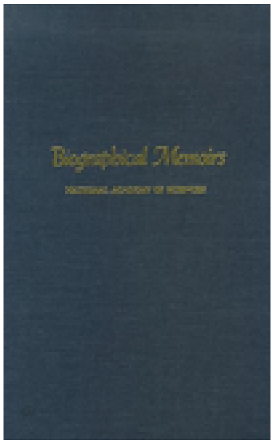

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Preface

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.

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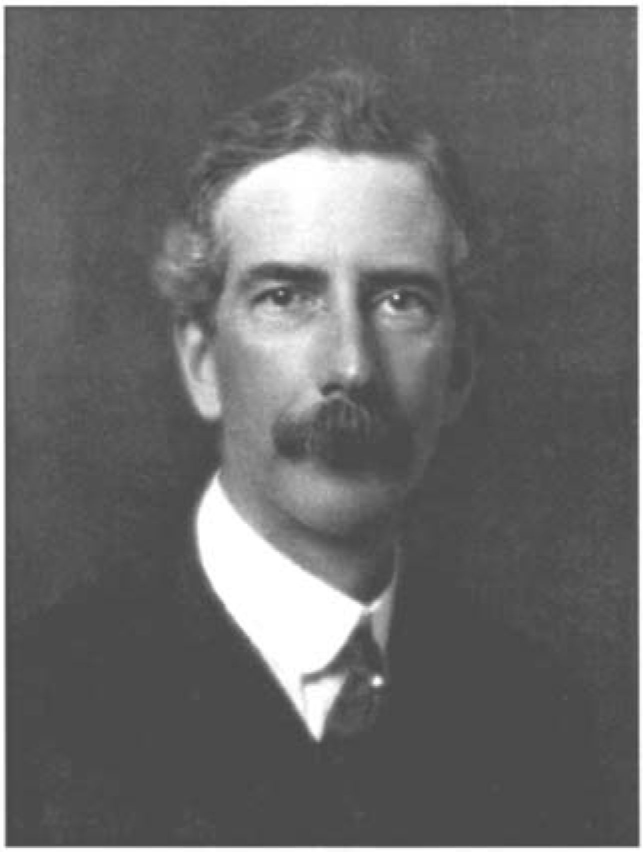
Home Secretary

Biographical Memoirs

VOLUME 73



Photograph by Bachrach

Charles Greeley Abbot

May 31, 1872–December 17, 1973

BY DAVID H. DEVORKIN

CHARLES GREELEY ABBOT was the second director of the Smithsonian Astrophysical Observatory and the fifth Secretary of the Smithsonian Institution. He was the second and last person to hold both posts simultaneously and is remembered today for his skill as an instrumentalist and his unshakable belief that the Sun is a variable star and that its variations had a measurable effect on the Earth's weather. He was elected to the National Academy of Sciences in 1915 and served as its home secretary from 1919 to 1923 under President Charles Doolittle Walcott, who was Abbot's predecessor as Smithsonian Secretary.

Abbot was born in May 1872 in Wilton, New Hampshire, the son and grandson of farmers. The youngest of four children of Harris and Caroline Ann (Greeley) Abbot, Charles Greeley attended public schools, but finished at Phillips Andover Academy. He then attended MIT, where he graduated in 1894 with a thesis in chemical physics. He expected to teach, but remained at MIT, studying osmotic pressure and earning an M.Sc. in 1895. Skilled at laboratory work, he came to the attention of Samuel Pierpont Langley, who was looking for an assistant at the Smithsonian's Astrophysical Observatory (APO). Abbot soon was hired, though he lacked any experience in astronomy when he arrived in Washington in June 1895. Langley, however, was not a traditional astronomer and Abbot was just the type of assistant he wanted to aid his mapping of the infrared spectrum of the Sun, adapting bolometers for photographic recording and determining dispersion standards for rocksalt and fluorite prisms to measure fundamental wavelengths in the infrared region of the solar spectrum.

Under Langley, Abbot flourished as a creative designer and builder of delicate devices for measuring solar radiation. As Langley focussed more and more on his aeronautical experiments, Abbot, working with F. E. Fowle, became responsible for maintaining the observatory's solar program, including an expedition to observe the 1900 solar eclipse in Wadesboro, N.C., where Abbot applied a vastly improved bolometer to take readings of the Sun's inner corona. He was also a leading member of the American eclipse expedition to Sumatra in 1901. He proved to be a reliable observer and impressed many astronomers who encountered him at these places.

Abbot was an affable fellow, deferential to his superiors while making significant contributions to the mission of Langley's institution. That mission, very much representative of the times, was to demonstrate the utility of government-supported science. Years later, in his rambling autobiographical essay *Adventures in the World of Science,* Abbot recalled Langley's words explaining why measurement of the heat of the Sun was important:

> If the observation of the amount of heat the sun sends the earth is among the most important and difficult in astronomical physics, it may also be termed the fundamental problem of meteorology, nearly all whose phenomena would become predictable, if we knew both the original quantity and kind of this heat.[1]

Certainly the idea that solar radiation governed the Earth's fate as an abode for life was not original with Langley. The key to Langley's mission, however, was to make the amount and character of that radiation "predictable" and thereby useful for planning strategies for agricultural management and control. Langley believed that solar radiation varied in a cyclic manner. As Smithsonian Secretary, however, Langley had other interests, but what may have been promotional rhetoric for him became a permanent and passionate conviction for his able, dutiful assistant.

Within a few weeks of Langley's death in February 1906, Abbot was made acting director of the APO, becoming its second director in 1907 under Secretary Charles Walcott. Astrophysical operations continued unabated, with Walcott providing advice and support that allowed Abbot to extend Langley's mission in two ways: first, by developing refined techniques for the specific determination of the solar constant; and second, by applying these techniques in a standardized manner to build a synoptic monitoring program that would search for solar variations. As under Langley, Abbot found Walcott wholly attuned to the progressive notion of useful science. Before he became the Smithsonian's fourth Secretary, Walcott was head of the U.S. Geological Survey, and campaigned for practical research in publicly supported agencies.

When Abbot became APO director in 1907, American astronomy's most significant strengths and potential lay in vast cataloguing projects centered at a few major observatories, including Harvard, Yerkes, and Lick. American astronomy was in the throes of organizing itself as a profession, and its standards and modes of conduct were in flux. Celestial mechanics and mathematical astronomy were still the strengths of the discipline, but now the photographic plate and the spectroscope were available for assessing the physical nature of the Sun and stars. Langley had practiced the new astronomy. Primarily an engineer, he had created an astrophysical program at the Allegheny Observatory in Pittsburgh, defining it by the use of new types of instruments, the bolometer and spectrobolometer, and brought both these instruments and their practice to the Smithsonian to establish the first and only federally funded astrophysical observatory in the United States.

When Abbot retired as APO director and as Smithsonian Secretary in 1944, setting a precedent as the first Smithsonian Secretary not to die in office, most but not all of the great cataloguing projects were gone and the discipline was undergoing profound change. Problem-oriented research, informed by modern physical theory, dominated the discipline. Yet the Smithsonian's Astrophysical Observatory pursued its single mission all along, elaborating on its purpose not by a broadening of its astronomical base but by refining its instrumentation and technique, searching for evidence that Earth's meteorology and biology were intimately connected to variations in the Sun's output of energy. Although he eschewed physical theory, Abbot was thoroughly modern in his problem-oriented approach to research. Thus, his failure to broaden the astrophysical scope of the APO during his long tenure has to be appreciated as due to a complex set of factors centered on his singular sense of mission, which transcended disciplinary lines between astronomy, geophysics, meteorology, and biology.

The amount and character of the Sun's radiation are basic quantities for a wide range of scientific and environmental concerns. Determining these quantities in practice, however, was far from simple. Astronomers long knew that the absorption of solar energy by the Earth's atmosphere was both selective and general. Langley's method of determining the solar constant was to take observations of the Sun as it rose in the sky, noting its increase in radiating power and then extrapolating to the top of the atmosphere. Given the vagaries of the atmosphere and the limitations of technology, the value of the solar constant could vary as much as 50%. Langley established the value 3.00 cal/ cm^2/ min outside the atmosphere as the Smithsonian standard and held to it tenaciously to the end of his life. But others who made different assumptions about atmospheric absorption coefficients or other variables came up with values between 1.5 and 4.0.

After some seven years working for Langley, Abbot knew that the Smithsonian value for the solar constant was too high, but he carefully avoided the issue until he was in charge. Then, he quickly announced results from observations at Mount Wilson, California, that reduced the solar constant first to 2.1 and then to 1.93, largely through the introduction of improved, standardized methods and better thermal isolation for his pyrheliometers and bolometers.[2] Abbot paid close attention to detail.

Abbot's revision, however, drew criticism from various quarters, mainly from a disgruntled and generally combative Langley protege, but also from two Europeans who argued that the way in which he accounted for atmospheric absorption was incorrect. Abbot met this criticism by returning to the highest mountain in the Rockies, following Langley's lead in 1881. Abbot cooperated with W. W. Campbell at the Lick Observatory and with the Sierra Club to build a sturdy field station on the summit of Mount Whitney. Abbot used the site sporadically in 1909 and 1910 to measure the solar constant and accompanied the Lick astronomers to study the spectrum of Mars.

Still harried by critics, however, Abbot turned to balloonsondes to reach greater heights. Collaborating with the Weather Bureau and Signal Corps, with Anders Knut Angstrom, who had been in residence for several years, and with the help of his chief assistant Loyal B. Aldrich, Abbot flew special pyrheliometers on balloons. He created a new type of robotic pyrheliometer out of parts from standard Weather Bureau meteorgraphs that was fully automatic and self-recording. Automatic techniques for meteorological observations from balloons were well developed by then. But Abbot was the first to use such automata in America for astronomical measurements.

Abbot's instruments, built by Andrew Kramer, were marvels of sophistication and planning. They were flown by Aldrich from the California coast in 1913 and 1914, and some of the balloonsondes reached over 25 kilometers; at least one of them returned clear evidence for thermometric and barometric variations that confirmed his terrestrial extrapolations and allowed him to determine the value of the solar constant at the top of the Earth's atmosphere. This technical feat, requiring the cooperation of the Weather Bureau and the Signal Corps, quieted criticism of the Smithsonian value for the solar constant. It helped to affirm Abbot's reputation and established the modern range for the solar constant.

Even before he assumed the directorship of the APO, Abbot was among the astronomical elite. In a 1903 census by the AAAS he was listed among the top thirty astronomers by his peers. Langley was among the first rank, and both scored even higher among physicists who were polled. Abbot won the prestigious Draper Gold Medal of the National Academy of Sciences in 1910 and the Rumford Medal of the American Academy of Arts and Sciences in 1916.

With his solar-constant critics vanquished, Abbot focussed more on Langley's ultimate goal: to search for evidence of variations in the solar constant and to show that these cycles influenced cycles in weather and climate. He believed that such evidence was already at hand from the findings of H. H. Clayton, the chief forecaster of Argentina and a colleague of A. Lawrence Rotch of the Blue Hill Meteorological Observatory outside Boston. Clayton had excitedly written Abbot in 1912 with what he believed was proof that changes in the world's weather correlated with changes in the solar constant that he had gleaned from published Smithsonian data. Clayton soon became one of Abbot's closest allies, and over the next three decades, confirming these clues would define Abbot's mission.

To confirm Clayton's findings Abbot had to account for local variations due to seasonal weather conditions, and so he set about searching for widely spaced observing sites where air transparency was constant. High mountains in desert regions spread over the accessible parts of the Earth became his target, and again, following a well-worn Smithsonian tradition, Abbot built these stations in the manner of field expeditions.

His first major permanent station was at George Ellery Hale's Mount Wilson Solar Observatory, which Abbot started visiting as it was being built in 1903 and 1904. Hale tried once to hire Abbot away from Langley, but soon accepted the Smithsonian man as an ally in Washington, where his own patron the Carnegie Institution of Washington was based. By 1915 Abbot had built a permanent field station on the south side of a spur of the mountain that emulated Hale's own tower telescopes. Abbot visited the station often and built a large solar cooker which his wife Lillian used to bake and roast to feed the local staff. The Smithsonian was a welcome neighbor on that mile-high mountain top.

With Walcott's backing, which included securing endorsements from astronomers around the world, Abbot soon gained additional funds to search for other sites to complement Mount Wilson. In 1911 and 1912 he and Angstrom set up a temporary station at Bassour in Algeria, where they observed the Sun and weather patterns using a wide array of sensors ranging from small portable pyrheliometers to Abbot's huge spectrobolometer, which sifted the Sun's radiation through a slowly rotating prism that focussed different parts of its spectrum onto a tiny platinum wire. The wire's electrical resistance was changed by the Sun's light heating it, and this caused a flow of current in a delicate galvanometer, which moved a tiny mirror supported by a quartz fiber. The moving mirror sent a beam of sunlight onto a strip of moving photographic film, which recorded the varying energy of the Sun as a function of wavelength.

Throughout World War I Abbot maintained the Mount Wilson station and hoped to establish a southern hemisphere site too. He also attended to various wartime activities. He patented a new way to rifle a bullet to improve accuracy and with Aldrich developed a portable searchlight, making great friends with General Electric in the process. Abbot also promoted Robert Goddard and pushed Army Ordnance to award the Smithsonian a lucrative contract for Goddard's continued work on solid rockets. But after the Armistice, Ordnance canceled the contract, to Abbot's great annoyance.

At the end of the war, Abbot reactivated his site search, looking for a place that would have clear weather during the poorer winter season in California. The new "place" had to be clear (weather) when his California site experienced cloudy weather. He had hoped to gain the cooperation of the Australian government, but eventually Wolcott approved the use of Hodgkins Fund income to build a station in South America, where the United States was building a strong mining base. The Guggenheims operated a huge copper mine at Chuquicamata, and were happy to host a U.S. government presence. The mining company provided a residence just south of Calama, on the eastern edge of the Atacama nitrate desert in the northern Chilean Andes. Manned by Alfred Moore and an assistant, it was a most desolate place, but it was not far from the Guggenheim operations and a small mining town where many Americans lived.

By 1920 expenses were rising at both the Mt. Wilson and Calama stations and Abbot, through Walcott, secured a modest increase in federal appropriation for the APO. More important were private gifts from John A. Roebling, heir to the designers and builders of large suspension bridges, including the Brooklyn Bridge. Roebling was a major Smithsonian benefactor; on the death of his father Washington Augustus Roebling, he presented his father's enormous mineral and gem collection to the Smithsonian, along with an endowment to manage it. Roebling was also sympathetic to Abbot's mission and its hope of practical application, not only for weather forecasting but for the use of solar energy.

As Abbot campaigned to keep his stations running, he found that the data coming from them were influenced by local weather. Mt. Wilson suffered from maritime air and local dust, and Calama was compromised by the dust from the huge open-pit mines. With Roebling's support, Abbot shifted the Mt. Wilson station to the Harqua Hala Mountains in southwestern Arizona, which C. F. Marvin, chief of the Weather Bureau, believed was a better site; it was clearer, had less dust, and was dryer than Mount Wilson. The Calama station was also closed and moved to 9,500-foot Mount Montezuma, about 12 miles from its original site. Harqua Hala, however, soon proved to suffer from the same inconsistent weather patterns that had plagued Calama; so again Abbot managed to obtain Roebling support to transfer Harqua Hala to a higher and more stable site on Table

Mountain in California, above the Mojave Desert at about 7,500 feet elevation and some 30 miles northeast of Mount Wilson. Among many logistical problems Abbot faced, seeking out the best sites was to keep members of his field staff willing to sacrifice their lives in these terribly isolated spots.[3]

As he sought the best sites, Abbot also constantly improved his equipment. One major problem was thermal stabilization for his spectrobolometers, which he solved by mounting them inside tunnels at Mount Montezuma and Table Mountain, what he later heralded as the "Smithsonian observing tunnel," a new form of observatory. The Montezuma station remained active for several decades. The observers were sensitive to local custom, and the site was used during World War II as a field station to study the effect of intense radiation on fabrics, one among many wartime studies Abbot's APO fostered.

By the mid-1920s Abbot believed he had confirmed Clayton's findings and began to report on solid connections between solar cycles and weather patterns, offering his results as proof that, with continued study, true long-range weather prediction was at hand. C. F. Marvin, however, worried about Abbot's claims and set his staff of statisticians to a re-analysis of the past two decades of Smithsonian data. They soon found that the variations Abbot had found in the solar constant were just as easily accounted for by the "diminishing amplitude of scatter as stations and methods of observations were improved." In the spring of 1925 Marvin warned Abbot that "*If* the 20 years of work of the Astrophysical Observatory on the solar constant shows anything at all it shows the variations of the sun are of the same or smaller order of magnitude as the unavoidable errors of observation."[4]

Marvin suggested that Abbot needed to make more observations, not only from remote stations, but through further improvements in technique, mainly doubling up his pyrheliometers and pyranometers to account for instrumental variation.[5] Abbot reacted to Marvin's critique with more bluster than scientific argument. He appealed to cooperation and loyalty between kindred government bureaus and tried to convince Marvin not to reveal his conclusions, assuring him that better data were now at hand. Marvin, however, was not convinced and, since Abbot was unable to face his statistics square on, did deliver his conclusions at a meeting of meteorologists in Washington. Abbot again defended his position with bluster, but privately accepted Marvin's suggestion to build redundant devices to search for instrumental error. This was only the first of many clashes between Abbot and traditional meteorologists. But Abbot knew how to play on the hopes of the day and was even able to keep Marvin as an ally, capitalizing on his suggestion that more observations were needed. In the 1920s, cycles were a fascination to students of nature. The Carnegie Institution hosted "cycles conferences" looking for correlations in all natural phenomena; there were many voices in support not so much of Abbot's conclusions but of his continued work, holding out the hope that his conclusions would be vindicated.

Turning any criticism into a challenge for support of a noble cause, Abbot found the means to improve his instruments and to establish a third outstation, since he knew that three independent stations were the minimum number he required for a definitive synoptic monitoring network. In 1925 he had little trouble convincing the Grosvenor family that the National Geographic Society should grant $55,000 to establish a third station somewhere in the eastern hemisphere. Abbot and the National Geographic Society chose Mount Brukharos in Southwest Africa. The expedition started in April 1926 under W. H. Hoover and Frederick Greeley, who had been at Harqua Hala and at Table Mountain for a few years. The National Geographic made much of the expedition, and back home Abbot mounted exhibits to keep people informed of the Smithsonian's far-flung research expeditions. They built another Smithsonian observing tunnel to thermally stabilize the most delicate instruments, and Abbot displayed a scale model of it in the Smithsonian castle. Abbot knew how to keep Smithsonian science in the news.

By 1930, however, wind-blown dust at Brukharos caused Abbot to search for a better site. Though by now he had succeeded Walcott as fifth Secretary of the Smithsonian, Abbot's focus remained on the Astrophysical Observatory and its programs. With support from the Research Corporation of New York he established a new Division of Radiation and Organisms in 1929. With Roebling and National Geographic Society support he closed Brukharos and mounted an elaborate expedition to build a new station above the monastery at the base of Mount St. Katherine on the Sinai Peninsula. By the summer of 1931 they had settled on a site 10 miles from the monastery on Zebil Gebir, a spur of the mountain in sight of Mount Sinai.

By 1936, however, the St. Katherine station had serious logistical and supply problems. Abbot was still able to secure gifts, but funds were harder to come by. Abbot decided to close St. Katherine in December 1937 in favor of a continental spot that would cover the months December through February, when his other two stations were usually clouded out. He eventually selected Burro Mountain in New Mexico, and sent the Gebir instruments and staff to what was the Tyrone station, where another Smithsonian tunnel was excavated into the mountain.

Abbot's construction of field stations demonstrates his tenacity and his considerable success in not only maintaining, but expanding, his focussed program to continuously monitor the Sun's radiation during a time when the Smithsonian itself was undergoing retrenchment. Up to 1930 the APO staff grew steadily, and maintained itself throughout the 1930s. All three stations plus the home station on the Mall in Washington, D.C., continued to operate without major breaks throughout the Depression. Abbot maintained a trustworthy and highly capable staff able to build instruments, use them, and reduce their data according to the systematic procedures Abbot created. Over the years the staff developed five distinct types of pyrheliometers, including the silver disk, water flow, water stir, improved Angstrom, and the automatic balloon device, all of which Kramer built. Kramer and his assistants also built Abbot's devices for reducing computational labor, such as a special ganged slide-rule extrapolator for determining atmospheric transmission factors quickly and efficiently. Abbot introduced time-saving methods and new computational devices whenever he could. Eventually, with a specially built differencing engine he called a "periodometer," Abbot unraveled what he believed was a complex nested set of some twenty-three cyclic variations in the Sun's energy output, all acting simultaneously.

Kramer also built many of Abbot's solar heaters and cookers, which Abbot used in lectures and in countless demonstrations to keep the importance of knowing about the Sun and its radiation before the public and his patrons. His solar heaters were reminders of the power of the Sun.

By the mid-1930s, while continuing to make claims before patrons and public that the means for weather prediction were at hand, Abbot realized that he needed more support to prove his contentions. His staff had made a detailed comparative analysis of the Montezuma, Table Mountain, and St. Katherine observations and felt that three stations were not enough. Willis Gregg, the new chief of the Weather Bureau, and a new blue ribbon panel Gregg had assembled, made up of Abbot's old friends, concluded that Abbot's findings were real and warranted not only continued support but a substantial increase. The Smithsonian had been in the business so long, Robert A. Millikan, K. T. Compton, and Isaiah Bowman argued, it would be a pity to stop. It was, after all, where solar constant studies had their longest history and their greatest advocate. Abbot's plan for a vastly expanded program, from three stations to ten, and synoptic balloonsonde programs would cost some $300,000 per year and passed the Senate with President Franklin D. Roosevelt's endorsement; but it lost in conference in 1936. Abbot therefore had to retrench for the first time. He closed the expensive St. Katherine station.

In 1918 Abbot was designated Assistant Secretary of the Smithsonian under Walcott with responsibilities for the Smithsonian library and the venerable International Exchange Service. The latter was a world-wide clearinghouse for the diffusion of scientific literature, which had been set in motion by Joseph Henry and fostered by Congress to keep open lines of communication between governments. Abbot succeeded Walcott in 1928 and guided the Smithsonian through the Great Depression and World War II. Despite Abbot's extreme focus on the APO, by the late 1930s the Smithsonian had weathered the Depression intact but not undamaged. Walcott's campaign for a $10 million dollar endowment was too short-lived to be effective and after his death was not supported by the Smithsonian Regents. The number of gifts to the general endowment did increase somewhat, but the amount and number rapidly dwindled in the 1930s, even as smaller gifts earmarked for the APO increased. When one looks at the fate of the Smithsonian overall during Abbot's tenure, one sees opportunities missed and paths not taken, which confirm that his interests were narrowly defined. His greatest missed opportunity was not securing Andrew Mellon's National Gallery of Art as part of the Smithsonian Institution. He also left the workings of the National Museum largely to his Assistant Secretary, Alexander Wetmore, who succeeded him as Secretary in 1944.

During the Depression, Abbot and his staff took advantage of a variety of federal job relief programs to support the Smithsonian. The National Zoological Park was significantly improved and completed through a $1.3 million dollar WPA program, largely as a result of effective campaigning by its director, William Mann. Most important, Abbot did whatever he could to insure that the Smithsonian continued to generate knowledge and diffuse the knowledge it generated. He parlayed the legacy of the International Exchange Service to make the Smithsonian's *Annual Reports* thicker, swollen in part by his own reprinted papers. But he also cannily included reprints from authors he knew would be sought out and read, and who could, in turn, aid the Smithsonian itself. Abbot's popular writings were florid and numerous. Beyond his many books, he also helped to create, under Walcott, the *Smithsonian Scientific Series* as a fund-raising venture, and wrote three of the dozen books in the series starting in 1929. Written by Smithsonian researchers largely about Smithsonian-related work and published in various editions by a New York publisher, the books averaged $25,000 in revenue a year for the next two decades after the Regents forced the publisher to relinquish a larger piece of the pie.[6] Abbot also fostered a radio program with WPA support, "The World is Yours," hiring actors to dramatize the world of science, bringing the Smithsonian into American homes during the 1930s.

During World War II, Abbot directed Smithsonian resources to the war effort, forming the Smithsonian War Committee to disseminate the Smithsonian's technical knowledge and expertise in fields such as aviation, entomology, geography, desert and Arctic conditions, and anthropology. The Smithsonian created a series of twenty-one pamphlets describing the lands where the war was being fought. Called "War Background Studies," they were published in the hundreds of thousands. The Smithsonian also joined the National Research Council, the State Department, and other governmental and private organizations to form the Ethnographic Board and the Institute for Social Anthropology, both housed at the Smithsonian, to use the social sciences for national goals.

Overall, Abbot was very successful at keeping the work of the APO and the Smithsonian before the public, through his writings, lecturing, and exhibits, as well as through his role on the Board of Trustees of Science Service and of the Research Corporation of New York, through endorsements from scientists and political contacts, and continuing support from patrons like Roebling. In the late 1940s as Abbot reached retirement and continued to work as a research associate of the institution, he never stopped searching for earthly correlations with his purported solar constant variations, corresponding widely with meteorologists, crop specialists, and even medical researchers. To the end of his long life, Abbot continued to publish revisions of his analyses of decades of solar constant data and always defended his belief that solar constant variations existed and could predict terrestrial weather changes. He met his critics head on whenever and wherever they appeared. One critic was close at hand, but spoke only in private. When Aldrich succeeded Abbot, he confided to an astronomical friend who was a member of the Smithsonian's "future policies committee" that, although he had the greatest affection and respect for his old boss, he knew that his correlations of solar variations contained systematic errors which Abbot refused to admit.[7] Aldrich carried on for several years as APO director until the Smithsonian closed its Washington observatory in favor of an alliance with Harvard.[8] Aldrich chose not to emphasize weather prediction, but turned to another facet of Abbot's mission—the practical utilization of solar energy—to foster science in service to mankind.

Abbot's ability to develop and maintain solar monitoring stations around the world for over forty years marks his tenacity and conviction for what was his mission in life. It demonstrates too his ability to secure support through professional relations, which he fostered and enjoyed with other like-minded institutions in scientific Washington, such as with the Weather Bureau, the National Geographic Society, the National Academy of Sciences, and the Carnegie Institution. Many forces were promoting Abbot's work. The "cycles conferences" already mentioned put Abbot's program at center stage. In the early 1930s the need for long-range weather forecasting was also a major concern of the Secretary of the Navy, who asked the National Academy of Sciences to look into its feasibility. John C. Merriam of the Carnegie Institution of Washington led a committee of ten, which included Abbot, Bowman, Compton, and Marvin. Abbot remained active within this small but influential circle of scientists and called upon them more than once to endorse his programs. The focus of Abbot's research during his long tenure required continual endorsement, and he received it at critical times from his colleagues in astrophysics as well as in geography, meteorology, and physics. His success at gaining patronage explains why the program was so long-lived and why the APO did not change its mission during the Abbot years, even though more than one influential astronomer tried to get Abbot to apply his techniques of spectrobolometry to the stars. Abbot did study the energy spectra of the stars sporadically in the 1920s, devising a radiometer using housefly wings that measured the distribution of energy in stellar spectra from the 60-inch reflector at Mount Wilson. It was an impressive technical feat, but for Abbot it was only a diversion.

The variations Abbot claimed to exist in the solar constant, ranging from 3% to 10%, were certainly due to varying weather conditions and flawed analysis, but his life-long mission helped to keep the problem alive. A re-analysis in the 1970s of four decades of Smithsonian solar constant data did show evidence for minute variations, but it was not until satellite evidence became available that tiny variations were confirmed to exist, due to sunspot and faculae variations.

A music lover, Abbot sang and played the cello. He was a dedicated member of the First Congregational Church of Washington and served as deacon for years. Abbot's marriage to Lillian Elvira Moore on October 13, 1897, ended with her death on June 1, 1944, a month before his retirement. He married Virginia Andes Johnston in 1954. Abbot left no issue.

MATERIAL FOR THIS MEMOIR came from letters in the Charles Greeley Abbot papers, Smithsonian Institution Archives, which contains some 176 cubic feet of Astrophysical Observatory correspondence, data books and charts, photographs, manuscripts, speeches, and budgets. Also important are the Records of the Office of the Secretary, 1925–1949, which contains another 96 cubic feet documenting Abbot's role as Secretary. There are also oral history interviews at the Smithsonian Institution Archives and an Abbot biographical file in the archives of the National Academy of Sciences. Secondary sources were:

Abbot, C. G. 1958. *Adventures in the World of Science*. Washington, D.C.: Public Affairs Press.

Burggraaf, P. 1996. *Harqua Hala Letters: The Story of Arizona's Forgotten Smithsonian Observatory*, Monogr. No. 9. Phoenix: Arizona State Office of the Bureau of Land Management Cultural Resource Series.

DeVorkin, D. H. 1990. Defending a dream: The Abbot years, *J. Hist Astron.* 21:121–36.

Doel, R. 1990. Redefining a mission: The Smithsonian Astrophysical Observatory on the move. *J. Hist. Astron.* 21:137–53.

Hufbauer, K. 1991. *Exploring the Sun: Solar Science Since Galileo*. Baltimore: Johns Hopkins.

Jones, B. Z. 1965. *Lighthouse of the Skies. The Smithsonian Astrophysical Observatory: Background and History, 1846–1955*. Washington, D.C.: Smithsonian.

Oehser, P. H. 1970. *The Smithsonian Institution*. New York: Praeger.

Hellman, G. T. 1967. *The Smithsonian: Octopus on the Mall*. Philadelphia: J. P. Lippencott.

NOTES

1. S. P. Langley. Report of the Mount Whitney Expedition. Quoted in Abbot, 1958, p. 17, above.

2. The pyrheliometer is a shielded thermometer that is exposed to direct sunlight through a carefully baffled tube. Bolometers are general purpose radiation detectors capable of sensing a broad wavelength range of energy.

3. See Burggraaf above.

4. Marvin to Abbot, April 21, 1925, Abbot papers, pp. 7–8. This agrees with contemporary assessments. See Hufbauer, chapter 9, above.

5. Abbot's pyranometers were designed to record the overall brightness of the daytime sky but were not exposed to direct sunlight. They were used in conjunction with pyrheliometers to determine the incident radiation from the Sun subtracted for sky brightness.

6. See Oehser, p. 176 and Hellman, p. 206 above.

7. Aldrich to Adams, circa January 30, 1946, Abbot papers, Smithsonian Institution.

8. See Doel above.

Selected Bibliography

The majority of Abbot's technical publications appeared in *Annals, Smithsonian Astrophysical Observatory*, volumes 1–7 from 1900 to 1954; and in *Smithsonian Miscellaneous Collections*, volumes 65–153 from 1915 through 1969. The more notable among them, along with his popular monographs, include:

1904 *The 1900 Solar Eclipse Expedition of the Astrophysical Observatory of the Smithsonian Institution*. Washington, D.C.: U. S. Government Printing Office.

1911 *The Sun*. New York: Appleton.

1915 With F. E. Fowle and L. B. Aldrich. New evidence on the intensity of solar radiation outside the atmosphere. *Smithson. Misc. Collect.* 65(4).

1916 With F. E. Fowle and L. B. Aldrich. On the distribution of radiation over the Sun's disk and new evidences of the solar variability. *Smithson. Misc. Collect.* 66(2).

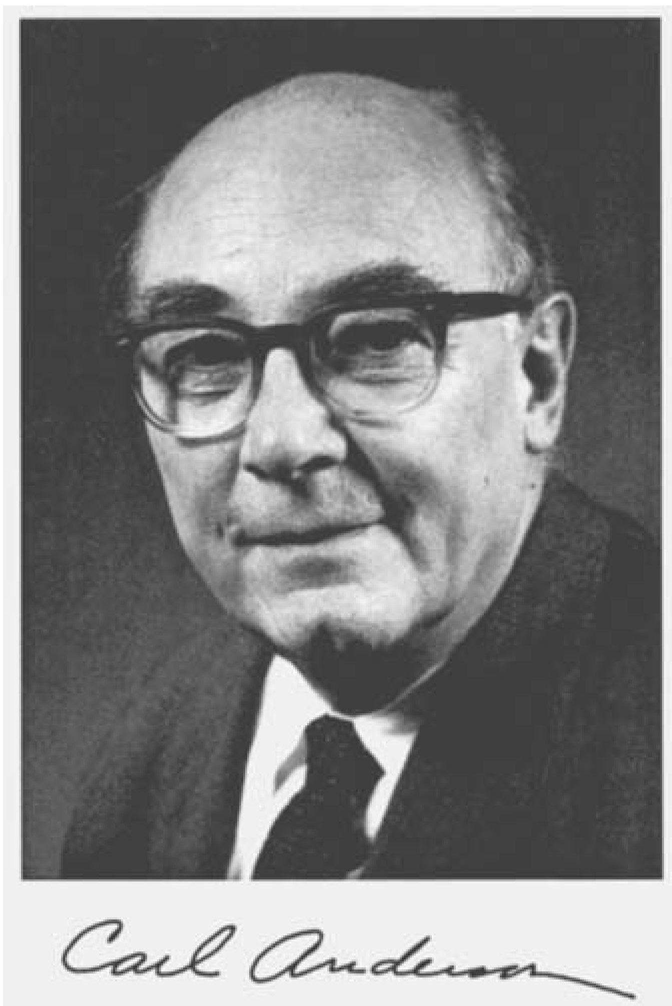
1923 *Everyday Mysteries: Secrets of Science in the Home*. New York: Macmillan.

1925 *The Earth and the Stars*. New York: D. Van Nostrand.

1929 *The Sun and the Welfare of Man*. Smithsonian Scientific Series, vol. 2. New York: Smithsonian Institution Series.

Energy spectra of the stars. *Astrophys. J.* 69:293–311.

1932 *Great Inventions*. Smithsonian Scientific Series, vol. 12. New York: Smithsonian Institution Series.
1944 Weather predetermined by solar variation. *Smithson. Misc. Collect.* 104(5).
1952 Periodicities in the solar constant measures. *Smithson. Misc. Collect.* 117(10).
1953 Solar variation, a leading weather element. *Smithson. Misc. Collect.* 122(4).
1958 *Adventures in the World of Science*. Washington, D.C.: Public Affairs Press.
1960 A long-range forecast of United States precipitation. *Smithson. Misc. Collect.* 139(9).
1963 Solar variation and weather; a summary of the evidence, completely illustrated and documented. *Smithson. Misc. Collect.* 146(3).
1966 An account of the Astrophysical Observatory of the Smithsonian Institution, 1904–1953. *Smithson. Misc. Collect.* 148(7).
1967 Precipitation in five continents. *Smithson. Misc. Collect.* 151(5). Solar magnetism and world weather. *Smithson. Misc. Collect.* 152(6).
1969 With Lena Hill. A long range forecast of temperature for 19 United States cities. *Smithson. Misc. Collect.* 153(5).



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Carl David Anderson

September 3, 1905–January 11, 1991

BY WILLIAM H. PICKERING

BEST KNOWN FOR HIS discovery of the positive electron, or positron, Carl David Anderson was awarded the Nobel Prize in physics in 1936 at age thirty-one. The discovery of the positron was the first of the new particles of modern physics. Electrons and protons had been known and experimented with for about forty years, and it was assumed that these were the building blocks of all matter. With the discovery of the positron, an example of antimatter, all manner of theoretical and experimental possibilities arose. The Royal Society of London called Carl's discovery "one of the most momentous of the century."

Born on September 3, 1905, in New York City, Carl was the only son of Swedish immigrant parents. His father, the senior Carl David Anderson, had been in the United States since 1896. When Carl was seven years old, the family left New York for Los Angeles, where Carl attended public schools and in 1923 entered the California Institute of Technology. Caltech had opened its doors in 1921 with Robert A. Millikan, himself a Nobel laureate, as chief executive. Together with chemist Arthur A. Noyes and astronomer George Ellery Hale, Millikan established the high standard of excellence and the small student body that today continues to characterize Caltech.

Carl was an excellent student. Originally interested in electrical engineering, he changed to physics after attending a class with Professor Ike Bowen. In his junior year he was one of two students awarded the highly coveted travel prize for scholastic achievement. This prize consisted of a grant sufficient for two students to travel in Europe for six months, from March of their junior year to the following September, with an itinerary made up largely of their own choosing. Of the candidates considered for the prize, six were selected to attend a special class on the arts, history, and culture of Europe, given by John MacArthur, dean of freshmen. This was intended to help the winners plan an interesting trip. The chosen two spent most of their time visiting the prescribed round of museums and cathedrals, but they did get to meet scientists H. A. Lorentz and H. Kamerlingh-Onnes.

Carl graduated in 1927 in the option of "physics engineering." He received his Ph.D. magna cum laude in 1930 in the same option. His thesis on the spatial distribution of electrons ejected from gases by X rays, resulted in a *Physical Review* publication. (Caltech dropped the option "physics engineering" in 1931 and replaced it with "applied physics" in 1934.)

Millikan became Carl's graduate advisor. In those days graduate students had a great deal of freedom in their research. Consequently, Carl received very little direction from Millikan, but he advised Carl upon receiving his Ph.D. to get a National Research Council fellowship and go to some other school to broaden his experience. Carl applied to Compton in Chicago, but after being accepted, Millikan changed his mind and urged him to remain at Caltech for at least another year to study the Compton scattering of cosmic rays. Millikan thought that cosmic rays were primarily photons and this experiment would give him information on the energy of the radiation.

Carl had wanted to extend his X-ray thesis work to gamma rays. In this, fortunately, he agreed with Millikan's desire to work with cosmic rays, because, except for gamma rays from thorium C", these natural rays do not have enough energy to produce positrons.

Millikan had become interested in cosmic rays in the early 1930s. Cosmic radiation was first studied by Victor Hess in Austria, but was not well understood. To study the radiation Millikan organized three groups at Caltech, which were to use electroscopes, Geiger counters, and cloud chambers as tools. Carl Anderson handled the cloud chamber investigations. H. Victor Neher developed electroscopes. I was responsible for the Geiger counters. The Wilson cloud chamber is a short cylinder with glass end plates containing a gas saturated with water vapor. The pressure is dropped suddenly so that the gas expands and cools to a supersaturated state. If an ionizing particle has just passed through the chamber, there will be a trail of water droplets on the ions along its path. These droplets are photographed. The density of the droplets is a measure of the ionization produced and, therefore, of the nature of the ionizing particle. For inventing the chamber C. T. R. Wilson was awarded a Nobel Prize in 1927. Carl improved his own chamber by using a piston expanding into a vacuum to drop the pressure very rapidly and by using a mixture of water and alcohol in the chamber. Consequently, he obtained much better photographs than most other cloud chamber experimenters.

Anderson built his chamber on the top floor of the aeronautics building at Caltech, where there was adequate electric power to run the large electromagnet surrounding his chamber. The magnet provided a field of 25,000 gauss over an area 6 inches in diameter. Ionizing particles traveling across this field bend into circular paths. By measuring the curvature of the tracks in the chamber, he calculated the momentum of the particles causing the track and determined the polarity of the charge on the particle. To confirm the direction of travel of the particles, he placed a lead plate across the chamber. Particles passing through the plate emerged with lower than the initial energy, and therefore the direction of travel was confirmed.

At first, the cloud chamber expansions were randomly timed and many photographs showed no tracks. Later, Geiger counters were used to trigger an expansion after a counter recorded the passage of a particle. The result was a marked increase in useful data.

Early pictures showing tracks of cosmic ray particles were a surprise. The cosmic rays produced showers of particles both positively and negatively charged. The two polarities of track showed the same droplet density. If the particles were electrons and protons, the proton track would have been much denser, except at very high energies. This experimental result worried Anderson as well as Millikan. They even speculated that the positively charged particles were actually electrons traveling in the opposite direction. To settle this question Carl put the lead plate in his chamber. A particle passing through the plate loses energy and thus the direction of travel is uniquely determined.

In 1932 Carl recorded the historic photograph of a positively charged electron passing through the lead plate in the center of the cloud chamber. The change in curvature of the path on the two sides of the lead plate showed the direction of travel. It was definitely a positively charged particle. Surprisingly, the particle was traveling upward.

Several well known scientists, including Niels Bohr, were very skeptical of this result, but it was soon confirmed when P. M. S. Blackett and G. P. S. Occhialini published their data in the March 1933 *Proceedings of the Royal Society*.

They proposed that the positron appeared as one of a pair of positive and negative electrons produced when a gamma ray was converted into matter. The positron had a very short life before being absorbed by a collision with an electron, which produced two gamma ray photons of 511 kev energy each.

Anderson said that he discussed the problem of the formation and disappearance of the positron with J. Robert Oppenheimer and, in retrospect, was surprised that Oppie did not come up with this pair-production mechanism. He also commented that it was very difficult to understand Oppenheimer's answers to his questions. Later, when he talked with Richard P. Feynman, just the opposite was true. Feynman was clear and precise.

Anderson's positron and J. Chadwick's neutron, discovered and reported in the *Proceedings of the Royal Society* in 1932, were the first new fundamental particles. Chadwick also received a Nobel Prize in 1935, a year before Anderson. In a few years these discoveries led to the "zoo" of strange particles of modern physics.

Millikan had carried electr0oscopes and Geiger counters to various places so that he could use the earth as a giant magnet to analyze the energies of primary cosmic rays. Carl and his graduate student, Seth Neddermeyer, determined to follow Millikan's lead and take their cloud chamber to high altitudes and various latitudes. The cloud chamber was mounted on an old flatbed truck and, with great difficulty, driven to the summit of Pike's Peak. In fact, they were towed up most of the way. The two experimenters found evidence for a new short-lived particle intermediate in mass between the electron and the proton. This was originally called the mesotron, but is now known as the mu meson. Photographs taken in Pasadena and in Panama confirmed the existence of this new particle.

