sc.
1962	Israel Institute of Technology, D.Sc.
1963	Gustavus Adolphus College, D.Sc. University of Pittsburgh, D.Sc. University of Chicago, D.Sc.
1965	University of Notre Dame, LL.D.
1966	University of Manchester, D.Sc.
1967	University of Michigan, D.Sc.
1969	Franklin and Marshall College, D.Sc.
1970	McGill University, D.Sc.

PROFESSIONAL ASSOCIATIONS

Academia Scientiarum Olisiponensis, Lisbon (Lisbon Academy of Sciences)
Academie Royale des Sciences, des Lettres et des Beaux Arts de Belgique (Honorary)
American Academy of Arts and Sciences
American Association for the Advancement of Science
American Association of University Professors
American Astronomical Society
American Astronautical Society (fellow)
American Chemical Society
American Geophysical Union (honorary fellow)
American Institute of Chemists (honorary)
American Philosophical Society
American Physical Society
Associacion Venezolana para el Avance de la Ciencia (honorary)
Chemical Society, London (honorary fellow)
Federation of American Scientists (life member)
Franklin Institute (honorary)
French Chemical Society (honorary)
German Society of Aeronautics and Astronautics (honorary)
Geological Society of America
Illinois State Academy of Science
International Association of Geochimica and Cosmochimica
International Astronautical Academy
International Platform Association
Mellon Institute (honorary)
Meteoritical Society (fellow)
National Academy of Sciences
National Institute of Sciences of India (honorary)
Phi Sigma Biological Society (honorary)
Royal Astronomical Society, London (associate)
Royal Institution, London (honorary)
Royal Irish Academy (honorary)
Royal Society, London (foreign member)
Royal Society of Arts and Sciences, Göteborg
Royal Swedish Academy (honorary)
Smithsonian National Association
Société Royale des Science de Liège (foreign member)

Weizmann Institute of Science (honorary fellow)

World Academy of Arts and Sciences, American Division (corresponding member)

CLUBS

Chemists' Club, New York (honorary)

Cosmos

Quadrangle and Tavery (Chicago)

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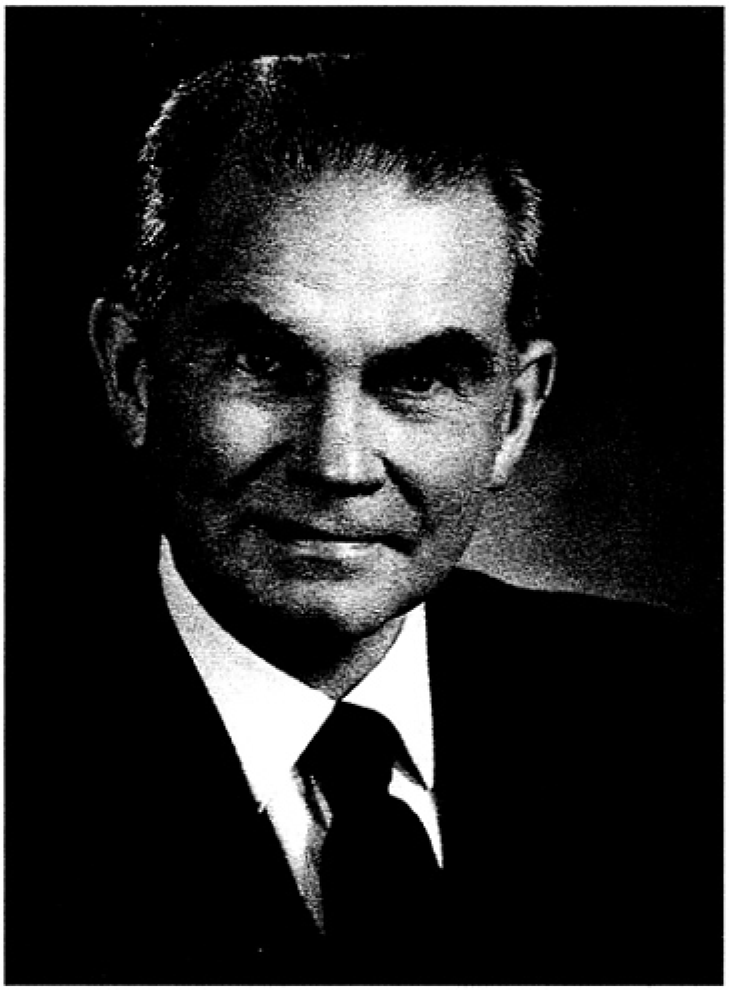

Carroll M. Williams

CARROLL MILTON WILLIAMS

December 2, 1916–October 11, 1991

BY A.M.PAPPENHEIMER, JR.

ARROLL MILTON WILLIAMS was born in Richmond, Virginia, on December 2, 1916. Even in his early school days he showed great interest in science and soon after entering the University of Richmond at the age of sixteen began collecting and studying lepidoptera. Upon graduation, he gave his outstanding collection to the university.

