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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 67

Robert Kyle Burns

July 26, 1896-June 26, 1982

By James Murray

Robert Kyle Burns was born in Hillsboro, West Virginia, on July 26, 1896, and died in Bridgewater, Virginia, on June 26, 1982. His long and productive scientific career was devoted to understanding the processes of sexual differentiation in vertebrates. He pioneered the experimental manipulation of sex hormones in order to establish their roles in sex determination and differentiation.

Although both his parents were Virginians, Burns' boyhood was spent in West Virginia, where his father was a blacksmith and mechanic, charged with keeping the mill of a local lumber company in running order. In 1906 the elder Burns was killed in an accident with a horse and buggy, and the rest of the family moved back to Virginia. Burns completed his high school studies in the public schools of Culpepper, Virginia. As the family then found itself in straitened circumstances, there was some question whether the young Burns would be able to continue his education beyond high school. Fortunately, his uncle, who lived in Bridgewater, Virginia, persuaded his mother to move there so that Burns could attend Bridgewater College as a day student, thereby reducing the expense. Thus began an association with the college that was to last a lifetime.

Burns had not intended to become a scientist. In both high school and college he was fascinated by history, and by the time he was a senior at Bridgewater he had completed his major in that subject. But, as so often happens, a stimulating teacher was responsible for changing the direction of his life. He enrolled in a course in general biology taught by Dr. Frank J. Wright, and as a direct result he began to think of a career as a professional biologist. A strange link in this chain of circumstances is that Wright was not himself primarily a biologist. He was a geologist who happened to be filling a gap in the curriculum, there having been no biology taught at Bridgewater before then.

Bridgewater must have recognized Burns' developing talent for biology since he was kept on after graduation as an instructor in biology. The First World War interrupted his career with a year and a half of military service as a private in the U.S. Marine Corps, no doubt giving him time to consider his future plans. In the end the pull of biology was irresistible, and he entered graduate school at Yale University in 1920 with a laboratory assistantship, receiving his Ph.D. degree in 1924.

These were exciting times to be starting work in developmental biology. Not long before, F. R. Lillie had created a sensation by successfully explaining the occurrence of freemartins in cattle. A freemartin is a genetically female individual that has the external genitalia and mammary glands of a female. However, its internal organs are intersexual, and it is always sterile. The gonads are rudimentary but show some testis-like characters. The freemartin is invariably the twin of an animal of opposite sex, and the two share a network of the placental blood vessels. Deducing that a bloodborne factor from the male must be altering the development of the female, Lillie elaborated his theory that hormones are responsible for sexual differentiation among vertebrates.

Since at that time the hormone theory rested only on observation, the stage was set for testing it experimentally. Burns began trying to recreate the conditions that would produce the freemartin state. He chose to work with amphibians, more directly accessible to manipulation than mammals. By joining two embryos in parabiosis he was able to provide the first experimental evidence that hormones can alter the course of sexual development. Parabiotic pairs of embryos establish a common circulation before there is any sexual differentiation of the gonads, thereby exposing the gonads of pairs of opposite sex to a common hormonal environment. Although the earliest experiments gave variable results (1925), Burns demonstrated that it was possible to alter the female gonad in such pairs to an intersexual condition. Burns also pioneered the technique of transplanting gonads from younger larvae into older ones. In this case, if the younger gonad is of opposite sex to the host, it becomes intersexual (1928). A further set of experiments with salamanders of different species and sizes showed that parabiotic pairs tend to develop the sex of the larger species (1935).

During this period steady progress was being made in the isolation and characterization of the mammalian sex hormones, and Burns was not slow to apply to his work the results of these advances. He first used extracts of the mammalian hypophysis (1931,2; 1934) and then crystalline androgens and estrogens (1938,1; 1939,1) to test their effects on developing salamander larvae.

During this time Burns also turned his attention to the same kinds of questions in mammals, in which experiments designed by others to alter sexual differentiation had proved to be disappointing. Burns reasoned that the problem prob ably lay in the difficulty of intervening early enough in development. He therefore decided to work with the American opossum *(Didelphis marsupialis)* because of its very short gestation period (twelve and one-half to thirteen days) and the accessibility of the developing young in the maternal pouch.

Burns achieved immediate success in altering the development of the secondary sexual characters of both males and females. In 1939 he showed that the injection of female hormone into male pouch young caused them to retain the Müllerian ducts and to develop typical derivatives such as the oviduct, uterine tube, and vaginal canal. The prostate was suppressed and the genital tubercle reduced. On the other hand, treating female young with male hormone tended to inhibit development of the vaginal segment of the female reproductive tract and to cause the genital tubercle to develop into a penis (1939,1,3; 1942). These results were still disappointing, however, since they were not successful in stimulating the production of germ cells of the opposite sex in the developing gonads. It was therefore still possible to argue that the known hormones were not those responsible for normal development.

The key to the problem was discovered by Burns in a series of experiments in which males from one litter treated with estradiol in very low doses developed testes with a cortical zone similar to the cortex of developing ovaries. This litter appeared to have been born at stage 34 instead of the usual stage 35 and was therefore about twelve hours premature. Following up this result, Burns began to work with litters of the earliest possible stage and with very small doses of hormone. The results fully justified his hypothesis that the reason for the earlier failures was that treatments could not be begun early enough. He was able to produce genetic males in which the testes produced a persistent germinal epithelium, a well-developed cortex, primordial follicles, and growing oöcytes (1955,1). Thus he was able to establish the adequacy of the hormonal theory of sex determination as a general mechanism for vertebrates. For this work he was elected to the National Academy of Sciences in 1955.

In addition to being a gifted experimental scientist, Burns was a keen student of natural history. During his work on the effects of sex hormones in the opossum, he spent several spring seasons at the University of Florida's Conservation Reserve near Welaka, Florida. There he seized the opportunity to collect original data on food, movement, breeding season, gestation period, numbers of young born and reared, pouch life, and even the folklore of these animals (1956, 1; 1957).

Burns not only was able to present his experimental results in a clear and lucid series of papers, but he also had a gift for writing succinct reviews, placing his work in the larger context of the field. His final paper (1961), on the role of hormones in the determination of sex, represents the state of the art at the culmination of his career.

Burns began teaching with an instructorship in zoology at the University of Cincinnati in 1924 and was promoted to assistant professor a year later. In 1928 he moved to the University of Rochester, first as assistant professor of anatomy (1928-30) and then as associate professor (1930-40). Much of his active research career was spent at the Carnegie Institution of Washington's Department of Embryology in Baltimore (1940-62). He also held an honorary professorship of zoology at Johns Hopkins University (1945-62).

On his retirement from the Carnegie Institution, Burns returned to his alma mater, Bridgewater College. He began teaching there in 1962 and continued until his second retirement in 1968, except for a short stint in 1965 as visiting professor of biology at the University of California, Santa Barbara.

During much of his active career Burns spent his summers at the Mountain Lake Biological Station of the University of Virginia, where I first met him and learned to appreciate his quick mind and generous nature. He began his work there with research in the summer of 1940 and taught courses in experimental embryology for several summers.

Burns took an active interest in the development of the Mountain Lake Station. He built a cottage for use during his lifetime, with reversion to the station at his death. Around it he planted an extensive wildflower garden that served not only as an aesthetic attraction but also as a botanical garden for teaching plant taxonomy and ecology. In 1966 he financed the construction of a lake at the station. With characteristic modesty he refused to let us announce his gift. Each year he spent many hours developing, maintaining, and signposting the trail network surrounding the station, a contribution to the station community that has benefited generations of students and researchers.

When he finally became unable to move to the station in the summer, Burns lived quietly in Bridgewater until his death in 1982. In death as in life he preserved his modest bearing, directing that there should be no public notice of his death and no memorial service.

Burns was married to Emily Lucile Moore on June 21, 1924. There are three children from the marriage, Robert Kyle Burns, Jr., William Moore Burns, and John McLauren Burns.

In the preparation of this memoir I received generous help from John M. Burns, curator of lepidoptera at the National Museum of Natural History, and Harry G. M. Jopson, professor emeritus of

Bridgewater College. I also was able to use an incomplete autobiographical account of Burns's early life and the files of the Mountain Lake Biological Station.

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William B. Castle

October 21, 1897-August 9, 1990

By James H. Jandl

William Bosworth Castle died peacefully on August 9, 1990, at age ninety-two. Castle's eminence in medicine and physiology was secured early in his career by the celebrated discovery of gastric intrinsic factor, absence of which is the proximal cause of pernicious anemia. Within a very short period, he and his colleagues extracted the active hematopoietic principle of liver, characterized it as a B vitamin, later identified as vitamin B_{12} (cobalamin), and formulated an effective parenteral therapy for pernicious anemia. He then demonstrated that tropical sprue was caused by "intestinal impermeability" to this and other hematopoietic factors present in food and in related studies defined the stoichiometric need for iron in hemoglobin synthesis. This body of work transported hematology from a descriptive art to a dynamic interdisciplinary science. In ensuing years Castle and associates characterized the red cell defects responsible for paroxysmal nocturnal hemoglobinuria and hereditary spherocytosis and established the role of heightened hemoglobin viscosity in the pathogenesis of sickle cell anemia.

Castle once attributed his record of achievements to "being in the right place at the right time—especially with the right people." In addition, he had the right genes. His father, William Ernest Castle, was professor of zoology at Harvard and a pioneer in the study of mammalian genetics.[1] When W. B. Castle was elected to the National Academy of Sciences in 1939, the two Castles became the first father and son members in history. Examination of the abundant scientific and philosophical writings by and about William Ernest Castle provides uncanny prophecy of the forthcoming character of William Bosworth Castle. The unconventional views of the father, as recorded by L. C. Dunn,[1] resonate marvelously with those to emanate later from the son: "Orthodoxy or easy agreement with generally held notions seemed not to be a normal position for Castle . . ." "He had a natural aversion to complicated experiments and . . . regarded abstractions with suspicion . . ." "The memory of him which his former students probably treasure most is that of being treated . . . as intellectual equals."

CASTLE'S EARLY LIFE

William Bosworth Castle was born in Cambridge, Massachusetts, on October 21, 1897, a few months after his father assumed a position as instructor in zoology at Harvard. Insight into some of Castle's boyhood experiences is given in his informal account written in 1986 at the request of the National Academy of Sciences:

> My father now exhibited the knacks and interests of the farm boy that he had been by grafting fruit trees and cultivating a kitchen garden bordered by phlox and iris. . . . When in 1909 his animal work was transferred perforce to Harvard's Bussey Institute in another Boston suburb, Forest Hills, a tedious daily commuting trip became a necessity of his academic life. Salvation came at last with the purchase of a 1915 Model T Ford, though not without its perils, as he was a slow learner at the wheel. I was at once interested in the machine and its "planetary transmission," and being young required no more than a few hundred yards of open road to learn

> how to drive and shortly to make elementary repairs. A subsequently purchased Model A Ford of my own ran with few repairs from 1929 to 1953, the only factory-new car I ever owned.

Castle was enamored with the workings and visible logic of the Model T engine and became convinced of the superiority of physical evidence over abstraction. In later years his legendary dedication to the Model A Ford roadster and its rattling successors earned him a local reputation for eccentricity, but for him ownership of a vehicle that he himself could examine, diagnose, and repair was a matter of prudence, a measure of self-reliance. The overt purpose and simplicity of mechanics held a moral authority that conformed with puritanical convictions instilled in his childhood; these rejected the evils of adornment, fashion, and other forms of evasion or conceit. Even in his last years he was never entirely comfortable with the concealment of electronics. This elemental distrust of abstraction, reminiscent of his father's, was to shape his scientific attitudes throughout his career. He did not deny the usefulness of abstract concepts as staging devices but regarded them as the disposable means toward the discovery of facts through scientific investigation.

The world of art did not escape Castle's good-natured but strongly reasoned assessment of the part played by abstraction in viewing the realities of life. An amateur at watercolors who particularly admired Homer, Eakins, and J. S. Sargent, Castle expressed doubts while still a student about the mental status of postimpressionists. He was amused to learn that one producer of abstractions had sued an art museum in New Hampshire for hanging his painting upside down, commenting that no museum should accept paintings in which "up" could not be distinguished from "down."

Castle entered Harvard College in the month after the first firing of the "Guns of August" in 1914. There, as re called in his 1986 autobiographical sketch, Castle first heard of and saw the periodic table of the elements in Professor Kohler's course on inorganic chemistry:

> Far from its present completeness it gave exciting evidence of a fundamental order of the Universe. The experiments of Jacques Loeb with plants and animals seemed to obey the same laws of Nature.

The sense that human systems might be subject to laws comparable to those so gloriously defined in physics was inspiring; furthermore, it aroused the suspicion that in medicine—somewhat in contrast to surgery—one had a wide-open field for scientific exploration. Medicine had an added appeal in that it was separate and distinct from his father's field of animal genetics. On this precarious point Castle once explained to me:

> My disinterest in my father's work did not escape his attention, for when I departed with several college friends for a summertime tour of Europe, he provided me with a letter of introduction and greeting to the distinguished British geneticist, R. A. Fisher. Frankly, I wished to see London museums and the English countryside and cathedrals and was not really interested in visiting his eminent colleague. I returned with the letter undelivered. On opening it myself, I discovered that he had concluded the note by writing, "I think I should tell you that my son is innocent of any knowledge of genetics."

Castle approached postgraduate training warily, harboring doubts that he was bright enough to be a physicist or biologist. In a fuller vein he stated in a 1971 autobiographical note:

> Success in most professions leads to a desk job with domination of or by others. It seemed to me that this was not necessarily so for a career in medicine, where, for example, in contrast to law, Nature not man is the adversary. Indeed, I saw that if a doctor wished he could always remain, and usefully so, in direct contact with the object of his endeavors: the sick man, woman, or child.

Spurred by the apprehensions of wartime and government tracts urging that doctors were sorely needed for service in the army, Castle enrolled at Harvard Medical School at the end of his third year of college as a member of the class of 1921. His impression while a student of the state of medical science at the time was expressed in comments made to me in 1978:

> After reading the 1910 edition of Osler's textbook, I was impressed with how little could be done for the sick by doctors—other than surgeons, who had long had direct experimental experience. Most of the few specific medical remedies available at the time had been stumbled upon by primitives, witches, milkmaids, and sailors. I thought therefore that even I might have something to offer, either in research or surgery. As an initial step to either I sought a medical internship and was fortunate to be offered one at the Massachusetts General Hospital.

As a twenty-one-month house pupil at the "Old Massachusetts" from 1921 to 1923, Castle had his first direct exposure to some of the great clinicians of the time, including Chester M. Jones, with whom he collaborated in his first medical publication, and the meticulously observant George R. Minot,[2] who later became Castle's mentor and unflagging supporter. Having made a favorable impression as a clinician, Castle was invited to continue on at the MGH, but his consuming interest remained in "finding out how things work," a passion fired by the great discovery of insulin by Banting and Best, announced in 1922 and applied in the nick of time at the MGH to save Dr. Minot from dying in diabetic coma. With this dramatic example of the raw potential in medical research, Castle could not envision a life of rushing up brownstone stairs to attend to indisposed Boston merchants. Instead, in 1923 he accepted a position in Cecil Drinker's laboratory at the Harvard School of Public Health. Reminiscing on this, he said:

> I acquired a respect for accurate experimental protocol and for the private feelings of living tissue. . . . It was there too that I learned from Harold Himwich . . . the meaning of hard work while we investigated the a-v differences in oxygen and carbon dioxide of blood supplying an isolated dog gastrocnemius muscle while at rest. Its R.Q. was found to be 0.71, rather than unity as was then expected.[3]

These observations anticipated by over three decades general recognition that the primary source of energy for muscle was fatty acids, rather than glucose.

SCIENTIFIC ACHIEVEMENTS

In 1925 Castle was enticed back into a clinical setting at the Thorndike Memorial Laboratory, Boston City Hospital, by the luminous Francis W. Peabody. A profoundly admired and gifted man and physician, Peabody had within several years attracted to the Thorndike a remarkable nucleus of talented young men destined soon to become leaders in academic medicine; these included Joseph Wearn, Perrin H. Long, Henry A. Jackson, Jr., Alberto A. Hurtado, Herrman Blumgart, Charles Doan, Chester Keefer, Soma Weiss, and the young Castle. Peabody retained a favorable impression of Castle gained several years earlier during the latter's third-year clinical clerkship. This came about, interestingly enough, because Castle had flunked the previous year's examination in hematology owing to an inability to count red cells. Peabody granted the flunkee an opportunity to redeem himself via an interview, and Castle's performance was sufficiently memorable to gain him an historic appointment. At this time Peabody was deeply interested in the cancer-like marrow morphology of pernicious anemia, and Castle assisted him in the collection of tibial marrow biopsy specimens obtained by surgeons employing mastoid trephines and cuvettes. Peabody took the slides to Northeast Harbor, Maine, during his escapes from administrative duties and described the specimens presciently as showing "inefficient erythropoiesis." Suddenly, the following year pernicious anemia became a focus of international attention, with Minot's stirring discovery that this dread, uniformly fatal ailment could be cured by feeding patients odious quantities (300 grams daily) of raw or lightly cooked liver. With Peabody's untimely death in 1927 and public awareness of Minot's pending arrival as his successor as director of the Thorndike, patients flocked to the City Hospital. Minot's grand achievement, which later earned him the Nobel Prize (shared with Whipple and Murphy), was an empirical consequence of his fastidious attention to strict dietary regimens and meticulous reliance on reticulocyte levels as a quotidian measure of therapeutic response. But Minot, a nineteenth-century prototype, was scientifically unprepared to pursue his great discovery. Castle's mechanistic mentality was perfect for grappling with this lingering riddle: If normal people do not have to eat any liver to maintain normal blood counts, why should ingestion of liver in large quantity cure pernicious anemia?

INTRINSIC FACTOR, THE SPUR TO FAME

In the fall of 1927 Minot was scheduled to give an address before the American Academy of Arts and Sciences in Boston to present the latest bulletins on the new therapy of pernicious anemia. Apprehensive as to how Minot would look upon his uneventful first two years at the Thorndike, Castle attended the talk. While descending to the ground floor of the Thorndike via the back elevator en route to Minot's lecture, Castle experienced a flash of insight. If it was true, as had been recorded most convincingly by S. A. Levine and W. S. Ladd, that all pernicious anemia patients lack gastric juice, the trouble must be that—deprived of effective digestion in their stomachs—they were unable to "make" their own liver extract. The essentials of Castle's experiments to explore this fanciful thought were deceptively simple. Castle, who slept in a room next door to the Thorndike ward kitchen, resolutely fed himself 300 grams of raw beef patties every morning while in fasting state and after one hour induced regurgitation by pharyngeal stimulation. The captured semiliquid contents were adjusted to pH 2.5 to 3.5 with HCI and incubated for six hours at body temperature, after which the liquefied material was passed through a fine sieve and neutralized before introduction via Rehfuss tube into the stomach of the unwitting patient. Patients who were unresponsive to beef muscle alone (or to gastric juice alone) showed brisk reticulocyte responses when this sour admixture of predigested beef muscle was administered. These indelicate but "classic" studies were amplified and confirmed in every conceivable way in a series of fifteen papers bearing the heraldic banner, "Observations on the Etiologic Relationship of Achylia Gastrica to Pernicious Anemia." In the fifth paper (1936) of this memorable series, Castle and T. Hale Ham summed up experiments performed in sixty-one consecutive cases, and silenced critics of the Castle hypothesis, in a succinct synopsis of their long labors:

> If beef muscle and gastric juice are administered without opportunity for contact, they are not effective. It is obvious, therefore, that the activity of mixtures of beef muscle and gastric juice cannot be due to the simple addition of two subthreshold substances but requires an interaction between them.

Castle called the essential gastric secretion "intrinsic factor" because it was formed in the body and could not at the time be chemically identified. Indeed, it was not until the 1970s that the primary structure of this essential glycoprotein secreted by gastric parietal cells was elucidated. In the tenth paper of the "Observations" series, published in 1948, Castle and associates showed by therapeutic trial (in a study confirmed independently by Randolph West) that "extrinsic factor" was identical to the B vitamin now known as cobalamin, which had been purified earlier that year by Karl Folkers's group and by E. L. Smith and L. F. J. Parker on the cis and trans sides of the Atlantic. This was capped off by Dorothy Hodgkin's three-dimensional crystallographic analysis of cobalamin structure in 1956, for which she received the Nobel Prize in chemistry in 1964.

It became clear in retrospect that Castle's seminal experiments had been correct in targeting an interplay between intrinsic and extrinsic factors and in identifying extrinsic factor as a B vitamin, but he had not pinpointed the role of intrinsic factor as an intestinal transport vehicle for this essential nutrient. Nonetheless, Castle's series of papers established for the first time that nutritional deficiencies can result not only from defective diets but also from faulty absorption or metabolism of nutrients. The latter mechanisms Castle grouped together as "conditioned deficiencies." Pernicious anemia, in his words, was a form of "starvation in the midst of plenty."

In the present time, when clinical observation is tightly regulated by public agencies and strict human studies' protocols, it is unlikely that Castle's benign but covert experiments would pass muster. It would, indeed, be an interesting fantasy to listen in as Castle, with his flare for euphemism, explained his regurgitation studies to round-eyed patients gripping consent forms. When he submitted his initial studies of intrinsic factor to the Warren Triennial Prize Committee at the MGH in 1928, Castle was bound by rules requiring anonymity. He identified his winning entry with the regal disclaimer, "Honi soit qui mal y pense." In a larger view, taken by Yale University when in 1933 it conferred on

Castle his first honorary degree, Professor Phelps commented on both the brilliance and heroism of the awardee, adding that "Prometheus showed no greater self-command."[4]

After affirming and extending his studies of intrinsic factor, Castle wasted no time in attempting to prepare an injectable form of liver extract containing the active ingredient. Minot and the physical chemist Edward J. Cohn were engaged in systematically fractionating liver using heat, alcohol, and acidification and came up with an effective oral preparation that reduced the daily requirement from 300 grams of liver to about 12.5 grams of yellow powder ("fraction G"). Knowing from Cohn's work that the active factor was not a protein, Castle minced up liver in capacious crucibles, added water, boiled his brew, stirred it with paddles, and strained off the strange soup through cheesecloth. From this crude beginning, with simple modifications, Castle and F. H. Laskey Taylor produced a "domestic liver extract" that could be injected safely either intramuscularly or (given slowly) intravenously. This discovery was not only of great practical merit but also led to the provocative finding that the protein-free, water-soluble principle was far more potent, unit for unit, when given parenterally than when taken orally.

LESSONS FROM THE TROPICS: SPRUE, IRON, AND SHOES

Stimulated by George C. Shattuck and aided by his influence in the field of tropical diseases, Castle in 1931 enlisted the interest of the Rockefeller Foundation in underwriting an expedition to Puerto Rico to assess the efficacy of his liver extract in patients with tropical sprue. Under Rockefeller auspices, an entourage consisting of Castle, Cornelius P. (Dusty) Rhoads, and a group of assistants set sail for this subtropical island, which at that time was no paradise, for a majority of its citizens were laid low by the coendemic rav ages of tropical sprue and hookworm-induced anemia. At the Presbyterian Hospital in San Juan, in which their laboratories were located, Castle and Rhoads were at first fascinated by the rich aromas borne by the breeze, "a nostalgic mixture once you have breathed it—compounded of the smell of moist humus and humanity, burning coconut husks, and a trace of ginger. . . . On a nearby beach . . . deep blue shoaled to bright green towards the yellow sand gently fingered by the moving shadows of palm fronds. . . ."[5]

But there were also darker shadows and the hovering stench of sickness caused by tropical sprue, a dispiriting disease endemic to this island. Tropical sprue patients had megaloblastic anemia and severe glossitis similar to that of untreated pernicious anemia, but these patients failed to respond to oral liver extract or to combinations of beef muscle and normal gastric juice. Furthermore, many of them had ample amounts of acid gastric juice, which contained normal concentrations of intrinsic factor, as determined with the "human reticulocyte assay" performed on frozen specimens mailed to the expedition's anchor man in Boston, Maurice B. Strauss. In the treatment of sprue, Castle's homebrewed parenteral liver extract paid off wonderfully, patients responded promptly, and the debilitating anemia of sprue was established as resulting from "intestinal impermeability," later referred to as malabsorption syndrome. The pathogenetic mechanisms responsible for initiating tropical sprue, and its curiously spotty distribution, still have no tidy explanation, but since the Castle-Rhoads expedition empirical prophylaxes with folate and cobalamin have eradicated this melancholy menace in Puerto Rico.

While contending with tropical sprue, Castle, Rhoads, and company confirmed B. K. Ashford's observation that 90 percent of the island population was infected with hookworm. Vermifuges expelled the worms and stopped intestinal bleed ing, but hematologic responses were uneven and subject to relapses attributed to obscure wormborne toxicities. Blood morphology was identical to that of the iron deficiency characteristic of bleeding Bostonians. Both in the hospital and in the small cooperative town of Cidra, Castle's group showed that removing hookworms from anemic patients did nothing to improve hemoglobin levels, but that iron induced a rapid rise whether or not hookworms were removed. With a characteristic canny blend of logic and social pragmatism, Castle convinced public health authorities that their emphasis on priority for purging parasites was not as sensible as a direct attack on the anemia. Hematologic remissions he recounted ". . . would quickly restore subjective health and return the patient to work. He could then learn the value of a better diet and could build a latrine in the backyard, buy shoes, and have his worms eliminated without the likelihood of recurrence." For his contributions to the wellbeing of Puerto Ricans, Castle was awarded the key to the city of San Juan.

Castle's interest in pernicious anemia and other megaloblastic anemias persisted throughout his lifetime, engendering scores of papers dealing with assays of liver extracts; identification of cobalamin (vitamin B_{12}) with "extrinsic factor"; recognition that pernicious anemia of pregnancy responded to a different vitamin, later identified as folate; interactions between folate and ascorbate; and the mechanisms of absorption and metabolism of pteroylpolyglutamates. When receiving the Kober Medal, Castle reminisced that he had been privileged to experience several great moments of insight and promise for the future. Foremost among these were the appearance of the first reticulocyte response in pernicious anemia to autodigested beef muscle, the dramatic improvement in tropical sprue induced by parenteral liver extract, and the startling increase in viscosity of deoxy genated sickle cell anemia blood that he and T. Hale Ham first witnessed.

HEMOLYTIC ANEMIAS

One afternoon in the late 1930s Castle and Ham were methodically observing the effects on red cell volume of changes in O_2 and CO_2 tensions. Among the several varieties of hemolytic anemia blood so studied was that from a patient with sickle cell anemia. As he recalled it later, "We discovered that when the pCO_2 was held constant, and the pO_2 was lowered in one sealed tube and not the other, deoxygenated sickle blood failed to sediment in the centrifuge. It immediately occurred to us that this was because the elongated, sickled red cells had become tangled up 'like haywire.'"

From this Ham and Castle deduced a general theme that stagnation ("erythrostasis") and packing ("erythroconcentration") of red cells were primary mechanisms in many kinds of hemolytic anemias, including hereditary spherocytosis (HS) and possibly immunohemolytic anemias. The genetically defective cells of HS were shown by Castle, Ham, Charles P. Emerson, Jr., and their associates to lose control of volume, shape, and surface area when subjected to erythrostasis for long periods in vitro, unless the cells were repeatedly reinvigorated by additions of glucose or fresh serum. The vulnerability of these cells to packing and substrate depletion led to origination of the "incubation fragility test." More importantly, spherocytosis and cell membrane loss observed in vitro during erythrostasis and erythroconcentration were shown to correspond with changes in red cells recovered from blood and spleens of patients with HS; this both defined the essential role of splenic vasculature in expressing the latent cell defect and explained the efficacy of splenectomy in curing the anemia despite persistence of the membrane abnormality. These observations, first recorded in general and in abstract form by Ham and Castle in 1940, had a profound influence on the direction of research on nonimmune mechanisms of hemolytic anemias in the late 1940s, although publication of the full body of experimental evidence and clinical specifics (gathered mainly by C. P. Emerson) was delayed by a Darwinian sixteen years.

THE VISCOUS CYCLE

The potential for hematologic havoc through erythrostasis was most grippingly evident in sickle cell disease. Using the Ostwald viscosimeter and sealed cylindrical flasks containing blood equilibrated with gas mixtures, Ham and Castle observed in 1940 that reduction of PO_2 to levels approximating that of mixed venous blood dramatically increased the viscosity of sickle cell (but not normal) blood. This led to Castle's famed postulation that the painful crisis, organ infarcts, and hemolytic anemia of sickle cell disease were the consequence of a vicious cycle initiated by increased viscosity, the endpoint being vascular occlusion by ensnarled rigidified red cells. This 1940 interpretation of events at the cellular level took wing in 1945 during a legendary conversation between Castle and Linus Pauling, as they traveled together by overnight train from Denver to Chicago. As recorded by Castle's former colleague in megaloblastic matters, M. B. Strauss,[6] Castle sketched out to Pauling some of the work he and Ham had been doing since 1940 on sickle cell disease, adding that ". . . as stated by Dr. I. J. Sherman in 1940, when the cells were deoxygenated and sickled they showed birefringence in polarized light. This, I stated, meant to me some type of molecular alignment or orientation, and ventured to suggest that this might be 'the kind of thing in which he would be interested.'"

It was indeed the kind of thing, for in the following year, Pauling, Harvey A. Itano, and others began studies using moving boundary electrophoresis that showed sickle cell hemoglobin to be intrinsically abnormal in its electrophoretic migration. The report[7] of this finding ushered the age of molecular biology into clinical medicine. Knowledge of the discovery by Pauling's group inspired John W. Harris, then a fellow of Castle, to investigate the physical properties of sickle cell hemoglobin solutions divested of cell membranes. When sufficiently concentrated, deoxyhemoglobin S was shown by Harris using phase optics to form long, watery crystals or tactoids;[8] these rigid and birefringent bodies, created by cable-like growth of filaments of polymerized hemoglobin S, account for the vascular impactions by sickled cells. Our elaborate knowledge today of the molecular biology and pathogenesis of sickle cell anemia stems directly from Castle's bold inference based on viscosity plus birefringence.

CASTLE'S INFLUENCE AS A TEACHER: THREE GENERATIONS OF TRAINEES

The Minot-Castle era of hematology research began before hematology had become a business. It started with W. C. Townsend, M. B. Strauss, C. W. Heath, J. H. Burchenal, R. W. Heinle, and F. H. Laskey Taylor, a gifted group of investigators who overlapped with T. Hale Ham, followed by P. F. Wagley, J. Watson, F. H. Gardner, and C. P. Emerson, Jr. Castle's first generation of trainees, which included several of these, ended with the departure in the early 1950s of Ham, J. W. Harris, Robert F. Schilling (author of the Schilling test), S. C. Shen, and others schooled by Castle in learning from the "mistakes of nature." Clearly, Thorndike hematology under Castle was immensely productive but physically restricted, forcing newly spawned investigators to swim off to less crowded waters, where their activities could be amplified. Consequently, a brief lull in scientific productivity set in, occasioned in part by Castle's increasing administrative responsibilities. Progressively hampered by complications of diabetes, Minot had gradually transferred the reins of office to Castle during the 1940s, a process made official by Castle's appointment as director in 1948.

Thorndike tradition had strongly nurtured a do-it-yourself philosophy, often placing outrageous demands on the investigator, and Castle and his chancellor of the exchequer, Maxwell Finland, kept a keen watch on the number and quality of incoming fellows and the resources grudgingly allotted them. In 1952 hematology was represented by only three fledgling fellows (M. S. Greenberg, J. H. Jandl, and A. A. Lear), and Castle was seldom at liberty to roam the laboratories or fuss with the various gadgets, such as the mechanical fragility machine, which he had invented, assembled, and hitherto kept in good repair. Nevertheless, those of us starting then were peculiarly fortunate, for while we spent most of our time learning the techniques of science the hard way, we were repaid for our labors by access to frequent, informal conferences with this legendary man, who treated us all as his intellectual equals.

Associating with Castle, exchanging ideas, debating data, absorbing tactful corrections, and learning to search the entire natural universe if necessary for clues to disease processes challenged every neuron in our bodies. Castle's curiosity was insatiable, his mind was never idle, and it seemed almost impossible to get enough of him. He conveyed the power to think more clearly, observe more sharply, and function more effectively than we ever had on our own. I think this was because he could see in us potentials we did not know we had. Certainly his incorruptible example served as our gold meter bar, by which we could measure ourselves and direct our endeavors. If Castle had approved our ex periments and interpretations, we felt no nervousness whatever on reporting them before national societies. Soon to share in this heady atmosphere was Allan J. Erslev and, on an informal basis, Rudi Schmid. Castle's preoccupation with the mechanisms of anemias (rather than further development of their remedies) led him to sustain his lifelong interest in pernicious anemia and allied processes. Under his benign direction, experimental observations were resumed through the efforts of a succession of colleagues, including Bernard A. Cooper, Hendrick O. Nieweg, and later Victor Herbert; Herbert in turn shared the responsibilities of training and collaborating with Louis W. Sullivan, Ralph Zalusky, and Richard R. Streiff. Jandl and co-workers already had launched a broad attack on the mechanisms of hemolytic anemias, bearing out many of the predictions made earlier by Ham and Castle, and then conducted increasingly independent studies that among other things defined the role of Fc receptors in immune hemolysis and demonstrated the existence of transferrin receptors. With characteristic magnanimity, Castle transferred responsibility for the hematology division to Jandl in 1962, and the following year stepped aside as Minot Professor and director of the Thorndike enabling Finland to take on these titles and duties in recognition of his extraordinary contributions in funding and remodeling the aging facility. These maneuvers, involving Castle's appointment as Francis Weld Peabody Faculty Professor of Medicine, led to a second generation of Castle fellows, his scientific grandchildren. Jandl ran the division under Castle's sparkling firmament. From this lively and collegial arrangement spilled forth a remarkable new crop of talented investigators, all of whom became doyens in hematology or deans of medicine: David W. Allen, Harry S. Jacob, Manuel E. Kaplan, Richard H. Aster, H. Franklin Bunn, Christian Gulbrandsen, Richard

A. Cooper, Herman A. Godwin, Albert F. LoBuglio, Neil Abramson, and Sanford J. Shattil.

In 1968 the hard rules of retirement then in force had already been stretched by five years in Castle's case through his appointment as faculty professor. Castle's own mental faculties at this time had remained so much livelier than those of most younger colleagues that it was unthinkable to let him go. Through the collusion of benevolent admirers Castle was appointed distinguished physician by the Veterans Administration and assigned to the West Roxbury V.A. Hospital. Here he was welcomed by old friends (T. A. Warthin and R. P. Stetson) and stirred excitement and strong loyalties among the young staff and residents of that hospital and of members of the Brigham and Women's Hospital staff who rotated through this V.A. affiliate. As he had done at the Boston City Hospital, Castle soon imbued the staff of the West Roxbury V.A. Hospital with his view that in both patient care and research the patient is the essential analyzer and the clinical investigator should never abandon the unique, often heuristic, experiences arising during direct patient care. Castle resigned as distinguished professor to become senior physician at West Roxbury V.A. Hospital in 1972, and for several years thereafter he conducted regular tutorial sessions, clinic rounds, and conferences in hematology. After Harvard's withdrawal from Boston City Hospital in 1974, the epicenter of the Castle mystique shifted back to Harvard Medical School, where one of the four educational pathways (programs) was designated the William B. Castle Society.

HONORS

Honors came to Castle early on as well as late. He received nine honorary degrees, fifteen honorary memberships, and two dozen special awards, including the Walter

Reed, Leonard Wood, and George Kober medals. Conferrers of awards were usually rewarded in return by the felicitous phrasing characteristic of Castle's speech. In addressing a prestigious gathering of gastroenterologists in 1959, he explained that, "The human stomach is a convenient receptacle for food in both savage and civilized man who, for quite different reasons, must often eat and run."

PERSONAL PROPERTIES

Physically, Castle was tall and angular, moved about restlessly, and gestured with long sinewy arms and spidery but powerful hands. His deep, penetrating voice imparted authority, and his leveled eyes held their focus attentively on the subject or person at hand. However impatient with interruptions by telephones and by the impositions of administrative persons, whom he classified collectively as "chronophages," Castle was always generous with his time in discussing scientific matters with colleagues. This was for him a pleasure, not a duty—or perhaps deep in his good-natured and conscientious soul, helping out on research problems, like lending a hand, was a pleasurable duty. Long after official retirement, while at home or when attending rounds, seminars, or scientific programs (many of which were held in his honor by various generations of the "Castle family"), his astute comments and biological or philosophical excursions invariably added sparkle as well as insight.

HIS LANGUAGE AND PHILOSOPHY

Castle's facility with words added immeasurably to the impact of his written and spoken thoughts, which nearly always contained one or more graceful phrases lastingly treasured in the memories of rapt listeners. This gift not only enabled him to link human values to dry facts but served to deflect and neutralize those awkward moments or phrases incidental to human intercourse. When a visiting dignitary holding forth on grand rounds once expressed patronizing amazement that "such splendid science was being conducted here—at the Boston City Hospital," Castle uncoiled slowly to his feet during the dead silence to remark in his deep rumbling tones, "at last Mohammed has come to the mountain." This gift for recasting adversity was his first weapon of defense when expenditures loomed, for the generosity of the great man was equaled only by his parsimony. For him the most pleasurable of all situations occurred when resourcefulness could be substituted for expense. When in the 1950s impecunity forced me to make periodic demands for incremental levels of support, my requests were received with visible agitation, followed by a well-paced stream of anguished propaganda that issued forth for weeks. After dropping handbills in my letterbox advertising $7 pants and $3 shirts, and then extolling the thrift of powdered milk as a solution available to the impecunious, Castle's final stratagem on one occasion was to send me a copy of a communication from Francis Peabody written in 1925. In this letter Castle was offered a stipend of $1,000 per year; on the margin he penned the words, "As are you, Jim, so once was I." Frugality and abhorrence of waste were ironed into his soul, but with the resources possessed by him—his scientific instincts, his language skills, use of his aged cars, the summer house he and Louise made available to friends and fellows—with these he was always generous. For years the Castles offered to our enlarging family vacation use of their spacious home on the Cape. An annual preliminary ceremony was the vernal installation of their dock, a process Castle preferred to perform personally. Among the most unnerving experiences of memory was that of standing on the cross-member of a sawhorse, wielding a 16-pound maul to drive in the vertical posts grasped beneath me by Castle, neck-deep in river water. On one occasion I hesitated while apprehensively studying the famous but fragile cranium below, spaced inches from the post. "Anything wrong?" he asked. "No, just shifting my position." His smiling response left me dumbfounded. "I thought perhaps you were pondering the future directorship of the Thorndike!" After sweaty tasks of this kind, we generally cooled off in the river and then resumed discussions of how skunk cabbages melt their way through hard ice or other mysteries of nature that invited exploration.

For anyone with broad interests in the external world (Castle had no use for introspection and almost none for psychiatry), his panoramic appraisals of "all the uses of this world" were astute and fearless. As recorded by his son, William Rogers Castle,[9] he wanted to know, after reading a few pages of *Portrait of the Artist as a Young Man,* whether Joyce suffered from eye disease. "I thought so," he said; "there was all that emphasis on losing his glasses." A naturalist by habit, he was irked by Thoreau for "misidentifying a few birds in his nature writing." Castle loved steam engines both for their working parts and for their efficiency in transporting people in a state of productive privacy and was outraged at the dismemberment of railroads by financiers. He once burst into the lab exclaiming that it no longer was possible to go by train from Boston to Cincinnati. He feared and disapproved of airplanes, especially the supersonic Concorde, and brooded over the inequity of a privileged few gaining dubious minutes while we earthlings lost our hearing.

A GIFT FOR GIVING

Castle's sharp eye for particulars and gift of insight remained with him until his final days. Thanks to the indispensable and inspiring efforts of his remarkable wife, Louise

Muller Castle, to whom he had been married since 1933, and with encouragement from their daughter, Ann Castle Morris, and son, Castle was able to receive a procession of friends and colleagues until the week of his death. Visitors made pilgrimages to the venerable Victorian edifice at 22 Irving Street more to receive than to give. Time spent with Castle, even as congestive heart failure shortened his sentences (but not his wits or comprehension), continued to be an adventure. As recently as 1988 he journeyed to the medical school to participate with characteristic zest and nimble humor in a special hematology research session convened by H. F. Bunn. Then, as throughout his long life, William Castle was stimulating, original, inquisitive, tactful, humorous, courtly, considerate, and constantly interesting. One could never get enough of his company. That company we shall forever miss.

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Preston Cloud

Preston Cloud

September 26, 1912-January 16, 1991

By John C. Crowell

Preston Ercelle Cloud, Jr., eminent biogeologist, paleontologist, and humanist, has left a significant and diverse legacy that cuts across scientific and humanitarian disciplines. As an historical geologist he contributed more than any other single scientist to understanding the evolution of the atmosphere, oceans, and crust of the earth and to understanding the concurrent evolution of life. His work and vision emphasized complex interrelationships through the whole 4.5 billion years of earth history involving the interplay of biological, chemical, and physical processes. His deep delving through these researches led him to a special appreciation of the place of humankind within this evolving environment. He worked diligently to focus attention on the restricted carrying capacity of our planet and for human intelligence to recognize that population increases, limited material and energy resources, and the intergrown complexities of the life-web demand appreciation and action now before the activities of humans lead the planet into calamity. He was a member of the Academy for thirty years and served on its Council and Executive Committee and as chairman of the Geology Section. In many ways he contributed both to the Academy's welfare and to its ser vice to the nation and the world through wise and informed leadership on National Research Council projects.

In October 1989 I asked Cloud at a relaxed lunch what he considered his most important work. He replied that his forte had been in seeing the larger connections between processes to events, particularly as they affected early biospheric evolution. He felt that his model of the primitive earth (from 1968 on), connecting biospheric, atmospheric, hydrospheric, and lithospheric evolution, had been challenged, tested, and validated in all significant elements and that it is now widely accepted as the best-available approximation. He recognized in 1968 that free oxygen first began a significant atmospheric accumulation about 2 billion years before present, setting the stage for eucaryotes, and first rose to levels supportive of metazoan evolution about 700 million years before present. He perceived (as early as 1948) that the Metazoa first evolved and rapidly diverged into phylum-level categories during the first 200 million years or less of Phanerozoic time. He was also an early and continuing contributor to discussion and legislation concerning preservation of the human habitat and the converging problems of population growth, management of natural resources, and deterioration of the environment.

Pres Cloud, the name his friends used, was born in West Upton, Massachusetts, on September 26, 1912. He was the third of seven children of Preston Ercelle and Pauline L. (Wiedemann) Cloud; his father was an engineer-draftsman. His wife, a genealogist, traced his ancestry to William Cloud, who was given land by colonist William Penn in 1683. During high school, from which Cloud graduated in 1929, his family lived in Waynesboro, Pennsylvania. Pres was especially attracted to the outdoors and followed Boy Scouting right through to the Eagle rank. After high school he enlisted as a seaman in the U.S. Navy for three years and was bantam-weight boxing champion of the Pacific Scouting Force. Following discharge in California in 1933, he spent several months hiking and working his way back to his home in the East. The Great Depression was in full swing, so Cloud was unable to enter a university as a daytime student. Instead, he went to Washington, D.C., where he attended night school at George Washington University, taking a full load and supporting himself by odd jobs during the day. One of his professors of geology, Ray S. Bassler, was also curator of geology at the U.S. National Museum. Bassler arranged employment for Cloud at the museum, first as a man-of-all-work. Soon Cloud showed scientific interest and skills, so he became a preparator in the paleontology laboratory of the museum, working for G. Arthur Cooper, an outstanding expert on Paleozoic fossil brachiopods. Cloud's interest in research paleontology was launched, and Cooper invited Cloud to join him as junior author on a paper dealing with Devonian brachiopods from Illinois, published in 1938. Although Cloud worked full-time at the museum during the day, he completed work for a bachelor of science degree in geology in four years and was elected to Phi Beta Kappa. (In 1990 George Washington University invited him to present the 1991 Distinguished Alumnus Address, but, sadly, his death intervened.)

Cloud entered Yale University graduate school in geology in 1937 and supported himself largely as a fossil preparator. For his doctoral dissertation he completed in three years a major systematic monograph on a group of Paleozoic brachiopods, under the direction of C. O. Dunbar, and was awarded the Ph.D. degree in 1940. The work was awarded the A. Cressey Morrison Prize in Natural History by the New York Academy of Sciences and was published by the Geological Society of America as Special Paper 38 in 1942. Cloud enjoyed the summer of 1939 as field assistant to A.

Lincoln Washburn on Victoria Island in western Arctic Canada. Washburn's work, also a dissertation undertaking, when published, contained a contribution on the stratigraphy and paleontology by Cloud. After receiving his Ph.D. Cloud taught for a year at the Missouri School of Mines in Rolla but then returned to Yale University to continue work on brachiopod evolution as a Sterling Research Fellow (194142).

World War II was under way, and Cloud was soon called to the U.S. Geological Survey for work within the wartime Strategic Minerals Program. He joined a field party studying manganese deposits in Maine during the summer of 1941 and then became chief of party for bauxite investigations in Alabama. D. L. Peck, director of the U.S. Geological Survey, wrote in a letter dated January 29, 1991, that while Cloud was examining clay pits alone, "his method was to tie a rope to a tree and lower himself into the pit, leaving at day's end by walking up the wall and taking his rope with him. This created quite a stir among the locals who suspected that this person of slight stature, emerging from holes in the ground, must certainly be a Japanese spy in their midst." He was then assigned to work with V. E. Barnes of the Texas Bureau of Economic Geology in a study of the Ellenburger Limestone of the Lower Paleozoic sequence in central Texas. This stratigraphic unit, an important subsurface reservoir for oil in nearby regions, required precise mapping and investigation of the stratigraphy and paleontology. Barnes writes (June 19, 1991) of these times when they worked together: "Pres was persistent and dedicated to accuracy in all that he did. For example, in establishing measurable stratigraphic sections, frequent offsets along the bedding were needed to reach better exposed strata. On one of these offsets, Pres was crawling along a bed through a dense juniper thicket, and came face to face with a huge rattlesnake. Pres was not easily bluffed: after a few minutes of staring at each other, the rattlesnake crawled away." The experiences made Cloud a first-class field stratigrapher and thoughtful student of carbonate rocks and their depositional environment. The Ellenburger studies led to several significant publications and convinced Cloud that ancient carbonate rocks could only be understood through investigation of similar deposits forming today.

In 1946 Cloud accepted a position as assistant professor of paleontology and curator of invertebrate paleontology at Harvard University, but he resigned in 1948 to return to the U.S. Geological Survey as chief of party to map and investigate the geology of Saipan in the Mariana Islands in the western Pacific. Several important publications resulted, dealing with coral reefs and the geology and ecology of this modern carbonate environment, including early comments concerning the geochemical processes involved. During this time he published the then-controversial theory that multi-celled and complex organisms evolved rapidly from many different ancestors since early Phanerozoic time, about 700 million years ago. He produced evidence to show that when the oxygen level climbed over the next 80 million to 100 million years these early organisms expanded into a host of vacant ecological niches. They demonstrated evolutionary opportunism.

For ten years (1949-59) Cloud was chief paleontologist at the U.S. Geological Survey in Washington, D.C. During this interval he guided the growth of the Paleontology and Stratigraphy Section from twelve professional scientists, two clerks, and two technicians to a group of forty-five professionals and a total staff of about 120. His leadership reflected his conviction that paleontology had a dual function: to provide a basis for the essential chronology of geological strata and to document the evolution of life on earth. I. G. Sohn wrote (in a letter dated January 29, 1991) of these times: "Pres recruited young paleontologists and built what we considered to be the largest unit of specialists in paleontology in the world under one roof. He read every manuscript submitted for publication, and made valuable constructive suggestions. He always complimented good work, and never criticized any of us in public, although he could be brutally frank in private. He made it unequivocally clear to each of us that we had to complete our assigned task in publishable form." Cloud instigated weekly "brown-bag" lunches for his colleagues and brought in outside geologists to join in informal and stimulating discussions and usually on an announced topic. All paleontologists were expected to attend. Moreover, able young scientists, newly arrived, learned much about the service and functioning of the museum when they were assigned to serve a half-year as "assistant to the chief" to keep track of the flow of fossils into the museum and reports and publications flowing out. These practices greatly improved the esprit of the group and its visibility and service to the scientific and general community.

During this decade of administration Cloud continued research and publication of results of new and previous studies. He organized his time very efficiently and maintained a closed-door policy for much of the day and then opened it widely at other announced times so he would be available to his colleagues. Many evenings he worked until midnight and frequently on weekends. According to legend, he used slip-on shoes so that he would not waste time in lacing! He also organized occasional field trips to nearby areas so that his colleagues could enjoy the social fun of field excursions and a bit of science along the way. In 1952 he took leave and participated in a reconnaissance study of the petroleum resources of northern Spain.

Cloud's studies in the Pacific had shown him that work in present depositional environments was required, so he investigated the Great Bahamas Banks on marine expeditions in 1955 and 1956. He was instrumental in moving the U.S. Geological Survey from studies of the land only to studies of the sea floor and in organizing the program in marine geology. After stepping down as chief paleontologist of the survey, Cloud turned to the seas and oceans in earnest. He saw that there was a critical gap: active oceanographic institutions were concentrating their investigations on the deep ocean and the Survey should study the continental shelves and coastal zones. This required stimulating interest among Survey managers and generating support within the National Research Council and with congressional contacts so that legislation and support were forthcoming for Survey investigations beyond the shoreline. This program is responsible for much of what we now know about the U.S. continental shelves and coastal zones, including Alaska. It plays a key role in the appraisal and development of offshore petroleum and minerals.

In 1961 Cloud accepted appointment as chairman of the Department of Geology and Geophysics at the University of Minnesota. He recognized that many disciplinary approaches are required in understanding the earth and its history and so organized the School of Earth Sciences at that university. This school, of which he became the first head, included his department, the Minnesota Geological Survey, and the Limnological Research Institute. Up to this time, Cloud's personal research had largely concentrated on the last 600 million years of earth history and its life. He now began to concentrate primarily on the complex problems of understanding the interacting processes that shaped the first 85 percent of the history of our planet. He developed his own techniques in paleomicrobiology and blended them with the methods and results of geochemistry and field geology toward the goal of reconstructing the past. These led him to appraise the levels of oxygen and carbon dioxide through time and to consider the buffering systems and geochemical sinks that affect atmospheric composition and the sequestration and recycling of carbon. For the remainder of his life he concentrated on studies of the pre-Phanerozoic record and the evolution of life. He personally examined key outcrops around the world, such as those in southern Africa, South America, Siberia, China, Australia, and North America. He worked on all continents except Antarctica but managed to visit that continent a few years ago as a perceptive tourist. On excursions he always joined local geologists, experts in the regions, and enjoyed thoroughly the experiences of such fieldwork, especially the socializing with his colleagues in the evenings around campfires.

Cloud's scientific writings illuminated many subjects. "Two features set his papers apart from the ordinary: painstaking attention to empirical detail coupled with intellectual boldness in interpretation. Without the first, no claim about the earth can be taken seriously. In the absence of the second we will not see farther, even when perched atop a mountain of data."[1] In pursuit of his ultimate goal of understanding the evolution of the biosphere as well as he could, Cloud studied the story revealed by specific sequences of strata and in so doing unlocked understanding of many associated processes and products. For example, banded iron formations about 2 billion years old tell much about the geochemistry of ancient oceans and the conditions surrounding their deposition and subsequent alterations. The origin of these important ores has long intrigued mining geologists, and Cloud's scope of outlook contributed to understanding both their genesis and the geochemical processes in very ancient seas. His approaches, anchored in investigating enigmatic strata perceptively and then in reasoning to broader environmental interpretations, disclosed much concerning the origin of carbonates, the conditions prevailing when siliceous rocks were laid down in the company of primitive microorganisms, and in documenting their change through time from unicellular to complex organisms. Cloud concluded that the slow increase in oxygen in the atmosphere and hydrosphere had indeed left a decipherable record. His inventiveness led him to publish the first electron micrographs of isolated pre-Phanerozoic microbes and show that cellular differentiation was under way by 2 billion years ago. He joined with Soviet paleontologists and confirmed their view that stromatolites displayed useful variations in pre-Phanerozoic stratigraphy.