In the Depression days of the 1930s financing scientific research was difficult. The Pike's Peak trip was done on a shoestring. First they found a 1932 ton-and-a-half Chevrolet truck, which they purchased for $400. They then found a flat bed trailer and improvised a hitch. Some old packing cases were made into a housing for the equipment. Arriving in Colorado Springs, they had the motor overhauled and the clutch replaced. Even so, they couldn't quite make it to the toll gate up the mountain. The Pike's Peak Company towed them up, probably to clear the road.

Their troubles were just beginning. They had an old Cadillac engine generator set that wouldn't produce adequate power at 14,000 feet. When they took the generator down to Colorado Springs to be fixed, the Chevrolet truck broke down because the repair work had not been done properly. However, as luck would have it, a new Chevrolet test truck appeared, which was being monitored on a trip up the mountain. It carried as a passenger a General Motors vice-president, who stopped to inquire about the scientist's problem. He then very kindly had the truck towed up the hill and subsequently replaced the engine free of charge. But their troubles weren't over. They couldn't afford to pay the $2.50 per night for the six-week stay in the shed on top of the mountain, so they bought a Chevy Roadster for $50 and stayed at a road crew bunk house half way down the mountain. They were obviously two dedicated scientists and they were first-class experimenters. During their month and a half on Pikes' Peak they took about 10,000 photographs.

Shortly before America entered World War II, Compton contacted Anderson and invited him to be the director of what soon became the atomic bomb project. Compton suggested he choose Oppenheimer as his assistant. Anderson turned him down because he felt that he lacked the necessary administrative experience, and because of concern for his mother who was in ill health.

During World War II, Carl remained at Caltech and joined physicists Charles Lauritsen and Willie Fowler on the Caltech artillery rocket project for the Navy. This project was to develop a solid propellant rocket with enough accuracy to deliver an artillery barrage from a simple launch platform. Anderson was primarily concerned with problems associated with launching the rockets from aircraft. By the end of the war, these rockets were being produced by Caltech in very large numbers, and were launched from ships in several Pacific island landings. The aircraft version that Carl helped develop was used against submarines, which had been detected by magnetic sensors.

Carl's student, Seth Neddermeyer, who had received his Ph.D. in 1935, left to join the staff of the University of Washington. He later left the university and went to Los Alamos to work on implosion technology for the atomic bomb.

In 1944 Carl traveled to the Normandy beachhead to observe the use of Caltech rockets under battlefield conditions. He spent a month in France and helped install rockets on Allied aircraft. The reports of the rocket's effectiveness were excellent.

In 1947 Anderson received support from the Office of Naval Research to fly cloud chambers in two B-29 airplanes. Data were collected at the maximum height the aircraft could attain. New information about the decay of primary cosmic ray particles was obtained, including a photograph of the disintegration of a mu meson. However, problems of installing and operating the equipment on this aircraft inhibited their work. Carl felt they should have spent more time on physics and collected more data on the strange particles. Later, Carl with Professor Robert Leighton and some Caltech graduate students made observations with an improved apparatus at an altitude of 10,000 feet in the White Mountains of northeastern California.

Anderson's work with cosmic rays in the 1930s and 1940s was important for the development of modern particle physics. Other physicists in Europe and the United States using both cloud chambers and photographic emulsions collected data that confirmed Anderson's results and added additional particles to the physicist's "zoo."

The theoretical physicists set the stage. Paul A. M. Dirac postulated the "anti-electron" in 1928 and received the Nobel Prize in 1933. Hideki Yukawa required the existence of a heavy, short-lived particle for his theory of the nucleus, and for this theory he received the Nobel Prize in 1949. For a time Anderson's "mesotron" was assumed to be the Yukawa particle, but it turned out to have too long a lifetime and to be too light. Anderson was not actually searching for Dirac's anti-electron. Its discovery was a complete surprise. However, once the positron and the neutron were known, the search for additional particles was guided by speculations of the theoretical physicists.

Today experimental work in particle physics is done almost exclusively with large particle accelerators. These machines now produce energies in the same range as primary cosmic rays. Also, the machines produce large energy fluxes on demand, so that they are much more useful than the natural cosmic rays.

Caltech elected not to enter the very large machine race, so that Carl found his research area preempted by others. Caltech needed Carl for other tasks. He was appointed chairman of the freshman admissions committee and chairman of the division of physics, mathematics, and astronomy from 1962 until his retirement in 1970.

Carl's parents divorced shortly after the move to Los

Angeles, and Carl found himself helping to support the family. In retrospect, he said that he was amazed that his mother was able to do so well on so little money. Money was so short that he had to borrow $500 from Millikan to make the trip to Sweden for his Nobel Prize.

Carl married Lorraine Bergman in 1946. It was her second marriage. Her three-year-old son Marshall David was immediately adopted by Carl. Another son, David Anderson, was born in 1949. Marshall is now a computer mathematician and David is an engineer. The Anderson family lived in San Marino, not far from Caltech. Lorraine passed away in 1984.

All of Carl's professional career was at Caltech. He was a teaching fellow (1927–30), research fellow (1930–33), assistant professor (1933–37), associate professor (1937–39), and a full professor (after 1939). He retired in 1970 and in 1976 was made the Board of Trustees professor of physics emeritus.

In 1936, when he was awarded the Nobel Prize, he was an assistant professor. He shared the Nobel Prize with Victor Hess, who had first measured cosmic radiation in 1912. They each received about $20,000—a princely sum in 1936. Carl used half of his money to cover his mother's medical expenses and invested the balance in California real estate.

In his Nobel lecture Carl described how he obtained his first cosmic ray tracks in 1931. A preliminary report published in 1932 indicated that energies in excess of a billion electron volts were involved, and that approximately equal numbers of positives and negatives appeared in these cosmic ray showers. Even the relatively low energy positives, those with energy less than 500 mev had the same ionization track density as the negatives.

By putting a 6-mm lead plate across the center of the cloud chamber he settled the question of the direction of travel of the particles, and also allowed a measure of the energy loss in passing through the plate. The key photograph had a positive particle entering the plate with 68 mev and leaving it with 28 mev. Had this particle been a proton its range in the gas of the chamber would have been at least ten times smaller than the actual path.

Anderson's data from his Pike's Peak and Panama experiments encouraged him to conclude his Nobel lecture as follows:

> These highly penetrating particles, although not free positive and negative electrons, appear to consist of both positive and negative particles of unit electric charge, and will provide interesting material for future study.

Within a few months it was obvious that a new particle with mass intermediate between electron and proton was needed to explain the data.

Carl enjoyed teaching, and enjoyed research as an individual effort. He was not interested in being part of a large team. His Nobel Prize, of course, exposed him to many calls for public appearances and speeches. He did not enjoy this exposure.

Other awards he received included the Gold Medal of the American Institute of the City of New York (1935), the Elliott Cresson Medal of the Franklin Institute (1937), the Presidential Certificate of Merit (1945), and the John Ericsson Medal of the American Society of Swedish Engineers (1960). He received honorary degrees from Colgate University (1937), Temple University (1949), and Gustavus Adolphus College of St. Peter, Minn. (1963).

Elected to the National Academy of Sciences in 1938, Carl was chairman of the Physics Section from 1963 to 1966. He was a fellow of the American Physical Society and a member of the American Philosophical Society, the American Association for the Advancement of Science, the

American Academy of Arts and Sciences, Sigma Xi, (president, 1947–48), and Tau Beta Pi.

Carl was not particularly interested in politics, although he did sign a petition prepared by Tommy Lauritsen of the physics department against the testing of hydrogen bombs. About ten faculty members signed, but the Caltech administration officially stated this was not Caltech policy. Apparently, there were no repercussions against any of the signatories. Carl's graduate student Seth Neddermeyer may have registered as a communist at one time, but neither Carl nor Seth played an active role in any political campaign or movement.

Outside of his professional interests Carl was most interested in automobiles and auto racing. He was also a radio ham (call sign W6KGR). He listed his recreations as "tennis, mountains, desert, music." He was a member of the Twilight Club, a social club for Pasadena leaders.

In 1962 President Kennedy held a White House dinner for Nobel laureates, and Carl attended. He was honored to be seated at a table with the Swedish ambassador's wife on his right, Mrs. Ernest Hemmingway on his left, the President next to Mrs. Hemmingway, and Mrs. George Marshall on the President's right. Carl recalled the President's comment that this was the greatest gathering of talent at a White House dinner since Thomas Jefferson dined there alone. The dinner was on the same day that Linus Pauling had picketed the White House. In the reception line Mrs. Kennedy commented to Pauling that she wished he wouldn't picket, because when that happened Caroline would ask, "What has Daddy done wrong now?"

The period from 1930 to 1940 was a very fruitful one for particle physics. Not only were new fundamental particles discovered, but they also induced radioactivity and, of course, nuclear fission. Studies of natural radioactivity and of cosmic rays showed the complex nature of the atomic nucleus. Bombarding the atom with high-energy particles or photons broke up the nucleus and produced new nuclei. By 1940 most of the great collection of stable and unstable nuclei that we now know had been found.

In retrospect Anderson's achievement was due in part to R. A. Millikan's intuition that the study of cosmic rays was important and that Anderson had the experimental ability to build a superior cloud chamber. The intellectual climate at Caltech encouraged the young physicist. This was still the period when physics was being done with "love and string and sealing wax." Brilliant scientists with very little money or other support were pushing back the frontiers of physics and in the process giving us new concepts of the world. Anderson's "anti-matter" was the first step that led to an understanding of the atomic nucleus.

Selected Bibliography

1930 Space distribution of X-ray photoelectrons ejected from the K and L energy levels. *Phys. Rev.* 41:405.

1932 The apparent existence of easily deflectable positives. *Science* 76:238.

1933 Energy loss and scattering of cosmic-ray particles. *Phys. Rev.* 43:381.

The positive electron. *Phys. Rev.* 43:491.

Free positive electrons resulting from the impacts upon atomic nuclei of the photons for Th C". *Science* 77:432.

Positrons from gamma rays. *Phys. Rev.* 43:1034.

1934 With R. A. Millikan, S.H. Neddermeyer, and W. H. Pickering. The mechanism of cosmic ray counter action. *Phys. Rev.* 45:342.

With S. H. Neddermeyer. Energy spectra of positrons ejected by artificially stimulated radioactive substances. *Phys. Rev.* 45:498.

Disintegrations with positron ejection. *Phys. Rev.* 46:322.

1936 With S. H. Neddermeyer. Cloud chamber observations of cosmic-rays at 4300 meter elevation and near sea level. *Phys. Rev.* 50:263.

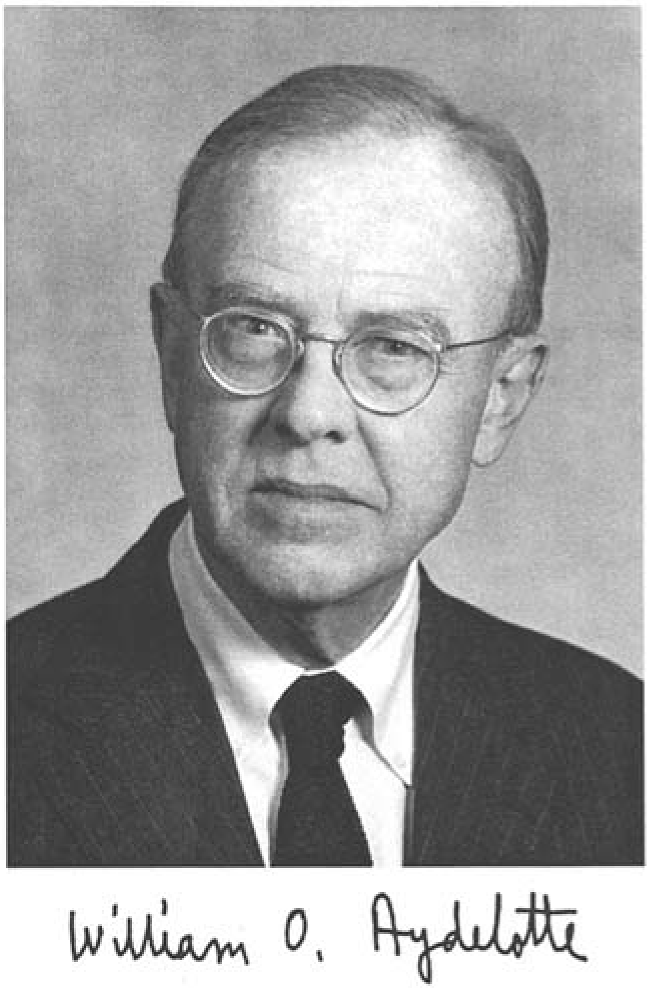
The production and properties of positrons. *Les Prix Nobel.*

1938 With S. H. Neddermeyer. Cosmic-ray particles of intermediate mass. *Phys. Rev.* 54:88.

1939 With S. H. Neddermeyer. Nature of cosmic-ray particles. *Rev. Mod. Phys.* 11:191.

1940 *Cosmic Rays and Elementary Particles of Matter*. Yale University Press.

1947 With others. On the mass and the disintegration products of the mesotron. *Phys. Rev.* 72:724–27.



Courtesy of Kent Studio, Iowa City, Iowa

William Osgood Aydelotte

September 1, 1910–January 17, 1996

BY ALLAN G. BOGUE AND GILBERT WHITE

AYDELOTTE WAS A leading figure in the development of social science history in the United States. Many historians consider themselves humanists or argue that theirs is a unique discipline. Others are social scientists in intellectual commitment and methods of research, and it was from this group that Aydelotte was the first to be elected to the National Academy of Sciences. Beneath the affiliative differences among historians lie disagreements as to whether history should be presented in narrative or analytical form, the degree to which historical evidence can serve as the basis for generalization and, indeed, what constitutes appropriate evidence. Aydelotte's career illuminates these issues. Publishing initially in the field of narrative diplomatic history, he emerged as the most innovative investigator of legislative behavior of his era in the history profession.

EARLY LIFE AND TRAINING

Aydelotte described his childhood as "odd." He was born in Bloomington, Indiana, the only child of Marie Jeannette Osgood and Frank Aydelotte, a union of small town Indiana and academia on the father's side and the world of international music on that of his mother. Frank Aydelotte was a man of great intellectual and athletic ability and a talented academic administrator, who pursued graduate work in English literature at Harvard University and studied thereafter as a Rhodes fellow at Oxford University. He taught English literature at Indiana University, moved to the Massachusetts Institute of Technology in 1915, became president of Swarthmore College in 1921, and director of the Center for Advanced Study at Princeton University in 1939. Frank Aydelotte established a program of honors seminars and student concentrations at Swarthmore that brought the college into the first rank of American colleges. He was American secretary of the Rhodes trustees (1918–52), chairman of the education advisory board of the John Simon Guggenheim Memorial Foundation (1925–50), and held many other advisory positions relating to education, government, and international affairs.

William Osgood remembered a household that was much open to students, to his father's colleagues, and, after the move to Swarthmore, to the varied visitors and dignitaries who move through leading college and university communities. The parents were kind and supportive and their son never lacked for the means to further his academic education. At the appropriate time in his musical training he received a Steinway grand piano. Nor did William lack family encouragement in making friends, although he found the process difficult in his younger years. Particularly after the move to Swarthmore, however, his parents' attention was often turned elsewhere. Troubled with recurrent poor health and an indifferent athlete, young Aydelotte experienced acute feelings of loneliness. Good literature was always at hand in great quantity in the Aydelotte home, however, and the boy read voraciously the works of leading nineteenth- and twentieth-century European and American authors.

As a child, William impressed relatives as precocious, in part imp and in part serious beyond his years. Apprehensive of the results of placing their son in the public schools of Cambridge, the Aydelottes enrolled him in the Shady Hill School, a progressive elementary school that local faculty families had established and where leadership was provided by Agnes Hocking, an inspiring teacher of poetry and literature. When the family moved to Swarthmore, William entered public school, but the results were unsatisfactory. Various remedial arrangements failed and the Episcopal clergyman at Swarthmore successfully tutored the lad for a time. William spent his last two pre-college years in the William Penn Charter School in Germantown, Pennsylvania, a beneficial matching of talent and institution. He graduated at the end of his first year, but being only fifteen, stayed on for a "post-graduate" year and took the instruction in Greek that was normally studied during the course of three years.

Majoring in classics at Harvard University, Aydelotte graduated in 1931. In his estimation he did not have an outstanding undergraduate career, but did make Phi Beta Kappa. Although at Harvard he found the teaching of E. K. Rand and Milman Perry to be challenging, the classics program was not outstanding in his years there. In contemplating graduate work, he decided to study history and proceeded to take a Ph.D. in history at the University of Cambridge. There distinguished scholars Harold Temperley and Herbert Butterfield provided more rigorous intellectual discipline than he had hitherto experienced. But, neither his classical training nor his graduate studies in history acquainted him with systematic inquiry or scientific approaches to the acquisition of knowledge. Throughout his educational and early professional years, astigmatism prevented him from undergoing military training or service.

PROFESSIONAL POSTS AND HONORS

Although the Cambridge training and his published research in European diplomatic history gave Aydelotte academic credentials, he remained undecided as to his future course. Following completion of his doctorate in 1934 he became an assistant in the office of the chairman of the Federal Home Loan Bank Board in Washington, D.C. While in Washington, he associated with a group of young economists, geographers, political scientists, and sociologists, who were enthused at the opportunity to use their analytical skills in solving the social problems confronted by the New Deal. Aydelotte always considered his experience in Washington to have been valuable, but he left the public policy arena after two years to begin his career as a college and university teacher. At that time, he also thought seriously of becoming a writer or essayist, an ambition that his father strongly opposed. After a decade of teaching, involving appointments at Trinity College in Hartford, Conn., Smith College in Northampton, Mass., and Princeton University, Aydelotte accepted a position in 1947 as assistant professor of history at the State University of Iowa, Iowa City (now the University of Iowa), where he remained for the remainder of his scholarly career. He served as department chairman (1947–59 and 1965–68) and as Carver professor of history (1976–78), retiring in the latter year. Other appointments during the Iowa years included visiting professorships at Harvard University and the University of Leicester in England.

Aydelotte served on the Council of the Social Science Research Council (1964–70) and on its Committee on Historical Analysis. He was a member of the History Advisory Committee of the Mathematical Social Science Board (1968–72) during the period when that group planned an extensive program of symposia dealing with the use of mathematics and statistics in historical research. He edited volumes of research papers delivered at two of these conferences. He was a member of the Quantitative Data Committee of the American Historical Association (1968–72) and of the organizing committee of the Social Science History Association (1973–77), an offshoot of the Quantitative Data Committee. He held the position of chairman of the mid-western section of the Committee of Selection for Marshall Scholarships (1955–60), his service recognized by an honorary Order of the British Empire in 1961. He was a member of the Committee of Selection for Woodrow Wilson Dissertation Fellowships (1962–68) and was active in the American Association of University Professors. He was a fellow at the Center for Advanced Study (1945–47) and the Center for Advanced Study in the Behavioral Sciences (1976–77). The members of the National Academy of Sciences elected him to membership in 1974, and he served on the National Research Council's Panel on Privacy and Confidentiality as Factors in Survey Response. The Iowa Academy of Science named him a distinguished fellow in 1975 and he became president of the Social Science History Association in 1980. He was a member of the editorial boards of the *Journal of Modern History* and of *Social Science History*.

PERSONALITY

Well over 6 feet in height, blonde, blue-eyed and wearing glasses, Aydelotte appeared on first meeting to be reserved in manner or even intimidating. He was in actuality, a very kind person who was prepared to exchange views on an extremely broad range of subjects, extending from European and American authors—"Proust is best read in the original"—to music, British and American politics, current trends in social science research, the mission of higher education, and regional or national eccentricities. Although he was essentially a serious, introspective man of quiet dignity, his conversation was lightened by a subtle but kindly wit, and often conducted with quiet animation. A tennis player and hiker, he did not discuss baseball and to him football presented an opportunity to study crowd psychology rather than backfield intricacies. A personal trade mark was the 3 x 5 notepad, which he always carried in a pocket and used whenever conversation or discussion produced a tip on research or departmental administration, or some useful fact about his other interests. He could, when on leave and focussed on his research, carry midwestern informality to the point of coming to his university study in bib overalls; he once shattered the shadowy ambiance of San Francisco's best French restaurant by requesting that an extra lamp be placed at the table so that he could more satisfactorily show his companion the latest set of tables derived from his research data.

Aydelotte was educated at two of the western world's leading institutions of higher learning. During his university training he spent parts of two pleasant summers traveling with his mother and father abroad. His family ties later gave him a unique understanding of the "world-celebrities and prima donnas" gathered at the Institute for Advanced Study. As an adult, therefore, he was superbly equal to the demands of any social occasion, intimidated by neither don nor dowager. But for many years he suffered self doubt as to whether he could be as successful as his father. Although he never severed relations with his parents, there were painful generational differences in values involving his father's attitudes toward Aydelotte, his research, and his scholarly objectives. At his memorial service, a longtime friend found one of the most striking aspects of Aydelotte's career to be his passage from "a state of satellite dependence on his father" to one of stalwart independence in which he transformed a department of modest reputation into an exciting intellectual environment, while at the same time pioneering new modes of research and analysis in his field of history.[1] Aydelotte's marriage on June 22, 1956, to Myrtle Kitchell, the wise and sensitive dean of the University of Iowa College of Nursing, contributed greatly to the happiness of his later career, as did two lively daughters, Marie and Jeannette. Aydelotte died in Iowa City, Iowa.

THE IOWA HISTORY DEPARTMENT

When Aydelotte (now usually known as Bill Aydelotte) arrived in Iowa City in 1947, he joined the faculty of a university whose constituency had suffered severely during the Depression of the 1930s. Financial stringency had limited university development and the situation in the history department was further complicated by the failing health of the elderly department head. After the latter's death in 1947, younger elements in the department took control and Aydelotte became chairman. For many years he put much thought and energy into developing a department of the highest quality. In making faculty appointments, the university provost once remarked that Bill Aydelotte always wished to appoint Jesus Christ, and failing that, demanded one of the twelve disciples.

After luring to Iowa a very able intellectual historian from Princeton University, Aydelotte supervised a series of appointments that made his department widely respected. University resources were insufficient to allow the hiring of senior eminences. Iowa faculty salaries on average ranked close to the bottom in comparison with those in other "big ten" institutions, but were competitive at the junior levels. In reaction to this situation, Aydelotte tried to identify and hire the ablest young scholars available, a strategy that was easier to implement successfully during the 1950s than at other times, because the academic job market had tightened after the colleges and universities had absorbed the postwar influx of veterans. As a result of these circumstances and his skill in search and selection, Aydelotte filled his department with scholars who were destined to have very successful careers. Few leading American advisors of graduate students failed to be subjected, at some point during Aydelotte's Iowa tenure, to intense interrogation about the abilities of one or more of their graduate students, even to the point of having their placement letters of previous years quoted to them. They who maintained that their geese were swans were given little credence once Aydelotte established the fact.

In approaching young scholars, Aydelotte emphasized the democratic nature of the department, now led by a chairman serving a three-year term, and the opportunity for instructors and assistant professors to be involved directly in departmental decision making. Aydelotte promised lighter teaching loads in the first year of teaching and early access to the pool of research assistants. Many of the appointees were later hired away by better funded institutions after they had demonstrated their abilities at Iowa. Some among Aydelotte's colleagues later filled chairs in leading history departments, accepted appointment at the Institute for Advanced Study, and became presidents of the American Historical Association and the Organization of American Historians. Aydelotte's hiring policies and personal example made the Iowa department a place of high intellectual voltage, rigorous standards, and absolute integrity. The pioneering research of faculty and graduate students in both European and American history caused some observers to see evidence of an "Iowa school" of behavioral history. Aydelotte led by example in the class room as well; his lectures on the philosophy of history and British and European intellectual history were constantly in a state of meticulous revision.

RESEARCH CONTRIBUTIONS

Aydelotte recalled that he found much of his training and early professional career to be unfulfilling. Periods of his pre-college schooling were frustrating and few of his instructors at Harvard excited him. He left the study of classics for history when he began doctoral work, but with that completed he entered government service for which training in neither classics nor history provided full preparation. Viewed from Washington, his future career appeared unpromising, but his experiences there pointed him back to academia with a deeper understanding of the social usefulness of research. His interests continued to shift during his first decade of teaching as he turned from diplomatic history and the study of imperialism to English social, political, and intellectual history. He planned research projects on the role of the churches in the industrial revolution and Charles Dickens, but he did not carry either through to completion.

At the University of Iowa, Aydelotte's research interests came into focus. In his early years there he was a member of an informal seminar of social scientists, who were, he wrote, "interested in problems of social psychology and in the new methods and concepts it suggest[ed]" for their work.[2] With these men he discussed major publications in social science, including the writings of Paul F. Lazarsfeld, Max Weber, Robert Michels, Maurice Duverger, Hannah Arendt, and David Riesman. He now understood that his primary interest lay in explaining human motivation within its historical contexts and that elements in the behavioral analysis of the time promised to be rewarding in his research. In 1948 he published an article that explored the social attitudes reflected in the work of the leading English novelists of the 1840s. He followed this during the next year with a study of the detective story in which he argued that "a careful study of literature of this kind [might] reveal popular attitudes which shed a flood of light on the motivation behind political, social, and economic history."[3]

Although these papers attracted favorable attention, Aydelotte realized that conclusions derived from the content analysis of literary sources were too conjectural to be completely satisfying. But the division lists of the British Parliament of the 1840s provided a body of data revealing political opinion that was much more amenable to rigorous analysis. This source was all the more intriguing because, in repealing the Corn Laws, the parliament of 1841–47 effected one of the fundamental policy reversals in modern British history. Other researchers had maintained that modern political attitudes had their source in the 1840s and believed that agrarian interests were yielding governmental power to industrial elements at that time. The Corn Laws Parliament provided Aydelotte with the data for a research project that he began during the late 1940s and continued during the remainder of his career. In retrospect he explained that his research focus from the early 1950s forward lay "in general problems about legislative behavior; defining the nature of this behavior, using for this purpose the rich information in the division lists; and examining the relation of behavior to other variables that may be hypothesized to have affected it, such as party, personal background, and constituency."[4] This commitment allowed him to produce significant theoretical and substantive findings in the fields of legislative behavior and theory, as well as to revise scholarly understanding of British history during the 1840s.

Some members of earlier generations of professional academic historians had tried to use quantitative evidence analytically, but that practice had declined by the 1940s. There was need, therefore, to explain the relevance of quantification to historians on the one hand and the usefulness of the historical dimension in research to social scientists on the other. These challenges defined a second major area of writing for Aydelotte during his Iowa years.

Beginning in the late 1940s, Aydelotte assembled personal and political biographical data bearing upon the 815 members who sat in the British House of Commons between the British general elections of 1841 and 1847 and undertook the task of relating them to their behavior in parliament. Locating the information, transcribing, and classifying it involved an intimidating amount of labor and thought, given the primitive state of data processing at the time and the complex relations between British politics, economy, and class structure. Aydelotte's experience provides a case history of the development of research technology in social and political research during the years from 1945 to 1980. First, he used an electric calculator in analyzing his various categories of data and their interrelationships, but then transferred his materials to punched cards and used IBM machines for counting and sorting. As the computer revolution took hold, he employed the university computer.

Reaction to descriptions of Aydelotte's technological adventures ranged from awe to scorn. A critical English scholar accused him of using a machine for thinking—"Shame!" On these matters, Aydelotte never succumbed to zealotry, always noting that the evidence bearing on some important areas of behavior would always be lacking; that some evidence resisted classification and quantification; that statistics could be a dangerous form of weapon; that statistical tables created a false sense of completeness and security and might also be subject to different interpretations. In themselves, he cautioned, quantitative data did not create hypotheses, serving primarily as evidence to be used in testing them. Despite the skepticism, Aydelotte soon found a sympathetic audience both in the United States and in England, where historians at Oxford University were particularly supportive.

Between 1954 and 1977 Aydelotte published a series of papers dealing with the political behavior of the members of the Corn Laws Parliament. In them he described his data and methods and established, as no British historian had thus far done so completely, the exact proportions of the various economic and social groups represented in parliament. But the complexity of the data base was bewildering. Previous researchers had advanced a number of major hypotheses about the relations between class and economic groupings and political behavior. Aydelotte's statistical tests initially showed that the connections were weak or did not hold. Negative findings demonstrated progress of a sort but left the roots of behavior still mysterious. In the mid-1950s Aydelotte learned of Guttman scaling from colleagues in the social sciences at the University of Iowa. He realized that this powerful technique allowed him to order his data so as to allow reconsideration of questions about which investigators had long argued. Employing Guttman scales as his basic statistical tools, Aydelotte used the votes of the members of the House of Commons to construct the ideological dimensions of behavior in the Corn Laws Parliament and to analyze the relation of party affiliation, economic and social position, and constituency to the voting patterns.

A paper dealing with the influence of constituencies on parliamentary behavior, appearing in 1977, was Aydelotte's last published work in the field of British political behavior. He continued his investigations and hoped to produce a series of volumes that would combine much unpublished analysis with the work that was already in print. Deteriorating health prevented him from doing so. Some scholars publish too much of their research, and others, unfortunately, publish much too little. Aydelotte is numbered among the latter.

OTHER PUBLICATIONS

During the 1950s Aydelotte was a leader among the historians who envisioned the adoption of analytical approaches in history involving greater application of social theory, systematic analysis, and quantitative evidence. By the early 1960s these trends had created a backlash within the history profession. A president of the American Historical Association denounced "worship at the shrine of that bitch-goddess, QUANTIFICATION."[5] Several of the discipline's brightest younger stars pronounced quantitative analysis to be sterile or destructive. These calls for purification of the discipline were also indirect attacks on the social sciences, the presumed source of infection. In a number of notable statements, Aydelotte explained and justified the use in history of quantitative data and methods more commonly used by political scientists and sociologists. Preparing these papers also helped him to clarify his thinking on the nature of the research process and its component elements of data collection, hypothesis, verification, and generalization.

Aydelotte published the first of these publications as "Notes on the Problem of Historical Generalization," a chapter in *Generalization in the Writing of History* (1963) prepared by the Committee on Historical Analysis of the Social Science Research Council. Here he dealt with the problems of nomenclature, proof, theory, and procedure, including an extended argument on behalf of the use of statistical procedures in the last section. In a second major essay, "Quantification in History" (1966), he considered the quantification issue in greater detail, refuting critics and explaining the benefits of adding statistical methods to the historian's arsenal. This was published in the *American Historical Review,* the journal in which the sponsoring organization's president had inveighed so trenchantly against quantification three years earlier.

Subsequently, Aydelotte included these essays in a collection, *Quantification in History* (1971), along with an introductory statement of argument, a discussion of the feasibility of establishing a machine-readable archive of British political data, and an earlier published discussion of the problems of using quantitative analysis in the study of the Corn Laws Parliament. An appendix to this volume contained an exchange of correspondence with Jack Hexter, a specialist in English history, which began when the latter asked Aydelotte to comment on the text of his article "The Rhetoric of History." Later Hexter reacted adversely to the attitude of historians presenting papers illustrating the use of statistical methods of analysis in history at a symposium organized by the History Advisory Committee of the Mathematical Social Science Board of which Aydelotte was a member. In the letters of Hexter and Aydelotte, the pungency of the debates of those years is clearly revealed. Hexter complained that historians who used statistics had "lost mastery of their native tongue," and evinced no concern that other historians could no longer understand them, while Aydelotte deplored scholars who "simply gas about the subject in an inconclusive fashion, toss in a few cheesy epigrams and a little vivid writing, and let it go at that."[6]

In presenting his presidential address, "The Search for Ideas in Historical Investigation," to the Social Science History Association in 1978, Aydelotte returned to a long-time interest, discussing theories about the generation of ideas and the circumstances that were conducive to the process. Although directed to historians, this paper, as with his other papers on method and theory, could be read profitably by workers in any field of the social sciences.

SIGNIFICANCE OF AYDELOTTE'S RESEARCH

Aydelotte was keenly aware of the difficulty of developing useful generalizations in history about social processes that could be considered valid in all times and places (that is, laws of behavior or historical development). He preferred rather to work at "middle range," endeavoring, in his own words, to "produce for a single period some reliable findings relating to … great issues, and to make a contribution also to a better understanding of British politics in the mid-nineteenth century." [7] His research allowed him to argue convincingly that ideological dimensions underlay behavior in one of the most important parliaments in British history and to demonstrate that voting behavior there was systematic and could be ordered into a major left-right dimension. He showed as well that voting was multidimensional, the dimensions according, however, with the divisions between the major parties. Although party defections sometimes influenced the outcome of voting in the parliament of the 1840s, independent members did not form a swing group whose influence prevented party leadership from adopting too radical a stance, as some scholars had suggested. Aydelotte also corrected prevailing interpretations to the effect that nineteenth century parties were primarily instrumental in focus and that multidimensionalism in legislative behavior was a twentieth-century phenomenon only. He showed as well that parliamentary leaders during the mid-nineteenth century did not accept the rationale for a parsimonious majority advanced by modern game theorists.

Aydelotte's research constituted the most careful analysis of the social composition of the British parliament to that point in time. He qualified the importance of the Reform Act of 1832 and industrialization in changing the social character of the parliament and demonstrated that it was extremely difficult to show strong relationships between social and economic backgrounds and legislative behavior. Although the characteristics of constituencies were strongly related to behavior, deviations from constituency position by members, once elected, did not appear to have produced measurable constituency reaction.

Aydelotte's investigations also provided a very important illustration of the data, methodological problems, and rewards involved in systematic quantitative research when very little of that kind of analysis was being attempted in history. His theoretical papers dealing with the broader issues involved in historical research and the place of quantitative analysis within that setting were even more widely distributed due to reprintings and translations into foreign languages and did much to create a climate of opinion in which his kind of research was accorded general acceptance, if not universal approval. According to one observer, he also demonstrated that "it could be written with a measure of grace, that jargon could be avoided, and that pretension and arrogance are never warranted."[8]

Due to Aydelotte's activity in the programs of the Social Science Research Council, the Mathematical Social Sciences Board, the Inter-University Consortium for Political and

Social Research, and the Social Science History Association, he became acquainted with numerous social scientists. Sometimes as well, Aydelotte joined members of the Iowa political science department in teaching an interdepartmental graduate seminar in legislative behavior. He found these contacts with social scientists to be stimulating, but they also encouraged investigators working in the related disciplines to incorporate historical elements in their research designs, a direction that growing numbers in political science and sociology were to take.

During the years of Aydelotte's greatest professional activity, researchers were demonstrating the research value of the manuscript United States census data, assembled in the decennial enumerations of the years 1790 to 1880. They looked forward to using equivalent information from later censuses, due to be opened for research generally at the end of seventy years from the date of enumeration. Fearing that concern about public use of these data would affect the rate of response in future censuses, employees of the Bureau of the Census became reluctant to allow the data of 1900 and subsequent enumerations to be released, even suggesting that the census data of 1980 should remain closed indefinitely. In the face of widespread concern within the research community, the National Research Council established the multidisciplinary Panel on Privacy and Confidentiality as Factors in Survey Response to report on the issues involved. Aydelotte was a member of this panel and, when the final draft of its report was unclear in stating the implications of the findings, he appended a statement noting that their upshot, "suggests that concern over the issue of confidentiality may have been exaggerated, and they tell against the claim that a promise of perpetual confidentiality or of long delayed access to identifiable data is essential to obtaining information."[9] His point of view prevailed. The practice of releasing federal census data to the research community at large after a period of seventy years was retained.

In a broad sense, Aydelotte's election to the Academy was a recognition of the validity of adopting quantitative social science methods in historical research and of the relevance of that research to other scientific analyses.

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NOTES

1. S. Persons. William O. Aydelotte, 1910–1996, p. 1, unpublished.
2. W. O. Aydelotte to M. Hastings, February 19, 1950. William O. Aydelotte papers, box 28. Special Collections Department, University of Iowa, Iowa City, Iowa.
3. W. O. Aydelotte. The detective story as a historical source. *Yale Rev.* 39(1949):91.
4. W. O. Aydelotte. Autobiographical statement. National Academy of Sciences archives, Washington D.C., p. 18.
5. C. Bridenbaugh. The great mutation. *Am. Hist. Rev.* 68(1963):326.

6. W. O. Aydelotte. *Quantification in History*, p. 161. Reading, Mass.: Addison-Wesley, 1971.

7. W. O. Aydelotte. Autobiographical statement. National Academy of Sciences archives, Washington D.C., p. 19.

8. J. Clubb. Personal communication, July 18, 1996.

9. W. O. Aydelotte. In *Privacy and Confidentiality as Factors in Survey Response*, p. 119. Washington, D.C.: National Academy of Sciences, 1979.

Selected Bibliography

1937 *Bismarck and British Colonial Policy: The Problem of South West Africa, 1883–1885.* Philadelphia: University of Pennsylvania Press.

The first German colony and its diplomatic consequences. *Cambridge Hist. J.* 5(3):291–313.

1948 The England of Marx and Mill as reflected in fiction. *J. Econ. Hist.* 8(suppl. 8):42–58.

1949 The detective story as a historical source. *Yale Rev.* 39(1):76–95.

1951 Wollte Bismarck kolonien? In *Deutschland und Europa: Historische Studien zur Volker—und Staatenordnung des Abendlandes, Festschrift fur Hans Rothfels* , ed. W. Conze, pp. 41–68. Dusseldorf: Droste-Verlag.

Nineteenth century British pamphlets at the Newberry Library. *Newberyy Libr. Bull.*, 6:179–81.

1954 The House of Commons in the 1840s. *History* 39(137):249–62.

A statistical analysis of the parliament of 1841: Some problems of method. *Bull. Inst. Hist. Res.* 27 (76):141–55.

1959 Patterns of national development: Introduction. In *1859: Entering an Age of Crisis*, eds. P. Appleman, W. A. Madden, and M. Wolff, pp. 115–30. Bloomington: University of Indiana Press.

1962 With W. P. Metzger and C. Morris. Academic freedom and tenure: The George Washington University. *AAUP Bull.* September, pp. 240–47.

The business interests of the gentry in the parliament of 1841–47. Appendix to G. K. Clark, ed. *The Making of Victorian England*, pp. 290–305. London: Methuen.

1963 Voting patterns in the British House of Commons in the 1840s. *Comp. Stud. Soc. Hist.* 5(2):134–63.

Notes on the problem of historical generalization. In *Generalization in the Writing of History*, ed. Louis Gottschalk, pp. 145–77. Chicago:University of Chicago Press.

1966 Quantification in history. *Am. Hist. Rev.* 71(3):803–25.

Parties and issues in early Victorian England. *J. Brit. Stud.* 5(2):95–114.

1967 The country gentlemen and the repeal of the corn laws. *Engl. Hist. Rev.* 82:47–60.

The conservative and radical interpretations of early Victorian social legislation. *Victorian Stud.* 11 (2):225–36.

1971 *Quantification in History*. Reading, Mass.: Addison-Wesley.

1972 Ed. with A. G. Bogue and R. W. Fogel. *The Dimensions of Quantitative Research in History*. Princeton, N. J.: Princeton University Press.

A data archive for modern British political history. In *The Dimensions of the Past: Materials, Problems, and Opportunities for Quantitative Work in History*, eds. V. R. Lorwin and J. M. Price, pp. 333–59. New Haven: Yale University Press.

The disintegration of the Conservative Party in the 1840s: A study of political attitudes. In *The Dimensions of Quantitative Research in History*, ed. with A. G. Bogue and R. W. Fogel. pp. 319–46. Princeton: Princeton University Press.

1973 Lee Benson's scientific history: For and against. *J. Interdiscip. Hist.* 4(2):263–72.

1977 Ed. *The History of Parliamentary Behavior.* Princeton:Princeton University Press.

Constituency influence on the British House of Commons, 1841–1847. In *The History of Parliamentary Behavior*, ed., pp. 225–56. Princeton, N.J.: Princeton University Press.

1979 Panel Member. *Privacy and Confidentiality as Factors in Survey Research* . Washington, D.C.: National Academy of Sciences. Dissenting opinion, pp. 118–119.

1981 The search for ideas in historical investigation. *Soc. Sci. Hist.* 5(4):371–92.

Photo by Betty Jane Nevis, Berkeley, California

Frank Ambrose Beach

April 13, 1911—June 15, 1988

BY DONALD A. DEWSBURY

FRANK A. BEACH WAS arguably the premier psychobiologist of his generation, influencing the development of psychobiology in numerous, diverse ways. Believing that learned behavior was too complex for detailed analysis, he shifted the focus of the field toward the study of instinctive, or as he preferred, species-specific behavioral patterns, such as mating and parental behavior.

A major impact was Beach's movement of the field toward increased physiological considerations, as in research on the neural and endocrine determinants of behavior. Along with William C. Young, he established the field of behavioral endocrinology. Physiological analysis can quickly become reductionist; in Beach's hands, by contrast, it was integrative. He sought to understand behavior not only with respect to the two-way relationships with neural and endocrine processes but in dynamic relation to the complex environment in which animals live. Further, Beach believed that behavior should be understood in an evolutionary framework. The function of behavior was to permit animals to adapt to complex and ever-changing environments. He sought an integrative psychobiology that would transcend these levels of analysis and focus on behavior, but it would be rooted in the study of its physiological correlates on the one hand and its adaptive function on the other. He thus tried to unify the physiological and comparative factions of psychobiology.