Carroll published his first paper on butterflies in 1937, when he was twenty, just before graduating from college. In the fall of that year he became a graduate student at Harvard University, where he was to remain for the rest of his life. His thesis adviser was Professor Charles Brues, a well-known entomologist. Carroll's remarkable and brilliant thesis was titled "A Morphological and Physiological Analysis of the Flight of *Drosophila,* with Special Reference to Factors Controlling Wing Beat" and was written in what was to become Williams's characteristic and unique style with its humorous overtones. In collaboration with Leigh Chadwick and with advice from Professor Edgerton of MIT, Carroll designed a small apparatus that measured accurately and reproducibly, by a stroboscopic method, the wing beat frequency of both wild-type and mutant strains of tiny fruit flies in flight under a wide variety of conditions, such as temperature, atmospheric pressure, O_2 tension, etc. Individual flies from various inbred strains varied between 12,000 and 14,000 beats per minute of sustained flight until exhaustion set in after as much as three hours, or more than 2 million double wing beats. He measured the glycogen content of the thoraces of the blowfly (a slightly larger insect) during flight to exhaustion and determined the energy expended in terms of glucose consumption. Finally, he succeeded in demonstrating, where others had failed, the neuromuscular network in the thorax that controls the wing beat.

In 1941, after receiving his Ph.D., Carroll was appointed a junior fellow of the Harvard Society of Fellows. It was clear from his thesis that he needed a larger experimental animal than *Drosophila* to pursue the studies he contemplated on insect development and morphogenesis. He therefore selected the giant silkworm, *Hyalophra* (formerly *Platysamia*) *cecropia,* as his experimental animal and soon made the important and useful observation that insects can be anesthetized for long periods of time, under continuous flow of carbon dioxide in a Buchner funnel, thus permitting surgical manipulations without loss of blood or damage. While still a junior fellow, he decided to obtain a medical degree and in 1946 received his M.D. summa cum laude from the Harvard Medical School.

The years that followed were exciting and fruitful ones. Carroll was appointed assistant professor of biology in 1946, promoted to associate professor two years later, and became professor of zoology in 1953 at the age of thirty-six. Finally, in 1965, he was appointed the first Bussy Professor of Biology. As one of his graduate students during the earlier period wrote:

> When I think of Carroll's achievements, I am overwhelmed by memories of

> hilarious events and merry times. And I am sure this was one of the reasons for Carroll's success in attracting students and bringing out the best in them: life in his lab was usually such fun and we all shared so many laughs.

Cecropia moths lay their eggs in early summer. After hatching, the tiny caterpillars grow rapidly and after four molts attain a length of more than three inches. They then spin a cocoon inside of which they metamorphose and enter a prolonged period of pupal diapause over the winter. If pupae that have entered diapause are placed at 3° to 5°C for a few weeks, adult development may be initiated promptly by removing them to warm temperatures. Without this period of chilling, adult development will not begin for many months, if indeed at all. Carroll began his studies on adult development by placing diapausing pupae in different orientations under temperature gradients with one end kept at 3° to 5° C and the other at 25° to 30°C. He observed that although development began in the *chilled anterior* end, once started, the heated end developed faster. It was these initial observations that led Carroll to publish a long series of remarkable and highly original papers in the *Biological Bulletin* on the physiology, biochemistry, and hormonal control of insect diapause and adult development. Many of these and his subsequent papers and lectures were illustrated by the excellent photographs and slides made by his wife Muriel.

Carroll's experiments were often amusing as well as ingenious and revealing. He began with parabiotic experiments in which he joined together by their heads diapausing pupae and *chilled* diapausing pupae. Almost simultaneously, both began to develop into adult moths. He soon found that removal of the brain from a pupa leads to permanent diapause but that adult development took place promptly if the brain from a *chilled* pupa was dropped into a brainless diapausing pupa at 25°C, even if the latter was of a differ ent species, such as *Antheraea* (formerly *Telea*) *polyphemus* or *Samia cynthia.* By means of plastic windows placed in either the face or the tip of the abdomen, development could be followed day by day from its onset until emergence of the adult moth twenty-one days later. Although it was evident that the *chilled* brain secreted a hormone necessary for initiation of adult development, it was soon shown that this was not a sufficient condition. When brainless pupae were cut in half and chilled brains were dropped into each half-pupa, only the anterior half went on to develop into *half* an adult moth! However, if both a *chilled* brain and a bit of prothoracic “gland” tissue, dissected from a normal pupal thorax, were dropped into the posterior end, an adult abdomen developed (see Figure 1). Further work showed that a tropic hormone was synthesized by a set of eleven neurosecretory cells in the anterior part of the chilled brain that activated the prothoracic glands to produce a growth and development factor. In 1954 Peter Karlson, working in Butenant’s laboratory in Germany, isolated 25 mg of the crystalline growth factor from 500 kg of *Bombyx mori* pupae. He named it *ecdysone* and determined its steroid structure.