Cloud was a true founder and leader in the burgeoning field of pre-Phanerozoic studies. Because the record is piecemeal, only a holistic approach to understanding the first 85 percent of earth history is feasible. Cloud concluded that the earliest Paleozoic metazoan record when fossils become abundant was less an accident of an incomplete record in strata previously and actually a display of evolutionary opportunism related to the availability of new ecological sites and conditions. The Metazoa descended from many ancestors, probably starting when there was sufficient oxygen available about 680 million years ago. Life cells with nuclei came along between 1.3 billion and 2 billion years ago, perhaps when the oxygen level dipped slightly. Free oxygen first began to accumulate on earth about 2 billion years ago, mainly as the result of biological activity. Life cells with nuclei came along during the next several hundred million years. Before these times there were intervals when banded iron formations were laid down, mainly owing to the activities of microbial life. In fact, indirect evidence implies that oxygen-producing microbial life was present when the oldest-known sediments were laid down, about 3.76 billion years ago. This history of the earth as now viewed is nicely portrayed diagrammatically on the front end-papers of Cloud's life-summative book, *Oasis in Space* (1988).

Cloud was a stickler for accuracy in nomenclatural concepts and was eloquently outspoken before international commissions on the distinctions between time and rocks and other matters. For example, he saw a need for erecting a new geologic period (the Ediacarian) before the Cambrian Period. This is a period with a physical and biological record indicating closer relations to the Phanerozoic than to conditions prevailing before.

In 1965 he joined the University of California, Los Angeles, as professor of biogeology, jointly with the Institute of Geophysics and the Department of Geology, of which, at the time, I was chairman. The UCLA faculty was convinced that interdisciplinary approaches were essential in understanding the earth, both at present and during past eons. The geophysicists at the Institute led the way in accepting a paleontologist! Three years later Cloud transferred to the Santa Barbara Campus (UCSB) and served actively on the faculty until 1979, both as professor and professor emeritus. On the UCSB campus he established a "clean lab" for the study of ancient life, a facility in part set up by NASA to examine moon and other extraterrestrial material for evidences of life activities. In 1969, upon examining some of the first samples brought back from the moon, Cloud determined that the moon was devoid of life. This lab, formally dedicated as the Preston Cloud Laboratory, is a separate building adjacent to the Department of Geological Sciences, where research continues on pre-Phanerozoic life and history. From 1974 to 1979 Cloud was again a member of the U.S. Geological Survey based in Santa Barbara and continued as a strong and wise scientific influence on the UCSB campus. He advised and conferred with students and colleagues and regularly attended talks and seminars, regardless of the topic. At these he inevitably asked searching questions and many times hosted evening discussions in his home afterwards. He participated enthusiastically on departmental field trips and even went to areas where the rocks were of no special interest to him. His concern over the welfare of our planet as a human habitat and his deep knowledge of environmental interplays provided us with sound advice as we organized a Program in Environmental Studies at UCSB. Cloud was indeed a special influence in my own life over the twenty-five years we were colleagues. I owe much to lunchtime discussions that guided my thinking concerning paleoclimates and tectonics as well as organizational matters.

Cloud was widely sought as a public speaker and symposium participant on the subjects of resources, the human future, and the primitive earth, and he gave many lectures a year. He was one of the few speakers I have known who could read a lecture with a natural and seemingly extemporaneous intonation and therefore say exactly what he wished to say! He organized, chaired, and participated in several invitational symposia on emerging scientific opportunities that have had seminal influence: the Shelter Island Conference on Paleoecology (1956), the Woodring Conference on major biological innovations and the geologic record (1961), the Laramie Conference on Pre-Cambrian History (1970), the Rubey Conference on Crustal Evolution (1973), and others. He also contributed his thoughts while serving on several visiting committees to universities throughout the nation. In his later years Cloud went away from Santa Barbara on extended visits, such as to accept a Luce Professorship of Cosmology at Mount Holyoke College and a Queen Elizabeth II Senior Fellowship at Canberra, Australia. During these visits he continued to work and write. His life was truly characterized by remarkable intellectual energy and an ability to organize and concentrate on the task at hand.

Along the way Cloud continuously reflected upon the bearing of his increasing knowledge of the history of the earth to problems facing humankind and to our understanding of the environment around us. He was the prime motivator and organizer of several influential studies, including at least three undertaken by the National Research Council. As chairman of the Committee on Resources and Man, he saw through to publication the volume *Resources and Man,*[2] which brought together authoritative chapters written by leading ecologists, resource and energy specialists, demographers, and others. This book, with an introduction and set of recommendations primarily written by Cloud, has been widely used as a text or reference book in universities and has guided many environmental students and professionals along thoughtful paths. Later Cloud organized the Committee on Resources and the Environment and steered it toward fruitful objectives—the eventual long report (completed under the chairmanship of B. J. Skinner) has been particularly influential on U.S. energy and mineral policies. His efforts in these directions involved testimony before and preparation of materials for various congressional committees and panels, including the Joint Economic Committee of both houses of Congress, for which he prepared a statement on the mineral raw materials and national welfare (1976). Early on Cloud saw the importance of understanding the long evolution of the earth's climate throughout geologic time in approaching problems of climate change in the near future, especially those changes anticipated as industrial society burgeons. As a consequence of this vision, Cloud saw through to publication the report titled *Geological Perspectives on Climate Change,* which in turn has stimulated the preparation of additional NAS-NRC studies.

Cloud's research therefore led him directly from studies of the long record of life and environmental factors that influence it to reports that have aided the guiding of societal and governmental policy. His list of diverse publications exceeds 200 titles. These include not only scientific papers and policy reports but also books of appeal to the serious nonspecialist. He assembled a collection of previously published papers by many authors in *Adventures in Earth History.*[3] This book is held together by enlightened essays, written by Cloud, that show the relation of each specific contribution toward our goal of understanding the whole. And he wrote *Cosmos, Earth, and Man,*[4] which thoughtfully places man into his tiny spot in the universe.

More recently Cloud wrote *Oasis in Space: Earth History from the Beginning,*[5] which is a comprehensive work of synthesis and reflection aimed at perceptive intellectuals and university students. It is a documented history of the earth and life on it as we now understand them and an impressive capstone to his remarkable scholarship. Only Cloud could have written it. We are fortunate that he placed on paper this distillation of his understanding for all of us, specialists and generalists alike. The book points to the uniqueness of our planet and our dependence on very special circumstances and events over 4.5 billion years. It is intercalated with wise and informed comments concerning the nature of science, of geology, and of the future. Cloud was above all an interdisciplinary and holistic scientist but also a specialist in several fields. He moved from discipline to discipline as he perceived new challenges or saw new data and approaches emerging that bear on arriving at understanding the biospheric history of the earth. In many ways he was driven to spread the excitement of science and the need for humankind to wake up to its place within the universe. He felt that all should realize, as he states in *Oasis* (pp. 14-15), "We are made of star stuff, processed through supernovae, concentrated from the contracting solar nebula, spun into biochemical aggregates with a difference, and graced, during our tenure here, by the ability to imagine, to conceptualize, to hypothesize, to create science, poetry, music, and works of art and technology." He leaves a deep influence upon a vast and diverse assemblage of scholars, both scientists and humanists, who have read his works or listened to his lectures or who have, as I, discussed matters of earth history with him personally. He has indeed left an influence upon educational and governmental policy across the world.

Pres shared his life with three wives, who worked closely with him on his scholarly activities and immensely enhanced his achievements. He met Mildred Porter of the Peabody Museum at Yale University while he was a graduate student. They were married when he moved to Rolla, Missouri, and she shared with him experiences during the war years in Alabama and Texas and then returned with him to Harvard University. When Pres left Harvard and joined the U.S. Geological Survey, they were divorced. During his time in Saipan, he met and later married Frances Webster. They had two daughters, Karen and Lisa, and a son, Kevin. Pres and Fran were divorced in 1965. Following his move to Santa Barbara, Pres met Janice Gibson whom he married in 1972. They made their home in Santa Barbara, where together they raised her three children: Morgan, Dante, and Amanda De Lucia. Pres took great pleasure in the companionship and accomplishments of his children and stepchildren, especially as he relaxed more in his later years. In October 1990, a few months before he died, a group of his friends, family, and colleagues from across the nation met in Santa Barbara for a surprise party on the occasion of the publication of a special volume of the *American Journal of Science* dedicated to him (titled "Proterozoic Evolution and Environments"). He took special and humble pleasure in this honor. Most of his life he profited from vigorous health, unusual energy, and the ability to focus his intellect on problems and work at hand. During his last few years, however, his body began to fail owing largely to the inroads of amyotrophic lateral sclerosis. Despite this, Cloud's strong will and work habits of a lifetime carried him on with no sign of diminished intellectual activity. He died at home on January 16, 1991.

In preparing this memoir I was very much helped by Mrs. Jan Cloud and Mrs. Fran Cloud and by permission to read before publication a memorial written by John Rodgers for the American Philosophical Society, which has also been published by the Geological Society of America. Letters from Cloud's colleagues at different times over the years also aided me, especially those of Jack Dunlap, Virgil Barnes, Ean Zen, Ellen Moore, Dallas Peck, Greg Sohn, Pete Palmer, Tom Dutro, Link Washburn, Reuben Ross, and many many others.

NOTES

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2. National Research Council. *Resources and Man* (San Francisco: Freeman and Company, 1969).

3. National Research Council. *Geological Perspectives on Climate Change* (Washington, D.C.: National Academy of Sciences, 1978).

4. *Cosmos, Earth, and Man* (New Haven, Connecticut: Yale University Press, 1978).

5. *Oasis in Space: Earth History from the Beginning.* (New York: Norton and Company, 1988).

HONORS AND DISTINCTIONS

1941	A. Cressey Morrison Award in Natural History, New York Academy of Sciences
1956	Rockefeller Public Service Award Honorary Fellow, Paleontological Society of India
1959	Distinguished Service Award and Gold Medal, U.S. Department of Interior
1961	Member, National Academy of Sciences
1969	American Academy of Arts and Sciences
1971	Paleontological Society (of America) Medal
1973	American Philosophical Society Lucius Wilbur Cross Medal, Yale Graduate School Corresponding Member, Geological Society of Belgium
1975	Fourteenth A. L. DuToit Memorial Lecturer (and first American), Royal Society of South Africa and affiliated societies
1976	Penrose Gold Medal, Geological Society of America
1977	Walcott Medal, National Academy of Sciences
1980	Foreign Member, Polish Academy of Sciences

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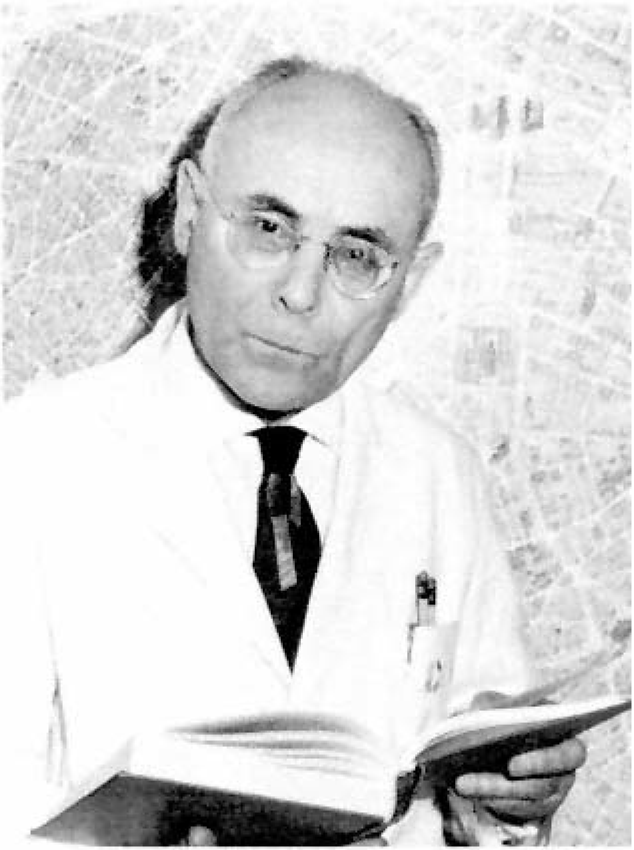

André Frédéric Cournand

September 24, 1895-February 19, 1988

By Ewald R. Weibel

The subtitle of André Cournand's autobiography—*The Intellectual Adventures of a Medical Scientist*—conveys the essence of his life. As a basic scientist he was a medical man concerned about helping his patients through fundamental research. As a medical scientist he was adventurous, just as he had dared to climb high mountains in his younger years. His courage to introduce a catheter into a man's heart changed physiology, but only because of its intellectual backing by a rigorous analytical concept. André Cournand was an artist among scientists; he combined imagination with discipline and rigor in his analytical approach, a sense of drama with critical thinking about the course to take both in his research projects and, in later years, in his concerns about shaping the future.

André Frédéric Cournand was born in Paris, where he lived the first thirty-five years of his long life until his emigration to the United States. He felt very much French and European. His mother was the daughter of an Alsatian businessman, and his father descended from a Corsican author and poet who had participated in the French revolution of 1848. As Cournand writes in his autobiography, the influence of his mother was to give him a strong sense for the arts and an inclination toward adventure: "In my mother the adventurous spirit expressed itself primarily through imagination and sympathetic recognition of the impetus of adventure in others. This same eager readiness for the new and the unexpected was to make its influence felt in my own life." He was to receive a classical education, but at the age of sixteen he left the lycée to split his time between private tutoring in philosophy and work in a private laboratory to learn about scientific techniques. He nevertheless completed his undergraduate studies and enrolled in the Faculty of Sciences in order to be admitted to the Faculty of Medicine, a decision prompted chiefly by the influence of his father.

Cournand's father, Jules, was a dentist in private practice but of high renown academically, particularly because of his many innovations in dental technology for which he was awarded twenty-five patents. "My father inspired my interest in medicine and helped me to conceive it as neither an academic discipline nor a field of practical action alone; rather, he showed me by example how the interaction of theoretical and clinical interests could provide the basis for imaginative creations that contributed both to general knowledge and to solution of practical problems," writes Cournand, and this should, indeed, become the hallmark of his career in medical science. With his father he would not only explore the treasures of Paris but also begin his long career in mountain climbing.

In 1914 Cournand entered the Faculté de Médecine, just as World War I broke out. Even before his first year of medical school ended, he was enlisted in the army to serve as auxiliary battalion surgeon, for the most part in the trenches near the German enemy. For his distinguished services tending wounded soldiers at the front he was awarded the Croix de Guerre with three bronze stars. This experi ence in the battlefield lasted three and one-half years and left a deep impression on him: "It had been necessary to develop the attitudes of mind and feeling to face danger and take risks," and he believed that this was the undergirding of his emerging dual disposition to be open to the intellectually new and skeptical of received wisdom.

After returning from the war and recovering from some injury incurred at the end of his service, Cournand resumed his medical studies in 1919. During his internship he trained in pediatrics, chest diseases, internal medicine, and neurology in the Hôpitaux de Paris. He prepared his dissertation, required for the M.D. degree, on the topic of acute disseminated sclerosis ("La Sclérose en plaques aigue"), which involved experimental studies at the Institut Pasteur on isolating a virus from brain tissue. In his autobiography Cournand remains rather brief on his career as an intern in Paris, but he notes that his promotions came with difficulty. For one, his "conception of liberal thinking and action" did not help him in the still conservative French medical system marked by paternalism, but then also he spent much of his time and energy on a different type of activity.

During his service at the front he became friends with the young painter Jean Lurçat, who later became the famous designer of tapestries. This friendship brought Cournand into close contact with the so-called modern movement that determined the art life in Paris during the 1920s; he was introduced into the circle around Jeanne Bucher, whose gallery was an important meeting point for the modern artists. Here he met, among many other artists of renown, Jacques Lipschitz, one of the leading sculptors of the cubist period, with whom he maintained a lifelong close friendship. It was in this circle that he met his future wife, Sibylle, the younger daughter of Jeanne Bucher and her husband, the Swiss pianist Fritz Blumer. They married several years later and raised four children: three daughters, Muriel, Marie-Eve, and Claire, and Sibylle's son by a first marriage, Pierre Birel Rosset.

Around 1930 Cournand felt ready to go into private medical practice. But since he wanted to specialize in chest diseases he decided to enlarge his experience by working for one year in an American hospital. He felt fortunate to be admitted as resident to the well-known Chest Service at Bellevue Hospital in New York City. After a few months of service, partly in a sanatorium, Cournand was offered the possibility to participate in a long-range research project on pulmonary physiology by joining the group of Dickinson Woodruff Richards, a man of his own age but already quite advanced as an investigator. To accept this offer, however, meant not to return to France. Considering Cournand's exquisitely "French" or even "Parisian" life-style during the past decade this required a radical decision, but one taken by him and his wife with a positive mind. In his autobiography Cournand justifies this decision with several arguments. One was that the "free exchange of views in the United States had made a strong impression" on him and that here he would have "the prospect of an academic career where achievements count far more than nepotism." And he says that, in retrospect, the possibility to "leave behind a way of life whose disregard for the conventional bore little relationship to some values that constituted the treasure of my education" was an additional though unconscious element. Last, but not least, he felt excited "to participate in creating techniques to be applied to new protocols of clinical investigation and in rationalizing treatment." He returned to Paris briefly in 1932 to arrange his affairs after the accidental death of his father and then came back to New York to stay. But he always remained much attached to his home country. A large artistic map of Paris adorned one wall in his office at Bellevue, and he used to remark: "Je n'aime pas les départs . . . sauf pour Paris."

THE SHAPING OF AN INVESTIGATOR

Cournand's serious research activity began with what he called his "transplantation to the United States." In his bibliography he lists ten papers from his time in Paris, mostly case reports presented to French medical societies. When, in 1933, he published his first major paper he was already thirty-eight years old. It was a report on his work at the Bellevue Hospital Chest Service but was written in the European tradition, based purely on clinical evidence (1933,1); it makes no reference to pulmonary function tests that would be the main focus of Cournand's subsequent work, which, indeed, was already ongoing at the time of publication (1933,2).

Cournand became an experimental investigator through his association with Richards, with whom he remained closely associated throughout their lives and with whom he won the Nobel Prize in 1956.[1] Born in New Jersey the same year as Cournand, Richards also took up his medical studies in 1919 after some war service. Already during his residency he began with research projects that he extended during a fellowship in Sir Henry Dale's laboratory in London. Back in New York he directed his research to blood and circulation. One aspect of these studies was to improve on blood gas measurements, particularly of CO_2, because this was needed for the estimation of cardiac output. The so-called indirect Fick method then in use calculated cardiac output (or total blood flow) as the ratio of CO_2 output from the lung to the CO_2 concentration difference between the blood entering and leaving the lung. Whereas CO_2 concentration in arterial blood was easy to measure, CO_2 concentration in mixed venous blood had to be estimated indirectly from the CO_2 partial pressure in alveolar air.

When Cournand joined Richards in 1932 he became involved with this line of work. His first project was to test and improve a rebreathing method for estimating mixed venous CO_2 content and to apply it to some cases of pneumothorax (1933,2; 1935,1). The results remained only partially satisfactory, which led Cournand and Richards, some eight years later, to develop the method of right heart catheterization in order to obtain direct samples of mixed venous blood as it enters the lung.

In the meantime they directed their attention to some problems that had emerged when studying diseased lungs, namely that gases do not mix evenly in the lung, particularly in cases with pulmonary emphysema (1937,1,2). An important series of studies were undertaken in collaboration with Robert Darling, who developed a breath-by-breath analysis of intrapulmonary mixing of inspired air, introducing the simple method of washing out intrapulmonary nitrogen through the inhalation of pure O_2 (1940,1), a method that has been widely used and further improved by many other investigators. In the normal lung alveolar nitrogen is rapidly washed out, but in emphysematous lungs this is much slower because nitrogen is retained in the enlarged air spaces. In subjecting the closed-circuit method for estimating residual air volume mentioned above to a systematic critique (1940,2), they concluded that its failure was due to unequal distribution of gases *within* the lung. To overcome these shortcomings, a new open-circuit method with pure oxygen breathing was introduced (1940,3) that had theoretical advantages but still did not solve all the problems. At the time, these systematic studies of pulmonary ventilatory function, in which Cournand, Darling, and Richards themselves served as the normal subjects, made a very significant con tribution to the advance of clinical respiratory physiology (1941,6).

With these methods in hand, Cournand and Richards proceeded to a systematic study of pulmonary insufficiency, which they classified according to the prevailing ventilatory, respiratory (i.e., gas exchange), or cardiocirculatory disturbances (1941,2). They developed the tests by which to differentiate between these functional disturbances. The efficiency of alveolar ventilation, studied at the time by several other groups, was combined with the measurement of arterial O_2 saturation as a test for adequate matching of alveolar ventilation with capillary perfusion. It is of historic interest that two papers of this series (1941,3,4) use these concepts and tests to estimate the effects on "pulmonocirculatory" function of various types of collapse therapy: in the late 1930s pulmonary tuberculosis was still a major disease, and collapse therapy was one of the major modes of treatment.

THE BREAKTHROUGH: CARDIAC CATHETERIZATION

In reflecting in his Nobel lecture on the state of their capabilities at that time, Richards concluded: "We were able to describe the ventilatory functions of the lung and . . . to define to some extent the mixing and the diffusional aspects of pulmonary alveolar or alveolar-capillary functions. But we still could not measure blood flow through the lungs and could not, therefore, move into those broader concepts of cardiopulmonary function which now began to be our goal." The problem still was how to obtain adequate samples of mixed venous blood to reliably apply the Fick principle. In 1936 Cournand and Richards decided that the only way of securing such samples was to introduce a catheter from a peripheral vein into the right atrium. They knew that this technique had been used in animals since the pioneering work of Claude Bernard in 1846 and that the young German surgeon Werner Forssmann had, in a heroic self-experiment in 1929, introduced a thin ureteral catheter into his own right atrium from an arm vein, but yet the procedure was not considered safe for human application. In order to assess the question of risk, Cournand went to Paris, where a former medical teacher of his, Dr. P. Ameuille, had introduced a catheter from an arm vein toward the right atrium in over 100 cases in view of introducing radio-opaque contrast medium for visualization of pulmonary vasculature. "I reviewed all the cases and returned to New York persuaded that cardiac catheterization could be used safely and would meet our needs," Cournand writes in his autobiography. He then reports that for the next four years, in collaboration with Robert Darling, he carried out experiments in dogs and one chimpanzee and "adapted Bernard's method" to their problem of obtaining samples of mixed venous blood for estimating O_2 and CO_2 concentrations. It is said that Cournand and Richards also tried the catheterization technique on human cadavers, but there is no mention of this in the published record.

Finally in 1941 Cournand and Hilmert Ranges published a note on "Catheterization of the Right Auricle in Man" (1941,1), detailing the technique already developed to near perfection and assessing the possible effects of the catheter on blood and heart function; the catheter was left in position fifteen to sixty minutes and no ill effects were found. They obtained mixed venous samples and could report the calculation of cardiac output by the Fick principle in one case. That was a breakthrough. Cournand did not invent cardiac catheterization, as is often said; his first paper on the method starts out as follows: "Forssmann first used catheterization of the right heart on himself" (1941,1). But he perfected the technique for safe and widespread use in humans, even in severely ill patients, and thus brought it to fruition; more importantly still, he pioneered the use of this method by obtaining the first significant measurements of cardiopulmonary function in health and disease. Cardiopulmonary physiology was different after that.

The paper of Cournand and Ranges is based on eight catheterizations in four cases of which the first one was catheterized on October 25, 1940. The third patient, considered normal with respect to the heart and lung, was catheterized three times in December, with the most comprehensive set of measurements performed on New Year's Eve 1940. In May 1941 Richards and Cournand presented their estimations of right atrial blood pressure, mostly based on the study of the same cases (1941,5), and on January 6, 1942, a paper (1942,1) was accepted by the *American Journal of Physiology* that reported in detail in these and some additional cases the direct measurement of the blood pressure in the right auricle and in peripheral veins, demonstrating the rise in atrial pressure in right heart failure. In this paper they also reported on some of the results of their preliminary animal studies. The first measurements were done with saline manometers; improved recordings of the actual pressure waves were obtained a few years later when the catheter was connected to a Hamilton manometer, a technique introduced by Stanley Bradley from the group of Homer Smith at New York University (1944,2).

On December 2, 1941, a paper by Cournand, Ranges, and Richard L. Riley was published in the *Journal of Clinical Investigation* (1942,2) that reported on twenty-one estimations of cardiac output by the direct Fick method. Using both O_2 uptake and CO_2 discharge, excellent agreement between the estimates was obtained in all cases. When on June 27, 1944, a much extended study was submitted for publication (1945,1), the group had reportedly performed some 260 catheterizations on humans, and they had introduced many improvements in their methods of analysis. The resulting estimates of cardiac output were very consistent and consequently appeared definitive.

With these studies the great value as well as the safety and feasibility of catheterization of the right heart were established. Some small but ingenious technical details contributed to the success: the catheter tip was given a curve to allow better positioning, and a double lumen catheter was constructed for simultaneous pressure recording or blood sampling from two serial points along the bloodstream (1945,2); a special needle for arterial blood sampling (the "Cournand needle") was designed. But, most importantly, a battery of physiological methods was set up to obtain the most reliable measurements of blood gases and of pressures. It is the whole concept of an analytical system that set the precedents just as much as the ingenuity of probing the heart with a fine catheter.

PHYSIOLOGICAL CONTRIBUTIONS WITH THE CARDIAC CATHETER

Cournand and Richards were now ready to approach questions of the pathophysiology of pulmonary circulation. The first condition that imposed itself was traumatic shock, a most pressing problem in those years of World War II with its many casualties in the U.S. armed forces that had just entered combat. The foremost problem of shock is hemodynamic deterioration due to severe blood loss. In the shock unit set up at Bellevue Hospital, cardiac catheterization was used to show that with about half the blood volume lost cardiac output became critically depressed, with the result that shock worsened; also, the importance of variable reduction in peripheral blood flow, particularly to the kidneys, was elucidated by Stanley E. Bradley. Most importantly, the new method was used to monitor therapy in these severe conditions.

With the end of the war a period of intense research began that established the basis for understanding the role of blood flow in pulmonary gas exchange, directed mainly to three topics: (1) pulmonary insufficiency, (2) maldistribution of ventilation and perfusion in chronic lung disease, and (3) congestive heart failure and cor pulmonale.

A most important set of by now classical studies approached the problem of alveolar ventilation-perfusion relationship as a determinant factor of pulmonary gas exchange (1949,6; 1951,7,8). Richard L. Riley, who had already been associated with the shock program, and Cournand (1949,6) applied a method for estimating "ideal" alveolar air composition and addressed the importance of variations in the ventilation-perfusion relationship for gas exchange, a concept that triggered innumerable subsequent studies by many investigators to the present day: ventilation-perfusion mismatch is considered one of the most important impairments of lung function. Complex four-quadrant graphs were used to interpret physiological measurements of the composition of gas and blood obtained simultaneously through a cardiac catheter and an indwelling arterial needle while the subject was breathing into a spirometer. The equipment and approach described in perfected form in 1951 (1951,8) is a masterpiece of strategy and rigor reflecting the highest laboratory standards, so the conclusion was certainly justified: "The method . . . makes possible the quantitative evaluation, for the lung as a whole, of all the major factors affecting the partial pressures of oxygen and carbon dioxide in the gas and the blood of the lungs." These papers should be mandatory reading for respiratory physiologists.

Cardiac catheterization also allowed pressures to be re corded from the right auricle, the right ventricle, and the pulmonary artery once the catheter tip could be positioned at will along the bloodstream. Cournand and his collaborators therefore directed their major research effort to studying the hemodynamics of the lesser circulation: if flow and pressures could be measured, the work of the heart could then be calculated. And since an indwelling needle was routinely placed in the brachial artery, systemic pressure recordings could also be obtained, which allowed an integrated assessment of the hemodynamics in the greater and lesser circulation. In exercise, they noted that the increase in cardiac output was accompanied by a slight fall in pulmonary artery pressure and hence in pulmonary vascular resistance; the lungs' small vessels open up, and this also improves the efficiency of gas exchange.

Cournand reviewed the new insights on pulmonary hemodynamics gained during the first few years in two major lectures. In the first review of 1946 (1947,2), he could base his report mostly on work done in his own laboratory, whereas four years later several groups from different places in the United States and Europe had already made significant contributions after they had adopted the technique of cardiac catheterization. In the Walter Wile Hamburger Memorial Lecture delivered in 1950 (1950,5)—which Cournand considered one of the first recognitions of his work—he reviewed the knowledge on the physiology of pulmonary circulation acquired during the few years since his first successful attempt at cardiac catheterization and then went on to discuss the value of "modern physiologic methods" for the study, understanding, and treatment of pulmonary diseases. He focused on pulmonary hypertension as related to left ventricular failure and on pulmonary emphysema where severe structural changes and functional disturbances add up to increase the load on the heart, thus setting the stage for right heart failure. Cournand concluded this lecture with the statement that "a better knowledge of the physiology of the pulmonary circulation in normal man has been of help in understanding some of the adaptive changes which occur in the course of diseases of the heart and lungs. In retracing the steps that lead to the breakdown of homeostasis . . . the cause of sound and effective treatment of these diseases has been well served."

In 1947 Cournand had confirmed in man the observation of the Swedish investigators U. S. von Euler and G. Liljestrand that hypoxia causes a rise in pulmonary arterial pressure (1947,4). This suggested that hypoxic vasoconstriction could regulate blood flow distribution to well-ventilated alveoli. With Alfred P. Fishman and Harry W. Fritts, a series of studies (1955,3; 1960,3,4) were undertaken to identify the mechanism and site of action of acute hypoxia on the pulmonary vasculature, but they failed to do so; indeed, even at the time of writing this memoir some three decades later the true cause of this effect remains elusive.

The potential to probe the cavities of the right heart and the pulmonary artery with a catheter found its use in yet another field: in the accurate diagnosis of congenital malformations of the heart and great vessels. Together with the pediatric cardiologist Janet Baldwin and the surgeon Aaron Himmelstein, Cournand set up a group devoted to the study of congenital malformations. Cardiac catheterization and angiocardiography were used to establish the functional disturbance of the anomaly in preparation for surgery (1947,5; 1949,1; 1952,2). In his Nobel lecture Richards noted that the advances in surgery of congenital heart disease were under way before cardiac catheterization and that it has moved ahead on its own but that the cardiac catheter has been a primary aid.

"A TIME FOR REWARDS"—PEER RECOGNITION

By the early 1950s André Cournand had published about 100 papers on the physiology and pathophysiology of the cardiopulmonary system, several of which have become true classics as they have brought significant innovations. He introduced the nitrogen washout method for studying the gas compartment of the lungs. He greatly advanced the study of the role of uneven ventilation-perfusion relationship for gas exchange. He pioneered cardiac catheterization in humans and was the first to measure cardiac output with the direct Fick method using mixed venous blood samples and the first to record blood pressure in the right heart and the pulmonary artery. By that he has most significantly contributed to the development and innovation of cardiopulmonary physiology. If one reads his early papers one is impressed by the scientific rigor of his approach as much as by the ingenuity of the meticulous techniques he had to develop. The artistic spirit of adventure inherited from his mother had blended with the imaginative inventiveness received from his father to bring about these advances.

Recognition of these achievements did not come his way easily. Cournand was made professor of medicine at the Columbia University College of Physicians and Surgeons only in 1951, at the age of fifty-six and after having served in various teaching positions at this university for eighteen years. In 1946 he was awarded the Andreas Retzius Silver Medal by the Swedish Society of Internal Medicine, and in 1950 he received the Lasker Award. Then in 1956 he was awarded the Nobel Prize for Physiology or Medicine together with his long-time partner, mentor, and friend Dickinson W. Richards, and the German surgeon Werner Forssmann, the heroic pioneer of heart catheterization. This prize emphasized cardiac catheterization as the most sig nificant innovation in cardiopulmonary physiology. But by this emphasis it does not perhaps render true justice to the achievements of Cournand and Richards, for it is what they did using this technique in association with a whole battery of other methods that set a new standard of physiological research and thus fully justified this highest distinction.

Following the Nobel Prize, broad recognition flowed freely. Cournand was made a member of the National Academy of Sciences in 1958 together with Richards. He, the Frenchman, was admitted as a foreign associate to the French Academies of Sciences and of Medicine and in time became an honorary member of many learned societies around the world. He collected nine honorary doctorates, including one from Columbia University and three from France. His home country distinguished him by the highest honors it could give, naming him Commandeur de la Légion d'Honneur and Commandeur des Palmes Académiques. The years following the Nobel Prize were a true "time for rewards," as Cournand titles a chapter of his autobiography, and he thoroughly enjoyed it.

THE UNRELENTING SCIENTIST

I had the privilege of joining Cournand's group for two years as a research associate. I came into a newly refurbished laboratory that was sizzling with scientific activity. Cournand was full of energy and of a restlessness that bordered on impatience. It was still his time for rewards, and the part he enjoyed most was that he was in great demand as a lecturer. He was indeed a good speaker, who, with his French charm and accent, could elicit enthusiasm and sometimes irritation in his listeners; and furthermore he had a scientific message to deliver. For the Cardiopulmonary Laboratory at Bellevue Hospital this meant that he was in and out of the place. He would come to the laboratory, stir up the crew, discuss all the projects, collect information and slides, and take off for the next lectures.

Disputes on scientific and other issues were almost the order of the day in the laboratory when Cournand was around; he loved arguments. They were indeed an important part of what Richard Riley called "the Cournand magic" in his presentation of the Trudeau Medal to Cournand in 1971, and he went on to say that "one must be a little irreverent to convey a feeling of this magic. . . .He was outrageous at times but always exciting to be with. His moods changed from moment to moment, sometimes bringing about a graceful end to an argument with a sudden tension releasing: 'You are right' and sometimes a withering exclamation: 'That man is impossible.'" For all that he was certainly exciting to be with, particularly for all the young investigators who were in his laboratory at my time. And he was still eager to explore new scientific avenues.

In those years the research was chiefly directed towards two main topics: study of the ventilation-perfusion relationship and its disturbance, particularly in emphysema, directed by William A. Briscoe; and pulmonary blood flow and cor pulmonale, led by Réjane Harvey, Irène Ferrer, and Harry W. Fritts. In typical Cournand tradition these topics were approached with novel techniques. Radioactive tracer gases were introduced and used in various ways to estimate the degree of uneven ventilation and perfusion of airspaces and to find new ways of determining pulmonary hemodynamics.

Cournand and Richards had for a long time nurtured an interest in introducing structural studies into their program, as Richards noted even in his Nobel lecture. During my training as an anatomist I studied the structural relation between bronchial and pulmonary arteries; following a seminar I gave at Bellevue on this topic, they invited me to join their laboratory "to do anything on the structure of the lung that is of interest for physiology," an unusual offer that turned out to be a true challenge. Cournand gave me complete freedom and generous support to set up a histological laboratory. My great fortune was that, by the time I arrived, Cournand had given refuge to Domingo M. Gomez, who had fled his native Cuba to escape Fidel Castro's reign of terror. A cardiologist and biomathematician, Gomez had formerly worked with Homer Smith and had thus become involved with some of the early studies on estimating cardiac output. Now he was interested in theoretical aspects of pulmonary gas exchange that could establish a link between lung structure and function. Together we began to study the architecture of the human lung with new quantitative methods, and this led to what is now called morphometry. Cournand was not directly involved in these studies, but his way of asking questions and his keen interest in seeing this new line of work develop were most important. His laboratory provided the best environment to pursue this kind of new enterprise; it was traditionally a place to do new things.

In 1964 André Cournand retired as director of the Cardiopulmonary Laboratory at Bellevue Hospital, and a few years later the laboratory he had established thirty-two years earlier was closed. This ended a most productive period of research that can be divided into three main phases: the first ten years were devoted chiefly to developing new methods of investigation; the study of heart and circulation followed; and finally, exploration of the lung and pulmonary circulation along several tracks.

"LATE BUDDING": TURNING TO BROADER ISSUES

André Cournand freely admitted that "the idea of enforced termination of a scientific career that had provided great satisfaction . . . was in my case hardly welcome. . . .

How was I to escape idleness by starting new and satisfying activities?" In retrospect this was indeed an understandable concern considering that he would have over twenty years ahead of him, in full vitality and with a vivid intellect. He would, in time, have some difficulties with his eyes, ears, and knees but not with his mind, which remained sharp and lucid to his last weeks. His autobiography, published in 1986, shows no sign that its author had passed the ninetieth-year mark. During these twenty years Cournand directed his interest and still considerable work capacity to three fields: planning for the future, the ethics of science and social responsibility, and exploration of the historical roots of his life's work.

Cournand notes that the Nobel Prize had somehow transformed his life in that he felt a greater responsibility to step out and address broader issues. After his retirement he was able to devote himself fully to a concern that had captured his interest a few years earlier: study of the "Prospective Approach to the Future," a methodology formulated by the French philosopher Gaston Berger, which offered a creative way of planning the future by placing preferred futures as the driving force for planning, instead of merely allowing the present to project itself into the future. Cournand, who had met Berger during the fifties, translated some of his works into English; compiled them in a book, *Shaping the Future,* that was published in 1973; and organized meetings and seminars on the topic in an attempt to disseminate these ideas.

Concern about the role of responsible creativity led Cournand to reflections about the specific responsibilities of scientists. Clearly, he had an excellent background of a practical nature, and I should say that three features of his scientific career demonstrate his sense of responsibility in his own scientific work: the fact that he strived for investi gative methods that would stand all tests and yield credible insights; his endeavor to address the important and pressing questions; and his willingness to employ his new methods for the benefit of his patients. But now he wanted to work out principles of an ethical code of the scientist. In that vein he joined the Frensham Pond group and developed a close relationship with the social scientists Robert K. Merton and Harriet Zuckerman. In 1976 he published in *Science* his views on "The Code of the Scientist and Its Relationship to Ethics," identifying seven principles: objectivity, tolerance, recognition of error, recognition of priorities, doubt of certitude, unselfish engagement, and sense of belonging to the scientific community.

His third line of interest concerned "looking back at the roots" with his remarkable autobiography, *From Roots . . . to Late Budding,* as the main result. He looked back at the development of cardiopulmonary physiology from Claude Bernard to his own time. And finally, he wrote a biographical memoir on Dickinson Woodruff Richards, a very personal portrait of his lifelong partner to whom he was bound, as he said, in "a friendship [of which] the essence is not to look into each other's eyes, but to look in the same direction."

André Cournand had a very long and productive life. He certainly knew that he had fulfilled his mission. This life full of work saw joy and hardship. During the Second World War he lost his only son in combat, and in 1959 his wife Sibylle died at the age of fifty-eight after long suffering. Four years later he married Ruth Fabian, his associate of many years; with her he would start his active post-retirement life that caused him to travel frequently to Europe. On one of these trips to London in 1973 Ruth died unexpectedly, leaving Cournand to "plunge [himself] into concentrated thinking and writing," as he says. In 1975 he in vited Beatrice Bishop Berle, an old family friend and widow, herself a physician, to accompany him on a trip to Lindau, where Nobel laureates regularly meet, and a few months later they were married. They led a very happy life, full of rewards, splitting their time between their New York apartment and Beatrice's beautiful farm in Massachusetts, when they were not in Paris or traveling in Europe, attending meetings and visiting friends and former disciples. These years were marked by Cournand's active interest in the education and careers of his grandchildren, who were often at his side. Then, for the last months of his life he "retired" to the Berkshire farm; he put down his pen. His heart began to weaken and he felt he had come to the end of his long journey. Early in February of 1988 I visited him for the last time; our intercourse was wordless. On February 19, 1988, he passed away peacefully at the age of ninety-two. A long rich life had come to an end.

NOTE

1. Dickinson Woodruff Richards, by André Frederic Cournand. In *Biographical Memoirs,* vol. 56, pp. 459-85. Washington, D.C.: National Academy Press, 1986.

BIOGRAPHICAL DATA

EDUCATION

1914	B.A., University of Paris, Faculties of Arts and of Sciences
1930	M.D., University of Paris, Faculty of Medicine

PROFESSIONAL APPOINTMENTS

1926-30	Interne des Hôpitaux de Paris
1930-68	Resident, Attending Physician, Consultant, and Director of Cardiopulmonary Laboratory, Bellevue Hospital, New York
1935-88	Columbia University College of Physicians and Surgeons
1951-61	Professor of Medicine
1961-64	Professor of Clinical Physiology
1964-88	Professor Emeritus
1965-69	Member of the Institute for the Study of Science in Human Affairs

HONORS AND DISTINCTIONS

Croix de Guerre, France (1914-18)

Commandeur de la Légion d'Honneur (France)

Commandeur des Palmes Académiques (France)

1946	Andreas Retzius Silver Medal of the Swedish Society of Internal Medicine
1950	Lasker Award, U.S. Public Health Service
1956	Nobel Prize for Physiology or Medicine (with D. W. Richards and W. Forssmann) Associé Etranger, Académie des Sciences de 'Institut de France Associé Etranger, Académie Nationale de Médecine, Paris
1958	Gold Medal, Royal Academy of Medicine, Brussels Member, National Academy of Sciences Honorary Fellow, Royal Society of Medicine, London
1959	President, Harvey Society, New York
1962	John Phillips Memorial Award, American College of Physicians
1966	Academy Medal, New York Academy of Medicine
1969	Associé Etranger, Académie Royale de Médecine de Belgique
1970	Jimenez Diaz Prize, Madrid
1971	Trudeau Medal, American Thoracic Society
1975	Fellow, American Academy of Arts and Sciences Honorary Associate Member, Académie des Sciences, Belles-Lettres et Arts de Lyon

Doctor Honoris Causa

1957	University of Strasbourg
1958	University of Lyon
1959	University of Brussels
1961	University of Pisa
1961	University of Birmingham
1963	Gustavus Adolphus College
1965	Columbia University University of Brazil
1968	University of Nancy

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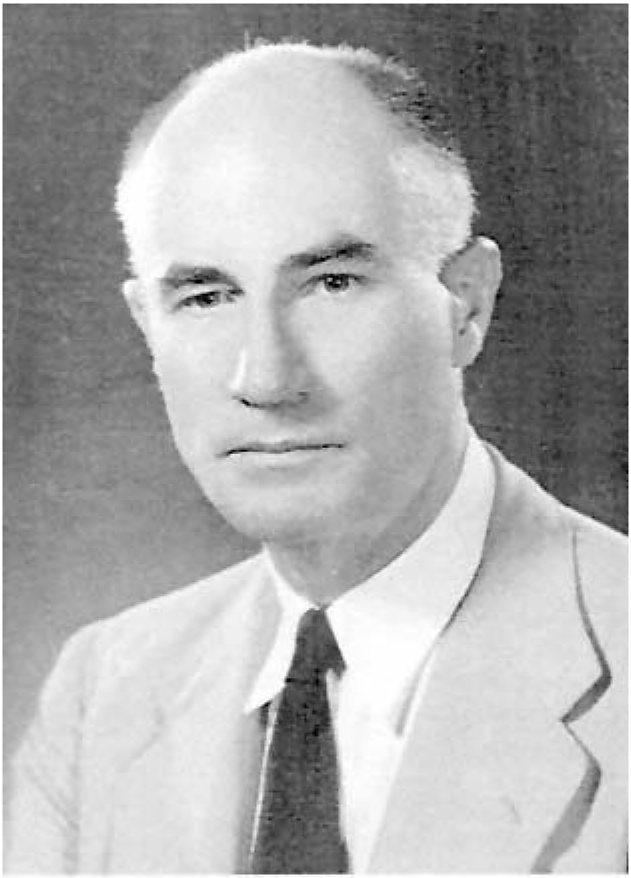

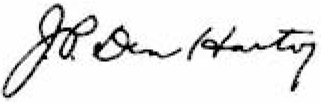

Jacob Pieter Den Hartog

July 23, 1901-March 17, 1989

By Stephen H. Crandall

Although I had studied his book *Mechanical Vibrations* in 1940 when I was an undergraduate at Stevens, I did not meet Professor Den Hartog until the fall of 1946 when I joined the faculty of the mechanical engineering department at MIT. Den Hartog had himself come to MIT only a year earlier after serving in the navy during the war. Twenty years my senior and already world famous, he was both an inspiring role model and a gracious mentor. He was internationally famous as a vibration consultant with an uncanny ability to identify and explain the cause of a mysterious vibration. But, above all, Den Hartog was a consummate teacher. He could hold the attention of a single colleague or a class of a hundred students as he explained a particular mechanism and wrapped his audience in the sheer fun of imagining how it would move and why. He taught dynamics by creating vivid images of particular cases that dramatized generic concepts. Generations of students were enriched by his verve, wit, and captivating physical insight.

Jacob P. Den Hartog was born in Ambarawa on the island of Java in the Dutch East Indies on July 23, 1901. His father, Maarten, had been a school teacher in Amsterdam until he was dismissed because of radical activity. Maarten had been an outspoken supporter of Alfred Dreyfus in the early phases of that famous affair when the popular view was strongly against Dreyfus. The family was forced to go to the Indies, and Maarten taught school in the colonial system in Ambarawa, Makassar, and Batavia. Young "Jaap" grew up speaking Dutch and Malay. He attended elementary school and took violin lessons. When it was time for Jaap to enter high school, it was decided that he, his two younger sisters, Wilhelmina and Clara, and his mother, Elizabeth, would return to Amsterdam, while Maarten would remain in Java. Because Holland was a neutral country during the First World War, their ship could sail, brightly lit, without interference from submarines, although it was necessary in 1916 to bypass the Suez Canal and sail down around the southern tip of Africa and up around Iceland to reach Holland.

The following eight years were difficult for the Den Hartog family. Maarten, the father, died in Java soon after the family returned to Holland, and his widow was left with three children to support. Jaap was such an outstanding high school student that some of his relatives undertook to pay his expenses at the Technical University of Delft. Entering Delft in 1919, young Den Hartog decided to become an electrical engineer after seeing a dramatic physics demonstration in which a bolt of lightning jumped from one charged sphere to another. He was a good student, but because of his limited financial situation he was unable to participate in sports or social activities. He compensated for this by developing a strong prejudice against the rich. Economic conditions in Holland were sufficiently bad in 1924 when he graduated that even the best Delft students could not be sure of finding a job. For some reason, Den Hartog did not try very hard. He made only two applications, and when he was rejected, he impulsively decided to leave Holland to seek his fortune in the United States.

Arriving in New York without connections and essentially penniless, Den Hartog took to America with great enthusiasm. He worked briefly at a sequence of temporary jobs until he learned that Westinghouse was hiring electrical engineers in Pittsburgh. Luckily he was accepted by Westinghouse just in time to be placed in an in-house training course for new engineers. Among the lecturers was Stephen P. Timoshenko, an emigré Russian professor of mechanics who had been hired by the Westinghouse Research Laboratories the previous year. Timoshenko was impressed by the eager young Dutchman. None of the American engineers had even heard of a Bessel function. When the training course was completed, Timoshenko requested that Den Hartog be assigned to the mechanics section of the research laboratories to work as his assistant. It was here that Den Hartog served his real professional apprenticeship. In the next three years Timoshenko converted the young electrical engineer into a mechanical engineer by assigning him a wide variety of vibration problems across the whole spectrum of Westinghouse products: electric motors and generators, steam turbines, hydropower turbines, railroad electrification, etc. While working at the research laboratories during the day, Den Hartog studied mathematics in the evenings at the University of Pittsburgh. In 1926, after receiving a steady salary for nearly two years, he considered his prospects sufficiently good that he proposed to his childhood sweetheart, Elisabeth F. Stolker. They decided to have the wedding in Amsterdam during his summer vacation and to consider the ocean voyage back to America as their honeymoon. The young couple called each other by their Dutch diminutives, Jaapie and Beppie, as did most of their friends throughout their lives. They set up housekeeping in Pittsburgh and bought a piano for Beppie, who was an accomplished pianist. As enthusiastic emigrés, they made a pact to always speak English to one another. A year later their first son Maarten was born.

Technically, Westinghouse was an exciting place to be during the twenties. This was a period of industrial growth and expansion. Challenging engineering problems were being successfully solved in innovative ways by highly talented individuals. In the mechanics section the most influential person was Timoshenko. With his powerful personality he was an inspirational leader for the young men in the division. He made them feel they were capable of great accomplishments and encouraged them to publish their results as technical papers. The five years that Timoshenko spent at Westinghouse have been called its "golden era of mechanics" in which rational analysis blossomed and took precedence over empirical methods. By the time Timoshenko left to join the faculty at the University of Michigan, the mechanics sections had grown to include J. M. Lessells, G. B. Karelitz, R. E. Peterson, J. Ormondroyd, A. M. Wahl, and A. Nadai, in addition to Den Hartog. They also worked closely with outstanding engineers in other departments, such as C. R. Soderberg in the generator department and L. S. Jacobsen in the motor department.

The theory of vibrations had been assembled in volume 1 of Lord Rayleigh's *Theory of Sound* in 1877 and subsequently been applied to technical problems by European engineering professors, but in the twenties it was still terra incognito to most American engineers. Den Hartog was forced to acquire this expertise in a hurry under the supervision of Timoshenko. Because of his strong mathematical background Den Hartog had little difficulty. Furthermore, having to learn the theory in the context of a sequence of urgent real problems gave him a unique practical approach to the subject, which illuminates his famous text, *Mechanical Vibrations.* Early on he exhibited a flair for the dramatic.

One of his first cases involved the shaft of a motor-generator set that was continuously breaking. Timoshenko recognized the fracture as due to torsional fatigue and suggested that Den Hartog calculate the torsional resonance. It turned out that the torsional critical speed was exactly at the operating speed. Furthermore, only a slight detuning was required to alleviate the situation. Although the detuning could be accomplished equally well by either stiffening or softening the shaft, Den Hartog boldly recommended that the diameter of the shaft be reduced by one-sixteenth of an inch. When this unlikely cure completely solved the problem of broken shafts, his reputation as a vibration expert was launched.

While at Westinghouse, Timoshenko was one of the activists pressing for the establishment of a separate Applied Mechanics Division within the American Society of Mechanical Engineers (ASME). When the division was finally started in 1927, Den Hartog plunged into its activities with great energy. In the *Transactions* for that first year are his first three published papers. It should be remembered that this occurred at the same time that he was working on his doctoral dissertation. When he received his Ph.D. degree from the University of Pittsburgh in 1929, he had published a total of eight papers on technical problems he had solved at Westinghouse. His dissertation, "Nonlinear Vibration with Coulomb Damping," provided material for three more publications. In 1930 he received another document to which he had long looked forward: his certificate of naturalization as a citizen of the United States of America.

The mechanics section continued to grow. A new addition in 1929 was O. G. Tietjens, who had been an assistant to Professor Ludwig Prandtl in Göttingen. Tietjens had published a two-volume book in Germany based on Prandtl's lectures. Den Hartog encouraged him to have these trans lated into English and published in America and undertook the translation of the second volume, *Applied Hydro and Aero-Mechanics,* himself. In 1930 there was a major reorganization of the mechanics section. The section became a department divided into two sections: dynamics and materials. J. M. Lessells became manager of the mechanics department, while the dynamics section was headed by Den Hartog and the materials section by R. E. Peterson, who later became department manager when Lessells was transferred to Philadelphia. At this time, management did not have much appeal for Den Hartog. He arranged to spend the following year, his sabbatical year at Westinghouse, as a postdoctoral student in the laboratory of Professor Prandtl, Jaapie and Beppie, with young Maarten, enjoyed this year in Göttingen immensely. For Jaapie it was an unexcelled opportunity to meet the engineering research leaders of Europe. This was to stand him in good stead fourteen years later. Soon after returning to Pittsburgh, he was offered an appointment as assistant professor of mechanical engineering at Harvard University. He had already done some lecturing in the Westinghouse training courses for new engineers, and he jumped at the chance to try teaching as a full-time occupation.