Beach strongly valued experimentation and was a skilled experimenter—a seat-of-the-pants, follow-your-nose kind of experimentation, rather than one based on sophisticated mathematical analysis or elaborate equipment. He was a down-to-earth Midwesterner. Beach's most lasting influence stemmed from his ability to think about the field and to write integrative articles that would synthesize developing trends and shape their evolution toward an improved integrated psychobiology. His sense of timing was exquisite.

PERSONAL HISTORY[1]

Frank Ambrose Beach was born in Emporia, Kansas, on April 13, 1911. His mother was Bertha Robinson Beach; his father, Frank A. Beach, was a professor and head of the Department of Music at Kansas State Teachers College in Emporia. The music building at what is now Emporia State University is named in his honor. Beach rarely used the "Jr." associated with his name.

Beach attended the teacher's college with the goal of becoming an English teacher. He received his B.S. degree in education in 1932. His course was not always smooth. Freshman year grades were so low that Beach's parents sent him away to Antioch College for a year to improve his academic motivation. The strategy worked, and he returned to Emporia for his junior year, becoming a campus leader and taking his first course in experimental psychology. Although its effect was not immediate, this course altered his life's direction. His instructor, James B. Stroud, had earned his doctorate with Harvey A. Carr at the University of Chicago, and Beach later named Stroud as the teacher who had exerted the greatest influence on his professional development.

Unable to find a job teaching English when he graduated at the peak of the Depression, Beach accepted a fellowship from Stroud on the condition that Beach would pursue an M.S. in psychology. Although his work was in clinical psychology, Beach chose as a thesis topic the determination of whether rats had color vision. Because the department had no animal facilities, Beach had to establish and run them himself. He did this successfully and received his master's degree in 1933.

Still unable to find a steady job, Beach took Stroud's advice to explore the possibility of further study in anthropology at the University of Chicago. He traveled to Chicago and found that he was unable to pursue that path because the Department of Anthropology had no stipends for beginning students. However, a courtesy call to Carr led to a fellowship and graduate study in psychology during 1933–34. Between the fellowship and odd jobs, including singing in a choir, Beach was able to study for a year in Chicago. Carr, the noted leader of the Chicago functionalist school of psychology, proved to be an ideal teacher. Beach was not enamored of the mathematical approach of Louis Thurstone, one of Chicago's "star" faculty members. He had a lifelong distrust of complex mathematical operations, believing that just because it was possible to perform statistical operations did not mean one should do so. It was a third faculty member, Karl Lashley, a notoriously poor classroom teacher, who exerted a lasting influence on Beach. Lashley allowed students to work on their own, and Beach found the laboratory environment and the problems of physiological analysis in which Lashley was interested to be irresistible. Outside of psychology, courses with Paul Weiss and C. Judson Herrick also made an impact.

As he experienced financial difficulties and a job teaching high school English finally opened up, Beach took off a year to teach junior and senior English at Yates Center, Kansas. It was an active year, during which he directed plays, supervised the student newspaper, and performed other routine duties of a high school English teacher. He even conducted psychological research on the rate of learning of a stylus maze by his students.

The lure of psychology was great, however, and a university fellowship enabled Beach to return to Chicago after one year of teaching. By now, Lashley had moved to Harvard, but Carr was receptive to a dissertation proposal for a study in Lashley's field—on the effects of lesions to the cerebral cortex on maternal behavior in rats. With Lashley gone, Beach again had to rely on his own skills and on fellow students for help, but he was able to complete his Ph.D. candidacy examination and his dissertation research in one year, only his second at Chicago. Among his important associates at Chicago were David Krech and Leon Pennington. Beach sought employment while he wrote the dissertation and completed his language requirements. Lashley came to the rescue, offering Beach an assistantship in neuropsychology at Harvard.

In March 1936 Beach married Anna Beth Odenweller, a fellow Kansan, who had been studying at the Goodman School of Theater at the Chicago Art Institute and whom Beach met in the choir. They would have two children, Frank A. Beach III born in 1937 and Susan Elizabeth Beach born in 1942.

At Harvard during 1936–37, Beach studied the effects of similar brain lesions on another instinctive behavior in rats, copulation. He would come to spend more time during his career studying copulatory behavior than any other problem. He later called this a halcyon year, with much time available for research but little time for language study. While at Harvard, he formed lifelong friendships with

Donald Hebb, Edwin Ghiselli, George Drew, and Andre Ray.

After a year at Harvard, Lashley "kicked him out of the nest," recommending Beach for a position as an assistant curator in the Department of Experimental Biology of the American Museum of Natural History in New York. The museum position allowed Beach an opportunity—both time and facilities—for "full-time" research. The chance to interact closely with diverse biologists in fields such as mammalogy, herpetology, and ornithology had a broadening effect and helped shape the expansive biological perspective that so characterized his later work. The only problem was in getting along with department chair G. Kingsley Noble.

By 1940 Beach had finally learned sufficient German and French to pass his language exams at Chicago, and he returned to the university for his oral examination. Carr came out of retirement to conduct the examination, and Beach received his Ph.D. degree in 1940.

A colleague at Harvard had suggested that the loss of copulatory behavior that followed the cortical lesions Beach had given to rats might be due to indirect effects of the lesions on the endocrine system. To learn more about endocrinology, Beach audited a course with Robert Gaunt at New York University. When he found that there was little on endocrinological effects on behavior, he began library research for a term paper that he eventually developed into his first book, *Hormones and Behavior,* published in 1948.

When Noble died in December 1940, museum director Roy Chapman Andrews was prepared to shut down the Department of Experimental Biology. Beach, however, lobbied various scientists around the country to intervene, with the result that Beach became the new chairman with the rank of full curator. He changed the name to the Department of Animal Behavior. The department provided a home for numerous very active comparative psychologists for many years. It was during this period, however, that Beach acquired his lifelong aversion to academic administration.

In 1946 Beach left the museum for a position in the Department of Psychology at Yale University. He later claimed that he had done a poor job at that point in his career in teaching lecture classes, but he was allowed to teach mainly smaller seminars and to supervise graduate students. In 1952 Beach was named a Sterling professor of psychology at Yale.

Beach valued science and believed himself responsible for communicating his results to his colleagues, but during most of his career he was not especially active in popularizing science. An exception appeared during the early Yale years with a series of articles that dealt with learned behavioral patterns, which he published in *Natural History* magazine. His rhetorical strategies in these articles focused on the importance of hard-nosed science in understanding even the most complex behavior.

During his tenure at Yale, Beach became progressively more active in the affairs of various scientific organizations. He regarded this as a responsibility of scientists, but as a finite one. He was elected president of the Eastern Psychological Association and was selected as a charter member of the psychobiology panel of the National Science Foundation. In 1955 he became a member of the National Research Council's Committee for the Study of Problems of Sex, and two years later was made chairman. Years later, he would pride himself in having closed down the committee. Federal funding had come to dwarf that of the committee, and Beach believed that committees often search for functions long after their useful purpose has been achieved. He was determined not to let this happen. He also served on the Publications Board and Policy and Planning Board of the American Psychological Association, the Advisory Board of the Marine Studios and Marine Research Laboratory in St. Augustine, Florida, and on the Board of Scientific Directions of the Roscoe B. Jackson Memorial Laboratory in Bar Harbor, Maine.

Beach also was invited to several prestigious lectureships, including the William James Lectures at Harvard, the Smith College Lectures, and the Jake Gimbel Lectures on the Psychology of Sex at the University of California, Berkeley, and Stanford University.

While at Yale, Beach became more interested in human sexual behavior and teamed up with anthropologist Clellan S. Ford to write a book, *Patterns of Sexual Behavior,* which surveyed that field in broad perspective.

Prior to 1948 Beach worked with rats, hamsters, cats, and pigeons, but he then instituted a program of work with dogs that would last much of the rest of his career. Beach valued his Yale years for the opportunity to work with bright students and colleagues under ideal conditions and to experience life in a first-rate university.

Beach spent the 1957–58 year as a fellow at the Center for Advanced Study in the Behavioral Sciences in Stanford, California. His primary endeavor while there was to write a textbook on comparative psychology, but after writing numerous chapters he finally gave up in frustration.

During his stay in Stanford, he was approached regarding the possibility of a permanent move to the University of California, Berkeley. He accepted the position partially because he had come to learn that with each such move he had become acquainted with a new set of colleagues from whom he could learn and enrich his own research program.

He also was attracted to the warmer climate and the easy-going atmosphere. Beach accepted the position on the conditions that he would determine his own teaching assignments, have ample research space, be given a fulltime secretary, and never be asked to serve as department chairman.

Beach continued to flourish in the Berkeley climate. He liked the California graduate students and the fact that the department was not dominated by learning theory as had been Yale in the heyday of Clark Hull. A major accomplishment in Berkeley was Beach's founding of the Field Station for Behavioral Research on a beautiful site overlooking the campus and San Francisco Bay. There Beach could continue his program of research on dogs in a more open environment. The site later became the location of a major program of research on hyenas in which Beach participated on a part-time basis after his retirement.

In 1961 and 1962 Beach hosted conferences under the sponsorships of the Committee for Research in Problems of Sex, the National Science Foundation, and the National Institute of Mental Health. These conferences brought together students of sexual behavior with very diverse perspectives and resulted in the edited volume *Sex and Behavior* in 1965.

On the occasion of his sixty-fifth birthday, his former students and postdoctoral fellows held a working conference in Berkeley in his honor. Beach regarded it as a highlight of his academic life. This conference resulted in another book, *Sex and Behavior: Status and Prospectus.*[2]

Beach was a splendid mentor for graduate students and postdoctoral fellows. Indeed, he provided a role model for many. He knew exactly when to turn a sympathetic ear and when to deliver a swift kick in the pants. His work with students was so strong that he received the American

Psychological Foundation's award for distinguished teaching in biopsychology in 1985. He was cordial, yet maintained distance; graduate students did not call him Frank. Beach thought the roles of father figure and buddy incompatible.

Through much of his career, he limited his teaching to graduate students and small groups. In a remarkable reversal of his earlier career pattern, Beach became motivated for undergraduate teaching late in his career. He taught large undergraduate classes in comparative psychology and introduced an experimental class in human sexuality. The latter effort led him to edit another book, *Human Sexuality in Four Perspectives* (1976). He was not satisfied with his first efforts at these courses, but he felt he improved with experience.

Beach formally retired in 1978, but he remained active in research. His first wife having died in 1971, Beach married Noel Gaustad, who was especially important to him as his health declined. Beach died in Berkeley in 1988 at the age of seventy-seven. In the hospital the week before his death, Beach was still reading literature and working with a co-author on one more article.

Among his honors were honorary doctorates from McGill University, Williams College, and Emporia State University; the Warren Medal of the Society of Experimental Psychologists; and the Distinguished Scientific Contribution Award of the American Psychological Association. He was elected to the National Academy of Sciences (at age thirty-eight), the American Philosophical Society, and the American Academy of Arts and Sciences. The Frank A. Beach Award and Lectureship were established in 1990 as a means of encouraging and rewarding young researchers in behavioral endocrinology. The lectures are published annually in *Hormones and Behavior*.

Frank Beach was an extrovert—at ease with people and able to get along well with almost everyone. He could live hard at times. He knew how to party. He was as comfortable at the poker table as he was in the laboratory. At the same time, he had an exquisite sensitivity for the English language and its use, along with a deep respect for learning of all sorts and for the culture in which he lived. He was possessed of great wit, eager to deflate pomposity, and was not afraid to ask questions others would regard as naive. When he left the room, however, he understood fully what had transpired. Beach knew well how to use his rich sense of humor in the interest of making and dramatizing salient points of a serious academic nature.

Beach was a conservative in many senses of the word. His academic values were old-fashioned, as were his political views. He was proud that he did not let me spend all of the funds in my National Science Foundation stipend; we returned some money to the federal government. This is unusual! He did not support the activism rampant on the Berkeley campus during the 1960s.

PROFESSIONAL HISTORY

Beach's primary contributions lay more in programmatic accomplishments and in directing the field than in any single discovery. Nevertheless, his achievements in some areas can be summarized in approximate topical and chronological order.

EARLY RESEARCH

In his early work, Beach explored the effects of various interventions on instinctive behavior, primarily in rats. He started out with brain lesions and such behavioral patterns as copulation, parental behavior, and activity patterns. Themes that would be prevalent in his work emerged early.

The first study of the effects of hormones on mating behavior and the first developmental study came in 1941; the first study of stimulus control of mating was in 1942.

Beach's first synthetic review dealt with the central nervous mechanisms of reproductive behavior in vertebrates and came in 1942; others would follow. He concluded that both hormones and sensory input were needed to act on neural mechanisms for the display of copulatory behavior. Among his views was the belief that hormones have a more important effect in the lower vertebrates; the sexual activity of higher vertebrates is more dependent on cortical mechanisms. He also believed that the activity of males is less hormone-bound and more dependent on cortex than is that of females. Beach considered the stimulus arousal of behavior to be multi-sensory, including olfactory, tactile, auditory, and visual. None is critical; rather, they sum in the brain to activate a sexual arousal mechanism essential for the initiation of behavior. The arousal mechanism, he thought, is independent of a hypothetical sub-cortical copulatory mechanism that is responsible for the execution of the behavior once an arousal threshold is reached—as a result of hormonal and sensory activity—and the behavior occurs. Because hormones and sensory input from different modalities sum, Beach proposed that activity in one component could compensate for the inactivity elsewhere in the initiation of behavior.

Beach viewed the development of behavior as a complex interaction of genes and environment, but he thought that early play and other early experience were more important in primates than in the lower vertebrates. He argued that once there has been a proper study of the impact of genes and the environment the very necessity of a category of "instinctive behavior" would become unnecessary. These themes of the complex development and control of behavior, which emerged early in his career, were explored throughout his tenure as a leading psychobiologist.

BEHAVIOR

Although much of Beach's work was with laboratory rats and dogs, he published a study of the pouchless marsupial *Marmosa cinera* in 1939 and added hamsters, pigeons, alligators, pigeons, and other species over the years. It is not clear who first labeled Beach "the conscience of comparative psychology," but he was noted for urging a more comparative focus than had most psychologists studying animals, especially in his 1950 article "The Snark Was a Boojum." Beach called on psychologists to expand the range of species they study and the range of behavioral patterns and problem areas they investigate. Beach wrote various articles advocating the study of instinctive behavior, or what was more often termed species-specific behavior.

It was shortly after World War II that the European ethologists, especially later Nobel Prize winners Konrad Lorenz and Nikolaas Tinbergen, began to become truly visible in North America. Beach was a leader in calling attention to this approach and to encouraging productive interaction between North American comparative psychologists and European ethologists. He served on the first editorial board to the ethological journal *Behaviour*.

Beach believed in the careful description of behavior in objective terms that could be understood and used by different investigators. He hesitated to label behavior as sexual or aggressive because so many motor patterns could appear in different functional contexts. The description of what occurred was to be objective and kept separate from the functional interpretation given the behavior by the observer.

During the 1950s and 1960s, Beach and his students conducted a long series of studies in rats of the determinants of the complex pattern of mounts, intromissions, and ejaculations, which are displayed during copulation. Many of these were related to a conceptual model carrying forward the theme of independent sexual arousal and copulatory mechanisms. By manipulating various temporal parameters and stimulus situations, they probed the manner in which these mechanisms worked first to bring the animal to the threshold of sexual activity and then to bring the hypothetical copulatory mechanism to the point of ejaculation. After that, attention was addressed to the problems of the recovery of sexual motivation, both during and between multiple-ejaculation sessions.

As noted, Beach tended early in his career to believe male behavior more complex than female behavior. In a changing cultural climate, he later realized the previously unrecognized complexity of female behavior. In 1976 he wrote the definitive article delineating the differences among receptivity (to the male), attractivity (to the male), and proceptivity (the active solicitation of the male) in female sexual behavior. As always, his careful descriptions and his timing were well tuned.

BEHAVIORAL ENDOCRINOLOGY

Throughout this whole period, Beach's interest in hormone-behavior interactions was growing. His early book *Hormones and Behavior,* along with some writings of W. C. Young, had first crystallized the area. Beach continued to do research and to write synthetic articles outlining and promulgating what came to be known as behavioral endocrinology. But Beach understood that an emerging discipline needs the accouterments. In 1975 he provided an integrative article in *American Scientist*. Six years later he wrote a scholarly history of the field. In 1979 Beach, along with Julian Davidson and Richard Whalen, founded the field's first journal, *Hormones and Behavior*.

One of the important developments in psychobiology during the 1960s was the realization of the important effects of early hormone action on later behavior. Beach with others conducted much research on such early hormone effects. The dominant view was that, whereas the effects of hormones in adults were activational, the effects early in life were in the organization of neural tissue. Beach was skeptical of these conclusions and wrote several articles questioning the evidence. Although the conclusions appear to have been largely correct, Beach's skepticism helped focus research and thinking on the clarification of the concepts and on the development of sufficient evidence to warrant general conclusions.

It is important to remember that for Beach hormone-behavior interactions were a two-way street. Hormones not only affect behavior, they also are affected by behavior. Beach devoted much effort to writing a textbook on behavioral endocrinology, but like the textbook on comparative psychology, it lay incomplete at his death.

In his series of studies of dogs, conducted at the Berkeley field station, Beach could fully explore the hormonal and environmental interactions in the regulation of sexual behavior in dogs. Here he could breed his own animals and study them year round out of doors. In this context he found that individuals have distinct, if somewhat idiosyncratic, preferences for mating partners. When copulating, dogs achieve a lock, or mechanical tie, that makes separation of the male from the female mechanically difficult. Always gifted with the ability to turn a phrase and create a colorful title, he summarized the work in his 1969 article "Locks and Beagles." The work continued for many years, and Beach later embraced with characteristic enthusiasm the research by his successors at the field station on hormonal and developmental factors in hyena behavior.

HUMANS

The topic of human sexuality emerged early in Beach's career. It is worth noting that investigation of this topic required more courage in the climate of the 1940s than it did in the 1990s. Beach returned to questions of human sexuality repeatedly throughout his career. Many psychologists studying nonhuman animals study them in order to understand humans and often to enable social interventions. For Beach, by contrast, an interest in humans was more a matter of intellectual challenge. Humans are complex, and unraveling their behavior presents a special challenge for which all methods and disciplines are important. His 1977 book *Human Sexuality in Four Perspectives* dealt with developmental, sociological, physiological, and evolutionary perspectives. Beach believed that the way to understand humans lay not in generalizing from animals to humans but rather by studying humans the same way as other species and looking for similarities and differences.

He was especially cautious in generalizing across species. There are many descriptions of homosexual and masturbatory activity in nonhuman animals, for example. Beach stressed, however, that the male-male or female-female mounting that can be seen in laboratory rats or monkeys were quite different from the culturally complex processes of gender preferences in humans. Beach cautioned that "surface similitude by itself does not justify theoretical inferences."[3] He stressed the importance of understanding underlying mechanisms and only generalizing across patterns with true functional and causal similarity.

In conclusion, Frank Beach was a firm believer in basic research for the sake of knowledge, with practical application a secondary concern. Science is not technology. He recognized, however, that if one accepts the support of society in these endeavors, one has the responsibility to conduct the work with integrity and to record the results in the public record. Students were taught that experiments are not complete until they are reported for the scientific public. Science is serious—but it should still be fun.

Beach believed in careful and precise behavioral measurement, but the mere gathering of facts through what he called "ant-like industry" was of little value unless integrated into a theoretical concept. His work shows the effects of attention to the forest and the trees. Beach believed that much of the research in journals was not worth doing because it lacked a clear focus. He believed that if research is not worth doing, it is not worth doing well and that the last thing most scientists seem to understand are the fundamental questions with which they are dealing.

Beach was a true believer in the progress of science. He conducted many experiments and developed various theoretical models. He genuinely believed, as should all scientists, that his work was the best effort possible at the time but that it was likely to be surpassed by later research. It was important to him that his successors understand that what he did was reasonable in the context of his time and of what he could have been expected to know when he conducted the research. It did not matter that his work would be superseded; indeed, he encouraged it. As long as his work moved the field in the right direction, the work itself could fade. He wanted to contribute to scientific progress and to help shape the field in a way he felt would maximize long-term scientific understanding.

He succeeded—and he had fun doing so.

I THANK Stephen E. Glickman, Benjamin D. Sachs, and Irving Zucker for comments on an earlier draft of this biographical memoir.

NOTES

1. There are numerous sources on Beach. He wrote three autobiographical chapters: Frank A. Beach. In *A History of Psychology in Autobiography*, vol. 7, ed. G. Lindzey, pp. 31–58. Englewood Cliffs, N.J.: Prentice-Hall, 1974; Confessions of an imposter. In *Pioneers in Neuroendocrinology*, vol. 2, eds. J. Meites, B. T. Donovan, and S. M. McCann, pp. 19–35. New York: Plenum, 1978; Conceptual issues in behavioral endocrinology. In *Autobiographies in Experimental Psychology*, ed. R. Gandelman, pp. 1–17. Hillsdale, N.J.: Erlbaum, 1985.

A number of obituaries are useful: S. E. Glickman and I. Zucker. Frank A. Beach (1911–1988). *Am. Psychol*. 44(1989):1234–35; D. A. Dewsbury. Frank Ambrose Beach: 1911–1988. *Am. J. Psychol*. 102(1989):414–20; B. D. Sachs. In Memoriam: Frank Ambrose Beach. *Psychobiology* 16(1988):312–14. Also useful is a set of recollections by former students and colleagues: In Memoriam: Frank A. Beach (April 13, 1911–June 15, 1988). *Horm. Behav*. 22(1988):419–43.

There is also an especially insightful interview: J. D. Fleming and D. Maxey. The drive of the pure researcher: Pursuit of intellectual orgasm. *Psychol. Today* 8(1975):68–77.

A list of Beach's publications and pre- and postdoctoral students is available in *Sex and Behavior. Status and Prospectus*, eds. T. E. McGill, D. A. Dewsbury, and B. D. Sachs. New York: Plenum, 1978.

A selection of Beach's academic papers can be found in the Archives of the History of American Psychology at the University of Akron (Ohio).

2. T. E. McGill, D. A. Dewsbury, and B. D. Sachs, eds. *Sex and Behavior: Status and Prospectus*. New York: Plenum, 1978.

3. F. A. Beach. Cross-species comparisons and the human heritage. *Arch. Sex. Behav.* (1976):469.

Selected Bibliography

1937 The neural basis of innate behavior. I. Effects of cortical lesions upon the maternal behavior pattern in the rat. *J. Comp. Psychol.* 24:393–436.

1942 Central nervous mechanisms involved in the reproductive behavior of vertebrates. *Psychol. Bull.* 39:200–226.

Analysis of the stimuli adequate to elicit mating behavior in the sexually inexperienced male rat. *J. Comp. Physiol. Psychol*. 33:163–207.

1944 Relative effects of androgen upon the mating behavior of male rats subjected to forebrain injury or castration. *J. Exp. Zool.* 97:249–95.

1945 Current conceptions of play in animals. *Am. Nat*. 79:523–41.

1947 Evolutionary changes in the physiological control of mating behavior in mammals. *Psychol. Rev.* 54:297–315.

1948 *Hormones and Behavior*. New York: Hoeber.

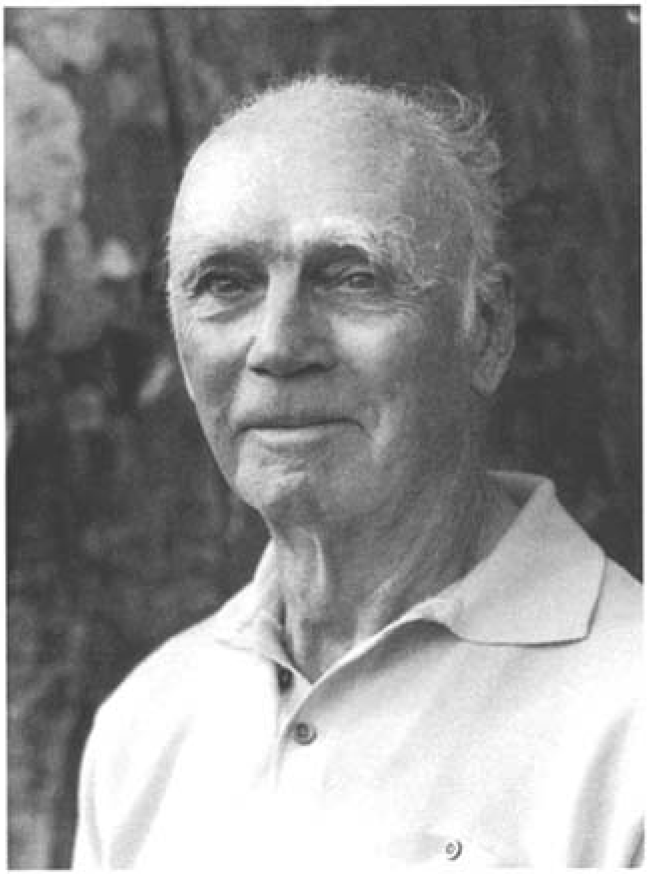
1950 The snark was a boojum. *Am. Psychol*. 5:115–24.

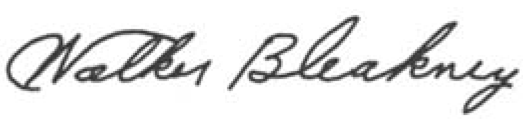
1951 With C. S. Ford. *Patterns of Sexual Behavior*. New York: Harper.

1954 With J. Jaynes. Effects of early experience upon the behavior of animals. *Psychol. Bull.* 51:239–63.

1955 The descent of instinct. *Psychol. Rev.* 62:401–10.
1956 With L. Jordan. Sexual exhaustion and recovery in the male rat. *Q. J. Exp. Psychol.* 8:121–33.
1965 *Sex and Behavior*. New York: Wiley.
1966 The perpetuation and evolution of biological science. *Am. Psychol.* 21:943–49.
1967 Cerebral and hormonal control of reflexive mechanisms involved in copulatory behavior. *Physiol. Rev.* 47:289–316.
With B. J. LeBoeuf. Coital behavior in dogs. I. Preferential mating in the bitch. *Anim. Behav.* 15:546–58.
1969 Locks and beagles. *Am. Psychol.* 24:971–89.
1970 Coital behavior in dogs. VI. Long-term effects of castration on mating in the male. *J. Comp. Physiol. Psychol.* 70:1–32.
1971 Hormonal factors controlling the differentiation, development and display of copulatory behavior in the ramstergig and related species. In *Biopsychology of Development,* eds. L. Aronson and E. Tobach, pp. 249–96. New York: Academic Press.
1975 Hormonal modification of sexually dimorphic behavior. *Psychoneuroendocrinology* 1:3–23.
Behavioral endocrinology: An emerging discipline. *Am. Sci.* 63:178–87.

1976 Sexual attractivity, proceptivity, and receptivity in female mammals. *Horm. Behav.* 7:105–38.
Cross-species comparisons and the human heritage. *Arch. Sexual. Behav.* 5:469–85.
1977 *Human Sexuality in Four Perspectives*. Baltimore: Johns Hopkins University Press.
1981 Historical origins of modern research on hormones and behavior. *Horm. Behav.* 15:325–76.





Photograph by Jacqueline McBride, Santa Barbara, California

Walker Bleakney

February 8, 1901–January 15, 1992

BY GEORGE T. REYNOLDS

WALKER BLEAKNEY, THROUGHOUT a career devoted to experimental physics, has left a legacy of respect and appreciation among his many students and colleagues. He possessed a remarkable combination of intuition and laboratory technique that resulted in significant contributions in several diverse fields. Along with these professional attributes, he had a sensitivity for his students and associates that earned him, respect as a humanist as well as a scientist. These qualities also made him a successful administrator in the Princeton University physics department during a period of transition in the scale of research and teaching activities, as well as the construction of a new physics building.

Bleakney was born in a farmhouse in Armstrong County, Pennsylvania, a few miles from the tiny village of Elderton. His parents were farmers who had left school at the fifth grade, believing that the basic abilities to read, write, and do sums were all the education necessary for their chosen life. When Bleakney was six years old, the parents and their six children moved to a farm near Milton, Oregon, and then to another near Echo, Oregon. These moves were significant in Bleakney's education, since they resulted in his spending three years in the second grade, a situation he did not particularly regret at the time. In his own words, "the work had become rather easy by the third year."

His boyhood experiences did much to develop his self-reliance and mechanical abilities, traits that served him well in his laboratory career. He knew the demands and satisfaction that come with hard physical labor. His determination to secure an education was tolerated by his parents so that he was able to complete high school (as the only boy in a graduating class of four), an accomplishment that required a fourteen-mile round trip on horseback over a ridge "too dry to farm and too high to irrigate."

Determined to continue in college, Bleakney took a year out to earn money.[1] This provided him experience in the fields of Oregon harvesting wheat behind a team of twenty-seven mules. He recounted with pride in later years that during this period his pay was 30% higher than the average field hand's because he had learned to handle wheat sacks so skillfully that he could tie the ear of the sack, roll the seam, put in fifteen stitches, tie the other ear, dethread the needle, and rethread it ready for the next sack in twelve seconds. Since the wheat came out at about three sacks per minute, this left him eight seconds to dump the sack in a straight line for later pick up. This quantitative analysis of his achievement was typical of him. Bleakney never lost respect for people who could do things with their hands, an attitude appreciated by his subsequent graduate students who might not be slated for outstanding careers in theoretical physics. At the end of that year, with about $1,000 saved to start college, he entered Whitman College in Walla Walla, Washington, as a member of the class of 1924. Here he worked his way through odd jobs that included firing furnaces and peeling potatoes at a hotel as pay for his dinner. He was also able to win letters in football and track.

As an undergraduate Bleakney majored in physics. Here he had a remarkable physics teacher, Benjamin H. Brown (the Oersted medalist of the American Association of Physics Teachers in 1939), and unusual classmates. The four physics majors in the class of 1924 were Bleakney, Walter Brattain (co-discoverer of the transistor and Nobel laureate), V. Rojansky (author of an early quantum mechanics text), and E. J. Workman (for more than twenty years president of the New Mexico Institute of Mining and Technology in Socorro).

In the spring of 1924 Bleakney and Rojansky were encouraged to take a competitive examination offered by Harvard University for a $1,000 scholarship for graduate work in engineering. Until this time, Bleakney had assumed that he would go back to farming, but when the results were announced, it turned out that he had won the scholarship (Rojansky was second). A year at Harvard in electrical engineering convinced him that research in physics was more appealing. At the time, the large Midwestern state universities offered the best opportunities for a student to earn his way while engaged in graduate study, and Bleakney found an opportunity to enter the University of Minnesota in 1925. After receiving his Ph.D. in 1930, he went to Princeton University as a National Research Fellow and two years later became an instructor of physics, thus beginning thirty-seven years of service to the department and university. He was named Cyrus Fogg Bracket Professor of Physics in 1953 and Class of 1909 Professor of Physics in 1963. He was a highly respected chairman of the department from 1960 to 1967, which were years of significant transition and changes.

Bleakney's research in physics divides distinctly into two time periods: 1925 to 1940 and 1940 until his retirement in 1969. His graduate work at Minnesota was under the general guidance of Professor John T. Tate. His doctoral dissertation was published in the form of two papers in *Physical Review* in 1929 and 1930 and initiated the period during which he investigated critical potentials and ionization products primarily in gases, as well as isotopic properties and the relative abundance of isotopes. In 1939 the outbreak of war in Europe caused many physicists, including a significant number at Princeton, of which Bleakney was one, to consider the contributions that physicists could make to the Allied cause. A group within the National Academy of Sciences obtained a small fund from the Army Corps of Engineers to form the Committee on Passive Protection against Bombing (CPPAB). By the autumn of 1940 Bleakney was recruiting colleagues and graduate students to participate in the research that ultimately led to his postwar research in fluid dynamics, notably in studying the physics of shock waves.

In the prewar period Bleakney made significant contributions to the field of molecular physics in the area of instrumentation development, as well as by the performance of key experiments focused on studies of critical potentials for the ionization of gases. Although he did not label it as such for several years, the technique he used was the mass spectrograph, an instrument that he was to improve markedly in the mid-thirties. As early as 1916 A. J. Dempster had developed a method of determining the masses of various atoms and molecules, followed by improvements by F. W. Aston, all of which led to more precise measurement of atomic and isotopic masses. Bleakney began a program at Minnesota, developing instruments of increased resolution and precision, which he continued to carry out at Princeton. There his National Research Fellowship was to allow him to work with K. T. Compton, but just as Bleakney arrived Compton left for MIT. As he described it in a later brief autobiographical note to an ex-graduate student, "Compton's leaving left a very big hole in the department … Smyth, Turner, Shenstone, Condon, and Robertson were associate professors, I believe. Of these Harry Smyth was the closest to my interests and became my nominal advisor. But, bless him, he was not a dictator. … Harry was helpful when needed, interested in my work, but made no effort to direct it. This was just great for me. I could freely follow my own interests. I cannot imagine a finer environment that those early years at Princeton."

As a later graduate student of Walker Bleakney (1940–43), I can attest that these same qualities as an advisor made him appreciated and respected by all those fortunate enough to have him as a mentor. Bleakney's program at Princeton throughout the 1930s was directed to improvements in the mass spectrometer in the areas of precision, resolution, and sensitivity. He was successful in all of these efforts and became recognized for his experimental skill. These contributions were not merely for the sake of technique, however; they were the result of his desire to explore important problems in the physics of atoms and molecules. As a result, more than thirty papers were published in journals and sixteen papers were presented at meetings during this period, describing studies of the isotopes of elements ranging from hydrogen to platinum and ionization products of organic molecules. Much of his research during this period concerned hydrogen, and he later remarked that he felt one of his most important contributions was the confirmation of the existence of deuterium, the determination of its abundance, and other chemical properties. His work also provided some of the first reliable evidence for the abundance of tritium in ordinary hydrogen and confirmation that ^{3}H may be unstable. In early 1938 he published a paper describing the design of

"A New Mass Spectrometer with Improved Focusing Properties" and a description of initial results of the application of an early model to the mass spectrum of ethane. His arrangement of crossed electric and magnetic fields resulted in ion paths that were trochoidal when projected in the plane perpendicular to the magnetic field and provided "perfect focusing properties." The paper ended with the optimistic statement: "It is hoped that a much larger apparatus will be completed in the near future." It was to be the superior mass spectrometer of the times. During this period of development, seven graduate students received their Ph.D.'s under his direction, all of them going on to successful careers in physics.

My arrival at Princeton for graduate work was in anticipation of joining in the final stages of completing this larger model and its application to the determination of certain atomic constants. World War II, however, abruptly changed the direction of Bleakney's research interests. With a small fund the National Academy of Sciences set up the CPPAB. H. P. Robertson, a professor of mathematical physics at Princeton, became involved early on and recruited Bleakney to join in as an experimentalist in an effort to determine the fundamentals of armor and concrete penetration by projectiles. A literature search soon showed that there was little or no experimental data on the subject.

Bleakney, anxious to gain some hands-on experience and to have some reliable data, decided to do some model tests on the penetration of bombs into concrete. As one of the first two graduate students recruited, I was able to witness firsthand his ability to "make do" with the meager facilities available. He had been given the magnificent sum of $50 to get started. With this he purchased an old military flintlock gun (he wanted a large bore) to use for firing model bombs into concrete and to study, the penetration of missiles through steel. For the studies in concrete, arrangements were made with the civil engineering department to permit him to mix concrete and have it cured according to his specifications. The sample was in the form of a cube, one foot on a side. He wanted to purchase a wheelbarrow to transport the concrete cube from the engineering building to the physics laboratory, but so much of the $50 had been used in the purchase of the gun (and railroad fare to New York to secure it) that he could afford only a small wagon. The work was classified "confidential," so a camouflage cover was draped over the concrete during transport. For the studies of armor penetration, a double pendulum system was designed and built. (A subsequent patent on the system led Bleakney to say that we had a patent on the conservation of momentum.)

By early 1941 the need for fundamental knowledge in the field of military weapons was clear. Bleakney, together with Smyth, determined to initiate research on terminal ballistics on a larger scale. The newly formed National Defense Research Committee accommodated such an effort and provided a source of funds. As part of Division 2, the Princeton University Station was established with Walker Bleakney as its director. He proceeded to establish a program with personnel suitable for theoretical investigations, relevant laboratory experiments, and full-scale field tests. For this purpose he recruited a team of two architects, two field engineers, several theoretical physicists, an electronics engineer, and a small number of graduate students. Under his direction, blast measurements were made and correlated with damage to a number of buildings constructed for the purpose at Edgewood Arsenal. These measurements, which used full-scale bombs, taken together with small-charge blast measurements made at Princeton, provided confirmation of scaling laws useful in application to strategic bombing. Another major full-scale program was conducted at Camp Gruber, Oklahoma, in which underground blast measurements were correlated with damage to concrete structures similar to the pillboxes that would be encountered during the invasion on the Normandy beaches.

Blast pressures were measured by means of piezoelectric gauges. Under Bleakney's direction, several important results were obtained early on, such as the verification of scaling laws and the first "open-air" demonstration of irregular reflections of blast waves at air-ground interfaces, named "Mach Reflection" by von Neumann, a consultant in the work (von Neumann later used these results to specify the height of detonation of the Hiroshima and Nagasaki bombs). To calibrate these gauges, a brass cylinder was constructed with a diaphragm that supported a pressure differential. When this diaphragm burst, the known pressure difference was recorded by the gauge and a calibration secured.

Bleakney and the mathematical physicist A. H. Taub recognized that the device produced a one-dimensional shock wave. Taub developed an equation that utilized the Rankine-Hugoniot relations describing the velocity and pressures involved in the shock wave that resulted from the bursting diaphragm. Taub suggested to me that his equation be checked quantitatively with the calibrated pressure gauges. I designed a three-inch diameter brass tube for the purpose, and a crude basis for the elegant postwar shock tube research at Princeton was established. Bleakney's direction and participation in this work has been described in detail by R. J. Emrich.[2] As the technique developed, the physics of shock waves was studied in steel tubes of larger and larger dimensions that were fitted with large optical windows, permitting the incorporation of interferometric observations for the analyses. Of the many elegant results of this program, the one that gave Bleakney the most satisfaction was an interferogram showing a compression wave growing steeper and steeper as it progressed down the tube until it formed a shock, thus confirming a sequence that for many years could only be imagined. Bleakney particularly enjoyed recounting experiments in which apparently difficult problems were solved by ingenious use of simple apparatus and an understanding of basic physics.

Bleakney's pioneering work with the shock tube technique led to similar activities in other laboratories in the United States as well as several in other countries. Consistent with his knack for finding simple solutions for difficult problems, his group at Princeton never grew large. As his own administrative responsibilities increased, members of the group left Princeton to engage in related activities elsewhere.

Although focused on shock wave research, Bleakney was interested in physics as a broad field. He was a fellow of the American Physical Society and a member of its council from 1958 to 1963. He was a member of the American Association of Physics Teachers and served on the editorial boards of *Physical Review, Review of Scientific Instruments, Journal of Applied Physics, and Physics of Fluids.* While still an active teacher, experimentalist, and departmental chairman, Bleakney pursued several hobbies, which included golf, bowling, flying, and dachshund breeding. He took several of these activities with him to Santa Barbara when he retired in 1969 and supplemented them with hiking in the mountains of Wyoming. He enjoyed physical activity, refusing to go to a retirement home, which he called a "finishing school." His interest in physics and experimental techniques persisted. In retirement he rigged up an apparatus to take flash photographs of his golf swing and of his club striking the ball in an effort to understand better the correlation between the shape of the ball and the hook. He took great pride in physical activity, accepting the use of a golf cart only reluctantly. A few months before he died, he fell from a ladder while making alterations in a community shop for craft work.

Bleakney had an unfailing sense of humor. His anecdotes of his early life on the farm, with all the misadventures, his early school experiences, and various research endeavors during the war years provided amusement on the several occasions at which he was honored. He was indeed an accomplished storyteller. Students were always welcome at his home, where they were assured of the warm hospitality of his wife Tommie (nee Dorothy Clyde Thomas), who survived him until her death on December 30, 1995.

Walker Bleakney was elected to the National Academy of Sciences in 1959. He was the recipient of the honors listed below. He is also honored in the memories of his students and colleagues as a wise mentor and outstanding physicist. He was appreciated as well for his modesty and sense of humor. One of his many lasting contributions to the Princeton University physics department was his gift of a large, tastefully designed wastebasket (the "W. B. Waste Basket") strategically placed near the departmental mailboxes for the efficient disposal of junk mail. When he retired in 1969, more than 140 friends and colleagues, many of whom were former students, joined to pay him tribute. Many had benefited from his wise guidance and help, which he gave unselfishly to their research efforts. All benefited from having known a man with a zest for life and with outstanding human qualities.

AWARDS AND HONORS

National Research Council Fellow, 1930–32
Citations for World War II research
Honorary D.Sc., Whitman College, 1955
National Academy of Sciences, 1959
American Academy of Arts and Sciences, 1963
Cyrus Fogg Bracket Professor of Physics, Princeton University, 1953
Class of 1909 Professor of Physics, Princeton University, 1963

NOTES

1. W. Bleakney. Reminiscences of my youth in Oregon. *Am. J. Phys.* 40(1972):953–59.
2. R. J. Emrich. Walker Bleakney and the development of the shock tube at Princeton. *Shock Wave* 5 (1996):327–39.