It was obvious that each morphological change from larva to diapausing pupa and finally to an adult moth must be accompanied by dramatic changes in metabolism. During the next few years these changes were studied by Carroll and his students—R.C.Sanborn, H.A.Schneiderman, D. G.Shappirio, and W.R.Harvey. It came as no surprise to find that oxygen consumption dropped precipitously upon entering diapause and rose again during adult development. Nor was the fact that, upon entering diapause, most components of the cytochrome system were broken down and, with the exception of the intersegmental muscles of the pupal abdomen, tissue respiration, including that of the heart, which continued to beat slowly, became insensitive

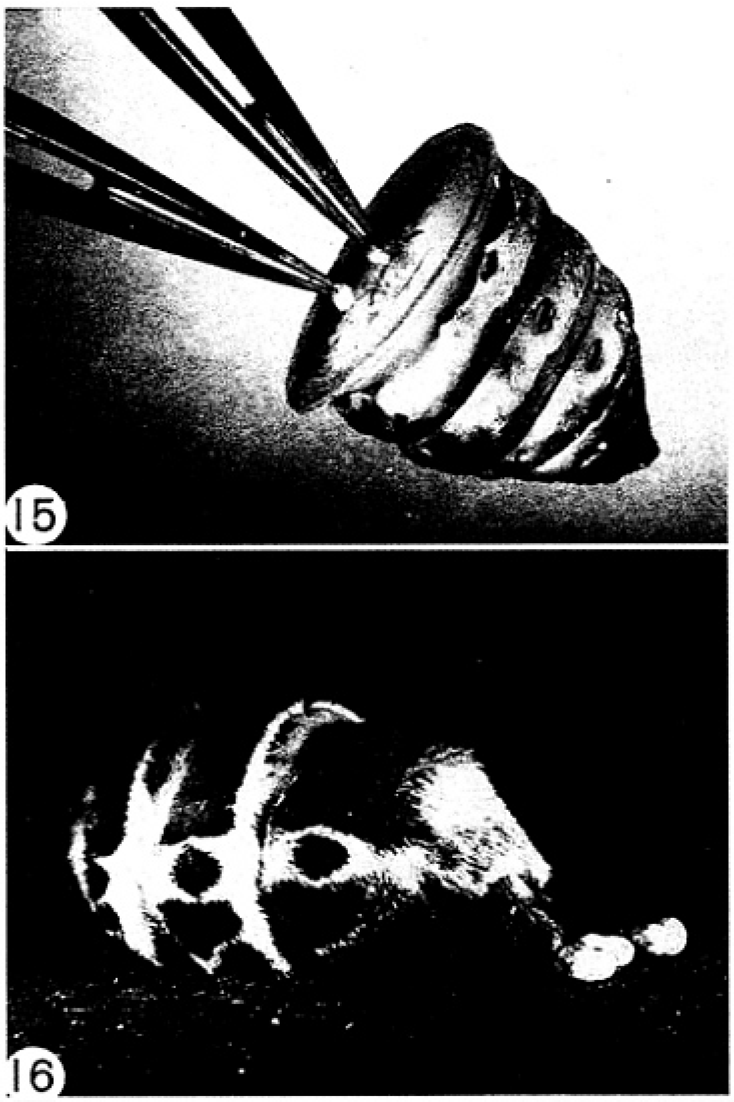

FIGURE 1 Upper: Brain and prothoracic glands obtained from previously chilled pupae being implanted into an isolated pupal abdomen. Lower: Implanted endocrine organs have caused adult development of the abdomen, which is shown laying eggs.

to inhibition by cyanide or carbon monoxide. These studies led to the discovery of a new cytochrome X (later renamed cytochrome b_5) present in the caterpillar midgut and in the pupal heart during diapause.

While these metabolic studies were in progress, Carroll became interested in juvenile hormones, which V.B. Wigglesworth showed many years before to be secreted by the *corpora allata,* two small glands connected to the brain of the bug *Rhodnius* by a pair of tiny nerves. This hormone opposes metamorphosis. Carroll made the surprising observation that excision of the *corpora allata* from *chilled cecropia* pupae had no effect on development into normal fertile adult moths, although when removed from adults and tested, they proved to be more active than at any stage of its life history. However, during the very early stages of adult development, addition of the hormone caused transformation into a second pupa with mere traces of adult characteristics. Finally, the most surprising finding was that the highest concentrations of all were present in the abdomens of adult *male cecropia* moths. Carroll found that the hormone could be extracted from homogenates of male abdomens by petroleum ether, which yielded a potent water-insoluble oil upon evaporation. Even after 50,000-fold purification, the active component still contained impurities, but the purest preparations had the properties of a terpenoid acid, and certain synthetic derivatives of farnesoic acid had potent juvenile hormone activity.

In 1964 Dr. Karel Slama came from Czechoslovakia to work in Carroll's laboratory, bringing with him fertile eggs from the bug *Pyrrhocoris* that he had been rearing in Petri dishes without difficulty in his Prague laboratory for ten years. In the Harvard laboratory, however, all larvae continued to experience additional larval molts and died without becoming adults. The difficulty was finally traced to Scott paper toweling that had been placed in the rearing jars. When replaced by Whatman filter paper, all larvae developed normally into adult bugs. The following is a quotation from a paper by Slama and Williams published in the *Proceedings of the National Academy of Sciences* in 1965:

> Indeed, pieces of American newspapers and journals *(New York Times, Wall Street Journal, Boston Globe, Science,* and *Scientific American)* showed extremely high juvenile hormone activity when placed in contact with *Pyrrhocoris* larvae. The *London Times* and *Nature* were inactive.