In September 1932 Den Hartog arrived at Harvard and started his academic career. Full of enthusiasm, he poured his energies into his vibrations course, getting an extensive collection of demonstration models made and starting to write his famous text *Mechanical Vibrations.* Although only thirty-one years old, he already was widely known as a vibration expert. Professor E. S. Taylor at neighboring MIT told me how he and other young MIT faculty used to regularly ride the trolley over to Harvard to sit in on Den Hartog's lectures.

During the decade at Harvard, Den Hartog was engaged in a variety of professional activities. Perhaps the most important achievement was the publication of *Mechanical Vibrations* in 1934, with a second edition in 1940. This book remains *the* classic vibration text. The early chapters follow the grand outline of Rayleigh but have a less mathematical, more appealing, practically motivated style. What distinguishes it from any other text, before or since, are the chapters on vibrations of real machines (reciprocating engines, rotating machinery) and the simplified physical explanations for an extensive catalog of self-excited vibration phenomena. During these years Den Hartog continued his active involvement with the Applied Mechanics Division of ASME. When the *Journal of Applied Mechanics* began separate publication in 1933, it contained at least one of his contributions each year for the first seven years. He served as division chairman in 1940 and 1941. Another important involvement was with the International Congresses of Applied Mechanics, which renewed and extended his relations with European scientists. He presented a paper at the fourth congress held in Cambridge, England, in 1934 and took an active part in hosting the fifth congress, held in Cambridge, Massachusetts, in 1938. He also served as coeditor of the proceedings of the fifth congress.

While at the University of Michigan, Timoshenko organized an annual special summer school for teachers of mechanics, which had an important influence on mechanics education in America. Den Hartog acted as a guest lecturer for several summers in this program while he was at Harvard. It was also during this period that he began to take on consulting jobs. His principal clients were Hamilton Standard, an aircraft propeller manufacturer, and two builders of Mississippi tugboats that had torsional vibration problems in their diesel engine drives.

The decade at Harvard also saw many changes in the family life of the Den Hartogs. They bought a house in Wellesley. Their second son, named Stephen Ludwig, in honor of Timoshenko and Prandtl, respectively, was born in 1933. A weekly ritual was the musical evening when Jaapie, joined by two fellow amateurs plus a professional cellist, played string quartets. In 1938 the Den Hartogs bought a small island in Lake Winnipesaukee, some 75 miles north of Boston, and built a cabin from the timber provided by the big hurricane that year. The island was to play an important part in their lives after the war.

In 1939 Den Hartog volunteered for a commission in the U.S. Naval Reserve. This may seem like a strange thing for a successful academic to do in mid-career. I believe he had two major reasons. First, he had come to the conclusion that war was inevitable and he wanted to position himself advantageously for that outcome. Second, he had begun to chafe at the low esteem with which engineering seemed to be regarded by most of the Harvard administration. His own position was safe (tenure had come with his promotion to associate professor in 1936), but he was irked by the policy decision to move away from engineering toward applied science. At any rate, he was given a commission as a lieutenant commander on inactive duty, which he held for two years until June of 1941 when he was called up for active duty. A year later he resigned from Harvard.

The four navy years proved to be an exciting entr'acte in Den Hartog's academic career. Initially, he was assigned to the Taylor Model Basin in Bethesda, Maryland, near Washington, D.C. He immediately contracted to have a house built in the neighborhood, put a "For Rent" sign on the house in Wellesley, and moved the family down to Washington. After Pearl Harbor he was transferred to the Bureau of Ships in Washington. The next three years were full of hard work but were very stimulating from a professional point of view. The navy was building ships as fast as it could, and they all had potential vibration problems, from windshield wipers on P.T. boats to propeller shafts on cruisers. Den Hartog shuttled back and forth from design conferences on one ship to sea trials on another. He was involved in most of the interesting vibration problems, and he had the opportunity to interact with many of the country's leading engineers. In February 1943 he was promoted to the rank of commander. Later that year he was approached by MIT administrators and it was decided that he would join their faculty "after the war."

In August of 1944 it began to be clear that the Allies were prevailing over the Nazis in Europe. The navy decided to send a special technical mission to Europe. This group of some forty officers and sixty enlisted men was to follow the advancing Allied forces with the aim of debriefing enemy technicians and capturing interesting technical equipment. Commander Den Hartog was an ideal choice for this mission. He spoke Dutch, Flemish, German, and French and knew many European scientists personally. In his wartime diary Den Hartog called this final year of the war "the most interesting year of my life."

The technical mission arrived in France shortly after its liberation and set up headquarters in Paris. For most of the year normal operations involved trips in teams of two or three officers to target locations. Typically, the information and equipment could be obtained in a few days, and the team returned to Paris to write up its reports and plan the next trip. Travel close to the front lines was always difficult and involved many frustrations with interservice red tape. Den Hartog's targets in the winter of 1944-45 were in Belgium, France, and England. In the spring his targets were in a sequence of German cities, including Göttingen, Nürnburg, and Buchenwald as the Nazis retreated. When the Germans surrendered in Holland and Denmark, he immediately went to Holland and was in Amsterdam on V-E day. The technical mission continued in high gear for an additional three or four months. In Denmark Den Hartog stumbled on a number of midget submarines still in shipping containers and was able to arrange to have five of them, plus five trained German operators, sent back to the States for careful appraisal of their capabilities.

The technical mission worked hard for a year. It also played hard. Evenings back in Paris often involved dates, dinner and dancing, and theaters or concerts. American officers with easy access to cigarettes and nylons were the privileged rich of wartime Paris. Den Hartog was an enthusiastic participant in these extracurricular activities. He organized a string quartet and rented instruments for them to play. He had connections with well-placed French families who were only too happy to be invited to the officers' dining room. He also enjoyed striking up friendships from chance encounters. With his command of languages he was often the one who provided dates for his colleagues. In his diary he observes that, "The upper class and the lower class know how to enjoy themselves, but Lord deliver me from the up-tight middle class." When he and two of his colleagues were promoted to captain, he arranged for a dinner party of sixty men and thirty ladies, with corsages for the ladies, 100 bottles of champagne, a dance band, and twenty bottles of cognac.

In September 1945 Captain Den Hartog was deactivated, and Professor Den Hartog took up his new post as professor of mechanical engineering at MIT. After the excitement of the previous year, it took a while to get back into the academic groove. He began by teaching one class and working on the third edition of *Mechanical Vibrations.* Gradually, Den Hartog began to expand his activities. He took on the task of acting as graduate student registration officer for the department and recommenced supervising doctoral students. He also began to play in the violin section of the MIT orchestra. The buildings on the island in Lake Winnipesaukee were expanded to facilitate entertaining, and an isolated study cabin was built with a huge desk looking out on the water. It was here that Den Hartog wrote his textbooks on *Mechanics* (1948), *Strength of Materials* (1949), and *Advanced Strength of Materials* (1952). His consulting practice began to grow. Typically, a newly built structure or machine would be found to be inexplicably vibrating. Professor Den Hartog would be telephoned and would travel to the site by overnight train. He would observe the phenomenon intently and ask many questions. In his mind an image of the underlying phenomenon would develop, along with a diagnosis of the most probable cause. He would then share his surgical insight with the client, with clarity and humor. In many cases his initial insight was sufficient to provide the basis for a satisfactory solution. In obstinate cases additional tests would be recommended to pinpoint the difficulty. A great many of his clients were satisfied with the results after only one or two days of professional service. An exception was Exxon, which kept him on a retainer for thirty-four years and regularly sought his advice over a wide range of problems.

In the fifties Den Hartog was at his peak as an educator. He had become an entertaining raconteur with a large store of real case histories that he used to illustrate fundamental concepts. For the graduate students in mechanical engineering, his lectures were the high point of their MIT education. Following in the footsteps of his mentor Timoshenko, he organized his own special summer courses in vibration for engineers in industry. He and Beppie often entertained students and visiting academics on their island. For many foreign scientists, their most vivid memory of America is of being enthusiastically bundled into a car, driven up to New Hampshire, seated in a canoe, and paddled across the lake to spend an idyllic weekend on the island. In the early fifties the Den Hartog's son Maarten, now an architect, designed them a spacious house in Concord, ideal for entertaining. The house, only 18 miles from MIT, was heavily used for that purpose, especially during the years 1954-58 when Den Hartog served as head of the Department of Mechanical Engineering. It was in this period that he sandwiched a sabbatical term in Japan as a Fulbright lecturer in 1955 and went to England in 1957 to give the Thomas Hawksley Lecture, the first American to be so honored. He also made a final improvement of *Mechanical Vibrations,* publishing the fourth edition in 1956. By this time the book had become world famous. In all there were fifteen foreign editions published in eleven languages.

When Den Hartog returned to teaching in 1958, the jet airplane had arrived and was making travel more convenient, especially for lecture tours and consulting visits. He lectured widely in America and abroad, including the Soviet Union in 1960 and 1961. As he began to approach retirement, he was honored with an increasing stream of awards. The Design Division of ASME dedicated the proceedings of its first vibration conference as a Festschrift to Den Hartog on the occasion of his retirement at age sixty-five in 1967. He continued part-time lecturing and consulting for an additional five years but had to cut back on some activities. Arthritis in his fingers made it impossible to play the violin. In 1972 the Applied Mechanics Division of ASME awarded him the Timoshenko Medal, established in honor of his early mentor. The ceremony took place just a few months after Timoshenko's death at the age of ninety-three.

In the following decade there was a gradual slowdown in

Den Hartog's consulting activities and occasional lectures as the grip of arthritis strengthened throughout his body. The year that he was eighty (1981-82) was the final year before he became totally bedridden. It was, however, a memorable year. On his birthday MIT's Department of Mechanical Engineering established the Den Hartog Prize for "excellence in teaching." A few months later the British Institution of Mechanical Engineers awarded him the prestigious James Watt Medal. Then the National Academy of Engineering awarded him its top honor, the Founders Award. Finally, the following spring he received the Order of the Rising Sun, signed by the Emperor of Japan.

At this point his arthritis-ravaged body finally surrendered, imprisoning his active spirit in a skeleton he could not move. His last major project, which he directed from his bed, was the sale of the Concord house and the purchase of a condominium for Beppie next door to a nursing home in Hanover, New Hampshire. The plan was sound. Jaapie was installed in the nursing home. Beppie had her own apartment a few steps away, and their son Stephen, who worked in Hanover, could conveniently drop by to check on his parents. And so it worked until Easter day 1985, when Beppie passed away peacefully in her sleep.

Jaapie lived another four years unable to move himself, unable to read or write. He spent his time listening to music, mostly string quartets, and the Public Broadcasting news programs. He never complained. He could look back on a very full life: the excitements of emigration and the wartime adventures, the challenges of strange new consulting problems, and the solid accomplishments of a beloved teacher. His major contribution was the book *Mechanical Vibrations.* There were also several valuable contributions to the theory of vibrations in his research papers. Of particular importance are his extensions to systems with damping of the theory of dynamic vibration absorbers (1928,2) and of the Holzer method for torsional vibration (1946). He was the first to obtain solutions for vibratory systems with Coulomb damping (1931). He was also the first to give a quantitative explanation for the phenomenon of galloping of ice-laden transmission lines (1932,2). But most of all he could look back on the fun he had teaching generations of students who went on to make successful careers of their own.

In his twilight years Jaapie was not forgotten. Many of his students and colleagues made the pilgrimage to Hanover to visit him. He enjoyed these visits. His mind remained clear, his memory excellent. He would retell the old stories, but he also retained his curiosity about the present and the future. In 1987 the Design Division of ASME announced the establishment of the J.P. Den Hartog Award for "sustained meritorious contributions to vibration engineering" at its eleventh vibration conference. The first recipient of the award was Den Hartog himself. This was twenty years after the first vibration conference had been dedicated to him. When the medal was delivered to him he was pleased to think that "his boys still remembered him." Those of us who were fortunate enough to have known him in his active days will always remember his uncanny physical insight and the energetic enthusiasm and sparkling wit with which he made it all seem so clear.

I wish to thank Stephen L. Den Hartog for his gracious help in giving me access to his father's files and wartime diaries and for supplying details of the family history.

HONORS AND DISTINCTIONS

Professional Societies

American Society of Mechanical Engineers
American Consulting Engineers Council
American Society of Engineering Education
Institute of Aeronautical Sciences
Sigma Xi
Society of Naval Architects and Marine Engineers
Tau Beta Pi

Honors and Awards

American Academy of Arts and Sciences, Fellow
American Society of Mechanical Engineers, Honorary member
Japan Society of Mechanical Engineers, Honorary member
National Academy of Sciences, Member
National Academy of Engineering, Member
Royal Dutch Academy of Arts and Sciences, Foreign member
Charles Russ Richards Medal, Worcester Reed Warner Medal, Timoshenko Medal, ASME Medal, and the Jacob Pieter Den Hartog Medal of the American Society of Mechanical Engineers
Founders Award of the National Academy of Engineering
Lamme Medal of the American Society of Engineering Education
Order of the Rising Sun
Thomas Hawksley Lecture and the James Watt International Medal of the Institution of Mechanical Engineers
Trente-Crede Medal of the Acoustical Society of America

Honorary Degrees

Carnegie Institute of Technology
Salford University
Technical University of Delft
University of Newcastle-Upon-Tyne
University of Ghent

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Photo by The Brookner Studio, Pittsburgh, Pa.

Paul Hugh Emmett

September 22, 1900-April 22, 1985

By Walter S. Koski

Paul Hugh Emmett was an outstanding investigator in the field of heterogeneous catalysis. His work is distinguished by the use of highly ingenious experimental methods to probe the basic mechanisms in catalytical processes. His studies on the adsorption of gases on solids led to a method of measurement of the surface area of catalysts and laid the foundation for the Brunauer-Emmett-Teller theory of adsorption, which has been of fundamental significance in the field of heterogeneous catalysis.

Emmett was born in Portland, Oregon. His father worked in various jobs associated with railroad construction. His mother kept house, and in the summer months she frequently cooked for a crew of ten to forty workers who might have been working under his father's supervision. Both parents had a limited education, but they provided Paul with a happy home with all the feeling of security a growing boy could want. They were determined that their son should go as far in school as his interests and ability permitted. All of Emmett's pregraduate school training took place in Oregon. He attended Washington High School in Portland. He was a good student but disliked that portion of high school English that required oral presentation. He had a fear of public speaking. He overcame this fear by active participation in both high school and college debating, and, when he attended the California Institute of Technology, A. A. Noyes asked him to help out with coaching the debating team. After these experiences, Paul had no difficulty with addressing an audience so long as he had something to say.

Emmett's interest in chemistry was sparked by an English teacher who served as a part-time vocational counselor. She noticed his high grades in mathematics and languages, and she insisted that he take chemistry and physics when he was a senior in high school. Paul was further influenced by an inspiring high school chemistry teacher, William Green, and by J. F. G. Hicks at Oregon State College. After graduating from Oregon State at Corvallis, where he received a bachelor of chemical engineering degree in 1922, he went on to the California Institute of Technology for graduate training.

His selection of catalytic work as a major interest was due largely to the influence of A. F. Benton who had just arrived at Cal Tech fresh from Princeton University, where he obtained his degree under H. S. Taylor. The work that Benton was doing appealed most to Emmett, and he spent two years working with him on the catalysis of the hydrogen-oxygen reaction to form water over nickel catalysts and on the kinetics of the reduction of nickel oxide by hydrogen. Emmett also worked with Linus Pauling on the crystal structure of barium sulfate. He received his Ph.D. degree from Cal Tech in physical chemistry in 1925. After graduation he spent a year teaching at Oregon State College and then accepted a position at the Fixed Nitrogen Research Laboratory in the U.S. Department of Agriculture in Washington, D.C. There he launched a successful research career in adsorption and catalysis. In 1937 he accepted an appointment to organize the chemical engineering department at Johns Hopkins

University. Here he continued his research in the field of adsorption. During World War II Emmett worked on the Manhattan Project under Harold Urey on the development of barrier materials suitable for the diffusional separation of U-235 and U-238. He continued as a consultant to the Atomic Energy Laboratory at Oak Ridge, Tennessee, for the rest of his career. In December 1944 he joined the Multiple Petroleum Fellowship at the Mellon Institute as a senior fellow and carried on a long series of experiments directed toward the understanding of catalytic processes using radioactive tracers. In July 1955 he was appointed the W. R. Grace Professor of Chemistry at Hopkins, where he carried on a combined teaching and research program in catalysis. Also at this time he became a lifetime consultant to the Davison Chemical Division of the W. R. Grace Company. He retired from Hopkins in 1970 and returned to Portland, Oregon, where he accepted a position as visiting research professor of chemistry at Portland State University. He taught a course in catalysis, directed research, and wrote papers until his final illness.

When Emmett joined the staff at the Fixed Nitrogen Research Laboratory in 1926, he was involved in a program that was attempting to find the mechanism by which iron catalysts caused hydrogen and nitrogen to combine and form ammonia and why some catalysts were more effective than others. It became increasingly clear that a method was needed for determining the surface area of the iron catalyst. Benton had already published an adsorption isotherm for nitrogen on iron at -191°C, and he suggested that certain breaks in the adsorption isotherm were probably related to the formation of one or two layers of adsorbed nitrogen. Emmett initiated research to follow Benton's suggestions, and the effort culminated in the development of what has come to be called the Brunauer-Emmett-Teller (BET) method for measuring the surface areas of finely divided or porous solids. This approach was widely accepted, and it represents Emmett's most important contribution to the field. The experimental aspects of the investigation were carried out by Brunauer. Edward Teller, who had just recently joined the faculty at George Washington University, was responsible for the theoretical portion of the study. Throughout his career Emmett tried to obtain independent evidence for confirming the values for surface areas as deduced from the BET method. He and his colleagues, for example, applied it to nonporous finely divided carbon blacks whose size could be estimated from electron microscope measurements and to virgin glass spheres whose size and surface area could be estimated by direct microscopic measurements. All of these checks tended to confirm the validity of the surface-area measurements by low-temperature gas adsorption. The method was found to be applicable to all sorts of solids, including ethylene and propylene polymers and ethylene-butane copolymers.

A second aspect of Emmett's research that should be cited is the use of radioactive and stable isotopes to elucidate the mechanism of catalytic reactions. He and his co-workers pioneered the use of ^{14}C as a tracer in investigating catalytic reactions. This activity stemmed from the interest during World War II in the synthesis of hydrocarbons and other products from carbon monoxide-hydrogen mixtures by iron and cobalt catalysts through a modification of the original Fischer-Tropsch process. At the time, two mechanisms were in contention. In one case it was felt that the reaction proceeded through the formation of metallic carbides as intermediates. On the other hand, the intermediates could be oxygenated complexes on the catalytic surface. The work started in the spring of 1945 when ^{14}C became available. This experimental study of the Fischer-Tropsch process soon demonstrated that the carbide mechanism was untenable and that it was more likely that the complexes formed by CO and H_2 were similar to those formed by chemisorption of primary alcohols. The final experiments showed that successive addition of carbon atoms occurred on that carbon of the intermediate oxygen complex to which an OH group was attached. Since then, these results have been repeated and confirmed by other investigators.

Tracer work was also applied to cracking catalysts with the objective of determining to what extent olefins, paraffins, and aromatic products of the cracking process undergo secondary reactions before exiting from the catalytic reactor. It was established that once paraffins are formed, they change very little on passage through the remainder of the catalyst bed. Olefins higher than ethylene, on the other hand, build up into higher molecular weight hydrocarbons by polymerization, alkylation, and cyclization. They are responsible for the high aromatic content of products from the cracking of straight chain reactants.

Stable isotopes were also used in some studies. For example, the exchange of nitrogen between $^{28}N_2$ and $^{30}N_2$ to form ^{14}N-^{15}N over ammonia catalysts was investigated. Deuterium was used to study hydrogen chemisorption by metallic catalysts, and heavy water was used to study the nature of active sites on silica-alumina catalysts. In connection with their many catalytic studies, Emmett and his co-workers measured the equilibria involved in a number of catalytic systems. These involved ammonia-hydrogen mixtures in contact with Fe-Fe_4N, Fe_4N-Fe_3N, and Fe_3N-Fe_2N; the equilibrium of water vapor-hydrogen with Fe-FeO, Fe-Fe_3O_4, $FeOFe_3O_4$, Co-CoO, and SnO-SnO_2; and the equilibrium of carbon monoxide-carbon dioxide mixtures with Co-CoO and the equilibrium of CH_4-H_2 mixtures with Fe-Fe_2C, Fe-Fe_3C, Ni-Ni_3C, CO-CO_2C, Mo-Mo_2 C, and Mo_2C-MoC. These equi libria and free energy values have proved to be of considerable value in catalytic work. An important by-product of these studies was the demonstration played by the phenomenon of thermal diffusion on equilibria measurements. Specifically, they showed that most of the equilibrium data in the then-existing literature for the Fe-FeO, Fe-Fe_3O_4, and FeO-Fe_3O_4 systems in contact with H_2O vapor were in error by as much as 40 percent. They showed that this error was the result of the thermal diffusion phenomenon and that proper procedures could eliminate it.

Emmett and his group devoted a significant amount of their effort to studying the mechanism of ammonia synthesis over iron catalysts. This program included a study of the solid iron nitride-iron-nitrogen system from a thermodynamic and phase rule approach; a study of the chemisorption of nitrogen, hydrogen, oxygen, carbon monoxide, and carbon dioxide; an examination of the poisoning of iron catalysts by water vapor; a study of the distribution of promoters on the surface of the reduced catalyst; and a study of the kinetics of ammonia synthesis and decomposition. The solid nitride studies furnished equilibrium data for the first time for the ratio NH_3/H_2 in equilibrium with Fe-Fe_4N, Fe-Fe_3N, and Fe_3N-Fe_2N systems. It showed conclusively that several thousand atmospheres of N_2 would be needed to convert iron to Fe_4N at synthesis temperatures, and it established that synthesis did not occur through the alternate formation and reduction of the bulk nitride. The nitrogen chemisorption measurements indicated that the rate of adsorption of nitrogen was probably the slow step in the synthesis. Nitrogen seemed to be adsorbed in atomic rather than in molecular form at synthesis temperature. They were able to confirm that one of the functions of alkali promoters was to prevent the retention at high temperatures of inhibiting NH and NH_2 groups, which clearly were present in the catalyst containing only aluminum oxide as a promoter. Although not all aspects of the mechanism were clarified, the study revealed a number of important factors that influenced the synthesis and gave deep insight into the detailed mechanism of ammonia synthesis.

Paul's years of association with work on iron catalysts for ammonia synthesis led him to explore the behavior of iron for hydrogenation of molecules such as olefins, hydrogenation of CO to higher hydrocarbons, and hydrogenation of benzene to cyclohexane. Interest gradually spread to hydrogenation over other metals such as Pt, Rh, Pd, and Ni. After the announcement by Dowden of the importance of d-band vacancies in catalytic metal hydrogenation, Emmett's group turned its attention to the behavior of Ni and its alloys. Dowden suggested that filling all the d-bands in Ni by alloying it with Cu would destroy the catalytic activity of the metal. Indeed, it did in the hydrogenation of styrene; however, this generalization did not hold for the case of ethylene. Emmett's group helped to clarify a number of questions related to this field and pointed out that many questions still remained to be answered.

In a research program, frequently new apparatus or techniques are developed, and one associated with Emmett's group that should be cited is the microcatalytic-chromatographic approach for studying catalysts. This technique, which was suggested by R. J. Kokes, involved putting a small catalyst tube in series with a chromatographic column. The latter was used to identify the amounts of products formed on injection of micro quantities of reactants into the stream of carrier gas. This has proved to be a valuable approach and is now widely used.

The outstanding nature of Paul Emmett's work was recognized by many awards, including membership in the National Academy of Sciences (1955); honorary doctorates of science from Oregon State College (1939), the University of Lyon (1964), and Clarkson College (1969); and an honorary doctor of laws from the University of Hokkaido (1976).

Emmett was married three times. His first marriage was to Leila Jones, who died in 1968. He had a brief second marriage that ended in divorce. His third marriage was to Pauline Pauling Ney, who survived him.

Personally, Paul was a kind individual who greeted his friends with a pleasant smile and a twinkle in his eyes. He enjoyed discussing science and was a pleasure to interact with.

The writer of this biographical memoir expresses his appreciation to the archives of Johns Hopkins University, to Alfred S. Levinson of Portland State University, and to John M. Kopper of Johns Hopkins University for much of the information reported here.

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K. O. Friedrichs

Kurt Otto Friedrichs

September 28, 1901-January 1, 1983

By Cathleen Synge Morawetz

This memorial of Kurt Otto Friedrichs is given in two parts. The first is about his life and its relation to his mathematics. The second part is about his work, which spanned a very great variety of innovative topics where the innovator was Friedrichs.

PART I: LIFE

Kurt Otto Friedrichs was born in Kiel, Germany, on September 28, 1901, but moved before his school days to Düsseldorf. He came from a comfortable background, his father being a well-known lawyer. Between the views of his father, logical and, on large things, wise, and the thoughtful and warm affection of his mother, Friedrichs grew up in an intellectual atmosphere conducive to the study of mathematics and philosophy. Despite being plagued with asthma, he completed the classical training at the local gymnasium and went on to his university studies in Düsseldorf. Following the common German pattern of those times, he spent several years at different universities. Most strikingly for a while he studied the philosophies of Husserl and Heidegger in Freiburg. He retained a lifelong interest in the subject of philosophy but eventually decided his real bent was in math ematics. He chose to complete his studies in Göttingen, the mecca of mathematics in the 1920s. There he met Richard Courant, director of the Institute of Mathematics, who admired enormously his talent but found him somewhat unworldly. In fact, Friedrichs's childhood asthma had prevented him from participating in the activities where children naturally socialize and he was painfully shy. Courant and Friedrichs enjoyed a lifelong friendship that included a great deal of mathematical stimulation, cooperation and interaction, a lot of practical advice from Courant, and a fair dose from Friedrichs of his logical approach to life and his special values. On the basis of my own observations for over twenty-five years, they dealt with each other's idiosyncrasies in a remarkably comfortable way.

Friedrichs stayed in Göttingen for five years. During this time he completed his first paper clarifying the logical significance of Einstein's general covariance postulate (1927,1) and then wrote his dissertation on boundary and eigen-value problems for elastic plates (1927,2). This was followed by a paper with Hans Lewy (1927,3) on initial value problems for linear hyperbolic partial differential equations.

Those first three papers demonstrate most of Friedrichs's lifetime in mathematics—the first on the fundamental laws of the nature of matter, the second on applied mathematics viewed through analysis, and the third on basic theorems of wave propagation.

The paper on hyperbolic partial differential equations led naturally to what turned out to be one of the best-known and most used results of mathematics of the time (1928,2). In investigating whether difference schemes for a time-varying partial differential equation like the wave or heat equation yield a good approximate solution, Courant, Friedrichs, and Lewy were led to consider the stability of the difference scheme. In a simple difference scheme every partial derivative like $\partial f/\partial x$ is replaced by a difference of the approximating function at two values of *x* divided by the difference of "step" in the values of *x*. The three authors made the remarkable discovery that the steps in time could not be chosen arbitrarily but had to be smaller than some constant times the steps in the space variable. For the wave equation that constant was the reciprocal of the speed of propagation. For other equations there may be many such speeds or one may have a different kind of propagation, but there is always a limitation of space step (Δx) and time step (Δt). But the basic idea comes from this paper, and the constants are all known as CFL numbers. There is scarcely a talk or a paper on modeling phenomena governed by so-called explicit difference schemes where this number does not come up.

Friedrichs was interested, however, in proving existence theorems for partial differential equations by letting the mesh size and time step become vanishing small. The modeling aspect was a side product that only became important after World War II when one could compute something useful on a large computer. In later life, when Friedrichs was pressed to say something about the important role of computer modeling in applied mathematics to which he had made such a fundamental contribution, he simply would not bite.

After Friedrichs completed his dissertation and two years of an assistantship, according to Constance Reid's fascinating obituary[1] for the *Intelligencer,* Courant advised his shy young friend that in the severe competition for positions at German universities Friedrichs would need some special advantage and therefore he should become an applied mathematician. Thus, Friedrichs followed Theodore v. Karman to Aachen where v. Karman had become the first professor of aeronautical engineering. That was an exciting time in aerodynamics in Germany. First, it was a new and eminently practical subject, not like the exotic theories of quantum mechanics and relativity. Second, the interdiction of the building of airplanes in Germany in accordance with the Versailles treaty made the understanding of aerodynamics from a theoretical point of view a matter of vital concern to the nation. Friedrichs returned to Göttingen two years later with a deep knowledge of aerodynamics, which was to serve him later in America.

But his mathematical interest had shifted to the modern theory of operators in Hilbert spaces. He even rewrote a paper (1934) couched in classical language so that it was in von Neumann's "new" abstract language. He solved several problems in spectral theory and used the new method to solve the initial value problem for hyperbolic equations with only energy integrals. This led some years later to the idea of weak solutions, a concept that is the backbone of the modern theory of linear and some nonlinear equations.

In 1931 Friedrichs was called to Braunschweig to the Technische Hochschule as a full professor, a rare recognition in prewar Germany for a man of thirty. But the Hitler era was about to begin. After five years of increasing political difficulties at Braunschweig, a visit to Courant who had emigrated to New York, and most important, after meeting Nellie Bruell, his future wife, Friedrichs saw clearly that he too would have to emigrate, which he did in 1937.

Once again under Courant's influence, Friedrichs started dancing on what he liked to call his "other foot"—namely, doing applied mathematics. On the whole, until the end of the Second World War his main contributions were in fluid dynamics with Courant and in elasticity with J. J. Stoker.

When I first met him in 1946, Friedrichs was celebrating the end of the years of war effort in applied mathematics by teaching an innovative course in topology. But he and

Courant were also finishing their basic and still used book on compressible fluid dynamics (1948,1). I was selected to edit it mainly for English and thus I came to know Friedrichs's meticulous, very careful, and correct, but at times slightly pedantic, approach to the subject. Since English was after all my native language, I had some trouble with Friedrichs's desire to find a rule (mostly from Fowler's) to follow. His English was remarkable, but he never quite became idiomatic. Courant enlivened the text considerably, sometimes to Friedrichs's disappointment at the expense of correctness.

By 1951 his spectral theory work, which he had renewed after 1945, led him into his fundamental work in the quantum theory of fields. He published five monographs, which inspired a number of today's mathematical physicists, especially in the circle of Olga Ladyzhenskaya and Ludwig Fadeev in Russia. All the time as the spirit moved him he would do a basic piece for applied mathematics or a big chunk of writing for the famous Courant-Hilbert[2] volume 2, completed in German in 1937 but essentially rewritten in English by many of the faculty of the burgeoning institute that was to become the Courant Institute in the early sixties.

By the 1940s, Friedrichs was recognized as one of the new leaders in American mathematics. He was elected to the National Academy of Sciences in 1959. He received many awards and honorary degrees culminating in the National Medal of Science in 1976, which he received "for bringing the powers of modern mathematics to bear on problems in physics, fluid dynamics and elasticity."

Despite all the recognition, Friedrichs's modesty could be overwhelming. When we discussed what should be republished in his *Selecta,* he kept reiterating about each of his discoveries that someone else had done it better later and why should someone want to read his less effective original presentation. I succeeded in most cases in overruling him.

He was never shy mathematically and was, by the 1950s, much less shy socially. He had a great impact on what Courant's fledgling institute became and thereby he greatly influenced applied mathematics in America both directly and indirectly.

I must mention one other important influence. While afternoon teas as a fundamental ingredient of the intellectual life of a mathematician had been conceived at the Institute for Advanced Study in Princeton, Friedrichs carried the idea to the surroundings of NYU with his imposing principles. When the secretaries balked at washing the dishes after colloquia and Anneli Lax and I refused to take over, Friedrichs persuaded Courant to hire someone to make tea and coffee and wash up every day of the week. Today the notion that this custom is conducive to producing mathematics has been taken over almost everywhere.

He was a prodigious worker. Nellie shielded him from unnecessary dealings with the outside world, even to providing breakfast on a tray for his most productive early morning labors. But his five children, Walter, Liska, David, Christopher, and Martin, were a source of enormous interest and pleasure, and he meticulously guarded the time he spent with them from being interrupted by some mathematical spasm.

When he died at the age of eighty-one, one of Friedrichs's wishes was that his last works that dealt with the true way to regard the uncertainty principle and other quantum mechanical concepts should be properly understood and seen correctly from a philosophical point of view.

PART II: WORK

If Friedrichs's contributions to analysis and applied math ematics had a central theme, it was partial differential equations (p.d.e.), and it is appropriate to start there. We shall concentrate on three fundamental subjects covered in three long papers dealing with regularity of solutions for elliptic systems (1953), existence and uniqueness for symmetric hyperbolic systems (1954), and symmetric positive systems (1958). All of these papers might be considered natural applications of the watershed result (1944), where the weak extension of a system of first-order differential operators with C^1 coefficients is shown to be the same as the strong extension. Mollifiers, of which hints were given earlier, emerge as the main tool, as they are in the theory of distributions.

The first major outcome was the regularity for elliptic systems using mollifiers as a tool. These regularity results are now standard theory. To mollify a not-so-smooth function you convolute it with a very smooth one concentrated in a neighborhood of a point. The convolution is close to the identity operator. Then one presses out the desired estimates by letting the neighborhood shrink. By this device Friedrichs avoided using the Lebesgue theory, where, as he liked to put it, you have to write almost everywhere, almost everywhere.

The second major outcome was for symmetric hyperbolic systems. Friedrichs's interest in this problem goes back much further, to his work with H. Lewy and to the paper described in Part I with Courant and Lewy. The basic idea of the work with Lewy (1932) was to base not only uniqueness but also existence of solutions to the wave equation on energy estimates for the solution and its higher derivations. This idea was taken up subsequently by Schauder, Petrovsky, and Sobolev. The two authors would have gone further, via difference equations, but were stumped by the need to prove that the expansion around the origin in three-space of

$$\frac{1}{(1-\beta)(1-\gamma)+(1-\alpha)(1-\gamma)+(1-\alpha)(1-\beta)}$$

has positive coefficients. An elegant proof was supplied by G. Szego, but the new paper had become so involved and technical that the effort was set aside. By the time Friedrichs returned to the linear problem in his paper "Symmetric Hyperbolic Differential Equations" (1954), he was able to devise a more direct difference scheme with positive coefficients. The problem has a long history at various levels of generality going back to Hadamard. It was of deep physical importance since almost all the key hyperbolic systems, those of electromagnetic theory, compressible flow, and magneto fluid dynamics can be reduced, as Friedrichs showed for the last two, to the symmetric case.

The key ingredients Friedrichs used are energy estimates, the projection theorem in Hilbert space, and mollifiers. Finite difference methods are used to establish differentiability. The result is the existence and uniqueness for all time of a solution to mixed initial boundary value problems for very wide classes of initial data and boundary conditions.

The third major result is on "Symmetric Positive Linear Differential Equations" (1958). The type of equation given in the title is special and is defined as follows. A first-order linear operator

$$K=\sum\alpha^{\rho}\frac{\partial}{\partial x^{\rho}}+\gamma,$$

α^{ρ} and γ square matrix valued functions, is called *symmetric positive* if $K + K^*$ is formally positive, where K^* is the formal adjoint of L:

$$K^{*} = -\sum \frac{\partial}{\partial x^{\rho}} \alpha^{\rho *} + \gamma^{*} .$$

Along with the operator *K* go special boundary conditions of positive type.

A remarkably wide variety of classical and nonclassical ones, such as various boundary value problems for a certain class of elliptic systems, the Cauchy problem for a certain class of hyperbolic equations, mixed initial boundary value problems for hyperbolic equations, and, last but not least, certain boundary value problems for equations of mixed type, such as Tricomi's equation, are symmetric positive. Friedrichs's main motivation was to treat systematically equations of mixed type and, as he often said, to establish a method of proof that was "deaf," as he put it, to changes of type and would at the same time yield the number and kinds of boundary conditions necessary for well-posed problems. He succeeded in "deaf" proofs only in some isolated cases but instead introduced a fundamental new approach to weak existence.

Pseudo-differential operators came into being near the end of Friedrichs's career. He grasped their importance immediately, especially for symmetrizing recalcitrant differential operators. He made many useful technical innovations, and he invented the name for the subject.

Two of the big applications of partial differential equations are in fluid dynamics and elasticity. Aside from the "bible" of shock wave theory written with Courant, Friedrichs made many contributions to fluid dynamics, several of them unpublished results from wartime work of the forties. These include work on flow through nozzles, over surfaces of revolution, in detonations, and deflagrations. From a mathematical point of view, his most important contribution to elasticity was in simplifying and clarifying the very long proof of Korn's inequality (1947). This has been recently reduced to two pages[3] by Olga Oleinik. Friedrichs showed how to solve the natural boundary value problems of elasticity. His work with J. J. Stoker (1941, 1942) on buckling problems broke new ground in a nonlinear problem that was of great importance to engineers. The methods were basically asymptotic boundary layer methods adapted from Prandtl's fluid boundary layer theory. An important key was matching two asymptotic expansions, a so-called inner and an outer expansion. This technique, which Friedrichs used rigorously, also entered the folklore of applied mathematics, often less rigorously. Friedrichs loved this work, particularly because it was the basis of a long-lasting cooperation with his friend Stoker.

Friedrichs's years in Göttingen coincided with the rising interest among mathematicians, notably Hilbert and von Neumann, to put the rapidly developing new physics on a logical basis. In his basic book on quantum mechanics von Neumann had identified the states of a quantum mechanical system with unit vectors in a Hilbert space and observables as self-adjoint operators. A self-adjoint operator, according to von Neumann and Marshall Stone, is an operator L defined on a domain $D(L)$, whose adjoint has the same domain as L and coincides with L there. To apply this definition, one needs an exact description of the domain of L. In practice this is a painfully pedantic process, straining the abilities of mathematicians and never accepted by physicists. Friedrichs was able to eliminate the need for such pedantry. He showed that if an operator is bounded from below—and almost all Schrödinger operators are so bounded—one can start with a crude skeleton of the operator, defined on a much smaller set, and then reconstruct the true operator from the skeleton by a process known ever since as the Friedrichs extension. Such important cases as the harmonic oscillator, the hydrogen atom, and the boson quantum field could be treated immediately. Other cases have followed.

In 1938 Friedrichs investigated in several crucial examples what happens under perturbation to the spectrum of an operator that has continuous or mixed continuous and discrete spectrum. This work was prompted by the work for discrete spectra of F. Rellich[4] but was applicable to quantum mechanical scattering. When Heisenberg and Møller introduced the scattering matrix and wave operators, respectively, in the forties, Friedrichs had already developed the tools for a time-independent approach in his early work. This work is described in English with some additions in "On the Perturbations of Continuous Space" (1948,1).

Friedrichs went on to write a series of books in quantum physics. In the first he gave precise definitions to many of the basic but somewhat confused notions of quantum field theories. Many mathematical physicists have used these books as starting points for their quantum field work. One of Friedrichs's last contributions to quantum mechanics was his paper "Unobserved Observables and Unobserved Causality" (1981). This paper is a contribution to one of the great controversies of twentieth-century physics, the debate between Niels Bohr and Albert Einstein over the completeness of quantum mechanics. Einstein felt that the laws of nature should be deterministic or causal, and for this reason he felt the quantum mechanical notion of the state of a particle was incomplete. Friedrichs introduced the "intrinsic state" of a particle and argued that for this intrinsic state causality is valid but, in accordance with quantum theory, not verifiable. It is yet to be seen whether this truly reconciles the two sides of the controversy.

There are still many other areas where Friedrichs made fundamental and deep contributions. The most notable of these was asymptotic theory, which grew out of his nonlinear elasticity work. In 1954 Friedrichs gave his Gibbs lecture, a semipopular expository talk in applied areas, at the American Mathematical Society on this subject. There he surveyed the role of discontinuities in many physical phenomena, pointing out how important they are to us as observers (see 1954 paper). Shock waves, boundary layers, shadows, edge effects, and Stokes phenomena in ordinary differential equations are all examples. They all come about because of a singular limiting process where some parameter that could be viscosity, frequency, the reciprocal of Hooke's constant, etc., goes to zero. Every one of these phenomena is, however, slightly different, and Friedrichs helped clarify and rigorize the process for many of them. Friedrichs ended the Gibbs lecture by showing how asymptotics enter the adiabatic theorem of quantum mechanics. It is another mark of his modesty that there is only one reference to himself in the bibliography, although he had made so many vital contributions to the subject.

Taken altogether, as one looks at Friedrichs's very extensive list of publications, one cannot help but be struck by the variety of ways and the number of times Friedrichs broke new ground and then, with the passage of only a few years, how many of his ideas were absorbed into modern analysis and applied mathematics and became standard, so that it is almost forgotten today that they were Friedrichs's.

As I write this, the spirit of Friedrichs hangs over me fussing over the details that are not quite right and reminding me that I have left out this and I should have put in that, but I remind him that it was he who said, "Open your own newly published work on any random page and you will find a mistake." For those who would like to read his works in detail, I refer to the *Selecta* published by Birkhauser

(1986) from which I have drawn heavily, in particular from David Isaccson's article about his role in physics.

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Herbert S. Gasser

Herbert Spencer Gasser

July 5, 1888-May 11, 1963

By Merrill W. Chase and Carlton C. Hunt

Herbert Gasser was a major scientific figure. An outstanding physiologist, he was a pioneer in the field of neurophysiology. In addition, as Director of the Rockefeller Institute for Medical Research from 1935 until 1953, he exercised an important national and international influence on science. This long-overdue memoir has been written almost thirty years after his death. We both knew him at the Rockefeller Institute and have had access to extensive archival material. Though our information about his early years is limited, Gasser's autobiography (1964), written characteristically with great reserve and in the third person, contains much of interest.

ORIGINS AND EARLY YEARS IN WISCONSIN

Herbert Gasser was born in 1888 in Platteville, a small town in southwestern Wisconsin. His father, Herman, was an immigrant from the Tyrol, who, after working as a pharmacist, studied medicine and became a practicing physician. His mother, Jane Elizabeth Griswold Gasser, came from a family of early Connecticut settlers. The given names of their first child, Herbert Spencer, stemmed from his father's perusal of books by Wallace, Darwin, and Spencer after local newspapers attacked the concept of evolution. There were two younger siblings, a sister and a brother.

Gasser recalled his youth in Platteville: ". . . excursions into the countryside, fishing in the summer and skating in winter. There were few distractions, no cinema or radio, and travel was by horse and buggy. Children had to find their own amusement." Reading was of great importance to him. He also built furniture, taking pleasure in benchwork. Fine craftsmanship remained a lifelong interest. Young Gasser had a simple box Kodak camera that he supplemented with much "improvised equipment." As he remembered: "This experience later turned out to be a good training for a physiologist."

In accordance with his father's wishes, Gasser studied at the state normal school in Platteville. A year was lost because of an unspecified but serious illness; he graduated in 1907. Gasser wished to continue studies in engineering, but his father preferred that he enter medical school. Gasser was not attracted to a country doctor's life. He wanted further education but felt medical training was too great a price to pay. At last, he and his father struck a compromise that he later considered a turning point in his life: "The university would be allowed if no specialization were to take place." Gasser attended the University of Wisconsin at Madison, receiving a bachelor's degree in 1910 and, in the next year, a master's degree. At Wisconsin he took some medical school courses. He wrote about himself: "That is how it came about that his introduction to physiology was through a lecture course given by Prof. Joseph Erlanger before the latter's departure to Washington University in St. Louis. Erlanger was a beautifully clear lecturer, but the subject matter he presented differed so widely from what was anticipated that it amounted to a revelation. Gasser listened to the lectures with bewilderment and felt he was getting only a feeble grasp of their content. He had no realization that Erlanger was aware of his attendance."

Gasser remained at the university as an assistant in biochemistry, meanwhile completing his preclinical subjects, and then was appointed an instructor in physiology. As a student at Wisconsin, Gasser was involved in several studies with Loevenhart, an imaginative chemist, on the effects of oxygen want and with Meek on the responses of the heart to exercise.

At the medical school in Wisconsin, there were young faculty who were enthusiastic about research and Gasser came to realize that "medical science could be considered a discipline in its own right. In this light medicine appeared in a form which Gasser found acceptable." Because Wisconsin had only a two-year school, Gasser had to complete his medical education at another school, and he wisely chose Johns Hopkins. It apparently required some persuasion to get his father's consent, and the costs were a considerable strain.

JOHNS HOPKINS

The Johns Hopkins medical curriculum provided for elective time; Gasser utilized this for study in physiology. Professor William Howell suggested that he work on mechanisms of blood clotting, studies that he did not complete until after leaving Baltimore. Howell confirmed Gasser's results. Gasser was pleased that this independent work had been successful, but it was not a field of long-term interest to him.

When Gasser received his M.D. in 1915, Howell offered him a position in physiology, but Gasser did not feel free to accept it and returned to Wisconsin where Loevenhart appointed him as an instructor in pharmacology. As he wrote: "The salary was higher and the location was adapted to helping in the support of a younger sister and brother at the University of Wisconsin."

ST. LOUIS (1916-31)

A year later Gasser had an opportunity to return to physiology. Joseph Erlanger invited him to take a position in his new department at the recently reorganized Washington University School of Medicine in St. Louis. In the spring of 1917 the United States entered the First World War. Gasser worked with Erlanger on problems related to the war effort, investigating wound shock and publishing eight papers on this topic. In the summer of 1918 he joined Loevenhart at a chemical warfare station at American University in Washington, D.C., and worked on Lewisite. At the war's end he returned to St. Louis, in December 1918. At this time he had no clear research direction but had some interest in the nervous system. There were discussions in the department about the problem of recording nerve activity, and Gasser was aware of the need for a new and innovative technical approach.

A classmate from Johns Hopkins, H. S. Newcomer, who had a background in physics, had constructed a three-stage amplifier using thermionic valves. He visited St. Louis, where he and Gasser tried using this amplifier in conjunction with a string galvanometer to record impulses in the phrenic nerve. While the amplification was sufficient to detect the nerve action potentials with the string galvanometer, the latter was too slow to record the fast potential changes with fidelity.

A recording device with sufficient speed did exist, the cathode ray or Braun tube, in which a beam of electrons was deflected by potentials applied to pairs of vertical or horizontal plates. Bernstein had mentioned its possible use for the recording of rapid nerve potential changes in his book on "Electrobiologie" in 1912. But there was a problem of sensitivity. As Gasser wrote: "In the old Braun tubes, cathode beams were composed of high velocity electrons wrenched from cold cathodes by high voltages. There was still a large gap between the sizes of the potentials needed to deflect them and those which could be produced through the augmentation of a nerve action potential with the aid of the new amplification. Then there occurred an event crucial in its significance."

This crucial event occurred in 1920, when both the Physical Society and the American Physiological Society happened to meet at the same time in Chicago. Professor Horatio Williams, of Columbia, told Gasser there would be a paper read at the Physical Society meeting that would interest him. J. B. Johnson of Western Electric Laboratories was describing a modification of the Braun tube with a heated cathode, allowing the tube to be operated at lower voltages and thus increasing its sensitivity. Newcomer's amplifier had enough gain for the modified Braun tube to record nerve potentials. It was following this development that Erlanger joined Gasser in the studies on nerve potentials. Their first attempt to acquire a cathode ray tube met with resistance from Western Electric, which refused to sell them one. Undaunted, they made a tube by coating a phosphor inside an Erlenmeyer flask. Western Electric subsequently relented and sold them a tube.

To provide for triggering of sweeps of the cathode ray oscillograph and for stimuli to be delivered to the nerve, much ancillary apparatus had to be constructed. This was done largely with mechanical devices. Condenser discharges were applied to the plates deflecting the beam horizontally for the time axis, while vertical deflection was produced by the amplified nerve signal. Records were obtained by holding film on the tube face in the dark room and repeating the sweeps until a sufficient exposure was obtained. To minimize vibration, the apparatus was mounted in a basement laboratory on a foundation separate from that of the building. Nonetheless, passing streetcars often disturbed the delicate recordings. The images were blurred, and careful correction had to be made for the nonlinearities in horizontal and vertical deflections. In spite of technical difficulties, in shortly over a year Erlanger and Gasser were ready to publish a first paper on the nerve potentials recorded with the oscillograph. Writing some forty years later, Gasser recalled: "The most difficult step in opening up a new field had been taken. Ever afterward, Gasser never had any doubt about the direction he would follow."

It was evident from these earliest studies that the action potential recorded from the frog sciatic nerve was compound in nature (i.e., it resulted from the summation of potential changes in many individual axons). But the characteristics of the action potentials in the individual axons were not known. Much remained to be done.

In 1921 Gasser was made head of the Department of Pharmacology at Washington University, a position he held until 1931. The pharmacology position was accepted with some misgivings and with the stipulation that he would be free to follow his own line of research. Gasser's interest in pharmacology probably stemmed from the influence of Loevenhart. Later, Gasser described taking the chair at Washington University as his "final excursion into Pharmacology." Already, Gasser had come to view himself primarily as a physiologist.

Shortly after Gasser became professor of pharmacology, Abraham Flexner visited the Executive Faculty and, referring to his boyish appearance, asked: "What have you been doing making freshmen Professors?" Soon thereafter Flexner indicated that the General Education Board of the Rockefeller

Foundation would support an extended stay in Europe, for him to gain further experience in pharmacology and to better his knowledge of foreign languages. Gasser had already planned to attend the International Physiological Congress in Edinburgh; there he delivered a paper on the new electrophysiological technique and met with European physiologists. Gasser was soon elected to membership in the Physiological Society of Great Britain (1924).

Exploitation of the new oscillographic technique had barely begun when Gasser began his sojourn abroad, which was extended to a second year. He first worked on muscle with A. V. Hill at University College, London. They published a long paper on the dynamics of muscular contraction; and, in another paper with Hartree, Gasser found that the mechanical and thermal responses of muscle were inseparable. He then went to Straub's laboratory in Munich and, following that, to Louis Lapique in Paris.

Lapique was well-known for his work on nerve and muscle excitation and for his ideas about "chronaxie," an indirect measure of the speed of voltage change of the tissue to an applied current pulse. When Gasser showed his records of compound action potentials to Lapique, "he suggested forthwith that the velocity differences might be associated with the sizes of the fibers." His visit with Lapique was the only one during the European trip that had direct relevance to Gasser's long-range interests and led to a special publication.

Gasser next went to Henry Dale's laboratory. They studied the development of increased sensitivity of muscle to nicotine and acetylcholine following denervation. Doing no further work on muscle, he later wrote an important review on the subject of muscle contracture (1930).

Back in St. Louis, Gasser resumed studies on peripheral nerve with Erlanger. George Bishop had joined in the ef fort and brought to the group a considerable array of skills, both practical and theoretical. His knowledge of physics was extensive, and he was very helpful in designing the recording conditions so as to avoid artifacts. In 1924 a paper titled "The Compound Nature of the Action Current in Nerve as Disclosed by the Cathode Ray Oscillograph" was authored by Erlanger and Gasser "with the collaboration in some of the experiments of George H. Bishop." This important paper was a milestone in the development of knowledge about the physiology of peripheral nerve. No less than eight further papers carried Bishop as a joint author.

The excised sciatic nerve of the bullfrog was the principal experimental subject, mounted in a closed humidified chamber on an array of electrodes for stimulation and recording. One recording electrode was placed at the cut end of the nerve, the other some distance centrally, and the potential difference between the two was recorded. With such extracellular recording, impulses arriving at the electrode located on the intact nerve produced a negative deflection.

To provide a quantitative understanding of the action potential recorded from the multiaxonal nerve at different distances from the site of stimulation, it was necessary to know the number and sizes of the axons in the nerve, the form of the action potential in the individual axon, the speed of conduction of the action potential in different axons and its relation to axonal diameter. It would later be found that after-potentials also had to be taken into account.