Selected Bibliography

1929 A new method of positive ray analysis and its application to the measurement of the probability and critical potentials for the formation of multiply charged ions in Hg vapor by electron impact. *Phys. Rev.* 34:157–60.

1930 Probability and critical potentials for the formation of multiply charged ions in Hg vapor by electron impact. *Phys. Rev.* 35:139–48.

The ionization of hydrogen by single electron impact. *Phys. Rev.* 35:1180–86.

Ionization potentials and probabilities for the formation of multiply charged ions in helium, neon, and argon. *Phys. Rev.* 36:1303–1308.

1932 Additional evidence for an isotope of hydrogen of mass 2. *Phys. Rev.* 39: 536.

A search for isotopes of hydrogen and helium. *Phys. Rev.* 41:32–38.

1933 With A. J. Gould. The relative abundance of hydrogen isotopes. *Phys. Rev.* 44:265–68.

1934 With D. Rittenberg and H. C. Urey. The equilibrium between the three hydrogens. *J. Chem. Phys.* 2:48–49.

With A. J. Gould and H. S. Taylor. The inter-relations of hydrogen and deuterium molecules. *J. Chem. Phys.* 2:362–73.

With S. H. Manian and H. C. Urey. An investigation of the relative abundance of the oxygen isotopes O^{16}:O^{18} in stone meteorites. *J. Am. Chem. Soc.* 56: 2601–2609.

1936 The mass spectrograph and its uses. *Am. Phys. Teach.* 4:12–23.

With M. B. Sampson and L. N. Ridenour. The isotopes of cobalt and their radioactivity. *Phys. Rev.* 50:382.

With M. B. Sampson. A mass spectrograph study of Ba, Sr, In, Ga, Li, and Na. *Phys. Rev.* 50:456–60.

Isotope analysis with the mass-spectrograph. *Proc. Am. Philos. Soc.* 76:774–76.

1937 With P. T. Smith, W. W. Losier, and L. G. Smith. A high sensitivity mass spectrograph with an automatic recorder. *Rev. Sci. Instrum.* 8:51–55.

The relative abundance of isotopes. *Proc. Am. Philos. Soc.* 77:395–409.

1938 With J. A. Hipple, Jr. A new mass spectrometer with improved focusing properties. *Phys. Rev.* 53:521–29.

1948 With R. G. Stoner. The attenuation of spherical shock waves in air. *J. Appl. Phys.*_19:670–78.

1949 With D. K. Weimer and C. H. Fletcher. Transonic flow in a shock tube. *J. Appl. Phys.* 20:418.

With T. Mariner. A large mass spectrometer employing crossed electric and magnetic fields. *Rev. Sci. Instrum.* 20:297–303.

With A. H. Taub. The interaction of shock waves. *Rev. Mod. Phys.* 21:584–605.

With D. K. Weimer and C. H. Fletcher. The shock tube: a facility for investigations in fluid dynamics . *Rev. Sci. Instrum.* 20:807–15.

1951 With C. H. Fletcher and A. H. Taub. The mach reflection of shock waves at nearly glancing incidence. *Rev. Mod. Phys.* 23: 271–86.

1954 Transient phenomena in supersonic flow. In *Modern Physics for the Engineer,* ed. L. Ridenour. New York: McGraw Hill.

With W. C. Griffith. Shock waves in gases. *Am. J. Phys.* 22:597–612.

Photograph by Frank S. Salisbury

James Frederick Bonner

September 1, 1910—September 13, 1996

By Frank B. Salisbury

BOTH THE SCIENTIFIC and personal lives of James Bonner were highly active, extending over wide ranges of diversity, and so productive that a significant legacy is left for us to contemplate and build upon. His range is indicated by over 500 publications, including 10 books, devoted to roughly three-dozen fields of scientific and philosophical inquiry, not to mention over 300 graduate students, postdoctoral fellows, visiting professors, and others who worked in his laboratory and gained from his penetrating insights and always active mind. Most of these friends and acquaintances would agree that James Bonner's brilliant mind went well beyond the norm for human society.

Here is a preview of his diverse interests. Early on, he studied plant hormones including auxin, B vitamins, and wound hormones. He coauthored a seminal paper in 1938 on the physiology of flowering, studied rubber production over a period of at least forty years, and spent most of his final forty years attempting to understand how chromosomes with their genes and proteins function in the growth and development, not only of plants, but of animals as well. As if this were not enough to keep him occupied, he was an active member of the National Ski Patrol, traveled over much of the world, climbed mountains in the Himalayas, Nepal, and many other places, wrote on the philosophy and future of science, and made hundreds if not thousands of close friends in many parts of the globe.

FAMILY MATTERS

James Bonner was born September 1, 1910, in Ansley, Nebraska. When he was six weeks old, his father, Walter Daniel Bonner, moved the family to Kingston, Ontario, Canada, where he became professor of chemistry at Queen's University. Five years later the family moved to Salt Lake City, Utah, where James's father became head of the chemistry department at the University of Utah. His mother, Grace Gaylord, was also a chemist, as was his paternal grandfather. The Bonner siblings in addition to James were Lyman (b. 1912), Priscilla (b. 1914), David (b. 1916), Robert (b. 1917), Walter (b. 1919), and Francis (b. 1921). All received doctoral degrees; four of them became biochemists, two became physical chemists, and one (Robert) became an applied mathematician and computer specialist.

In Salt Lake City the family lived in a semi-rural environment including, for much of James's early years, a "minifarm/orchard" or "farmlet," as James called it, on the outskirts of Salt Lake City. These surroundings were chosen by the parents so the children could have ample opportunity for physical work in an agricultural setting.

James married Harriet Rees on January 1, 1939. Their daughter Joey was born June 10, 1948, and their son James Jose on May 1, 1950. The marriage was dissolved in 1963, and James married Ingelore Silberbach in 1964. They remained very happily married until Ingelore's death on September 3, 1995. James's death followed a year later on September 13, 1996.

All of James's friends and even his children called him James. When someone referred to "Jim Bonner" it was obvious to all those friends that someone did not know James very well—unless it was one of his mountaineering friends, who often called him "Jim."

STUDENT AND POSTDOCTORAL YEARS

James graduated from high school in 1927, entered the University of Utah, and spent two years as a chemistry major with a minor in mathematics. He found chemistry and math to be both easy and enjoyable—"Fun," he said. After his sophomore year, James's father took a sabbatical year at the California Institute of Technology (Caltech). James and his brother Lyman had tuition scholarships, and James signed up for physical chemistry. He found the first term at Caltech to be most exciting. Everything was fun because they learned by solving problems and never by rote. He thrived on physical chemistry, physics, and quantitative analysis. For the second term, he studied biology under Thomas Hunt Morgan and also became acquainted with such other luminaries as Alfred H. Sturtevant, Calvin B. Bridges, and Henry Borsook. He enjoyed the biology class, but it was "too easy because there were no problems."

Theodosius Dobzhansky was a new assistant professor who came with Morgan from Columbia University. Dobzhansky had arrived from Leningrad in Morgan's laboratory only to find that the laboratory was moving to Caltech within the year. Morgan offhandedly made him assistant professor when his one-year Rockefeller fellowship ran out. As such, he was teaching the biology class, including the laboratory. James was amazed to discover that biologists took field trips, specifically to Corona del Mar with Dobzhansky to trap *Drosophila* (fruit flies) in half-pint milk bottles with yeast suspension on paper towels. Because of the Depression, the tuition scholarship could not be funded, but Dobzhansky made James his research assistant, and, when the family returned to Salt Lake City, James remained to work with the fruit flies. There were more field trips, not only to the beach but also camping in the nearby mountains. For James, the idea that biologists got to take field trips to the great out-of-doors was very seductive! Dobzhansky had arrived at Columbia without speaking any English, yet he was submitting and publishing approximately one paper per month, a practice that he continued for the fifty-five years that he was active in genetics. James had quite a bit of writing experience, particularly under Professor Crabtree at the University of Utah, so he corrected Dobzhansky's language in his early research papers. At the end of the summer, James hitchhiked home to Salt Lake City to finish his bachelor's degree in chemistry and mathematics at the University of Utah in 1931.

The Depression was deepening, but James returned to Caltech on a $750-per-year teaching assistantship in the Division of Biology. He had also been accepted in the Division of Chemistry. Upon arriving, he went to the Kerckhoff Laboratories, where the biology division was located, and found them deserted. He rode a borrowed bicycle out to the Caltech farm, and there was E. G. Anderson hoeing weeds out of his corn—the maize that made Anderson a famous geneticist. He met George Wells Beadle, who later received the Nobel Prize (with Edward Tatum) for the concept of "one gene, one enzyme." Bonner and Beadle became lifelong friends.

Back at Caltech, they were building the Dolk Greenhouse, and there James met Herman E. Dolk, a plant physiologist, and Kenneth V. Thimann, who was instructor of biochemistry, later to become a famous plant physiologist. James learned from these young scientists that Fritz W. Went in the Netherlands had discovered a plant growth hormone, which he had named auxin. After a stint in Java, a common interval for young Dutch botanists, Went came to Caltech in 1933.

Thanks to Morgan and Dobzhansky, James had set out to become a geneticist, but Dolk and Thimann put him to work on production of auxin by a fungus *(Rhizopus)*. He was very successful, wrote a paper, saw his name in print, and was "hooked" as he said. He determined that he would try to write as many publications as Dobzhansky did!

During that summer and for three years until he finished his Ph.D., he played the flute in the Pasadena City Symphony. He practiced for an hour each day with the windows of his graduate-student office in the Kerckhoff Laboratories open, and the music wafted out across the campus.

James bought a used Pasadena police motorcycle (Henderson four cylinder). He found a friend with one just like it, and for a while they rode together. One day he learned that his friend had been hit while standing still. He visited the friend, who never fully recovered, and immediately traded his motorcycle for a 1924 Chevrolet Superior roadster.

He graduated with a Ph.D. in biology in 1934. Sturtevant was his thesis chairman because Morgan was off collecting his Nobel Prize. James's father came to graduation. Although Sturtevant was the thesis chairman, James actually worked with Dolk and Thimann. Dolk was killed in an automobile accident in 1933 and was replaced by Went, with whom James also studied for his doctoral research.

In the midst of the Depression, James received a fellowship to support a postdoctoral year in Europe. Morgan had arranged for him to work in Utrecht, but when he arrived in the summer, the laboratories were almost deserted. He toured Europe by train and by bicycle, finding that his German worked well, and visiting various famous professors with whom he had corresponded. He visited Berlin, Jena, Leipzig, Dresden, Prague, Munich, Heidelberg, Innsbruck, Zürich, Bern, Basel, Cologne, and finally back to Utrecht. There he worked with Professor Kruyt, the most famous colloid chemist of the time. Biologists thought that colloid chemistry would provide insight into the functioning of protoplasm. Although protoplasm is indeed colloidal in nature, colloid chemistry provided little insight into protoplasm function. Nevertheless, James learned to speak Dutch and also worked in the laboratory where Went had discovered auxin, working under Went's father F. A. F. C. Went. Fritz Kögl, with his assistant Hanni Erxleben, had recently arrived to be head of organic chemistry with A. J. Haagen-Smit as chief assistant in botany. While James was there, that team isolated indoleacetic acid (IAA) from urine and showed that it had auxin activity. They called it heteroauxin, and, although it was one of the most important discoveries in plant physiology, James heard about it only after he had returned to Caltech.

Kögl, Haagen-Smit, and Erxleben were studying two other "auxins" that they had isolated from urine and corn oil. They called these auxin a and auxin b. They eventually published molecular structures for these compounds—structures that were like nothing known then or since. Although textbooks included the structures until the early 1950s, no one was ever able to isolate the compounds again or even to confirm any part of the proposed structures. James Bonner and Samuel Wildman became convinced in the late 1940s that the compounds as described had never existed. Indeed, they concluded that Erxleben, who was carrying out the actual experiments and reporting the data to Kögl, had somewhere gone astray and continued to report faulty or even fraudulent data to Kögl, who, using the data, developed the unlikely structures. To add to the romance of the story, Erxleben proved to be a German spy located in the Netherlands during those years before World War II. In 1966, J. A. Vliegenthart, who by then occupied Kögel's chair, found samples of auxins a and b in a locked cabinet and analyzed them by mass spectrometry. Auxin a turned out to be cholic acid, for example, confirming that the compounds as originally claimed had never existed.

While in Utrecht James also worked in the Department of Biochemistry of Leiden Medical School under Professor Bungenberg de Jong, who was an expert in coacervates (one colloid suspended in another). This work was not very fruitful although James studied pectins (the "glue" between cell walls of adjacent plant cells), which continued until as late as 1960.

In early 1935 James moved to Zürich, where he worked in A. Frey-Wyssling's laboratory writing an important paper as noted below. Frey-Wyssling was for many years the world authority on cellulose and cell walls. In the autumn of 1935 James attended a botanical congress in Amsterdam, where he made a lifelong friend of Hiroshi Tamiya, who at that time, in common with most of his Japanese colleagues, wrote papers in German. He and James conversed in German, but Tamiya later became highly adept at writing English and wrote most of his important papers in that language. James came home on the *Bremen* in five days near the end of 1935.

PROFESSIONAL CAREER

Morgan offered James a position as research fellow at Caltech, which he began in late 1935, being advanced to instructor (1936—37), then assistant professor (1937—43), associate professor (1943—46), professor (1946—81), and finally professor emeritus (1981—96). After retirement, he set up a company called Phytogen (a California corporation), of which he was chairman, chief executive officer, and chief scientist. The company was eventually subjected to a takeover.

He was a member of at least twelve national and international societies: the National Academy of Sciences (1950), German Academy of Sciences (1970), American Association for the Advancement of Science (fellow), American Society of Plant Physiologists (president, 1948—49), and the Botanical Society of America (chairman, Physiological Section, 1949—50). In 1949 he was elected to the American Alpine Club, and as noted above he was for many years a member of the National Ski Patrol System. He was also active in the Sierra Club.

James's incredibly varied and productive research career produced 108 graduate students (1939—88) and approximately 200 postdoctoral fellows, visiting professors, and others. His 500-plus publications (including 10 books) outline his research interests. Beginning with his undergraduate study at Caltech and continuing until at least 1961, he studied various topics, including plant growth substances and related matters (47 publications), pectins and cell-wall characteristics (17 publications), wound hormones (6 publications), various B vitamins as root growth substances and effects of the vitamins when added to whole plants (31 publications), embryo culture and tissue culture (4 publications), rooting of cuttings (8 publications), and the physiology of flowering (18 publications). During World War II and continuing until 1983, James studied the environmental effects and biochemistry of rubber production (16 publications).

Other topics to which James contributed include allelochemics (3 publications), plant nutrition, especially of camellia (16 publications), miscellaneous biochemical studies (12 publications), plant respiration (9 publications), the proteins of green leaves (7 publications), crassulacean acid metabolism (CAM) in succulents (2 publications), various plant responses to environment, which James called phytronics (5 publications), sterol metabolism (10 publications), and uptake of solutes and water (8 publications).

Beginning about 1956 (at the urging of a former graduate student and by then postdoctoral fellow, Paul O. P. Ts'o), but with a significant acceleration in 1961, James became interested in protein synthesis, microsomal/chromosomal proteins, histones and chromatin (including non-histone chromosomal proteins) and molecular biology in general (including 3 papers on the molecular biology of memory!), nucleic acids, and the genome. This work encompassed about 190 publications with about 112 coauthors. James made numerous contributions in this area, far too many to discuss in limited space.

In addition to the topics listed above that seem to bear at least some relationship to each other, there were studies in physiological ecology, the biology of plant growth and cell chemistry, many exotic travel logs, examination of a lunar sample (2 papers), the geochemistry of biomolecules, and carcinogens. James was highly interested in the functioning and infrastructure of science (14 papers) and speculations about the future of biology (30 papers). There were at least five editorials and autobiographical and biographical papers.

Obviously, space won't allow discussion of all those fields. I have somewhat arbitrarily chosen the following for some discussion: growth substances including studies of the cell wall, vitamins as root-growth hormones, photoperiod and the physiology of flowering, rubber, the "new plant biochemistry" (including plant respiration, mitochondria, protein of green leaves, etc.), and chromosomes and related topics (histones, non-histone chromosomal proteins, etc.).

Growth Substances Including Studies of the Cell Wall

While the geneticists were on vacation when James returned to Caltech as a graduate student, Dolk and Thimann convinced him that he should study *Rhizopus suinus.* They were using the fungus as a source of auxin to apply to plants to study auxin effects. James discovered that the addition of bactopeptone made the fungus produce 100 to 200 times as much auxin than without the bactopeptone, especially when the fungal culture was aerated. This had the potential to become a breakthrough discovery. We now know that the auxin was indoleacetic acid (IAA) and that it is produced in the plant from tryptophan, a component of the bactopeptone. James and the others could have discovered the nature of auxin many years before it was actually established. But a connection between auxin and tryptophan was not evident to them; instead, the work was hailed only as a great discovery of how to produce more auxin to apply to plants!

As a graduate student, James also developed the section-growth test for auxin in which sections of oat *(Avena)* coleoptiles are floated on auxin solutions; in response to auxin they can double in length within 24 hours. This test has been widely used. Although it is somewhat less sensitive than some other tests, it is much simpler and quite suitable for many studies. Using this system, James measured growth of the sections in solutions with different pH values. He discovered that, even in the absence of auxin, the sections grew in acid solutions much more than in neutral solutions. This discovery of "acid growth" was not pursued at the time, but it generated much interest in the early 1970s. A leader in the study of acid growth was Robert Cleland, one of James's former graduate students (and my office mate at Caltech).

While in Zürich working with Frey-Wyssling, James used the polarizing microscope to study cell-wall properties. His manuscript, proudly written in German, showed that auxin made the cell-wall microfibrils slide past each other more easily. In his autobiography, he referred to this as "a considerable contribution." Indeed it was. James was a coauthor of ten subsequent papers related to cell-wall stretching and cell growth, and numerous other investigators have pursued this initial observation of "wall loosening." When James returned to Caltech, he studied effects of auxin on root growth with Johannes van Overbeek and J. B. Koepfli. Much was learned about auxin during the late 1930s.

In response to advice from Frits Went, James studied the wound hormones that had been proposed many years before by Haberland in Austria. String beans can be cut lengthwise, the seeds removed, and a drop of juice from ground-up pods added to the exposed inner part of the pod, causing cell division. With his first postdoctoral fellow, James English, Jr., he isolated the active substance, which he and English called traumatic acid. This proved to be 1-decene-1,10-dicarboxylic acid, a previously unknown substance that was active in plants. This substance also worked on potato slices. Forty years after the work by English and Bonner, Zimmerman and Coudron showed that it was apparently the product of a non-enzymatic oxidation of 12-oxo-trans-10-dodecenoic acid, the first compound in the jasmonic acid pathway, a pathway that is now well known, widely studied, and involved in plant growth regulation.

In the early 1950s Carlos Miller and Folke Skoog, as well as F. C. Steward, discovered compounds that cause cell division. These are now known as cytokinins, and they are well accepted as plant hormones. The wound hormone has the same effects and might be considered in the same class, but this is seldom done. James's work with the wound hormone has been almost forgotten.

Vitamins as Root-Growth Hormones

Phillip White had grown tomato roots through repeated transfers by adding yeast extract to a medium that contained the essential mineral nutrients and sucrose as an energy source. James set out to find what it was in the yeast extract that allowed the growth of the excised tomato roots. He obtained some vitamin B_1 (thiamine), which had just been synthesized, and it made the pea roots grow nicely, although growth slowed after six to eight transfers. James was ecstatic about his discovery and wrote to Phillip White to "tell him the joyous news." White never answered, but he published similar experiments quickly in the *Proceedings of the National Academy of Sciences*. James's paper was written first, but it appeared only later in *Science,* with a longer paper in the *American Journal of Botany*. James's conclusion: "Be careful how you spread the joyous news."

There was much more work on roots from various species as influenced by known vitamins. Fred Addicot was James's first graduate student, and the two of them discovered that many roots required thiamine and also niacin. Tomato roots require thiamine and pyridoxine for unlimited growth. They further showed that the B vitamins were synthesized in the leaves and transported to the roots where they apparently make root growth possible. The papers James published, and there were at least thirty-one of them, pointed out that these vitamins ideally met the definition of plant hormones—organic substances that are synthesized in one location, transported to another in the plant, where at millimolar concentrations they cause some noticeable growth or metabolic response. Although this was expounded in the 1952 textbook *Principles of Plant Physiology,* which

James coauthored with Arthur Galston, the idea never caught on among plant physiologists. Apparently it is difficult for many of us to think of vitamins, often known to act as coenzymes, as also being hormones. The study of B vitamins as plant hormones took most of James's time up to the beginning of World War II.

Photoperiodism and the Physiology of Flowering

James received a letter from E. J. Kraus, chairman of the botany department at the University of Chicago, inviting him to spend the summer of 1938 working with Karl Hamner on photoperiodism. Kraus had worked on the physiology of flowering, specifically on the theory that the carbohydrate/ nitrogen ratio controlled flowering, and Hamner with graduate student Edith Neidle was studying the effects of nitrogen on flowering of *Xanthium pensylvanicum* (now *X. stumarium,* the cocklebur), a short-day plant that flowered when days were shorter than about sixteen hours. Plants were maintained in a vegetative condition by keeping them under artificial lights so the day length exceeded sixteen hours. One day Neidle discovered that all the plants in the greenhouse were flowering. Although the greenhouse managers were at first reluctant to admit it, they finally reported that the power had been off for one night so the plants had received a short day. Hamner and Neidle then found that *Xanthium* would indeed flower in response to a single short day.[1] (Nitrogen, however, slightly promoted flowering rather than inhibiting it as the carbohydrate/ nitrogen theory predicted.)

Actually, Kraus wanted James to spend the summer in Chicago and then probably join the University of Chicago botany department. James asked R. A. Millikan, who was de facto president of Caltech and who had come from Chicago, if this sounded like a good idea. Millikan said to go but not to accept any money because James wouldn't like it and would be back! That was how it worked out.

James and Karl Hamner did much during that brief summer and made at least one discovery that changed the direction of research in photoperiodism from that time on. They asked the question that seems obvious now, but it had not been asked since the discovery of photoperiodism in 1920 by Garner and Allard (and before that by Julien Tournois in France, who published his findings in an obscure paper): In the day-length response, which is more important, the day or the night? Hamner and Bonner exposed plants to various day lengths with a constant night or to various nights with a constant day. The length of the day did not seem to be very important; to produce flowering, the night had to exceed about 8.5 hours almost independent of the day length. They suggested that we should speak of long-night plants rather than short-day plants.

In the most important series of experiments, Hamner and Bonner interrupted the day with brief intervals of darkness (which had no obvious effect) and the night with brief intervals of light, which completely suppressed the flowering response. The inductive dark period had to be long enough and without light interruption if the cocklebur plants were to flower. This was the discovery of the night-break phenomenon, which was extensively studied for many decades, up to and including the present. (Thanks to James, I studied the phenomenon for almost three decades.) One of the most important discoveries in plant physiology came a few years later at the U.S. Department of Agriculture laboratories in Beltsville, Maryland, when the pigment phytochrome was discovered. Harry Borthwick and Sterling Hendricks used lettuce seed germination and the night-break phenomenon in *Xanthium* to make this discovery. Phytochrome accounts for literally dozens of plant responses to light.

There were numerous other studies during that summer of 1938. For example, with grafting experiments, they studied the nature of the flowering hormone that had been postulated by Mikhail Chailakhyan in Russia, and many attempts were made to extract and isolate the hormone—a goal that has yet to be achieved in a satisfactory fashion. There were experiments to show which part of the plant was sensitive to day length (the leaf). The facilities at the University of Chicago left nothing to be desired, and there was a platoon of student assistants to help with the experiments. The paper reporting the results was published in December 1938, in the *Botanical Gazette,* a University of Chicago publication edited by Kraus. James wrote in his *Annual Review* autobiography, "I have no hesitation in describing this paper as a minor classic."

James's interest in photoperiodism continued until the early 1960s. Graduate student John Thurlow worked with James to discover an inhibitory effect of auxin in flowering of *Xanthium*. James Liverman followed up on this work and with James wrote an important review of flowering in the early 1950s. With James, Jan Zeevart and I independently studied nucleic acid synthesis as a part of the flowering process in *Xanthium* in the late 1950s and early 1960s, and Erich Heftmann working with James and Zeevart implicated sterol metabolism in *Xanthium* flowering.

As a graduate student, I worked on flowering of cocklebur with James for two years (1952—54). My story provides some insight into James's scientific personality and philosophy. Liverman and Bonner, based on the very recent discovery of phytochrome, had developed a theory about the interaction of auxin and phytochrome in *Xanthium* flowering. They called this the photocycle and were excited about its possible implications. I set out to test the hypothesis with *Xanthium.* Liverman was a postdoctoral fellow by then, testing the hypothesis with the *Avena* section-growth test. With Liverman's help, I developed a quantitative method to measure the flowering of *Xanthium* based on stages of floral development. Virtually every experiment that I did, however, seemed to refute the photocycle, but it was almost impossible to talk to James about my findings, because he was preoccupied with other studies during the first six months of my sojourn at Caltech. When he finally became available for consultation, I spent many hours on numerous occasions presenting my data on the little blackboard in his office. I would finish the explanation of the most recent series of experiments by saying, "See, that certainly disproves the photocycle." James would look at the data for a few moments, contemplate them, and reply, "Yes, but …." He would invariably have an alternative explanation for my data. It was in sessions such as these that I realized I was dealing with a truly brilliant mind! The scientific tension between him and me continued for the next eighteen months of my graduate study. Finally, I had set up a typewriter and even a bed in the headhouse of the Dolk Greenhouse, where I was staying while I wrote my dissertation. I wrote the literature review, description of methods, and lengthy presentation of the approximately 125 experiments that I had been able to complete. Finally, it came time to write the discussion chapter. How could I come to conclusions diametrically opposed to the theory that my major professor had published and described in symposium talks during the previous two years? James came to the Dolk Greenhouse and spent a full day and a half reviewing my results, experiment by experiment. At the close of this intense study, James said, "You're right!" He never again defended the photocycle, and indeed within a few weeks he was at another symposium describing my results with no mention of the photocycle. Some scientists develop a hypothesis and defend it to their deaths without flinching and in spite of any contrary evidence that might appear. James was not cast in that mold. For him, only the truth mattered.

Rubber

All natural rubber comes from Southeast Asia. Guayule (*Parthenium argentatum*) is the one plant in the western world that has been a serious rubber producer. It is native to northern Mexico and part of Texas, and it was producing small amounts of commercial rubber at the beginning of World War II. James and Frits Went decided to become specialists "by making ourselves master of all guayule knowledge, learning about how it was grown and what one could do to improve it." Frits arranged a meeting with officials of the Intercontinental Rubber Company (IRC), which was the only large company involved in production of rubber from guayule. The meeting was in Salinas, California, where IRC had large guayule plantations. Unfortunately, the shrubs had been planted on a ten-year rotation, not exactly a quick way to get quantities of rubber for the needs of World War II. Nevertheless, the plants produced 8—10% of their dry mass as rubber suspended in a milky latex. James and Frits studied the nutrient requirements of guayule plants and how to kill the various pests that reduced the yields. James was appointed a special agent of the U.S. Forest Service assigned to the Emergency Rubber Program. He showed that the production of rubber is controlled by night temperature; below 10°C (50°F) rubber is produced. Much better production occurs below 7°C. This finding was ignored in spite of several publications, although later work by others specified the enzyme that was induced by the low temperatures.

James eventually entered into a long-term association with the Rubber Research Institute of Malaysia to which he was an advisor from 1965 to 1975. He was made a member of the Malaysian Rubber Research and Development Board, and in 1975 he became chairman of the Agricultural Science and Biology Subcommittee of this board, a position that he occupied until shortly before his death. In this capacity, James developed a technique for adding ethylene (ethaphon) to the bark of the rubber trees, which increased latex production and essentially doubled world rubber production!

The "New Plant Biochemistry"

After the end of the Second World War, Samuel G. Wildman arrived as a postdoctoral fellow. With Sam, James made a new start with what would today be called cell biology, the isolation of "chloroplasts, mitochondria, cytoplasm, and lots of enzymes!" They ground spinach leaves in a colloid mill, centrifuged the product at $20{,}000g_n$ and found that the supernatant contained the soluble leaf proteins. Furthermore, over half of the soluble leaf proteins consisted of a single component of molecular mass ca. 500,000, which they called fraction I. Sam found this fraction in many other leaves besides spinach. It was later shown by John Littleton of Palmerston North, New Zealand, a former postdoctoral fellow of James, that fraction I was the main protein in the stroma or fluid part of chloroplasts. He, Paul Ts'o, and others went on to show that fraction I is ribulose-1,5-bisphosphate carboxylase/oxidase, which is now often referred to as rubisco. Rubisco is the enzyme that fixes CO_2 in photosynthesis. It is the most abundant protein in the world, and Sam Wildman continued to study it until he retired.

James with Adele Millerd, a postdoctoral fellow from Australia, and others showed that plant mitochondria were much like those of animals. They could carry out the Krebs cycle as well as oxidative phosphorylation, including ATP production. Their 1951 paper brought the level of understanding of this aspect of plants up to that of animals.

Chromosomes and How They Work

Paul O.P. Ts'o, who took his Ph.D. with James and by 1956 was a postdoctoral fellow, convinced James that they should study the most fundamental problem of biology—how chromosomes control cellular metabolism. Much was already known following the discovery of the DNA double helix in 1953; James thought that it might be too late to begin such work. Nevertheless, studies were devoted to protein synthesis and other topics related to what was to become molecular biology. In early 1960, Ru-Chih C. Huang came from the laboratory of Joseph Varner to work with James as a postdoctoral fellow. The goal was to isolate chromatin (the genetic DNA combined with protein and constituting chromosomes) from the nuclei of pea epicotyls (young stems above the cotyledons or "seed leaves" in young pea seedlings). Ru-Chih Huang quickly found that crude nuclear extract would indeed incorporate the <sc>14</sc>C-labeled nucleoside triphosphates into something that was insoluble in TCA (trichloroacetic acid). She purified the enzyme activity and found that it caused incorporation of all four riboside triphosphates into something that RNAase would degrade, which could only be RNA. Furthermore, this synthesis of RNA depended on the presence of DNA in the reaction mixture. This discovery with Huang was extremely important, and three other groups published similar results near the end of 1960.

James and Huang found that RNA transcription worked much better if the DNA was stripped of its histone proteins. Suddenly, it became important to learn all that could be known about histones. Paul Ts'o encouraged James to arrange a conference on histone biology and chemistry. They obtained money from the Office of Naval Research, the National Science Foundation, and private donors and invited "everyone in the world (56 people) who knew anything about histones." The result of this conference (published as a book) was the realization that knowledge about histones was in a state of complete confusion. Various workers had estimated that the number of histones varied from a dozen to thousands; no pure histones had been prepared up to that time, and nothing was known about the relation of different histones in different species.

Douglas Fambrough, a new graduate student, was sent to Stanford University for a month to learn from Kenneth Murray how to isolate histones using amberlite CG-50 chromatography and polyacrylamide gel electrophoresis to monitor the purity of individual histone fractions. With these techniques they could purify individual histones. They found that there were only five different species of histones (each with its subspecies), and they compared the histones of pea plants and calf thymus. They found that the histones III from these two organisms had similar amino-acid compositions except for one cysteine amino acid in pea compared with two cysteines in the calf histone. These cysteines reacted in such a manner that it was possible to form "a great variety of multimers upon oxidation of solutions just by sitting in the refrigerator." This had been a source of confusion in study of the histones. About this time a postdoctoral fellow, Keije Marushige, published a paper showing basically that what can be done with pea histones can also be done with rat liver histones.

James discussed these matters with Emil Smith who was the chairman of the Department of Biological Chemistry at the University of California, Los Angeles, Medical School. Clearly, it would be interesting to compare histone amino-acid sequences from peas and calf thymus. They decided to begin with histone IV, which is the smallest of the histone molecules and therefore the easiest to separate from the others. Smith said that he needed two grams of pure histone IV for the analysis. They obtained the calf histone rather easily by collecting thymuses from slaughter houses, but it took 24 tons of dried pea seeds, germinated in barrels, with manual separation of shoots from roots and cotyledons, to obtain the 2 grams of pure pea histone. The effort took a full year. Nevertheless, the sequencing showed that the histones IV of peas and cows were essentially identical. There were only two conservative amino acid replacements between the two species. This was an amazing finding, suggesting that the sequence of amino acids in histone IV is so essential that it has been conserved from the time when animals diverged from plants, possibly close to a billion years ago.

Bonner's lab continued to sequence histones for a while, but because many others started doing it, James concluded that it had become "a growth industry … one of those things that's best to turn over to others." As part of this work, the stoichiometry of the five species of histone molecules emerged. There is one molecule of histone I for two each of the other four histones, making a total of nine molecules in the group. Interestingly enough, in spite of this provocative work with the histones in the early 1960s, it is only in a current spate[2] of papers that molecular mechanisms controlling gene regulation via histone acetyl-transferase have been identified. Bonner's lab carried out much additional exciting work in this field, too detailed for discussion here. Characteristically of much "progress" in science, the foundation work of the 1960s is now lost in the dimness of the distant past.

James's Philosophy: Far-flung Pastures

In his autobiography for the *Annual Reviews of Plant Physiology and Plant Molecular Biology,* James notes that his most important contribution is probably the graduate students, postdoctoral fellows, visiting professors, and others who have worked in his laboratory. He further says, "Some will no doubt complain that it is more profitable for the serious scientist to stick to his problem and flog it to death. To them I say, for myself, browsing in far-flung pastures is more fun. Dark CO_2 fixation by succulents, chemical plant ecology, the path of carbon from CO_2 to rubber, plant taxonomy [!], and treatment of plant-chemical interaction by enzyme kinetics are all matters that I have also touched, and they have all been fun."

James's final word in that biographical review is typical Bonner, including the double explanation points. "Finally, I spoke earlier about the world that awaits exploration. I have studied it pretty thoroughly. It's all wonderful. From Katmandu to Timbuktu to Kota Kinabulu and beyond. Do not miss it!!"

IN ADDITION TO MY personal acquaintance with James Bonner, I learned many details of his life from five autobiographical sources,[3] his amazing list of publications, and memories supplied by acquaintances, including his son Jose Bonner, Paul O. P. Ts'o, Robert (Bob) Cleland, and Ru-Chih C. Huang. I am most grateful for this help. In addition to these sources, I taped an interview (29 single spaced pages) with James on November 22, 1991. Ray Owen and Stephanie Canada, longtime associates at Caltech, worked with me during the preparation of the manuscript.

NOTES

1. Either Hamner or Bonner told me that Hamner had given a lecture telling how he had recently discovered that the cocklebur was an unusually sensitive short-day plant. James heard this story

and was excited about the possibilities. He asked Hamner to see if they could spend the summer together, and Hamner arranged this with Kraus. In my 1991 interview, James had forgotten about hearing the talk but agreed that it probably had happened that way.

2. See, for example *Nature* 387:43, 49 (May 1, 1997) and the five papers from *Cell* that are cited in the nature paper.

3. Chapters from my life. In *The Annual Reviews of Plant Physiology and Plant Molecular Biology* 45:1–23, 1994. My life as a chromosomologist. In *The Molecular Biology of the Mammalian Genetic Apparatus,* ed. P. O. P. Ts'o, 2(1977):317–26, and The life and, times of James Bonner, pp. ix–xii, in the same volume. The beginnings of an autobiography sent to the National Academy of Sciences and the Caltech archives. A manuscript dictated to longtime Caltech secretary Stephanie Canada.

Selected Bibliography

1932 The production of growth substances by *Rhizopus suinus*. *Biol. Zentralbl.* 52:565–82.

1934 The relation of hydrogen ions to the growth rate of the *Avena* coleoptile. *Protoplasma* 21:406–23.

1935 Zum Mechanismus der Zellstreckung auf Grund der Micellanlehre. *Jahrb. Wiss. Bot.* 83:376–412.

1937 With J. English, Jr. Purification of traumatin, a plant wound hormone. *Science* 86:352–53.

Vitamin B_1, a growth factor for higher plants. *Science* 85:183–84.

1938 The hormones and vitamins of plant growth. *Sci. Mon.* XLVII:439–48.

With K. C. Hamner. Photoperiodism in relation to hormones as factors in floral initiation and development. *Bot. Gaz.* 100:388–431.

1943 Effects of temperature on rubber accumulation by the guayule plant. *Bot. Gaz.* 105:233–43.

1946 Further investigations of toxic substances which arise from guayule plants: Relation of toxic substances to the growth of guayule in soil. *Bot. Gaz.* 107:343–51.

1951 With A. Millerd, B. Axelrod, and R. Bandurski. Oxidative and phosphorylative activity of plant mitochondria. *Proc. Natl. Acad. Sci. U. S. A.* 37:855–62.

1952 With R. J. Foster and D. H. McRae. Auxin-induced growth inhibition a natural consequence of two-point attachment. *Proc. Natl. Acad. Sci. U. S. A.* 38:1014–22.

With A. W. Galston. *Principles of Plant Physiology*. San Francisco: W.H. Freeman.

1960 With R.-C. C. Huang and N. Maheshwari. Enzymatic synthesis of RNA. *Biochem. Biophys. Res. Commun.* 3:689–94.

1961 With R.-C. C. Huang and N. Maheshwari. The physical state of newly synthesized RNA. *Proc. Natl. Acad. Sci. U. S. A.* 47:1548–54.

1962 The upper limit of crop yield. *Science* 137:11–15. With R.-C. C. Huang. Histone, a suppressor of chromosomal RNA synthesis. *Proc. Natl. Acad. Sci. U. S. A.* 48:1216–33.

1963 With R.-C. C. Huang. Properties of chromosomal nucleohistone. *J. Mol. Biol* 6:169–74.

1964 With R.-C. C. Huang and K. Murray. Physical and biological properties of soluble nucleohistones. *J. Mol. Biol.* 8:54–64.

1966 With K. Marushige. Template properties of liver chromatin. *J. Mol. Biol.* 15:160–74.

With D. Fambrough. On the similarity of plant and animal histones. *Biochemistry* 5:2563–70.

1968 With others. Isolation and characterization of chromosomal nucleoproteins. *Methods Enzymol.* 12:3–65.

With others. The biology of isolated chromatin. *Science* 159:47–56.

1969 With others. Calf and pea histone IV. II. The complete amino acid sequence of calf thymus histone IV: Presence of ε-N-acetyllysine. *J. Biol. Chem.* 244:319–44.

With D. M. Fambrough. Limited molecular heterogeneity of plant histones. *Biochim. Biophys. Acta* 175:113–22.

With others. Calf and pea histone IV. III. Complete amino acid sequence of pea seedling histone IV; comparison with the homologous calf thymus histone. *J. Biol. Chem.* 244:5669–79.

Courtesy of the Rockefeller University Archives, New York, New York

Rodney Lee Cool

March 8, 1920–April 16, 1988

BY ROBERT K. ADAIR

RODNEY LEE COOL, whose work in experimental elementary particle physics over more than four decades played a significant role in the genesis of that field, was born March 8, 1920, at Platte, South Dakota, to George Edwin and Muriel Post Cool. George Edwin Cool was of Dutch and Norwegian stock. His father anglicized the Dutch name Koel to Cool, which is pronounced the same in the two languages. Muriel, of Dutch and English descent, was born and raised in Connecticut and moved to Platte with her family at the beginning of her high school years.

Platte is about 110 miles west of Sioux Falls and the Minnesota border and 30 miles north of the Nebraska border; about 10 miles to the northwest runs the Missouri River. The town was largely settled in the last decades of the nineteenth century when Cool's grandparents established their homes there. Rod Cool, born only thirty years after South Dakota entered the Union and only thirty years after the tragedy at Wounded Knee when the Indians in the western part of the state were subdued, was very much a son of Hamlin Garland's "middle border." He was raised in the small town with a sister, Harriet Jane, two years younger.

A town of nearly a thousand inhabitants in 1920 and the largest one for 40 miles around, Platte was the commercial center of the wheat growing farms that formed the economic basis of the area. Both of Rodney's parents were schoolteachers. His father, a graduate of Dakota Wesleyan University, earned his four-year degree in education; his mother taught after her graduation from high school in Platte, where she and Rodney's father were the two top students. School teaching was a common and highly respected occupation for educated young women before marriage, and the high schools of that time provided the requisite education.

Rodney Cool's grandfather and two of his brothers established the mercantile and banking facilities in the town of Platte, which prospered during the first three decades of the century. Hence, when Rod was a boy in the 1920s, his economically comfortable parents, well educated for that time and place, assumed they would send their very bright son east to college when the time came. But, the economic crash in 1929, beginning in the eastern stock markets, didn't take long to hit South Dakota. With the general collapse of the world economy augmented by the decade-long mid-western drought—not less bad in the Dakotas than in Oklahoma—the prosperity of the Cools evaporated along with that of their neighbors. While the decade of the 1930s were hard years in all of America, they were desperately hard years in the Dakotas.

So, when Rod finished high school in 1938, a few months after his eighteenth birthday, there was no money to send him east to school and little enough money to help him to go to school anywhere. But he found work that summer, saved a little money, and with a little help from home, in the fall of 1938 he enrolled at the University of South Dakota in Vermillion in the southeast corner of the state about 120 miles from home (and 30 miles from Canton, South Dakota, where Ernest Lawrence and Merle Tuve lived a generation earlier). Discarding an interest in law kindled as an outstanding debater in high school, Cool majored in mathematics and physics at the small school of about a thousand students.