The active factor could be extracted from Scott paper towels with organic solvents and was found to be a petroleum ether-soluble oil that was highly active as a juvenile hormone when tested on *Pyrrhocoris* but that had no effect on metamorphosis of *Cecropia.* The factor could easily be extracted from American balsa fir, but only traces were present in European paper pulp. Even in his first paper on juvenile hormone, which appeared in 1956 as a letter to *Nature,* Carroll realized its potentiality as a pesticide and wrote as follows:

> In addition to the theoretical interest of the juvenile hormone, it seems likely that the hormone, when identified and synthesized, will prove to be an effective insecticide. This prospect is worthy of attention because insects can scarcely evolve a resistance to their own hormone.

By the mid-1970s this prediction had been verified. Three closely related juvenile hormones had been synthesized, and several substances that derail development of certain insect species by turning off secretion of juvenile hormone by the *corpora allata* had been isolated from plants in other laboratories. The chemical industry was engaged in synthesizing hundreds of cheaper and more stable analogs of these compounds. In a number of cases, these analogs have shown highly *specific* activity for certain insect species. For example, the juvenile hormone analog Methoprene is now in use in controlling the floodwater mosquito *Aedes nigromaculus,* a species that had become resistant to conventional pesticides. Less than 5 g spread per acre gives good control.

About this time, Carroll found that he was unable to mate *polyphemus* moths in the laboratory. The larva of this moth feed on oak. With Lynn Riddiford, he soon found that, after adding a few oak leaves to the cages, the females secreted a pheromone that attracted males and stimulated mating. (I cannot resist quoting here from a short note published in *Science* by Riddiford and Williams: “The action of oak factor on the female can be masked by other volatile agents including Chanel No. 5.”

In 1970 or 1971 Carroll abandoned his favorite experimental animal, *cecropia,* and switched to *Manduca sexta* (the tobacco hornworm). The great advantage of *Manduca* is that it can be raised, even during the winter, on a simple artificial diet, making possible the study of the larval stages all year round.

Manduca larvae during their final fifth instar increase their weight within four to five days from about 1 g to as much as 10 g, after which they stop feeding and purge their gut, reducing their weight to 5 g. After the purge the larvae enter a “wandering stage” that soon ends in an abrupt onset of negative phototaxis. In the wild, larvae would then dig into the ground before pupation. The experiments at the Harvard laboratory, however, were carried out on a twelve-hour light/dark cycle. Most of the experiments on hormonal control of *Manduca* development were carried out by postdoctoral fellows and students during the decade before Carroll’s retirement to emeritus status. *Manduca* has now become one of the most important model systems for the study of insect physiology, development, neurobiology, and molecular biology.

During the last few years before his final illness, Carroll did very little at the bench himself. Nevertheless, he kept up with current literature and realized that many of the questions raised by his experiments on insect development and the regulation of insect hormone expression could be answered by the techniques of present-day molecular biology. After departmental colloquia he often rose to ask visiting lecturers important and penetrating questions relating to the biological significance of their molecular findings, even though the subject might be quite remote from his own field.

Carroll enjoyed teaching and was not only an entertaining and popular lecturer but also stimulated many students to become interested permanently in biology and often to seek to do graduate study under his guidance. But he never directed a large team of graduate students and postdoctoral fellows as is so often the case in molecular biology today. Almost all of his students came to his laboratory because they fell under his spell while listening to his lectures or because they were fascinated by his published experimental work. Carroll's five o'clock laboratory teas were legendary and were attended by everyone, from undergraduates to visiting professors. The following are direct quotations from letters written to me by three of his former graduate students, now tenured professors of biology at the State University of New York at Stonybrook, the University of Washington, and the University of Michigan, respectively:

> Carroll was also exceptional—certainly by the standards of today—in his willingness to point students to problems that were quite remote from the work he did himself. He did not hesitate to launch students on projects that required techniques he had never used and which were founded on principles about which he had little knowledge. Carroll would learn along with the student and seemed always to contribute the needed experimental trick or flash of insight.

> Carroll was always full of ideas and tried to instill into his graduate students and postdoctoral associates the importance of doing experiments to test ideas particularly those that seemed far-fetched. He had little patience with students who would find various theoretical reasons why something might not work and would not go to the lab to test an idea. He also, though, was a hard task master in ensuring that experimental results were repeatable and that further experiments necessary to explain the results were done before they were published. Hence, many of his papers talk about work done over years.
>
> In the years I was a graduate student, we had tea each afternoon. Undergraduates, graduate students, postdoctoral fellows and visitors regularly attended. Of all the insect hormones then known (and perhaps now known), juvenile hormone was the most mysterious and fascinating. At the tea-table I heard what I think were his first statements about using it and perhaps other insect hormones as insecticides of the future or third generation pesticides.

It will come as no surprise to learn that Carroll was in much demand as a gifted lecturer. He was invited to deliver more than forty named lectures, among which, to mention only a few, were the Lowell lectures in Boston (1948); the Harvey Lecture in New York (1952); the AAAS Holiday Lecture, University of Chicago (1970); and the CSIRO Lectures in Australia (1973).