Gasser was particularly interested in reconstructing the "compound" action potential by summing the predicted potential changes in all the individual axons of the bullfrog nerve, at a particular conduction distance, and comparing this with the recorded action potential. This approach proved to be very useful and one that Gasser used for his entire research career. From cross sections of the nerve, the numbers of axons (nerve fibers) of various sizes were counted, the action potential of each size group was approximated by a triangular waveform of depolarization, the group of axons of a given size being assumed to have a constant conduction velocity that was proportional to their diameter. Only the myelinated axons of bullfrog peripheral nerve, the so-called A fibers, were studied in their early work. When recording near the cathode, where all the axons were stimulated at the same instant, the action potential had a simple form, the axon potentials in all the axons being nearly synchronous. But with increasing conduction distance the difference in the time of arrival of the action potentials in the fastest and slowest axons increased progressively. The successive elevations in the potential record were produced by axons conducting progressively more slowly; the deflections were called alpha, beta, gamma, and delta. The best fit between the actual compound action potential and the reconstructed plot was obtained when the action potential in the individual axon was given a duration of 1 millisecond, the amplitude of the externally recorded action potential in each axon was made proportional to axonal size, and conduction velocity was related directly to axonal diameter.

Other important questions were pursued. For example, was conduction in the various fibers independent? As the strength of a stimulus was increased, first the largest and then the smaller axons were excited. If a weak shock that excited only the largest (A alpha) axons was given and then, while they were refractory, a second stronger shock was delivered, the latter evoked no alpha wave but only later deflections. This indicated that action potentials in the larger and smaller fibers were conducted independently. Subsequently, it was possible to record the action potential in an individual axon and to measure directly its time course. Studies were also made on the absolute and relative refractory periods following impulse activity.

The first observation of the C fiber action potential was made by Peter Heinbecker, a research fellow with George Bishop. Applying exceedingly strong shocks to the cervical sympathetic nerve of the turtle, he saw small, very late potential elevations that conducted very slowly. These were the responses of unmyelinated or C fibers. Erlanger and Gasser explored the details of the C fiber potentials, publishing a detailed paper in 1930. Decades later Gasser was to return to this problem. The threshold of C fibers to electrical stimulation was nearly 100 times that of A fibers. C fiber action potentials were longer in duration than those in A fibers; the proportionality between conduction velocity and diameter was also different, although the relationship appeared to be linear.

Myelinated preganglionic sympathetic fibers were designated B fibers. Their properties differed from A fibers in duration of action potential, relation between fiber diameter and conduction velocity, and in after-potentials.

The order in which nerve fibers were blocked by cocaine was found to be from smallest to largest. Conversely, pressure blocked the largest fibers first, the smaller fibers later. This was useful in relating function to the various fiber groups, a topic of lasting interest to both Gasser and to Bishop.

Toward the end of his St. Louis tenure, Gasser, together with Erlanger, became interested in the potential changes that follow the brief nerve impulse, the after-potentials, and their effect on excitability. After the nerve impulse or "spike," the potential only gradually returned to the baseline level; the potential during this period being negative to the baseline, it was called the negative after-potential. During the nega tive after-potential, a smaller than normal stimulus was needed to excite the nerve; it was a period of supernormal excitability. There then followed a much slower change in which the potential became positive to the baseline, a positive after-potential. During this period the nerve was subnormal in its excitability. The after-potentials showed characteristic differences in A, B, and C fibers. Also, the size of the after-potentials depended on the amount of preceding activity. The excitability changes produced by the relatively long lasting positive after-potentials in peripheral nerve fibers and their augmentation by preceding repetitive activity were of interest because they might help explain certain long-lasting excitability changes in the central nervous system.

With the widespread interest in studies of electrical conduction, group discussions took place at the time of the annual meetings of the American Physiological Society. In 1930 Ralph Gerard invited a group of ten, representing seven different institutions, to meet the day before the scheduled program. They adopted the name "axonologists," a term proposed by Alexander Forbes of Harvard. Three members were from St. Louis: Erlanger, Gasser, and F. O. Schmitt. The size of the group expanded over successive annual meetings until the council of the Physiological Society protested that its regular program was being diminished; the axonologists disbanded.

In 1926 Gasser made an interesting appointment to the Department of Pharmacology, that of Helen Tredway Graham. The wife of Evarts Graham, then head of surgery at Washington University, she had been an outstanding student at Bryn Mawr and later received a doctorate from the University of Chicago. At this time, prejudice against women in science was strong. Even a woman as gifted and privileged as Graham had difficulty finding a suitable position. Though Graham was a neurophysiologist, it was Gasser, rather than Erlanger (then head of physiology), who gave her a position. In subsequent publications Gasser referred to her simply as H. T. Graham. They collaborated on five articles, two dealing with pharmacology. Graham later did important independent studies on nerve function and went on to very original work on histamine. Her description of Gasser as she remembered him during their collaboration reveals his enjoyment of lively interchange:

> To Dr. Gasser an integral aspect of research is discussion and, in those days when he had the right partner, discussion seemed never to weary him. Many were the hours spent over endless cups of coffee in the physiology seminar room, in the cafeteria (then run on a leisurely schedule in the medical school without a closing hour for lunch), in friends' houses or in neighboring or downtown restaurants; and many were the diagrams drawn on odd envelopes, on cafeteria checks, on paper napkins, or even on restaurant tablecloths. His indifference to time and his ability to make his colleagues ignore it not infrequently prolonged the Monday afternoon physiological seminars to an hour that tried the patience of the colleagues' families waiting at home for dinner. But there was a limit even to Gasser's zeal for discussion: during an era when lactic acid was regarded as the key to muscular contraction he announced privately, if not publicly, that he was too fed up with lactic acid to attend one more session of a certain group in the school given to a discussion of its metabolism.

Helen Graham and Gasser remained in contact for many years thereafter, and she continued to seek his advice even after he became Director of the Rockefeller Institute. Clearly, she regarded him as her mentor, and their friendship was enduring.

CORNELL UNIVERSITY MEDICAL COLLEGE (1931-35)

In 1932 new structures going up on East 68th Street in Manhattan united the previously separate Cornell Medical College and the New York Hospital. In that same year the professor of physiology was to retire and Gasser was approached to replace him. He accepted the position in 1931 with one free year to formulate his plans. During this time, he prepared the first student manual for experiments using the oscillograph, to eliminate the old spring-wound kymographs, and ran through the experiments to check them out.

In 1934, the year of his election to the National Academy of Sciences, the final meeting of the axonologists occurred in New York City under Gasser's leadership. At that meeting, much speculation was given to the topic of synaptic transmission—whether it was chemical or electrical.

When Gasser moved to Cornell, the direction of his work changed slightly. With Grundfest he investigated mammalian peripheral nerve fibers and found important quantitative differences between amphibian and mammalian nerve. With Hughes he studied potentials within the spinal cord, work that he had initiated with Helen Graham in St. Louis.

In 1937 Gasser gave a Harvey Lecture on "The Control of Excitation in the Nervous System." In this talk he considered possible mechanisms of excitation and inhibition that operate within the central nervous system and the possible relevance to this of information gained by the study of peripheral nerve. At that time, little was known about the underlying processes of activity in the central nervous system. His insights seem remarkably clear and imaginative. Some of these ideas found expression in the later studies of Lorente de Nó, Renshaw, and Lloyd at the Rockefeller Institute.

Gasser attempted to relate the phenomenon of inhibition in the central nervous system to some properties of peripheral nerve—namely, the subnormality that followed activity. He devised a scheme to explain reciprocal inhibition on this basis, although it was later found that inhibition has a quite different mechanism. Lloyd's studies (1941) showed clearly that inhibition could occur without prior excitation.

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

After only three years at Cornell, the Rockefeller Institute for Medical Research invited Gasser to become its new Director. Charles Stockard, professor of anatomy at Cornell, and a member of the Board of Scientific Directors of the Rockefeller Institute, had suggested his name. The offer surprised Gasser, and he doubted his administrative abilities. But he was attracted ". . . by the argument that the most important function of a director of a research institute was maintenance of assurance of complete freedom to the investigators."

In 1935 Gasser did accept the Directorship, although with some reluctance. It would offer him funds for developing superior recording equipment and freedom from teaching. He was assured that experienced and competent support staff would make his administrative duties light. Clearly, the Rockefellers, concerned about the effect of the continued economic depression on the institute's endowment, needed a director who would be a wise steward of funds. The institute did provide Gasser, a bachelor, with an apartment in the east 60s sufficiently large for entertainment.

Before taking his position, Gasser went to Russia where he gave a paper at the Georgian Academy of Sciences. During this trip, he located a master instrument designer, J. F. Toennies, who had lost his position through the political upheavals in Germany. Toennies came to Gasser's staff in the physiology division for a three-year term.

Gasser at once designed his laboratories for neurophysiological studies on the unoccupied first floor of the "new North" building, then only five years old. Since its opening, the Institute had utilized d.c. current generated in its own power plant. Gasser had alternating current, necessary for more modern equipment, brought to this area. By 1936 the new laboratories and an instrument shop with heavy equipment were in operation, with experienced toolmakers from European apprenticeships. Neurophysiological equipment then had to be constructed locally from purchased parts, to be installed in freestanding upright racks. Toennies' new design for amplifiers, with better frequency response and rejection of common mode noise, greatly improved the accuracy of recording.

The assurances that Flexner's established system for running the Institute would give Gasser free time for his own laboratory work were fulfilled fairly well for six years. There was an array of very competent people in charge of support services. During his first six years there, Gasser published eleven papers, six under his name alone.

In 1935 Erlanger and Gasser were invited to give a series of lectures at the Johnson Foundation in Philadelphia. These were published in a volume titled *Electrical Signs of Nervous Activity* in 1936. By this time their views had diverged, as they openly acknowledged in their joint preface:

> Two summers ago, as we sat on a ledge high up in the Rocky Mountains, resting from our walk and viewing the panorama of lofty peaks spread out before us, our conversation turned to problems of nerve physiology. We were on holiday together, one of us had just arrived and much had gone on in our widely separated laboratories which we had not had an opportunity to discuss. After a while during a pause in the conversation one of us said, "Shall we ever be able to collaborate in a set of lectures when there are so many points which we interpret differently?"

The problem was resolved by their each giving an independent series of lectures.

Gasser soon established the Institute as a major center for research in neurophysiology. He brought a group of investigators there, including Harry Grundfest, Raphael

Lorente de Nó, Birdsey Renshaw, and David P. C. Lloyd. A number of visitors also worked there for various periods of time, among them Alan Hodgkin (1937-38), who carried out his classical experiments on the role of local current circuits in the propagation of the nerve impulse.

Gasser's own laboratory work represented a continuation of his research at Cornell. In 1938 he published papers on recruitment of nerve fibers and on properties of mammalian nerve fibers of slowest conduction with Grundfest and Richards and in 1939 an article on axons as samples of nervous tissue.

Starting with the tense prewar days of 1941, Gasser's administrative responsibilities made it progressively more difficult for him to devote time to research. In the ensuing thirteen years, Gasser published six articles, his colleagues sixty-five. In 1942 the neurophysiological laboratories were closed, Lloyd moved to Yale University for two years, and Gasser devoted his energies toward the war effort. Under Gasser's direction, the Institute had a number of contracts from the Office of Scientific Research and Development, chiefly for work on nitrogen mustard agents in which Gasser himself was involved.

In October 1944 a cablegram from Stockholm announced that Erlanger and Gasser had won the Nobel Prize for their work on nerve function. Gasser learned of this when he was in the office of one of the Institute's trustees. He wrote of himself: "Dismay rather than elation was his immediate reaction. So estranged from his thinking had become the physiology of nerve fibers, that at the end of the conference he went into retreat in order to regain touch with the state of the subject through reading his own reprints." This comment reflects his exceptional honesty and lack of pretense. The award was presented in New York because of the war; the Stockholm formalities took place on December 10, 1945, when peace had returned.

GASSER AS DIRECTOR OF THE ROCKEFELLER INSTITUTE

In taking up the reins from Simon Flexner, Gasser devoted much time to studying the staff and the ongoing research, visiting each laboratory in turn. So effective did he find this method that he arranged for staff to visit laboratories in place of certain Friday staff meetings. Gasser served on both the Board of Scientific Advisors and the Board of Trustees (known collectively as the "Corporation"), which determined policies.

Each laboratory submitted an annual report for the Corporation, which was carefully reviewed by Gasser. He was also concerned with many other details of the Institute's operation, dealing with appointments and promotions as well as the retirement of a number of members who approached their sixty-fifth year. Some of them chose to remain as working scientists with small staffs (Michaelis, Osterhout, and Landsteiner), Avery keeping an office, while Rufus Cole, Florence Sabin, and Alexis Carrel left.

Under Gasser there was a change from Simon Flexner's policy of abolishing a laboratory and disbanding its staff when a member left or died. In contrast, when a member departed, Gasser judged everyone in that laboratory individually and retained those whom he judged would offer productive years in research.

Expenditures were held down from 1935 through the period of the Second World War. By then the aging laboratories were in need of refurbishing. Gasser designed standardized laboratory furniture, beautiful oak cabinetry of modular dimensions. He made a thorough upgrading of the facilities, again relying on the Institute's "shops" to re build, rewire, and replumb those laboratories. Also, a southern extension of the Rockefeller Hospital was built.

Through pressure from the Trustees, the Princeton branch of the Institute, which had contained the Department of Animal Pathology since 1913 and the Department of Plant Pathology since 1931, was to be discontinued; the task fell to Gasser. New quarters were made for those who wished to transfer their activities to New York, and a number of greenhouses were built.

Central to Gasser's Directorship was the maintenance of the individual investigator's independence to study the problems of his or her choice. This had been a key factor in attracting Gasser himself to the Institute. Some of his remarks indicate how strongly he was committed to this policy:

> The product of the Rockefeller Institute is new knowledge . . . . It cannot be forecast and it can not be achieved through administrative direction. All that can be done is to create optimal conditions for its production.
> The opportunity which the Institute has above all else is to concentrate on the production of scientific capital . . . . The production cannot be planned. No one knows how. But the conditions for it can be maintained, as they are now and always have been. That means fostering individuals and allowing them freedom.
> . . . grants are made in the interest of defined projects and for a limited period. . . . In order to receive aid an individual must outline a project. At the onset, he is in effect being asked to make a prediction. . . . Projects, by definition, are not consonant with free inquiry.

While he was Director, Gasser refused to accept federal funds to compensate the institute for laboratory expenses of postdoctoral students. He feared this might compromise the Institute's independence. This policy was abandoned at once after he retired.

RETIREMENT

The war's hiatus and the weight of administrative obligations kept Gasser from active laboratory work until his re tirement as Director. On returning to the bench, he took up the same problems he had left earlier because he felt that this was the area where he could best contribute. Concerned that his research might not be optimal for training a young scientist, Gasser decided to work without a collaborator.

The major aim of his research after retirement was to continue exploring the structure and function of unmyelinated axons in peripheral nerve. When he studied this matter earlier, accurate measurement of axon diameters in unmyelinated axons had not been possible because they were so small; silver staining of such axons allowed them to be visualized but rendered measurement of their diameters inaccurate. Fortunately, new techniques of electron microscopy were being developed at the Institute as Gasser was approaching retirement.

The Princeton branch saw the usefulness of the electron microscope when Wendell Stanley went to the RCA laboratories in Camden, New Jersey, and shortly one was acquired by the Princeton branch for examining viruses. In 1947 the International Health Division of the Rockefeller Foundation, located in the Institute, purchased an electron microscope, and it became available to Porter.

Keith Porter and George Palade were working on problems of fixation, embedding and sectioning of tissues for study under the electron microscope. If this could be done satisfactorily, accurate measurements of axon diameters would be possible. Thomas Rivers's opinion that electron microscopy would not be useful for examining tissues may have made Gasser hesitant, but he came to believe otherwise. With his delayed decision, the Institute itself purchased an electron microscope in 1948. Palade and Porter were able to obtain sufficiently thin sections of tissues embedded in methacrylate, using a microtome that Porter and Blum de veloped. These early electron microscopic studies of cellular fine structure at the Institute, of great importance to the development of cell biology, provided a way for Gasser to study the morphology of the small unmyelinated axons.

The electron microscopic studies of nerve, initiated by Gasser with the help of Porter and Palade, permitted the size of unmyelinated axons to be measured accurately and also showed their relationship to the Schwann cells in which they are embedded. The outer membrane of the Schwann cell is infolded to enclose an unmyelinated axon, an arrangement Gasser called a mesaxon by analogy with the mesentery. The unmyelinated axon was thus surrounded by a thin layer of extracellular space, although lying within the Schwann cell. Since a number of axons could be contained within one Schwann cell, Gasser was interested in finding whether interactions might occur between such axons. He made three-dimensional reconstructions to determine over what length one axon might lie close to another. This turned out to be quite limited, suggesting that interactions from this cause were not likely to be important.

Unmyelinated axons in peripheral nerve may be sensory, their cell bodies lying in the dorsal root ganglion, or efferent, postganglionic sympathetic fibers en route to the periphery. Gasser found that these two types of C fibers showed differences in their physiological properties. In comparing the unmyelinated axons of dorsal root ganglion cells central to and distal to the ganglion, Gasser found that their diameters diminished by about an order of magnitude central to their cell bodies as compared to the periphery.

Reconstruction of the compound action potential of unmyelinated (C) axons from the diameter distribution of the axons was impressively successful, based on a linear relationship between axon diameter and conduction velocity and on appropriate contributions of after-potentials to the recorded action potentials.

With the higher resolution of the electron microscope, Gasser examined olfactory nerve fibers in mammals and in the pike. The unmyelinated fibers were found to be extremely small. The length of the olfactory nerve in the pike permitted recording from these nerve fibers. Although afferent, they showed action potentials that differed from those of mammalian dorsal root C axons, showing a simple form consonant with all of the axons having nearly the same diameter.

Gasser also sought an answer to a problem in the recording of the compound action potential, which had long bothered him. The problem arose from the fact that the potential was recorded by two electrodes, one at the end of the nerve and the other some distance centrally. Even if the nerve end was crushed and treated with a local anesthetic, impulses in some fibers propagated into the stretch between the recording electrodes, creating an artifact. The solution was to integrate the response recorded by a pair of electrodes located quite close together on the nerve, a procedure that he called a tangent lead. The results showed that some of the previously described elevations were artifactual; in the compound action potential produced by myelinated fibers of mammalian cutaneous nerves there were only two peaks, the alpha and delta. This finding was consonant with the size distribution of myelinated axons in these nerves. The paper describing these studies was his last.

Working alone or with one or two collaborators, Gasser was a very focused investigator, intensely concentrated on his subject. Most of his research was, in fact, within a circumscribed although important area. An approach he utilized throughout his career was reconstruction of the com pound action potential from the calculated sum of the activities of the unitary axons. He returned to this approach, first used in St. Louis, after his retirement as Director. As Lloyd later wrote of Gasser's style of selecting problems: ". . . Gasser espoused the principle that there are two times for working on a problem—before anyone has thought of it and after everyone else has left it. As a result, Gasser was always the innovator or the finalist."

GASSER IN PERSON

As Director of the Rockefeller Institute, Gasser was a striking figure. Tall, elegant, and graced for many years with an extraordinarily youthful appearance, his formidable intellect made him impatient with trivial conversation. While he was clearly a fastidious man of great integrity, those privileged to know him realized that he was also a warm, engaging person who treated friends with much kindness, loyalty, and concern. He had a keen sense of humor, enjoyed puns, and could be very good company. Fine art, classical music, and good food were among his pleasures. He read widely and his knowledge on many topics was profound. His high-pitched voice reflected a hormonal deficiency, but a strong personality and rigorous intellect made this unimportant. Not a facile speaker, Gasser wrote beautifully, with clarity and grace.

An example of Gasser's uncompromising standards can be seen in his response to a letter from Erlanger in 1938, requesting information to be transmitted to the Nobel committee. Their colleague, Dr. Evarts Graham, had nominated them. Gasser wrote Erlanger:

> It must be well known in Stockholm that nominations for the Nobel Prize coming to hand with full information must be made with the cooperation of the nominee. One is thus forced into the position of appearing, at least in some measure, to nominate oneself. . . . I am greatly pleased that Evarts

> should value our work highly enough to place it in nomination, and I am grateful to him for the proposal, but my considered opinion about the effect of our becoming a party to the proposal impels me into not consenting to do so.

While such an attitude may seem quaint today, it reflected Gasser's deeply rooted antipathy to any kind of self-promotion.

Gasser's laboratory days ended when he suffered a cerebral accident on April 17, 1961, at the age of seventy-three, eight years after resigning as Director. Thereafter he resided in the New York Hospital. He made a partial recovery from his stroke but died from a respiratory infection in the hospital on May 11, 1963.

The fact that this memoir is written almost thirty years after his death permits some perspective on Gasser's view that independence of scientific inquiry was essential. By the time he retired in 1953, grant support had expanded considerably, and Gasser's reservations seemed old-fashioned to many. There was then an abundance of governmental money, and research of quality found support with proposals that were liberally and flexibly reviewed. Gasser had admitted that under a grant system "there is no gain-saying that accomplishments of the highest type can come out of it in spite of its shortcomings." Now, with the stringency of funding, an applicant for federal grant support must not only predict the results to be obtained but must also demonstrate that the proposed experiments will yield the anticipated results. Thus, the investigator's independence is even more compromised than Gasser once feared. From the vantage point of the 1990s, his concerns now appear prescient.

Valued sources are Gasser's autobiography, published posthumously with an introduction by Joseph C. Hinsey in *Experimental*

Neurology, 10 (Suppl. 1, 1964):1-38 (our quotations from Gasser come from this), also David P. C. Lloyd's obituary of Dr. Gasser in volume 5 of the *Dictionary of Scientific Biography,* pp. 290, 291 (New York: Charles Scribner's Sons, 1972) and Lord Adrian's article in the *Biographical Memoir of Fellows of the Royal Society,* 10(1964) :75-82. The history of the axonologists comes from F. O. Schmitt to MWC and is recounted in volume 2 of *Advances in American Medicine: Essays at the Bicentennial* in the article "The Neurosciences" by Robert J. Frank, Louise H. Marshall, and H. J. Magoun, pp. 552-616 (New York: Josiah Macy, Jr., Foundation, 1976). The reference to the status of women in science, as related to H. T. Graham, comes from M. W. Rossiter's *Women Scientists in America* (Baltimore: Johns Hopkins University Press, 1984). The homemade cathode ray tube and its trundling to the XIII International Congress of Physiology in Boston, 1929, is described in Hallowell Davis' memoir of Joseph Erlanger in *Biographical Memoirs,* vol. 41, pp. 111-39 (Washington, D.C.: National Academy of Sciences, 1976). It is to be noted that the 1936 book by Erlanger and Gasser was reprinted thirty-one years later by the trustees of the University of Pennsylvania, along with Gasser's bibliography and a foreword by David P. C. Lloyd. Gasser's picture was taken at age forty-seven, shortly after he became Director of the Rockefeller Institute for Medical Research.

HONORS AND DISTINCTIONS

AWARDS AND MEMBERSHIPS

1924	Physiological Society (British), Ordinary Member
1934	National Academy of Sciences
1935-53	Director, The Rockefeller Institute for Medical Research
1936	Sociedad Argentina de Biologia, Corresponding Member
1937	American Philosophical Society
1942	Asociacion Medica Argentina, Honorary Member
1943	Asociacion Medica Argentina de Buenos Aires, Honorary Member Royal Society of Edinburgh, Honorary Fellow
1944-45	Nobel Prize in Physiology and Medicine (with J. Erlanger)
1946	Royal Society of London, Foreign Member Royal Swedish Academy of Sciences, Foreign Member
1947	Finnish Academy of Sciences, Foreign Member Académie Royale de Médicine de Belgique, Corresponding Member American Society of Electroencephalography, Honorary Member
1948	American Academy of Arts and Sciences Accademia della Scienze dell' Istituto di Bologna, Corresponding Member
1949	Physiological Society (British), Honorary Member
1953	Société Philomathique de Paris, Corresponding Member
1954	Kober Medal, Association of American Physicians
1958	Société de Biologie, Collège de France, Associate Member

HONORARY DEGREES

1936	Sc.D., University of Pennsylvania
1940	Sc.D., University of Rochester LL.D., Washington University
1941	Sc.D., University of Wisconsin
1945	Sc.D., Columbia University
1947	LL.D., Johns Hopkins University

	Sc.D., Oxford University
1948	Sc.D., Harvard University
1949	Doctor honoris causa, Université Libre de Bruxelles Doctor of Medicine, Honorary, Université Catholique de Louvain
1953	Honorique de docteur, Université de Paris
1959	Sc.D., The Rockefeller University

PROFESSIONAL ORGANIZATIONS

American Physiological Society

American Association for the Advancement of Science, Fellow

Society for Experimental Biology and Medicine, President, 1937-39

Harvey Society, President 1940-42

Association for Research in Nervous and Mental Diseases

American Society of Pharmacology and Experimental Therapeutics

Association of American Physicians

History of Science Society

New York State Society for Medical Research

American Neurological Association (Associate Member)

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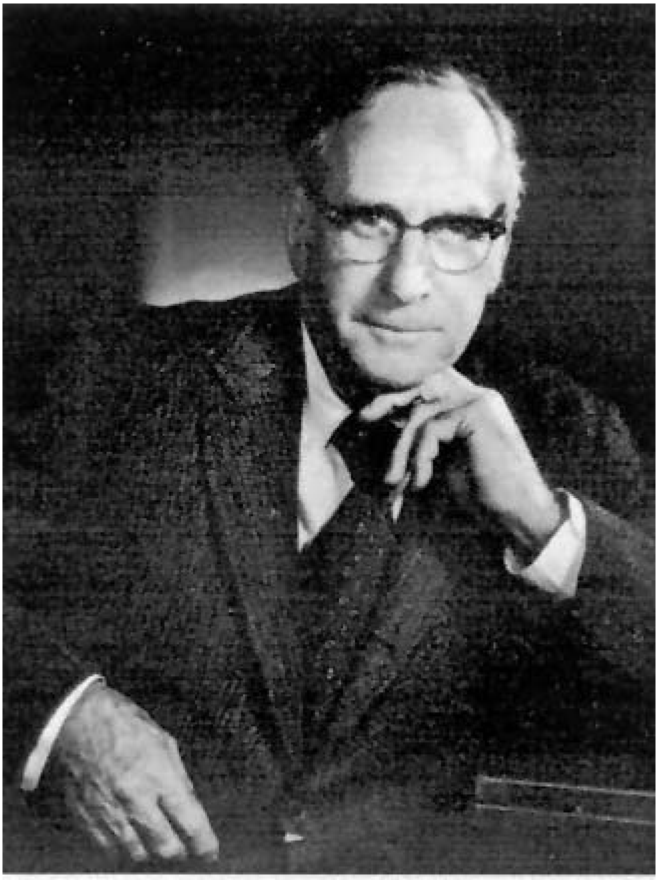

David R. Goddard

David Rockwell Goddard

January 3, 1908-July 9, 1985

By Ralph O. Erickson

David R. Goddard had an eminent career as a plant physiologist, as an educator, and as a university administrator. His advice was frequently sought in matters of national importance.

FAMILY BACKGROUND AND EARLY EDUCATION

Goddard was able to trace his male ancestry in America back six generations to William Goddard (1628-91), a merchant who left London with his wife and children after they had lost all their possessions in the great fire of 1666 and settled in Watertown, Massachusetts. In England another five generations can be traced. Goddard's mother's family, the Rockwells, also trace their descent from an ancestor who settled in present New York state before the American Revolution. He is related in some way to Robert H. Goddard ("rocket Goddard"), the pioneer investigator of rocket propulsion; to Maurice Goddard, who as a long-term Pennsylvania state official was a leader in environmental matters (Department of Environmental Resources); and to H. H. Goddard, the early and perhaps misguided advocate of the Binet test of intelligence, whom Dave sometimes jokingly referred to as the "feebleminded Goddard."

Goddard was born in 1908 in Carmel, California, the fifth of six children of Pliny E. Goddard. Since Goddard believed his father to have been the strongest influence in his life, something should be said of him. Born in Lewiston, Maine, in 1868, Pliny Goddard attended school in Maine and Poughkeepsie, New York. He earned his B.A. and M.A. degrees at Earlham College. After teaching school in Indiana and Kansas, Pliny married Alice Rockwell, a fellow teacher, and was sent by the Society of Friends as a lay missionary to the Indians of the Hoopa Valley in northern California. There he made a study of the Hupa Indian culture. At the University of California, Berkeley, he earned a Ph.D. degree in 1904 for the now classical work with the Hupa. After five years on the faculty at Berkeley, he was appointed associate curator of ethnology at the American Museum of Natural History in New York City, and the family moved east. He remained at the museum as curator until his death in 1928.

The Goddards lived in Leonia, New Jersey, a pleasant residential town virtually in the shadow of the George Washington Bridge, an easy commute to Manhattan. Goddard attended school there from kindergarten through high school. He has said that the schools were generally excellent but that, except for biology, the science was mediocre, and with his father's advice he postponed chemistry and physics, to begin them at the university level. In early 1922, in his first year of high school, Goddard was seriously ill with what was thought to be influenza and spent a long convalescence at home, sleeping in his father's library rather than the room he shared with his brother. During this time he read widely, T. H. Huxley, Henry Drummond, Havelock Ellis, Dickens and so forth. He then realized that he must prepare for final examinations and studied at home. He returned to school in early June for the final examinations, which he passed with higher grades than in any previous year. At age fourteen he realized the valuable lesson that one can learn outside the classroom.

When he was fifteen, his father bought a secondhand greenhouse. He, his father, and his brother dismantled it and reassembled it about one-half mile from their home. During his last two years of high school he spent most of his spare time operating the greenhouse and selling nursery stock. He said this experience turned him toward ornamental horticulture and later to botany. He also said that during the period when he and his father operated the greenhouse, his father directed his reading and was often critical of his views and challenged his "facts" and interpretations; he felt that without doubt his father was his best teacher.

UNIVERSITY OF CALIFORNIA, BERKELEY

Goddard chose to go west for his university education. In the company of an anthropologist friend of his father, Gladys A. Reichard, they drove from New Jersey to New Mexico and Arizona over mostly primitive roads, camping along the way. After a brief visit in the desert with his father (his last), he went on by train to Berkeley. This, his first trip across the continent, was an exciting and educational experience. Goddard found Berkeley a "wonderful place for an independent and self-reliant student," where he quickly got to know many faculty members, such as Alfred Kroeber and Robert Lowie, friends of his father in the anthropology department. He has listed faculty members in the botany department whom he got to know outside class: Ernest B. Babcock, Lee Bonar, Roy Clausen, H. H. Dixon, Thomas H. Goodspeed, R. M. Holman, Willis L. Jepson, Herbert L. Mason, Lucille Roush, and William A. Setchell. In his first year Goddard was given a key to the botany building and a corner where he could keep books, collections, and a microscope. Setchell later became his advisor, mentor, and friend and even gave him access to his private library. Goddard said he probably took too many courses that were primarily descriptive, such as local flora, morphology, and taxonomy. But he had genetics with Babcock and Clausen, chemistry with Joel H. Hildebrand, and cell physiology and biophysics with Sumner Brooks, and he took courses in anthropology, philosophy, and the history of science. As a student with an independent bent, he was admitted to a graduate seminar in his sophomore year.

Goddard's summers were spent in the field: at the end of his first year, with Bonar, as a collector and preparator for the freshman courses; after his second year, as an assistant to Babcock searching for *Crepis* (hawkweed) from the White Mountains of Arizona to the Colorado Rockies and the Grand Tetons; a year later, on a collecting trip in the Sierra foothills with Jepson and in a job with the white pine blister rust program of the U.S. Forest Service. Goddard said that much of his education came in these summer periods.

Goddard went on to graduate work at Berkeley, largely on Setchell's advice. Setchell assured him that "he would see" that Goddard obtained a National Research Council fellowship on getting his Ph.D. degree. He kept up his friendship with the faculty members he had known as an undergraduate and added those of Charles Lipman, a plant physiologist; Paul Kirk in biochemistry; Harold Blum and Sherburne Cook in physiology; Victor Lenzen in physics; and A. J. Salle in microbiology. Though not required, he took such courses as physical chemistry and thermodynamics, biochemistry, advanced bacteriology, and history of science. He continued his independent ways. Told of a graduate seminar course in comparative physiology organized by Blum and Cook, he signed up for the course as a student and discovered that the course faculty were listed as Blum, Cook, and Goddard.

For his doctoral research Goddard studied the metabolism and nutrition of a dermatophyte fungus, *Trichophyton.* This led to his later interest in protein structure. He stayed on at Berkeley for a year after his degree, as a teaching fellow. During this year he and Fred Uber investigated the X-ray killing of spores of the fungus *Neurospora.* In this research many mutant spores were produced, including some that were nutritionally deficient. Some of these mutants were given to B. O. Dodge and served as markers in his genetic work. It might be said that Uber's and Goddard's work set the stage for the later work of George Beadle and Edward L. Tatum, their one-gene/one-enzyme hypothesis and the 1958 Nobel Prize, which they shared with Lederberg.

ROCKEFELLER INSTITUTE

The following year Goddard was awarded a National Research Council fellowship to work with Leonor Michaelis at the Rockefeller Institute in New York. There he assimilated Michaelis's concept that the oxidation of organic compounds occurs in single electron steps (semiquinones) and later published a landmark paper with James LuValle that laid the foundation for what are now known as free radical reactions in biology.

Michaelis was not interested in the *Neurospora* work that Goddard had done, but he was excited when Goddard showed him that keratin (hair, wool, feathers), which is not normally digested by proteolytic enzymes, could be so digested if the disulfide bonds were first reduced. He and Michaelis showed that the fibrous structure of the protein could be degraded by reduction of the disulfide bonds:

$$2\ H^{+} + R{-}S{-}S{-}R \rightarrow 2\ R{-}SH$$

and that such reduction changed the inertness of the fibers to a biologically and chemically reactive form. The structure was partially restored by later spontaneous oxidation. This was one of the earliest transformations of protein structure. Goddard found thioglycolic acid to be a good reducing agent for this purpose. This reagent and other thiol compounds became the basic ingredients of the permanent wave solutions used in quantity in the cosmetics industry. On more than one occasion proposals were made to Goddard that he apply for a patent to protect this application. He rejected all these proposals out of hand, being interested in the scientific implications, not the commercial potential, of the discovery.

Subsequently, he showed that the tripeptide glutathione, which occurs as a dimer with the two units, R—SH, linked by a similar R—S—S—R bonding, is readily reduced under biological conditions by the coenzyme triphosphopyridine nucleotide, TPN. This work will be related below.

ROCHESTER

In 1933, after his two years at the Rockefeller Institute, Goddard was offered a position at the University of Rochester by B. H. Willier, who was then building the Biology Department, a department that was to include a remarkable group of biologists: Curt Stern, James Neal, Herman Rahn, David Perkins, and Ernest Hadorn. All of these men, and Willier as well, became members of the National Academy of Sciences. In addition, George Corner, Wallace Fenn, and George Whipple, at the medical school, became members of the Academy.

At first Goddard was housed at the older campus of the university with very little equipment and little contact with the more active members of the department, but he then moved to the new River campus. At Berkeley he began studying the metabolism of fungi, which until then had been rather neglected. He picked up the observation of B. O. Dodge that the spores of *Neurospora* are dormant and need brief heating in order to germinate. At Rochester Goddard defined the heating temperature as 49°-52°C and found that they could be thus activated to germinate even if respiration was fully inhibited by cyanide (HCN) provided that the HCN was removed afterwards. The activation was accompanied by a large increase in oxygen consumption. Apart from the intrinsic interest of these results, they seem to have channeled Goddard's interest into the respiration of plant cells. He and Paul Allen studied the respiration of wheat leaves infected with powdery mildew; the infection increased the respiration rate by six times, and this was not due to the fungus but to a change in the oxidation system of the leaf cells. The increased respiration was sensitive to cyanide or azide, thus resembling the respiration of animal and microbial cells. Some of this work was carried out during two summers at the Woods Hole Biological Laboratory.

In a short-lived flier into a quite different field Goddard spent part of the year 1941-42 on a Guggenheim fellowship with Edward Adolph studying water and salt balance for man at the Desert Training Center at Indio, California.

Returning to the heat activation of *Neurospora* spores, he and Paul Smith showed that the dormant spores produce no carbon dioxide, CO_2, anaerobically, but the activated spores do, that is, they ferment. The heat treatment thus activates the enzyme carboxylase. From there he proceeded to study the respiration of plant tissues, first with carrot slices and then with tissues of ash and maple trees, showing that they are cyanide sensitive and probably catalyzed by cytochrome oxidase. Barley seeds behaved similarly. The logical next step was to try to isolate the cytochrome oxi dase of plants, which he and Allen H. Brown did with wheat embryos, showing that it is photoreversibly inhibited by carbon monoxide, CO. This was followed by the demonstration of cytochrome C in wheat germ. This proof, in 1944, that the cytochrome oxidase operated with the cytochrome C from plants was a milestone, for with Keilin's earlier studies at Cambridge on the participation of the cytochrome system in the respiration of cells and tissues it served to set up respiration as a major field of plant physiology; it also established Goddard's reputation as leader in this field.

Subsequently, in the 1950s, the work was extended to pathological conditions, including crown gall and some pathogenic fungi. Goddard studied cyanide respiration in fungi and showed that under anaerobic conditions the cytochrome system would operate with glutathione or dehydroascorbic acid as hydrogen acceptor. Two important and widely cited reviews, one a chapter in Höber's *Physical Chemistry of Cells and Tissues,* the other written with LuValle in 1948, served to clarify Goddard's own understanding of the intermediate stages of respiration and to make his name well known in the oxygen consumption of plant tissues. In all this work Goddard's broad biological approach, rather than a narrowly botanical, zoological, or medical one, is evident.

UNIVERSITY OF PENNSYLVANIA

In 1946, with the termination of World War II, Goddard was offered professorships at other universities, and he made the decision to go to the University of Pennsylvania as professor of botany. The Goddards lived in a house in the Morris Arboretum, a former private estate in the Chestnut Hill section of Philadelphia, which had been willed to the university to be administered by the botany department until 1954, when they purchased a house in Chestnut Hill.

The botany department in 1946 was housed in MacFarlane

Hall, an old building that was in miserable shape. It has now been razed. Goddard had negotiated with the university for funds for refurbishment of the building and for equipping laboratories. He immediately activated renovations to the building, revitalized the teaching program, attracted graduate students, and organized an active modern research laboratory. New appointments were made, not only in botany but in microbiology and zoology: Daniel O'Kane in microbiology; Edward C. Cantino, mycology;John Preer, genetics; and myself in developmental biology.

The course in cellular physiology that Goddard taught, and a graduate seminar given by himself and others, attracted many students and often postdoctoral fellows, not only from botany and zoology but from other parts of the university. He took the responsibility for the botany seminar, which became a center of common interest for the university's entire biological and biomedical community. He often brought in a distinguished speaker, who attracted a standing-room-only audience.

At Penn Goddard continued his research interest in cellular respiration. He and Constance Holden isolated cytochrome oxidase from potato tubers showing that it is distinct from the tyrosinase that is very active there and is photoreversibly inhibited by CO. With Richard Darby he demonstrated the activity of cytochrome oxidase in the fungus Myrothecium. He wrote two reviews of plant respiration, with Bas Meeuse and Walter Bonner. It was my great good fortune to collaborate with him, M. Ogur, and technicians C. Holden, G. Rosen, and K. Sax in studies that we hoped might bring biochemical and physiological information to bear on problems of growth and development of plants, classically considered largely in the morphological context. These ideas are discussed in a preliminary way in his 1950 article "Metabolism in Relation to Growth." These hopes were not realized, though Goddard contributed much to a study by Ogur, Rosen, and me of nucleic acids in relation to pollen development in *Lilium* and to the study of nucleic acids and other cellular constituents in relation to growth of the root of *Zea mays.* In the root growth studies, data on respiration of the roots was obtained, which with the growth data would have allowed estimates of the energetic requirement of growth and developmental processes. But this analysis could not be completed without Goddard's participation.

In 1950, after four years in the department, Goddard received a scholarly leave from the university and was awarded a Guggenheim fellowship (his second), which he chose to take at Cambridge University, England. His research at Cambridge resulted in the discovery of the enzyme, glutathione reductase, as mentioned above. His reason for going to the biochemistry department at Cambridge was undoubtedly that it was there that Sir Frederick G. Hopkins's pioneering studies of respiratory metabolism led to the discovery of the tripeptide glutathione. As Goddard tells it, the first seminar in the department after his arrival was by Leslie Mapson on ascorbate synthesis in plants. The role of glutathione was discussed and the fact that no enzymatic mechanism was known for its reduction, even though it is usually found in the reduced state in organisms. In the discussion he suggested that the reason a mechanism had not been found was that a suitable hydrogen donor was not known, and he suggested a candidate. Mapson and he, next morning, began experiments that quickly showed that tri-phospho-pyridine nucleotide (TPN, coenzyme II) served as a hydrogen donor in the reaction of an enzyme they characterized and named glutathione reductase. "There was considerable amusement among the biochemists at Cambridge that an American should have found the missing enzyme in the glutathione system that was considered 'Cambridge property.'"

On his return to Penn Goddard became chairman of the Botany Department. However, he felt that separation of the disciplines of botany, microbiology, and zoology was artificial, as did younger members of the faculty. Largely by his efforts, a Division of Biology was formed in 1954, and he became its director. With retirements of older faculty and some increased funding of the new division, there were opportunities to make new appointments. In addition to those mentioned above, appointments made soon after the division was set up were: Allen Brown, plant physiology; Paul Green, plant development; Sidney Rodenberg, microbiology; William Telfer, animal embryology; Robert MacArthur, ecology; Alan Epstein, animal nutrition; Vince Dethier, animal behavior, and others. The distinguished reputation of Penn biology in those years was in large part due to Goddard's energy and talent for recruitment and administration of the group.

The physical facilities for biology were inadequate and scattered among three separate buildings. The university appropriated money for partial renovation of one of the buildings, the Leidy Laboratory, and Goddard was given a mandate to raise money for a new building. This required a great deal of his time and energy; the new building, now named the Goddard Laboratory, replaced the antiquated quarters of botany and microbiology with modern facilities.

ADMINISTRATOR

One of Goddard's reasons for leaving Rochester was that he felt he was likely to be pressed into administrative work, and he hoped that by going to Pennsylvania he would have more time for research. In fact, it did not work out that way, for in his very first months at Penn, Goddard assumed some of the duties of the chairman of the Botany Department, succeeded Jacob Schramm as chairman of botany in

1952, as said above, and became chairman of the Division of Biology in 1954. He was often called on by the dean of the college, the provost, and the president to make administrative recommendations or to find a new senior faculty member. When the university undertook an exhaustive study of its facilities, organization, and programs, the "Educational Survey," Goddard played an important part in the survey, writing the report on the university faculty. In 1961 President Harnwell asked him to become provost, the senior educational officer of the university, and he accepted. Based in part on the data and reports of the Educational Survey, Goddard wrote the "Integrated Development Plan," adopted by the trustees in 1962. This led to a $93 million fundraising campaign and guided the subsequent growth of the university. Goddard pressed for physical plans that were properly designed for the academic program. But his real charge as provost was to develop the quality of the faculty and the student body, undergraduate, graduate, and in the professional schools. He personally interviewed all candidates for appointment or promotion in rank to assistant professor or higher. Formal appointments were made by the trustees on recommendation of the provost; in Goddard's tenure no such recommendations were ever vetoed. He pressed for upgrading salaries and overcoming gross inequities in salaries. Quoting Eliot Stellar, his successor as provost, "His personal scholarship, his values of independent thought and academic freedom, his dedication to the highest standards of academic excellence, his warm and affectionate spirit, and his sterling qualities of leadership all had a chance to express themselves in one of the greatest nine-year periods the University of Pennsylvania has known. It was the 'right man and the right time.' His decisions and actions were not always popular and sometimes led to animosity, but it was generally agreed that Dave was 'tough but fair.'"

During the turbulent years of the Vietnam war, questions were raised about the ethics of military research on campus, some of which was secret. Recognizing that most of these activities were legitimate, and relying on the principle of academic freedom, Goddard worked out a formula requiring that all technical research be publishable without undue delay and that neither the government nor industry be given the authority to determine who conducted and published the research. This became the policy of the university. (Student unrest during this period led to a six-day sit-in in College Hall to protest the razing of homes for expansion of the University City Science Center. An anecdote, as I recall, relates that each morning on his way to the office Provost Goddard and the students greeted each other cheerily, and when they became concerned about the safekeeping of some money they had collected to support the sit-in they asked Goddard to keep it for them in a safe in his office. Seriously, however, by genuinely responding to the students' concerns, he and his staff helped the students to organize a peaceful demonstration, and to present their demands without dangerous disruption.) Despite urging by many students and faculty that the university should take a stand on the Vietnam issue, the position of the administration was to remain unaligned so as to preserve personal academic freedom.

As provost, Goddard was often asked to address various groups concerned with academic policy, and he expressed the theme of many of these addresses in a talk before the American Philosophical Society in April 1971: "Universities serve society best by being centers of free inquiry, where conclusions are openly arrived at, and where there is a receptivity to new ideas" *(Science* 173:607-10).

Goddard retired as provost at the end of 1970 and became professor of science and public policy, then university professor in biology emeritus. In 1975 he was elected home secretary of the National Academy of Sciences for a four-year term. This entailed spending a part of each week in Washington. He served also as a member of the Council of the Academy and the Governing Board of the National Research Council, by virtue of his position as home secretary. The position brought him into contact with many prominent scientists and scientific problems of the time, but he did not find the position a challenging one, feeling that the intellectually interesting work went through the Office of the President of the Academy. He returned to the University of Pennsylvania as provost emeritus and professor emeritus.

Most unfortunately, in his last years Goddard was afflicted by Alzheimer's disease, which led to his death on July 9, 1985. He suffered the loss to cancer of his first wife, Doris Martin, in 1951, her illness having cut short their stay in Cambridge; and in 1984 his daughter Alison also died of cancer. He is survived by his second wife, Katharine Evans, and his son Robert.

Throughout his career Goddard also applied his talents to a continuous series of offices, committee assignments, and consultative positions. Thus, he brought his ideals of academic freedom and insistence on intellectual excellence and honesty to a broader community than his own university. He was president of the American Society of Plant Physiologists in 1958; editor-in-chief, then associate editor, of *Plant Physiology* (1953-63); and recipient of the society's Stephen Hales Medal in 1948. He served on the Board of Directors of the American Association for the Advancement of Science (1963-68); was president of the Society of General Physiologists (1948); and was president of the Society for the Study of Growth and Development (1953). He was a member of the editorial committee of the *Annual Review of*

Plant Physiology (1949-54) and a consultant to the Manhattan Engineering District (1943-46). As a biological consultant to Commercial Solvents Corporation (1944-50), he contributed to the development of production methods for penicillin. He served on a variety of other advisory committees, panels, and boards concerned with scientific and educational matters.

In 1962 Goddard authored a report of a White House conference on narcotic and drug abuse, which defined the problem of drug abuse as a social illness rather than a criminal offense. He was a member of a panel of the President's Science Advisory Panel, which issued a report on the use of pesticides in 1963, and also participated in a 1970 report on space biology by the Space Science Board of the National Academy of Sciences.

Over the years Goddard was associated with the National Academy of Sciences in various ways. His work with Leonor Michaelis at the Rockefeller Institute (1933-35) was supported by a National Research Council fellowship. He was elected to the Academy in 1950, though already in 1948 he was made a member of the Advisory Committee of the Academy's Chemical-Biological Coordination Center, representing the Botanical Society of America. He served as a member of the Division of Biology and Agriculture (195255), which was succeeded by a Biology Council and in turn was transformed into the American Institute of Biological Societies (AIBS). Among several other Academy tasks, perhaps one of the most important was his membership on the Advisory Committee on Science Exchange with the USSR and Eastern Europe (1960-68; chairman, 1966-68). As stated above, Goddard served as home secretary of the Academy from 1975 to 1979.

I have consulted a manuscript autobiography that Goddard prepared with Katharine E. Goddard and transcripts of talks at a memorial gathering on October 1, 1985, by Stanley E. Johnson, Sheldon Hackney, Britton Chance, Martin Meyerson, Robert Trescher, and Jack Russell. A memorial resolution was recorded in the minutes of a meeting of the Council of the National Academy of Sciences on August 11, 1985. A biographical memoir of Goddard by Eliot Stellar was published by the American Philosophical Society.

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C. R. Hauser

Charles Roy Hauser

March 8, 1900-January 6, 1970

By Charles K. Bradsher

Charles R. Hauser characterized himself, and has been characterized by others,[1] as a physical organic chemist, but more than his rivals, he devoted his efforts to the study of reactions that have been or could become of interest to the *synthetic* organic chemist. The index[2] to collective volumes I through V of *Organic Syntheses* shows that Hauser contributed twenty-six articles, almost equal to the combined total for the other ten physical organic chemists whose names appear in Tarbell and Tarbell's[1] extended list of prominent American physical organic chemists. Hauser's contributions to synthetic organic chemistry were recognized with the American Chemical Society's Award for Creative Work in Synthetic Organic Chemistry in 1962 and the Synthetic Organic Chemical Manufacturers Association Medal for Creative Research in Organic Chemistry in 1967, leaving no doubt that he qualified also as an outstanding synthetic organic chemist. A quotation from the *SOCMA Meeting Call*[3] *of* August 30, 1967, considered his role as being that of a synthetic as well as a physical organic chemist:

> . . . The subject of Dr. Hauser's talk will be "Some Applications of Physico-Chemical Principles to Organic Synthesis." Dr. Hauser will draw on his own experience to illustrate the progress since the 1930's. At that time, the

> physico-chemical reactions began to move into the mainstream of development from a base where the work in structures was of limited maturity and reactions were extremely difficult to predict. He will comment on how this pattern has taken shape in terms of the flexibility of the carbon atom, and how it has advanced into the far more sophisticated forms of today, often at the expense of cherished concepts. Incidentally, this latter is a process that leaves him wary of all theories, concepts or generalities that seem too well established.

Interestingly, Tarbell and Tarbell,[4] in their brief characterization of Hauser's contribution to organic chemistry, began by discussing his contribution to synthesis: "He developed new and useful synthetic methods and knowledge about reaction mechanisms, for which he had a real instinct."

In his over 450 publications, Hauser showed wide-ranging interests, but his most memorable work was in the field of bases. By the time of his death, he was certainly the international authority on the role of bases in organic synthesis. It is remarkable that so much was accomplished by someone who had once been a school dropout!

I was asked to write this memoir because I knew Hauser over almost the entire span of his career at Duke University and was the author of an earlier biographical sketch[5] on him. Two years after his arrival at Duke I was in a recitation section of his; then after 1939 I was a junior colleague, and at the time of his death I was his department chairman. Despite the long association I cannot claim to have been a close friend, but from records, personal observation, and discussions with former colleagues and students, I know something about Hauser's professional life. Fortunately, Hauser's three children have been kind enough to share with me some of their reminiscences about his life away from the chemistry building. I am indebted to his two daughters, Frances M. Grate and Betty Yourison, and his son, Charles F. Hauser (also an organic chemist), for their letters. Professor Hauser's widow, Mrs. Madge B. Hauser, survived until 1992.

Hauser's father, also named Charles, but with no middle name, was brought to this country from Germany as a child. As he grew up he had few opportunities for education but learned to support himself as a truck farmer and part-time master carpenter. He married Elizabeth Rogan, and the Hausers were living in San Jose, California, in 1900 when Charles Roy Hauser was born. Shortly before World War I the family moved to Homestead, Florida, a small community about 40 miles southwest of Miami, where the mother set up a boarding house. This is the place that Hauser always thought of as home, and he preferred to be thought of as a Floridian rather than a Californian.

Later he was to amuse his children with accounts of his life in Florida. One concerned his frequent swims in a canal close by his house. On one occasion just before diving in he saw something that looked very large and suspiciously reptilian. A local hunter was called to shoot the alligator, which proved to be 14 feet in length!