Working summers, evenings, and weekends at school, Rod earned most of the $450 a year required for his tuition and living at the state school, where tuition and fees were only about $50 a year for a state resident. (Families in Platte were doing well at that time if they had an income of $1,000 a year.) Family members recalled that "Rod worked all of the time." But, in between his jobs he studied enough to be elected to Phi Beta Kappa before graduating in the spring of 1942.

In most of the mid-western state universities at that time two years of elementary ROTC courses (Reserve Officer Training Corps) were required of all male students and those judged to be militarily exceptional were invited to enroll in the senior-level training where graduates were awarded a commission in the Army Reserve. Senior ROTC students were paid a sum that was not insignificant in those difficult times. So, Cool, good at soldiering as in everything else, accepted a position in the senior ROTC and was able to give up a few of his more onerous jobs on the campus.

When war came to the United States in 1941, the nominal obligation of the senior ROTC members to serve in the Army Reserve after graduation changed effectively to active service. Hence, shortly after Cool's graduation in 1942, he entered the Army as a second lieutenant in the Signal Corps. He served for four hard years in the Pacific, where his unit landed, usually under fire, to set up communications on the invasion beaches in the course of MacArthur's island hopping that led from the Solomons to

Okinawa and to the war's end in 1945. An excellent soldier and commanding officer, Cool was discharged in 1946 as a major holding the Bronze Star medal—no sinecure at that time.

After leaving the Army, Rod entered Harvard as a graduate student in physics in 1946. With his communications engineering experience and maturity, Rod was immediately useful in experimental physics and J. Curry Street (elected to the National Academy of Sciences in 1953) was pleased that Rod asked to work with him on his cosmic ray work, which was directed towards the determination of the properties of elementary particles. In 1937, Street with E. C. Stevenson had conducted seminal cloud chamber studies of cosmic ray events that demonstrated conclusively the existence of the meson, a particle of mass intermediate between the proton and electron.

Cool worked first in Cambridge with Street and fellow student Earle Fowler on work that resulted in a publication in 1948 of one of the first clear cloud chamber pictures of a meson (now muon) beta decay. Later, again with Street and Fowler and with Robert Sard and William Fowler (Earle's brother) from Washington University in St. Louis, they set up a cloud chamber at Climax, Colorado, where they measured the absorption of muons by aluminum, noting in a paper published in 1949 that the negative muons usually emitted a proton upon absorption.

At 3,410 meters above sea level, Climax, along with Pikes Peak at 4,300 meters and Berthoud Pass at 3,500 meters, all in Colorado, were important centers of cosmic ray work directed in those years primarily towards the determination of the properties of the intermediate mass particles (now pions and muons) produced in the interaction of the high-energy cosmic ray particles—largely protons. As a consequence of the large molybdenum mining operation at

Climax, there was a good road to a high altitude point on the mountain and power was available—even as there was a road and available power at Pike's Peak and at Berthoud Pass.

In the summer of 1948 with his father dead, Rod as the senior member of his branch of the family accepted an obligation to travel to Ketchikan, Alaska, to help settle the estate of his aunt after her husband's death. There he met Margaret MacMillan. In that small town the MacMillans, who had long lived in Alaska (Margaret's mother, Ellen Rogers MacMillan, had taught school in Skagway, where she was born in the gold rush days), were friends of Rod's relatives.

In June 1949 after receiving his Ph.D. from Harvard, Rod and Margaret were married in Ketchikan. After spending the early summer in the Canadian Rockies, where they hiked the trails about beautiful Moraine Lake (shown on Canadian $20 bills circa 1970) from their comfortable cabin on the lake, the couple drove east to Brookhaven National Laboratory—stopping, of course, in South Dakota, where family and friends awaited them.

In the spring before he received his degree, Cool had talked with I. I. Rabi about joining the junior faculty at Columbia. But, teaching undergraduates did not appeal to Rod—although he was an excellent speaker and always at ease on the platform. He also talked with Oreste Piccioni, a few years older than Rodney Cool, who had joined the new Brookhaven Laboratory after conducting a remarkable experiment (with Conversi and Pancini) in wartime Italy which showed that the observed "mesons" (e.g., by Street and Stevenson) were not strongly interacting—and, hence, were not Yukawa's mesons (but the muons we know today.) Piccioni was looking for a colleague to work with him on experiments at the new accelerator, the Cosmotron, the very-high-energy proton synchrotron, then under construction at Brookhaven. Rod found the prospect of working with Oreste—an enthusiastic, exciting, and strikingly original person—to be attractive, and he accepted the position at Brookhaven.

After Rodney and Margaret arrived at Brookhaven on August 1, 1949, Rod learned that the cosmotron was not expected to be ready for two years. Therefore, it was decided that Brookhaven would build a log cabin laboratory at 11,000 feet, just below the 11,500 foot summit of Berthoud Pass, where he and Oreste Piccioni would carry on cosmic ray research using cloud chamber techniques familiar to Rod from his thesis work at Climax.

In January 1950 the five men in the group and their wives left for Georgetown, Colorado, a silver mining ghost town of about 500 people near Berthoud Pass, 50 miles from Denver. Here the five families moved into the only apartments in town, above the bar and post office. It was during their stay in Georgetown that the Cools' two older children Ellen and John were born, with Rod driving their mother through snowstorms to the maternity ward in a Denver hospital.

At Colorado Cool and Piccioni used an iron electromagnet and Geiger counter hodoscopes to examine pion production by protons, which they described in a paper published in 1952. This experiment with Piccioni was the first of a seven-year partnership that generated pioneering measurements of pion interactions using the Brookhaven National Laboratory cosmotron, which came on line late in 1951.

When the cosmotron—soon to accelerate protons to an energy of 3 GeV—was near completion, Rod and Margaret and the two children moved to Long Island near the Brookhaven laboratory where Rod and Oreste began designing the experiments they planned to conduct using the Cosmotron, which was then and for some time to come the world's highest energy accelerator.

At Brookhaven with his proven administrative ability as a field-grade officer in the army and with strong recommendations from Street, Cool was appointed by Sam Goudsmit to the position of assistant physics department chairman (and research physicist) under the department chairman, cosmic ray physicist Tom Johnson. This was the first of the ever-more-important administrative positions that Cool held at Brookhaven, which set much of the general administrative form of high-energy physics there and elsewhere.

When the Cosmotron began running in 1952, providing 10^{10} protons every 5 seconds at the unprecedented energy of 2.2 GeV, Cool and Piccioni, working with Leon Madansky from Johns Hopkins, transferred their efforts to the new accelerator and began an important series of measurements of pion-nucleon total cross sections that did much to establish the complexity of the pion-nucleon interaction. They concentrated on measurements at pion energies greater than 450 MeV while Sam Lindenbaum and Luke Yuan worked at the lower energies to complete the investigations of Fermi and his colleagues at the University of Chicago cyclotron. These investigations had shown a sharp increase in the pion-nucleon scattering and charge-exchange interactions up to energies of about 150 MeV.

The pions were produced by the interaction of the protons with an internal target where pions produced at small angles were then deflected out of the machine by the accelerator magnets to be transported by further magnets to make up a meson beam beyond the accelerator shielding. In this way negative pion beams could be produced up to energies of about 1.5 GeV as the magnetic field that bent the proton beam towards the center of the machine served naturally to eject negative particles. The internal magnetic fields were less fitted to eject positive pions, but reasonable intensifies of π^+ mesons in an external beam were achieved up to energies of 1.0 GeV.

Using these beams, Cool, Piccioni, and Madansky—later with David Clark —completed a series of pion-nucleon total cross sections over the accessible energy ranges using fast coincidence circuitry they had developed. In their early experiments they used carbon and hydrocarbon targets to determine proton cross sections through subtraction and water and heavy water targets to determine the deuteronproton cross section differences, allowing a good estimate of the pion-nucleon cross sections. After 1954 they used liquid hydrogen targets developed at Brookhaven to determine the proton cross sections.

The restriction to internal proton beams at the Cosmotron especially constrained measurements with positive particles, which could be extracted only if they were produced at large angles from targets in the short straight sections of the machine. Since the fluxes of high-momentum particles produced at large angles is necessarily small, this technique severely limited studies of the interaction of high-energy positive particles. Piccioni and Cool with Clark, radio-chemist Gerhart Friedlander, and engineer Dave Kassner developed a scheme for ejecting nearly the whole of the primary proton beam as a well defined external beam. With that beam applied to external targets high-energy positive pion beams were developed and used to extend both the π^+ and π^- cross sections to 1.9 GeV.

Their early measurements at discrete energies of 1.0 and 1.5 GeV showed that the cross sections did not simply vary monotonically with energy and the set of measurements from three years of work presented in an important summary paper published in 1956 showed significant structure.

The results of measurements of the whole set of π^+p, π^+n, $\pi^- p$, and π^-n cross sections from 450 MeV to 1.5 GeV listed in that paper allowed an interpretation in terms of cross sections for the more fundamental states of definite I-spin, I=1/2, and I=3/2. Moreover, the magnitude of the total cross sections showed that states of high total angular momentum, J, were involved. While taken alone, the I=3/ 2, J=3/2 enhanced cross section found by Fermi, which was firmly established as a resonance by Lindenbaum and Yuan, might have been considered as just a singular kinematic effect of a strong attractive interaction in that state, the existence of much more complex structure at higher energies in both the I=3/2 and I=1/2 states demonstrated clearly deep inadequacies in the simple pion-nucleon models that had been proposed. Later, extensions of this work led to the spectral information that formed the basis of the quark model of baryon and meson states.

When the Cosmotron, plagued by fatigue failures in its copper bar magnet windings, was scheduled for a year-long repair in 1957, Rod took a year's leave and moved to Berkeley, along with Margaret and children Ellen, John, and now Mary Lee. At the invitation of Ed Lofgren (extended to those Brookhaven physicists who had become under-employed by the Cosmotron shut-down), Rod planned to work at the Lawrence Radiation Laboratory Bevatron, a 6-GeV accelerator that had been running for two years.

At Berkeley, initially working with Bruce Cork and Bill Wenzel and then with Jim Cronin, who had joined Rod at Brookhaven after receiving his degree at Chicago, Cool did the first of a series of measurements that led him into a new research direction. Previous bubble chamber work had shown that the hyperon decay, $\Lambda \rightarrow p + \pi^-$ violated parity inasmuch as the pion was ejected preferentially in the direction of polarization of the lambda. With his colleagues Cool observed sigma decays produced by 1.0 GeV/c π^- mesons interacting with protons. They measured the product, αP, where α is the decay asymmetry of polarized hyperons and P is the degree of polarization generated in the production process. There results showed that the decay $\Sigma^+ \rightarrow p + \pi^0$ displayed a decay asymmetry, α, similar to that of the lambda, while the decay $\Sigma^+ \rightarrow n + \pi^+$ did not, thus demonstrating that P was large, and that the parity violating decay asymmetry depended strongly on the I-spin of the final state. This experiment marked the first work Cool had done pertaining to the weak interactions, rather than the strong interactions, since his work with cosmic rays.

A second experiment at Berkeley by the same group plus Leroy Kerth working with hyperons produced by the interaction of 1.00 GeV/c π^+ mesons on hydrogen led to improved accuracy in the sigma measurements and showed that P (and σP) was large for lambdas.

Back at Brookhaven with the cosmotron now running well and armed with the information that lambdas produced by 1.0 GeV/c π^+ on protons were strongly polarized, Cool with Jenkins, Kycia, Hill, Marshall, and Schluter made the first measurements of the magnetic moment of a hyperon. For technical reasons they preferred to work with charged particle products and hence the reaction, $\pi^+ + n \rightarrow \Lambda^0 + K^+$. By precessing the lambda over its decay path through a strong magnetic field and measuring its decay asymmetry as a function of that field strength, they measured a magnetic moment of -1.5 ±0.5 Bohr nucleon magnetons. The value was a little large (the present value is 0.614±0.005), but the technique was established and the sign of the moment and general magnitude was clearly determined.

A few years later Cool with others measured the magnetic moments of the Ξ^- hyperon, helping to complete the measurements of the moments of the members of the SU3 octet (which include the neutron and proton). The pattern of hyperon magnetic moments set by the measurements showed simplicities that fit the quark model and, hence, helped establish the validity of that model and the reality of quarks.

This remarkable set of experiments setting the magnetic moments of the hyperons, together with the earlier meson cross section measurements, were cited specifically in connection with Rodney Cool's election to the National Academy of Sciences in 1972.

The advent of the large accelerators at Brookhaven and the Lawrence Radiation Laboratory led to new ways of doing physics, and with the new procedures came unprecedented problems concerned with the sharing of resources. For the most part, physicists had worked in the general scientific tradition of individual effort and responsibility. By and large, even as the individual physicist chose the problems he would investigate, he also constructed and operated the equipment that he used to attack these problems.

However, beginning in the middle 1930s with the construction of relatively large cyclotrons and electrostatic generators, nuclear physics had moved into a state where the effort required to build and operate a facility was sufficiently great that the cooperation of groups of physicists was necessary. While the American democratic tradition of governance usually held in form, there was generally a *primus inter pares* at these laboratories, such as Ernest Lawrence at Berkeley. However, there was no such figure among the experimentalists at Brookhaven—or at Berkeley after Lawrence's death in 1956—and with the set of ambitious, strong minded, relatively young physicists competing for the scarce resource of access to these very large accelerators, serious problems of governance arose. Moreover, with the responsibilities of construction and operation of the accelerators largely separated from the responsibilities of the design and operation of experimental programs, the leavening of shared responsibility for the whole facility was reduced. A possibly apocryphal story attributed to Berkeley, but equally applicable to Brookhaven, concerned a conversation after a stormy meeting addressing the scheduling on the accelerator. Someone was supposed to have commented, "What we need is an experienced psychiatrist." Someone else answered, "Hell no! What we need is an experienced kindergarten teacher."

On the small committees developed at Brookhaven in the early 1950s to advise cosmotron department head George Collins on the scheduling of experiments, Rod Cool was always a sensible and conciliatory calm point in the often stormy deliberations. Consequently, laboratory director Leland Hayworth and physics department chairman Sam Goudsmit constructed mechanisms to place Cool at the wheel of the whole experimental program. In 1960 Cool was appointed chairman of high-energy physics, in 1964 assistant director for high-energy physics, and in 1966 associate director of high-energy physics. During this decade Cool served as chairman of an advisory committee that selected the experiments to run on the 30-GeV AGS accelerator—finished in 1957 and again the largest in the world—from proposals by groups of experimenters and set the running schedules.

With his own strong vision of the laboratory, his hard work that led to a command of the details of the many proposals, and his tact and sense of the practical and possible, Cool put his own imprint on the very successful accelerator program at Brookhaven. Moreover, by setting smoothly working procedures in the positions he held and partially created, he set the accelerator governance form at the laboratory that was used with little change in the more than two decades after he left Brookhaven in 1970. At that time he accepted the newly established position of professor of high-energy physics at Rockefeller University.

Cool took a year's sabbatical leave to CERN in the summer of 1962, living in Geneva with Margaret and the children—and new baby Adrienne (who later followed her father in science and is in 1977 an assistant professor of astronomy in the physics and astronomy department at San Francisco State University.) At CERN he worked on the experimental program at the PS accelerator, nearly a twin of the 30-GeV AGS accelerator at Brookhaven, and renewed old friendships with the French physicists from the Ecole Polytechnic, who had worked as students in Colorado on cosmic ray experiments at the same time as Rod in the late 1940s. Rod, Margaret, and the family were to return to Geneva again for a year in 1968–69 and after Cool left Brookhaven for Rockefeller University in the fall of 1970 he worked primarily at CERN, where he and Margaret kept an apartment in Ferney-Voltaire, France, five minutes from CERN. Over the next two decades Rod and Margaret spent about half of their time in their French apartment and about half in their apartment near Rockefeller University in New York.

When Cool joined Rockefeller, his old antipathy to the classroom teaching of elementary physics to undergraduates was not tested, as Rockefeller admitted only graduate students. And after twenty years of increasingly high-level administration at Brookhaven, where he had helped mold the laboratory and set a pattern for high-energy physics administration everywhere, Rod, now fifty years old, never again held an onerous administrative position; he had paid his dues and more, and from then on he would concentrate on the physics he liked best.

Beginning in the summers of the 1950s Rod Cool would spend occasional evenings at moderate-stake poker games with visiting physicists. Sometimes the games were continued at hotel rooms during physics conferences in the United States and abroad. With his wartime training of poker played with fellow officers in the Signal Corps, Rod held his own and more. More important, the poker led to long-time collaborations with fellow players John Tinlot of Rochester and Leon Lederman then at Columbia.

With Tinlot, Lederman, and others, Cool played a major role in measurements of muon proton scattering at high momentum transfers. Then—and to a lesser extent now—the question "Is the muon just a heavier electron?" was unanswered. The most important result of the experiment was that at rather high momentum transfers and correspondingly small distances the muon did just act as a heavy electron. Although then and now the difference in mass was not understood. Also, the electron-proton scattering measurements that probed the electromagnetic structure of the proton and were used by Friedman, Kendall, and Taylor to demonstrate that nucleons had "hard" constituents (i.e., quarks), albeit invaluable, were marred to some extent by the necessity of large corrections for the radiation of the electrons upon collision. While the fluxes of muons were much inferior to the electron currents available, the muons radiated less by a factor of about 40,000 enabling analyses that were superior in some ways.

After going to Rockefeller, Cool assembled an excellent group of younger physicists and with these colleagues Rod moved his efforts again to the world's highest energy accelerator newly built at Fermilab, 30 miles west of Chicago, which accelerated protons to 400 GeV. His work there largely concerned essays into the character of the small angle scattering of protons on protons and neutrons. Some very interesting attempts to find simple ways of understanding the complexities of the physics of elementary particles had centered on the analytic character of particle-particle scattering amplitudes. In particular, the amplitudes that determined the forward scattering of protons by protons and neutrons were considered to be governed by causal dispersion relations similar to the Kramers-Kronig relations that held for the scattering of electromagnetic radiation.

If the results of the program were disappointing in that no surprises emerged, the agreement with the consequences of simple causality generated confidence in the validity of causality and special relativity at small distances, a constraint that remains basic to the important particle theories we have today.

During the decades Cool worked on the physics of elementary particles, the complexities of experiments increased greatly. Along with that increased complexity came increased monetary costs and, sociologically most important, a significant increase in the scientific effort required to conduct an experiment. While Rod's early experiments involved two, three, and four scientists with a few technicians, and typically one or two scientist-years of effort, there are sixteen names on the first Fermilab paper, including seven from the Soviet Union, and those names represent perhaps twenty-five scientist-years of effort. Some of the later CERN papers list thirty names that again represented a very large effort. With so many participants in experiments that are so complex the organization of effort is important and that organization and leadership can only be exercised by a physicist who is knowledgeable about all details of the experiment and has the trust and confidence of everyone. Rodney Cool was often singular in his broad knowledge of the experiment and in how he held the confidence of his colleagues.

Soon after going to Rockefeller Cool also began programs at the CERN intersecting storage ring (ISR), where head-on collisions of 25-GeV protons generated center-of-mass energies that were appreciably higher even than the energies reached at Fermilab—although with a much lower intensity than at Fermilab. Here, he worked mainly on problems connected with the highest momentum transfers—and correspondingly the smallest interaction distances.

Among the high-momentum transfer experiments, Cool worked with Lederman on studies of electron pairs emitted with large invariant masses in the very-high-energy ISR collisions. While they missed (barely) the discovery of the J/ψ particle found independently by Ting and his colleagues at Brookhaven and by Richter and colleagues at SLAC, they confirmed those discoveries almost immediately. But, the very large flux of high transverse momentum π^0 mesons that obscured the J/ψ was in itself a major discovery, proving that the partons seen in deep inelastic scattering were strongly interacting and thus the quarks of quantum chromodynamics.

The effective interaction between quarks is weak at small distances (asymptotic freedom) and strong at large differences—so strong that quarks cannot be separated from their combinations and free quarks are not observed. Consequently, the basic interactions of elementary particles at very small distances (and correspondingly high momentum transfers) can be understood through perturbative calculations, while the interactions at larger distances (and smaller momentum transfers) are relatively intractable. However, among the products of the small-distance, high-momentum-transfer collisions of quarks and electrons with quarks are "jets" of particles where the character of the jets is determined by quark-quark interactions at larger distances. Hence, to extract the character of the fundamental small-distance collision from the experimental data, it is necessary to understand the phenomenology of the jets generated by the collisions.

Much of the rest of Cool's career was spent on high-momentum transfer experiments at CERN, where, through a large number of elegantly designed experiments, Cool and his colleagues managed to construct both the phenomenology of the jets they saw and the character of the fundamental interactions that produced the jets. The analyses of those fundamental collisions provided tests and verifications of quantum chromodynamics in the perturbative region and then played an important role in establishing the validity of that *Standard Model* description of elementary particles.

From his first experiment concerning the beta decay of "light mesons," which we now know as muons, published in 1948 to his last papers published forty years later, Rodney Cool was fortunate enough to live through the intellectual explosion that drove elementary particle physics from its birth up to the significant level of maturity we see today. We who also followed that path are fortunate to have known Rod, who contributed so much to that explosion.

At the time of his death, Cool's survivors included his wife Margaret MacMillin Cool-Dole; daughters Ellen Cool Kwait, Mary Lee Gupta, and Adrienne Margaret Cool; son John Post Cool; and seven grandchildren

IN WRITING THIS MEMOIR I drew extensively on the generous help of Mrs. Margaret MacMillin Cool-Dole, especially in the recounting of the personal side of Rodney Cool's life. Michael Tannenbaum and Leon Lederman, Cool's collaborators in much of his later work, helped greatly in putting that work in proper perspective.

Selected Bibliography

1948 With E. C. Fowler and J. C. Street. Example of the beta-decay of the light meson. *Phys. Rev.* 74:101.

1949 With E. C. Fowler, J. C. Street, W. B. Fowler and R. D. Sard. Cloud chamber study of mesons stopping in aluminum foils. *Phys. Rev.* 75:1275–76.

1952 With O. Piccioni. Interaction of pions originating in penetrating showers. *Phys. Rev.* 87:531–33.

1954 With L. Madansky, and O. Piccioni. Total cross sections of pions at 1.5 BeV. *Phys. Rev.* 93:249.

1955 With O. Piccioni, D. Clark, G. Friedlander, and D. Kassner. External proton beam of the Cosmotron. *Rev. Sci. Instrum.* 26:232.

1956 With O. Piccioni, and D. Clark. Pion-proton total cross sections from 0.45 to 1.9 Bev. *Phys. Rev.* 103:1082.

1959 With B. Cork, J. W. Cronin and W. A. Wenzel. Asymmetry in the decay of Σ hyperons. *Phys. Rev.* 114:912–20.

1960 With B. Cork, L. Kerth, W. A. Wenzel, and J. W. Cronin. Decay asymmetry of Σ^+ and Λ^0 hyperons. *Phys. Rev.* 120:1000–1004.

1962 With E. W. Jenkins, T. F. Kycia, D. A. Hill, L. Marshall and R. A. Schluter. Measurement of the magnetic moment of the Λ^0 hyperon. *Phys. Rev.* 127:2223–30.

1966 With others. Muon-proton scattering at high momentum transfers. *Phys. Rev. Lett.* 14:724–28.

With G. Giacomelli, T. F. Kycia, B. A. Leontic, K. K. Li, A. Lundby, and J. Teiger. New Structure in the K^--p and K^--d total cross sections between 1.0 and 2.45 GeV/c . *Phys. Rev. Lett.* 16:1228–32.

1967 With R. J. Abrams, G. Giacomelli, T. F. Kycia, B. A. Leontic, K. K. Li, and D. N. Michael. Structure in the $p^{\cdot}$ p and $p^{\cdot}$ n total cross sections between 1.0 and 3.3 GeV/c. *Phys. Rev. Lett.* 18:1209–12.

1972 With G. Giacomelli, E. W. Jenkins, T. F. Kycia, D. A. Leontic, K. K. Li, and J. Teiger. Measurement of the $\Xi^-\Xi^-$ magnetic moment. *Phys. Rev. Lett.* 29:1630–32.

With others. Small angle elastic proton-proton scattering from 25 to 200 GeV. *Phys. Rev. Lett.* 29:1755–57.

1973 With others. Real part of the proton-proton forward-scattering amplitude from 50 to 400 GeV. *Phys. Rev. Lett.* 31:1367–70.

With others. Observation of π^0 mesons with large transverse momentum in high-energy proton-proton collisions. *Phys. Lett. B* 46:471–76.

1974 With others. A search for electron pairs at the CERN ISR. *Phys. Lett. B* 48:377–80.

1975 With others. Diffraction dissociation of high energy protons in p-d interactions. *Phys. Rev. Lett.* 35:766–69.

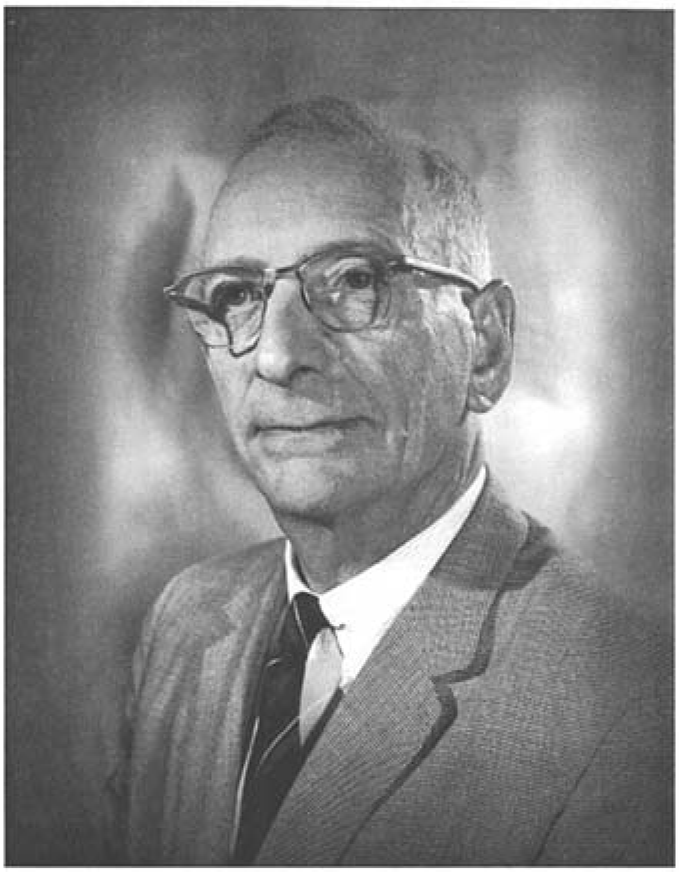
1976 With others. Total cross sections of $\pi^{\pm}$, $K^{\pm}$, p and $p^{\cdot}$ on protons and deuterons between 23 and 280 GeV/c. *Phys. Lett. B* 61:303–307.

1982 With others. Determination of the angular and energy dependence of hard constituent scattering from π^0 pair events at the Cern Intersection Storage Rings. *Nucl. Phys. B* 209:284–300.

1983 With others. Observation of jet structure in high transverse energy events at the CERN ISR. *Phys. Lett. B* 126:132–36.

1985 With others. Diffraction dissociation of photons on hydrogen. *Phys. Rev. D* 31:17–23.

1987 With others. A measurement of the inclusive π and η production cross sections at high p_T and pp and pp collisions at $s^{1/2}$ = 24.3 GeV. *Phys. Lett. B* 194:568–72.



Louis B. Flexner

Photograph by William Fore

Louis Barkhouse Flexner

January 7, 1902—March 29, 1996

BY JAMES M. SPRAGUE

LOUIS FLEXNER WAS A major scientific figure, in direct line with his distinguished uncles Abraham and Simon. He was a pioneer in the field of neurochemistry and made notable contributions also to the physiology of the cerebrospinal fluid and meninges, to the function of the placenta, and to the biochemistry of development. Through his organization and direction of the Institute of Neurological Sciences at the University of Pennsylvania, he had a major role in the development of neuroscience in this country.

Louis Barkhouse Flexner was born in Louisville, Kentucky, on January 7, 1902, to Ida Barkhouse and Washington Flexner. He grew up in Louisville and Chicago in a home of modest but adequate means and in a family that was congenial, cooperative, industrious, and interested in literature and sports and to a smaller degree in music. Education for the children was emphasized, with nothing spared in its support. He came from a family famous for its contributions to medicine. Uncle Simon Flexner was a professor of pathology at the University of Pennsylvania School of Medicine and later became director of the Rockefeller Institute for Medical Research in New York City. Uncle Abraham Flexner conducted the definitive study on the organization of U.S. medical schools and wrote a book that revolutionized U.S.

medical education. Abraham was responsible for the organization of the Institute for Advanced Study in Princeton and became its first director. Simon Flexner's influence was felt early by Louis, who, at age seven, won a Louisville newspaper's writing contest on "How I Intend to Earn My Living" with an essay on his intention to cure epilepsy.

Louis received his undergraduate education at the University of Chicago (B.S., 1923) after which he received his medical education at Johns Hopkins University (M.D., 1927). Flexner's rating at Hopkins placed him with Keffer Hartline (who was later to receive a Nobel Prize in physiology) at the bottom of the class of 1927, something of which both men were inordinately proud! This rating did not, of course, reflect on their intellects or abilities. Medical school proved to be difficult for Louis. He was confused by the volume of material, the variety of subject matter, and the unsettled state of numerous important areas. Essential support and stimulus from his earliest days came from Lewis H. Weed, director of the Department of Anatomy and dean of the school.

From Weed, he received an orientation that largely determined his subsequent activities and career. Soon after Flexner entered medical school, Weed started him on a problem in experimental embryology concerning the central nervous system, which was carried through to publication. For many years thereafter, his major interest centered on the development of the nerve cell. Weed also gave him a point of view towards medical education and the organization of a school of medicine.

On graduation from medical school, Flexner was not, however, certain that he wanted a career in anatomy. Thus, he welcomed an opportunity to work with the famous bio-chemist Leonor Michaelis, then at Hopkins, who was beginning his studies on oxidation-reduction potentials (1928, 1929). Although he had valuable time with Michaelis for about two years, he recognized his inadequacies in physical chemistry and mathematics and decided against continuing the collaboration with him. Still unsure of the kind of career he wanted, Flexner took an internship at the University of Chicago Clinics. This experience was decisive. Although offered the possibility of spending a year in Vienna with Sigumd Freud, he refused. After eight months his request to be relieved of clinical duties was accepted; he spent the remaining part of the year working on the development of the human cerebral cortex with G. W. Bartelmez at the Department of Anatomy of the University of Chicago. At the beginning of the next year he was back at Hopkins on the staff with Dr. Weed, where he spent the next nine years (1930–39) in the Department of Anatomy doing research on the cerebral-spinal fluid and establishing himself as an investigator of real promise. He absorbed from chairman and mentor Professor Weed a philosophy of medical education to which he adhered for the rest of his life. The keystone of this philosophy was the absence of spoon-feeding and the recognition that an instructor's main contribution was to teach students how to educate themselves.

Flexner's continuing interest in problems of mammalian development was broadened and stimulated by several experiences. First among these was a year spent with Sir Joseph Barcroft in the Physiological Laboratories at Cambridge (England). Barcroft at that time (1933–34) was essentially beginning his work in the field of fetal physiology. Here Flexner also came under the influence of Joseph Needham, author of *Chemical Embryology,* and an American, Donald Barron, who moved to physiology at Yale University in 1939 and became a lifelong friend.

In 1941 Flexner was offered a research position as a staff member in the Department of Embryology of the Carnegie

Institution of Washington. His studies there on the bio-chemistry of development received wide recognition, including a Harvey Lecture (1951–52). The environment and the support and insight of laboratory director George W. Corner and Vannevar Bush, president of the Carnegie Institution of Washington, proved ideal for the rapid development of his interest in this area. He remained with the Carnegie for a decade, having spent four of these years during the Second World War on a full-time basis as technical aide to the Committee on Aviation Medicine of the National Research Council's Committee on Medical Research.

In 1951 he was offered the chair of anatomy at the University of Pennsylvania. Before accepting the post he went to Philadelphia several times to meet the members of the department and sound out their feelings about having him as the new head. He satisfied himself that he would meet a welcome reception — a good example of his consideration of other people's feelings.

This switch to the University of Pennsylvania occurred for two reasons. First, he desired to get back into medical education both at the department and at the school level. Second, he wanted to see whether he might develop a department of anatomy whose faculty was working at the frontiers of biological knowledge in what appeared to him to be two of the important fields of biology: (1) structure and function of the central nervous system and (2) the central problems of embryology, that of cellular differentiation.

With customary vigor he quickly set up a research laboratory with his wife Josefa as coinvestigator and research associate and began to expand the horizons of a good but traditional anatomy department. He foresaw that three fields of medical research would become prominent in the future and would be appropriate for an anatomy department:

(1) cell differentiation and development, (2) ultrastructure and imaging, and (3) neuroscicnce. He began to build a department around these three disciplines.

Flexner understood that neuroscience research must be multidisciplinary in order to grow; in 1953 he undertook the organization of the Institute of Neurological Sciences (now known as the Mahoney Institute) by adding neuroscience staff to the department and encouraging other university departments to do likewise. From a beginning nucleus in 1954 of six members, including William Chambers, Eliot Stellar, John Brobeck, Per-Olaf Therman, Louis Flexner, and James Sprague, the Institute now includes over 130 neuroscientists distributed among 24 basic science and clinical departments throughout the university. The Institute has been an enormous success nationally and locally; its members helped shape the development of the departments of anatomy, psychology, biology, neurology, and psychiatry at the University of Pennsylvania.

Such is the power of a new and dynamic concept conceived and brought to term at the right time. This development was supported initially by a small grant from the National Institute of General Medical Sciences, part of the National Institutes of Health. The general support of NIGMS director Fred Stone was of great importance to us in securing the first training grant awarded by the National Institutes of Health. The Mahoney Institute led the nation in recognizing the impending explosive growth in the brain sciences, and celebrated its fortieth anniversary in 1994 at the annual meeting of the Society for Neurosciences.

While Flexner was chair, the other branches of the department also developed outstanding research and teaching programs with a faculty that included Howard Holtzer, John Marshall, James Lash, John Liu, Gabriel de la Haba, Donald Scott, Andrew Nemeth, Frank Pepe, Jean Piatt, Roy Williams, Culver Williams, Mary Jane Hogue, Michael Harty, and Robert Johnson. Their work attracted many excellent graduate and medical students and postdoctoral fellows. What emerged during the Flexner era (1951–67) was the picture of anatomy as a broad, comprehensive, and very active discipline in the School of Medicine and the university.

The significance of Flexner's own research in the bio-chemistry of memory, done in close collaboration with his wife, was recognized by his election to the National Academy of Sciences (1964), American Academy of Arts and Sciences, and the American Philosophical Society. He was also a member of the American Society of Biological Chemists and the American Association of Anatomists, which he served as secretary-treasurer from 1956 to 1964. The Democratic National Committee named him to a newly created Advisory Council on Technology in 1959. Flexner served on numerous scientific boards, including those of the U.S. Public Health Service, United Cerebral Palsy Association, National Council to Combat Blindness, National Research Council, National Paraplegic Society, and the National Foundation for Infantile Paralysis, where he joined the original Committee on Research. He received the Weinstein Award in 1957 in recognition of his work on the development of the central nervous system. In 1974 he was awarded an honorary degree (Doctor of Laws) by the University of Pennsylvania for his "unremitting pursuit of the highest ideals of scholarship and his uncompromising standards of research and teaching." Another Penn tribute was the establishment of the Louis B. Flexner Lectureship, one of the signal gatherings of neuroscience investigators each year, sponsored by the Mahoney Institute of Neurological Sciences.

The Flexners met when Josefa was a research associate in the Biochemistry Department of Johns Hopkins University from 1930 to 1931. A Catalan by birth and daughter of a distinguished Spanish family, Josefa studied at the University of Barcelona and at the University of Madrid, where she received her doctorate in pharmacy in 1927. She also spent a year studying pharmacology in England before coming to the United States for a year. Unrest and the civil war interrupted her career in Spain. Louis and Josefa were married in a village in the Pyrenees Mountains sixty years ago, and worked together until his death in 1996. She was known as Pepita and was indeed a very important colleague in his research and proved to be a lively and intellectually stimulating person to his laboratory visitors.

Louis served as chair of the Department of Anatomy from 1951 to 1967. His retirement was a simple process, which consisted of moving his office and laboratory across the hall, where he continued his research on memory and his teaching of medical students into his early nineties.

Flexner's first paper, published with J. Berkson in the *Journal of General Physiology* in 1928, was titled "The rate of reaction between enzyme and substrate" and was a forerunner of his scientific interests for the rest of his life. His mentor in anatomy, Prof. Lewis Weed, suggested the meninges and cerebrospinal fluid as a promising area for him.

Flexner began with the development of meninges in amphibia (1929), later joining Weed in the study of cerebrospinal fluid and intracranial pressure in the cat, macaque, and chimpanzee. From 1933 to 1937 Flexner made several important discoveries, among which was the proof that the production of cerebrospinal fluid was a process of secretion by the choroid plexuses. This work culminated in two important reviews, "Some problems of the origin, circulation, and absorption of the cerebrospinal fluid" (1933) and "The chemistry and nature of the cerebrospinal fluid" (1934).

Flexner spent the next two years at Cambridge University, working in the laboratory of Prof. Joseph Barcroft, where he began studies of fetal development, including physiology and biochemistry of the placenta and uterus. Returning to Baltimore, Flexner initiated a series of studies on placental permeability in the rat, guinea pig, goat, rabbit, pig, cat, and human, using radioactive substances as tracers. He published his findings in a long series of papers titled "Biochemical and physiological differentiation during morphogenesis" between 1937 and 1960. These studies initially concentrated on the transfer of substances in the placenta, but they were extended to the developing kidney, liver, and cerebral cortex in collaboration with many colleagues—Isador E. Gersh, R. Stiehler, Arthur Grollman, Richard Roberts, H. Pohl, Alfred Gellhorn, William Straus, Walter Wilde, Dean Cowey, Gilbert Vosburgh, Leslie Hallerman, Nathaniel Proctor, Virginia Peters, Max Hamburgh, A. Hughes, and especially his wife Josefa Flexner. His productivity was marked by brilliant insight, broad understanding of the material and technical ingenuity. His findings were summarized and discussed in *Genetic Neurology* in 1950, in a Harvey Lecture in 1952, and in the *Biochemistry of the Developing Nervous System* in 1955.

An important contribution in this period was the discovery by Flexner and colleagues (1960) that the lactic dehydrogenase (LDH) of the cerebral cortex of mouse and guinea pig consists of four components, two of which accounted largely for the increase in LDH activity that occurs when neuroblasts differentiate into neurons. This multiple form of enzymes was discovered independently by Flexner and by Markert and Moller (1959).

The last three reviews indicated Flexner's ongoing interest in the developing nervous system and pointed the way toward the later research on memory, for which he was best known. This line of research stemmed from a visit to an aging relative, whose long-term memory was intact but whose short-term memory was severely deficient. An important step was the discovery that the developing brain synthesized protein at a remarkably high rate. A second crucial step came with the discovery by Yarmolinsky and de la Haba (1959) that the antibiotic puromycin inhibited protein synthesis in liver extract by 90%. When this substances was injected subcutaneously by Flexner and colleagues, protein synthesis in the mouse brain was inhibited by approximately 80% two to eight hours after injection. Disappointingly, learning and retention were unaffected. Flexner and collaborators next injected puromycin intracerebrally, the spread of which could be estimated by the use of fluorescin and the extent of inhibition of protein synthesis under these conditions was about 90%. When puromycin was injected so that it reached primarily the hippocampus and entorhinal cortex, short-term memory measured after one day was completely lost. Loss of long-term memory measured eleven to forty-three days later required a more extensive injection of the frontal, temporal, and ventricular areas. There was a clear dissociation between recent and long-term memory. based on anatomy, biochemistry, and behavior.

Continued experiments indicated that this loss of memory caused by puromycin was not due to its effect on protein synthesis but to peptydyl puromycin formation. This was shown by use of another antibiotic acetoxcycloheximide (AXM), an even more potent inhibitor of protein synthesis than puromycin. AXM, however, failed to affect memory when injected one or more days after learning (1966, 1967; Agranoff, 1982). These and other findings caused a shift in their experiments to the role of neurotransmitters and their receptors in the formation and expression of memory. These studies, which yielded many important findings, occupied the Flexners for the next thirty years.

During his fifties and sixties, Louis became an avid sailor; he purchased a modern version of the old Chesapeake bugeye and named it *Josefa B* after his wife and colleague. Many of his friends, including the writer, had the pleasure of weekend cruises around the bays and anchorages of the Chesapeake Bay.

Louis Flexner was a man of many accomplishments, one of the more intangible of which was his leadership and the esprit de corps he infused in a faculty strong on ego and independence. Many are the students and postdoctoral fellows who trained in the department and who came back later to visit and said that their experience in the department was outstanding and unique. He built a strong department of talented people, and he took great care in selecting them, weighing not only their accomplishments but also, in so far as possible, their character. He once said, "I can admire the guy who is a great scientist, but at the same time I'll admire him a hell of a lot more if he has respect for other people." Before offering a position to a person outside the university, he would make a trip to the candidate's school to learn about his (or her) personality, talking to students, technicians, and peers. He had a strong character and was supportive and considerate of his staff, always having time to discuss research as well as personal problems. His advice was insightful and given to help the individual involved. The honoraria he received for lectures and service on committees and study sections was put into a separate account to help staff members purchase expensive equipment or animals or to attend meetings not fully covered in research grants. His forceful personality, high standards, and robust sense of humor made him a leading and colorful figure in the School of Medicine and the University of Pennsylvania.