Carroll was elected a fellow of the American Academy of Arts and Sciences in 1951 and served on its council from 1952 to 1955 and again from 1974 to 1977. He was elected to the National Academy of Sciences in 1960 and was a member of its council from 1973 to 1976 and again from 1985 to 1988. He was chairman of the Section on Biological Sciences from 1981 to 1984. He also became a member of the Philosophical Society in 1969 and was a member of numerous other learned societies, including the Pontifical Academy of Rome.

I AM GREATLY INDEBTED to Professors Lynn M.Riddiford, William G. Van der Kloot, and David G.Shappirio for sending me their reminiscences of Carroll and to Daniel Branton and Fotis Kafatos for critical reading of the manuscript.

HONORS AND DISTINCTIONS

Borden Research Award, Harvard Medical School, 1946
AAAS-Newcomb Cleveland Prize, 1950
Guggenheim Fellowship (Cambridge University), 1955–56
Founders Memorial Award, Entomological Society of America, 1958
Boylston Medalist, Harvard Medical School, 1961
Trustee of Radcliffe College, 1961–64
George Leslie Award, Harvard University, 1967
Howard Taylor Ricketts Award, University of Chicago, 1969
Chief scientist to *Alpha Helix* expedition to the upper Amazon

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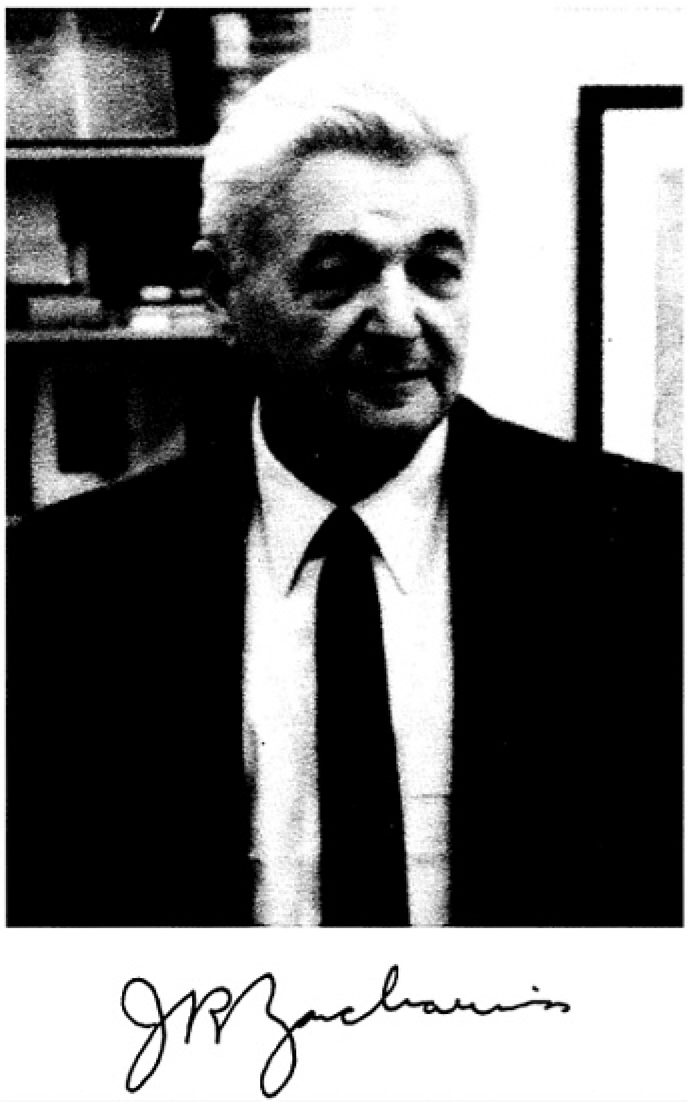

JERROLD R.ZACHARIAS

January 23, 1905–July 16, 1986

BY NORMAN F.RAMSEY

ERROLD ZACHARIAS MADE important, creative, and lasting contributions to physics, national defense, and education. He participated in the first molecular beam magnetic resonance experiments; he measured nuclear magnetic moments and electric quadrupole moments of various nuclei, including the proton and deuteron; he confirmed the anomalous hyperfine separation in atomic hydrogen; and he developed the first commercial atomic clocks. During World War II, he developed radar systems at MIT and nuclear weapons at Los Alamos. In the years following the war he initiated and led important national defense studies. As a result of his deep concerns about science education in the United States, he initiated numerous fruitful studies to improve it. He established the Physical Sciences Study Committee (PSSC), which not only made successful recommendations but also stimulated the production of movies and textbooks. The PSSC became a model for similar educational reforms in mathematics and most other sciences.

EARLY YEARS

Jerrold Zacharias was born on January 23, 1905, in Jacksonville, Florida. His father, Isadore, was a lawyer with interests in real estate, and his mother was a talented violin ist. His childhood and education are well described in an excellent biography by Jack S.Goldstein.[1] Jerrold had little interest in music, while his sister, Dorothea, and his adopted brother, Beryl Rubenstein, were talented musicians. Their musical mother was consequently less interested in Jerrold, who found real companionship in his nurse, Anna Liza Johnson, who later cared for Jerrold's children in New York City.