Another story concerned the origin of a vision problem that was to trouble him for the rest of his life. The following account is from his son, Charles:

> Dad had to drop out of school in the eighth grade because he lost his sight. He thought it was because of poor food, combined with the glare of water and sand of Florida. He worked on the truck farm for a year, during which time his sight returned partially. The local high school principal recognized Dad as a sharp lad and induced him back to school by letting him enter the ninth grade and not miss a year. (Dad said that he wanted to get back to school because he was tired of hoeing tomatoes.)[6]

With a high school diploma and savings of $200, Hauser entered the University of Florida and acquired a B.S. degree in chemical engineering. His college annual (class of

1923)[7] reveals that he was known as "C.R." and belonged to the American Chemical Society and the Flint Chemical Society plus some engineering associations. It was a custom in those days for yearbook editors to salute each senior with some descriptive phrase, which in Hauser's case was particularly perceptive: "He likes chemistry."

Hauser stayed on at Florida to acquire the M.S. degree in 1925, working at least part-time as an assistant. For his Ph.D. he moved on to the University of Iowa, where he did research in chloramine chemistry under the direction of G. H. Coleman. At Iowa he also met and married a fellow chemistry graduate student, Madge L. Baltimore, whose technical knowledge was to become important to him later on when his eyesight began to degenerate.

After a year as instructor at Lehigh, he moved to Duke University still with the rank of instructor. When the Hausers arrived in Durham, North Carolina, in 1929 the Neo-Gothic West Campus of Duke University had not been completed, and two years were to elapse before he could move his research to the new chemistry building. Fortunately, his department chairman, Paul M. Gross, appreciated Hauser's zeal for research and provided him with a reasonable share of the very limited resources and facilities available.

Hauser, as we knew him, was a modest gentleman, intense and endlessly worried about details. Physically slight, he had a long-time preoccupation with the need for proper nutrition and exercise. He was one of the Duke faculty's better tennis players but abandoned the game at some time in his forties, when he took up swimming and then walking for physical recreation. His fitness program was evidently a success for it has been reported that as late as in his fifties he could walk on his hands to amuse his grandson.[8]

A single physical limitation, the sensitivity to light that had made him drop out of school earlier, led to self-im posed restrictions that influenced both his personal and professional life. Although light sensitivity did not appear to affect his tennis game before World War II, it was the excuse usually given when he refused invitations to lunch or dinner, even with distinguished visitors, or to attend evening meetings or social functions.

His limited reading was usually done in his office by daylight only, and he was known to walk into a colleague's office, extinguish the lights (with an apology), state his business, and leave turning the lights back on. Despite the willingness of his wife to read to him, he was not as well read as many of his contemporary physical organic chemists. As his daughter Frances (Grate) remembers, "Daddy credited the handicap of his poor eyesight with forcing him to do original thinking—he couldn't read other people's work."[8] His son Charles recalls: "He liked poetry and classical music. He said that classical music (played softly) would relax him so he could think about chemistry."

Long after Hauser's work had attracted national attention he was not well known on the Duke campus. He kept a low profile, avoiding service on university-wide committees or participation in social events involving the general faculty and appeared content with a modest, almost frugal, life style. Perhaps this inconspicuousness accounts for his being overlooked when in 1953 fourteen distinguished members of the Duke faculty were selected to become the first James B. Duke professors. Only five years later, and still not recognized as a "distinguished professor," Hauser became the first member of the Duke faculty to be elected to the National Academy of Sciences!

In addition to his numerous research papers and chapters, Hauser had hoped to publish a book on organic reaction mechanisms, embracing the material contained in his graduate course, but he never found a suitable coauthor.

This was unfortunate, for he best communicated his ideas in writing, and worked hard at achieving clarity. The manuscripts that he presented to the typist were usually first typed by him on yellow paper to prevent glare, with all corrections made by overstriking. This format was familiar to his colleagues and graduate students, since we were frequently polled to determine which of alternate word arrangements was clearer. To his students and research associates he emphasized the importance of acquiring writing skills. It was such a shock to him to learn that the University of Florida still had in the university library a copy of his master's thesis written in a more primitive Hauser style that he even made an effort to have it removed.[6] He appears to have been less disturbed by the preservation by his family of some verse written by him only a few years later, probably because publication was not contemplated.[8] The verses, dedicated to the future Mrs. Hauser, were reasonably competent and in a whimsical rather than sentimental mood. To most of us who knew him only professionally, the possibility of Hauser having an interest in verse or whimsy would have sounded about equally improbable, but his son has assured me that at home his father clearly liked poetry and had an excellent sense of humor.[6]

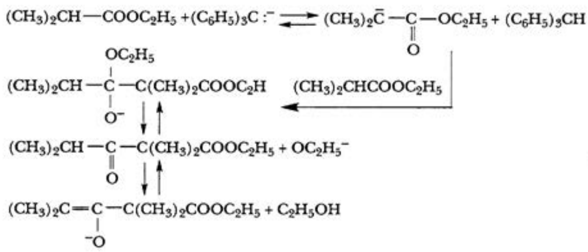
Hauser's very limited ability to read by artificial light led not only to his previously cited inability to even scan all of the chemical literature important to his research but also to an even more limited opportunity for general reading. It is quite understandable that this handicap, coupled with his never having been outside the United States, made him seem less sophisticated than many of his university colleagues.[8]

To each of his graduate students, but especially to those having problems with research or course work, Hauser could be counted on to be friendly and helpful, ready to give advice and inspiration. A student or indeed a postdoc who might be having difficulties could look forward to having his study or relaxation interrupted by an evening telephone call at home. I have found no support for the Duke legend that less zealous co-workers terminated such conversations by coughing into the telephone, relying on Hauser's well-known fear of catching cold to bring the conversation to an early conclusion. Hauser's interest in his students and coworkers did not end when they left his laboratory; their subsequent careers were followed with interest and, in most cases, justifiable pride.

Hauser once remarked to me that it took him a long time to learn how to do research. Unfortunately, he did not amplify his remark. Records show that six years after completing his Ph.D. degree he had published eight research papers, seven of which were on chloramine chemistry, the subject of his dissertation research. While my first reaction was that he felt that these early papers did not give evidence of the originality characteristic of his later publications, I now feel that what he had learned was how to organize his research, suiting the particular problem or part of it to the level of skill and development that his assistant might have. There is no doubt but that Hauser developed great skill at such organization.

Not long before his death Hauser described his research interests as "fundamental organic chemical studies of mechanisms and syntheses in the fields of condensations, cyclizations, substitutions, eliminations, and molecular rearrangements."[3] There is no doubt that his 450+ papers have provided us with new insights into each of these areas. Perhaps his most important contributions concern an operation central to organic synthesis, the establishment of a new carbon-to-carbon bond. A brief summary follows.

Working with W. B. Renfrow, Jr., in 1937, Hauser showed that the popular belief that the base-catalyzed self-conden sation of esters required the presence of two hydrogens on the alpha carbon of the ester was erroneous.[9] He found that even ethyl isobutyrate, which has only *one* alpha hydrogen, undergoes self-condensation in the presence of a sufficiently strong base, in this case the triphenylmethide ion. The authors explained the condensation in terms of a complex equilibrium (Scheme 1) that could also be extended to the self-condensation of ethyl acetate. This will seem familiar because it was to become essentially the modern textbook description of the mechanism.



(1)

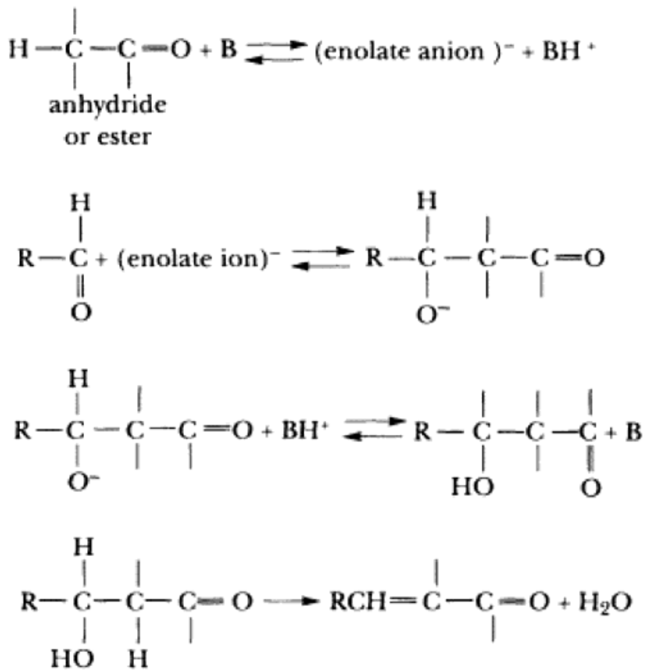
In his second paper of the series in 1938 Hauser enunciated what became Hauser's Rule: In all known condensations of ethyl esters a weaker base is formed than the one used to initiate the reaction.[10]

Hauser recognized the kinship between the ester condensation and the Perkin reaction in which, in the classic example, sodium acetate and acetic anhydride are heated together with benzaldehyde to yield, on addition of water, cinnamic acid. Although Perkin himself had assumed that condensation occurred between acetic anhydride and benzaldehyde, the textbook writers of the 1930s usually de picted the reaction as occurring between benzaldehyde and sodium acetate, the role of the acetic anhydride being that of a dehydrating agent.[11] A plausible defense for such a view had been provided by R. Fittig and F. L. Slocum, who showed that when sodium butyrate is substituted for sodium acetate in the experiment, some alpha-ethylcinnamic acid is formed.[12]

Hauser concluded that the anion of acetic anhydride was a much more probable intermediate than a doubly charged anion derived from sodium acetate. With David S. Breslow he demonstrated that when sodium butyrate is heated with acetic anhydride, or when sodium acetate is heated with butyric anhydride, essentially the same equilibrium mixture containing both anhydrides is formed, suggesting that the intermediate in Fittig's formation of ethylcinnamic acid most likely was butyric anhydride rather than sodium butyrate.

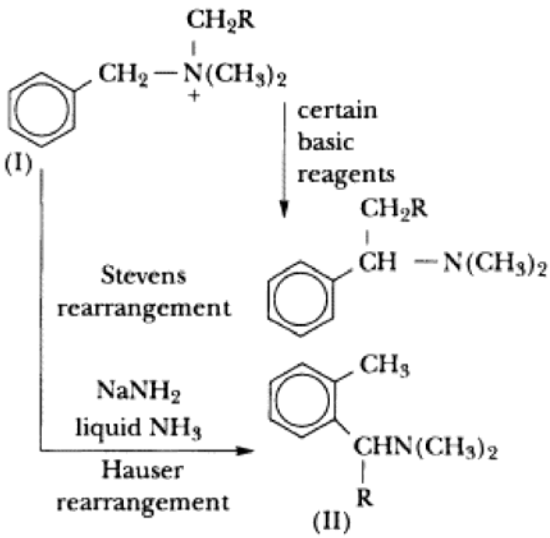
The condensation of benzaldehyde with ethyl acetate in the presence of bases has also been called a Perkin condensation. Although it had long been supposed that reactions of this type occur via an aldol intermediate, no such intermediate had ever been isolated. Hauser and Breslow showed that with the suitable choice of a base and reaction conditions the long-awaited aldol intermediate could be isolated.[13, 14]

An astute analysis of all results then available led Hauser and Breslow to propose a reaction mechanism (Scheme 2; B = base) that brings out the critical importance of the conjugate acid (derived from the base) in facilitating the dehydration step that serves to permit the reaction to go to completion.



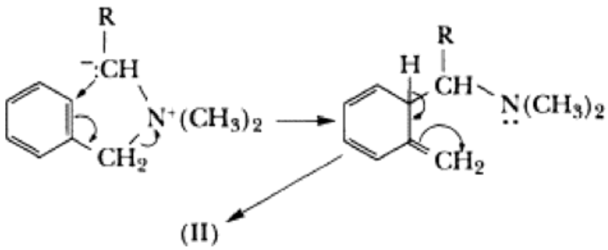
(2)

T. S. Stevens had shown that the action of various bases on benzylic quaternary salts (I, Scheme 3) resulted in a 1,2 shift of alkyl groups, quite properly called the Stevens rearrangement.[15] In 1951, in collaboration with S. W. Kantor, Hauser found that sodium amide in liquid ammonia caused another type of rearrangement, affording *ortho*-substituted benzene derivatives (II) in yields of over 90 percent (Scheme 3).[16]



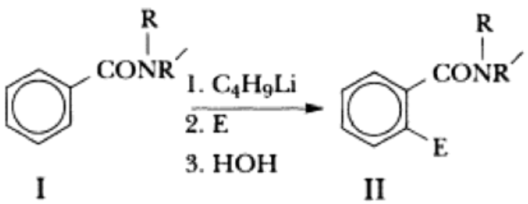
(3)

Although the reaction mechanism was obscure, it was correctly proposed in the first paper and confirmed by subsequent work in collaboration with A. J. Weinheimer[17] and D. Van Eenam.[18] The Hauser rearrangement takes place by what the authors have described as an aromatic nucleophilic mechanism in which the benzene ring serves as an electron acceptor in a five-atom ring displacement (Scheme 4). The *ortho*-methyl group, which appears in the rearrangement product, did not arrive there by migration from the nitrogen atom, but was instead created from the benzylic methyl group.



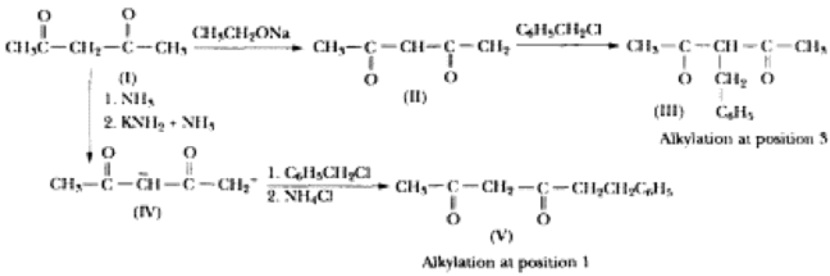
(4)

In 1964, in collaboration with W. H. Puterbaugh,[19] and as part of a comprehensive study[20] of the lithiation of aromatic nuclei and the side chains attached thereto, Hauser carried out the *ortho*-lithiation of N-methylbenzamide (I,R = H,R' = Me, Scheme 5) and, by reaction of the resulting lithium reagent with ketones, demonstrated the synthetic usefulness of the reaction. While significant as the first example of the *ortho*-lithiation of a benzamide, it was supplanted in 1977 by a more useful metalation reaction using N,N-diethylbenzamide (I,R = R' = Et) and secondarybutyllithium, reported by P. Beak and R. A. Brown.[21]



(5)

The discovery by Hauser and T. M. Harris that multiple anions may be made to react selectively perhaps will have the greatest impact on organic synthesis.[22] This breakthrough has made possible the easy synthesis of compounds that would be difficult to obtain by other means. For example, the monoanion (II, Scheme 6) of acetylacetone (I) has long been known to undergo benzylation at carbon 3. However, if the dianion (IV) was formed by reaction of acetylacetone with two equivalents of potassium amide in liquid ammonia, alkylation with benzyl chloride afforded the 1-alkylated product (V) in good yield.[23] Similar selectivity was shown in the reaction of other dianions[24] and, later, higher multiple anions.[25]



(6)

In 1944 Hauser directed a research project on antimalarial drugs for the Office of Scientific Research and Development and was awarded a Certificate of Merit. He served as a consultant for Union Carbide Chemicals Company from 1946 to 1961. He was a visiting lecturer at Ohio State University during the summer of 1956 and in 1961 was awarded a James B. Duke Professorship by Duke University. Hauser was the recipient of three American Chemical Society awards: first, in 1957 the Florida Section Award, given to an outstanding chemist in the Southeast; then in 1962 the Herty Medal, also for an outstanding chemist in the Southeast as well as the ACS Award for Creative Work in Organic Chemistry; and, finally, in 1967 the Medal for Synthetic Organic Chemistry from the Synthetic Organic Chemical Manufacturers Association.

On January 6, 1970, a few months before he was to retire, Charles R. Hauser gave up the fight against a debilitating heart condition that had bothered him for a long time.

Quite characteristically one of his last visitors was a graduate student who talked with him about research.

NOTES

1. D. S. Tarbell and A. T. Tarbell. *The History of Organic Chemistry in the United States, 1875-1955.* (Nashville: Folio Publishers, 1986):143.

2. *Organic Syntheses,* Index to Collective Volumes I-V, eds. R. L. Shriner and R. H. Shriner. (New York: John Wiley & Sons):432.

3. I am indebted to the Duke University Archives (William E. King, Archivist) for this and other pertinent information.

4. Tarbell and Tarbell, op. cit., p. 268.

5. *McGraw-Hill Modern Scientists and Engineers,* vol. 2. (New York: McGraw-Hill, 1980):29-31.

6. Letter. August 17, 1990.

7. *The Seminole.* (Gainesville: University of Florida, 1923):39. I am indebted to Joyce Dewsbury, coordinator of the University Archives, University of Florida, for a copy of this page.

8. A joint letter from Frances Hauser Grate and Betty Hauser Yourison, September 3, 1990.

9. C. R. Hauser and W. B. Renfrow, Jr. (1937,1).

10. C. R. Hauser and W. B. Renfrow, Jr. (1937,2).

11. For example, J. F. Norris. *Principles of Organic Chemistry.* (New York: McGraw-Hill, 1931):459. Other examples are cited in note 13.

12. R. Fittig and F. L. Slocum. Ueber die Perkin'sche Reaction: Einwirkung von Benzaldehyde auf einem Gemisch von Essigsaureanhydrid und Buttersaurem Natrium. *Justus Liebig's Annalen de Chemie* 227(1885):53-55.

13. C. R. Hauser and D. S. Breslow (1939,1).

14. C. R. Hauser and D. S. Breslow (1939,2).

15. T. S. Stevens. Degradation of quaternary ammonium salts. Part II. *Journal of the Chemical Society* (1930):2107-19.

16. C. R. Hauser and S. W. Kantor (1951).

17. C. R. Hauser and A. J. Weinheimer (1954).

18. C. R. Hauser and D. N. Van Eenam (1956,1; 1957,1,2).

19. C. R. Hauser and W. H. Puterbaugh (1964).

20. C. R. Hauser, F. N. Jones, and M. F. Zinn (1963,1).

21. P. Beak and R. A. Brown. The ortho lithiation of tertiary benzamides. *Journal of Organic Chemistry* 42(1977):1823-24. See also P. Beak and V. Snieckus. Direct lithiation of aromatic tertiary amides:

An evolving synthetic methodology for polysubstituted aromatics. *Accounts of Chemical Research* 15(1982):306-12.

22. C. R. Hauser and T. M. Harris (1957,3).

23. C. R. Hauser and T. M. Harris (1958).

24. For example, C. R. Hauser and W. R. Dunnavant (1960).

25. For example, C. R. Hauser, M. L. Miles, and T. M. Harris (1963,2).

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C.L. "Kelly" Johnson

Clarence Leonard (Kelly) Johnson

February 27, 1910-December 21, 1990

By Ben R. Rich

Be quick, be quiet, be on time.

That was the credo of Clarence L. (Kelly) Johnson, the aeronautical innovator who founded Lockheed's supersecret "Skunk Works" where he designed the world's fastest and highest-flying aircraft—the SR-71 Blackbird.

Johnson played a leading role in the design of more than forty aircraft and set up a Skunk Works—type operation to develop a Lockheed satellite—the Agena-D—that became the nation's workhorse in space. His achievements over almost six decades captured every major aviation design award and the highest civilian honors of the U.S. government and made him an aerospace legend. He was elected to the National Academy of Sciences in 1965 and enshrined in the National Aviation Hall of Fame in 1974 and was honored by many other prestigious institutions and organizations for his work.

Johnson achieved international recognition for the highly successful Skunk Works operation—"a concentration of a few good people . . . applying the simplest, most straightforward methods possible to develop and produce new products" with minimum overhead and outside oversight—and for his unparalleled management style. For someone whose inauspicious beginnings were in a small iron-ore mining town in the upper peninsula of Michigan, the seventh of nine children of impoverished Swedish immigrants, the Kelly Johnson story was in the fabled American Horatio Alger tradition of success. But with Kelly Johnson the story was all true.

Born in the upper peninsula town of Ishpeming, Michigan, on February 27, 1910, Clarence Johnson received his Irish nickname of Kelly in elementary school from a song of that day, "Kelly from the Emerald Isle," following a schoolyard fight. His classmates figured that someone who had licked the school bully should be known by a somewhat more pugilistic name. The nickname stayed with him from that point on—he was known as "Kelly" ever since. And he won the reputation in his career of never backing away from controversy on aircraft design, materials, and production techniques.

From the time he was twelve years old, Johnson knew what he wanted to do in life—design airplanes. "My whole life from that time was aimed at preparing for that goal," Johnson wrote in his autobiography, *More than My Share of It All.*

Before he reached his teen years, Johnson designed his first aircraft—called the Merlin battle plane—named for the magician of King Arthur's court. A model of his Merlin won a prize in a contest sponsored by a service organization. Pursuing his goal, Johnson entered Flint (Michigan) Junior College after graduating from high school to take engineering, mathematics, physics, and calculus. To support himself he worked in construction and on the Buick Motor Car Company production assembly line during vacations, weekends, and summers. And in Flint, Johnson had his first airplane flight-$5 for three minutes in an old biplane that got up to 700 feet before the engine failed and the aircraft had to make a forced landing. But it didn't dampen Johnson's enthusiasm for aircraft. By that time he had graduated from Flint Junior College and had accumulated $300. He was prepared to spend all his savings on flying lessons at an airport in Flint instead of continuing his education at a university.

After talking to Johnson, the instructor pilot advised Johnson: "Kelly, you don't want to start off your career by giving me $300 to learn to fly. That won't get you far enough. You have good grades and you'll go a lot farther if you go on to the university. I won't take your money." Johnson eventually learned to fly, after he joined Lockheed, but he first heeded the instructor pilot's advice and enrolled at the University of Michigan at Ann Arbor in 1929.

There was virtually no construction work in Ann Arbor at the onset of the Great Depression, so Johnson worked his way through college washing dishes in fraternity houses until becoming a student assistant to the head of the aeronautical engineering department at the university. The head of the engineering department also operated the university's wind tunnel, and Johnson became involved in the testing programs, which included not only aircraft but the design of a Union Pacific streamlined train, a smoke-removal project for the city of Chicago, and an early proposal for generating energy with a smoke machine.

The wind tunnel also provided additional money for Johnson. The university permitted him and a friend to rent the wind tunnel when it was not in use for $35 an hour. Among their customers was the Studebaker Motor Company, which was designing a streamlined automobile and wanted the most efficient configuration possible to fully utilize the power of the engine. And the student operators of the wind tunnel did just that for Studebaker.

In his spare time, Johnson tutored other students in calculus. He graduated in 1932 with a bachelor's degree in aeronautical engineering and started looking for an engineering position at aircraft firms on the east coast. But there were no jobs for even the most talented young engineers at companies struggling just to survive in the depths of the Depression. Johnson decided to join the U.S. Army Air Corps to become an aviation cadet. The Air Corps turned Johnson down when he failed the eye examination. Once again he sought work as an engineer at aircraft companies, this time on the west coast via a borrowed car. The only encouragement he received was at the small Lockheed Aircraft Corporation in Burbank, California, where the company had just been reorganized from bankruptcy.

"There were no jobs then at Lockheed in 1932, but engineering executive Richard von Hake at the plant suggested, 'Why don't you go back to school and come out again next year? I think we'll have something for you.'"

Johnson returned to the University of Michigan for a year of graduate study to obtain a master's degree, his expenses paid by the grant of a $500 fellowship. He studied supercharging of engines, to get high power at high altitude, and boundary layer control. He also went back to the wind tunnel, where among the projects was the design testing of cars that would race at the Indianapolis 500 race.

And then in the wind tunnel program there was a model of a proposed two-engine Electra passenger transport being planned by Lockheed. The aircraft had stability problems, but the university professors and Lockheed executives felt they were acceptable. Johnson didn't.

He left college in 1933 with a master's of science degree, a used car, and plans to return to Lockheed and the promised job in California. Lockheed executive Cyril Chappellet and Chief Engineer Hall Hibbard hired the young Johnson as an $83 a month tool designer until there was an opening in engineering.

What did Johnson think of the upcoming new Lockheed Electra, the aircraft the newly reorganized company was banking its future on? Although he was a young engineer with a fresh degree and just starting his first aircraft company job, the outspoken Johnson didn't hesitate to voice a strong opinion. "Practically the first thing I told Chappellet and Hibbard was that their plane was unstable and that I did not agree with the university's wind-tunnel report," Johnson recalled in his autobiography.

Hibbard sent Johnson back to the University of Michigan wind tunnel with the Electra model "and see if you can do better with the airplane." Johnson did just that. It took seventy-two tunnel runs before he found the answer to the stability problem. He came up with the idea of putting controllable plates on the horizontal tail to increase its effectiveness and get more directional stability. He then added a twin vertical tail and removed the main center tail. The solution worked fine.

When he returned to Burbank, Johnson was a full-fledged member of Lockheed engineering, the sixth in the department. Assigned to the Model 10 Electra, Johnson also flew as a flight test engineer on the aircraft. It was the first of many Lockheed planes on which Johnson served as a flight test engineer—finally accumulating 2,300 hours in this job.

Working on the Model 14 Electra, Johnson developed the Fowler wing flap for braking safety and for added speed in flight when retracted. In 1937 the Institute of Aeronautical Sciences presented the Lawrence Sperry Award to Johnson for "important improvements of aeronautical design of high speed commercial aircraft" for development of the Fowler flap on the Model 14. The Sperry award was given annually "for outstanding achievements in aeronautics by young men."

Johnson was then twenty-seven. It was the first of more than fifty honors and awards—most of them national—he was to receive during his life.

Sparked by the success of its family of commercial aircraft, Lockheed was growing rapidly. However, it was military aircraft and the looming dark clouds of World War II that made Lockheed one of the giant aircraft firms and Johnson one of the industry's leading aeronautical designers.

In 1937 Lockheed won a U.S. Army Air Corps competition for a swift two-engine fighter with the XP-38 prototype designed by Johnson. The twin-boomed aircraft was the forerunner of the legendary P-38 Lightning, with speeds of more than 400 mph. As the P-38 approached the speed of sound during its development, the aircraft encountered the problem of compressibility. Following wind tunnel tests, Johnson made design changes enabling the P-38 to cope with the problem that was still to face engineers and pilots in the future.

The P-38, the fastest and most maneuverable fighter of its day, fought on every front of World War II, and the two leading American aces won their victories flying Lightnings. Lockheed built almost 10,000 P-38s for the United States and Britain.

In 1938, with Hitler's Germany threatening war, Britain sent a purchasing commission to the United States to buy military aircraft—especially a coastal patrol bomber that could act as an antisubmarine plane. Visiting various American aircraft firms, the commission originally did not intend to come to Lockheed. However, there was a change in plans and Lockheed officials were informed that the purchasing commission would be there in five days.

Lockheed had only commercial transports in production at the time, but the Model 14 Electra could possibly be converted into a bomber. Lockheed engineers and shop personnel hurriedly designed and constructed a full-scale wooden mockup of a Model 14 converted to a medium reconnaissance bomber. It was ready when the British arrived five days after Lockheed first received word of the visit.

The enthusiasm and aggressiveness of the Lockheed people and the quality of their design so impressed the commission that the company was invited to send a team to England to confer with British Air Ministry, which would make the final decision on the proposed new bomber. On the team, led by high-level Lockheed executive Courtlandt Gross, was Kelly Johnson.

At the meeting with the Air Ministry, the British called for new specifications that required a major redesign. Working a straight seventy-two hours in a London hotel room over a three-day holiday, catnapping for brief periods, Johnson completed the engineering task in time for meetings with the Air Ministry. Following a week of additional discussions, the Air Ministry chief called Gross aside and said (as recalled by Courtlandt Gross later):

> Mr. Gross, we like your proposal very much, and we very much would like to deal with Lockheed. On the other hand, you must understand that we're very unused in this country to dealing—especially on transactions of such magnitude—on the technical say-so of a man as young as Mr. Johnson. And, therefore, I'll have to have your assurance . . . that if we do go forward, the aircraft resulting from the purchase will in every way live up to Mr. Johnson's specifications.

Gross assured the British air chief that Lockheed had "every confidence" in the twenty-eight-year-old Johnson and that the trust of the Air Ministry in Lockheed would not be misplaced. On June 23, 1938, the British Air Ministry signed a contract with Lockheed for 200 airplanes plus as many more that could be delivered by December 1939 up to a maximum of 250 at a total cost of $25 million. It was the largest single order ever received by any American aircraft manufacturer to that date. And so the famed Hudson bomber of World War II was born. In 1938 the twenty-eight-year-old Johnson became chief research engineer at Lockheed.

The origin of what was soon nicknamed the Skunk Works was in the World War II year of 1943 when the U.S. Army Air Corps asked Lockheed to hurriedly design a fighter around a British DeHavilland jet engine in the wake of disturbing reports that the Nazis had flown their own high-speed jet fighter in the skies over Europe.

Under an agreement negotiated by Johnson, Lockheed was to deliver a prototype jet aircraft within only ninety days. With the approval of Lockheed President Robert E. Gross, Johnson pirated personnel from other projects. He forged a team of twenty-three engineers and 103 shop mechanics working in a small assembly shed at Lockheed in Burbank. Lockheed top management gave Johnson a free hand in the shaping of the team and the aircraft they developed.

This Advanced Development Projects organization completed the prototype Johnson-designed XP-80 jet aircraft in 143 days—37 days under schedule. The aircraft made its first flight on January 8, 1944, at Muroc Dry Lake, California. It was the forerunner of the F-80 Shooting Star, the first U.S. fighter to exceed 500 mph and America's first operational jet fighter. Johnson's Skunk Works and the way it operated were firmly established at Lockheed.

What was the origin of the Lockheed-registered Skunk Works name? It came from Al Capp's "Li'l Abner" comic strip, which featured the "skonk works" where Appalachian hillbillies threw in skunks, old shoes, and other odd ingredients to brew a fearsome drink called Kickapoo Joy Juice.

Working in wartime secrecy, especially on the XP-80 project, engineers identified the assembly shed as the Skunk Works where Kelly was stirring up some kind of potent brew. Although World War II ended before the P-80 could see combat in it, the aircraft proved itself during the Korean War in 1950 when the Shooting Star won history's first all-jet battle.

Among Johnson's military aircraft from the Skunk Works following the Shooting Star were the T-33 trainer, the aerial "schoolroom" responsible for teaching more pilots to fly jets than any other plane; the record-setting 1,300-mph F104 Starfighter, the first operational airplane to fly twice the speed of sound in level flight; and the P2V Neptune antisubmarine patrol plane, which established a nonstop distance record of 11,235 miles in 1946.

Johnson also played a major role in the development of the Constellation, which started out as a commercial airliner design, then was taken over by the military during World War II as a transport, and once again was a pacesetting commercial airliner after the war in addition to a number of military versions produced by Lockheed. But far bigger challenges were in store for the Skunk Works and Johnson, who became Lockheed's chief engineer in 1952, vice president for research and development in 1958, and vice president for Advanced Development Projects in 1958.

In urgent need for a reconnaissance aircraft that could safely fly high over the Soviet Union to photograph missile and other military operations and return with the valuable data, the U.S. government again turned to Johnson and the Skunk Works. Out of the Skunk Works in 1955 came the long-winged U-2 jet, which could fly above 70,000 feet with a range of 4,000 miles on its U.S. Air Force missions. The U-2 was also a money saver. Johnson returned to the U.S. government approximately $2 million saved on the $20 million U-2 contract, producing an extra six planes for the same money intended to cover twenty aircraft.

Nor was this the first time. Johnson was known for his hard adherence to principles. On several occasions he turned back development contracts to the U.S. Department of Defense after initial work indicated the proposed aircraft would not be effective, no matter how much money the DoD was willing to provide.

Advanced U-2 versions, including the Air Force TR-1 and the NASA ER-2 high-altitude research aircraft, were developed. With improvements to the U-2 reaching their limit, radically new reconnaissance aircraft were on Kelly Johnson's drawing boards in the late 1950s: the family of titanium Blackbirds, culminating a few years later in the SR-71.

In January 1960 the U.S. Air Force gave the Skunk Works the go-ahead for the design, manufacture, and testing of twelve A-12s. "The aircraft that were to become the Blackbirds were the first to use the 'stealth' technology we developed for radar avoidance," Johnson said.

High speed was another prime objective for the Blackbirds. As Johnson said:

> The idea of attaining and staying at Mach 3.2 (more than three times the speed of sound) over long flights was the toughest job the Skunk Works ever had and the most difficult of my career.
>
> Aircraft operating at those speeds would require development of special fuels, structural materials, manufacturing tools and techniques, hydraulic fluid, fuel tank sealants, paints, plastics, wiring, and connecting plugs. Everything about the aircraft had to be invented.

But it all came together. Technologically ahead of their time, Johnson's Blackbirds were in the skies in the early 1960s: the A-12's first flight was in 1962; the YF-12A in 1963; and the SR-71 in 1964. With in-flight refueling, the SR-71 attained global range.

SR-71 Blackbirds went on in the 1970s to chalk up records for speed (2,193 mph), altitude (85,069 feet), a trans-Atlantic mark of one hour, fifty-four minutes, on a 3,470-mile flight from New York to London; and a world speed record of three hours, forty-seven minutes on a 5,463-mile flight from London to Los Angeles. In March 1990, the year the Air Force retired the Blackbirds from service, an SR-71 streaked across the United States in a record sixty-eight minutes on the 2,400-mile flight coast to coast.

When Clarence L. (Kelly) Johnson died in 1990, his SR-71 Blackbird, which first flew almost thirty years before, was still the world's fastest and highest-flying aircraft.

The secret of Kelly Johnson's success was really no secret. He was not only one of the world's foremost designers, but he was an innovative manager who gave people who worked for him challenges to constantly create better products.

Many of us in the Skunk Works turned down promotions to other Lockheed organizations to stay with Kelly. And uppermost for Kelly was to stay with the Skunk Works. He was offered a company presidency at Lockheed three times—and three times he declined it. "To me," said Kelly, "there was no better job within the corporation than head of Advanced Development Projects—the Skunk Works."

Even when he retired from Lockheed as a corporate senior vice president in 1975, Johnson continued at the Skunk Works as a senior advisor. His influence continues in the Skunk Works. "Our aim," he said, "is to get results cheaper, sooner, and better through application of common sense to tough problems. If it works, don't fix it."

"Reduce reports and other paperwork to a minimum."

"Keep it simple, stupid—KISS—is our constant reminder."

Johnson instinctively knew how to select people for his organization. He knew how to get the most out of the fewest people and how to get the job done—well. He let his managers run their programs with a minimum of interference. He not only gave you the authority but also the responsibility.

As a man of high integrity himself, Johnson expected complete honesty from the people of the Skunk Works. Mistakes were allowed, but they were to be brought to his attention immediately. And Kelly also expected recommendations to correct mistakes.

He was firmly convinced of the importance of being honest with people, not just telling them what they wanted to hear. He emphasized the necessity of good communication, urging us always to ask a lot of questions.

One of Kelly's challenges to employees was a standing 25-cent bet against anyone who wanted to differ with him. It was not the quarter, of course, but the distinction of winning it from the boss, Kelly said. "It's another incentive. And I've lost a few quarters, too," he admitted. But not often, it must be noted.

Said President Lyndon Johnson when he presented the National Medal of Science to Johnson at the White House in 1966:

> Kelly Johnson and the products of his famous Skunk Works epitomize the highest and finest goal of our society—the goal of excellence. His record of design achievement in aviation is both incomparable and virtually incredible. Any one of his many airplane designs would have honored any individual's career.

Clarence L. (Kelly) Johnson died on December 21, 1990. He was married to the former Nancy Powers Horrigan. His first wife, Althea Louise Young Johnson, died in 1970. His second wife, Mary Ellen Meade Johnson, died in 1980.

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TECHNICAL PAPERS AND REPORTS

The majority of Clarence L. (Kelly) Johnson's reports were classified and most of them still are.

HONORS AND AWARDS

1937

Lawrence Sperry Award, presented by the Institute of Aeronautical Sciences (now the American Institute of Aeronautics and Astronautics) for "important improvements of aeronautical design of high speed commercial aircraft" for development of the Fowler flap on Model 14. Presented annually "for outstanding achievement in aeronautics by young men."

1941

The Wright Brothers Medal, presented by the Society of Automotive Engineers for work on control problems of four-engine airplanes.

1956

The Sylvanus Albert Reed Award, presented by the Institute of Aeronautical Sciences, for "design and rapid development of high-performance subsonic and supersonic aircraft."

1959

Corecipient of the Collier Trophy as designer of the airframe of the F-104 Starfighter, sharing the honor with General Electric (engine) and U.S. Air Force (flight records). The F-104 was designated the previous year's "greatest achievement in aviation in America."

1960

The General Hap Arnold Gold Medal, presented by the Veterans of Foreign Wars for design of the U-2 high-altitude research plane.

1963

The Theodore von Karman Award, presented by the Air Force Association for designing and directing development of the U-2, "thus providing the Free World with one of its most valuable instruments in the defense of freedom."

1964

The Medal of Freedom, presented by President Lyndon B. Johnson in ceremonies at the White House. The highest civilian honor the President can bestow, this medal recognizes "significant contributions to the quality of American life." Kelly Johnson was cited for his advancement of aeronautics.

The Award of Achievement, presented by the National Aviation Club of Washington, D.C., for "outstanding achievement in airplane design and development over many years, including such models as the Constellation, P-80, F-104, JetStar, the U-2 and climaxed by the metallurgical and performance breakthroughs of the A-1 1 (YF-12A)."

The Collier Trophy (his second), following his work on the 2,000 mph YF-12A interceptor. Johnson's achievement for the previous year was called the greatest in American aviation.

The Theodore von Karman Award (his second), presented by the Air Force Association for his work with the A-11 (YF-12A) interceptor.

Honorary degree of doctor of engineering, University of Michigan.

Honorary degree of doctor of science, University of Southern California.

Honorary degree of doctor of laws, University of California at Los Angeles.

1965

San Fernando Valley Engineer of the Year, so designated by the San Fernando, California, Valley Engineers' Council.

Elected a member of the National Academy of Engineering.

Elected a member of the National Academy of Sciences.

1966

The Sylvanus Albert Reed Award (his second), given by the American Institute of Aeronautics and Astronautics "in recognition of notable contributions to the aerospace sciences resulting from experimental or theoretical investigations."

National Medal of Science, presented by President Lyndon B. Johnson at the White House.

The Thomas D. White National Defense Award, presented by the U.S. Air Force Academy in Colorado Springs, Colorado.

1967

Elected an honorary fellow of the American Institute of Aeronautics and Astronautics.

1968

Elected a fellow of the Royal Aeronautical Society.

1969

The General William Mitchell Memorial Award, presented by the Aviators Post 743 of the American Legion at Biltmore Hotel, Wings Club, February 14.

1970

The Spirit of St. Louis Medal by the American Society of Mechanical Engineers.

On behalf of Lockheed's Advanced Development Projects facility, which Johnson directed until his retirement in 1975, he accepted the first Engineering Materials Achievement Award of the American Society of Metals. Lockheed's ADP program "took titanium out of the development phase into full production for aircraft application."

The Engineering Merit Award presented by the Institute for the Advancement of Engineering, Beverly Hills, California.

Honored by the Air Force Association, Washington, D.C., for Johnson's design of the P-38 Lightning.

1971

The Sixth Annual Founders Medal of the National Academy of Engineering in recognition of his fundamental contributions to engineering.

1972

The Sliver Knight Award by the Lockheed Management Club of California for his contributions to Lockheed's success.

The first "Clarence L. Johnson Award" by the Society of Flight Test Engineers for his contributions to aviation and flight test engineering.

1973

Civilian Kitty Hawk Memorial Award by the Los Angeles Area Chamber of Commerce for outstanding contributions to the field of aviation.

1974

The Air Force Exceptional Service Award for his many outstanding contributions to the U.S. Air Force from 1933 to 1974. Presented by Secretary of the Air Force John McLucas.

Enshrined in the National Aviation Hall of Fame in Dayton, Ohio, for his outstanding contributions to aviation.

1975

Awarded the Wright Brothers Memorial Trophy for vital and enduring contributions over a period of forty years to the design and development of military and commercial aircraft.

1978

Sponsored by the American Institute of Aeronautics and Astronautics, "A Salute to Kelly Johnson" night—an hour-long multimedia presentation of his career highlights.

1980

Bernt Balchen Trophy, the highest award of the New York State Air Force Association, presented annually to "an individual of national prominence whose contribution to the field of aviation has been unique, extensive or of great significance."

1981

The Department of Defense Medal for Distinguished Public Service, presented by Defense Secretary Harold Brown.

Elected a fellow of the Society of Automotive Engineers for "his abilities to motivate a small staff to work within a tight time frame and budget in creating revolutionary aircraft designs."

The "Kelly Johnson Blackbird Achievement Trophy" was created by the USAF to "recognize the individual or group who has made the most significant contribution to the U-2, SR-71 or TR-1 program since the previous annual reunion."

The Daniel Guggenheim Medal "for his brilliant design of a wide range of pacesetting, commercial, combat and reconnaissance aircraft, and for his innovative management techniques that developed these aircraft in record time at minimum cost."

1982

Meritorious Service to Aviation Award from the National Business Aircraft Association, recognizing his designs of more than forty aircraft, including the world's first business jet, the JetStar.

1983

The Howard Hughes Memorial Award for 1982, presented by the Aero Club of Southern California in joint sponsorship with the Marina City Club. Recipient is recognized as a leader in aviation who has devoted a major portion of his life to the pursuit of aviation as a science and an art.

The National Security Medal, presented by President Ronald Reagan for exceptional meritorious service in a position of high responsibility and for outstanding contribution to the national security of the nation.

1984

Appointed Royal Designer for Industry, an honor originally established in 1936 by the British Royal Society of Arts recognizing designers who have attained eminence, efficiency, and visual excellence in creative design for industry. Limited to 100 recipients, Johnson was the seventy-second to receive the appointment. Diplomas are issued under the authority of the Council of the Royal Society of Arts.

1985

Honored by the Smithsonian's Air and Space Museum with an exhibit recognizing him as one of the founding fathers of the jet age. The exhibit ran for one year and was viewed by an estimated 16 million people.

Installed in the American Institute of Aeronautics's "1985 Aerospace Pioneer Hall of Fame," honoring him for his distinguished career in aerospace.

1986

Recognized by titanium producers association for the "earliest large-scale use of titanium in an aircraft primary structure."

1987

The Lord Medal for "Leadership in Wealth Creation," for "contributions to the development of products that add to the civilized aspects of human societies."

1988

The National Medal of Technology for "outstanding achievements in the design of a series of commercial, military and reconnaissance aircraft that have incorporated a wide range of technological advancements."

Inducted into the Michigan Aviation Hall of Fame in recognition of his many outstanding contributions to the field of aviation.

1990

National Air and Space Museum Trophy from the Smithsonian Institution "in recognition of extraordinary service in aviation, space science, and technology" and for the SR-71, a "past achievement that has contributed significantly to advancing aerospace activities."

1991

National Management Association Hall of Fame.

PATENTS

Kelly Johnson received forty-four U.S. patents. Some of the more important ones are listed below.

1939	Design for Airplane Model 27 (D-116,094).
1940	Design for Airplane Model P-38 (D-119,714).
1943	Anti-Icing Duct for Model 12 and P-38 (2,320,870).
1946	Design for Airplane Model P-80 (D-143,822).
1947	Auxiliary Fuel Tank for Model P-80 (2,421,699).
1954	Airplane Design for Model C-130 (D-172,969).
1956	Afterburning Means for Turbo-Jet Engines (2,771,740). Airplane Design for Model F-104 (D-179,348).
1957	Airplane with Variable Swept Wings (2,794,608). Landing Drag Flap and Lift Spoiler (2,791,385).
1958	Jet Utility Transport (D-183,657).
1959	Turbine Engine Blow-Out Preventer (2,870,684).
1960	Aircraft Propulsion Systems (Jet Flap) (2,928,627).
1961	Airplane Design for Model JetStar (D-191,243).


Harold L. Johnson

Harold Lester Johnson

April 17, 1921-April 2, 1980

By Gérard H. De Vaucouleurs

Harold Johnson, one of the most productive and influential observational astrophysicists of this century, was born in Denver, Colorado, on April 17, 1921, the son of Averill C. and Marie (Sallach) Johnson. He received his elementary and secondary education in Denver schools and went on to the University of Denver, receiving the B.S. degree in mathematics in 1942. His correspondence makes it clear, however, that his mind was already set on becoming an astronomer.

WAR YEARS AND GRADUATE STUDIES, 1942-48

Graduating with a strong physics background shortly after the entry of the United States in the Second World War, Johnson was immediately recruited by the Radiation Laboratory at the Massachusetts Institute of Technology, where he worked on radar interference techniques. Here he met Albert Whitford, an astronomer then applying electronic techniques to photoelectric measurements of the light of stars. Toward the end of the war years Johnson moved to the Naval Ordnance Test Station, Inyokern, California, where he worked with Gerald Kron, also an astronomer engaged in the photoelectric photometry of stars.

After the war, with Kron's support and encouragement, Johnson began graduate studies in astronomy at the University of California in Berkeley. He completed his thesis work in the remarkably short time of two years and received his Ph.D. in 1948. His thesis adviser was Harold Weaver, and his thesis project involved the development of an electronic plate measuring machine. Most of the work was done at Lick Observatory on Mount Hamilton, where further association with Kron turned Johnson's attention to photoelectric photometry. His first two papers, published in 194849 in *The Astrophysical Journal* and the *Publications of the Astronomical Society of the Pacific,* dealt with electronic circuitry and with the ultimate sensitivity limit set by quantum noise. These marked the beginning of a lifetime of dedication to high-precision astronomical photometry, a field of which he was to become the leading practitioner.

RESEARCH AT LOWELL, WASHBURN, AND YERKES OBSERVATORIES, 1948-59

Upon completion of his work for the Ph.D., Johnson accepted an offer to join the staff of Lowell Observatory, Flagstaff, Arizona, where he spent the second half of 1948, building an AC electronic amplifier for the observatory solar variation project under a Weather Bureau contract. This first AC amplifier did not work well, however, and it was eventually replaced by a standard DC amplifier. This initial period at Lowell Observatory did not measure up to Johnson's expectations either, and at the end of 1948 he moved to Washburn Observatory as assistant professor at the University of Wisconsin in Madison. There he joined the project started by Joel Stebbins and Albert Whitford to establish a photoelectric calibration for the stars in selected areas then used as standards for the magnitudes of Cepheid variables in local group galaxies. Johnson's expertise was important in extending the sequences to fainter limits. This study showed conclusively that the previous photographic calibration erred by nearly a magnitude (a factor of more than 2 in intensity) at the faint end. The corrections found necessary entered into the ensuing revisions of the cosmic distance scale, which put external galaxies considerably farther away than they had appeared to be in the first estimates. This work confirmed Johnson's determination to push the photoelectric technique to its extreme limits, which he did very successfully over the next ten years.

Johnson moved then to the Yerkes Observatory of the University of Chicago, where he was an assistant professor for two years (1950-52). There he met William W. Morgan, then the leading expert in stellar spectral classification and with whom he would soon begin a momentous research on the combined photometric and spectroscopic properties of stars which, as will be explained below, led to revolutionary advances in astrophysics. Neither the teaching profession nor the murky skies of the Chicago area were much to Johnson's liking, however, although he had access for his observations to the clear skies of McDonald Observatory in West Texas, which was then under the management of the Chicago astronomers. At any rate, upon learning that veteran Lowell astronomer C. O. Lampland (1873-1951) had passed away, Johnson revisited Flagstaff in early 1952 and convinced the elderly director, Vesto M. Slipher, that a second try at using his talents would be in the best interest of the observatory. This was agreed and in August 1952 Harold Johnson returned as staff astronomer to Lowell Observatory, where free of teaching duties and under a favorable sky, he could devote full time to his efforts to push stellar photoelectric photometry to its ultimate limits.

Prior to 1950 photoelectric photometry had been a difficult technique reserved to a few skilled practitioners. In these early years the charge generated over a specified time interval was measured with an electrometer, a delicate device transplanted from the physics laboratory to the focal plane of a moving telescope. Paul Guthnick in Germany and Joel Stebbins in the United States were among the few to have successfully mastered the technique. The charge sensitivity fell far short of reaching the quantum noise limit. The thermionic amplifier introduced by Whitford in 1930 considerably increased the sensitivity of photoelectric observations. The development of the electrostatically focused multiplier phototube by RCA in 1940 eliminated the need for a low-level amplifier and provided better cathode response. The greatly simplified technique encouraged a larger number of astronomers to enter the field, including those with smaller telescopes.

Photoelectric photometry was thus just coming of age when Johnson began his two-year period at Yerkes Observatory in 1950. He realized that the inherent linearity of the photoelectric process (favorable for direct subtraction of the superimposed light of the night sky—a large correction when observing faint stars) had to be preserved and that all fluctuations introduced by the equipment had to be reduced to a level below that of the unavoidable statistical fluctuations in the number of photoelectrons released by starlight. He also realized the need for better definition of the measured quantity, the stellar magnitude of the observed object. This required knowing the spectral sensitivity function of the system, including all the optics in front of the detector (atmosphere, telescope, filter).

Early photoelectric observers had attempted to match their measured magnitudes to the international photographic and photovisual systems adopted by the International Astronomical Union in 1922, following extensive measures of a set of stars near the north celestial pole, the "North Pole Sequence," by many astronomers at several observatories and especially by Frederick H. Seares at Mount Wilson Observatory. The photovisual system, based on orthochromatic emulsions and a yellow filter, was intended to match the average sensitivity of the human eye, the first "instrument" used to estimate stellar magnitudes. With a suitable color filter in front of the photocell, a reasonably satisfactory match could be obtained.

Attempts to match the international photographic magnitudes were complicated by the wide spectral range covered by blue-sensitive photographic emulsions, a difficulty that had led to poor agreement between magnitudes measured with different telescopes; the ultraviolet cutoff was dependent on absorption by the flint glass component in refractors or on the falling reflectivity of the silvered mirrors in reflectors. After 1933 the change from silver to aluminum coatings, with their higher UV reflectivity, led to further complications.

Johnson recognized that these difficulties arose from inclusion to a varying degree of the near ultraviolet around l 0.37-0.38 microns, where the energy distribution in stellar spectra varies rapidly. He demonstrated that a reproducible magnitude system could be established by placing a suitable filter before the photographic plate or photocell to block all wavelengths shorter than l 0.38 microns. Also, the overall shape of the spectral energy distribution could be better defined by using, in addition to the yellow and blue bands, a third color in the ultraviolet near the head of the Balmer series of hydrogen and thus sensitive to the size of the Balmer jump, a measure of the temperature and density in stellar atmospheres. In collaboration with W. W. Morgan and D. L. Harris, Johnson thus introduced a new standard system of stellar photometry, the U, B, V system, based on ten primary standard stars and, initially, 108 secondary stan dards well distributed around the northern and equatorial zones of the sky accessible from the McDonald and Lowell observatories where his observations were made. The new system was published in epoch-making papers in *The Astrophysical Journal* in 1953-54 and in *Annales d'Astrophysique* in 1955.

The system was rapidly adopted and quickly became the de facto international standard of stellar (and, later, galaxy) photometry, a role it has retained to this day. Johnson, alone or in collaboration with Allan Sandage, Richard Mitchell, Braulio Iriarte, and others, made massive applications of this system to galactic clusters, producing well-defined, precise color-magnitude and color-color diagrams, that is, plots of apparent V magnitude versus B-V color indices (resembling the Hertzsprung-Russell diagram of luminosity versus effective temperature) and of U-B color versus B-V. He established the fundamental properties of these diagrams and showed how to use them to disentangle the effects of temperature (intrinsic) and interstellar reddening (extrinsic). After his move to Flagstaff he demonstrated how clusters of different ages have characteristically different color-magnitude diagrams and, in particular, how the "turn-off" point where the stars begin to depart from the "zero-age" main sequence can be used to estimate fairly precisely the ages of the clusters. This striking observational confirmation of theoretical modeling, initially by Martin Schwarzschild, of the paths that stars follow on the color-magnitude diagram in their post-main-sequence evolution opened the way to many investigations of stellar and cluster ages that are continuing to this day. For this work Harold Johnson was awarded, in 1956, the Helen B. Warner Prize by the American Astronomical Society.