Despite the high quality of his research and administrative accomplishments, Flexner's modesty was noteworthy, and at no point in his career did he seek prizes or solicit special recognition. He was made uncomfortable by any exclusiveness, such as a journal club or symposium, which to be manageable was necessarily limited in membership; he preferred that everyone be invited. He failed to attend meetings of the National Academy of Sciences because several colleagues whom he considered worthy had not been elected. He thought they had been excluded unfairly. I was stimulated and felt privileged to be part of the development of the anatomy department and Institute of Neurological Sciences, which Louis Flexner took over and headed. He was very supportive of my own research and was a potent catalyst in developing a scholarly atmosphere of research and a spirit of democracy and scientific openness among faculty and students—a rare combination. All of us felt the euphoria of a shared enterprise of research and teaching at the cutting edge.

Louis died peacefully in his ninety-fourth year, only a few days after a massive stroke. He is survived by his wife, Josefa Barba, his sister Miriam Maderev of New Rochelle, and many nieces, nephews, and cousins.

REFERENCES

Markert, C. L. and F. Moller. 1959. Multiple forms of enzymes. *Proc. Natl. Acad. Sci. U. S. A.* 45:753–63.

Yarmolinsky, M. B. and G. L. de la Haba. 1959. Inhibition by puromycin of amino acid incorporation into protein. *Proc. Natl. Acad. Sci. U. S. A.* 45:1721–29.

Agranoff, B. W. 1982. Learning, memory formation and protein synthesis. In *Changing Concepts of the Nervous System*, eds. A. Morrison and P. Strick, pp. 717–28. New York: Academy Press.

Selected Bibliography

1928 With J. Berkson. The rate of reaction between enzyme and substrate. *J. Gen. Physiol.* 11:433–57.

With L. Michaelis. Oxidation-reduction systems of biological significance. I. The reduction potential of cysteine: Its measurement and significance. *J. Biol. Chem.* 79:689–722.

1929 Development of meninges in amphibia: A study of normal and experimental animals. *Contrib. Embryol.* 20:31–48.

1933 Some problems of the origin, circulation and absorption of the cerebro-spinal fluid. *Q. Rev. Biol.* 8:397–422.

1934 The chemistry and nature of the cerebro-spinal fluid. *Physiol. Rev.* 14:161-87.

1937 A thermodynamic analysis of ultrafiltration. The ultrafiltration of sucrose and colloidal solutions. *J. Biol. Chem.* 121:615–30.

1939 With R. B. Roberts. Measurement of placental permeability with radioactive sodium: The relation of placental permeability to fetal size in the rat. *Am. J. Physiol.* 128:154–58.

1941 With J. B. Flexner and W. L. Straus, Jr. Oxygen consumption, cytochrome and cytochrome oxydase activity and histological structure of the developing cerebral cortex of the fetal pig. *J. Cell. Comp. Physiol.* 18:355–68.

1949 With J. B. Flexner. Biochemical and physiological differentiation during morphogenesis. IX. The extracellular and intracellular phases of the liver and cerebral cortex of the fetal guinea pig as estimated from distribution of chloride and radiosodium. *J. Cell. Comp. Physiol.* 34:115–28.

1950 The cytological, biochemical and physiological differentiation of the neuroblast, In *Genetic Neurology*, ed. P. Weiss, pp. 194–88. Chicago: University of Chicago Press.

1952 The development of the cerebral cortex: A cytological, functional and biochemical approach. *Harvey Lect.* 42:156.

1955 Enzymatic and functional patterns of the developing mammalian brain. In *Biochemistry of the Developing Nervous System*, pp. 281–95. New York: Academy Press.

1960 With J. B. Flexner, R. B. Roberts, and G. de la Haba. Lactic dehydrogenases of the developing cerebral cortex and liver of the mouse and guinea pig. *Dev. Biol.* 2:313–28.

1963 With J. B. Flexner and E. Stellar. Memory in mice as affected by intracerebral puromycin. *Science* 141:57–59.

1966 With J. B. Flexner. Effect of acetoxycyclohexamide and of acetoxycycloheximide-puromycin mixture on cerebral protein synthesis and memory in mice. *Proc. Natl. Acad. Sci. U. S. A.* 55:369.

1967 With J. B. Flexner and R. B. Roberts. Memory in mice analyzed with antibiotics. *Science* 155:1377.

1971 With P. Gambetti, J. B. Flexner, and R. B. Roberts. Studies on memory: Distribution of pepidyl-puromycin in subcellular fractions of mouse brain. *Proc. Natl. Acad. Sci. U. S. A.* 68:26–28.

With J. B. Flexner. Pituitary peptides and the suppression of memory by puromycin. *Proc. Natl. Acad. Sci. U. S. A.* 68:2519–21.

1972 With S. Lande and J. B. Flexner. Effect of corticotrophin and desglycinamide 9-lysine vasopressin on suppression of memory by puromycin. *Proc. Natl. Acad. Sci. U. S. A.* 69:558–60.

1975 With R. Walter, P. L. Hoffman, and J. B. Flexner. Neurohypophysical hormones, analogs and fragments: Their effect on puromycin-induced amnesia. *Proc. Natl. Acad. Sci. U. S. A.* 72:4180–84.

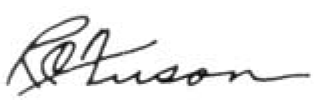
1978 With J. B. Flexner, R. Walter, and P. L. Hoffman. ADH and related peptides: Effect of pre- or post-training treatment on puromcin amnesia. *Pharmacol. Biochem. Behav.* 8:93–95.

1980 With T. C. Rainbow and P. L. Hoffman. Studies of memory: A reevaluation in mice of the effects of inhibitions on the rate of synthesis of cerebral proteins as related to amnesia. *Pharmacol. Biochem. Behav.* 12:79–84.

1983 With J. B. Flexner and A. C. Church. Studies on memory: The cerebral spread of an engram in mice as affected by inhibitors of dopamine betahydroxylase. *Pharmacol. Biochem. Behav.* 18:518–23.

1985 With J. B. Flexner, A. C. Church, T. C. Rainbow, and P. J. Brunswick. Blockage of beta 1—but not beta 2—andrenergic receptors replicates propranolol's suppression of the cerebral spread of an engram in mice. *Proc. Natl. Acad. Sci. U. S. A.* 82:7458–61.

1992 With J. B. Flexner and A. C. Church. Long-term suppression of the development of complementary memory storage sites in mice: Functional interdependence of acetylcholine and dopamine. *Pharmacol. Biochem. Behav.* 43:617–19.



Reynold Clayton Fuson

June 1, 1895—August 4, 1979

BY PETER BEAK, DAVID Y. CURTIN, AND DAVID A. LIGHTNER

REYNOLD C. FUSON began his career in organic chemistry at a time when structural determination by chemical methods was the principal area of research and the classification of reactions was just beginning. His research provided insights about structure and reactions that were significant in the development of organic chemistry for over thirty years.

Fuson's scientific contributions were recognized by his election to the National Academy of Sciences in 1944, his appointment as a founding member of the Center for Advanced Study at the University of Illinois in 1959, and a number of other honors. Fuson viewed teaching and research as inseparable; he regarded his awards as collective recognition for himself and his students. Throughout his career, Fuson had an outstanding reputation in research, teaching, writing, and as a research advisor.

Fuson was a complex and enigmatic figure, even to those who knew him well. He was usually private and self-contained, but on occasion he generously would share with his students and colleagues one of his many interests. Some found him reserved and remote while others, particularly students in some kind of difficulty or those with rural roots, found him responsive and involved. He retained an attachment to his early years in deeply rural southern Illinois, but he was also a sophisticated world traveler. Fuson was fluent in several languages, a student of painting and medieval architecture, and a collector of classical music. He was meticulous in his writing and dress and appeared to be the prototypical staid professor. Yet, his outstanding collection of classical music, which he would play for select groups, also included a recording made in his honor by a Mexican street band. Although Fuson appeared to be serious and sober, he had a wry sense of humor, and his inhibitions could disappear under the proper influence; perhaps in the case of this recording, tequila was involved.

Fuson's interests were not solely intellectual. He had a love of sports both as a participant and as an observer. He was an excellent bowler and squash player; he enjoyed professional baseball and Illinois football; and he would spontaneously take a group of students to those events.

Reynold Clayton Fuson was born on a farm near Wakefield, Illinois, as the sixth of eleven children of John Alvin Fuson and Nancy Jane Chestnut Fuson. He attended a rural one-room schoolhouse, where he was able to hear what was taught in higher grades, and he found he had a talent for learning. Fuson passed examinations above his grade level, and after the eighth grade he was expected to leave school to begin farm work. The teacher felt there was nothing further she could teach him. Attending high school in Wakefield was not a possibility; the town was 16 miles away over mud roads, which were impassable for much of the school year. However, a new teacher came to the remote schoolhouse, and Fuson then was able to attend two more years before going to Central Normal College in Danville, Indiana, where after one year in 1914 he was certified as a teacher. He returned to Illinois to teach in one-room schools in Jasper county for three years.

Although his ancestry included many preachers and teachers, Fuson did not enjoy teaching and was planning to become a railroad clerk when he was hired to teach the eighth grade in Montana. When he got to Corvallis, Montana, he found he had been reassigned to teach science in high school. He was given thirty days to learn seventeen subjects sufficiently well to pass an exam that would allow him to take up his assigned post. Fuson studied diligently for the exam, which lasted three days. Even after he had received many awards for his later work, he felt that passing that exam was the most noteworthy achievement of his career.

Although Fuson was certified as a high school teacher, his high school equivalency degree did not qualify him for college. However, he discovered when he took a correspondence course in English history from the University of Montana that this connection could offer him a backdoor admission, and he became a resident student.

Fuson intended to major in language and literature; his favorite subject was German. He had a lifelong interest in languages and later learned French, Italian, Spanish, and Russian. When he visited the Universities of Florence, Padua, Pavia, Palermo, and Rome in 1952, he was very well received not only for his chemistry but because he was the first foreign chemist to make a lecture tour in Italy speaking in Italian. His literary talents and chemical interests were also combined in poems he later wrote under the pseudonym of Robert Fox.

At the University of Montana, Professor J. W. Howard was responsible for encouraging Fuson to select chemistry as a field of concentration. Fuson saw in Howard an individual who was skilled in teaching and dedicated to organic chemistry and to his students. Fuson later remarked that it was the personal life of J. W. Howard that gave him an ideal to live by. Chemistry was also attractive because Fuson did not want a career that would involve any teaching.

Much to his surprise, Fuson was asked to join a fraternity at Montana. He had thought his farm-home, county-school background and failure to go to high school would separate him irremediably from the university students. It was in his college fraternity that he acquired the name Bob, which he chose to use for the rest of his life. Fuson valued his fraternal relationships. On his receipt of the Kuebler Award from Alpha Chi Sigma, he noted that he found in those relationships a spirit of cooperation and of helping one another for the common good, which greatly appealed to him.

Fuson graduated from the University of Montana in two years and was encouraged by the chemistry faculty to go to graduate school. He was admitted to the Universities of California, Minnesota, and Illinois; he chose to attend Berkeley. However, he left there with a master's degree, discouraged and thinking he would return to high school teaching. The details are not clear, but his California experience was unpleasant and it appears a decision may have been made by an administrator. Fuson's academic accomplishment was not to be terminated; in fact, he later received honorary degrees from the Universities of Montana and Illinois. On the occasion of the latter, he said:

> Unlike the other awardees, I felt incompetent for words of wisdom about life with problems facing mankind, so decided to essay the role of "farmboy makes good." My decision to turn to levity, I realized later, must have been influenced by the fact that for the first time at Illinois I found myself in the camp of the "enemy," the administrators. I wondered if they ever thought to the flesh and blood of the student forced to live on the Procrustean bed they had devised! When a student objected to being mutilated to conform to administrative norms, I almost always found myself on his side.

Fuson matriculated at Minnesota, where he received his

Ph.D. degree in two years. He later was awarded an Outstanding Achievement Award from Minnesota, and he noted that he was forever grateful to the university that rescued him at a low point in his career.

Fuson won a coveted National Research Council fellowship, which allowed him to work on his own research problems. He took the fellowship to Harvard under the sponsorship of Professor Elmer Peter Kohler. The first experiments he attempted gave unexpected results that challenged his intellectual and experimental skills. Success in that research was an epiphany for Fuson.

> The smoldering spark I have brought from Minnesota seemed to burst into flames. I had found what I was looking for. From then on the controlling interest (in my life) was to be research. The only comparison that comes to mind is with the person who "gets religion" and abandons the old life for the new. I was to spend the rest of my life as a missionary bringing the research cult to students.

In later reflection he noted:

> Doing research with students brought me two-fold satisfaction, to my obsession with chemistry was added a personal interest in the student. We were engaged in a process of co-learning. I never tired of discussing their problems, chemical or otherwise, many of them I coached for examinations in foreign languages. As always I took pleasure in helping them with the writing of the thesis or any papers that were prepared for publication.

With the transforming experience of accomplishment in research, Fuson's previous dislike for teaching evaporated. At Harvard, Fuson worked closely in teaching with Professor Kohler, who was an outstanding lecturer and was regarded as one of the deans of organic chemists. Kohler was very shy. He declined to give lectures outside of Cambridge, never accepted an award, and was somewhat remote from his students. Kohler knew of Fuson's burning interest to do research and recommended him to the University of Illinois. Fuson joined the Department of Chemistry at the University of Illinois at a time when that department, under the sequential leadership of Professors W. A. Noyes and Roger Adams, was beginning a rise to prominence in U.S. organic chemistry.

Fuson's training as a graduate student was somewhat different from most, and he appreciated the independence that Professor G. E. K. Branch at Berkeley, W. H. Hunter at Minnesota, and E. P. Kohler at Harvard gave him. He noted,

> To begin with I had more or less sidestepped the apprentice system as a graduate student. Two papers reporting (my) research were published at Minnesota under my name alone. Then I added five more papers from my work at Harvard and my name appeared on one of Kohler's papers.

His experience set his own style; as a research director Fuson was notable for giving his students exceptional independence. He selected research problems to fit the student, and the student was given a laboratory and helpful colleagues. When the first report was written, Fuson would provide a thorough criticism and the student was expected to respond with effort and accomplishment. Under his guidance, students developed a scientific maturity and self-confidence that served them well; many became leaders in their own right. Being known as a Fuson student became a mark of distinction, indicating a chemist who was independent in research and capable in written communication. Fuson was not an intrusive research director, but he had a high level of interest in the personal development of his students, which extended well beyond their graduate careers.

Fuson focused his energies on his research, teaching, and interactions with students. He never held a formal administrative role, although he was known for good judgement, and was quite influential. While his interest in research was based on inherent scholarly attributes, Fuson's skill in teaching required effort.

> When I came to Illinois I had behind me three years of teaching in one room schools in Jasper County and two years in high school at Corvallis. My dissatisfaction with teaching was one of the reasons I chose chemistry as a major rather than mathematics or modern languages. The chemist doesn't have to teach. The cause of my difficulty (with teaching) was shyness, which had plagued me from childhood. My mania for doing independent research changed all of that and committed me to a life of teaching.

Fuson was proud of the teaching award he received from the Manufacturing Chemists Association in 1960. His lectures were always extraordinarily well prepared and presented. He felt this came from his need to overcome his reserve and the models of Kohler and Howard.

> If I am a good teacher it is because I have always worked hard at it. The person that thinks he has arrived usually hasn't; if you don't prepare for the classroom and only wait for God to fill your open mouth with words of wisdom you cannot be a good teacher.

In Fuson's first semester at the University of Illinois he was assigned to teach organic chemistry to agricultural students, most of whom had little interest in the topic. He taught the course with such concern for the students that the dean of agriculture, who previously had apparently nothing good to say about chemistry, wrote a letter to Roger Adams praising Fuson and urging Adams to keep Fuson in that line of work. When Fuson learned of this letter some thirty-eight years after it was written, he provided an interesting analysis. He was, he said, so overcome with shyness that he felt the only way to appeal to the students was to make a personal approach; he learned every student's name, asked them about their high school, and helped them if they had difficulty. He noted that this approach, which was designed to win the students to his side, boomeranged. He found that his interest in the students was in fact genuine, and throughout his career, he maintained a remarkable level of personal interest in students. When photographs were required as part of the application for admission to graduate school, Fuson would memorize names of the incoming students and then at the first meeting of students he was able not only to recognize a student by name but also to introduce the students to one another. Fuson's interest in students and colleagues was both individual and collective; he later wrote a history of the chemistry department at Illinois, which emphasized the roles of many individuals in the accomplishments at Urbana.

At the time that Fuson began his independent research career, the understanding of organic chemistry was at an early stage. In two early papers, Fuson showed that a recently revived idea—that an aromatic ring was composed of puckered tetrahedral carbon atoms—was incorrect. This presaged a lifelong interest in aromatic compounds.

Fuson sought understanding of the relationship of structures to reactions. A major contribution was the principal of vinylogy in which he noted that functional groups separated by a carbon-carbon double bond took on the reactive characteristics of groups lacking the double bond. In an influential review article in 1935, Fuson pointed out this principle, which is still useful, albeit taught today in terms of resonance theory. He subsequently published a number of examples, as did many chemists around the world, after the explicit recognition of this structure-reactivity correlation.

His interest in molecular architecture was to be a cornerstone of many of Fuson's scientific contributions. Fuson explored the limits of structure by seeking to test limits. He would design molecules so they could not react in normal ways and would look for new and interesting reactions. This led him to discover that ketones with heavily substituted aromatic groups could form stable enols. In his work on reactions of conjugated sterically hindered systems, he discovered conjugate additions of Grignard reagents. In later years, he clarified rearrangement reactions of mustard gases. He also investigated coupling of Grignard reagents, ring closures, reversibility of Friedel-Crafts reactions, and unusual displacements in aromatic compounds. Many of these topics, which Fuson investigated over half a century ago, are still subjects of research in the 1990s. Fuson was awarded the Nichols Medal in 1953 for research that contributed to strengthening the foundations of organic chemistry.

Fuson's interest in research and teaching was reflected in an influential text on the qualitative identification of organic compounds. The book *Systematic Identification of Organic Compounds,* which R. L. Shriner and Fuson first published in 1935, was based on a systematic approach to identifying compounds, which had been developed by Oliver Kamm, an Illinois faculty member. This text set the standard for courses that brought attributes of research to student laboratories. The course required deductive logic, careful experimental observations, and a thorough knowledge of reaction chemistry. It allowed the student, often for the first time, to build and test hypotheses and to learn the pleasure of scientific accomplishment in a research mode.

Many of Fuson's students became important contributors to industrial chemistry in this country. His principle consulting arrangement with the Rohm and Haas Company lasted thirty years and was a forerunner of many of the consulting arrangements that have become common.

After retirement from the University of Illinois, Fuson spent fourteen years at the University of Nevada in a second career, first as a distinguished visiting professor and then as a professor emeritus. He was drawn to Reno by his love of the American West and its high mountains, engendered perhaps during his days as a young man in Montana. Fuson brought to Nevada a wealth of knowledge and a dedication to high standards. He continued teaching graduate as well as undergraduate organic chemistry, which afforded him an opportunity to continue his long association with students. Although he no longer continued active research, he was instrumental in guiding the research of graduate students in the newly founded chemistry Ph.D. program. Fuson served as wise counsel to the university community. He gave time generously to others, contributed to the building of chemistry at Nevada, and invited numerous distinguished friends and former students to visit. At a distinguished lecture series, which he initiated, he spoke to the students:

> At our Centennial Symposium in 1964, we had as speakers the President of the American Chemical Society, two members of the National Academy of Sciences and a representative of our largest industrial firm. In the subsequent four years, we managed to engage a continuing flow of similarly able and inspiring speakers. The formation last year (1967) of the Sierra Nevada Section of the American Chemical Society made it possible to attract additional distinguished guests. Their lectures are not only open but are provided primarily for your inspiration by giving you examples of excellence.

Nevada presented Fuson with another opportunity for historical scholarship, and he researched the history of chemistry in the state and in the university with thoroughness and vigor. He traveled to every site of historical interest and delved into archives to learn and catalog and then offer commentary on the rich legacy of chemistry in Nevada. He was a serious student and thorough documentarian, noting with relish that one of the first two professors at the university was hired to teach chemistry.

Fuson was instrumental in advancing and persistent in advocating the study of chemistry. Some thirty years ago, he spoke what is still true today:

> State-supported universities with their now traditional three-fold programs of teaching, research and public service enjoy a privileged position in our society. Yet, they are exceedingly vulnerable as becomes evident whenever unfavorable political winds arise. Students, who should be the best critics and who have every right to express their opinions seldom get a fair chance to do so because of a built-in weakness of our system.

The scientific contributions that Bob Fuson made are imbedded in the idiom of organic chemistry. These contributions were at the forefront of the field and revealed fundamental relationships about structure and reactivity. Fuson's greatest contribution, perhaps, was through his influence as a teacher, both of research students and of the many students who benefited from his lectures and books.

Bob Fuson was a lifelong bachelor who thought of his students and colleagues as family. His will divided his substantial estate between the Universities of Illinois, Montana, and Nevada. The endowment has been used at each university to support students and lecturers and to provide professorships, which were then named in his honor. The R. C. Fuson professors at the University of Illinois have been Nelson J. Leonard, David Y. Curtin, and Scott E. Denmark. The R. C. Fuson professor at the University of Nevada is David A. Lightner.

THE PREPARATION OF this biography was based on autobiographical notes, reflections of former students and colleagues of R. C. Fuson, and material from the archives of the University of Illinois.

Selected Bibliography

1933 With C. F. Woodward. The cleavage of carbonyl compounds by alkalies. XI. The action of hypobromite solutions on β-diketones. *J. Am. Chem. Soc.* 55:3472.

With A. P. Kozacik and J. T. Eaton. The reversible addition of aromatic compounds to conjugated systems. *J. Am. Chem. Soc.* 55:3799.

1934 With A. R. Gray. The highly activated carbonyl group. Mesitylglyoxal. *J. Am. Chem. Soc.* 56:739.

With A. R. Gray. The highly activated carbonyl group. Dimesityl tetraketone. *J. Am. Chem. Soc.* 56:1367.

With C. W. Tullock. The haloform reaction. XIV. An improved iodoform test. *J. Am. Chem. Soc.* 56:1638.

With J. F. Matuszeski and A. R. Gray. The highly activated carbonyl group. Dimestyl Triketone. *J. Am. Chem. Soc.* 56:2099.

With B. A. Bull. The haloform reaction. *Chem. Rev.* 15:275.

1935 The principle of vinylogy. *Chem. Rev.* 16:1.

With R. Johnson. The haloform reaction. XVI. The action of hypoiodite on hindered ketones. *J. Am. Chem. Soc.* 57:919.

1936 With L. L. Alexander and A. L. Jacoby. The reversibility of the Friedel-Crafts condensation. Hydrogenation phenomena. *J. Am. Chem. Soc.* 58:1233.

With R. T. Arnold. A new synthesis of mixed benzoins. Second paper. *J. Am. Chem. Soc.* 58:1295.

1937 With R. E. Christ. The application of the principle of vinylogy to unsaturated ketones. *J. Am. Chem. Soc.* 59:893.

1939 With C. H. Fisher, G. E. Ullyot, and W. O. Fugate. Reactions of bromomagnesium enolates of mesityl ketones. I. *J. Org. Chem.* 4:111.

With W. S. Emerson and H. W. Gray. Arylglyoxals and steric hindrance. *J. Am. Chem. Soc.* 61:480.

With J. Corse and E. C. Horning. Esterification of highly hindered acids. *J. Am. Chem. Soc.* 61:1290.

1940 With S. L. Scott, E. C. Horning, and C. H. McKeever. Enediols. IV. *cis*-trans-isomerism. *J. Am. Chem. Soc.* 62:2091.

With J. Corse and C. H. McKeever. A stable vinyl alcohol, 1,2,-dimesityl-1-propen-1o1. *J. Am. Chem. Soc.* 62:3250.

1941 With J. Corse and N. Rabjohn. Mesitoic anhydride. *J. Am. Chem. Soc.* 63:2852.

1943 With S. P. Rowland. β,βdimesitylvinyl alcohol. *J. Am. Chem. Soc.* 65:992.

1944 With N. Rabjohn and D. J. Byers. Vinyl alcohols. XI. 2-mesityl-2-phenylvinyl alcohol. *J. Am. Chem. Soc.* 66:1272.

1946 With B. C. McKusick and J. Mills. The addition of methylmagnesium iodide to benzoyldurene. *J. Am. Chem. Soc.* 11:60.

With C. C. Price, R. A. Bauman, O. H. Bullitt, Jr., W. R. Hutchard, and E. W. Maynart. Levinstein mustard gas. I. 2-haloalkylsulfonyl halides. *J. Org. Chem.* 11:469.

With R. D. Lipscomb, B. C. McKusick, and L. J. Reed. Thermal conversion of mustard gas to 1,2-bis(2-chloroethylthio) ethane and bis[2-(2-chloroethylthio)ethyl] sulfide. *J. Org. Chem.* 11:513.

1947 With J. F. Kerwin, G. E. Ullyot, and C. L. Zirkle. Rearrangement of 1,2-aminochloroalkanes. *J. Am. Chem. Soc.* 69:2961.

With J. Mills, T. G. Klose, and M. S. Carpenter. The structure of musk ketone and musk tibetene. *J. Org. Chem.* 12:587.

1948 With H. D. Porter. Addition of Grignard reagents to the olefin, bidiphenylene-ethylene. *J. Am. Chem. Soc.* 70:895.

With D. H. Chadwick and R. Gaertner. Replacement of nuclear alkoxyl groups by the action of Grignard reagents. *J. Org. Chem.* 13:489.

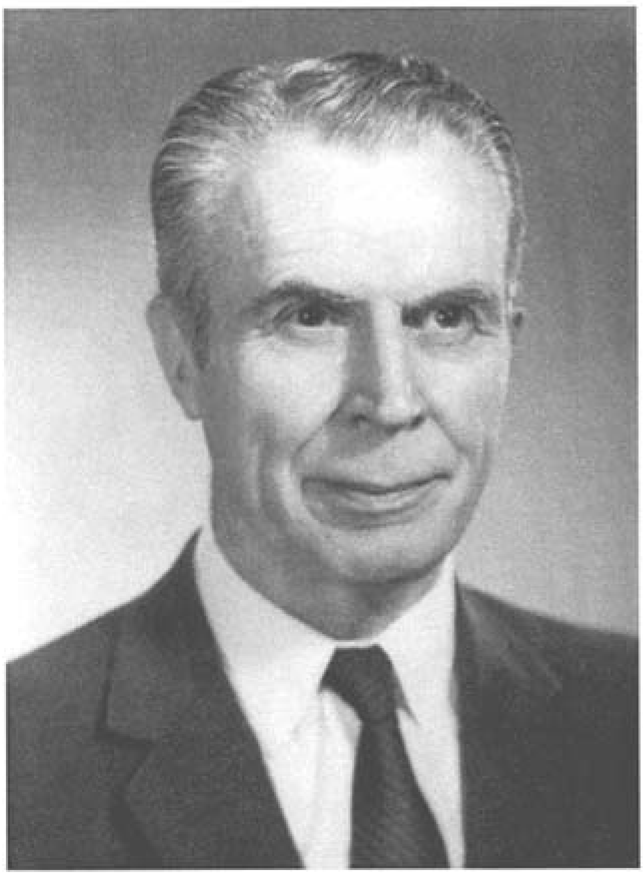
1949 With A. J. Speziale. Ring contraction by rearrangement of a β-chloro sulfide. *J. Am. Chem. Soc.* 71:1582.

1951 With W. D. Emmons. Replacement of nuclear acyloxyl groups by the action of a Grignard reagent. *J. Am. Chem. Soc.* 73:5175.

With C. Hornberger, Jr. Conjugate bimolecular reduction of mesityl phenyl ketone. *J. Am. Chem. Soc.* 16:631. Bimolecular reduction of carbonyl compounds. *Rec. Chem. Prog.* 12:1.

1953 With W. D. Emmons and J. P. Freeman. Nucleophilic displacement reactions of hindered ketimine methiodides. *J. Am. Chem. Soc.* 75:5321.

With H. O. House and L. R. Melby. Open chain analogs of tropolone. *J. Am. Chem. Soc.* 76:5952.



C.R. Henderson

Charles Roy Henderson

April 1, 1911–March 14, 1989

BY L. DALE VAN VLECK

TWO MEN, BOTH born in the farming community of Page County, Iowa, had more impact on modern animal production in the United States and the world than any other pair of scientists since the rediscovery of Mendel's laws. Both men specialized in the area of agricultural sciences known as animal breeding and genetics. The first, born near Shambaugh, Iowa, on January 3, 1896, was Jay Laurence Lush. The second, born near Coin, Iowa, on April 1, 1911, was Charles Roy Henderson, who became a graduate student under Lush at Iowa State College in 1946. Lush also preceded Henderson as a member of the National Academy of Sciences.

Lush, and later Henderson, led the profession for nearly all of the twentieth century. Lush outlined the basic principles and methods for genetic improvement. Henderson followed by developing methods first to estimate the genetic parameters needed for the application of those principles and then to predict most accurately the genetic values of animals available for selection. Henderson's methods have been since the 1950s—and will be for the foreseeable future—the basis for genetic evaluations for every breed of every class of livestock in every country of the world. Millions of beef cattle, dairy cattle, pigs, sheep, and other species are evaluated annually with what are called "Henderson's mixed model equations." These equations are also applied to statistical analyses in many other disciplines. The impact of Henderson's discoveries is perhaps incalculable. The discoveries, however, can be documented.

PERSONAL HISTORY

Farm Boy, Athlete, Scholar

Charles Roy Henderson was the first of four sons of Arthur James and Maud McMichael Henderson. One sister died at an early age. Charles Roy was born on the Henderson farm in Morton Township in Page County near Coin in southwest Iowa. He was forever proud of Page County and of growing up on a diversified farm with dairy cows and pigs, and with horses used for farm operations. Henderson was active both in 4-H and Future Farmers of America, which honored him as Iowa's Farmer of the Year during his final year in high school. He was probably even better known in Page County for his athletic achievements, which began at an early age. At one Page County Farm Bureau picnic he entered and won three consecutive races: for twelve and under, fourteen and under, and sixteen and under. The rules were changed the next year to be races for ages ten to twelve, thirteen to fourteen, and fifteen to sixteen! In addition to being the main member of the Coin High School track team, he was also the center of the basketball team when basketball still required a center jump after each score.

His track career flourished at Iowa State College (now Iowa State University) on both the indoor and outdoor teams. Highlights included a world indoor record for a relay (4 by 220 yards) of 1:31.8 in 1932; an Iowa State field house record for the indoor 440-yard run of 51.7 seconds in 1933, which stood for thirty years; and an outdoor personal best of 48.6 seconds in 1933, when the world record was 47.4 seconds.

While running track in both the indoor and outdoor seasons, he also was a member of the college livestock judging team and worked his way through college at numerous jobs, including waiting on tables at the dining room of the women's dormitory. The remainder of his time (or the other way around) was devoted to full time study. He graduated in four years and in the final year was taking courses for a graduate program in animal nutrition. Henderson's academic record at graduation was the best in his division, surpassed in the whole college by only one engineering student. His grades in chemistry included 96, 98, and 95 by quarter in his freshman year. Of his seven courses in mathematics the lowest grade was 98.

Sports And Music

The athlete from Iowa retained his interest in sports throughout his life. As a faculty member at Cornell, he was a faithful fan of most of the sports teams. His attendance at track, basketball, and football events was particularly regular while his sons were growing up. His interest in the Drake relays held in Des Moines was obvious. Although he could not attend with the regularity of one of his younger brothers, who was said to have attended 48 of 50 consecutive Drake relays, Henderson attended several times in connection with speaking trips in the Midwest.

Among his sports heroes were two near-contemporaries, Glen Cunningham of Kansas, who had overcome near fatal burns to become a premier miler, and naturally, the Olympian sprinter from Ohio, Jesse Owen. No doubt Henderson admired others as well, but an unlikely sport for an Iowan, ice hockey, furnished others. His loyalty to the Cornell hockey team, which rose from a new, usually soundly defeated team to become two-time national champions under Coach Ned Harkness, was nearly an addiction. Harkness, a rather unimposing person, was a motivator who could turn good athletes into superior team players, including a national championship team with a 29–0 record. What impressed Henderson even more was Harkness's off-the-rink interest in his players' success as students. Another hockey hero was an imposing athlete and scholar, Ken Dryden—varsity baseball shortstop, dean's list student, Nader's raider, and perhaps one of the best hockey goalies ever. For example, when he returned to hockey after a year of studying for his law degree, he joined the Montreal Canadians just before the play-offs and led them to a Stanley Cup. From the time Dryden signed with the then elite team of hockey, Henderson was a fan of "Les Habitants."

Henderson attributed his strong interest in music to its relationship to mathematics. When I asked his wife where or when that interest began (which was somewhat unusual for a boy from a country school in Iowa), she told me that the county school superintendent, later state school superintendent, had visited his grade school one day with a portable phonograph. Since Henderson most likely would have told that story many years later, my conclusion is that the superintendent's windup phonograph and the music, which he remembered as the William Tell Overture, must have made a strong impression. His radio at work and, I'm sure, at home was usually tuned to a classical music FM station—often WHCU in Ithaca, which coincidentally also carried all Cornell basketball, football, and hockey games. The dial did not need to be changed. My impression was that his favorite composer was Johannes Sebastian Bach. His wife told me she thought his favorite selection was Bach's Air for the G String. At his memorial service four pieces by Bach were featured, including Air for the G String at the interlude.

Nutrition, Extension, And Marriage

On graduation with a B.S. in the midst of the depression years of the 1930s, Henderson continued as an M.S. student in animal nutrition. His grades of A for ten hours of statistics and of B for eighteen hours of nutrition research suggest his early liking for statistics. The background in nutrition and obvious excellence in statistics eventually led him back to Iowa State to study with Lush, followed by an eminent statistical career in animal breeding and genetics at Cornell University.

In 1935 with an M.S. in animal nutrition, Henderson accepted a position as county agent in a county bordering Page County. In 1939 he was asked to become a district land use planning specialist. To prepare for this he enrolled for eight hours of economics at the Iowa State summer session. A fellow county agent one county closer to Ames asked if his daughter (who was working on her M.S. in education) could ride with Henderson to Ames that summer. Less than eighteen months later, on December 21, 1940, that daughter, Marian Mae Martin, married Charles R. Henderson in Chariton, Iowa. In January 1941 they moved to Ohio University in Athens, where Henderson taught animal husbandry and managed livestock enterprises on the university farm for nearly two years. Their first son, Charles, Jr., was born in Athens. Later children were James M. and Elizabeth Ann, both born in Ithaca, New York.

In late 1942 Henderson volunteered for and was accepted with a commission as a first lieutenant in the Medical Department's Nutrition Division of the U.S. Army. He served nearly four years and finished as commanding officer of the Army Medical Nutrition Laboratory in Chicago with the rank of major. He thought of his army experience as being particularly fortunate because nearly all of it involved research in nutrition and statistics. He was responsible for all aspects of data recording and statistical analyses. His enjoyment of that experience must have contributed to his decision to return to Iowa State as a Ph.D. student in animal breeding with Jay Lush. He had a joint major in animal breeding and genetics and a minor in statistics. His minor was much more than the usual one. In the two years at Iowa State his program included 18 hours in statistics, all with grades of A. This was begun at age thirty-five with a family, including his son Charles, Jr.

Academic Heroes

The person most influencing his professional career was undoubtedly Jay Lush, a true hero in Henderson's eyes. Although Lush accepted him for graduate work and signed course registration material for the first year, the official chair of his graduate committee was Lanoy N. Hazel, who joined the Iowa State faculty soon after Henderson arrived. Alexander Mood, representing statistics, was an original member of Henderson's graduate committee. When Mood left Iowa State, Oscar Kempthorne was his replacement. The other important committee member was John Gowen for genetics.

Other than Lush, Henderson's most significant role model was Sewall Wright, whose work with path coefficients influenced Lush's teaching of animal breeding. Despite the great regard of Henderson for Lush and Wright, Henderson took a completely different approach to the teaching of correlation and causation—the approach of mixed linear models, which was infinitely more understandable (at least to his students) than path coefficients. In addition to the basic foundation Lush and Wright had built for animal breeding and genetics, another basis for Henderson's admiration of Lush and Wright was that both continued their work long after formal retirement, Wright into his nineties and Lush into his eighties. Henderson was determined to follow that model, which he did until the week he died, two weeks before his seventy-eighth birthday shortly after preparing slides for a scientific meeting to be held a week later. A colleague presented his paper, using notes prepared by Henderson. He died after a brief illness on March 14, 1989, in Urbana, where for several years he had held a full time, half-year visiting professor appointment at the University of Illinois. He had been elected to the National Academy of Sciences in 1985 at age seventy-four (Lush had been elected at age seventy-one in 1967) as one of only a few animal scientists to receive that honor.

PROFESSIONAL HISTORY

The Mixed Linear Model Problem

Careers in science usually do not begin at age thirty-seven and continue for more than forty years, as did Henderson's. After four years in the Medical Division of the U.S. Army, Henderson returned to Iowa State to work with Lush and Hazel for a Ph.D. Within two years he had accepted a position as associate professor at Cornell University with a thesis to be completed that fall while at Cornell. At that time animal breeders relied on the techniques of Wright and Lush based on path coefficient methods which led to a later formalization called selection index. This method is used to predict unobservable breeding values assumed to be jointly distributed with observations that have been adjusted for all fixed environmental influences. Animal breeders also utilized least squares procedures based on work of Fisher and Yates in England to estimate the adjustment factors from what is now called "messy" data, using fixed effects linear models. The adjective "messy" refers to unbalanced data with many, if not most, subclasses having no observations. The selection index approach also requires estimates of components of variance for the random effects influencing the records, such as uncorrelated environmental effects and the nonobservable breeding values. And here was the problem of how to estimate these variance components from messy data. Prior to 1946, methods to do this had not been developed except for nested (not cross-classified) designs. These were problems that Henderson solved. Henderson almost single-handedly merged the selection index methods of Wright and Lush with the fixed linear model approach of Fisher and Yates into a single method to solve the mixed (fixed and random effects) linear model problem. The solution was what animal breeders and others now call Henderson's mixed model equations. Henderson credited the development of these equations to a homework question in a mathematical statistics course taught by Alexander Mood, a member of his graduate committee. That historic question (Mood, p. 164) is as follows:

> Suppose intelligence quotients for students in a particular age group are normally distributed about a mean of 100 with standard deviation 15. The I.Q., say x1, of a particular student is to be estimated by a test on which he scores 130. It is further given that test scores are normally distributed about the true I.Q. as a mean with standard deviation 5. What is the maximum-likelihood estimate of the student's I.Q.? (The answer is not 130).

Ph.D. Thesis

The Wright-Lush and Fisher-Yates approaches were the starting points for Henderson as he began his thesis research.

His thesis topic was on estimation of variances due to general, specific, and maternal combining abilities from single crosses among inbred lines of swine. The model for such data would require a linear model with many fixed environmental and many random genetic factors with many missing subclasses. No methods existed to estimate variance components from such data. The challenge of his thesis data led Henderson to develop two methods to estimate variance components, later known as Henderson's Methods 1 and 3. Method 3 was particularly powerful as it allowed unbiased estimation of variance components in the presence of confounding with fixed environmental factors. Method 2 was developed somewhat later and was widely used by animal breeders for many years as it was computationally much less demanding than Method 3. In 1951 he presented these methods at a conference in North Carolina where he met Fisher, who encouraged him to publish the methods. The full paper was published in *Biometrics* in 1953 and quickly became the standard reference for estimation of variance components. The paper was later selected by the Institute for Scientific Information as a most frequently cited scientific publication—"a citation classic." This work led to his being named a fellow of the American Statistical Association.

As part of his Ph.D. thesis research, Henderson invented a method to minimize the bias caused by interaction effects in estimates of fixed model effects in missing subclass cases. Many techniques for linear models were presented in detail in the appendix of his thesis. These techniques were incorporated into the most widely used statistical package of the 1960s, a package written by Walter Harvey, still used around the world by animal breeders. The thesis was also used for many years by animal breeders as a reference, especially the large number of students and visitors who studied with Lush and Hazel at Iowa State.

For his thesis Henderson also developed an ingenious method to evaluate merits of random inbred lines from large sets of data with many missing subclasses. He estimated breeding values by least squares as if they were fixed effects and then used those estimates as random data with selection index methods. This method was known as regressed least squares. He never, however, published his thesis material, even though it was used by many researchers. He had no joint papers with thesis advisors Lush and Hazel. The only published connection with his thesis is an abstract at the 1949 annual meeting of the American Society of Animal Science with the same title as his thesis.

Combining Least Squares And Selection Index

What seems to have happened is that soon after his arrival at Cornell he discovered the solution to the problem of mixed linear models that completed the combining of least squares procedures with selection index. An abstract for the 1949 annual meeting of the American Dairy Science Association indicates the birth of Henderson's mixed model equations. Rather than publish outdated although original contributions, he had simply moved on to a more correct approach. Although he was quite confident of his invention, proofs of the optimal properties would not be obtained until more than ten years later.