Jerrold's interest in physics began at age four with a fascination with his grandfather's automobile and continued into his teens with interests in photography, crystal set radio, and the Tom Swift books. Although he received excellent grades in high school, including his first physics course, Jerrold later complained that he did not learn much.

Soon after graduation from high school, his family moved to Riverside Drive in New York City, where Jerrold entered Columbia as an engineering student. By his sophomore year, he found physics challenged him more than engineering, so he changed to a major in mathematics with a minor in physics. Jerrold did well as an undergraduate but later described himself as having been a fraternity playboy with a generous allowance, a raccoon coat, and a Packard touring car. In 1925 as a Columbia undergraduate, Jerrold, on a blind date, met Leona Hurwitz, a delightful, serious biology major at Barnard. They married two years later. Leona remained his lifelong companion and became the mother of Susan and Johanna.

After graduating, Jerrold entered Columbia graduate school and began research with Shirley Quimby in solid state physics, a barely recognized research field at that time. Quimby suggested the problem of measuring the stress-strain relationship (Young's modulus) and internal friction in crystals of nickel by studying their vibrations. Zacharias discovered an unexpected diminution of internal friction of the crys tals at the Curie temperature, where their magnetic properties also change dramatically.

RESEARCH AT COLUMBIA

Jerrold Zacharias completed his thesis research in 1931— the middle of the great depression, when few jobs were available. He succeeded in obtaining an appointment as a tutor at Hunter College at a salary of $2,000 per year and with a heavy teaching schedule.

Zacharias was an excellent teacher but was eager to return to experimental research as well, so, in addition to his Hunter teaching duties, he soon arranged to work with I.I. Rabi of Columbia University. He spent his free evenings and weekends in Rabi's atomic beam laboratory as an unpaid research associate. Rabi had invented an atomic beam method for measuring the magnetic moments of nuclei by their interaction with the magnetic moment of the electron, which affected how the atom would be deflected by an inhomogeneous magnetic field. With his first graduate student, Victor Cohen, Rabi was applying this method to the measurement of the nuclear magnetic moments of sodium and cesium. Zacharias was especially interested in simpler fundamental atoms, so he joined with Rabi and Jerome M.B.Kellogg, a young Columbia instructor newly arrived from Iowa, on the more difficult task of measuring the nuclear magnetic moments of hydrogen and deuterium. They confirmed Otto Stern's earlier observation that the magnetic moment of the proton was much larger than the theoretically expected value of one nuclear magneton. These observations provided the first indications that the proton had a complex internal structure. Later Zacharias and his associates improved their results by using the new zero moment method and then further modified their apparatus to mea sure the signs of the proton and deuteron magnetic moments.

In the summer of 1937, Zacharias, S.Millman, P.Kusch, and Rabi measured the magnetic moments of indium and gallium and found a small contribution to the deflection patterns from the thermally excited higher energy states. By studying these contributions they could also measure the nuclear quadrupole moments of these nuclei. With characteristic humor, Jerrold described this period of research as indium summer. I had the good fortune to join the Columbia atomic beam laboratory at this time and to work on this experiment as an apprentice. It was a great experience and I learned much, ranging from apparatus design and vacuum techniques to data analysis and theoretical principles. Although Jerrold had not yet acquired his fame in science education, he was a talented teacher. He taught me one particularly memorable lesson. I had just given what seemed to be a highly successful colloquium and Jerrold asked me to hand him my third slide. Instead of the expected question about its contents, he dramatically broke the slide over the edge of the lecture table and said, “Don’t ever again show a slide that cannot be read from the back of the room”—a painful lesson I have never forgotten.

In 1937 Jerrold was appointed assistant professor at Hunter College and Leona obtained her Ph.D. and an appointment as instructor at the Columbia College of Physicians and Surgeons.

Also in 1937 the research at Columbia was revolutionized by Rabi’s invention of the magnetic resonance method. The first successful experiment with this method was by Rabi, Millman, Kusch, and Zacharias. This experiment was the forerunner of many different kinds of resonance experiments, including nuclear magnetic resonance and paramagnetic resonance. The first resonance experiment was soon followed by the experiments of Kellogg, Rabi, Ramsey, and Zacharias, which not only obtained accurate values for the proton and deuteron magnetic moments but also discovered the electric quadrupole moment of the deuteron, which in turn showed the existence of a new kind of force—a tensor force—between the proton and neutron.

Zacharias then turned his attention to measuring the magnetic moment of ^{40}K, since some theorists had predicted that its unusually long lifetime might be associated with a large nuclear magnetic moment. The measurement was successful though difficult because only one atom in 10,000 of natural potassium is ^{40}K. In 1940 Jerrold and I were invited to give talks on our magnetic and electric dipole moment measurements at a meeting of the American Physical Society in Seattle, Washington, so Jerrold drove my new wife and me cross country to Seattle and then back to Glacier Park where we were joined by Leona. The trip was scientifically and scenically exciting but politically distressing since the Nazis were moving through France almost as rapidly as we moved across the United States. It appeared that the war would soon be upon us.

THE WAR YEARS

Our fears soon proved to be valid; within the next four months Zacharias, Rabi, and I had moved to the MIT Radiation Laboratory, where we worked on microwave radar.