During this period in Flagstaff, Johnson was also one of the first (simultaneously with William Baum at Palomar) to push the sensitivity of photoelectric photometry to the quantum noise limit by developing pulse-counting photometers, essentially counting photoelectrons one by one. In his pursuit of the ultimate precision, he also built what was probably the first two-channel photometer, one measuring the star under study and the second simultaneously measuring through an identical aperture the variable luminosity of the night sky in a nearby spot, thus eliminating, by difference, the troublesome fluctuations of the night airglow. By rapidly reversing the roles of the two channels, any systematic difference between the two optical trains and photomultipliers was neatly eliminated. Johnson developed convenient forms to facilitate (in those precomputer days) the reduction of photoelectric observations, designed an ingenious analog-to-digital device to measure the star and sky deflections on chart records of the photo-current, and defined the rigorous procedures to be followed to obtain the highest precision in this type of observation.

During his second period on the Lowell staff, Johnson was also actively engaged with Aden B. Meinel in the site survey for the future National Optical Astronomical Observatory (NOAO), which was eventually built on Kitt Peak near Tucson.[1] In 1956 he actually spent six months in Phoenix, where the initial NOAO office was located at the time. He was even considered by the first director, Leo Goldberg, for a top research position in the new organization but eventually decided to return full time to Flagstaff at the end of 1956.

The author of this memoir was fortunate to become Johnson's colleague and friend during his stay at Lowell Observatory in 1957-58, when he began a long-term program of galaxy photometry in the U, B, V system, initially with Johnson's photometer attached to the 21-inch reflector. This program, since continued at McDonald Observa tory (and elsewhere by many others), has provided the basis for the most generally used systems of total magnitudes and colors of galaxies.

AT THE UNIVERSITY OF TEXAS, 1959-62

Toward the end of 1959 Johnson accepted an invitation of Gérard Kuiper, director of the Yerkes and McDonald observatories, to join, as professor of astronomy, the newly formed Department of Astronomy of the University of Texas at Austin. He became briefly its chairman in 1961-62 after Kuiper relinquished his directorship and the chairmanship of the joint Chicago-Texas department to move to the University of Arizona in Tucson, where Johnson was soon to follow him.

Johnson's years at Texas were very productive in the sense that he developed and used much new equipment at McDonald Observatory, but frustrating because he failed to receive from the university administration whole-hearted support for the kind of research and development he wanted to give to the department and, especially, the observatory. He was more interested in the directorship of the observatory than the chairmanship of the department, but the administration saw things differently.

It is during this period in Texas that Johnson first came in contact with Frank Low, who was then a physicist with Texas Instruments in Dallas, where some very sensitive infrared detectors were being developed. Johnson immediately seized on this opportunity to build a photometer extending the U, B, V system to the longer wavelengths of the near infrared, the R, I, J, K, and L bands out to 4 microns. With Low's germanium bolometer, this was later extended to the N band at 10.2 microns. The ability of the longer wavelengths to better penetrate the selectively obscuring interstellar haze opened new avenues of research. The longer wavelength also gave more information on the radiation from cool stars, which, by Wien's displacement law, is mainly emitted in the infrared and is heavily blanketed by molecular absorption bands in the visible. The results were first reported in *The Astrophysical Journal* in 1962.

It was also during his stay in Austin that Harold Johnson became aware of the remarkable work of Larry Mertz, at Harvard Observatory, where he had built the first experimental Fourier-transform interference spectrometer (195859). This author, who was there at the time, remembers vividly how the Harvard faculty failed to appreciate the significance of what Mertz was doing and dismissed him as a mere "tinkerer." Made aware of Mertz's work, Johnson immediately grasped the enormous potential of interference spectrometry, particularly for the infrared, and before leaving Texas proceeded to build the first successful Fourier-transform stellar interferometer working in the near infrared. He was to greatly improve and develop this technique after his move to Arizona.

AT THE LUNAR AND PLANETARY LABORATORY, UNIVERSITY OF ARIZONA, 1962-69

In February 1962 Johnson accepted Gérard Kuiper's invitation to follow him to join the newly created Lunar and Planetary Laboratory (LPL) at the University of Arizona in Tucson, where he served as a research professor (1962-67) and then associate director (1967-69). Although Johnson did make some applications of his early version of the Fourier-transform spectrometer to the infrared spectra of the major planets, he was free to pursue his main line of interest—namely, the infrared photometry and interference spectroscopy of stars. He used to joke that he was the "stellar division" of the Lunar and Planetary Laboratory. It is fortunate for astronomy that Kuiper was a far-seeing scientist able to accept on his staff a gifted and productive individual, even if he was not working on the main line of interest to the institution. [2]

Among the many contributions made by Johnson in this atmosphere of freedom under the favorable skies of southern Arizona, where Kuiper had established a fine high-altitude observatory in the Catalina Mountains northeast of Tucson, we may mention major papers on the infrared photometry of late-type and carbon stars, studies of atmospheric and interstellar extinction, massive catalogs of eight- and later thirteen-color photometry of bright stars (in collaboration with R. I. Mitchell, W. Z. Wisniewski, and B. Iriarte) published in the *Astrophysical Journal* and *Communications of the Lunar and Planetary Laboratory* between 1962 and 1969. Johnson, not satisfied with the differential measurements of stellar magnitudes, also undertook the more difficult task of performing an absolute calibration of stellar magnitudes in terms of energy fluxes. He was thus able to produce absolute energy curves, eventually extended up to 20 microns, for all sorts of important categories of stars: subdwarf stars, cepheids, M stars, carbon stars, infrared objects, and even circumstellar shells.

One of the most important discoveries made by Johnson during this period was the great intensity of the infrared emission of the prototype quasar, 3C273, surpassing even its visible emission. This result was soon found to be a general property of the radiation from quasars and other active nuclei of galaxies.

During the same period Johnson utilized the accumulated observational data on the spectral energy distributions of stars of all types to derive a new set of bolometric corrections to their visual magnitudes and then to establish a revised temperature scale more directly based on observed energy distributions. He also used observations of reddened stars in galactic clusters to discuss the wavelength dependence of extinction by interstellar dust. The infrared excess at the longest wavelengths in certain clusters over the normal absorption was, in the end, identified as radiation from warm circumstellar dust shells, rather than abnormal properties of the interstellar dust particles. The fundamental contributions were summarized in classical review papers in *Annual Reviews* and in the standard compendium *Stars and Stellar Systems,* where Johnson also described in some detail the construction of his stellar photometers. Many photometers built after Johnson's design are still in use around the world. Johnson's election to the National Academy of Sciences in 1969 recognized his major influence on the progress of astronomy during the previous twenty years.

During this same period also Johnson began to be interested in the design and construction of low-cost medium-size reflectors, specially designed for infrared photometry and Fourier-transform spectroscopy. An experimental 60-inch reflector made of spun aluminum was successfully built and tested under his direction at the Catalina station. It was later transferred to the Mexican National Observatory in Baja California. The "poor" optical quality (by ordinary standards) was quite good enough to feed most of the energy of the infrared image of a star into the rather large entrance aperture of the photometer or spectrometer. His ideas of building cheap telescopes for specialized tasks were not without their detractors, but Johnson was more interested in doing "great" science than "big" (meaning expensive) science. If results of equal significance could be gotten more cheaply, he would prefer the latter, a case of brain versus brawn, which proved itself a decade later in his contributions to Mexican astronomy.

THE ARIZONA-MEXICO CONNECTION, 1969-80

Harold Johnson had for many years collaborated with Mexican astronomers Braulio Iriarte and Eugenio Mendoza, among others, informally assisting them in their research with advice and the loan of photometers. In 1969 this association was formalized when he became a part-time member of the scientific staff of the Institute of Astronomy of the National University of Mexico, eventually becoming a full-time professor in 1979 when he actually moved to live in Mexico City. In 1973 he was one of the founders and in 1975 became head of the Department of Applied Physics of the newly created Center for Scientific Research and Higher Education in Ensenada, Baja California Norte. He maintained, however, his ties to Arizona, where in 1969 he transferred to the Optical Sciences Center (then headed by Aden B. Meinel) as research professor and to the Steward Observatory (then headed by Bart J. Bok, 1906-83) as astronomer. He maintained this dual connection with Mexico and Arizona until the end.

Johnson's active involvement in Mexican astronomy began in 1964 with his participation in multicolor observations of bright stars with the 1-meter (40-inch) reflector of the Tonantzintla Observatory near Puebla. He helped with the search for the best location for the new national observatory. His support of a proposal by E. E. Mendoza to then director G. Haro, identifying a peak in the Sierra de San Pedro Mártir, Baja California, at an altitude of 2,800 meters (9,200 feet) among the pine trees of a protected national forest, as suitable for the proposed observatory, was crucial—according to Mendoza, "Without his help, no SPM observatory, most likely." Of special importance and of interest to Johnson was the low water vapor content of the atmosphere, making it very suitable for work in the infra red. The main telescope, with a 2.1-meter main mirror, embodying many of Johnson's ideas, was dedicated and began operation in 1979. In grateful recognition of his role in this project and in the development of Mexican astronomy in general, the National University of Mexico conferred him the degree of doctor honoris causa in 1979 and, after his untimely death the next year, the Universities of Mexico and Arizona named after him the 1.5-meter aluminum-mirror infrared telescope Johnson had brought from Arizona to Mexico. Symposium 96 of the International Astronomical Union on Infrared Astronomy held in 1980 was dedicated to Harold Johnson's memory.

Building on the laboratory facilities he had in Tucson, where he remained active during his Mexico years, Johnson perfected a high-resolution Fourier-transform infrared spectrometer, using as its core a Michelson interferometer built by the Block Engineering Company of Boston, under the direction of Larry Mertz. Johnson and his associates, F. F. Forbes, R. I. Thompson, and D. L. Steinmetz of the University of Arizona and O. Harris of the National University of Mexico, used it on several telescopes in Arizona and on the NASA Lear jet stratospheric observatory to produce high-resolution spectra of the sun and bright stars in the spectral range of 1.0 to 4.0 microns (stars) and 5.6 microns (sun). The results were first reported in the *Publications of the Astronomical Society of the Pacific* in 1973. Later the results were collected in a comprehensive *Atlas of Stellar Spectra.* The resolution of 0.5 centimeters^{-1} corresponds to about 0.1 angström unit in the middle of the range, possibly the highest resolution ever achieved on astronomical sources in this spectral region. The tracings of the infrared spectra of bright stars and planets, displayed in Johnson's Tucson laboratory, covered several tens of meters on the walls!

Toward the end of his life Johnson became increasingly interested in the design and construction of mirror arrays, that is, multiple-mirror telescopes, as a way to realize large apertures at a fraction of the cost of solid monolithic mirror telescopes. In this field too he was a pioneer, and he proposed to build for Mexico an array of twenty 2-meter telescopes —"Mextels," as he called them—to provide the light-gathering power of a 10-meter telescope at half the cost. A prototype was built and installed at the national observatory site. The multimirror scheme has now been widely accepted and implemented around the world as the way to build super-large telescopes.

HAROLD JOHNSON AS FRIEND AND COLLEAGUE

Little is known of the private life of Harold Johnson. He was married to Mary Elizabeth Jones in 1954. They had two children: August Harold and Selma Marie. After Harold's untimely death in 1980 in Mexico City, Mary returned to Tucson, where she lives in retirement.

To those who did not know him well, Harold Johnson may have seemed to be often blunt, brusque, and lacking in the suave polish necessary to become a successful academic. He did not care much for formal teaching. It may be true that as a colleague Johnson was occasionally difficult to live with, but it was well worth the effort. He suffered all his life from breathing problems that got worse with time and may well have influenced his personality. But he was a fundamentally honest man, with a strong religious bent (once he even attended a revival meeting) and a profound dedication to the truth in science as well as everyday life. He was impatient with mediocrity, and all his life was dedicated to striving for the ultimate precision and exactitude in his several fields of endeavor, in each of which he made fundamental contributions. He was always willing, even eager, to share his profound knowledge of photometry and electronics with students, associates, and colleagues. He had an extraordinary skill in designing and building new instruments; he had "electrons in his finger," as an envious and admiring competitor remarked once. He knew better than anyone how to build amplifiers whose response was linear over an enormous range of intensities; he built one at McDonald Observatory with which he could measure with the 2.05-meter reflector without loss of linearity from Sirius to 22-nd magnitude stars—that is, an interval of 2.5 billions to 1 in light flux.

EPILOGUE

Harold Johnson brought extraordinary instrumental and electronic talents to devising equipment that utilized to maximum advantage newly developed photoelectric detectors as they became available. His measurements of the colors and magnitudes of stars in galactic clusters on the precisely defined system he devised in collaboration with W. W. Morgan led to age determinations that opened the way to exploitation of the color-magnitude diagram as a diagnostic in studies of stellar evolution. He had a leading role in the use of new infrared detectors in the photometry of stars and galaxies. With his colleagues he measured thousands of stars that became reference standards.

Johnson applied these measurements to calibrate spectral energy distributions of stars and thus provide an improved observational basis for the stellar temperature scale and the bolometric corrections to visual magnitudes. He was the first to apply Mertz's concepts to build practical stellar Fourier-transform spectrometers; for cool stars, in particular, these gave unsurpassed resolution in the infrared. These fundamental contributions to observational astrophysics constitute Harold Johnson's enduring scientific legacy.

The twenty-five titles in this memoir's selected bibliography are among the most important of some 135 papers published by Johnson between 1948 and 1980, but many of the others were no less significant and influential. His last two papers appeared posthumously in 1981 in the proceedings of the symposium "Recent Advances in Observational Astronomy (UNAM, 1981)," which he helped organize. He died of a heart attack in Mexico City on April 2, 1980.

This memoir has benefited enormously from the generous collaboration of Albert Whitford, who not only provided his own reminiscences of Harold Johnson's early scientific contributions but also communicated copies of letters in the Lick Observatory Mary Lea Shane archives and secured a valuable testimony from Gerald E. Kron, all referring to Johnson's period as a graduate student at Lick and Berkeley. Whitford kindly revised, corrected, and enlarged several sections of this memoir but modestly declined to be named as a coauthor. I am deeply grateful for his contribution. I also acknowledge valuable communications from H. C. Giclas, Lowell Observatory; E. E. Mendoza V, University of Mexico; and W. Z. Wisniewski, University of Arizona at Tucson. The frontispiece photograph of Harold Johnson, taken in 1965 at LPL by D. Milon, was kindly provided by E. A. Whitaker, University of Arizona at Tucson.

NOTES

1. The writer remembers that Johnson's own preference for the site of the new observatory was an isolated peak, Slate Mountain, in the desert northwest of Flagstaff as a better, darker, and dryer site than Kitt Peak and likely to remain free of light pollution for many years, but practical considerations of accessibility, development costs, and living convenience for the staff prevailed in the end.

2. A very readable account of the founding and early years of LPL written by Ewen A. Whitaker (University of Arizona, 1985) includes a section on Johnson's contributions.

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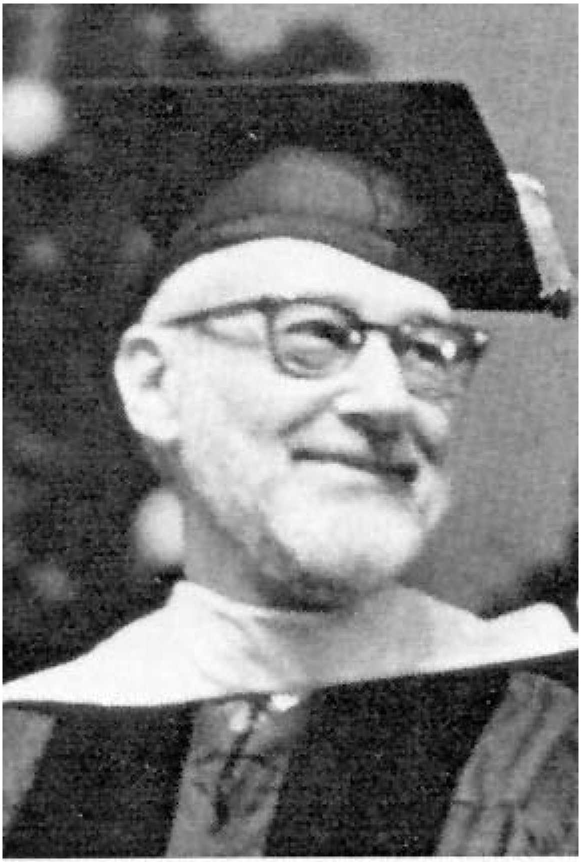

Tjalling C. Koopmans

Tjalling Charles Koopmans

August 28, 1910-February 26, 1985

By Herbert E. Scarf

Tjalling Charles Koopmans, one of the central figures in modern economic science, played seminal roles in the modern theory of the allocation of scarce resources and in the development of statistical methods for the analysis of economic data. In both of these areas Koopmans creatively mobilized and developed the methods of other quantitative disciplines for the purposes of economics: mathematical statistics became econometrics, and linear programming became the activity analysis model of production. Koopmans was also one of the major scholars concerned with the study of economic growth and the economic consequences of the depletion of nonrenewable resources. He was a remarkably inspired and inspiring leader of research who combined his considerable mathematical power with a deep concern for the ultimate practical applications of his work.

Koopmans was born in the village of 's Graveland, near the town of Hilversum, in the Netherlands, on August 28, 1910; he was the third son of Sjoerd Koopmans and Wijtske van der Zee. Both his mother and father were born in Frisia, a province in northeastern Holland. Sjoerd's father was the owner of a small shop in the rural area of Toppenhuizen; Wijtske's father was a painter of fancy carriages and also an artist who painted many landscapes and portraits, now owned by his great-grandchildren. The family in which Sjoerd grew up was severe and Calvinistic, in contrast to Wijtske's family, which was more relaxed and liberal about religious matters. At the age of sixteen Sjoerd became the schoolteacher of a small school in Toppenhuizen and was entrusted with the education (including bible instruction) of the neighborhood children. He was said to have been very stern in the classroom, perhaps as a consequence of the many responsibilities he assumed at so early an age. Wijtske was also trained as a schoolteacher and, after their marriage, the couple left Frisia and eventually settled in 's Graveland, where Koopmans's father became the principal of a much larger "school with the bible."

The family house, as Koopmans described it in an autobiographical sketch written when he received the Nobel Prize in economic sciences in 1975,

> . . . was squeezed between two sections of that school. The row of these three buildings was, as [were] almost all houses in the village, sandwiched between one long street and a parallel straight and narrow canal marking one of the village's boundaries. Across the street were large wooded estates each with meadows and a large mansion. The occupants of the mansions kept aloof from the life of the village except for the employment of coachmen, gardeners, servants and contractors.
>
> Every weekday morning at nine, our living quarters and the narrow strip of garden at the back were engulfed by the sound of three different hymns sung dutifully, simultaneously, but independently in true Charles Ives fashion, by the schoolchildren on both sides.

Despite frequent illnesses Koopmans had a happy childhood in this rural environment, with its many meadows and canals. His formal education began at his father's school, with its heavy emphasis on biblical studies, and was followed by five years at the Christian High School at Hilversum, some ten miles away. At the high school Tjalling studied

Latin, Greek, mathematics, physics, chemistry, and three modern languages. He was instructed in the theory of evolution by a teacher who remarked at the end of the course, "the Bible says otherwise."

The Koopmans family was very musical, and sang together regularly. Sjoerd played the harmonium, and Tjalling was taught the violin as a child. He was not entirely satisfied with his skill on this instrument, and in his later life he replaced the violin with the piano. Both secular and sacred learning were highly valued in the Koopmans household. There were prayers before every meal and Bible reading in the evening, with the servants called from the kitchen to collect around the dinner table and participate in religious instruction. Tjalling's father was the dominant influence, and the atmosphere in the home and the school was a stern and disciplined one.

Tjalling left home for the University of Utrecht at the age of seventeen. At Utrecht, boarding was arranged with the minister to the city prisons, whose surname was Couvée. This was an experience very different for Koopmans from living at home; there were many young children, some close to Tjalling in age, and much lively social activity. Due to his post the father of the family had seen a good deal of the raw life of the city, and, while religious, he was not strict nor dogmatic. The mother was French, and Tjalling became quite comfortable with the language. He stayed with the family for two years.

It was customary for a young man to take formal religious vows at the age of seventeen or eighteen. Koopmans wrestled with the issue for a considerable period of time, and, in what was a difficult experience both for himself and his parents, he formally renounced his ties to the Protestant faith while at the university. But the moral and educational values of his early home remained with him and were probably the central source of the great personal integrity and strong sense of purpose that he displayed throughout his lifetime.

Koopmans's academic abilities must have been apparent quite early, for he was awarded a generous stipend by a private foundation—the St. Geertruidsleen—at the age of fourteen. This scholarship supported his studies until his twenty-sixth birthday and relieved his family of the financial burden of his education. At the university Koopmans commenced with the study of mathematics—in particular, analysis and geometry. He had a vivid geometrical intuition, and, in many of his subsequent publications, elaborate analytical arguments are frequently simplified by the use of insightful geometrical figures. He read widely in other subjects, ranging from physics to history, psychology, and psychiatry. For a while he contemplated entering the profession of psychiatry, but, in a somewhat less dramatic change of field, he moved (in 1930) from pure mathematics to theoretical physics. This shift in subjects, a first step toward his eventual decision to take up economics, was "a compromise between my desire for a subject matter closer to real life and the obvious argument in favor of a field in which my mathematical training could be put to use."

Koopmans's professor at Utrecht was Hans Kramers, the leading theoretical physicist in Holland at the time. He admired Kramers enormously and described him as "a humane and inspiring person with a gentle wit." In 1933 Koopmans wrote an important paper on quantum mechanics, which is still frequently cited by physicists many years after its publication. But, of course, these were the years of the Great Depression, and theoretical physics must have seemed remote from the distress of daily economic life. As Koopmans later said, "It dawned on me that the economic world order was unreliable, unstable, and most of all, iniq uitous." He began, at the suggestion of fellow students, to read the works of Karl Marx; this was his first exposure to abstract economic reasoning. While he was not persuaded by Marxian economic analysis, he felt deeply moved by Marx's description of the plight of workers during the Industrial Revolution.

It was at this point that Koopmans was introduced to Jan Tinbergen, who was seven years older and already one of the leaders in the new field of mathematical economics. Tinbergen, who was to share the first Nobel Prize in economic science with Ragnar Frisch in 1969, had been trained in mathematical physics as a student of Ehrenfest. He had been a conscientious objector to military service at the age of eighteen and, as an alternative obligation, was required to spend some time at the Statistical Office in the Hague, where he became acquainted with and concerned about social and economic issues. Despite his change in interest Tinbergen continued to work with Ehrenfest; his Ph.D. thesis, written in 1929 at Leiden, was on the topic of minimization problems in both physics and economic theory. After receiving his degree Tinbergen began to develop the elements of a mathematical theory of business cycles and to construct a formal mathematical model of the Dutch economy.

Koopmans decided to affiliate himself with Tinbergen. He moved from Utrecht to Amsterdam in January of 1934 and joined a group of Tinbergen's young disciples, among them Truus Wanningen, whom Koopmans was to court and, finally, marry in October 1936.

Tinbergen offered a weekly lecture in economics, which Koopmans attended. As he later said in his Nobel biographical sketch,

> In the first half of that year [1934], I had the privilege of almost weekly

> private tutoring from him over lunch after his lecture. I have been deeply impressed by his selflessness, his abiding concern for economic well-being and greater equality among all of mankind, his unerring priority at any time for problems then most crucial to these concerns, his ingenuity in economic modeling and his sense of realism and wide empirical knowledge of economic behavior relations.

Tinbergen instructed Koopmans in many aspects of mathematical economics and econometrics. He suggested that Koopmans read the works of the theorists Cassel and Wicksell and that he become familiar with the field of statistics and its applications to economic problems.

Tinbergen had a profound influence on Koopmans's professional career, and it may be useful to make a brief digression about Tinbergen's work on business cycles and macroeconomic models. In order to place this work in perspective, let me describe a fundamental distinction between two attitudes toward dynamic models in economic theory. We are all familiar with the basic idea that prices are determined so as to equate the supply and demand for goods and services. In its most elementary form, the demand for a particular commodity may be thought of as a function of its price (and perhaps the prices of other competing commodities) and demand declines as the price rises. Similarly, the supply brought forth by producers of a particular commodity may be viewed as a function of the price at which the commodity may be sold (and the prices of the factors of production required to manufacture the commodity); typically, the supply of a commodity rises as its price increases. The static equilibrium price is at the intersection of these two curves.

Suppose that we wish to examine a dynamic variant in which the commodity is produced and consumed at a sequence of consecutive points of time. On the one hand, we can imagine that the production and consumption deci sions are made in the presence of perfect futures markets and with the full knowledge of the prices that are expected to prevail over time. Making use of this information, producers purchase factors of production and consumers purchase outputs at times when they are inexpensive and store them for future use, seeking to smooth their production and consumption plans over time. On the other hand, we can imagine that the imperfections of financial institutions require that such choices be made in a myopic fashion, attending only to those prices and values of other significant economic variables that prevail today.

In the first version, prices would clear both spot and futures markets instantaneously; the model would describe an economic situation of full dynamic equilibrium with no underemployment of resources. In the latter variant, markets would respond sluggishly to previous signals and the evolution of the economy might best be described by a mathematical system in which the future values of major economic variables are an extrapolation of their past values.

Clearly, the depression years of the early 1930s could not be accurately described by a classical model in which all economic resources are fully employed. Tinbergen was drawn to the alternative formulation, which had played an important role in the analysis of business cycles and which was ultimately to lead to the Keynesian model. For example, Tinbergen published a paper in 1931 in which cycles in shipbuilding are analyzed by means of a simple difference-differential equation stating that the increase in available shipping tonnage at a particular time is related linearly to the stock of tonnage with a fixed time delay. There is no explicit consideration of freight rates or the costs of constructing new shipping. Freight rates are examined in subsequent papers but not in the neoclassical manner as those prices that equilibrate the demand for shipping services with its supply. Instead, Tinbergen engaged in skillful curve fitting; he fitted a regression of freight rates to a pair of indices purporting to measure the demand and supply of shipping services and the cost of coal.

A number of themes that appear in these early works of Tinbergen became major influences in Koopmans's later research agenda. Tinbergen's concerns with the shipping industry were to stimulate Koopmans's subsequent interest in formal mathematical models of transportation. Tinbergen's use of statistical analysis opened up a series of questions that were to preoccupy Koopmans and other scholars for many years, and Koopmans's fundamental research in economic growth theory very probably had its roots in the early dynamic models of Tinbergen.

Koopmans's Ph.D. dissertation, titled "Linear Regression Analysis of Economic Time Series," was supervised jointly by Tinbergen and Kramers; the degree was granted in November 1936. In retrospect, this thesis can be seen as an important step in the development of modern econometric methodology. By the 1930s economists had already been exposed to the use of regression analysis and other statistical techniques in analyzing the relationship between the demand for a particular good and its price and in the study of business cycles. The parameters in Tinbergen's model of the Dutch economy had been estimated using multiple correlation analysis with a degree of care and detail not seen in previous economic reports, and Frisch had developed his own ingenious statistical methods. But the new paradigm for statistics offered by R. A. Fisher had not yet found its way into econometric analysis prior to Koopmans's thesis.

The major innovation suggested by Fisher was an assessment of the merits of various statistical methods based on a formal probabilistic model. To take an important example, consider a set of observations $(y_i,x_i)_{i=1,\dots,T}$ of a dependent variable y and an independent variable x. A linear relationship, $y = \alpha x + \beta$, between these two variables can be obtained by a least squares regression of y on x. But such a regression is essentially an exercise in curve fitting, and the parameters could equally well be found by other contending methods, such as one that minimizes the sum of the absolute values of the deviations, rather than the sum of their squares. In order to justify the use of one particular method, Fisher introduced an underlying probabilistic model that is assumed to generate the observed data. For example, assume that the observations y_i are independently drawn from normal distributions with means $ax_i + b$, and with a common standard deviation σ. Given the parameters a, b, and σ and the sequence of values of the independent variable $x = (x_1,\dots,x_T)$, the probability of observing the sequence $y = (y_1,\dots,y_T)$ can be expressed as a function $F(y|a,b,\sigma,x)$. For the observed sequence (y, x), Fisher suggests that the parameters a, b, and σ be selected so as to maximize this likelihood function, that is, to select those parameters that give the highest probability to the sequence of observed data.

Economic data are distinctly different in at least two very significant ways from those arising in the agricultural experiments that motivated Fisher's analysis. Economic data are similar to astronomical observations in the sense that they are natural observations that do not arise in experimental laboratories. The independent variables x, which might represent temperature and other experimental parameters in Fisher's controlled experiments could, in an econometric study, become the prices at which a sequence of commodity demands were observed. But even if prices were thought of as being independent variables in the sense that the price of food would cause a certain level of de mand for food to arise, these prices could not be set by the experimenter and would, themselves, be measured with error.

After an exposition of Fisher's program, Koopmans's thesis contains a lucid set of proposals for accommodating the particular econometric problem that all of the relevant variables might be measured with error. He does not, at this point, address a second major problem, that is, the fact that causal connections are far from obvious in economics and the values of many economic variables might very well be considered to be simultaneously determined. This is a point that will arise again.

In the period 1936-38 Tinbergen was called to the League of Nations at Geneva to find out, with the aid of statistics, which theory of the business cycle was closest to reality. At Geneva Tinbergen also prepared a business-cycle model of the United States. Koopmans took over the teaching of his class in mathematical economics at the Netherlands School of Economics in Rotterdam. During this time Koopmans embarked on a lengthy study of the relationship between freight rates and the construction of oil tankers. The study was not based on a formal mathematical model, but it did display a sure grasp of economic theory and a detailed knowledge of the tanker industry that was remarkable for a young scholar recently preoccupied with mathematical physics. The work was published as a monograph titled *Tanker Freight Rates and Tankship Building* by the Netherlands Economic Institute in 1939. There is a clear foreshadowing in the monograph of Koopmans's subsequent interest in the construction of optimal transportation routes.

In 1938 Tinbergen and Koopmans exchanged places. Tinbergen returned to Rotterdam and Dr. and Mrs. Koopmans moved to Geneva, where Koopmans was assigned the task of constructing a mathematical model of the United

Kingdom's economy. In early 1939 he attended a conference on Tinbergen's work at Oxford University. At the conference Koopmans met a number of economists, including Jacob Marschak, with whom he was to have a long and significant relationship. Later in the year the Koopmans went on a leisurely vacation, traveling through the French Alps by bus. As Mrs. Koopmans later related to me, "We had a good time and I became pregnant." Their first child, Anne, was born prematurely in April of 1940.

It was, of course, a time when the signs of war were everywhere; the invasion of Poland took place during the Koopmans's vacation. In April 1940 the Germans invaded Norway, and the Koopmans family decided to leave Europe for the United States. As Mrs. Koopmans described it to me:

> Not a stitch of work was being done because everybody foreign to Switzerland was struggling desperately to get away. We ourselves were scrambling for a visa —to the U.S., Canada, Cuba, even to Martinique. We were lucky; we had an invitation to come to Princeton, arranged for us by Professor Samuel Wilks, with whom we had become very friendly the year before, and we had gotten a visitors' visa. Furthermore, because Tjalling's term at the League of Nations was coming to an end, we had already arranged for passage on a Dutch ship for Genoa to the U.S. Somehow that passage on the Dutch ship was converted into passage on an American ship almost on the spot. I believe that happened in Bordeaux.
>
> The chance to get away came up suddenly, so I had hurriedly packed a small trunk with necessities and clothes, and a suitcase with diapers and milk powder for our 6-weeks-old baby. Then we got word that the U.S. ship (the *Washington)* was ordered to Bordeaux instead of to Genoa after Italy entered the war. We heard that at 9 a.m. on June 4; at 12:00 noon, we were on the train to Bordeaux. The Polak family had given us a travel basket for the baby; others supplied us with sleeping bags; Tjalling carried his briefcase, the luggage and gas masks; I carried the baby. We never saw our trunk again. Because we had a baby, we were given a small cabin to ourselves while the rest of the ship slept dormitory style. The vessel was only half full in Bordeaux—the day after we left Switzerland France closed all its

> borders—and many Americans who had been booked to sail were stranded in Italy and Spain. But while we were en route, the ship was ordered to Lisbon to pick up many people there, so that then the ship was filled to its capacity of 1,000 passengers. After that, we went to pick up more Americans in Galway, Ireland. Our adventure was not over for on the way to Ireland we were halted by German submarines and ordered into the lifeboats. Fortunately, it got across to the Germans that the ship was an American one, and America had not entered the war yet, so after some 4 hours of terror in the water, we were on our way again. In Galway, we took aboard another 1,000 persons. The rest of the trip was uneventful. We learned of the fall of Paris while at sea and we arrived in New York with only the clothes on our back, the child in her basket and some borrowed money. We had nothing else whatsoever.

The next several years were to be peripatetic. The departure from Europe was sudden, and long-term employment could not be arranged before arriving in this country. In 1940-41 Koopmans was engaged as a research assistant at Princeton and, simultaneously, taught a course in statistics at NYU. During this time, Koopmans worked on a celebrated problem of mathematical statistics in the tradition of earlier work by R. A. Fisher: the exact distribution of the serial correlation coefficient in normal samples. Koopmans derived a representation for this distribution by means of a contour integral and illustrated the use of an ingenious smoothing approximation that facilitated numerical computations. His paper, titled "Serial Correlation and Quadratic Forms in Normal Variables," was published in the *Annals of Mathematical Statistics.* It remains a permanent contribution to a problem that was never fully solved analytically yet absorbed the interest of many of the world's leading mathematical statisticians throughout the 1940s.

After a year the jobs at Princeton and NYU were terminated, and Koopmans took a position as an economist at the Penn Mutual Life Insurance Company in Philadelphia. A paper, "The Risk of Interest Fluctuations in Life Insur ance Operations," which does not seem to have been published, was written at this time.

In 1942 the family left Philadelphia for Washington, where Koopmans was to be employed for two years as a statistician for the British Merchant Shipping Mission. The work was interesting though routine, and Koopmans found the time to initiate a line of inquiry about the economics of cargo routing. This was eventually to be of great significance in the development of linear programming and in the study of the activity analysis model of production.

Koopmans's problem can be described in the following way. Given a list of ports, the flows of a homogeneous shipborne cargo can be described by a graph, whose vertices are the ports and whose edges are marked by the tonnage shipped between that pair of ports. Given also a fixed set of supplies at some ports and demands at others, an increase in the amount shipped from one particular port to another will cause compensating changes in the matrix of flows between other pairs of ports. In the paper, "Exchange Ratios Between Cargos in Various Routes," written in 1942, Koopmans showed how to calculate these compensating changes and their consequences for the total cost expressed in ton-miles.

The problem of determining the shipping plan that minimizes total cost, given a preassigned pattern of availabilities of supplies and demands, is known as the transportation problem. It is one of the most elementary examples of a linear programming problem, that is, the maximization of a linear function of several variables, subject to a series of linear inequality constraints. But in 1942 the concept of linear programming had not yet been proposed in the West, and Koopmans was unable to see his work as an instance of this more general problem.

In 1939 Jacob Marschak, whom Koopmans had previously met in Oxford, left Europe to become a professor at the New School for Social Research. There he organized a seminar in mathematical economics and econometrics, and the relationship between the two scholars was renewed when Koopmans attended the seminar on a regular basis in 1940 and 1941. In 1943 Marschak was appointed director of research at the Cowles Commission for Research in Economics at Chicago, and in 1944 Koopmans wrote to Marschak about his desire to leave Washington. Soon after, Koopmans accepted Marschak's invitation to join the staff of the Cowles Commission, and thus began a long association—both with Marschak and the commission—that was to prove extraordinarily productive.

The Cowles Commission for Research in Economics was founded in 1932 by Alfred Cowles, the president of Cowles and Company, an investment counseling firm with offices in Colorado Springs, Colorado. Mr. Cowles's initial motivation in establishing the commission was to assemble a group of mathematicians, statisticians, and economists whose combined efforts might provide a rational basis for investment choices. The formal charter of the organization, however, allowed for a broader mandate and contained the phrase, "The particular purpose and business for which said corporation is formed is to educate and benefit its members and mankind, and to advance the scientific study and development . . . of economic theory in its relation to mathematics and statistics." It was this broader mandate that was ultimately adopted by the commission, which, during its long history, was to become a primary vehicle for the elaboration and dissemination of quantitative methods in economics. During the last half-century, the subject of economics has been transformed by the introduction of quantitative techniques, and the Cowles Commission has played a major role in this process. I know of no other example in the history of science in which a research institution, founded and nourished by a private patron, has had so profound an impact on an intellectual discipline.

Initially the organization was located in Colorado Springs, with a small research staff headed by Charles A. Roos, who became the commission's first director of research in 1934. Starting in 1935, summer conferences were held regularly, with an ever-widening research agenda and group of participants from the United States and abroad. As pleasant as the location was for summer conferences, however, Mr. Cowles found it difficult to attract permanent staff to Colorado Springs, and he arranged for the commission to move to Chicago, where it became affiliated with the University of Chicago in 1939. Theodore Yntema, the first director of research at Chicago, was succeeded by Jacob Marschak in 1943.

Marschak was a scholar of great intellectual force, curiosity, and initiative. As director he continued the program of summer conferences, but now there was a dramatic increase in the number of visitors and the size of the resident staff. Marschak organized a series of weekly seminars, as well, and initiated the practice of disseminating research results as discussion papers and reprints. Leonid Hurwicz had been recruited by Yntema, and in the next several years Trygve Haavelmo, Koopmans, Herman Rubin, Lawrence Klein, Theodore Anderson, Kenneth J. Arrow, Herman Chernoff, Herbert Simon, and other distinguished statisticians and economists were to be associated with the commission in one way or another. The early research agenda, set by Marschak, was primarily concerned with the particular statistical problems arising in the estimation of parameters in a set of simultaneous equations.

The idea that the relationships among economic variables are best described by a set of simultaneous equations is a time-honored concept of economic theory. The price of a given commodity and the quantity purchased may be depicted by the intersection of a demand curve and a supply curve—the first relating the demand for the commodity to its price (given the incomes of consumers), and the second relating the supply of the commodity to its price (given the prices of the factors used in its production). Each of these equations will involve various parameters whose estimation is required if the system is to be used for the prediction of future values of price and quantity. The naive approach is to estimate the parameters in each equation separately using ordinary least square regressions. The question was: How good are the naive methods?

In several extremely important publications, Trygve Haavelmo, previously a student of Frisch, laid the groundwork for answering this question. Using the probabilistic methods of R. A. Fisher, Haavelmo assumed that the observed series of economic variables satisfied a system of, say, linear equations with stochastic errors governed by specific probability distributions with unknown parameters. Given the parameters of the error terms and of the equations themselves, any particular set of possible values will have a well-defined probability. The maximum likelihood estimates of the unknown parameters are those that give the highest probability to the values of the economic variables actually observed. As Haavelmo had shown, these maximum likelihood estimates could differ substantially from ordinary least squares estimates.

At an even more basic level, the structure of the system of equations may make estimation of the unknown parameters impossible. If, for example, prices and quantities are derived from the intersection of demand and supply curves, there may not be enough information to ascertain the separate slopes of each of these curves. It was the study of these statistical problems that Koopmans took up as his major area of concern soon after arriving at the Cowles Commission. A first paper concerned the bias arising from an ordinary least squares regression of the parameters of a single equation, if the equation is, in reality, part of a larger system. A second paper, written with the assistance of Herman Rubin and Roy Leipnik, provided a complete solution to the problem of "identification," that is, a description of the necessary and sufficient conditions that permit the structural parameters of a linear system to be determined uniquely from the probability distributions of the data and hence amenable to statistical estimation. This latter paper also developed systems of maximum likelihood estimators and derived their large sample statistical properties. The theoretical advances in this paper proved to be of lasting significance. Its results are still the core of the theory of simultaneous equations and endure in every textbook treatment of the subject.

In addition to his research on these and other aspects of econometrics, Koopmans organized a Cowles Commission Conference (in early 1945) devoted to the statistical problems arising from a system of simultaneous equations. He also edited the report of the conference, published as Cowles Commission Monograph No. 10, in 1950. This volume eventually became a classic in the field, and its themes have been fundamental in both the teaching of econometrics and subsequent research.

Koopmans became the acknowledged leader of that school of econometrics, focusing on the problem of simultaneity and insisting on a complete probabilistic model of the data to be analyzed. In 1947 he took the battle to the profession as a whole in his review of the volume, *Measuring Business Cycles,* authored by Arthur F. Burns and Wesley C. Mitchell. Koopmans found this work, written by two senior econo mists associated with the National Bureau of Economic Research, deficient in several respects. First of all, it was a detailed analysis of a great volume of data relating to business cycles, but its categories were not based on an underlying theoretical model incorporating maximizing behavior of the individual agents in the economy. Second, the statistical approach was eclectic, with no formal probabilistic model to account for the data and to justify the use of the author's statistical techniques. The methodology used by Burns and Mitchell was descriptive, Koopmans maintained, rather than flowing from the logical and analytical stance toward economic data that was at the heart of the Cowles program.

A passionate rebuttal to Koopmans's review was offered by Rutledge Vining, who stressed the merits of a synthetic approach capable of suggesting tentative hypotheses in an important area of economic discourse lacking a formal model. There was much jockeying about on the issue of whether economics was currently in the Tycho Brahé phase—simply codifying and mastering unstructured masses of data—or in the Keplerian and Newtonian phase in which a parsimonious and robust paradigm was available for explanation and illumination. Both the review and the rebuttal were written with such lucidity, scholarship, and care for these eternal economic concerns as to commend them to the general reader some four decades later.

At the Cowles Commission, Koopmans continued his study of the transportation problem that he had initiated in 1942. By the end of 1946 he realized that his earlier problem of transporting a homogeneous commodity from a set of origins to a set of destinations so as to minimize the total cost of transportation could be formulated as a problem of minimizing a linear function of a number of variables, subject to a set of linear inequalities constraining the values assumed by these variables. He also proposed a method of solution based on an economic idea that was to become of central importance in his subsequent research.

A particular instance of the transportation problem is specified by the supply at each origin, the demand at each destination, and a matrix of unit costs for shipping from each origin to each destination. Koopmans observed that a vector of prices, one for each location, could be associated with the optimal shipping plan. The prices would meet the condition that each route in use would make a profit of zero, in the sense that the price at the destination would equal the price at the origin plus the unit cost of shipping along that route. The routes not in use would, moreover, have a profit less than or equal to zero. He also demonstrated that if such a system of prices could be associated with an arbitrary feasible solution to the constraints of the transportation problem, the feasible solution would indeed be the optimal solution. The arguments made use of the theory of convex sets, which were to become of great importance in the study of the general linear programming problem.

Koopmans presented these ideas at a meeting of the International Statistical Conference in Washington in September 1947. Several months earlier he had a consequential meeting with George B. Dantzig, who was the first Western scholar to study the general linear programming problem. Dantzig had initiated his work on linear programming while employed by the U.S. Department of the Air Force, and in the summer of 1947 he developed the details of the simplex method, an algorithm for their solution. The simplex method is a remarkably effective computational technique that converges to the optimal solution in a relatively small number of iterations, even for problems of substantial size. The method makes use of a system of dual variables—one for each inequality—that are used at each step of the algo rithm to test whether some of those activities not currently in use should be introduced. In the special case of the transportation problem, these dual variables are precisely those prices previously employed by Koopmans.

Subsequent to his meeting with Dantzig, Koopmans extended his observations about the relationship between prices and optimality to the general activity analysis model of production. In an activity analysis model the possible techniques of production available to a firm, or to the economy as a whole, are given by a finite list of elementary activities that can be used simultaneously and at arbitrary non-negative levels. The resulting production possibility set is a polyhedral cone, approximating the smooth transformation sets of neoclassical economics to an arbitrary degree of accuracy. The activity analysis model, a generalization of the Leontief input/output model, can be used to generate a large number of distinct linear programs, depending on the objective function to be chosen and on the specific set of factor endowments.

Koopmans demonstrated that an efficient plan—a plan for which no alternative existed using less inputs and providing no less of any output—would be associated with a vector of prices with a special property. The prices, intimately related to Dantzig's dual variables, would yield a zero profit for the activities used in that plan and a profit less than or equal to zero for all the remaining activities. Conversely, a feasible production plan associated with such a vector of prices would in fact be efficient. This permitted Koopmans to make the fertile suggestion that if the correct prices were known the optimal selection of activities could be accomplished in a decentralized fashion by managers who were mindful of their private considerations of profit maximization. In this way Koopmans gave precision to the intuitive beliefs of economists, from Adam Smith onwards, that a decentralized competitive economy achieves socially optimal results "as if by an invisible hand."

In 1948 Koopmans succeeded Marschak as the director of the Cowles Commission. A conference on activity analysis was sponsored by the commission in 1949, and the results of the conference appeared in Cowles Commission Monograph No. 13 in 1951. The monograph, edited by Koopmans, contained a paper by Dantzig on linear programming as well as a lengthy exposition of the activity analysis model by the editor. In this paper and in a non-technical essay published in *Econometrica,* Koopmans demonstrated a sharp awareness of the relationship of these ideas to the fascinating discussion of socialist economic planning in the 1930s.

His strong convictions regarding the importance of the activity analysis model for economic planning in Eastern Europe led Koopmans to make extended trips to the Soviet Union in 1965 and 1970. There he met Leonid Kantorovich, a Soviet mathematician who independently initiated the study of linear programming in 1939. Kantorovich, who was to share the Nobel Prize with Koopmans in 1975, had developed a test for optimality and an outline of an algorithm for linear programming that was similar to but more cumbersome than the simplex method. In Kantorovich's work the problem of the optimal allocation of resources was approached not only from the point of view of a pure mathematician, but also with the economist's appreciation of the fundamental role played by prices in reaching an optimal decision.

Research in econometric methodology continued at the Cowles Commission, but under Koopmans's leadership and guidance new lines of activity in economic theory were initiated. The modern study of the general equilibrium model, in which the theory of production is united with a descrip tion of consumer preferences, was inaugurated by Arrow and Gerard Debreu; Arrow's classic *Social Choice and Individual Values* was in the making. At the same time Harry Markowitz was working on portfolio analysis; Arrow, Theodore Harris, and Marschak were writing an optimal inventory policy, and formal theories of decision-making under uncertainty were proposed.

In 1955 the commission left the University of Chicago for Yale University, where it was renamed the Cowles Foundation for Research in Economics. James Tobin, whom the commission had earlier tried to lure to Chicago, assumed the directorship in New Haven. Moving along with Koopmans were Debreu, Marschak, Roy Radner, and Martin Beckmann.

The last several years at Chicago were charged with intellectual disagreements between the staff of the Cowles Commission and members of the Department of Economics. Tjalling felt under considerable pressure and began to compose music. The Koopmans and their three children, Anne, Henry, and Helen, spent two summers at Bennington, visiting with friends and attending a composers' conference in which instruction in composition was given and the members of the group had their works played and recorded. The children were small and the family—which was of great importance to Tjalling—enjoyed swimming, hiking, and other outdoor activities.

Koopmans's strong desire to make the results of theoretical and mathematical analysis available to a wide audience of nonspecialists is revealed in the remarkable volume, *Three Essays on the State of Economic Science,* published in 1957. The relationship between prices and economic efficiency in both static and dynamic models of production and the role played by the assumption of convexity in welfare economics are discussed by means of simple geometric diagrams and with a lucidity rarely attained by an active research scientist. A second expository tour de force was his paper, "Selected Topics in Economics Involving Mathematical Reasoning," written jointly with Bausch, which appeared in 1959.

In the decade of the 1960s Koopmans's major research preoccupation was the theory of economic growth, in which he directly addressed questions of efficiency and optimality in dynamic models of production. He published a masterful paper, "On the Concept of Optimal Economic Growth," in which his original presentation of the calculus of variations was used to study the maximization of an objective function given by a discounted sum of utilities. In the model the input of labor is assumed to be exogenously growing. Output, which can be allocated between consumption and investment, is specified by a production function based on inputs of capital and labor. In several other publications he introduced a class of stationary utility functions that properly included the previous discounted sum of utilities, and he used this larger class to study the concept of "impatience": roughly speaking, a preference for current rather than postponed consumption. The analysis was based on a sophisticated generalization of the concept of Haar measure independently arrived at by Koopmans and his collaborator, Richard Williamson.

In the autobiographical sketch written when he received the Nobel Prize, Koopmans says, "In most of my Yale period my research, chiefly on optimal allocation over time, had more of a solitary character." But this is only in contrast to the Chicago days, when the energies of the entire Cowles team were focused on specific projects. In Chicago the commission was engaged in a methodological revolution involving the use of formal mathematics in economic theory and econometrics. By 1960 the battle had been won; the troops no longer had to be massed for assaults on exposed positions. Mathematical reasoning had become an accepted mode of exposition for economic arguments, and the members of the Cowles Foundation felt freer to pursue their own individual substantive interests.

By the early 1970s Koopmans may have felt that the mathematical revolution led by him had been too successful that elaborate mathematical arguments were being advanced throughout the profession to the neglect of more immediate practical concerns. He began to apply the techniques of growth theory to the study of exhaustible resources and, in particular, those resources used in the provision of energy. A lengthy study of copper supplies was initiated, in collaboration with William Nordhaus, his colleague in the Department of Economics, and Robert Gordon and Brian Skinner, both geologists at Yale. He took on the chairmanship of a committee of the National Academy of Sciences devoted to the study of alternative energy systems. This was followed by a one-year visit to the International Institute for Applied Systems Analysis (IIASA), in Laxenburg, Austria, where he succeeded George Dantzig (in the second half of 1974) as the leader of the Methodology Group.

On the morning in October 1975 when his Nobel Prize was announced, I visited Tjalling and Truus Koopmans at their home. The prize was shared with Kantorovich for their independent work on the optimal allocation of resources. Much of our conversation was taken up by Tjalling's distress about the fact that George Dantzig had not shared the prize. In a characteristic gesture involving a fine blend of morality and precise computation, Tjalling told me that he had decided to devote one-third of his prize to the establishment of a fellowship in honor of Dantzig at IIASA. As we left the house for a press conference at Cowles, Tjalling said, with a certain shy amusement about what was awaiting him, "Now I have become a public man."

In 1978 Koopmans agreed to assume the presidency of the American Economics Association, after the death of his longtime friend, Marschak, who had been president-elect. His presidential address, "Economics Among the Sciences," was devoted to a discussion of the differences in outlook of economists, engineers, and natural scientists engaged in interdisciplinary collaboration. The paper, written with Tjalling's characteristic conceptual clarity and mastery of the facts, was illustrated by his work on energy modeling and other topics addressed in recent reports of the National Research Council.

Looking back, one can see a pattern in Koopmans's professional career. He would invest himself for an extended period of time in a particular area of study in which his analytical capabilities could be used to clarify a large issue of potential practical value. He would gather together a group of collaborators, scholars with diverse backgrounds, and energize them with his benignly patriarchal sense of purpose and direction. He would make personal friendships with his intellectual associates, play chess with them, listen to music with them, and take them on canoe trips and long walks. The customary anxieties of the isolated research scholar would be handed over to Tjalling, the leader of the group, whose confidence and resolve would provide comfort and quiet any doubts. But, at the same time, he himself would be engaged in an internal debate about the merits of the collaborative activity—and, if the reckoning so indicated, he could deliberately take leave of the activity and prepare himself for the next venture.

Tjalling suffered a series of cerebral strokes in the last months of 1984. In the short time between then and his death on February 26, 1985, at the age of seventy-four, he was still capable of intellectual and social interaction with his family and with the loving friends who surrounded him.

I am very grateful for many conversations with Truus Koopmans and for the advice and assistance given to me by Kenneth J. Arrow, Gerard Debreu, George Dantzig, Leo Hurwicz, Alvin Klevorick, Peter Phillips, Martin Shubik, Herbert Simon, T. N. Srinivasan, Jan Tinbergen, and James Tobin.