A colleague Henderson met as a Senior Fulbright Research Fellow to New Zealand in 1955 was Shayle R. Searle, who worked as a statistician for the New Zealand Dairy Board. Searle introduced Henderson to matrix algebra. Searle later obtained his Ph.D. with Henderson. After returning to New Zealand for two years, Searle accepted a position in biometry at Cornell and became again a colleague of Henderson. Searle helped considerably as a student and as a colleague with the matrix proofs of the desirable properties of Henderson's mixed model equations, which, parenthetically, were for years incorrectly called maximum likelihood equations.

Matrix algebra can be used to describe the power of Henderson's mixed model equations, which have been and will be used extensively by animal breeders and are now and will continue in the future to be the basis for linear mixed model analyses. Let y be a vector of observations of order n. The general linear model for the observations can be written as:

$$y = X\beta + \in$$

where

β is a vector of fixed effects,

X is the matrix that associates the fixed effects with the observations in y, and $\in$ is a vector of random effects.

The mean vector of y is E [y] = $X\beta$ and the variance of the observation vector, y, is V(y) = V($\in$) = V of order n X n.

With correlations among elements of $\in$ the structure of V is not the identity matrix. The generalized least squares solution for β has been known for sixty years to be obtained from the generalized least squares equations:

$$(X'V^{-1}X)\hat{\beta} = X'V^{-1}y$$

The problem is that V^{-1} must be calculated. Computational time to obtain inverses of matrices such as V^{-1} is proportional to $n^{3.}$ Thus, even with faster and faster computers (in 1948 few of any power were available) when, depending on the decade, the number of observations reached a few hundred, then thousands, later tens of thousands, and still later a few million, obtaining the inverse was impossible. If $X'V^{-1}X$ could be calculated, iterative methods can be used to obtain solutions to the equations but obtaining V^{-1} for all except trivial problems may still be impossible. The generalized least squares equations do define solutions for the fixed effects, β. Such solutions denoted as $\hat{\beta}$ yield $X\hat{\beta}$, which are best linear unbiased estimators (BLUE) of Xβ. But, what animal breeders and other people needed were predictors of random effects such as breeding values. These are included in ∊ as in the mixed linear model:

$$\epsilon = Zu + e$$

where u is the vector of random effects to be predicted (e.g., breeding values), Z is the matrix associating effects in u with y, and e is a vector of random effects uncorrelated with u.

Predictors of elements of u which have the properties of BLUP, best linear unbiased predictors, can be obtained as E[u1/2y] with the restriction that E[û] = E[u]. One way to obtain û is

$$\hat{u} = GZ'V^{-1}(y - X\hat{\beta})$$

where GZ' = cov (u,y'). The corresponding selection index predictor (also called best linear predictor) is:

$$u = GZ'V^{-1}y - X\beta)$$

where β, the vector of fixed effects, is assumed known exactly instead of using $\hat{\beta}$ the BLUE of β. To adjust for fixed effects to obtain BLUP, not only must V^{-1} be calculated, $\hat{\beta}$

must be obtained from solving the generalized least squares equations.

The Mixed Model Equations

This then is the problem Henderson saw and solved. How could $\hat{\beta}$ and $\hat{u}$ be obtained without inverting V? His solution, and also the approach of Wright and Lush later formalized by Hazel and Fairfield-Smith as selection index, require knowledge of variance components making up V. That was the stimulus for his development of methods to estimate variance components for which he first became well known, not only in the field of animal breeding and genetics but also in the field of statistics. His goal in developing methods to estimate variance components (he developed various procedures during the next thirty-six years) was not a mathematical-statistical exercise. His reason was to obtain the best possible predictions of breeding values to use for optimizing selection of breeding animals.

In passing, it can be noted that a 1979 conference honored Henderson for his contributions to the estimation of variance components and to the application of those techniques. All the conference papers (by leading researchers) were published, including Henderson's closing paper entitled "Using Estimates of Variances in Predictions of Breeding Values Under a Selection Model." More on that later.

Even with the assumption that good estimates of variance components represented in V are available, the problem was how to obtain V^{-1}. Henderson's solution was intuitive and did not involve calculation of V^{-1}. He very likely did try to find efficient ways to do that, but he was unsuccessful. What he did do was this. If elements of u are assumed to be fixed effects, then for the linear model

$$y = X\beta + Zu + e$$

the weighted least squares equations to solve when assuming both β and u are fixed are:

$$\begin{bmatrix} X'R^{-1}X & X'R^{-1}Z \\ Z'R^{-1}X & Z'R^{-1}Z \end{bmatrix} \begin{bmatrix} \hat{\beta} \\ \hat{u} \end{bmatrix} = \begin{bmatrix} XR^{-1}y \\ Z'R^{-1}y \end{bmatrix}$$

where R = V (e), which usually has a simple structure: diagonal and homogeneous for single traits or block diagonal or multiple-traits, so that R^{-1} is easily obtained. Henderson's first formulation involved single trait equations multiplied through by σ^2, the homogeneous variance of $R = I\sigma^2$. In that case, the equations are what are called ordinary least squares equations

$$\begin{bmatrix} X'X & X'Z \\ Z'X & Z'Z \end{bmatrix} \begin{bmatrix} \hat{\beta} \\ \hat{u} \end{bmatrix} = \begin{bmatrix} X'y \\ Z'y \end{bmatrix}$$

In the general formulation, what he did was add G^{-1} where G = V(u) to the $Z'R^{-1}Z$ block of the least squares equations:

$$\begin{bmatrix} X'R^{-1}X & X'R^{-1}Z \\ Z'R^{-1}X & Z'R^{-1}Z+G^{-1} \end{bmatrix} \begin{bmatrix} \hat{\beta} \\ \hat{u} \end{bmatrix} = \begin{bmatrix} X'R^{-1}y \\ Z'R^{-1}y \end{bmatrix}$$

What he thought and what he proved several years later was that the solutions to these "modified" least squares equations were, in fact, BLUE for β and BLUP for u (i.e., were identical to $\hat{\beta}$ and û presented earlier). Because R^{-1} is usually easily obtained and in many cases the structure of G is such that G^{-1} is also easily obtained, then simple iteration can be used to obtain $\hat{\beta}$ and û from the mixed model equations, known to most animal breeders and statisticians as "Henderson's mixed model equations." The importance of this discovery cannot be overemphasized. These equations are easily constructed and relatively easily solved even for millions of equations such as is now the case for genetic evaluations in many countries for most classes of livestock. These equations are the foundation for nearly all applications of analyses for mixed linear models. In fact, they are also the basis for rather sophisticated methods of variance component estimation using principles of maximum likelihood and restricted (residual) maximum likelihood. It seems likely that these equations will be used for decades if not longer for analyses of mixed linear models (and, in fact, in further modified form also for nonlinear or generalized linear models).

The proofs that solutions from mixed model equations are the BLUE of β and BLUP of u are in papers in 1959 and 1963. The proofs are based on an identity for V^{-1} with V = ZGZ' + R due to Henderson's colleague-student-colleague, Shayle R. Searle.

Improved methods of estimating variance components have been developed, many with the stimulus and prodding of Henderson. No doubt Methods 1, 2, and 3 for estimation of variance components would have been developed without Henderson, only later, but it seems unlikely that anyone would have discovered "Henderson's" mixed model equations.

Inverting a Special Matrix

What Henderson considered his second most important discovery was a way to obtain G^{-1} for a special case important to geneticists. Related animals have genetic material in common. Wright worked out a measure of this genetic relationship between pairs of animals called the numerator relationship. A numerator relationship matrix contains the relationships among any number of animals. Having a fraction of genes in common is like a fractional replication of genetic effects for related animals. In fact, for a simple model G = Aσ^2 where A is the numerator relationship matrix and σ^2 is the genetic variance (variance of breeding values). Animal and plant breeders typically want to make use of this partial genetic replication through Aσ^2 to obtain more accurate predictions of breeding values or to predict breeding values for animals without records. In animal models typically used in animal breeding the number of animals may range from thousands to millions. However, to apply Henderson's mixed model equations G^{-1} rather than G is needed. Thus, for G = Aσ_g^2 what is needed is A^{-1} ($G^{-1} = A^{-1}\sigma_g^{-2}$). Again the n^3 problem, even if A can be calculated by known n^2 rules. This discovery took longer. Henderson's first attempts were in New Zealand during his Fulbright leave with Searle in 1955. Not until 1975 did he publish a simple set of rules to obtain elements of A^{-1} without having to calculate A. All that is needed is a list of animals with each animal's sire and dam. These calculations take time in order of n^2 (and much less in the absence of inbreeding) and enable calculation of A^{-1} for applications such as the 13 million animals involved in genetic evaluations of the Holstein breed in the United States. What this discovery allows is use of information on all relatives of an animal (all partial replications of genetic effects) rather than only records of selected close relatives as with selection index procedures. An added advantage is that use of A through A^{-1} automatically corrects evaluations for one parent for the genetic merit of the other parent (the mate). This way of obtaining A^{-1} also results in sparseness of the mixed model equations which facilitates solution by iteration.

This discovery, and discovery is the correct word, was accomplished by looking at small examples with pencil and paper, an approach Henderson regularly followed. These examples would have involved inverting small matrices "by hand," a procedure Henderson was exceedingly proficient at doing. He finally was able to determine the pattern as well as the numerical elements of the inverse; these elements generally involved repeating fractions such as 4/3 or -2/3. It may have been fortunate that Henderson did not have a personal computer with a matrix inverter, as the patterns with 4/3 and -2/3 are much easier to see than 1.33333333 and -.66666667.

A potential disadvantage of this method of calculating A^{-1} is in application to the mixed model equations. The A^{-1} matrix computed by Henderson's rules is of the order of the number of unique animals, including sires and dams, some of which may not have records. Thus, equations need to be included for animals that do not have records. Usually least squares equations will involve only the model for the available records. What Henderson knew but did not publish until later was that the mixed model equations can be augmented to include equations for the animals without records. These equations are tied to the equations for animals with records with the $G^{-1} = A^{-1}\sigma_g^{-2}$ part of the mixed model equations. Solutions for animals without records are basically selection index predictions using relationships to relatives with records and their predicted breeding values. This method is particularly valuable for multiple trait models and for models with both direct and maternal effects when all animals do not have measurements for all traits or do not all become mothers of progeny with records.

The sum of the discoveries of 1) the modification of the least squares equations to form mixed model equations with solutions that have BLUE and BLUP properties, 2) the easy calculation of A^{-1}, and 3) the augmentation of the mixed model equations to include equations for animals without records (for all or some traits) has provided animal breeders worldwide with the best possible tools for genetic selection.

Henderson consolidated knowledge of the properties of selection index as an extension of the work of Wright and Lush. He also showed that the BLUP properties of solutions to the mixed model equations have the same properties as selection index. These properties are widely used in constructing standard errors of the predictors of breeding values and even for designing optimum selection plans.

The Selection Bias Problem

A problem with real breeding data is that selection of potential parents occurs based on their records. Selection violates the usual assumptions of random sampling needed to obtain unbiased predictors of breeding values. Henderson showed in a classic paper, which also clearly set out the properties of the mixed model equations, that under many selection methods and intensities of selection his mixed model equations provide estimators and predictors unbiased by selection if the data on which selection was based were included in the mixed model equations. Neither least squares nor selection index has this property.

Various simulation studies have shown even for other types of selection that the mixed model equations account for most if not all bias. Of particular importance is the finding that using the mixed model equations as a basis for REML estimation of genetic and environmental variance components provides estimates for the population as if selection had not occurred.

Many other examples of Henderson's contributions to the understanding of linear models, variance component estimation, and genetic evaluations could be described. However, a major contribution of a different nature should be listed.

Young Sire Testing Programs

Artificial insemination of dairy cattle was becoming important in New York State and throughout the United States when Henderson came to Cornell in 1948. He soon found that genetic evaluations of sires used in artificial insemination based on records from the originating herds were essentially worthless. He developed methods to account for the herd of origin effects which were widely adopted. More importantly, he and colleagues at Cornell early in the 1950s persuaded the New York artificial insemination organization to begin a program of using semen of young bulls randomly in many herds to produce samples of daughters having records in several herds. The New York group, although originally widely criticized and worse, continued the program. The group of sampled bulls totaled twenty in the first year. Other artificial insemination organizations gradually began similar programs after seeing the success of the rather limited New York program. By the 1980s, more than 1,200 young bulls were being sampled annually in the United States.

The synergistic effects of young sire sampling exploited through artificial insemination, records available through cooperative recording associations, and genetic evaluations (the BLUPs) have resulted in the greatest success story in applied animal breeding. The rate of genetic progress in dairy cattle has reached levels approaching theoretical limits. Henderson can be correctly credited with being the prime force in developing two of the three necessary bases for that success: young sire sampling and best possible genetic evaluations of those sires, potential parents of new young sires, and cows to produce replacement heifers. This was his practical contribution to one major industry, the dairy cattle industry.

Similarly, his mixed model equations with relationships of all animals included and augmented for animals without records are used nearly universally around the world and for all livestock species. A safe prediction is that no better methods will be found—perhaps better models, but not better methods. Henderson's mixed model equations have also allowed the proliferation of statistical packages that now handle messy unbalanced data with models including fixed and random effects. The generality of his discoveries and his perseverance in developing and proving the properties seems likely to result in Henderson's mixed model equations being used for many decades. New uses of the equations such as improved algorithms for estimating variance components seem to be found regularly. The contributions of an academic career that started at thirty-seven years of age and was carried forward eagerly with enthusiasm, patience, and insight for forty years will continue to be utilized and will not soon be forgotten by animal breeders and statisticians. Although his name eventually may become disconnected with the discoveries, the discoveries will continue to enrich the scientific community.

Many of his students and colleagues not only admired Henderson as a brilliant scientist but also as a person. We were certain his ideas came easily despite the evidence of twenty years to develop rules for A^{-1}. His wife, to whom he was a devoted partner, insisted to me that her husband felt his successes were not easily achieved and were due mostly to much hard work and perseverance. His work ethics contributed greatly to his success and also inspired by example his students. He listed some of these on a handwritten transparency he prepared by request at his farewell lecture at Kyoto University, where he was teaching as a Distinguished Visiting Professor in 1985:

Some Advice to Young Scientists:

1. Study methods of your predecessors.
2. Work hard.
3. Do not fear to try new ideas.
4. Discuss your ideas with others freely.
5. Be quick to admit errors. Progress comes by correcting mistakes.
6. Always be optimistic. Nature is benign.
7. Enjoy your scientific work. It can be a great joy.

C. R. Henderson
Kyoto University
December 16, 1985

MOST OF THE MATERIAL in this memoir was taken from papers presented at a symposium, The Legacy of C. R. Henderson, at the 1990 annual meeting of the American Dairy Science Association by five of Henderson's thirty-two Ph.D. and seventeen M.S. students and from a brief biography I prepared for the *Journal of Animal Science* (accepted, but not yet published). Much of the personal information was provided by his wife Marian or other family members and by the Department of Animal Science at Cornell University for my presentation at the 1990 symposium. Some material is the result of memories from having been one of Henderson's students and as a fellow faculty member in an adjoining office over a period of thirty one years.

HONORS AND DISTINCTIONS

1955	Senior Fulbright Research Scholar (New Zealand)
1964	Borden Award (highest award of American Dairy Science Association) Animal Breeding and Genetics Award, American Society of Animal Science
1968	Award of Merit, Eastern Artificial Insemination Cooperative
1969	Fellow, American Statistical Association
1971	Morrison Award (highest award of American Society of Animal Science)

1977	National Association of Animal Breeders Award, American Dairy Science Association
1980	Massey University Fellow (New Zealand)
1981	Sesnon endowed professorship, University of California, Davis Fellow, American Society of Animal Science Hermann von Nathusius Medal, German Society of Animal Production
1982	Jay L. Lush Animal Breeding and Genetics Award, American Dairy Science Association
1984	Henry A. Wallace Award for Service to Agriculture, Iowa State University
1985	Alumni Research Award, Iowa State University Distinguished Visiting Professor, University of Kyoto, Japan Member, National Academy of Science

REFERENCES

Freeman, A. E. 1991. C. R. Henderson: Contributions to the dairy industry. *J. Dairy Sci.* 74:4045–51.
Kennedy, B. W. 1991. C. R. Henderson: The unfinished legacy. *J. Dairy Sci.* 74:4067–81.
Mood, A. M. 1950. *Introduction to the Theory of Statistics*. New York: McGraw-Hill.
Schaeffer, L. R. 1991. C. R. Henderson: Contributions to predicting genetic merit. *J. Dairy Sci.* 74:4052–66.
Searle, S. R. 1991. C. R. Henderson, the statistician; and his contributions to variance components estimation. *J. Dairy Sci.* 74:4035–44.
Van Vleck, L. D. 1991. C. R. Henderson: Farm boy, athlete, and scientist. *J. Dairy Sci.* 74:4082–96.
Van Vleck, L. D. 1997. Charles Roy Henderson, 1911–1989: A brief biography. *J. Anim. Sci.* (accepted).
Van Vleck, L. D. and S. R. Searle, eds. 1979. Variance components and animal breeding: Proceedings of a conference in honor of C. R. Henderson. Cornell University, Ithaca, NY.

Selected Bibliography

1952 Specific and general combining ability. In *Heterosis*, ed. J. W. Gowen, pp. 352–70. Ames: Iowa State College Press.

1953 Estimation of variance and covariance components. *Biometrics* 9:226–52.

1954 Selecting and sampling young bulls. In *Proceedings of the 7th Annual Convention of the National Association of Animal Breeders*, p 93. Columbia, Md.: National Association of Animal Breeders.

1959 With O. Kempthorne, S. R. Searle, and C. M. von Krosigk. The estimation of environmental and genetic trends from records subject to culling. *Biometrics* 15:192–218.

1963 Selection index and expected genetic advance. In *Statistical Genetics and Plant Breeding*, ed. W. D. Hanson and H. F. Robinson, pp. 141–63. Washington, D.C.: National Academy of Sciences-National Research Council.

1965 A sire evaluation method which accounts for unknown genetic and environmental trends, herd differences, season, age effects, and differential culling. In *Proceedings of the Symposium on Estimating Breeding Values of Dairy Sires and Cows*, pp. 172–204. Washington, D.C.: USDA-ARS.

1968 With E. P. Cunningham. An iterative procedure for estimating fixed effects and variance components in mixed model situations. *Biometrics* 24:13–25.

1969 Design and analysis of animal science experiments. In *Techniques and Procedures in Animal Science Research*, pp. 1–35. Albany, N.Y.: Am. Soc. Am. Sci.

1970 A new sire evaluation method. In *Proceedings of the 23rd Annual Convention of the National Association of Animal Breeders*, pp. 31–35. Columbia, Md.: National Association of Animal Breeders.

1973 Sire evaluation and genetic trends. In *Proceedings of the Animal Breeding and Genetics Symposium in Honor of Dr. Jay L. Lush*, pp. 10–43. Champaign, Ill.: ASAS and ADSA.

With N. S. Urquhart and D. L. Weeks. Estimation associated with linear models. A revisitation. *Commun. Stat.* 1:303–30.

1974 General flexibility of linear model techniques for sire evaluation. *J. Dairy Sci.* 57:963–72.

1975 Best linear unbiased estimation and prediction under a selection model. *Biometrics* 31 (2):423–47.

A rapid method for computing the inverse of a relationship matrix. *J. Dairy Sci.* 58:1727–30.

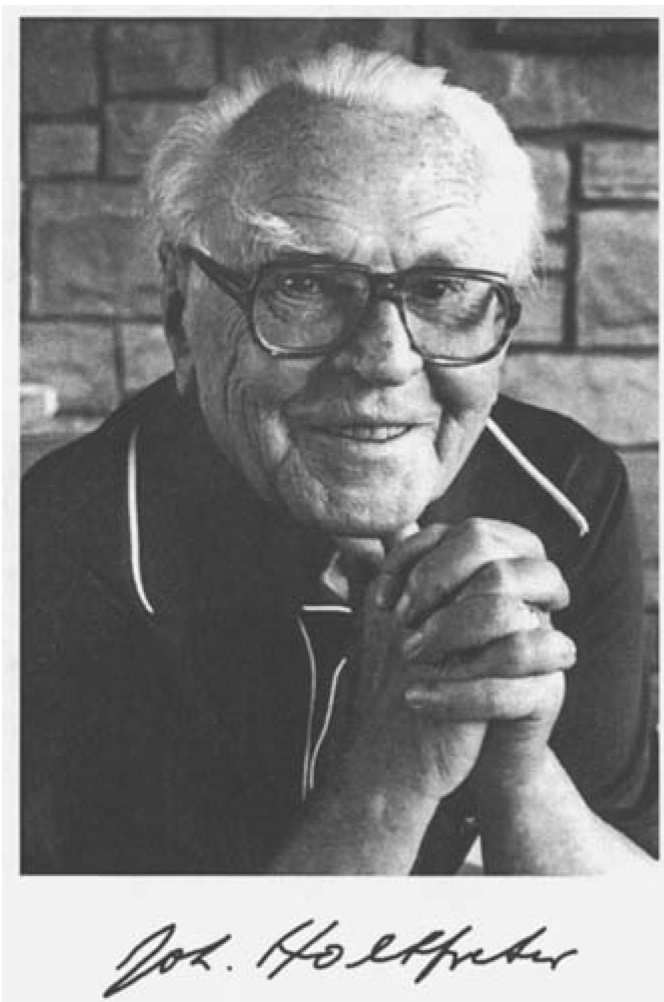
Comparison of alternative sire evaluation methods. *J Anim. Sci.* 41:760–70.

1976 A simple method for computing the inverse of a numerator relationship matrix used in prediction of breeding values. *Biometrics* 32:69–83.

Multiple trait evaluation using relatives' records. *J. Anim. Sci.* 43:1188–97.

1977 Best linear unbiased prediction of breeding values not in the model for records. *J. Dairy Sci.* 60:783–87.

Prediction of merits of single crosses. *Theor. Appl. Genet.* 49:273–82.
Prediction of future records. In *Proceedings of the International Conference on Quantitative Genetics*, pp. 615–38. Ames: Iowa State University Press.
1979 With C. R. Henderson, Jr. Analysis of covariance in mixed models with unequal subclass numbers. *Commun. Stat. Theor. Meth.* A8(8):751–87.
1984 *Applications of Linear Models in Animal Breeding*. Guelph, Ontario: University of Guelph.
ANOVA, MIVQUE, REML, and ML algorithms for estimation of variances and covariances. In *Proceedings of the 50th Anniversary of the Iowa State University Statistics Lab*, pp. 257–80. Ames: Iowa State University Press.
1985 MIVQUE and REML estimation of additive and nonadditive genetic variances. *J. Anim. Sci.* 61:113–21.
Equivalent linear models to reduce computations. *J. Dairy Sci.* 68:2267–77.



Courtesy of the University of Rochester Medical Center, Rochester, New York

Johannes Holtfreter

January 9, 1901–November 13, 1992

BY JOHN GERHART

JOHANNES HOLTFRETER WAS the world's foremost experimental embryologist in the decades between 1930 and 1960. His research was done entirely with amphibian embryos, the favored material of the time. He initiated and contributed substantially to many lines of experimentation that are still ongoing in the analysis of the embryonic organizer and of embryonic induction. For embryologists, his research shifted their view from the developing embryo as a supracellular organismal entity to the embryo as a complex population of interacting cells in which the numerous cells surrounding the organizer have a high competence for development, held in a latent state. The signals from the organizer mostly evoke or release this development, rather than provide detailed instructions for it. Our present-day concepts of secreted inductive signals, cell competence, and cellular morphogenetic activities sprang from Holtfreter's findings and insights.

Holtfreter's particular contributions include:

- The invention of Holtfreter's medium (a balanced salt solution in which operated embryos and clumps of embryonic cells survive and differentiate) and the introduction of sterile technique (1931).
- His discovery that dead and disintegrated organizer tissue could still induce locally organized parts of secondary axes (1932–38) and his findings that most tissues of embryos and adults of representative members of many animal phyla contain substances that induce neural development, findings that set off an international search for the true inducer.

- His improvement of the sandwich assay for inducers, by which the experimentalist can define the responding tissue and control its contact with inducing tissue or extracted test material (1933).
- The use of these conditions to test the autonomous differentiation capacity of small clusters of cells from various parts of the urodele or anuran gastrula embryo and the contribution of data to specification maps, competence maps, and distribution maps of head inducers and trunktail inducers in the early gastrula (1938).
- Discovery of conditions to produce urodele exogastrulae in which neural tissue does not form. These embryos provided evidence that the organizer may exclusively transmit neuralizing signals to the ectoderm by a vertical path in urodeles (1933).
- The use of interspecies (xenoplastic) grafting experiments (urodele-anuran) to demonstrate the species-specific competence of tissues to respond to the organizer's signals, yet the cross-species commonality of the organizers inductive signals (1935–36).
- Analysis of minimal conditions of pH extremes, Ca^{++} depletion, and hypotonicity (sublethal cytolysis) to obtain neural development in ectodermal fragments (1944–51).
- Analysis of the role of the notochord and somites in shaping the floor plate and walls of the neural tube (1933).
- Discovery of cell sorting and analysis of tissue affinities and tissue segregation in embryos (1939, 1955).
- Analysis of the three kinds of region-specific morphogenetic activities of cells in the amphibian gastrula and the integration of this information into a unified view of gastrulation (1942–43).

Holtfreter published approximately sixty papers in his career, and all but three were under his sole authorship. Many are still widely cited. Several of his techniques and modes of analysis have become standard practice in embryology, a subject now included in developmental biology. After becoming a U.S. citizen, he was elected to the National Academy of Sciences in 1955. Celebrations of his seventieth birthday and university retirement were accompanied by symposia and memorial volumes.

A recent issue of *Developmental Dynamics,* organized by Viktor Hamburger and Hazel Sive, was dedicated to his memory,[1] and I had the honor of contributing to that issue. A detailed account of Holtfreter's scientific contributions and aspects of his life has been prepared by his long-time colleague Viktor Hamburger in *The Heritage of Experimental Embryology*[2]. Holtfreter presented his own account in *A Conceptual History of Modern Embryology* (1991, pp. 109–28).[3]

EDUCATION AND EARLY LIFE

Holtfreter was born in Richtenberg, a small rural town in Pomerania in northeastern Germany on January 9, 1901, the second of three children and the single son. His father owned a prosperous whisky factory and rye fields. By Holtfreter's own account, he grew up in a stable supportive family and spent his early years collecting and drawing animals and butterflies. At the start of World War I, his family moved to Stralsund 20 miles away on the Baltic Sea, where he graduated from the Real gymnasium despite deteriorating conditions at the end of the war. As a student, he felt unsuited for mathematics, physics, and chemistry, yet he felt confirmed in his inclination as an incipient field biologist. He pursued natural science at the Universities of Rostock and Leipzig from 1917 to 1919 and then transferred to the University of Freiburg, attracted by the hiking and skiing in the area and by the possibility of working with a renowned naturalist on the faculty (Professor Doflein). However, the professor died shortly before Holtfreter arrived. His replacement was Hans Spemann, whose work as the preeminent embryologist of the time was unknown to Holtfreter. Nevertheless, Holtfreter began studying embryology and in 1924 received a doctoral degree in natural sciences based on thesis research completed in Spemann's laboratory. The thesis subject was the development of the liver and pancreas of the frog embryo, and Holtfreter commented that this subject was not of great interest to Spemann or himself.

During this time, he shared a laboratory bench with Hilde Mangold (nee Pröscholdt), who was in the process of discovering the amphibian gastrula organizer, a discovery later acknowledged in the award of the 1935 Nobel Prize to Spemann. In these experiments she extended a systematic study of Spemann's, which involved operating on gastrula stage amphibian embryos to remove small clumps of cells from various locations of one embryo and graft them into new locations in other embryos of the same age. Most clumps blended harmoniously into their new surroundings and joined the paths of development of cells there, giving a near-normal looking embryo. Hilde Mangold was to graft cell clumps from the dorsal lip of the blastopore, and the results with these cells were indeed different. When she transplanted these cells to the opposite side of a host, the host developed as a partial twin with a second embryonic body axis located at the site of the graft. The secondary axis contained a well-formed central nervous system and blocks of body muscles. A few tissues of the secondary axis derived from cells of the graft, but the nervous system and muscles were composed of cells of the host. The graft had certainly not blended harmoniously into its new surroundings; it had kept its own path of development and altered the paths of development of the surrounding cells. Later analysis confirmed that, in the presence of this particular graft, the nearby host tissues indeed developed along paths they would not otherwise have followed. The dorsal lip of the blastopore was called "the organizer" by Spemann, in recognition of its role in organizing the development of a body axis and central nervous system from cells surrounding it. Its influence on the surrounding tissues was called an induction. Thus, the nervous system was induced by the organizer. Spemann and Mangold published their landmark paper on the organizer in 1924 to great acclaim.

Although Holtfreter was to become the world's leading researcher on the organizer and on induction, he himself played no part in this discovery and felt that Spemann did not have a good opinion of his laboratory ability. He is said to have worked by night rather than by day and to have disappeared for long periods for hikes and outings with the Wandervögel. Neither behavior endeared him to the dedicated professor. After Holtfreter received his degree, Spemann suggested that he study marine biology at the famous Stazione Zoologica in Naples, and Holtfreter undertook this at his father's expense. In Naples, however, he avoided the laboratory, traveled throughout Italy (mostly on foot), and painted, finally settling in the small village of St. Angelo on the coast of Ischia. There, it is said, he oil-painted a large panel of the saint for the local church. On returning home to Stralsund after almost two years, he had no prospects for a research appointment. He tried portrait painting but to little effect. He traveled to Lapland

and wrote an account of his travels. He went to Helgoland and assisted at a marine biology institute, caring for the oyster beds. In the absence of a job prospect in Helgoland, he went to the University of Greifswald for a diploma to qualify him as a high school teacher. Balking at the prospect of high school teaching, he went to Holland in hope of getting a position in a botanical-zoological garden in Java but to no avail. By 1928 all prospects seemed exhausted. Then he received an invitation from Otto Mangold, chairman of a department at the Kaiser-Wilhelm Institute in Berlin-Dahlem, to accept a research position, and he took this with no hesitation. He began this position in 1928, four years after his degree, with but one scientific publication to his name, the 1925 presentation of his thesis research. Mangold, who had been a student and associate of Spemann (and widower of Hilde Mangold, who had died in a kitchen accident), knew Holtfreter's thesis work and training.

BREAKTHROUGH YEARS

Mangold left Holtfreter to his own research pursuits. Holtfreter chose to extend the organizer studies of Spemann's laboratory by addressing questions of how instructive the organizer is to surrounding cells versus how self-instructive are these cells regarding their choices of developmental paths. He entered a very productive and creative phase of his career, working until 2 a.m. or 3 a.m. almost daily (the laboratory had small attic rooms with bunks, where he lived), with several lines of experimentation conducted in parallel. He usually worked alone. He devised a balanced salt solution in which operated embryos and pieces of embryonic tissue could survive for periods of several weeks and differentiate, and he introduced sterile conditions to reduce bacterial infections. These techniques are now commonly used but were new at the time. (In the Spemann-Mangold experiments, only five of several hundred operated embryos survived infection and the hypotonicity of pond water.) With these conditions, Holtfreter undertook several revealing studies. The first (1931–38) was to see if the organizer retained its activities after being "devitalized" by heat, alcohol, drying, or freezing, or if its activities depended on its intact living structure as Spemann implied. Holtfreter soon showed that dead and partially extracted organizer material was strongly inductive, especially in eliciting neural development, including braining structures. He then tested a variety of embryonic and adult tissues from animals of diverse phyla and found that many tissues from many organisms and developmental stages release materials capable of neural induction. Surprisingly, agents with inductive activity were not unique to the organizer. Holtfreter soon saw the similarity of inducers to hormones, and this comparison has been upheld by modern studies, although the similarity may be more to growth factor proteins and their antagonists than to endocrine-type hormones. These discoveries set off an international search for inductive substances released either by the organizer or by heterologous sources such as chick embryo extract or HeLa cells. Joseph Needham and Conrad Waddington, who headed an English effort to isolate the inducer, visited the Berlin-Dahlem laboratory to learn techniques. Holtfreter's student H.-P. Chuang, was able to show that partial purification separates at least two kinds of inducers, one with neural inducing activity and one with mesodermizing activity. The evidence for two kinds of inducers was later incorporated by others into models of neural induction (by Nieuwkoop and by Saxen and Toivonen), which persist to this day.

Holtfreter also found conditions to produce exogastrulae in large numbers and used these to examine the path by which the organizer transmits neuralizing signals to the responding ectoderm (1933). He found that urodele embryos developing in a hypertonic salt solution retain a solid interior of cells because they fail to inflate the blastocoel. When gastrulation begins, the involuting surface cells have no internal space into which to move. Instead, they turn outward; the embryo exogastrulates. In particular, the organizer mesoderm pushes itself away from the ectoderm rather than rolling under it. Since the organizer of this exogastrula does not underlie the ectoderm, it cannot transmit inducing signals to it by a vertical path, that is, across planes of apposed tissue. However, even in the exogastrula, the organizer mesoderm and the prospective neural ectoderm remain connected across a planar boundary (which would become the limit of involution in the normal embryo). Thus, if a planar path suffices for the organizer to transmit inducing signals to the ectoderm (as Spemann considered possible), the exogastrula should still form a neural plate. As Holtfreter showed, the ectodermal cap of the exogastrula develops no neural tissue that can be detected by morphological criteria. Instead, it develops as a wrinkled atypical epidermis connected by a thin stalk to the mesoderm and endoderm. The result seemed to show clearly the indispensability of the vertical path and to eliminate the sufficiency of the planar path of induction. Although Holtfreter seemed to have settled this issue for the urodele embryo, it has arisen again in recent studies of *Xenopus* embryos, where some neural development does occur in exogastrulae and in planar explants. Some kinds of embryos may use both paths, whereas others may use only one path or the other.

Approximately twelve papers were published in the five years at Berlin-Dahlem. Holtfreter's artistic interests found expression in his numerous detailed drawings of embryos and differentiated explants for these publications.

UNIVERSITY OF MUNICH

The significance of Holtfreter's research became quickly recognized, and in 1934 he accepted an associate professorship at the University of Munich, in the Department of Zoology headed by Professor Karl von Frisch (the discoverer of the language of bees). His five years in Munich were also very productive, interrupted in 1935 by a one-year Rockefeller fellowship to work in the United States in the laboratory of Ross Harrison at Yale University. However, he did not undertake much laboratory work during the year. He had also received an unrestricted travel grant from a private donor (Dr. Gwinner), allowing him to tour the world first class by way of the western United States, Hawaii, Japan, China, and the Pacific islands. He spent several months in Bali, enjoying the music, arts, and dance, and engaging in painting and black-white scratchboard etching (a technique of scratching through a layer of India ink on chalkboard). Some colleagues feel he replenished his ideas and immense capacity for concentration during these periods away from the laboratory.

At the University of Munich, Holtfreter began a systematic study of the capacity of small pieces of the gastrula embryo to develop and differentiate in isolation in his balanced salts solution, that is, in the absence of signals from the organizer. This is currently called a specification test, and was then called a differentiation capacity test, or a test of the cells' state of determination. He found that cell clumps from some regions (such as the ectoderm) reliably differentiate only to epidermis, although they would form neural tissue and epidermis in the embryo. Hence, organizer signals of the neuralizing kind seemed stringently required for the development of ectoderm cells to nervous tissue, whereas the cells seemed self-instructed for epidermis development, a finding he probed more deeply a decade later. By contrast, small clumps of cells from the marginal zone mesoderm, which were expected to form somites in the embryo, would in isolation form not only somites but also notochord, neural tube, and epidermis, that is, much more than expected in the embryo in the presence of organizer signals. In fact, some of these explants developed as small bilateral embryoids. Thus, some regions seemed highly self-informed for paths of development and, if not wholly independent of the organizer's signals, were then perhaps inhibited in the embryo from developing their full range of capabilities. This work was done in both urodele and anuran embryos, with a very large number of cases. The two classic 1938 papers, in German, on the differentiation capacity of parts of the gastrula, have been recently translated into English by Viktor Hamburger.[4] The results led Holtfreter to suggest that, except in the case of the neural induction of ectoderm, the organizer does not provide detailed instructions for the differentiation of neighboring cells. The cells have extensive inherent capabilities of their own, and the organizer just evokes or releases these. As further evidence supporting this point, he made "sandwiches" of explanted ectoderm wrapped around an explanted organizer, using as tissue sources the embryos of different amphibian orders (urodeles, anurans). He found that the ectoderm gave a species-specific response to inducers, whereas the organizer's inducers seemed common to, and similarly distributed in, animals of both orders. This reinforced the conclusion that the type of differentiative response is defined extensively by the reacting tissue, and not only by the inductive source (1936–38). Around this time, he suggested that the term "organizer" may be misnomer.

Holtfreter also published several papers on the properties of inducers (1934–38), a subject of intense international attention, and this kept him in regular contact with Needham and Waddington in England. Holtfreter gave an invited presentation of his induction studies to a very large audience at the Congress of Physics, Chemistry, and Biology at the 1938 International Exhibition in Paris. Among his last experiments in Munich, he disaggregated cells of a neurula-stage embryo, mixed them together randomly and observed their extensive capacity to sort out, to selectively adhere, and to reconstitute well-organized tissues similar to those of the intact embryo (1939), a project he returned to after World War II.

When he returned to Germany in 1936 after his sojourn in Bali, he was concerned for his future, saying (1991), "I was full of hatred and disgust for the regime, but felt helpless. I knew that I was spied upon and sooner or later the Gestapo would get hold of me. I saw the war coming. In 1939, shortly before the war started, I managed 'by the skin of my teeth' to escape from Germany. Thanks largely to Joseph Needham, I found refuge in Cambridge." He was a guest lecturer at the Zoological Institute for a year. In 1940, when the German invasion of England seemed imminent, he was interned with thousands of German refugees and shipped to Canada, where he spent almost two years behind barbed wire.

MCGILL UNIVERSITY

In 1942 Holtfreter was released from internment. He found a research position at Montreal's McGill University, supported by a Rockefeller fellowship. At McGill, he began a study of the cell biological basis of gastrulation, one of the first systematic analyses of morphogenesis. He built on the 1929 work of W. Vogt to locate and characterize the various kinds of cell activities by which the lower half of the embryo (the mesoderm and endoderm) is internalized in this crucial period when egg organization is transformed into embryonic organization. He explanted single cells or clumps of cells from different regions and microscopically observed their movements and changes of shape in his culture medium. He also examined the surface coat of the cleaved egg, a coat seeming to hold the cells together. (More recently, it has been shown that this is not actually a coat, but an intercellular array of tight junctions and adhenens junctions located close to the embryo surface.) Holtfreter studied epiboly in ectoderm fragments, bottle cell elongation and bottle cell ingression in marginal zone explants, and convergent extension in organizer mesoderm explants. He favored the interpretation that bottle cells invaded the yolk mass and tugged other surface cells into the interior after them. At the same time, the layer of ectodermal cells expands to cover cells of the lower half due to the insertion of deep cells into a surface sheet of cells, thereby increasing its area. Although he observed the convergent extension of organizer cells, he gave this morphogenetic activity a lesser role in gastrulation than the tugging force of bottle cells; recent studies give it the primary role. He then integrated his findings in two major articles, "A study of the mechanics of gastrulation," Parts I and II (1943, 1944), which still serve as models for the ongoing analysis of morphogenesis.[5]

UNIVERSITY OF ROCHESTER

In 1946 Holtfreter accepted the offer of an associate professorship at the University of Rochester in the Department of Biology chaired by Professor Curt Stern (who had known him at Berlin-Dahlem). He was advanced to full professor in 1948, and he remained at Rochester until his retirement in 1969. Although he had several students at Rochester, he and they tended to publish their research independently. Holtfreter pursued several significant lines of analysis during the Rochester years. One line concerned autoneuralization and the question of whether the organizer's signals are really indispensable for neural induction. He began by confirming and extending the 1941 discovery by L. Barth that the ectoderm of some amphibian species (axolotl, *R. pipiens*) will develop neural structures if merely exposed to a saline solution. Holtfreter, of course, felt from his previous studies that his salts solution was inert and free of inducers. He found, however, that the gastrula ectoderm of certain amphibians (ones not previously examined by him) did respond to his medium by forming neural tissue, and that the ectoderm of yet other amphibians (ones he knew formed only epidermis in his medium) would also do this if briefly exposed to a slightly acidified or alkalinized solution before culturing in his medium. By optimizing the conditions, he could get the ectoderm to differentiate even brain vesicles with multiple sense organs (1947). This was a clear example of neural development without signals from the organizer and proof of the inherent capacity of ectoderm to develop into neural tissue if released to do so.

Holtfreter suggested that, although the ectoderm cells are inherently capable of neural development, this capability is self-suppressed in them (and hence epidermis develops unless inducers are added). He postulated that the sub-lethal conditions of the medium dissociate or inactivate the suppressive agent, allowing other agents to become active and initiate neural development of the tissue. This is autoneuralization. He also noted that since the differentiated tissue was locally well organized, spatially arrayed signals from an intact organizer must not be needed for fine grain pattern. The ectoderm had an inherent capacity to self-organize, at least on the local level of a brain vesicle and attending sense organ (though not on the larger scale of an entire nervous system). This work served to shift research attention from the inducer to the responding tissue as the source of specificity and organization. With these proposals Holtfreter may have reached his point of greatest departure from the 1924 views of the organizer held in Spemann's laboratory, namely, that its activity required the living intact state and that it provided detailed instructions to naive surrounding cells. Unfortunately for some researchers of 1947, Holtfreter's analysis of autoneuralization just emphasized the futility of studying induction and the organizer and confirmed for them the wisdom of switching to the upcoming field of molecular genetics. However, recent research has returned to these questions and has strongly supported Holtfreter's interpretation of the innate and suppressed capacity of the ectoderm for neural development, except that the current views hold that the suppression is enforced by intercellular, not intracellular, means.