Zacharias's first task at MIT was to establish a lab on the MIT roof and build a radar system there capable of detecting distant objects. His success led to his being asked by the Navy to install a radar set on a destroyer. When this was successfully completed, Zacharias became the Radiation Laboratory representative at the Bell Telephone Laboratories in Whippany, New Jersey, during the development of a production model for the MIT 10-cm night fighter radar.

He then went to England to adapt the Oboe blind bombing system from long wavelengths to microwaves. He returned to the Radiation Laboratory to head the Transmitter Components Division, and in the spring of 1945 he accepted the offer of a full professorship at MIT while remaining on leave to continue his radar research.

Zacharias left MIT to go to Los Alamos a few months before the end of the war to head the Ordnance Engineering Division, a position vacated by Captain (later Admiral) W.S.Parsons, who had left for the Pacific island of Tinian. That division later became the Z division, which evolved into the Sandia National Laboratories.

NUCLEAR SCIENCE, ATOMIC BEAMS, AND ATOMIC CLOCKS

At the war's end Jerrold Zacharias returned to MIT as professor of Physics and director of the newly established Laboratory for Nuclear Science and Engineering. He quickly arranged for financial support from the newly established Office of Naval Research and organized a strong research staff, including B.Rossi, J.G.Trump, M.Deutsch, R.J.Van deGraff, R.D.Evans, I.A.Getting, V.F.Weisskopf, M. Benedict, and C.D.Coryell. Zacharias was also an MIT representative on the Initiatory University Group and later the Associated Universities, Inc. (AUI), which established Brookhaven National Laboratory.

Zacharias and his associates established a vigorous and effective atomic beam research laboratory. They independently confirmed that the experimental value of the atomic hydrogen hyperfine structure disagreed with the theoretical value, a disagreement that stimulated the development of relativistic quantum electrodynamics. They measured a number of nuclear spins and magnetic moments and for the first time detected a nuclear magnetic octupole moment.

Zacharias at this time became interested in developing atomic clocks and pursued two versions concurrently. One was a cesium atomic beam clock using my separated oscillatory field method,[2] well engineered for reliability and commercial applications, including a source and vacuum system that could be operated for years rather than hours. He cooperated with the National Company in developing a commercial clock known as the Atomichron. The availability of this highly successful cesium atomic beam clock contributed greatly to the adoption of atomic time and to the international definition of the second as 9,192,631,770 oscillations of the cesium atom.

His other version had the potential for much greater accuracy but the risk of total failure. A very slow beam of atomic cesium was directed upward and allowed to fall as a fountain, with separated oscillatory field excitation on the way up and down. The half second required for the roundtrip in the fountain was approximately fifty times greater than that for an atom to traverse the oscillatory field region of a conventional atomic beam apparatus, so the resonance width, by the Heisenberg uncertainty principle, would be fifty times narrower with correspondingly increased clock accuracy. Despite valiant efforts by Zacharias and his associates, the fountain experiment failed because the numbers of ultra-slow atoms in the beam were far below theoretical predictions, probably due to scattering in the nonequilibrium region between the slits. It is of interest to note that thirty years later, in 1989, Steven Chu[3] succeeded in making an atomic fountain by using the new laser cooling techniques to produce ultra-slow atoms. The atomic fountain with laser cooling is now one of the most promising prospects for increasing the accuracy of clocks and frequency standards.

DEFENSE STUDIES

As the Cold War intensified in the late 1940s, Zacharias became involved in defense studies. The first of these was Project Lexington, commissioned by the Atomic Energy Commission in 1948 to study the feasibility of nuclear-powered aircraft. Although deputy director of the study, Zacharias was frustrated by its being limited to the narrow question of the feasibility of nuclear-powered aircraft to the exclusion of the broader questions of their practicality and desirability. He learned from this experience that later studies that he organized or directed must have broadly defined missions.

For the Navy he directed Project Hartwell, on the threat by Soviet submarines. At his insistence the mission was so broadly defined that the title of the final report was *A Report on the Security of Overseas Transport*. The conclusions included recommendations for a new high-speed merchant fleet and modernization of port and cargo facilities as well as recommendations for submarine detection.

In 1951 Zacharias served as associate director of Project Charles, which was established to make recommendations on the air defense of the North American continent. The project recommended tests of an experimental system of radars coupled through high-speed digital computers and the establishment of a new laboratory (Lincoln Laboratory) to continue research on this problem. Jerrold later directed a 1952 Lincoln Laboratory study of air defense that recommended establishment of the DEW (distant early warning) line, a chain of huge radars stretched across the Arctic and coupled to high-speed computers to detect approaching missiles. In 1955 Zacharias directed Project Lamp Light to study fleet and air defense.

Many years later, during the Vietnam War, Zacharias di rected one more Department of Defense study, this time on means for preventing the infiltration of men and materials across the demilitarized zone between North and South Vietnam. The study recommended an electronic barrier as an alternative to air bombardment.