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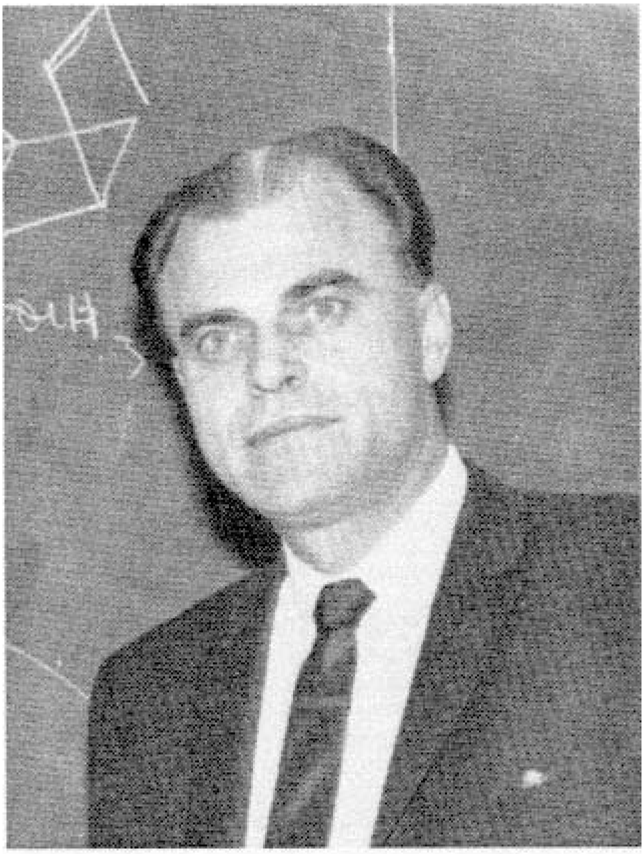


R. Pettit

Rowland Pettit

February 6, 1927-December 10, 1981

By John C. Gilbert

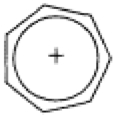
Rowland Pettit, although educated primarily in the area of physical-organic chemistry, focused his scientific research on organo-transition metal chemistry and brought a level of creativity and a "nose" for the issues of significance in the subject that justly earned him international recognition. As one who enjoyed both intellectual and technical challenges, Rolly made contributions that addressed problems ranging from those of largely theoretical and academic interest to those of substantial practical and industrial importance. The broad scope of his research is epitomized by his work on the synthesis and reactions of cyclobutadiene iron tricarbonyl, the role of orbital symmetry in metal-catalyzed isomerizations of strained hydrocarbons, and the mechanisms of the water gas shift reaction and the Fischer-Tropsch process.

Rolly was born in Port Lincoln, Australia, a small town in the southern part of the country, on February 6, 1927. The eldest of four sons, he was the only one to venture into the world of science. Many of the stories that Rolly enjoyed telling about his formative years in Port Lincoln revolved about fishing expeditions in the shark-infested waters near there. Following his precollege education he matriculated at the University of Adelaide, where he earned his B.Sc. (1949), M.Sc. (1950), and first Ph.D. (1953). Rolly often boasted that he had financed much of his schooling at the pool table; thus, he not only fished for shark but was himself a "pool shark!"[1]

His graduate research, under the supervision of A. K. MacBeth and G. M. Badger, was in natural products and polynuclear heterocyclic chemistry. Even at this early stage of his career, Rolly had an appreciation for the role of metals in fostering various chemical phenomena, not all of which had to do with his laboratory research. This interest is illustrated by the following story. As a student at Adelaide, Rolly had become enamored of a beautiful young blond, blessed not only with good looks but also with a father who was one of the most prosperous ranchers in the area. The family's affluence included a large swimming pool, a luxury virtually unknown in the region at the time. Because the state of pool technology was primitive by contemporary standards, control of algae was a continuing battle, and this was particularly vexing to "Daddy," who enjoyed his daily swim. Pettit, aware of the role of copper compounds as effective algicides, saw an opportunity to make a lasting, positive impression on his beloved's father, thereby fortifying his relationship with the daughter. Thus, he dosed the pool liberally with copper sulfate and soon presented a pool filled with crystal-clear, algae-free water for inspection. The father, having expressed his gratitude for this accomplishment, enthusiastically plunged into the pool, only to emerge minutes later with his formerly handsome head of silvergray hair exhibiting a stunning blue-green cast. Rolly was never again to explore the realm of bioinorganic chemistry!

Upon completion of his Ph.D., Pettit accepted an "Exhibition of 1851 Overseas Fellowship." These highly competi tive awards had been established using profits from the Exhibition (World's Fair) of 1851 and enabled graduates from universities in other parts of the British Empire to go to Britain for two years of study for higher degrees. It was undoubtedly the interest kindled by his experience in the area of polynuclear compounds that led Pettit to take up his fellowship under the tutelage of Michael J. S. Dewar, then the newly appointed professor of chemistry at the Queen Mary College (QMC) of the University of London. In retrospect, this decision was to have enormous influence on the rest of Rolly's scientific career, sparking as it did his lifelong fascination with nonbenzenoid aromatic compounds and organometallic chemistry, and furthering his interest in the application of theory to organic chemistry; Rolly had independently learned molecular orbital theory while at Adelaide, although no one in the department at that time either knew this technique or cared about it.

Pettit's best-known accomplishment at QMC was the first *intentional* synthesis of tropylium ion (1), the first truly stable carbenium ion to be discovered. The reason Pettit's synthesis of this ion was not the first one actually to appear in the scientific literature is unusual and, in retrospect, amusing. The synthesis started with the alleged 2,4,6-cycloheptatriene-1-carboxylic acid (2a). This was one of several isomeric cycloheptatrienecarboxylic acids that Büchner had previously prepared by alkaline isomerization of what he believed to be the norcaradienecarboxylic acid (3a), the ethyl ester (3b) of which was assigned as the product of addition of carbethoxycarbene to benzene. An obvious route from 2a to 1 thus would be by way of a Curtius degradation. However, A. W. Johnson, then at Cambridge University, was known to be trying this route. Consequently, Dewar and Pettit, feeling that it would be improper for them to "poach," attempted numerous alternative ways of transforming 2a to 1, all of which failed. At this point the news came from Cambridge that the urethane formed by Curtius degradation of the supposed 2a, and thus assigned structure 2b, hydrolyzed extremely easily to urethane (4) and other unidentified products. Johnson explained the facile hydrolysis in terms of an unprecedented SNl-type reaction, its ease owing to the aromaticity of the tropylium ion (1), derived from 2b by ionization. Because Johnson planned no further work on the problem and since the mechanistic explanation he provided to account for his results seemed dubious, Pettit repeated the preparation and hydrolysis of the alleged urethane 2b, obtaining not only 4, but also 2,4-cycloheptadienone (5), both in nearly quantitative yield. This seemed to refute Johnson's interpretation because hydrolysis of 2b in the manner he suggested should have led to 2,4,6-cycloheptatriene-l-ol (2c), not to a ketone. Rolly's result strongly implied that the structure of the urethane derived from the Curtius degradation had been wrongly formulated by Johnson and was instead an isomer (e.g., 6a, rather than 2b); as an analog of an enamine, such a species would be expected to hydrolyze easily. The structural reassignment in turn meant that the "Büchner acids" were isomers derived from hydrolysis and isomerization of the ester 2d, itself formed by the previously mentioned reaction of carbethoxycarbene and benzene. So, for nearly a year Pettit had been preparing the acid 2a that he wanted but had been converting it to an isomer, 1,4,6-cycloheptatriene-1-carboxylic acid (6b) that could not be transformed into 1! Indeed, *all* the routes used in attempts to convert "2a," as assigned by Büchner, to 1 succeeded when applied to the acid originally formulated as "3," which in fact is properly assigned as 2a. In the meantime, Doering and Knox "scooped" Pettit by realizing that a crystalline material made many years earlier by Meerwein, and described by him as "chlorocycloheptatriene," was in fact tropylium chloride.



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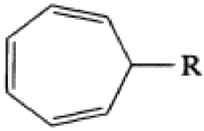
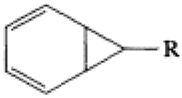


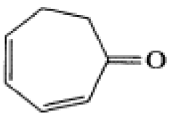
2a: R = CO_2H
2b: R = $NHCO_2Et$
2c: R = OH
2d: R = CO_2Et



3a: R = CO_2H
3b: R = CO_2Et

H_2NCO_2Et

4



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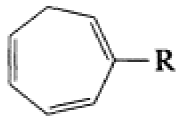
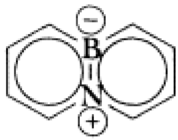


6a: R = $NHCO_2Et$
6b: R = CO_2H

Another notable contribution by Pettit at QMC was the synthesis of what proved to be the first two (7 and 8) of a series of astonishingly stable aromatic "borazaro" compounds, conceptually derived from normal aromatic hydrocarbons by replacing one or more pairs of adjacent carbon atoms by the isoelectronic moiety, B-N+. While borazine (9) is related in this way to benzene, it is highly reactive because the large difference in electronegativity between B^- and N^+ restricts p-bonding. Compounds 7 and 8, on the other hand, closely resemble the corresponding hydrocarbons, phenanthrene and naphthalene. In fact, 8 fails to undergo the Diels-Alder reaction with maleic anhydride, an unexpected result since naphthalene itself reacts reversibly with this anhydride in a Diels-Alder fashion.



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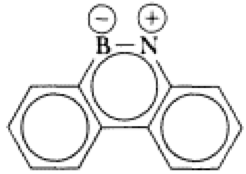

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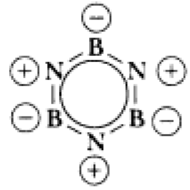

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Pettit's completion of the second of his two Ph.D. degrees is an amusing tale in itself, and Michael J. S. Dewar has provided the following account of certain events that led to it.

> Pettit's first Ph.D. Degree was from the University of Adelaide. At the time, Examining Committees at universities in Australia always included an outside member, usually from Britain, and I was asked to serve as one of Rolly's examiners. Under the circumstances, no oral examination was normally held because it was impossible for the committee to meet. However, Rolly's professor in Adelaide, G. M. Badger, traveled round the world regularly and was due to visit London just after Rolly had arrived. He suggested that it would be nice to set a precedent by having an oral examination. This was duly arranged, at a convenient period before lunch, in my office, and when we had all gathered, I produced a bottle of sherry to assist the proceedings. There was of course no question about the outcome—Rolly's Ph.D. thesis was superb—so we sat around happily "talking chemistry" until it was time for lunch. The next day Rolly was unwise enough to complain about his oral, saying that he had spent a lot of time reading up on organic chemistry in preparation for a grilling and that all we had in fact done was to spend an hour gossiping about chemists!
>
> This brash statement was destined to haunt him two years later. Rolly had had no intention of submitting a Ph.D. thesis in Britain because he already had his Ph.D. from Adelaide. However, as the end of his two years of fellowship drew to a close, I worked on him, pointing out what a unique distinction it would be to have *two* Ph.D.'s. I also reminded him that his Fellowship provided funds for typing and binding a Ph.D. thesis and that the money would be wasted if he failed to write one. So he relented and I arranged the oral examination carefully, planning it for our home after dinner, with one of my oldest friends, Christopher Longuet-Higgins, as the

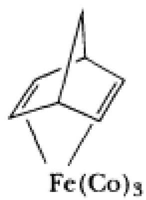
> other examiner. After we had lulled Rolly into a mistaken sense of complacency with one of Mary's dinners and a fair amount of good wine, we set to work, having spent the whole afternoon planning it. I forget now what the questions we asked were. Most of them were ones we could not answer ourselves. We reduced Rolly to total incoherence. After about an hour, when we had run out of prepared questions, I said sternly, "Please leave the room, Dr. Pettit, so that we can discuss our verdict." Mary said he came out, shaking like a jelly, almost in tears, and saying how awful it was that he had let me down by failing so disastrously. After a suitable interval, I summoned him back and said, "Well, Dr. Pettit, I hope you found this oral examination satisfactory?" To his credit, Rolly replied without batting an eyelid, "Yes, Professor Dewar, entirely satisfactory." It was of course a great piece of good fortune for me to have him as a founding member of my first research group, in which he remained for five years, the last three of which were as an ICI Postdoctoral Fellow.

In the spring of 1957 Rolly accepted a position as an instructor at the University of Texas at Austin (UT-Austin), never having set foot in the States, much less Texas. His recruitment to the Department of Chemistry was done "in the old-fashioned way," by virtue of personal contacts between eminent scientists. In Rolly's case, Dewar had alerted Doering that Pettit was a superb candidate for an academic post in the United States.[2] Doering, in turn, knew of the opening at UT-Austin and put the department in contact with Rolly. It was at this point that he discovered it would be impossible for him to emigrate to the United States with a permanent visa since the quota of these for citizens of Australia was 100 per year and the waiting list was already twenty years long. Indeed, the only option available that would allow him to enter the United States by the fall was as a participant in the "Exchange Visitor Program of the University of Texas," which was designed to enable foreign *students* to further their educations in Texas. This type of visa was good for only five years and made its recipients ineligible to apply for a permanent visa until such time that the holder had spent a minimum of two years back in his or her country of residence; this proviso was soon to challenge Rolly's creativity and power of persuasion. Nevertheless, always the optimist and anxious to embark on his independent scientific career, Rolly elected to come to Texas despite having only a student visa. The outcome of this decision was obviously to be of great mutual benefit.

Rolly found Austin's climate a welcome relief from the damp and cold of England, and he felt right at home in Texas—after all, Aussies are rugged individualists too! Indeed, whenever British visitors concluded their seminars by showing pictures of England under crystal-clear, sunny skies, Rolly used to needle them by asking how long they had to wait for the appropriate climatic conditions needed to obtain such "rare" photographs. Within a year, however, he began to address his visa status, hoping to circumvent the impending requirement for him to spend two years back in Australia. The request for an exception to the regulation was pursued at a number of levels, a process that at various points involved Dr. Alan Waterman, then director of the National Science Foundation; Dr. Wallace Brode, scientific advisor to the Secretary of State; and even the Honorable Lyndon B. Johnson, who was a U.S. senator at the time. Much to Rolly's chagrin, all appeals were to no avail, apparently because President Eisenhower felt strongly that the program under which Pettit had entered the country was designed to give foreign students an education in the United States with the intention that they would return to their homes and use their skills to uplift the economies of their own countries; to make exceptions would be to encourage the "brain drain" phenomenon. One of the effects of the prolonged but unsuccessful effort to evade the long arm of the Immigration and Naturalization Service was to reinforce Rolly's general distaste for paperwork and the necessity to work through administrative channels. In any case, Rolly was ultimately forced to explore other options and fortunately found a loophole that obviated the need to leave the country for an extended period. The strategy was simple enough, but its risk was great. All he needed to do was to leave the United States, and of course Nuevo Laredo, Mexico, was an easy drive from Austin, and then return immediately; absence from the States, however brief, was sufficient to permit renewal of his visa. The risk was that once having crossed into Mexico, Rolly would not be allowed to reenter the United States because he did not hold the proper type of visa for reentry—a classic Catch-22 situation! Fortunately, the overworked officials at the Immigration and Naturalization Service let the Aussie back in, failing to take proper notice of the type of visa he held. However, by stamping his visa according to standard bureaucratic practice, they provided Rolly with the evidence he needed to prove that he indeed had left the United States.

All of Rolly's visa problems were eventually rendered moot when, in 1959, he married Flora Hunter, a Ph.D. biochemist associated with the Clayton Foundation Institute of Biochemistry at UT-Austin. From this union came two children, George H. and Nancy S. In Flora, Rolly had a wife who succeeded in juggling her own professional career, childrearing, and support of her husband's aspirations in a way that few women could have. Given Rolly's predilection for professional travel and his aforementioned distaste for paperwork, Flora shouldered the lion's share of the tasks of family life and made it possible for him to concentrate on his passion for chemistry. Rolly was extremely proud of Flora's ability to successfully organize his life, provide love and nurturance to the children he too so adored, continue to pursue her own interests in scientific research, and aug ment the family's finances with astute actions on the stock market.[3]

Pettit's early independent scientific career at UT-Austin involved the synthesis and characterization of cationic nonbenzenoid aromatic species such as the perinaphthenylium, thiapyrylium, and homotropylium ions. The latter was the first example of a species exhibiting "homoaromaticity," a theoretical concept first introduced by Saul Winstein. Rolly also initiated his studies on iron tricarbonyl complexes of polyenes and, in 1959, prepared 10 from bicyclo [2.2.1] hepta-1,5-diene, the first such complex of a nonconjugated system. His potential and accomplishments at this point in his career were formally recognized by his being named as an A. P. Sloan fellow. On an informal basis, the fact that UT-Austin had an outstanding young chemist on its hands was noted by the faculty of the Chemistry Department at MIT. Norman Hackerman, then chairman of the department at UT-Austin, learned of this in 1960 when Arthur C. Cope, who was visiting in Austin at the time, mentioned that his faculty had listed the "potentially most able chemists in this country between the ages of 28 and 32 . . . and that Pettit was in that list."



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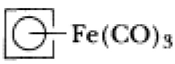
Extension of the investigation of various organoiron complexes eventually led Rolly and his co-workers to the preparation in 1964 of 1,3-cyclobutadiene iron tricarbonyl (11) and shortly thereafter to the long-sought and theoretically important diene itself.[4] Indeed, the facile release of the highly reactive diene from the complex made the hydrocarbon a readily available intermediate from which a number of fascinating molecules were ultimately prepared by the Pettit group and others. Among them were a number of examples of Dewar benzenes, (e.g., [12], as well as cubane [13], hypostrophene [14], and benzohomotropylium ion [15]). Rolly's interest in the properties of highly strained compounds and his unique combination of expertise in organometallic chemistry and theoretical organic chemistry led to seminal studies of metal-catalyzed cycloadditions and rearrangements that stimulated many other scientists to explore the mechanism and application of such processes.



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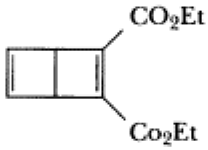

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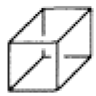

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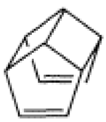

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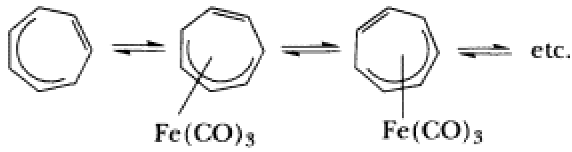
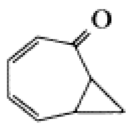
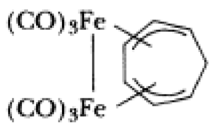
In the area of "pure" organometallic chemistry, compounds prepared in the Pettit group have provided considerable insight into the nature of bonding between organic species and transition metals. Rolly's pioneering work in the synthesis and examination of the properties of the iron tricarbonyl complexes of p-allyl, pentadienyl, and tropylium cations are noteworthy; his work with the latter species contributed much to the concept of "fluxionality" in molecules, species that Pettit delighted in calling "ring whizzers," as illustrated by the equilibria in Eq. 1.



Examination of the chemistry of the iron tricarbonyl complex of the homotropylium ion led to the ingenious preparation of the theoretically interesting 2,3-homotropone (16), and work with the bis(iron tricarbonyl) complex of 1,3,5-cycloheptatriene (17) produced an incredibly stable organometallic cation. The pK_R^+ value of the corresponding alcohol (8.0), a reflection of cationic stability, is one of the largest such values, if not the largest, recorded.





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Pettit recognized the importance of unraveling the mechanistic details of reactions catalyzed by organometallic species. His enviable chemical intuition and ability to design simple yet definitive experiments resulted in important mechanistic understanding of the olefin metathesis (disproportionation) reaction and the Fischer-Tropsch process, both of which are of great industrial importance. In the former case he was able to show that cyclobutane intermediates were *not* involved and instead suggested the interven tion of a metal complex, an insight that has since been confirmed. His use of a binuclear metal carbenoid species as a model compound to elucidate the mechanism of the Fischer-Tropsch reaction, coupled with his definition of the details of the chain-propagating step in the overall transformation, has provided important information in this area. In related work, Pettit and his group demonstrated the synthetic utility of carbon monoxide and water in an iron-catalyzed water gas shift reaction with the intermediacy of an anionic metal hydride. These results have significant implications for addressing the challenge of producing synthetic fuels by metal-catalyzed processes.

Rolly's renewed interest in the mechanism of the Fischer-Tropsch reaction, in particular, on the details of how carbon-carbon bond formation occurs under such catalytic conditions, developed only a year or so before he was diagnosed as having inoperable lung cancer. To be sure, these studies were merely an extension of one of his long-standing interests—that of C-C bond making and breaking as mediated by transition metals. Pettit in fact had just published a seminal communication in the area. He also had a very strong feeling that cracking and depolymerization reactions, of considerable interest to the petrochemical industry, were related to the Fischer-Tropsch reaction (i.e., all these processes involved transition metal-bridging methylene or methylidene reactive intermediates).[5]

As Rolly began to succumb to the disease and to the heroic yet debilitating attempts to control it by chemotherapy, he was hospitalized and remained so for the last months of his life. He continued to maintain almost daily contact with his research group of about ten individuals through Evan Kyba, who would visit him for anywhere from five minutes to an hour late each afternoon. Rolly would gather his strength for those visits so that he could focus fully on the discussion of his group's results. He would analyze the data and discuss with Kyba what key experiments were to be performed next. When this was done, Rolly was generally exhausted, and the meeting would be over—he simply did not have the energy for discussion of nonchemical matters. The next morning the results of the previous day's discussions would be communicated to the relevant members of the Pettit research team, and the cycle would continue.

Although there were many projects ongoing at the time, the one that commanded Rolly's interest on an almost daily basis was the effort related to the catalytic hydrogenolytic depolymerization of hydrocarbons. The chemistry required considerable fine tuning of the experimental procedures before meaningful results could be obtained. This took several months, during which Rolly became very frail, but he refused to give in to the inevitable while this crucial piece of work was still unfinished. Evan Kyba still vividly remembers when he presented the last data to Rolly for his inspection. "We've got it!" he exclaimed, "Write it up." He died less than a week after that meeting. Kyba is convinced that Rolly's interest in that scientific problem kept him alive at least two months longer than any other person would have survived under the circumstances—but such was the force of his intellect that he would not quit until the job was done.

Rolly's research accomplishments earned him election to the National Academy of Sciences in 1973. Earlier he had received the Southwest Regional Award of the American Chemical Society (1968) and membership in the American Academy of Arts and Sciences. He was named to the W. T. Doherty Professorship in Chemistry in 1980.

Although dealing with administrative tasks was not Rolly's forte or of interest to him, he did consent to serve as chairman of the department for a four-year term. This was at the behest of John Silber, then dean of the College of Arts and Sciences, whose powers of persuasion are legend. I remember well Silber's first and only meeting with the faculty of the department, during which he made it quite clear who was in charge. It was indeed fortunate for us that we had someone with Rolly's strength of character and reputation as our buffer with Dean Silber, as Rolly saw to it that the department continued to grow in quality and reputation of its faculty despite the maelstrom surrounding Silber's tenure as dean. Nevertheless, Rolly's term as chairman probably represented the longest four-year period of his life.

Rolly was an individual with a breadth of interests outside of chemistry. These included the aforementioned love of competitive pool, at which he excelled regardless of how many shots of scotch whiskey he had consumed; the raising of exotic flora, with an emphasis on bromeliads; and, in the last years of his life, ranching in the classic Texas style. The image of him leaning over the pool table, sighting along his personalized cue with those piercingly bright blue eyes, and then successfully making a difficult combination shot is impossible to forget. Similarly, his obvious delight at being able to conduct experiments on the efficacy of calcium carbide in coaxing his beloved bromeliads into bloom typified his excitement about research of all kinds. In an entirely different vein, he was a sight to behold as he tended to his small herd of cattle, handling the ranch equipment, all of which was foreign to this Aussie, with aplomb and ascertaining that his bull, "Taurus," was behaving himself.[6]

Rowland Pettit was an exceptional human being, much admired by his colleagues here and abroad, his students, and all those with whom he came in contact. His intelligence, wit, creativity, unrelenting optimism, and general love of life were characteristics that fit this man and brought out the best in all who knew him. He was a person who lived life to the fullest. As Rolly himself often said, in his usual self-deprecating manner, "All the victorious gladiators have left the arena. Only I lie bleeding in the sand." Until the very end, he lived to fight another day, and we all miss him.

The author appreciates contributions to this memoir from Michael J. S. Dewar, Nathan Bauld, Stephen Martin, Evan P. Kyba, Jeffrey S. McKennis, William Baird, and Michael Edens, all of whom benefited professionally and personally from their relationship with Rowland Pettit.

NOTES

1. Many years later (1970) Rolly finally obtained his own pool table and installed it in the basement of his house. This table invariably became the center of attention at many of the social events he and his wife hosted. It was a rare occasion when he lost at "eight-ball" or straight pool. His talents at the table were also well-known in a pool hall in Columbus, Texas, a small town on the route Rolly traveled on his frequent consulting trips to Exxon Research and Engineering Company in Baytown. The "locals," most of whom were Hispanic, were soon to learn that the "gringo" from Austin was awesome at the table.

2. It is an interesting coincidence that within five years Rolly was to be instrumental in luring his own mentor, Dewar, to UT-Austin to become the first holder of the Robert A. Welch Chair in Chemistry. Rolly knew that Michael was not entirely happy with his situation at the University of Chicago. One point of contention, according to a letter that Pettit wrote to Al Matsen, a colleague at UT-Austin, in June of 1962, was that the University of Chicago was dragging its feet with regard to providing Dewar with an air conditioner for his office, and Michael was finding the summer heat and humidity of the Windy City unbearable; air conditioning was something that Texans knew about, and it was made clear in the offering letter to Dewar that his research space would all be so equipped.

A somewhat different perspective on the matter has been provided by Dewar himself. He writes, "Curiously enough, five years after going to Austin, Rolly repaid the favour (of Michael's having helped Pettit to find a position at UT-Austin) by persuading me to follow him there. It was on the surface an idiotic move. I was very happy at the University of Chicago and UTA was then almost unknown. I went because I had heard from Rolly that the Texas Legislature wanted to make UTA a top University and because I had been greatly impressed by the chairman, Norman Hackerman, when I visited Austin in 1957 on Rolly's behalf. With Texas money and Norman in charge, the project seemed feasible and an exciting venture to be in on. Also Rolly was very persuasive."

3. This is not to say that Rolly was not involved with his family. For example, when the children reached the age when sex education was important, he enthusiastically joined Flora in the purchase of two pure-bred Persian kittens, naming the male, "Joe Kapp," in honor of the Minnesota Vikings quarterback of the time. Joe eventually performed his fatherly duties but had such a miserable personality that he was banned from the house shortly thereafter. What Nancy and George got out of the experience is not known, but both of them now prefer dogs as pets.

4. The successful synthesis of such iron tricarbonyl complexes evolved from the unsuccessful efforts of Rolly's colleague, Nate Bauld, to prepare the nickel complex of benzocyclobutadiene from the corresponding dibromide. Bauld, knowing that one of Pettit's students, George Emerson, was having great success making dieneiron tricarbonyl complexes, provided Emerson with a sample of the dibromide; the rest is history.

5. There is some belief that Rolly's interest in the Fischer-Tropsch process was triggered by the oil embargo imposed by OPEC in 1973. He took the embargo as an attack on the petrochemical industry, long his favorite, and hoped to teach the cartel a lesson by developing processes that would lessen our dependence on foreign oil.

6. There was a period when Taurus appeared to prefer to "smell the flowers" rather than service the cows, as was intended. Rolly duly consulted with a veterinarian, who told him that Taurus apparently had wandered into a large patch of cockleburrs, which had attached themselves in a manner that made certain activities painful for the bull. The cure recommended was for Pettit to remove the offensive burrs with the cautious use of manicuring scissors. The record is silent on whether this remedy was indeed pursued.

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Alfred C. Redfield

November 15, 1890-March 17, 1983

By Roger Revelle

Alfred Clarence Redfield was a member of a scientific family. His great grandfather, William C. Redfield, was the first president of the American Association for the Advancement of Science in 1848. Like most scientists in those days, he was an amateur. He made his living as a railroad and canal manager, but he contributed many ideas and observations to the then-infant science of meteorology, and particularly to the understanding of hurricanes, which he showed were giant whirlwinds. Alfred's grandfather was a botanist in the Academy of Natural Sciences in Philadelphia, and his father, Robert Redfield, was a naturalist photographer, whose works are in the Library Company in Philadelphia and at Yale. Alfred's son, Alfred Guillon Redfield, is a physicist and biochemist who is a member of the National Academy of Sciences. His nephew, Donald Griffin, is world famous for his studies of bats and their use of high-frequency acoustics to navigate and to find their prey. One of his two daughters, Elizabeth Marsh, is a member of the Division of Natural Sciences and Mathematics of Stockton State College in New Jersey.

Alfred C. Redfield was born on November 15, 1890, in Philadelphia. He died March 17, 1983, at his home in Woods

Hole, Massachusetts. During his boyhood his family spent their summers in Cape Cod, starting in Falmouth in 1898 when Alfred was seven, and afterwards for many years in Barnstable, on the shores of Cape Cod Bay. The boy was fascinated by natural history, being especially interested in insects and birds. He attended Haverford College for one year, 1909-10, and then entered Harvard University, where he received a bachelor's degree in 1913 and a Ph.D. degree in 1917. After receiving his doctoral degree, Redfield studied at Christ College in Cambridge University and at the University of Munich. He spent a year as assistant professor of physiology at the University of Toronto and then joined the faculty of Harvard University as assistant professor of physiology in 1921. Redfield remained on the Harvard faculty for the rest of his life, as professor of physiology from 1931, director of the Biological Laboratories in 1934, and chairman of the newly consolidated Division of Biology, which united botany, zoology, and physiology, from 1935 to 1938, and finally as professor emeritus after 1956.

Redfield married Elizabeth Pratt in 1913, shortly after receiving his bachelor's degree, but she was a casualty of the 1918 flu epidemic. In 1922 he married Martha Putnam. After more than sixty years together and three children, she outlived him by only a few months.

Redfield's doctoral research dealt with skin coloration in the horned toad, which he found was controlled by adrenalin, the "stress" hormone in mammals. After obtaining his degree, he studied the physiological action of radium radiation and X-rays, demonstrating that the effects on living tissue resulted from ionization caused by the radiation.

During his graduate years, Redfield visited the Marine Biological Laboratory at Woods Hole. In later summers at Woods Hole he became interested in the respiratory functions of the blood in marine invertebrates and made no table discoveries of oxygen binding and physiological behavior of the copper-bearing respiratory compound hemocyanin. This is the blood pigment of Limulus, the horseshoe crab, and several other ancient invertebrate species.

Redfield's life story from 1930 to 1970 was intimately intertwined with the first forty years of the Woods Hole Oceanographic Institution. The "Oceanographic" was founded in 1930 under the leadership of Frank Lillie, director of the Woods Hole Marine Biological Laboratory, with a $3 million endowment from the Rockefeller Foundation and with the marine biologist Henry Bryant Bigelow, a Harvard professor, as director. Following the tradition established by the Marine Biological Laboratory, the small staff gathered at Woods Hole only during the summertime. During the rest of the year they scattered to their respective universities and home institutions. Alfred Redfield was one of the first dozen or so staff members recruited by Bigelow, who was his intimate friend. Others were the meteorologist Carl Gustav Rossby and his students, Ray Montgomery and Athelstan Spilhaus; the bacteriologist Selman Waxman and his assistant, Charles Renn; Bigelow's students, Columbus Iselin, Mary Sears, and George Clark; the chemists Norris Rakestraw from Brown University and Richard Seiwell from the Carnegie Institution; Redfield's student Bostwick Ketchum; and the geologist Henry Stetson from Harvard. There were also Albert Parr, director of the Bingham Oceanographic Foundation at Yale, who used the Woods Hole ship *Atlantis* but was never actually a staff member, and Floyd Soule, physical oceanographer of the International Ice Patrol, which made its headquarters at Woods Hole after the oceanographic institution was founded.

Other than Bigelow, Iselin and Seiwell, none of the rest of the staff had much seagoing experience, let alone oceanographic experience. They had to teach themselves how to be oceanographers. They became seasick when they went out on *Atlantis* into the Gulf of Maine or beyond. Some of them more or less got over it and were able to work on the ship; others were never able to work at sea, though they were very useful in the laboratory. In any case, this small staff produced 260 scientific papers in ten years on a budget of about $80,000 a year. Nowadays the Oceanographic issues about 260 papers a year, but the budget is several hundred times as large.

Some of the research staff doubled as managers. Columbus Iselin was the first captain of *Atlantis.* He took delivery of the ship from her builders, Burmeister and Wain, in Copenhagen and sailed her across the Atlantic with an amateur crew. William Schroeder, an ichthyologist who was Bigelow's collaborator in writing about the fishes of the Gulf of Maine, was the business manager; his father, "Pop" Schroeder, was superintendent of buildings and grounds. The elder Schroeder was a retired plumber; according to Redfield, "he took care of all the problems that you could take care of with a monkey wrench." On the other hand, some of *Atlantis's* crew also did science. Harold Backus, the engineer, started the tradition of keeping records of the different species of land birds that came on board when the ship was at sea. Like the scientists, these birds usually became seasick when they landed on deck. A seagoing technician, Alfred Woodcock, observed and explained many of the essential but previously unobserved features of the ocean atmosphere circulation in the boundary layer between the two fluids.

In the early 1930s Redfield made the discovery for which he is best known. This is that the atomic ratios between the chemical components of marine plankton, specifically nitrogen, phosphorus, and carbon, are identical with their relative proportions in the open ocean. For every atom of phosphorus there are fifteen atoms of nitrogen and 105 atoms of organic carbon. Usually much more carbon is present in the form of carbonate and bicarbonate ions. But 105 atoms of carbon can be shown to be of biological origin, originating in the "soft parts" of marine organisms.

The content of dissolved oxygen in the open sea is in most regions adequate to account for the oxidation of the phosphorus, nitrogen, and carbon in marine organisms after they die (i.e., about 235 to 270 milligram atoms of oxygen for each milligram atom of phosphorus). In some regions, however, for example, in the Black Sea, in Norwegian fjords, in tidal marshes, and in the interstitial waters of many bottom sediments, the amount of dissolved oxygen is insufficient to oxidize the phosphorus, nitrogen, and carbon, and oxygen must be supplied under anaerobic conditions from the reduction of sulfate ions in the seawater to sulfides. (In a later paper Redfield suggested that oxygen in the earth's atmosphere may have originated in large part from sulfate reduction and subsequent release of oxygen in photosynthesis.)

The reasons why the proportions of biologically active elements in seawater and in marine plants are virtually identical are by no means clear. Redfield thought that bacterial nitrogen fixation might increase the ratio of nitrate to phosphorus and that somehow denitrifying bacteria operated to limit the increase of nitrate nitrogen in the ocean to the ratio observed in marine plants. He thought also that the total amount of oxygen in the atmosphere and in oxidized sediments was mainly limited by the quantity of organic matter available during the earth's early history for sulfate reduction and by processes that isolated the sulfide produced—for example, formation of iron sulfide and other insoluble compounds.

In recent years Wallace Broecker and other geochemists have referred to oceans in which there are constant ratios between phosphorus, nitrogen, and carbon as "Redfield Oceans." They have used these ratios to explain aspects of the carbon cycle in the sea. From Redfield's point of view the identity of the ratios of biologically active substances in marine organisms and in the ocean waters was one source of his famous aphorism: "Life in the sea cannot be understood without understanding the sea itself."

With the onset of America's role in World War II in 194142, the Oceanographic underwent a sea change. Alfred Redfield was appointed associate director, and he and Martha moved permanently from Cambridge to Woods Hole. Within a year the staff was multiplied thirtyfold. Research and development on underwater explosives and on many oceanographic problems of military importance was undertaken on a crash basis. Such a large number of people had never lived in Woods Hole during the winter. Most of the houses were not winterized, and it was very cold. I was the Navy's project officer for the Oceanographic, and I came up from Washington about once a month on a two-day trip. It was my impression that the old New England custom of bundling was widespread just so that people could keep warm.

During this time Redfield concentrated his research in two areas—the problem of fouling of ships' hulls by various marine invertebrate organisms and protection of submerged submarines from surface ships and aircraft. The former work, carried out in cooperation with Bostwick Ketchum and others, led to the development of antifouling paints, which were said to reduce the costs of ship operation by 10 percent or more.

The work on submarines was an outcome of a study of the behavior of echo-ranging equipment (called ASDIC by the British, and SONAR by the Americans) installed on surface ships to detect and track submerged submarines.

This equipment was observed to behave in a seemingly erratic way. Sometimes an echo could be received from a submarine several thousand yards away; at other times and places an echo would not be returned at a distance of only a few hundred yards. To find a rational explanation for this behavior, Columbus Iselin on *Atlantis* and Commander William Pryor on the U.S. destroyer *Semmes* carried out tests together in the Caribbean Sea. They found that, when there was a temperature gradient in the waters near the surface (e.g., in the afternoon when the surface waters were heated by the sun), echoes were returned only from nearby submarines or not at all, whereas in the morning, when the surface waters had cooled to a uniform temperature with depth, echoes were returned from submarines several thousand yards away. The "afternoon effect" was clearly caused by refraction or downward bending of the sound beam from the sonar, which created a "shadow zone" in the upperwater layers where the sound did not penetrate. Later studies showed that there are large areas in the oceans, particularly in coastal regions, where temperature gradients in the upper layers greatly weaken performance of the sonar gear. Athelstan Spilhaus's invention, the bathythermograph, which accurately and quickly recorded water temperatures in the top 150 meters beneath the sea surface, was refined and mass produced by the navy and installed on all ships used in antisubmarine warfare.

Maurice Ewing and Allyn Vine, two of the wartime recruits at Woods Hole, recognized that knowledge of subsurface ocean temperatures would be equally valuable to submarines in avoiding sonar detection. If a subsurface temperature gradient existed, a submarine could hide in the shadow zone and be immune from sonar. They designed a bathythermograph for submarines, and this was widely installed.

Redfield and Vine devised a completely different method for submarine use of vertical temperature gradients. They realized that a submerged submarine could control its buoyancy with sufficient accuracy to be able to "sit on a layer" (i.e., to remain in the middle of a vertical ocean temperature gradient without moving). The submarine could shut down its motors and remain absolutely quiet for many hours, thus avoiding detection by listening as well as echo ranging. Together with their colleagues Dean Bumpus and William Schevill, they installed submarine bathythermographs and taught this technique with great success to the U.S. fleet submarines in the Pacific. In Washington the oceanographic unit of the Navy Hydrographic Office under Mary Sears (by that time a WAVE lieutenant) and the Sonar Design Division of the Bureau of Ships cooperated with Redfield and Vine in producing training manuals and *Submarine Supplements to the Sailing Directions.* This work was done under the sponsorship of Division Six of the National Defense Research Committee—part of the Office of Scientific Research and Development, led by Vannevar Bush—and the astronomer Lyman Spitzer was very much involved, as was I.

The Woods Hole Oceanographic Institution never returned to its prewar status after World War II. The Navy and the country had learned of the value and importance of oceanographic research. The Bureau of Ships and the Office of Naval Research replaced the National Defense Research Committee in providing generous support for oceanography. But, unlike the war years, that support was given for basic undirected research. After a few years they were joined by the National Science Foundation and to a lesser extent by other federal agencies.

Alfred and Martha remained as residents of Woods Hole, although he retained his Harvard professorship. By this time he was senior oceanographer, as well as associate director of the institution. They purchased 5 acres of land at the corner of Water Street and Maury Lane, built a house on one corner of it, and gave much of the rest of the area away to a church and to the Oceanographic Institution. But they kept enough land to cultivate a vegetable and flower garden, which both Alfred and Martha farmed vigorously and effectively, almost until the end of their lives.

Redfield turned his research interests to the study of tides in coastal waters and the ecology of the salt marshes that characterize the subsiding East Coast of the United States. He concentrated on the marsh near Barnstable on Cape Cod, which he had known since boyhood. He showed that the marsh has developed over the past 3,000 to 5,000 years, as sea level has risen about 15 centimeters per century. The present surface around the edges of the estuary is at high water level and is vegetated with three marine grasses, which form a firm turf, punctuated with pond holes. Beneath the surface are consolidated deposits of peat 5 or more meters thick. Within the estuary there are patches of Spartina alterniflora that catch drifting sand to form small islands with relatively high levees around the edges. These islands are submerged about 30 centimeters below high-tide level. In their origin and behavior they resemble somewhat the "coral heads" on the floor of coral atoll lagoons in the Central Pacific.

The slowly accumulating peat in the salt marsh gave an opportunity to measure the heat flow from the interior of the earth. By measuring summer and winter temperatures at different depths of the peat deposits, it was possible to compute the average temperature gradient with depth. Combining this with the average thermal conductivity, two similar values for the upward heat flow were obtained, averaging 1.47 x 10-6 cal cm^{-2} sec^{-1}, very close to the average continental value of $1.43 \pm 0.57 \times 10^{-6}$ cal cm^{-2} sec^{-1}.

Redfield's last scientific paper, "The Tides of the Waters of New England and New York," was published shortly before his ninetieth birthday. In this treatise he dealt with the predictions of time and height of the tide and the velocity and direction of related tidal currents in the inshore waters of the northeastern United States, where bottom topography and narrow channels result in complex distortions of the tidal wave.

What sort of man was Alfred Redfield? His younger colleague, William S. Von Arx, has described him well:

> Alfred Clarence Redfield was an inherently civilized man: urbane, courtly, gracious, but at the same time forthright, redoubtable and demanding. He was neither modest nor immodest. A man of pure reason, he was thoroughly convinced of his own worth, yet always open to rational improvement and adventure. He enjoyed his mark as a plain-mannered patrician; one who could move as easily in the company of artisans and tradesmen as among scholars . . . . His working methods were honest expressions of his character. He was logical and not above hard work in all he did as a gentleman farmer, sailor, citizen, forester, architect, historian, lecturer, writer, pipe-smoking editor and friend.

Redfield was well appreciated by his peers. He was a fellow of the American Academy of Arts and Sciences and was elected to the National Academy of Sciences in 1958. The National Academy gave him its Agassiz Medal in 1956 for original contributions to oceanography. He became president of the Ecological Society of America in 1946 and president of the American Society of Limnology and Oceanography in 1956, at which time he was largely responsible for establishing the society's publication, *Limnology and Oceanography.* For many years he was also editor of the *Biological Bulletin.* In that capacity he gently improved the scientific writing of a generation of American biologists. He was a trustee of both the Woods Hole Oceanographic Institution and the Marine Biological Laboratory and president of the Bermuda Biological Station for Research in the early 1960s.

Like some other members of the staff of the Oceanographic, Redfield took an active and continuing interest in the village of Woods Hole and the town of Falmouth. He was a member of the Falmouth Town Meeting, the forest and finance committees of the town, president of the Woods Hole Public Library, and a member of the Cape Cod Chamber of Commerce. Laboratory buildings are named for him at both the Oceanographic and the Bermuda Biological Station. In 1960 he obtained a Woods Hole twin-masted "spritsail boat," which was then seventy years old. He restored it and donated it to the Mystic Seaport Museum.

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Biographical Memoirs V.67

John Milton Roberts

December 8, 1916-April 2, 1990

By Ward H. Goodenough

John Roberts, "Jack" to all who knew him,[1] has been justly characterized as "one of the most brilliant and creative thropologists of the second half of the twentieth century."[2] His brilliance and creativity were not only in anthropology, in the prevailing understanding of that term, but more broadly in behavioral science. He had a penchant for looking at things that others thought unimportant or took for granted and for coming up with intriguing insights and discoveries, at times with profound implications for anthropological and social psychological theory, at other times with equally profound implications for the practical conduct of human affairs.

Roberts was intrigued by problems that seemed too "messy" to most social scientists and not easily amenable to systematic data gathering, quantification, or rigorous analysis. Thus, although experimental in his approach, his work was largely in relation to subjects where carefully controlled experiments were often impossible and where the kinds of cultural and societal data available were far from adequate for controlled comparison. Creative in this as in other respects, Roberts relied on the approach that examines an idea in relation to several independent lines of evidence, where evidence in any one line is not in itself conclusive. He was convinced that support for an idea from several such lines could be as compelling, cumulatively, as support from a single, beautifully contrived and controlled experiment.

He was a "consummate collaborator" and published more often than not as coauthor with colleagues "on an extremely wide variety of topics and on numerous cultures, always drawing upon and drawing out the expertise of his coworkers."[3] His colleagues valued him highly for his intriguingly different ways of looking at things that so often provided productive veins for them to mine in their own research.

Roberts was born in Omaha, Nebraska, the only child of John M. Roberts, senior, and Ruth Kohler, his father's second wife. He had a much older half brother and half sister. His father, a highway engineer, moved the family to Lincoln, when Roberts was a year old, and it was there that he grew up and went to school, taking his A.B. degree at the University of Nebraska with distinction in 1937. He did not find the study of law at the University of Chicago congenial and left it after one quarter in the fall of 1937. He switched to anthropology, which he continued at the University of Chicago for two more quarters in 1938 and 1939, when he transferred to Yale. While there he studied with G. P. Murdock, B. Malinowski, C. S. Ford, and John Dollard, among others, and worked under Murdock as research assistant on the Cross-Cultural Survey (which became the Human Relations Area Files, Inc., after World War II).

His studies were interrupted in February 1942, when he was called up as a captain in the U.S. Army Reserve, less than a year after his marriage to Marie Louise Kotouc of Lincoln, Nebraska. In World War II, he commanded a company of infantry in northern France and was awarded the Silver Star for gallantry in action and the Bronze Star for meritorious service. He returned to Yale in November 1945, left for fieldwork among the Navaho in February 1946, and received his Ph.D. in 1947.

After one year as assistant professor of anthropology at the University of Minnesota, Roberts went, still as assistant professor, to Harvard's Department of Social Relations in 1948. He was also a research associate in the Laboratory of Social Relations and an assistant curator in the Peabody Museum. At Harvard, he was made coordinator of the project called The Comprehensive Study of Values in Five Cultures, which carried out field research from 1949 to 1953 in Zuni, Navaho, Spanish-American, Mormon, and Texan Homesteader communities in western New Mexico. This program was under the overall direction of Clyde and Florence Kluckhohn, who had already conducted long-range field research with the Ranch Navaho. Roberts' own research at that time was mainly with the Zuni.

From Harvard he went to the University of Nebraska in 1953, as associate professor, and went from there to Cornell University as professor in 1955. Three years later his wife died suddenly, victim of a stroke, a severe blow to Roberts and their two daughters, Tania Marie and Andrea Louise. In 1961 he married Joan Marilyn Skutt, who brought new happiness to his life and with whom he had two sons, James Barton and John Milton, Jr. Roberts remained at Cornell until 1971, when he went to the University of Pittsburgh, succeeding his former teacher, G. P. Murdock, as Andrew W. Mellon Professor. While at Pittsburgh he was also appointed Adjunct Mellon Professor of Sociology in 1975. He retired from both positions in 1987.

Roberts was a fellow at the Center for Advanced Study in the Behavioral Sciences in 1956-57, taught at the Summer Seminar in Quantitative Anthropology at Williams College in 1966, and was acting chair of the Department of Anthropology at Cornell in 1966-67. He held the chair of com parative cultures, Naval War College, Newport, Rhode Island, in 1969-70. In the spring quarters of 1975 and 1984, he was visiting professor of anthropology at the University of California, San Diego, and in the spring quarter of 1987 he was visiting distinguished professor at the School of Social Sciences, University of California, Irvine.

Roberts was president of the American Ethnological Society (1960), the Northeastern Anthropological Association (1965-67), the Society for Cross-Cultural Research (1974-75), and the Association for the Anthropological Study of Play (1979-80). He was elected to the National Academy of Sciences in 1982, where he was to have served as chair of Section 51 (anthropology) for a three-year term (1990-93).

In 1989 Roberts was honored with the publication of a massive Festschrift volume containing twenty-eight papers by thirty-eight contributors. [2] They show clearly the breadth of Roberts's interest in and impact on the social and behavioral sciences.

In keeping with that breadth, Roberts's principal contributions to science were in several disparate areas. First among them was his contribution to the ethnography of the Navaho and Zuni. Of special importance for cultural theory was his Navaho work. Anthropologists had widely recognized individual and local differences in how people did things and understood things but had ignored such differences in regard to culture theory. Roberts was the first to undertake systematically to document cultural differences at the household level (1951) and to stimulate anthropological recognition that every social group at every level of societal organization had its own distinctive culture. Culture theory had to account not only for this variation but also the processes that kept it within limits through time. Roberts further documented such differences in his study of daily life in Zuni (1956).

The role of culture in the processing of information was another topic in which he pioneered, notably in "The Self-Management of Cultures" (1964). Not only did small groups differ culturally, individuals differed in their knowledge of and expertise in the culturally structured activities of the groups of which they were members, another of those self-evident and hence ignored truths whose significance Roberts documented in his papers on Butler County Eight Ball (1979, 1984), lathe craft (1987), and the humble game of tic-tac-toe (1965), so that it could no longer be ignored.

How people conducted themselves in their various activities was not only a matter of knowledge and expertise but, as in tic-tac-toe, also presumably a matter of personality differences, but its demonstration and implications remained to be documented. Using psychological tests that sorted people into those who are "high self-testers" against those who are not, Roberts showed how this personality difference affected performance in driving automobiles (1972), in the conduct of war games (1972), and in the posting of speed limits on highways (1972), with obvious practical implications for driver education, military command, and highway safety.

The studies of games and pastimes that Roberts undertook in collaboration with Brian Sutton-Smith and others are a major contribution in themselves. His paper with Arth and Bush on "Games in Culture" (1959) developed what is now recognized as the standard classification of games into those of physical skill, strategy, and chance. In many subsequent publications, especially with Sutton-Smith, Roberts showed how such games serve in all societies as models of different kinds of activities in real life, activities that differ in the kinds of roles they require of their participants in such things as competing successfully, accomplishing objectives while being loyal and obedient, and being willing to make decisions with inadequate information and take responsibility for the consequences of those decisions. His collaborative researches into what he came to refer to as "expressive culture" also explored how activities involving music, hobbies, and riddles, similarly provide models of real-life situations through which people not only rehearse social roles but also find ways of managing the emotional conflicts that are associated with these roles. This work has, again, had practical implications for understanding the emotional bases for addiction to various kinds of games as different as football, poker, and bingo.

In his later years Roberts had begun, with Hugo Nutini, to study expressive behavior among Mexican aristocracy; and at the time of his death, he was about to undertake, with Garry Chick, a study of how work, leisure, and technological change were influencing the lives of machine shop workers in western Pennsylvania.[3] Funding for the latter had been approved by the National Science Foundation only a few weeks before Roberts's death. These projects, as well as many more that his work has inspired, go on after him.

We who knew him shall not forget the excitement he brought to the behavior science enterprise, his joy in exploring ideas, his generosity in sharing his ideas and letting others run with them, and the intellectual enrichment our discourse with him invariably gave us.

NOTES

1. I am indebted to Marilyn Skutt Roberts, Evan Z. Vogt, D. Fred Wendurff, and Garry Chick for information and helpful comments on an earlier draft of this memoir.

2. R. Bolton. *The Content of Culture: Constants and Variants. Studies in Honor of John M. Roberts.* New Haven, Connecticut: HRAF Press (1989).

3. G. Chick and H. G. Nutini. John Milton Roberts. *Anthropology Newsletter* 31(6) :4-5.

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Ernest Robert Sears

October 15, 1910-February 15, 1991

By Ralph Riley

Ernest Robert (Ernie) Sears was born on October 15, 1910, in the Bethel community about ten miles west of Salem in the Willamet Valley of Oregon. His parents, Jacob P. and Estella McKee Sears, were members of a large family in which teaching or farming were the principal occupations.

In his rural school at Bethel three teachers taught four grades in a single room and out of school Ernie Sears's time was mainly given to farming activities. Through 4-H Club work he got to know Oregon State College (now University), and in due course he enrolled there in the School of Agriculture. In the Farm Crops Department he enjoyed the courses on plant breeding given by Earl N. Bressman. It was Bressman who arranged for Sears to do graduate work with Professor E. M. East at Harvard in 1932. Professor East was in the Bussey Institution of Applied Biology, where Sears was also brought into contact with W. C. Castle, Karl Sax, and I. W. Bailey.