EDUCATION REFORM

In 1955, at the age of fifty and after ten years of service, Zacharias retired as director of MIT's Laboratory for Nuclear Science, and a year later he terminated his atomic beam research to devote himself to his newest interest—education reform, especially in the sciences. He deeply believed that the United States would suffer in the future from the inadequacy of its science education and that "in order to save our democracy, we've got to educate the people who vote."

Although his initial plans emphasized the development of a series of educational physics films, he approached the problems of education in the same broad manner that had been so successful in his defense studies. With the support of the National Science Foundation, he assembled a brilliant array of individuals, including research physicists, teachers, scholars, writers, and motion picture directors, to form the Physical Sciences Study Committee (PSSC). The committee agreed that an entirely new approach to physics education was required, including new textbooks, laboratory kits to provide firsthand experience with simple apparatus, films, supplementary reading materials, and teacher training. The U.S. public reaction to the spectacular success of the Russian Sputnik in 1957 added urgency to the PSSC project. Talented new recruits joined the PSSC, which subsequently was well supported by the NSF and private organizations such as the Alfred P.Sloan Foundation.

Nearly sixty films were eventually produced by the PSSC, with scientists rather than actors and with emphasis on fundamental principles and on holding student interest. A textbook was written and widely adopted, along with teaching aids and new laboratory materials, including simple but informative experiments for schools with low budgets. Since good teachers are essential to the success of such a program, special summer institutes for teacher training were established.

The PSSC influence extended far beyond the teaching of high school physics in the United States. In universities, physics courses were improved to meet the changed knowledge and expectations of entering students with PSSC experience. The PSSC also served as a model for similar studies and curriculum changes in mathematics and other sciences. The PSSC was followed with great interest abroad, and its textbooks were translated into more than twelve languages.

Zacharias also became interested in elementary school science problems and established Educational Services, Inc. (ESI), under which a study was initiated to develop a science program for elementary schools. Eventually, this had a large impact on American education. His meetings and discussions about the problems of science education in Africa led to the African nations cooperating to establish a Science Education Program for Africa (SEPA).

Zacharias's educational interests were not limited to science. He also encourageed ESI to establish the influential Social Studies Curriculum Program and the Negro College Program.

A triple bypass coronary operation in 1977 only slowed Zacharias as he continued to push for education reform. At the same time, his deep concerns about the threat of nuclear war led him to coauthor a thoughtful essay titled *Common*

Sense and Nuclear Peace. He died from coronary artery disease on July 16, 1986, at the age of eighty-one.

HONORS AND AWARDS

Jerrold Zacharias received many honors and awards, including election to the National Academy of Sciences in 1957 and to the American Academy of Arts and Sciences. He was a fellow of the American Physical Society, American Association for the Advancement of Science, and the Institute of Electrical and Electronic Engineers. He was an institute professor at MIT and a member of the American Association of Physics Teachers (AAPT) and the President's Science Advisory Committee. The U.S. government awarded him the President's Certificate of Merit and the Department of Defense Certificate of Appreciation, its highest civilian honor. He received the AAPT's Oersted Medal for his contributions to teaching, the National Teachers Association Citation for Distinguished Service to Science Education, and the I.I.Rabi Award "for technical excellence and outstanding contributions in the fields relating to atomic and molecular frequency standards." Zacharias also received honorary degrees from Tufts University, Oklahoma City University, St. Lawrence University, Lincoln University, and Brandeis University.

SCIENCE, DEFENSE, AND EDUCATION

Science, national defense, and education were all greatly changed by Jerrold Zacharias. His work on the first molecular beam magnetic resonance experiments led the way for later magnetic resonance techniques, such as nuclear magnetic resonance (NMR) and magnetic resonance imaging (MRI). He and his associates measured numerous nuclear spins and magnetic moments and discovered the quadrupole moment of the deuteron, which implied the existence of a new kind of nuclear force. His development of the first commercial atomic cesium beam clock eventually led to the international adoption of the atomic cesium oscillations as the definition of the second.

Zacharias's wartime work on radar increased the reliability, versatility, and effectiveness of radar. The studies he initiated during the cold war contributed importantly to national defense.

The teaching of physics in many elementary and secondary schools was greatly changed by studies, such as PSSC, initiated by Zacharias and by new movies, classroom materials, and teacher training programs. The success of these programs in turn stimulated similar education reforms in other sciences and in other countries.

In his scientific research his development of atomic clocks, and his defense and educational projects, Jerrold Zacharias was not deterred by formidable obstacles. He attacked formidable problems with enthusiasm and determination. His positive and constructive attitude is well characterized by the remark of I.I.Rabi: "Dr. Zacharias is a man who, if he were to discuss the weather, would finish not by just talking about it, but he would be doing something about it."

NOTES

1. J.S.Goldstein. *A Different Sort of Time: The Life of Jerrold R. Zacharias.* Cambridge, Mass.: MIT Press, 1992. This excellent biography is the source of much of my information about Zacharias's full and varied life and provides a much fuller account than can be presented in a brief memoir.

2. N.F.Ramsey. Molecular beam resonance method with separated oscillatory fields. *Phys. Rev.* 78 (1950):695.

3. M.Kasevich, E.Riis, S.Chu, and R.S.DeVoe. RF spectroscopy in an atomic fountain. *Phys. Rev. Letters* (1989):612.

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