Sears left Harvard with a Ph.D. in 1936 and moved to the University of Missouri at Columbia. There he commenced work on May 1 on a USDA project concerned with polyploids under L. J. Stadler. His colleagues on the project were J. G. O'Mara and Luther Smith.

Ernie Sears' research for a period of more than fifty years, since the publication of his first Missouri paper in 1939, concentrated on wheat and its relatives. It had a coherence and an inter-relatedness that makes it hard to subdivide and make episodic. However, I have attempted to do this to understand better the totality of Sears's practical and intellectual attainments. The components that to some degree are separable are those concerned with:

- Evolution, phylogeny, and systematics of wheat and its relatives;
- Genetic structure and analysis of polyploids;
- Misdivision of chromosomes and the breakage products;
- Introduction into wheat of alien genetic variation; and
- Genetics of meiotic chromosome pairing.

This subdivision of his work was probably never in Sears' mind when he was doing the research.

EVOLUTION, PHYLOGENY, AND SYSTEMATICS

Among the earliest of Sears' works were studies of hybrids between diploid species in the Triticinae (the family in which wheat is placed), and the amphiploids derived from the treatment of these hybrids with colchicine. Papers on these subjects published in 1939 and 1941 gave considerable attention to the methodologies of colchicine treatment as well as to the relationships between chromosome pairing in the initial hybrids and the derived amphiploids. This was the origin of a lifelong involvement in the study of polyploid evolution and on the genetic and meiotic equivalences and distinctions between the chromosomes derived from different parents in the creation of allopolyploids.

At this period a very influential discovery by E. S. McFadden and Sears was that the amphiploid between *Triticum turgidum* and *Aegilops squarrosa* was phenotypically very close to *Triticum spelta.* This confirmed earlier inference from hybrids involving *Aegilops cylindrica* that the seven pairs of chromosomes (genome) in hexaploid but absent in tetraploid wheat had been derived from *Ae. squarrosa.*

I have here ascribed the wild relatives of wheat studied by Sears to the genus *Aegilops,* which was the attribution used by him at the time. Subsequently, he and Rosalind Morris (1967) accepted that the application of the rules of taxonomy required the amalgamation of *Aegilops* and *Triticum* into a common genus called *Triticum.* However, in one of the last conversations I had with Sears, in 1990, he explained that he now considered that more appropriate usage required reversion to the use of the generic name *"Aegilops."*

GENETIC STRUCTURE OF POLYPLOIDS

Aneuploids

From his earliest work, Sears strove to comprehend the cytogenetic structure of hexaploid wheat. His analysis commenced in 1939 with the study of the thirteen mature plants obtained by the pollination of two 21-chromosome haploid plants found in the *T. aetivum* (*T. vulgare* in the terms of the day) Chinese Spring. Monosomics, nullisomics, trisomics, and tetrasomics occurred either in the direct progeny of these haploids or in their derivatives. By 1944, among these derivatives, seventeen of the possible twenty-one monosomic lines had been isolated and the related nullisomics observed. Also in the 1944 study, six of the seven chromosomes of the D genome were identified and the first example of nullisomic-tetrasomic compensation was described. Nullisomics permitted definition of the chromosomal location of genes, among others for red seededness, hooding, awning, and speltoid suppression. Nullisomic III (3B) was revealed to be a rich source of monosomics for other chromosomes because of an increased level of chromosome pairing failure at meiosis.

Ultimately, this led to the formidable work that culminated in the creation of the most complete aneuploid series known in any organism. This work was crowned by the publication in 1954 of "The Aneuploids of Common Wheat" as Research Bulletin 572 of the University of Missouri, College of Agriculture, Agricultural Experimental Station. The genetic effects of each chromosome were described, in turn, on the basis of its nullisomic effects. The definition also occurred at this time of seven homoeologous groups based on nullisomic-tetrasomic compensation. Homoeologous chromosomes are those of corresponding genetic activities each derived from a different diploid ancestor of allohexaploid wheat. Recognition of the homoeologous group and the genome in which each chromosome occurred allowed every wheat chromosome to be designated with a number and a letter showing its place in the overall chromosome organization.

In "Nullisomic Analysis in Common Wheat" *(American Naturalist* 38 (1953):245-52) the processes of genetic analysis are described. First is the recognition of gene absence in nullisomics. Next is the use of distortions of the normal Mendelian segregation ratios in F2s derived from a hybrid monosomic for a chromosome carrying the dominant allele. Expression of the recessive phenotype in monosomic Fls enables the identification of chromosomes with recessive alleles. The intervarietal substitution of intact chromosomes became possible with the availability of complete monosomic series, so enabling another form of genetic analysis.

From Sears' commitment to aneuploids arose a new struc ture of knowledge on homoeology and on the genetic organization of polyploid wheat.

Wheat Genes

Not surprisingly, because of his discovery of methodologies for determining the chromosomal locations of genes, Sears used this procedure to help others with practical problems. These included identification of the genetic status of disease resistance genes—for example, in 1957 with W. A. Loegering and H. A. Rodenhiser. In addition, using chromosome substitutions, the stem rust resistance genes in Hope, Thatcher, Red Egyptian, and Timstein were positioned on nine different chromosomes. Telocentric mapping subsequently enabled *Sr9* and *Sr16,* respectively, from Red Egyptian and Thatcher to be placed about forty cross-over units apart along the long arm of chromosome 2B. Subsequently, bunt and powdery mildew resistance genes were chromosomally located.

Aneuploids also enabled Sears to contribute to the understanding of gene action in wheat. In particular, he was the discoverer of the hemizygous ineffective condition in which, when a recessive allele is carried on a monosomic chromosome, so no dominant allele is present, the recessive phenotype is not expressed. Sears explained that two doses of such recessives are necessary for gene products to pass the threshold at which the recessive phenotype appears.

Ernie Sears' astonishing discoveries show that many homoeologous genes in hexaploid wheat continue to perform essentially the same function as in the diploid ancestors from which they came. Nowadays, RFLP mapping, by and large, shows similar gene orders on homoeologous chromosomes and that, although isoenzymes may show some differences, homoeo-alleles generally still produce essen tially the same proteins as each other. Genetic conservation and chromosomal stability are the principal characteristics of wheat displayed by Sears and his followers.

CHROMOSOMAL MISDIVISIONS AND BREAKAGE

For a research worker with monosomics, a concern for the misdivision of univalents was necessarily important. Sears was naturally drawn to their investigation and first reported on telocentrics and isochromosomes in 1946. He studied univalent misdivision at TI and TII of meiosis, concentrating particularly on chromosome 5A. In addition, he described the formation of isochromosomes from telocentrics and the reciprocal process. All of this was in conformity with other research on chromosome cytology of the period. However, the availability of the wheat monosomic lines enabled him to accumulate the telocentrics and isochromosomes for many different chromosomes. Consequently, by the publication in 1954 of "The Aneuploids of Common Wheat," descriptions could be included on telocentrics or isochromosomes for every chromosome of wheat complement.

By 1966 Ernie Sears could advocate genetic analysis by telocentric mapping because 42-chromosome lines, with one chromosome represented by a telocentric in the disomic condition, were available for every chromosome of the wheat complement. Mapping determined the frequency of crossing over between a genetic locus and the centromere.

Ernie Sears' wife, Lotti Steintz-Sears, had been a major collaborator with Ernie in this work on misdivision products, and by 1974 and 1978 they were able to report on the availability of an almost complete set of twenty-one 44-chromosome lines in which every wheat chromosome was represented by a pair of telocentrics for both arms of the chromosome. This was a quite astonishing achievement requiring perseverance and dedication to create plant material of enormous benefit to wheat geneticists.

ALIEN GENETIC VARIATION

About the time that Sears worked on interspecific hybrids between 14-chromosome species related to wheat and published his first study on aneuploids, J. G. O'Mara in Missouri was researching wheat-rye hybrids, triticale, and wheat-rye chromosome addition lines. O'Mara's work was done over the period of the late 1930s to the early 1950s. I do not know how scientific responsibilities were shared at that time, but Sears did not turn to wheat-rye combinations until some time after this period. Instead, he produced in 1953 hexaploid wheat forms to which were added the 7-chromosome haploid complement of *Haynaldia villosa.* Remarkably this was achieved by hybridizing *T dicoccoides (2n=28) x H. villosa (2n=14)* and by top crossing the hybrid with *T. aestivum* and backcrossing to *T. aestivum.* Subsequently, Beale Hyde used this material to make wheat lines in which, in turn, every chromosome of *Hynaldia* was separately added to wheat.

At the 1956 Brookhaven Symposium, Sears described remarkable work in which leaf rust resistance of *Aegilops umbellulata* was transferred to common wheat. This commenced with the addition to *T. aestivum* of a single chromosome of *Ae. umbellulata* that caused rust resistance. The added chromosome also produced economically disadvantageous modifications to the phenotype of T *aestivum.* Consequently, if the rust resistance were to be made available for wheat improvement, the determinant of resistance had to be disassociated from other deleterious genes on the chromosome. Using entirely innovative techniques, Sears X-rayed plants of wheat to which was added an isochromosome of the resistance-determining arm. The irradiated plants were used to pollinate normal wheat and resistance progeny were selected. Forty of these had one of at least seventeen different translocations between the *Aegilops* chromosome and wheat chromosomes. There was one line with the resistance chromosome segment apparently incorporated in the form of an intercalary translocation. The line had normal pollen transmission and was not detectable cytogenetically. Further work, published in 1966, showed that the *Ae. umbellulata* segment was not in an intercalary position but that a long *Aegilops* segment had replaced the terminal part of the long arm of wheat 6B. Undoubtedly, the absence of significant deleterious effects was related to the homoeologous relationship between the *umbellulata* segment introduced and the 6B segment removed. The alien leaf rust resistance introgressed with wheat in this way has been of considerable economic significance. A remarkable new technology had been created that was subsequently used in several laboratories outside the United States to incorporate other forms of alien disease resistance into wheat.

The notion that bibrachial chromosome could be created from the fusion of telocentrics of different, even unrelated, chromosomes was proposed by Jack (J. W.) Morrison in 1954, and several apparent examples were described by Muramatsu and Sears in 1969. However, all of them might have been explained alternatively as having arisen by homoeologous recombination. To evaluate this, Sears set up an experimental situation in which chromosomes 6B and 5R were simultaneously monosomic and showed that bibrachial chromosomes with one 6B and one 5R arm were produced with a frequency compatible with the separate likelihoods of simultaneous misdivision in each univalent. This validated the potential usefulness of centric fusion in breeding when deletion of a wheat chromosome arm is not phenotypically severe and where the added alien arm does not incorporate deleterious genes as well as the beneficial gene.

By 1971 Sears was able to report on the isolation of every chromosome of Imperial rye as a separate addition line to Chinese Spring, so completing work the first stages of which had been reported in 1958. Thus, Sears took over and completed for the USDA the investigation initiated by J. G. O'Mara in 1940.

GENETICS OF MEIOTIC CHROMOSOME PAIRING

One of the discoveries that wheat chromosome 5B has genes that affect chromosome pairing at meiosis was made in Sears's laboratory by M. Okamoto. Ernie Sears encouraged the work of Okamoto on these genetic systems. Subsequently, also in his laboratory, the work of Moshe Feldman resulted in proposals about the processes by which the *Ph* locus on chromosome 5B confines meiotic pairing to fully homoeologous partner chromosomes.

Although involved in the encouragement of work of this kind and as a frequent commentator on the genetics of chromosome pairing, Sears' direct involvement has principally concentrated on attempts to mutate the *Ph* locus. After considerable research, in 1977 Sears was able to report on the production of a viable mutant that appeared to be a deficiency for the *Ph* locus, on 5B, or in which the locus was rendered ineffective. I will rehearse some numbers from this work because they indicate the scale and patience that characterized Ernie Sears' work. X-irradiated euploid pollen was applied to previously emasculated spikes of *T. aestivum* plants monosomic for a genetically marked chromosome 5B. One thousand two hundred seventy-eight offspring were obtained, of which 675 were immediately eliminated because they carried the marked chromosome 5B or because they were clearly nullisomic for 5B. Those with the marker would not display any mutation to recessive that had occurred in the irradiated 5B chromosomes. Four hundred thirty-eight of the retained offspring were tested for changes in the regulation of pairing. One mutant was isolated that was apparently deficient for the *Ph* locus. This mutant, designated *phlb,* could be made homozygous and, although somewhat reduced in vigor and fertility, has proved to be useful in breeding and research. The persistence and perseverance that were regularly part of Sears' work are well revealed by this example—namely, pollinate hundreds of spikes, search through an initial 1,278 offspring to find one mutant, and use markers to simplify the task.

CHINESE SPRING

No tribute to Ernie Sears could omit a mention of Chinese Spring, the variety that, as a result of his work, has become the reference base for all wheat cytogeneticists. Sears and T. E. Miller have reported on this. Chinese Spring was the variety in which Sears first obtained the two haploids that were the origins of the aneuploids. The haploids arose in work on wheat-rye hybrids being used to test for chromosome doubling by heat shock. Chinese Spring was used because of its ready cross-ability with rye. It appears that the variety originated in Szechuan, China, and traveled via Cambridge, England, to North Dakota and Saskatoon, Canada, and then to Columbia, Missouri, before being used by Sears.

Although the use of Chinese Spring is often scorned by breeders, it has without doubt made possible an explosion of scientific knowledge about wheat.

A CAREER IN WHEAT SCIENCE

All who followed Sears' work and benefited from it will acknowledge the characteristics that pervaded it for more than five decades. They were intellectual rigor, experimental flair, cytogenetical insight, precision of communication, desire to collaborate, and extreme generosity with plant material and ideas. No matter how close or remote geographically any wheat cytogeneticist was to Ernie Sears all are in his debt.

Sears enjoyed the loyalty of the Missouri Agricultural Experimental Station and the University of Missouri, Columbia, for more than five decades. He returned it equally because he felt that Columbia was the place where he could work most effectively and comfortably. During his time at the university in Columbia, he was clearly much influenced in the first instance by L. J. Stadller. Among his many other distinguished colleagues were Alex Faberge, Melvin Green, Jack Shultz, George Sprague, A. P. Swanson, and Barbara McClintock. In his wheat group for varying periods of time were Moshe Feldman, Bill Loegering, Tris Mello-Sampayo, K. Tsunewaki, Gordon Kimber, Bikram Gill, Bob MacIntosh, Henry Shands, M. Okamato, M. Muramatsu, and Beale Hyde. While working collaboratively with these colleagues, and certainly greatly influencing their work, often his name did not appear on the papers that emerged. Ernie Sears was a modest person, even self-deprecating. His self-deprecating humor showed up in a conversation I had with him after dinner at my home in Cambridge, England, in 1958. In describing his home in Columbia, Missouri, Sears said, "Yes, I have a back yard. I planted 1,000 pine; 990 died."

His joy was the wheat plant and its relatives. It was rare for him to work on any other organism. The value of his work was widely recognized and brought him many well-deserved honors. Among these was the Hoblitzelle Award for Research in Agricultural Sciences. This $10,000 enabled him to buy his attractive home in Columbia, which characteristically he called "Mob Hill." Here he raised his happy family of three children by his second wife, Lotti Steinitz-Sears—John, a medical student; Barbara, now widowed and associate professor of botany and plant pathology at Michigan State University; and Katie, now married and living in Minneapolis. Mike, the son of his first wife Caroline, is now director of the Cloverwork Foundation.

Sears remained very fit right up to his death, playing tennis and badminton and cutting his four acres of grass at Mob Hill with only a motorized push mower. He was unflappable and simultaneously generous and frugal, the epitome of a hands-on scientist. He had the minimum of technical assistance—potting his own plants, watering them, making his own pollinations and slides, and harvesting and meticulously storing the seeds of an enormous collection of genotypes. It was only in this way that Sears felt he thoroughly understood his material and was able to "treasure his exceptions." The way E. R. Sears dedicated himself to a very specialized branch of science is an example to us all.

HONORS AND DISTINCTIONS

	ACADEMIC DEGREES
1932	B.S., Oregon State College
1934	M.A., Harvard University
1936	Ph.D., Harvard University
	HONORARY DEGREE
1970	D.Sc., Göttingen University
	AWARDS
1951	American Society of Agronomy, Stevenson Award
1958	Gamma Sigma Delta National Award for Distinguished Service Hoblitzelle Award for Research in Agricultural Sciences
1970	Sigma Xi Research Award
1973	Oregon State University, Distinguished Service Award
1977	Genetics Society of Canada, Excellence Award
1980	Hard Red Winter Wheat Workers, Wheat Science Award
1981	National Agribusiness Association, Agricultural Science Award
1983	Missouri Academy of Science, Scientist of the Year
1986	Wolf Prize in Agriculture
1990	University of Missouri, Curators Award for International Service
	USDA AWARDS
1958	Superior Service Award
1980	Distinguished Service Award
1987	Science Hall of Fame
	LEARNED SOCIETIES

National Academy of Sciences (1964)

American Academy of Arts and Sciences (1953)

Genetics Society of America (President, 1978-79)

American Society of Agronomy (Fellow)

Botanical Society of America

American Society of Naturalists
American Association for the Advancement of Science (Fellow)
American Institute of Biological Sciences
Genetics Society of Japan (Honorary)
Indian Society of Genetics and Plant Breeding (Honorary)
American Association of Cereal Chemists (Honorary)

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B. F. Skinner

Burrhus Frederic Skinner

March 20, 1904-August 18, 1990

By Howard Rachlin

Although Skinner saw himself and was seen by others as a psychological revolutionary—the type of behaviorism he founded is called *radical behaviorism* —he was also in a sense a conservative of American culture in American psychology. His rebellion was against the nineteenth-century German academic psychology brought to this country by Hugo Munsterberg and E. B. Titchner. Skinner's behaviorism represents a reaction from this basically romantic psychology with its focus on the "inner man," possessing an inner theater where "the life of the mind" could be played out independent of life itself.

Skinner was a true descendant of the American pragmatism of William James, John Dewey, and C.S. Pierce; the fact that Skinner was a William James Lecturer at Harvard is thus satisfyingly appropriate. The core of American pragmatism predominant in Skinner's work is its brilliant clarity, its focus on "pragmatic questions," and its avoidance of mysticism—represented in psychology by self-centered introspection.

Another strain of early American culture in Skinner's behaviorism is its emphasis on engineering above theoretical science, on building machines rather than building theo ries. Still another quintessentially American strain in Skinner's behaviorism is its democratic optimism. Skinner, following John Watson, behaviorism's founder, believed that given the opportunity most people could make themselves into anything they wanted. There is something in Skinner of Tom Swift, a sort of gee-whiz, can-do attitude. Here is a piece of a letter from Skinner to Fred Keller (erstwhile fellow graduate student, yet-to-be lifelong colleague, collaborator, close friend throughout) about a book of Keller's. The letter was written in 1937 while Skinner himself was just finishing *The Behavior of Organisms: An Experimental Analysis,* his first and best book:

> In the midst of a busy morning I stopped to read the section on functionalism and found it grand . . . I really didn't think you could do it, old man, it gives me a hell of a kick.

The letter is quoted by Skinner on p. 209 of *The Shaping of a Behaviorist* (New York: University Press, 1984), the second volume of Skinner's three-volume autobiography.

In his restrained scientific works, in his more relaxed popular works, and in his letters to Keller, where he let go completely, you will find a common thread, a practical, patient encouragement of both intelligence and industry and above all a tendency to identify the former with the latter. Skinner believed that behavior could and should be studied scientifically. But he believed, more than that, that life itself is a science. "If I can do it, you can do it," Skinner's work seems to say—from educating a child, to conducting a scientific experiment on a rat's behavior, to writing ten pages a day, to designing a utopian society, to enjoying old age. And he did it, Skinner implied, not because he was some sort of head-in-the-clouds genius but simply because he saw life as a subject of scientific study. As to his scientific contri butions, their importance is a matter of current interest and dispute and will occupy most of this memoir.

Since Skinner produced a three-volume autobiography, there is no necessity to do more than outline his life's "particulars." He was born in Susquehanna, Pennsylvania, a railroad town just below the New York border. The nearest reasonably large city is Binghamton, New York. Susquehanna is close to the snow belt below the Great Lakes and winters were cold. Old ladies wore "creepers," miniature crampons that could be folded into their boot's instep, to avoid slipping on the ice:

> In cold weather I pulled my underclothes into bed with me to warm them and put them on under the covers. Then I dressed quickly, washed, cleaned my teeth, brushed my hair, and went down to hover (with my brother and in very cold weather my mother) on the large grille between living room and dining room where the first warm air [from the furnace] had begun to rise. *(Particulars of My Life,* 1976, p. 24)

From his boyhood on, Skinner was always building and inventing things. As a very young boy he designed a system for getting oxygen from seawater (whether it worked or not, he doesn't say) and played with electric motors, magic lanterns, and stereopticons. As a young man he built model ships. *Particulars of My Life* includes a photo captioned, "The maker of ships on his [twenty-third] birthday," showing a studious-looking young man with an immense pipe sitting next to a model galleon (sails *plus* oars) of incredible complexity. You would have thought at this point that he'd grow up to be another Henry Ford or Thomas Alva Edison, and indeed there was much of the inventor in him. At one point Skinner said that his greatest contribution to psychology would be the cumulative recorder, a device (invented by Skinner while a graduate student) to record discrete actions like a rat's lever presses or a pigeon's keypecks or a person's button pushes as a continuous cumulative line. Here is his description of its invention:

> It so happened that a short spindle, like the hub of a wheel, was attached to one side [of a pellet feeder he had constructed], and I had left it in place. It occurred to me that if I wound a thread around the spindle, it would unwind slowly as the disk turned and could be made to lower a marker on the kymograph drum. The marker would record a curve rather than a row of marks. When the rat was working rapidly, the thread would be played out rapidly and the line would be rather steep, but as the rat slowed down, the curve would grow flatter. From the slope I could estimate the speed at which the rat was working at any moment. *(The Shaping of a Behaviorist,* 1984, p. 56)

From the "cumulative record" it was possible for an experienced observer to perceive patterns in an animal's response rate as it changed over time. Skinner hoped that the cumulative recorder would become a sort of X-ray machine for psychology. When it fell out of use, he wrote an article, "Farewell My LOVELY!" (1976), lamenting its fate. Skinner's aircrib, his teaching machine, the pigeon-operated guided missile, the Skinner-box itself, are all extensions of his early inventiveness.

At Hamilton College (B.A., English literature, 1926), Skinner's main interest shifted more and more to writing. He met Alexander Wolcott, Carl Sandburg, and Robert Frost. Frost read some of his stories and wrote Skinner a long approving letter ending with: "I ought to say you have the touch of art. The work is clean run. You are worth twice anyone else I have seen in prose this year" (*Particulars,* p. 249).

After graduating from college, Skinner spent a year in Greenwich Village (not yet gentrified) trying to write. He succeeded better at living a bohemian life than at writing and came to the conclusion that, "I was interested in human behavior, but I had been investigating it in the wrong way." He began to read in biology and psychology: Loeb, Watson, Pavlov, Thorndike. He became a convinced Watsonian behaviorist.

Having decided to become a scientist, Skinner gave up his bohemian life, but he never dropped his cultural interests. He remained a friend of writers and musicians throughout his life. He was to make his reputation attacking romanticism in psychology, but his taste in music was for Mahler, Bruckner, and other late romantic composers. On the advice of the president of Hamilton College, Skinner applied to Harvard's psychology department and was accepted. At that point he had no conception of what psychology was like there.

I met Skinner ("was exposed to" is probably more apt than "met") in 1962, when I was just beginning my academic career as a graduate student and he was in the final years of his professorship at Harvard. (It was just a few years before he stopped teaching, but he wrote some of his most popular books and published occasional research papers throughout his "retirement" years.) In those days there were only two full professors of psychology at Harvard, Skinner and S. S. (Smitty) Stevens. Jerome Bruner and George Miller were university professors, busy with developing the Cognitive Center, and the other big names at Harvard were in the social relations department (a result of undoubtedly baroque political intrigues much above the head of a graduate student). The pro-seminar, the first big hurdle for graduate students, was directed during the first semester by Stevens. The German system had been passed to Harvard's department from Wundt to Titchner to Boring to Stevens as teacher to student, and whatever its merits or demerits it succeeded in terrifying all of us. I recall actually feeling my hair stand on end before my first pro-seminar presentation. Skinner was away somewhere during the fall and made his appear ance, only at the start of the spring semester, with a six-pack of beer—the world's oldest desensitization device. I do not say that Skinner's technique with graduate students got better results than Smitty's. It probably didn't. Smitty's terrorism was interwoven with a deep personal concern while Skinner could not, in the few short weeks available to him, penetrate our awe. We saw him as a figure, not as a human being. In his autobiography Skinner repeatedly refers to his father's problem in getting people to warm up to him (they called him "bumpy"):

> My father apparently never knew how he looked to other people. Every successful step in becoming a self-made man intensified his zeal in improving himself and he saw no reason why everyone should not improve—why Susquehanna should not be a self-made town—but few of its citizens agreed with him. (*Particulars,* pp. 38-39)

It is an irony (not lost on Skinner) that on a much grander scale he himself was to have the same difficulty.

When I came to Harvard, E. G. Boring (long since "emeritized") was a sweet octogenarian who took each incoming graduate student to dinner at the Harvard Club so we could meet psychology's grand old man. Evidently things were different in 1928. Boring was chairman and behaviorism was derided or ignored.

Skinner found a sympathetic adviser in the Biology Department in the person of W. J. Crozier. Crozier was to Skinner as the biologist Jacques Loeb had been to Watson, a source of support for a biologically based psychology divorced from introspection. Crozier, like Loeb, was interested in the behavior of organisms as a whole in response to environmental forces. Crozier (and Skinner in turn) believed that he was studying *reflexes.* The status of the concept of the reflex was much more of an issue then than now, and Skinner plunged himself into the middle of it.

In 1896 John Dewey had written a remarkably prescient essay titled "The Reflex Arc Concept in Psychology." In that article Dewey had argued that the concept of the reflex as such had no meaning *within* an organism. Nervous connections in the brain and even at peripheral levels are so incredibly complex Dewey argued that nowhere in this network could one even conceivably identify coherent, self-contained, isolable structures (organs and suborgans) that could have *stimuli* leading to them and *responses* emanating from them. A whole organism, on the other hand, is indeed a coherent identifiable system that can have identifiable inputs (stimuli) and outputs (responses). The concept of a reflex is therefore applicable to whole organisms and not to parts of the nervous system. Dewey published his article in the *Psychological Review,* then as now very widely read by psychologists. But it had no effect on what was to become an overwhelming tide of reflex-based theories of the nervous system.

Watson's earlier behaviorism (derived from Loeb and possibly Dewey) reflected Dewey's attitude. Watson, however, fell under the influence of Pavlov, who in turn had been deeply influenced by Sechenov's *Reflexes of the Brain,* the very antithesis of Dewey's admonition to American psychologists. The behaviorists, Thorndike and after him Hull, were essentially followers of the Watson-Pavlov line. Thorndike's famous "law of effect" refers to the strengthening by reinforcement (or weakening by punishment) of the very "reflexes of the brain" that Sechenov was talking about. This issue is important in understanding Skinner because internal reflexes underlie the "S-R psychology" and the "theories of learning" from which Skinner was to take such pains to disassociate himself (often not successfully). Thorndike's internal connections and Hull's r_g-s_g connections are, Skinner argued, precomputer age versions of the *internal repre* *sentations* of modern cognitive psychology. Skinner's very first publications, beginning with his thesis on "the concept of the reflex" and some of his more recent publications, "Why I Am Not a Cognitive Psychologist" (1977) and "Whatever Happened to Psychology as the Science of Behavior?" (1987), reiterate Dewey's argument. The recent computer-based "cognitive revolution" in psychology disturbed Skinner because he regarded it as a resurgence in a more complex guise of the S-R psychology he had originally fought so hard against.

Not that Skinner ever placed any faith in Dewey's philosophical argument. He does not indicate that he read it. (His book collection began with Bertrand Russell's *Philosophy,* Watson's *Behaviorism,* and Pavlov's *Conditioned Reflexes.)* Philosophy generally interested Skinner only when it seemed to confirm his already conceived ideas. He was much more inclined to accept the arguments of biologists and physiologists (like Pavlov) themselves; he was especially influenced by Sherrington, whose *Integrative Action of the Nervous System* Skinner bought while a graduate student: "I read it with enthusiasm. . . . This, I was sure, was the way to study behavior!" (*Shaping,* p. 17). Sherrington conceived of the nervous system as bringing into focus a diverse stimulus array in what he called a "final common path." Skinner's thesis analyzed available evidence to demonstrate that the focus of a reflex, its final common path, could not possibly be within the organism but must be at the boundary line between the organism and its environment. A reflex, Skinner argued, is thus not a neurological entity at all but a correlation between an environmental stimulus and the organism's *overt* behavior.

Although Skinner was furiously conducting experiments throughout graduate school and afterward as a National Research Council fellow and a junior fellow at Harvard, his thesis was mostly theoretical. Nonexperimental theses were (and are) frowned upon by the psychology department, but it seems they didn't know what else to do with him.

In those days the premier tool for studying animal behavior was the maze. The first mazes used were the most complicated (the original was modeled after the famous Hampton Court shrubbery maze), but they had been getting simpler. But no maze was simple enough for Skinner: "I don't like the maze as a scientific instrument," he said. "The animal's behavior was composed of too many different 'reflexes' and should be taken apart for analysis" *(Shaping,* p. 32). Here are pieces of Skinner's description of a stage in the development of the Skinner box:

> I built a narrow rectangular track about three feet long, and mounted it like a seesaw: it tilted slightly as the rat ran from one end to the other [like a large lever that the rat walks on]. I made a food dispenser by drilling a ring of holes in a disk of wood. Pieces of food were put in the holes, and each time the rat ran around the track, the tilt turned the disk and dropped a piece into a cup. *(Shaping,* p. 56)

The Behavior of Organisms was published in 1938 after Skinner had married Yvonne (Eve) Blue and taken an assistant professorship at the University of Minnesota. It was there he first began to use pigeons as subjects. When World War II came, he designed a pigeon-operated guided missile (contemptuously rejected, although it worked). *Behavior of Organisms* sold slowly at first, although it was favorably reviewed and it gave Skinner a national reputation.

What was so remarkable about *The Behavior of Organisms?* Today it would be called, I suppose, a deconstruction of the concept of the reflex. In his thesis Skinner had argued that a reflex is a correlation between an external stimulus and an overt response. But what about the many cases where no external stimulus correlates with a given act—from playing a flute to taking a job to going to the movies to virtually all of verbal behavior? These acts, normally classified as voluntary, seem to have no external origin. The standard answer, stemming from Descartes, is that their stimulus is *internal.* It comes from "the will." But Skinner had already argued that there are no internal events that could conceivably function as "stimuli." Skinner's solution was to claim that voluntary behavior is controlled not by antecedent stimuli at all but by environmental *consequences.* And to remove all reference to internal forces like the will, Skinner called these acts *operants.* At first Skinner conceived of an operant as merely another form of reflex. But soon he dropped the notion of reflex and began to speak of operant behavior as a function of an organism's *history of reinforcement.* If a person performs a given voluntary act—a woman goes to law school, for instance—Skinner looked to its cause and source of control, not within her, not in the structure or contents of her nervous system, but in her history of reinforcement. The important questions are: which of her past actions (verbal and otherwise) were rewarded, which punished, and when? No doubt a history of reinforcement is a theory just as a hypothetical mentalistic or cognitive or physiological system inside her would be a theory. But a history of reinforcement is a theory *not* about events in an inaccessible and unobservable *place* but about events in a possibly recoverable and repeatable *time. The Behavior of Organisms* contained not only this argument (revolutionary then and still startling in its implications when seriously held) but a series of experiments that demonstrated a hitherto unique degree of behavioral prediction and control.

On the basis of his book, Skinner won a Guggenheim Fellowship in 1944. He was then hired as chairman at Indiana University. At this point he began to attract students and followers, and it could be said that a Skinnerian school was forming. After the war (1947), he went back to Harvard for a year as William James Lecturer. Evidently that was a sort of tryout since he was hired one year later as a professor. At Harvard he built "the pigeon lab," an automated laboratory where experiments were controlled by relay equipment (mostly telephone circuit equipment). In my day *all* psychology graduate students had to learn to construct a simple circuit and use it to "shape" a pigeon's peck at a translucent disk (a "key") by successive approximations. The tall vertical relay racks in the pigeon lab formed a sort of maze themselves. Dick Herrnstein, Skinner's successor as Edgar Pierce Professor and my own thesis adviser, tells the story of when as a graduate student he was standing in a corner of the pigeon lab practicing saying "Fred" instead of "Professor Skinner" when he heard "Yes, Dick" from another corner—and his problem was solved.

At Harvard, honors, awards, and honorary degrees came to Skinner. His name appeared on lists of best-known psychologists, best-known scientists, best-known Americans, best-known living people, best-known people of all time. But with this recognition came criticism, misunderstanding, and vituperation to a degree unusual for a scientist and from sources usually less violent. Some people, with a gift for quick intimacy, might have broken through the glass wall that this sort of fame can build around its object. But Fred Skinner, son of "bumpy" Will Skinner, succeeded only fitfully. In my own contact with him, sadly, the wall stood. There were several points at which it might have been breached. One of them was when I had just become an assistant professor at Harvard and Skinner had just retired from teaching. He would have been willing to conduct a small graduate seminar provided I would nominally teach the course and handle the grading and other administrative affairs. When you become an assistant professor in the same department where you were a graduate student, people tend to treat you like a graduate student. This plus my own lack of confidence resulted in an abnormal and largely self-defeating concern about status. I had written to the dean formally canceling one of my classes in retaliation for a parking ticket and now I indignantly refused to act as Skinner's TA. As I look back on the incident I see it as an attempt by Skinner to form a closer relationship with a colleague. Had he even implied such an intention I would have jumped at the chance, but I was too immature and Skinner was too "bumpy." His request to me was conveyed formally through the department chairman—and so the course was not taught.

Among his peers Skinner's very clarity worked against him. He was seen by romantics as a sort of Frankenstein meddling in mysteries better left untouched. The linguist Noam Chomsky's famous criticism of Skinner's book *Verbal Behavior* (1957) seems from the present perspective to have missed the point. It is a complaint by a Cartesian structuralist against a pragmatic functionalist, nothing more.

What then is Skinner's lasting contribution? Not, I think, his utopian vision of a self-experimental society, nor the educational technology, nor a highly successful mode of psychological therapy based on behavioral consequences, nor the Skinner box and a host of other useful inventions, nor his contribution to pharmacological testing, nor the journals and societies based on his work, nor the individuals he has influenced, nor the fact that he has put his stamp indelibly on the face of American psychology, although all of these flow from his central conception. That conception and Skinner's most lasting contribution is in my opinion more philosophical than psychological. It is nothing less than a new way to look at life; in other words (words to which he would strenuously object), a new way to conceive of the soul.

But I should not call his vision of the soul entirely new. The ancient Greeks, Aristotle in particular, conceived of souls as modes of living, as patterns of overt behavior of organisms, more or less complicated depending on species and individuals within species. Psychology for them was the identification and manipulation (the prediction and control) of these patterns of behavior, including one's own. To Skinner we owe the renaissance of this conception.

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Lee Irvin Smith

July 22, 1891-March 29, 1973

By Virgil Boekelheide

Lee Irvin Smith played an important role in the development of organic chemistry in the United States following World War I. During this period he was a leader in the development of the Chemistry Department of the University of Minnesota, especially its Organic Chemistry Division, to a position of prominence. In the course of training sixty-nine graduate students (thirteen M.S. and fifty-six Ph.D. degree recipients) and thirteen postdoctoral fellows, he pioneered research in many areas, including studies of the Jacobsen rearrangement; polyalkylated benzenes; reactions of quinones with metal enolates; the chemistry and synthesis of vitamin E; and the synthesis, structure, and properties of cyclopropanes. Smith was concerned for the success of not only his own students but for all of the students in the Organic Chemistry Division at Minnesota. Although small in stature, Smith was a born leader, had great energy, a large capacity for work, and the outstanding characteristic of insisting on excellence in every enterprise with which he was associated.

EARLY LIFE AND EDUCATION

Lee Irvin Smith was born in Indianapolis, Indiana, the son of Edgar Poe Smith (a piano maker and salesman) and Susie Louise (Amberg) Smith and was the oldest of three boys. His maternal grandmother's name was Irvin and she was Pennsylvania Dutch. When Smith was seven, his family moved to Columbus, Ohio, where he received his elementary and secondary education. In describing his early life Smith related that his home environment was pleasant but their circumstances were modest. Since his father made and sold pianos, Smith took piano lessons and developed a lifelong interest in music. At the age of eleven Smith became interested in photography, which also became a lifelong interest and provided an important source of income during his high school and college days.

Smith attended Ohio State University, and, after taking the beginning course in chemistry with Professor William Lloyd Evans, he was stimulated to undertake a bachelor's degree in chemistry. At the same time he was interested in engineering and the practical applications of chemistry. This led him to continue at Ohio State University and complete a master of arts degree in industrial chemistry, a version of chemical engineering popular at that time.

During his final year at Ohio State University, Smith became acquainted with a young professor from Harvard named Shipley, who persuaded Smith to enroll at Harvard in 1915 to study with Theodore Richards in physical chemistry. However, almost immediately after arriving at Harvard, Smith was inspired by the great teacher and scholar E. P. Kohler and changed his major to organic chemistry. When Professor Kohler became ill, Smith was selected to give his lectures in beginning organic chemistry, a course that, in those days and for many years afterward, was known affectionately as "Chem 5." It was probably during this period with Kohler that Smith developed his love for teaching and gained much of his insight into teaching methods that served him so well at Minnesota. In 1917 Smith received an M.A. degree from Harvard and was appointed an instructor there for continuing his graduate studies. Among the graduate students studying organic chemistry at Harvard at that time were Roger Adams, James Bryant Conant, Henry Gilman, Nathan Drake, and Ralph Helmkamp.

With the advent of World War I, Smith became a lieutenant in the Chemical Warfare Service and was involved in a project at Harvard on the war gas Lewisite. In March 1918 the group at Harvard working on Lewisite was transferred to the Chemical Warfare Service Laboratory at American University in Washington, D.C. The top people at this laboratory were E. P. Kohler, Roger Adams, and James Bryant Conant. With the end of the war Smith returned to Harvard, where he completed his doctorate with Kohler in 1920. In the same year he began an appointment as instructor in chemistry at the University of Minnesota.

BUILDING ORGANIC CHEMISTRY AT MINNESOTA

At that time the University of Minnesota was a relatively small institution, and the primary function of the Chemistry Department was providing instruction. Professor William Hunter was chief of the Division of Organic Chemistry, and Lee Smith, as an instructor, was the only other staff member. Despite his heavy teaching load, Smith immediately began an active program of research. Promotions to assistant professor and associate professor followed fairly quickly. After Hunter's death in 1931, Smith became a full professor and, then, chief of the Division of Organic Chemistry. Walter M. Lauer was the second staff member in the division at that time.

During these years Smith's closest friend was Frederic King Butters, professor of botany, who, like Smith, was a bachelor. Butters had a large family home in Minneapolis and Smith accepted his invitation to join him there. Later, Smith's widowed mother joined them to become housekeeper of the establishment. Smith and Butters were both very interested in horticulture and mountain climbing. Butters, Smith, and Smith's mother spent many summers in Estes Park, Colorado, enjoying these avocations.

In 1932 the opportunity arose to enlarge the staff of the division by two new appointments. The two appointments made by Smith were C. F. Koelsch and Paul D. Bartlett, and these appointments showed how exceptionally astute Smith was in judging promising young scientists. After receiving his doctorate at the University of Wisconsin, Koelsch had been a National Research Council fellow at Harvard. Within two years after his appointment to the University of Minnesota, Koelsch was the winner of the prestigious Langmuir Award (precursor to the American Chemical Society Award in Pure Chemistry). Likewise, Paul Bartlett, who received his doctorate with Conant at Harvard and who, after only two years at Minnesota, returned to a position at Harvard, also received the ACS Award in Pure Chemistry in 1938. When Bartlett resigned his post at Minnesota, Smith chose Richard T. Arnold as his successor, who in turn kept the tradition alive by receiving the ACS Award in Pure Chemistry in 1949. Smith truly had a remarkable insight into evaluating young people and their potential for leadership.

As Stan and Ann Tarbell have pointed out in their book, *History of American Organic Chemistry, 1875-1955,* the period between World War I and World War II was a time of change and development for organic chemistry in the United States. As they state, "The atmosphere of provincialism and scientific colonialism clearly discernible in American work before 1914 gradually disappeared, and by 1939 the best organic chemistry in the United States was plainly equal to the outstanding work in other countries." In large measure this development was spearheaded by midwestern universities. At the University of Illinois, Roger Adams led the Organic Chemistry Division to the forefront, and he was followed by Smith at Minnesota, McElvain at Wisconsin, Gomberg and Bachmann at Michigan, and Whitmore at Pennsylvania State University, who brought organic chemistry at their respective institutions to positions of leadership. It should be recognized, though, that E. P. Kohler at Harvard had been very influential in the training of Adams, Smith, and Whitmore.

During his forty-year tenure at Minnesota, Lee Smith was involved in many consulting, advisory, and editorial activities. As a consultant to the chemicals industry, he held long-term appointments with Merck and Company and with General Mills, Inc. Smith was a member of the Board of Editors of the *Journal of Organic Chemistry* (1936-45), the *Journal of the American Chemical Society* (1939-49), and *Organic Syntheses* (editor-in-chief of volumes 22 and 23). He was president of the Association of Harvard Chemists (1939), chairman of the Organic Division of the American Chemical Society (194142), and president of the Minnesota Academy of Sciences (1945-46). Aside from the American Chemical Society, Smith was a member of the Chemical Society of London, Deutschen Chemischen Gesellschaft, Swiss Chemical Society, American Biological Chemists, American Association for the Advancement of Science, and the American Alpine Club.

Lee Smith remained as chief of the Organic Chemistry Division at Minnesota until 1958, when he resigned to prepare for an orderly transition to accompany his retirement in 1960. He was succeeded in 1958 by William E. Parham, who began his career at Minnesota in 1946 after receiving his doctorate with Reynold C. Fuson at the University of Illinois.

Smith made significant contributions to the development and growth of the University of Minnesota. He gave valuable counsel on many important decisions of university policy, as well as in recruiting and making appointments to fill key vacancies. In describing Smith's role in committee work, a colleague stated, "He has a mind like a knife, dissecting away all unimportant material to reach the core of the discussion." When the Institute of Technology was first formed at Minnesota, Lee Smith was asked to serve as administrator of the School of Chemistry. After serving one term, he decided that he didn't care for this level of administration and returned to his post as chief of the Organic Chemistry Division.

In June 1972, at a special ceremony, the main building of the Chemistry Department at the University of Minnesota, in which Smith had spent his career, was named L. I. Smith Hall.

PERSONAL QUALITIES

The above remarks constitute an overall summary of L. I. Smith's life, accomplishments, and activities. Let us turn now to what it was that made Smith such a special person. A careful reading of his published work discloses much more than just the imagination to conceive these investigations and the accumulated knowledge and skill to carry them through to a satisfactory conclusion. The quality that sets them apart is the passionate insistence on excellence of method and scrupulous attention to accuracy in every detail. Lee Irvin Smith didn't publish until he was satisfied that the work, which he was committing to record, was done not just adequately but elegantly. When Smith published, the work was as he said it was and could be reproduced in any laboratory of organic chemistry.

This same passionate devotion to excellence characterized his teaching, both in and out of the classroom. His reputation in the classroom was unrivaled, because he had a priceless gift for communicating, even to a large class, not only his encyclopedic knowledge of organic chemistry but also, and more importantly, the fascination it held for him. He prepared for every classroom lecture with the same driving thrust toward perfection that characterized all of his activities. Despite his many years as a teacher, Lee Smith confided once that he never faced a class before beginning a lecture without suffering stage fright. He gave his students only the best he had to offer, and he demanded that they in turn give him their best. It was a rare student who failed to respond.

As a supervisor of doctoral thesis research and of postdoctoral research, Lee Smith brought to the task the same qualities that made him outstanding as a scientist, a teacher, and as someone decidedly mortal but nevertheless superior. He gave his students plenty of room to use their own imagination and ingenuity in the solution of a chemical problem. Smith was very concerned that the writings of his students were of high quality, using proper grammar and rhetoric. He insisted on diligence in pursuit of the objective, on scientific fidelity of the highest order, and on a certain style and elegance. Lee Smith knew the capabilities of the young people who worked with him, and he would tolerate nothing but the best performance of which each was capable. Smith would register his displeasure with a slovenly performance in terms that left the recipient with his hair singed and his skin reddened. No one wanted to experience a second such reprimand.

Lee Smith recognized the vital and delicate relationship between the training of graduate students and the scientific accomplishments such training produces. He never lost sight of the fact that he was a professor and his primary obligation was to foster the process of converting graduate stu dents and postdoctoral fellows into thoughtful, knowledgeable, independent investigators. To him research was important, indeed essential, but it was nonetheless a by-product of the training process. Smith understood that, if a professor does a proper job of training a graduate student, good research will be produced. However, the converse is not necessarily true.

It should also be noted that Smith followed the careers of his former students after their graduation and was always ready with help and support if they were faced with a difficult decision or a crisis.

For those of us who were privileged to be students at Minnesota at the time that its Organic Chemistry Division was emerging from relative obscurity to become a leading department, we recognized what was occurring and that Lee Smith was the central figure in this development. He knew what a fine department should be and how to build it. He persuaded and cajoled, and he led and drove colleagues, students, and the administration until the amorphous mass crystallized into an outstanding center of organic chemistry. Smith was never satisfied with the state of the Organic Chemistry Division, the department, or the university, for the pursuit of excellence for him was endless and there was always something more to be done to achieve the desired objective.

It is true that Lee Smith demanded from students a disciplined and devoted effort in doing chemistry. Those who were unwilling to measure up to his standards certainly found him forbidding. However, Smith understood that anyone devoting himself strongly to chemistry must also have time for fun and enjoyment, if he is to have the balanced judgment necessary for creating outstanding research accomplishments. Thus, he had a fine sense of humor and knew instinctively the proper time for a story or a joke to relieve tension.

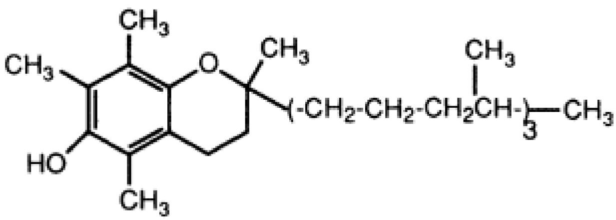
During Smith's tenure at Minnesota, all of the graduate students (usually about forty) who were doing experimental research in organic chemistry worked in one huge laboratory, known as the 490 Lab. Most of the students were teaching assistants who had various duties and classes during the day. It was at night that the laboratory was filled with activity. An obvious disadvantage of having so many students in one room was that a serious accident would put a large number of people at risk. However, there were also real advantages. A beginning student seeking knowledge about a reaction, a procedure, or a technique had a large pool of experienced investigators immediately available to him.

The morale and companionship in the 490 Lab were very good. It was a sociable place where all sorts of topics could be discussed in an intelligent and educated manner. Although Smith did not normally come to the department in the evenings, except on seminar nights, most of the organic chemistry staff would be in their offices in the evenings and so students could consult with them about ongoing experiments in the laboratory. Lee Smith enjoyed visiting the 490 Lab, though, and his influence was important in setting the tone of the laboratory. Each spring, in a tradition started by C. F. Koelsch, the organic staff would invite all the members of the 490 Lab to a party at a downtown hotel. These were very special affairs and included skits put together by the students after much preparation.

Lee Smith's hobbies included mountain climbing, horticulture (particularly growing orchids), photography, and music. He was a chain smoker of cigarettes, which was made conspicuous by his long cigarette holder. In his later years he developed chronic emphysema and died from its complications.

HIGHLIGHTS OF SMITH'S RESEARCH

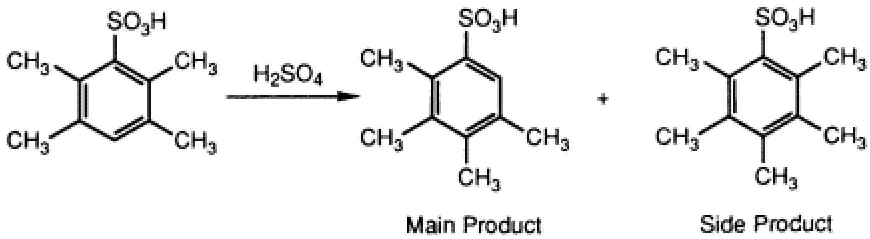
Undoubtedly, the outstanding research accomplishment for which Smith is best known is his work on tocopherol derivatives and his synthesis of vitamin E, whose structure is shown below. Altogether Smith published forty-seven papers on his experimental work related to vitamin E and the tocopherols. This research played an important role in his election to membership in the National Academy of Sciences in 1944.



VITAMIN E

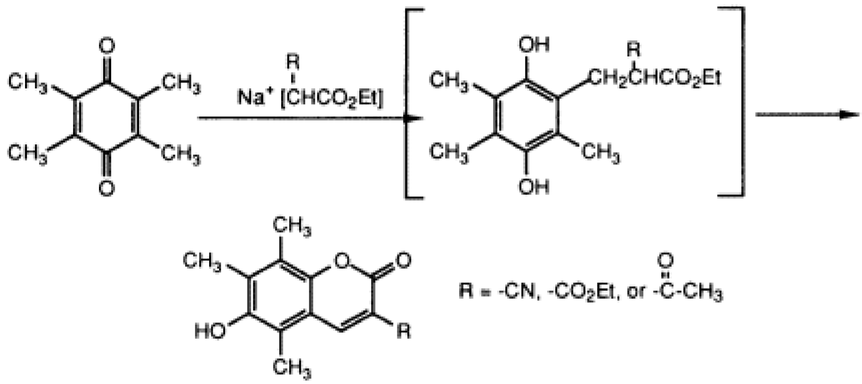
Related, but prior, to his work on the tocopherols, Smith was very interested in polyalkyl and polysubstituted benzenes in general. This led to an intensive investigation of the Jacobsen rearrangement, which is summarized in a series of nine publications. As illustrated in the example on the following page, the Jacobsen rearrangement occurs when polyalkyl- or polyhalobenzenesulfonic acid is allowed to stand in sulfuric acid and involves the intramolecular or intermolecular migration of one or more alkyl or halo substituents.

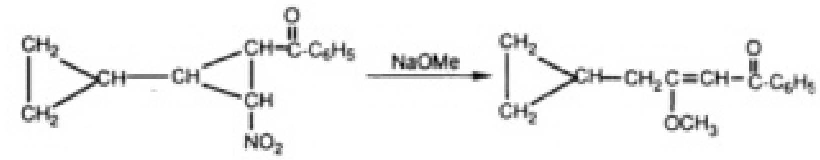
Similarly, in work related to the tocopherols and to polyalkylated hydroquinones, Smith studied in extensive detail both polyalkylated benzenes and the reaction of



benzoquinone derivatives and metal enolates. Ultimately, this led to his publishing a series of thirty-five papers on polyalkylated benzenes and twenty-five papers on the reaction of benzoquinones with metal enolates. An example of his studies on the reaction of benzoquinones with metal enolates is shown below.

During the course of his doctoral dissertation, Lee Smith became interested in cyclopropanes. In his program at Minnesota he explored this interest further by synthesizing cyclopropanes containing functional groups and studying the chemical behavior of such derivatives. His accomplishments in this area are recorded in a group of twenty-two





publications. As an example of Smith's work in this area, he synthesized the nitro(bicyclopropyl) ketone shown above, the first bicyclopropyl ketone ever to be made. Both possible racemates were formed, and each was isolated in a pure crystalline state. Each of these racemates, when treated with sodium methoxide, gave the same enolic ether, shown above, as the only product.

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