Also during this Rochester period, his student, P. L. Townes, renewed and extended Holtfreter's provocative 1939 study of tissue affinity, using disaggregated cells from different germ layers of a neurula-stage embryo, which were then mixed and reaggregated randomly in different combinations. Ectodermal and endodermal cells segregated strongly from one another in these mixtures while adhering to like cells, eventually forming separate spheres. Mesodermal cells, by contrast, adhered to both ectoderm and endoderm and held them together in a three-layered arrangement, occupying the middle layer as mesoderm would in the intact embryo. Neural tube cells sorted to an intermediate position between epidermis and mesoderm and reconstituted a remarkably normal looking hollow neural tube. Townes and Holtfreter suggested that the directional migration of cells as well as the elective affinities of cells ruled the organization of these recombinates and of the normal embryo's germ layers. The Townes and Holtfreter paper of 1955, which offered cell biological explanations for embryological phenomena, is one of the best known of Holtfreter's papers.

In 1955 Holtfreter and Hamburger co-published a large chapter on amphibian development, summarizing the field at the time and including many original observations and points of emphasis. This remains essential reading for students of amphibian embryology. In that article and elsewhere, Holtfreter remarks on his dislike for concepts of supra-cellular organizing agencies, such as gradients, and on his preference for explanations involving local cell-cell interactions. In the 1944–56 period he had moved increasingly into the area of cell biology, then in its infancy, and published several papers on the cell membrane, the nucleus, and various organelles. After 1956 Holtfreter turned his attention increasingly to individual cell behaviors, including the aggregation of *Dictyostelium* amoebae and the differentiation of muscle cells in culture, little of which has been published.

Overall it has been said that Holtfreter's contribution to embryology was to move the studies of the organizer in an analytical and reductionist direction,[2] along which the supracellular and extra-embryonic interpretations of development were replaced with ones based on hormone-like secreted agents, cell-specific and stage-specific competence, release of latent capacities by inducers, cell sorting, cell shape change, cell migration, and cell interactions in populations. Although this cell-centered view is now taken for granted in cell biology and developmental biology, it was a rare and penetrating view in Holtfreter's time. From our vantage point of fifty years, it seems as if Holtfreter brought to light an individualistic and anti-authoritarian view of the embryo in which competent responsive cells interact in a self-organizing community, in place of conceptions of the embryo as a collection of naive passive members dependent for their future on detailed directions from a central organizer. The term "organizer" was laden with connotations from the realm of human activity.

Among his students and research associates, in addition to H.-P. Chuang and P. L. Townes, have been A. Haggis, N. Cohen, L. Stevens, C. Loeffler, and W. B. Muchmore. Holtfreter was known as a demanding mentor who held high standards for experimentation and brooked no nonsense in the discussion of research results. Hiroko Ban was his last student. They were married in 1959. Her 1965 thesis on the autonomous differentiation of subregions of the organizer contains a wealth of information. It was never published in journal form, but it is available as a microfilmed thesis and in outline in Viktor Hamburger's book.[2]

Johannes Holtfreter retired from the University of Rochester in 1968 and continued as Tracy H. Harris emeritus professor of zoology until approximately 1981. In his last years he was in good health except for the decline and eventual loss of his eyesight, which precluded any writing or painting. He died in Rochester at the age of ninety-one. Hiroko Ban-Holtfreter continues to live in Rochester.

MOST OF THIS MATERIAL is taken from published accounts by Viktor Hamburger and by Holtfreter himself. The author did not have the opportunity to know Johannes Holtfreter personally, but he has greatly admired Holtfreter's research contributions for many years. It is the fate of nonagenarians that few of their contemporaries survive who could have written a more personal account.

NOTES

1. *Dev. Dynamics*, vol. 205, no. 3, 1996.

2. V. Hamburger. *The Heritage of Experimental Embryology*. New York: Oxford Press, 1988.

3. *A Conceptual History of Modern Embryology*, ed. S. F. Gilbert, pp. 109–28. Baltimore: Johns Hopkins Press, 1991.

4. *Dev. Dynamics*, vol. 205, no. 3, 1996, pp. 217–44.

5. See discussion of Holtfreter's work on gastrulation by R. Keller, *Dev. Dynamics* vol. 205, no. 3, 1996, pp. 257–64.

Selected Bibliography

1925 Defect- und Transplantationsversuche an der Anlage von Leber und Pancreas jüngster Amphibienkeime. *Roux' Arch. EntwMech. Org.* 105:330–84.

1931 Über die Aufzucht isolierter Teile des Amphibienkeimes. II. Züchtung von Keimen and Keimteilen in Salzlösung. *Roux' Arch. EntwMech. Org.* 124:404–66.

1932 With H. Bautzmann, H. Spemann, and O. Mangold. Versuche der Analyse der Induktionsmittel in der Embryonalentwicklung. *Naturwissenschaften* 20:971–74.

1933 Eigenschaften and Verbreitung induzierender Stoffe. *Naturwissenschaften* 21:766–70.

Der Einfluss von Wirtsalter und verschiedenen Organbezirken auf die Differenzierung von angelagertem Gastrulaektoderm. *Roux' Arch. EntwMech. Org.* 127:619–775.

Nachweis der Induktionsfähigkeit abgetöteter Keimteile. Isolationsund Transplantationsversuche. *Roux' Arch. EntwMech. Org.* 128:584–633.

Die totale Exogastrulation, eine Selbstablösung des Ektoderms vom Endomesoderm. Entwicklung und functionelles Verhalter nervenloser Organe. *Roux' Arch. EntwMech. Org.* 129:669–793.

1934 Der Einfluss thermischer, mechanischer, und chemischer Eingriffe auf die Induzierfähigkeit von Triton-Keimteilen. *Roux' Arch. EntwMech. Org.* 132:225–306.

Über die Verbreitung induzierender Substanzen und ihre Leistungen im Triton-Keim. *Roux' Arch. EntwMech. Org.* 132:307–83.

1935 Morphologische Beeinflussung von Urodelenektoderm bei xenoplasticher Transplantation. *Roux' Arch. EntwMech. Org.* 133:367–426.

1936 Regionale Induktionen in xenoplastisch zusammengesetzen Explantaten. *Roux' Arch. EntwMech. Org.* 134:466–550.

1938 Veränderung der Reaktionsweise im alternden isolierten Gastrulaektoderm. *Roux' Arch. EntwMech. Org.* 138:163–96.

Differenzungspotenzen isolierter Teile der Urodelengastrula. *Roux' Arch. EntwMech. Org.* 138:522–656.

Differenzungspotenzen isolierter Teile der Anurengastrula. *Roux' Arch. EntwMech. Org.* 138:657–738.

1939 Gewebeaffinität, ein Mittel der embryonalen Formbildung. *Arch. Zellforsch.* 23:169–209.

1943 A study of the mechanics of gastrulation. Part I. *J. Exp. Zool.* 94:261–318.

1944 A study of the mechanics of gastrulation. Part II. *J. Exp. Zool.* 95:171–212.

Neural differentiation of ectoderm through exposure to saline solution. *J. Exp. Zool.* 95:307–40.

1945 Neuralization and epidermization of gastrula ectoderm. *J. Exp. Zool.* 98:169–209.

1947 Neural induction in explants which have passed through a sublethal cytolysis. *J. Exp. Zool.* 106:197–222.

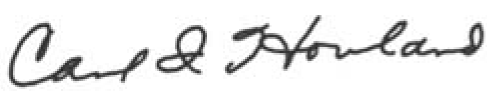
1948 Concepts on the mechanism of embryonic induction and its relation to parthenogenesis and malignancy. *Symp. Soc. Exp. Biol.* 2:17–48.

1951 Some aspects of embryonic induction. *Growth* 3(suppl.):117–52.

1955 With V. Hamburger. Amphibians. In *Analysis of Development,* eds. B. H. Willier, P. A. Weiss, and V. Hamburger, pp. 230–96. W. B. Saunders.

With P. L. Townes. Directed movements and selective adhesion of embryonic amphibian cells. *J. Exp. Zool.* 123:53–120.

1991 Reminiscences on the life and work of Johannes Holtfreter. In A *Conceptual History of Modern Embryology,* ed. S. F. Gilbert, pp. 109–27. Baltimore: Johns Hopkins University Press.



Carl Iver Hovland

June 12, 1912—April 16, 1961

BY ROGER N. SHEPARD

YALE PSYCHOLOGIST Carl Hovland made singularly important contributions to experimental, social, and cognitive psychology (focusing respectively on human learning, attitude change, and concept acquisition). In the process he worked unremittingly "to improve the standards and quality of research in psychology and related fields," earning (in the words of one of his longtime coworkers) universal recognition as a "statesman of the social sciences" (Janis, 1968, p. 530).

Hovland also served as an insightful and trusted consultant to numerous governmental and educational agencies, industrial organizations, and philanthropic foundations. All this he did within a life lasting not quite forty-nine years. He could hardly have foreseen how limited would be the time available to him (both his parents lived into their nineties). Yet he compensated, in effect, through his remarkable precocity, quickness of mind, and productive use of every waking moment—along with his extraordinary ability to bring together bright young researchers with widely differing theoretical perspectives, to provide them with support and subtle guidance, and to formulate coherent syntheses of the emerging results. A man of unsurpassed gentleness and moral integrity, he left a deep and permanent mark on everyone who knew him.

MY OWN RECOLLECTIONS OF HOVLAND

I first met Carl Hovland when I arrived for graduate study in Yale's illustrious Department of Psychology in the fall of 1951. Hovland's title, Sterling professor, seemed wonderfully euonymous for this tall, distinguished man, endowed as he was with rare personal qualities and wavy hair turning to silver. Now, over forty-five years later, I am astonished to realize that this revered member of the department, who had been serving as chairman of the department and director of the Laboratory of Psychology, was at that time only thirty-nine years old!

Particularly striking were the apparent ease and efficiency with which Hovland managed all the many things in which he was always engaged and his constructive use of every moment of time. While showing genuine interest in everyone with whom he had contact, he had a way of keeping administrative interactions brief and to the point. His extraordinary memory enabled him to carry out much of the department's business through chance meetings in the hall or stairway—venues that minimized the risk of someone plunking down in a chair in his office for more than the time needed to resolve whatever issue was at hand. If Hovland did not encounter a graduate student sufficiently soon concerning some matter, the student would find a slip of paper in his or her departmental mailbox with the succinct notation: "See me. CIH." More than once, discussions of my own research were carried out as I tried to keep up with Hovland's rapid stride to the New Haven railway station where he would be catching a train to New York—perhaps to consult with AT&T, Bell Laboratories, or the Rockefeller or Russell Sage Foundations.

On those occasions when I did actually sit down in Hovland's office, he would also be reading his mail and talking with someone else on the telephone. When I called him on the phone, I could hear someone else in his office and the occasional rattle of a letter being opened. And, when I sent him a note, I imagined that while he was perusing it, he would also be talking with someone in his office and someone on the phone. I fantasized having the delivery of my written letter, the playing over the phone of my recorded voice, and my physical entrance into his office converge upon him simultaneously—thus gaining, for once, his undivided attention! In truth, however, I welcomed the brief hiatuses that Hovland's time sharing entailed as I was striving to marshal my ideas for his assessment.

Another Hovland student, Herbert C. Kelman (now Cabot professor of social ethics at Harvard), described to me how the drafting of his 1953 paper with Hovland began: "In consultation with Carl, I designed and carried out an experiment on the sleeper effect [in which the tendency to endorse a proposition from a low credibility source increases as the source is forgotten]. When the data were collected and analyzed, I … told him that I would like him to coauthor the article reporting the research. In his customary generosity, he told me that this was my experiment and he was not expecting coauthorship. But I insisted—whereupon he pulled out a yellow pad and started writing! Right then and there!" (Kelman, letter of March 25, 1997).

Hovland was the most efficient and organized individual I have ever known. But the efficiency and organization was all in his head; it did not depend on external aids. He conducted classes and chaired meetings in his quiet, informal manner without notes, while the desk and side table in his office remained piled with papers in no visible order. When another of my fellow graduate students inquired whether he might retrieve a term paper to correct an error, Hovland briefly rummaged through papers piled on the side table. Then turning to my waiting friend, he remarked, "You may think there is no order here. Actually, there is an order; it's just not an order designed to meet that particular type of request." And order there evidently was; Hovland's secretary, Jane Olejarczyk, told me, "Quite often he would call and ask me to retrieve some document with instructions like: it's in the third pile from the left on the table by my desk, about a third of the way down, and there's a Russell Sage report, printed on blue paper, just before you get to it … Amazing! He was always on target" (personal communication of May 29, 1997).

Hovland was a master of the Socratic method. Seemingly without any prepared agenda, he would ask the graduate students around the seminar table for their comments on the (always seminal) readings he had assigned, or for their proposals concerning an illustrative problem of experimental design or data analysis he was working through on the chalk board. At first, this evoked frustration or anxiety in students accustomed to more structured styles of instruction. (A student who had volunteered to calculate—in those days, by means of a slide rule—a number called for by the illustrative problem might find that, before he or she was able to come up with the answer, Hovland was already writing it on the board, apparently having arrived at it by his own swifter, purely mental calculation.) Former Yale student Philip Zimbardo (now a professor of social psychology at Stanford) remarked that the combination of Hovland's shyness and intellectual mastery may have prevented him from even suspecting that some students found him intimidating (personal communication of April 3, 1997). Nevertheless, out of our bumbling efforts a coherent picture would gradually crystalize, to be succinctly articulated by Hovland at the end of each class session. It was the goal toward which Hovland evidently had been subtly guiding us all along.

I asked Hovland to serve as my dissertation advisor not only because I valued his quick intellectual grasp but also because he seemed uniquely free of commitment to any particular theoretical position and, hence, supportive of the exploration of promising ideas, wherever they might lead. Because of the great respect everyone had for him, Hovland was also able to give my career a couple of unexpected boosts at its very start. He endorsed the suggestion of a younger member of my dissertation committee, Burton Rosner, to take the unusual step of recruiting a mathematical psychologist from outside Yale to serve on the orals committee of my more-than-usually mathematical dissertation. One consequence was that the up-and-coming outside examiner selected, George A. Miller, invited me to join him a year later as a postdoctoral associate at Harvard. Then, following those two postdoctoral years, both Hovland and Miller recommended my appointment as a member of technical staff in a small basic research group that Hovland had been instrumental in establishing in the Bell Telephone Laboratories, Murray Hill, New Jersey. The research I was able to carry out during my two postdoctoral years at Harvard (where I first learned to program on the Univac 1, just given to Harvard) and during the next eight years at the Bell Labs (where I had access to a major computer facility) undoubtedly contributed to my own ensuing appointment to a professorship at Harvard.

In 1957 I participated—along with both Miller and Hovland—in a Summer Institute on the new computer simulation approach to modeling human cognitive processes organized by Alan Newell and Herbert Simon at the RAND Corporation in Santa Monica. Simon, who remembers Hovland "with great fondness," mentioned that Hovland

and Miller had "co-opted" him to join their small ad hoc committee of the Social Science Research Council, which had some Ford Foundation money for work in cognition. It was this money, Simon said, that made possible their Summer Institute (personal communication of May 27, 1997). Over a lunch with Hovland in Santa Monica that summer, I recalled how my doctoral research at Yale only two years before had necessitated my approximation of the eigen roots and vectors of matrices by hours of tedious computation on mechanical desktop calculators. "When," I wondered, "would Yale obtain a programmable electronic computer?" With a wry smile, Hovland replied that he was on a committee that had just been established at Yale to receive the gift of such a computer—in case one should be offered! Only three years later, the 1960 papers on computer simulation of thinking and concept attainment authored by Hovland, alone and with his student Earl Hunt, were already appearing.

It was shortly after joining the Bell Labs that I began my one direct research collaboration with Hovland. Herbert Jenkins and I had undertaken a study of classification learning in which human subjects learned by trial and error which of two responses was correct for each of the eight possible stimuli having either of two values on each of three binary dimensions (for example, square or triangular, large or small, and black or white). Jenkins and I sought to determine the number of trials required to learn different classifications in which correct responding required taking account of values on just one, on two, or on all three of the stimulus dimensions.

When we mentioned this study to Hovland, we learned that quite independently, he and two research assistants had just begun presenting subjects with explicit classifications of just such binary-valued stimuli into two groups of four (one displayed on the left, the other on the right). They, however, were measuring subjects' speeds and accuracies of reconstruction of the two groups from memory, and recording how the subjects described the rules they found to govern each classification. We quickly agreed to join forces and, during our ensuing collaboration, Jenkins and I (often together with the Bell Labs learning researcher Ernst Rothkopf) would meet with Hovland—usually at his home in Hamden, outside New Haven.

On these visits, the Hovlands' longtime housekeeper Elizabeth would serve us lunch, elegantly presented with fine china, silver, and linens in the Hovland's formal dining room. I must have been seated in Mrs. Hovland's customary place. For, under a slight bump in the rug there was a button that I sometimes inadvertently hit with my foot, summoning the housekeeper, to my mounting chagrin.

At about this time, a growth in Hovland's neck (in the parotid gland just below his right ear), which had been diagnosed as benign some years earlier, had recurred and was now determined to be malignant. Both the advance of the cancer and the measures undertaken for its treatment (surgery, radiation, and a then highly experimental chemotherapy) were soon exacting a toll on Carl's previously inexhaustible energy, entailing a temporary loss of his full head of hair, which had rapidly turned entirely white, and a total deafness in his right ear.

Long before, Carl's wife Gertrude, like himself, still relatively young and universally regarded with admiration and affection, had been increasingly afflicted with rheumatoid arthritis. Anticipating his own impending death, Hovland became deeply concerned about his wife's growing helplessness. Her neck was now so fragile that she had to wear a neck brace whenever she was up and about.

On August 26, 1960, my two colleagues and I made our last scheduled trip from the Bell Labs to the Hovlands' home to discuss the final stage of our collaborative project. We were met at the door by housekeeper Elizabeth, who, tearfully and barely able to speak, informed us that Mrs. Hovland had an accident earlier that morning and that Mr. Hovland would not be able to meet with us. We got in the car and headed back to New Jersey.

I later learned that Gertrude, having gotten out of bed without her protective collar, stumbled and fell. Her weakened neck snapped and she died instantly. A few days later, Carl called me to apologize for not being able to meet with us after our long drive. When I assured him that no apology was necessary and expressed my heartfelt sympathy, he became, for the only time in my experience, choked with emotion and was briefly unable to speak. The loss of his beloved wife was a terrible blow to this most caring and responsible of men —left, as he now was, with two children in their late teens and with less than a year remaining of his own life.

Right up to the end, Hovland continued doing (to the extent that he was physically able) just what he had been doing even before he learned that he was mortally ill. Apparently, Hovland had always proceeded each day with what he regarded as most important—as if that day might be his last. To avoid the stairs, his final weeks were spent in a bed that had been set up in the same dining room where my colleagues and I used to talk with him over lunch. He was cared for by his son David, then an undergraduate at Yale, and by his daughter Kathie, who, having just entered Wellesley College, traveled down from Massachusetts to be with her father during the weekends. Carl died on Sunday night, April 16, 1961, just after Kathie left for her trip back to Wellesley. Coincidentally, problems arising in their necks had cut short the lives of Carl and Gertrude alike, near the ends of their forty-eighth years.

The 1961 Shepard, Hovland, and Jenkins study "Learning and Memorization of Classifications" appeared in the *Psychological Monographs* in that same year—but not in time for Hovland to see it in print. Along with Hovland's own last book *Social Judgment,* written in collaboration with Muzafer Sherif (who completed it after Hovland's death), our monograph was thus one of the last publications on which Hovland appeared as an author. Some thirty years later, this monograph attracted renewed interest among cognitive scientists, who have used our results to test alternative connectionist or "neural net" models for classification learning; or to elucidate the roles of stimulus dimensions called perceptually "separable" (like size and shape—as in Shepard, Hovland, and Jenkins, 1961, p. 3) versus those called perceptually "integral" (like lightness and saturation of colors—as in Shepard and Chang, 1963, p. 96). And the three students who served as research assistants in this work—Albert Bregman and Earl Hunt (with Hovland) and John Gibbon (with Jenkins and me)—have all gone on to make their own influential contributions at three major universities (Bregman in auditory perception at McGill, Hunt in human cognition at the University of Washington, and Gibbon in timing behavior at Columbia).

FAMILY HISTORY

Carl Iver Hovland was born in Chicago on June 12, 1912, to two Lutherans of Scandinavian descent who, unlike Carl, both survived into their nineties—Ole C. Hovland (1871–1967) and wife Augusta Anderson Hovland (1876–1970). Carl's younger brother Warren described both parents as "deeply religious." Augusta had immigrated alone from Sweden at the age of twelve, and had never had any further formal education. Ole had grown up on the Minnesota farm of his immigrant parents—Iver Christenson

Hovland, who had been a shoemaker in Norway, and Marit Olsen Schjeie, whom Carl's older brother Roger described as "a sharp, quick-witted Norwegian lady, proud of her ten children." Carl's father Ole left the family's Minnesota farm to become an electrical engineer and inventor in Chicago. The traits for which Ole is commended in an article in the *Bulletin of Automatic Telephone Engineers* are similar to those that everyone came to admire in his son Carl. One of Carl's two brothers (long-lived like their parents), Roger (1907–94, six years older than Carl) followed his father into an engineering career, and C. Warren (born 1918, six years younger than Carl) became a professor of philosophy and religion and chair of the Department of Religious Studies at Oregon State University, where a building is named "Hovland Hall" in his honor.

Carl's son David Alan Hovland (born July 18, 1941) and his daughter, now Katharine Hovland Walvick (born December 12, 1942), both manifest intellectual aptitudes reminiscent of their father's. David obtained his Ph.D. in psychology from Harvard where I, who had been his father's advisee at Yale, served in turn as David's advisor until I moved to Stanford in 1968. David and his wife Carol now live in Austin, Texas, where David is a professor at Park College. Kathie received a Wellesley B.A. in mathematics and became at one time the youngest woman life master at bridge. She represented the United States in several bridge olympics around the world, winning Bronze Medals in the Canary Islands and Geneva. She and her attorney husband Walter now live in McLean, Virginia, outside Washington, D.C., where she is senior legal editor for Dickstein Shapiro Morin & Oshinsky LLP. David and Kathie each have one son and one daughter, all now grown.

A cousin, Mary Hovland Jenni, though never having met Carl, developed a keen interest in him and his work while pursuing her own doctoral studies in psychology at the University of Montana in the 1970s. She contacted several of Carl's family members and former colleagues, asking for their recollections of him. Much of my information about his family and childhood comes from her unpublished report (Jenni, December 1974). Carl was described, she said, as "a brilliant child, shy, quiet, introverted, unathletic, troubled by illnesses." Carl's first-grade teacher reportedly said that Carl "lived in his own dream world and did not relate to the group" (Warren Hovland's letter to Jenni of November 4, 1974). Everyone agreed that Carl found satisfaction in learning and scholastic achievement, and many spoke of the early emergence of Carl's love of music and his impressive proficiency on the piano. During college, Carl partly supported himself as an organist for the Lutheran church, though his formal association with the church otherwise ended during this period.

It was a shared love of music that brought together Carl and Gertrude Raddatz, his wife-to-be. Gertrude was born in Chicago on September 13, 1911, the first of five children. Carl and Gertrude both attended Chicago's Luther High North, studied piano with the same teacher (Esther Kittlesby), and enjoyed playing piano and organ duets. Gertrude went on to study piano at the American Conservatory in Chicago and then to teach piano—until her hands became too crippled by her rheumatoid arthritis. Carl and Gertrude were married on June 4, 1938, when Carl (whose mother reportedly had told her sons that a "boy" should not marry until he was thirty) was about to turn twenty-six.

Manifesting the engineering aptitude of his father and older brother, Carl experimented with 3-D photography and designed and built his own high-fidelity systems. He developed such expertise in sound reproduction that his advice was reportedly sought by professional audio engineers. (Once, while I was still a graduate student, Carl took obvious pleasure in inviting me to challenge his new system's capabilities with selections from his extensive collection of classical records. It was my first exposure to the just perfected stereo reproduction of sound and to the astonishing realism it could achieve.)

Until the untimely deaths of both parents, the Hovland home—in addition to being filled with music—seems to have been a consistently warm and supportive one. Kathie wrote to me of her "strongest feelings" about her father — "awe and pride in his brilliance and his accomplishments, joy in the tender memories of our togetherness (including playing piano duets, my 'helping' with his experiments … discussing everything from my academic goals and achievements to my boyfriends, listening to operas from the Met on the radio on Saturday afternoons, and my driving him to New York to Sloan Kettering Institute for cancer treatments), and admiration for his proud, quiet strength and courage (especially after my mother died and toward the end of his life)." She concluded, "I have nothing but superlatives to say about my father. He was the very best!" (letter of August 23, 1988).

PROFESSIONAL HISTORY

As an undergraduate at Northwestern University, Hovland acquired a strong background in mathematics, physics, and biology, as well as in experimental psychology, receiving his A.B. with highest distinction in 1932 (just before turning twenty). On a Catharine White fellowship he also obtained his A.M. there in 1933 and completed research that appeared in his earliest published papers (the first, coauthored with a stimulating new Northwestern faculty member G. L. Freeman on "diurnal variations in performance and related physiological processes").

Concerning a letter recommending Hovland for graduate study, Yale's Walter R. Miles recalled that, "The letter's language of so high approval and praise was such as to make [the] Yale professors smile and shake their heads. As events evolved they were using similar language in … recommending the same Carl Hovland … a very few years later" (Miles, 1961, p. 122). Hovland prepared six papers for publication during his first year and in just two more years he received his 1936 Ph.D. with honors under the prominent Yale learning theorist Clark L. Hull.

Hovland's dissertation provided the first evidence for a law of generalization, in which the tendency to make a response learned to one stimulus falls off exponentially with the distance separating a test stimulus from the original training stimulus along a sensory continuum, such as the continuum of auditory pitch (Hovland, 1937). Beginning with my own dissertation twenty years later, I developed a new approach that provided more definitive evidence for such a law (Shepard, 1958, Figure 2) and, thirty years still later, a theoretical justification for the law's possible "universal" character (Shepard, 1987, Figures 1 and 3). Such a law of generalization was also central to the interpretation of the results of our joint study of classification learning (Shepard, Hovland, and Jenkins, 1961, pp. 25–30). I still regard generalization as the most fundamental problem of human, animal, and machine learning—if not, indeed, of education and cognitive science generally.

On completing his dissertation, Hovland was immediately invited to join the Yale faculty, of which he remained a member for the rest of his life. Two 1940 publications illustrate the extraordinary range of his early work at Yale. As part of an interdisciplinary group investigating the connection between frustration and aggression, Hovland and Robert Sears (1940) discovered a substantial (negative) correlation, over a century of U.S. history, between economic indicators (such as the price of cotton) and number of lynchings. At the same time, according to one of his later coworkers, M. Brewster Smith, Hovland served as the "heavy hitter" on the team of Hull, Hovland, et al. that produced the 1940 monograph "Mathematico-Deductive Theory of Rote Learning" (Smith, personal communication of 1997). This book, though too technically demanding to have been read by many psychologists, has been deemed "as elegant a volume as ever published in psychology" by a later Hovland student who decided to pursue a career in psychology after "stumbling upon that volume in [his] undergraduate browsing days" (McGuire, 1996, p. 46).

From 1942 to 1945, during America's involvement in World War II, Hovland was on leave from Yale. Recruited by the noted sociologist Samuel Stouffer (himself on leave from the University of Chicago), Hovland headed the Experimental Section of Stouffer's Research Branch under Major General Frederick Osborn's Information and Education Division of the War Department. The primary mission of Hovland's section was to evaluate the training programs and films being prepared by the Information and Education Division for American troops in the United States and Europe. Hovland was responsible for guiding and synthesizing the work of some fifteen researchers.[1]

Despite his wartime leave, Hovland rose meteorically at Yale through the ranks of instructor (1936), assistant professor (1937), director of graduate studies (1941, at age twenty-nine), associate professor (1943, in absentia), full professor, chairman of the psychology department and director of the Laboratory of Psychology (1945, at age thirty-three), to Sterling professor (1947, at age thirty-six). Indeed, Hovland and his twenty-eight-year-senior mentor Clark Hull were both named to Sterling professorships in 1947. I was told that this made psychology the only department at that time with two Sterling professors and that this came about because Hovland, in his characteristic generosity and sense of fairness, would not accept the honor in advance of his mentor.

Beginning with his research during the war, Hovland brought the methodological talents he had honed in his experimental work on learning and generalization to bear on problems of communication and social psychology. He and a number of those who had worked with him in the Research Branch prepared a series of volumes titled "Studies in Social Psychology in World War II." Hovland was the senior author of volume 3, the highly influential 1949 *Experiments on Mass Communication*.

After returning to Yale, Hovland established the "Yale Communication and Attitude Change Program." With the help of the Rockefeller Foundation, this program supported for over fifteen years (until Hovland's death) research by Hovland and over thirty coworkers and students.[2] This work established how verbally presented information changes (or renders resistant to change) a recipient's opinions and beliefs as a function of experimentally manipulated variables, such as the recipient's prior position on an issue, the recipient's self-esteem, the credibility of the source, the extremity of the position advocated, the order of presentation of arguments, whether one or both sides of the issue are presented, whether the conclusions of an argument are explicitly stated or are left to the recipient's inference, whether the recipient actively attempts to reproduce the arguments for someone else, whether the recipient is induced to think of counter arguments, whether the presented information is designed to elicit the recipient's emotions (especially fear), the time that has elapsed since the information was presented, and the conditions imposed at the delayed time of assessment of attitude change (for example, whether knowledge of the forgotten high or low credibility source is reinstated).

Following Hovland's death, his attitude change program was characterized as "the largest single contribution [to the field of social communication] any man has made (Schramm, 1963, p. 5). Over thirty years later, it was still deemed "the biggest single force within psychology's communication-relevant attitude-change movement" (McGuire, 1996, p. 43), and as "the gold standard for research in social psychology" (Timothy Brock, personal communication of May 20, 1997). Zimbardo has suggested that the secret of the success of this program lay in Hovland's unique conceptual ability to decompose the complex relations between persuasive communications and attitude change in a way that rendered them susceptible to controlled laboratory experiments. Moreover, by "establishing a structural-sequential mode of the input-mediating-output variables and processes involved, Hovland anticipated the later information processing approach that proved so valuable in cognitive psychology (Zimbardo, personal communication of June 9, 1997).

Hovland also played a crucial role in the formation of what became the Bell Telephone Laboratories' Behavioral Research Center, of which I was a member from 1958 to 1966. It was, I believe, the longest lived of any group whose members were given the freedom to pursue basic psychological research within an industrial setting. According to William A. Baker, former president of Bell Labs, the establishment of this group came about when Robert Greenleaf of the personnel department at AT&T and Baker (then vice-president for research at the labs) decided that in view of the vast number of employees that the Bell System trains every year and the even vaster number of customers that daily interact with the telephone system, a small self-sustaining group of behavioral scientists might be justified within a large laboratory traditionally oriented toward the physical sciences. They turned to Hovland, whose earlier work in industrial psychology had impressed them with its "ingenuity" and "versatility." Baker said, "Carl achieved an extraordinary rapport with our industrial endeavor" (personal communication of May 11, 1995).

Hovland recruited two former students of the brilliant MIT social psychologist Kurt Lewin to establish strengths in both basic and applied social psychology—Morton Deutsch and Alex Bavelas. But Bavelas (who had been selected to lead the applied effort) did not stay long, whereupon a struggle ensued between Bell Labs and the personnel department of AT&T about whether the new group should be oriented toward basic or applied research. Hovland "played the pivotal role … in the decision to support its basic research orientation," said Deutsch, who warmly recalled "Carl's intellectual openness, personal support, and his skills as a mediator of conflict" (personal communication of March 24, 1997).

During the ensuing years, the Bell Labs' Behavioral Research Center attracted a number of creative young psychological researchers. Some time after Hovland's death, when changing circumstances led Bell Labs (and many other companies) to curtail support for basic research, virtually all of these scientists were able to move to professorships at major universities. Indeed, despite its relatively small size, this center has had as many as five of its members elected to the National Academy of Sciences.[3]

The area to which Hovland turned his attention toward the end of his life concerned the cognitive processes of concept acquisition, problem solving, and thinking. During the few years left to him, he advised or collaborated with at least ten researchers in this increasingly active area.[4]

The letters Mary Jenni received in response to her 1974 inquiries to Hovland's former colleagues are remarkably consistent in their expressed admiration of Hovland's intellectual powers, his administrative efficiency, the moral quality of his judgments and actions, and the affection everyone felt for him.

Leonard W. Doob, who was a young member of Yale's faculty when Hovland arrived there in 1934, wrote, "Clearly he was the outstanding student of the year, coming here with a tremendous recommendation from Northwestern." Even when he had joined the faculty, Doob said, Hovland was "shy and self-contained; you never quite knew what he was thinking. His IQ was incredibly high. He was a very efficient administrator; the details, externally at least, never seemed to bother him because he dealt with them so quickly and apparently painlessly" (letter of November 4, 1974).

Robert R. Sears, who had been on the Yale faculty with Hovland between 1936 and 1941 (though David Star Jordan professor of social science at Stanford when he responded to Jenni's 1974 request), wrote, "Carl was a big man in every respect. He was very gentle and … very musical. He was a cheerful, smiling person who came into the office every morning and put his head in my door and said 'what's new?' We both had classes over on the main quadrangle … at 11:00 … He walked so fast that … I got to class … puffing and panting while Carl went up to a second floor lecture room, bounding two or three steps at a time … He was a wonderful guy … At our house he would sit and play with my son David, who was then about a year old." Sears's letter concluded, "He was a remarkable man, brilliant in every sense of the word, and a delightful friend and warm companion" (letter of October 28, 1974).

Incidentally, Sears's son David later went on for graduate study with Hovland and became a professor at UCLA. About Hovland (who died during David's last year at Yale), David told me, "He took me into his home for several days after I was released from the hospital following an appendectomy … I treasure the memories of the times I [spent] with him, in class and out." An incident that David recalled well illustrates Hovland's mixture of warmth, shyness, and propriety: "One year a group of students went to the Hovland house to sing Christmas carols, as a gesture of appreciation; we saw Carl hasten to run upstairs to put on a coat and tie before coming to the door to greet us" (personal communication of May 19, 1997).

Leland DeVinney, one of Hovland's associates in Washington during the war, later became director of social science at the Rockefeller Foundation, which provided much of the support for Hovland's attitude change program at Yale. He wrote, "In the field of communication and attitude formation … [Hovland] is recognized as the leading pioneer." Concerning Carl and Gertrude, he said, "My wife and I … have never known lovelier or finer people," and added, "I also have known many of Carl's associates and students and know that Carl was an extraordinary teacher and research guide. He was highly respected and also loved by all of them" (letter of November 9, 1974).

Donald R. Young, another of Hovland's associates during the war, who later became director of the Russell Sage Foundation on whose Board of Directors Hovland served until his death, wrote that he had found it "a joy to work with a man of Carl's qualities. He was among the very best research psychologists, highly skilled, imaginative, and reliable. He always delivered a top product." Recalling his last visit with Hovland, Young said, "He was then so ill that he had to go to bed immediately when the meal was ended, yet he still was the perfect host giving little evidence of either the physical or mental suffering he must have been enduring." Young concluded, "In my seventy-six years I have known few men his equal and none his superior" (letter of November 19, 1974).

Claude Buxton, who succeeded Hovland as chairman of the psychology department at Yale, wrote, "Carl … became my dearest friend, … a very gentle [and] very moral person, and his code included never taking advantage of anyone or anything … He is one of the two or three people I have ever known who made a moral assessment of his own proposals or ideas … He was enormously efficient and organized—one of our colleagues used to say that everything Carl did he did on ball bearings, because it went so smoothly; he was tremendously stimulating to graduate students, … [who told] me they did more work for [Carl's evening special-interest] no-credit meetings than they ever did in their regular courses" (letter of November 8, 1974).

Irving L. Janis, who worked with Hovland both in his Experimental Section in Washington and then (as a younger faculty colleague) in his attitude change program at Yale, similarly concluded his letter to Mary Jenni by saying, "You can feel justifiably proud of your cousin. He was a truly great psychologist and a great person" (letter of October 30, 1974).

Much the same picture emerged from my own more recent inquiries. Jane Olejarczyk, who is now assistant business manager for academic affairs/ registrar for Yale's psychology department, but who began working as Hovland's secretary when she was only nineteen, said, "Knowing how inexperienced I was with academia he constantly assigned me to projects about which I had no clue and gently insisted that I could do [them]. He didn't lavish praise but I knew I did well when the next task was more difficult than the one before." Olejarczyk spoke of Hovland's "warmth" and said, "There was the feeling when he was about that you were part of a family and that you mattered." She added that "Gertrude Hovland was the epitome of grace" (personal communication of May 29, 1997).

Eleanor E. Maccoby (Browning professor emerita of developmental psychology at Stanford), who remembers Hovland well and whose late husband Nathan Maccoby worked in Hovland's group during the war, observed that Hovland was exceptional both in his quick and wide-ranging intelligence and, also, in his "complete absence of guile" (personal communication of 1996).

Harold H. Kelley (professor emeritus of social psychology at UCLA), who worked with Hovland in his Yale attitude change program in the 1950s, wrote, "Of course, the most important thing about Carl was his enormous intellect, his quick understanding of [nearly] everything that was going on, and the ways he let his thought and work roam far and wide … In organizing the personnel of his program, he was deliberately and sympathetically eclectic, grabbing here and there so as to include all possible lines of thought that might bear on the communication/persuasion process" (letter of June 24, 1995).

William J. McGuire noted that "it never bothered Hovland that members of the group … were driven by antagonistic theories that made opposite predictions" and remarked that what prevented these decentralized, individualistic projects from "becoming undesirably anarchical was Hovland's particular intellectual excellence as a synthesizer. He could attend a symposium of papers that seemed to have little in common and, if called on to summarize them, seemed able on the spot to abstract out their unifying themes and show that the papers converged in interesting and complex ways to produce a coherent picture" (McGuire, 1996, pp. 48–49).

About Hovland's own research style, Kelley observed that

Hovland would "analyze the shortcomings or special conditions of … prior work, identify intuitively the as-yet-unstudied factors that would reverse, undo, or clarify the problem." Kelley added, "It always seemed to me that that was his investigative forte—identifying the special conditions surrounding prior work and then expanding the design to pin down the phenomenon more clearly."

Following Hovland's death, the New England Psychological Association (of which Hovland was president in 1950) had a memorial session in which Herbert Kelman characterized Hovland as "the world's most non-authoritarian leader." Similarly, Abraham Luchins wrote me, "He was the most efficient and the least officious of people" (personal communication of May 29, 1997). And Hovland's wartime coworker M. Brewster Smith said, "My most vivid memory of Carl … was his unique ability to guide the development of appropriate research design by asking just the right questions—always in a tentative way that opened new perspectives or possibilities … I have never since experienced that degree of consultative skill …." (letter of May 15, 1997). It was in this way that Hovland was, in the words of Timothy Brock, a "visionary founder of subdisciplines" (personal communication of May 20, 1997). Speaking further of Hovland's low-key and indirect style of leadership, Kelley wrote, "I know that left some people (including myself) with a bit of anxiety. But still, he was so warm, interested in your personal life, etc., that one couldn't help feeling great affection for him." Continuing, Kelley said, "As you can see, I was very fond of Carl, and also had the utmost respect for him. I regard him as one of the handful of real geniuses in psychology.… (letter of June 24, 1995).

CONCLUSION

During his short life, Hovland published over seventy articles, was the editor or coauthor of seven books, and supervised at least twenty-two Yale doctoral dissertations.[5] His scientific achievements were recognized by his early election to the American Philosophical Society (1950), the American Academy of Arts and Sciences (1956), and the National Academy of Sciences (1960), as well as by conferral of the Distinguished Scientific Contribution Award by the American Psychological Association (1957) and of the Howard Crosby Warren Medal by the Society of Experimental Psychologists (1961). This last, awarded close to the time of Carl's death, was graciously received for Carl by his nineteen-year-old son David in what was recalled by another Hovland admirer, Yale professor emeritus Wendel R. Garner, as an unusually "emotional occasion" at the annual meeting of that august society (Garner, personal communication of May 17, 1997).

Beyond his earliest research on diverse problems of physiological, perceptual, and industrial psychology, and his subsequent public service and consulting work, Hovland's most influential scientific contributions emerged from the three fields on which he successively focused his principal research efforts: (1) basic processes of human learning and generalization (late 1930s), (2) social communication and attitude change (1940s and 1950s), and (3) human concept acquisition and problem solving (1950s, until his 1961 death). His work in learning is widely respected and it undoubtedly helped shape the quantitative and experimental skills that he later brought to bear on social communication. But it is his work in that second field that has had the most far-reaching impact. One can't help wondering: If Hovland's life had not been cut short while he was still at the height of his powers, might not the third line of work he had begun on thinking and concept attainment have had a similarly profound impact on the soon-to-burgeon interdisciplinary field of cognitive science?

Like so many others, I feel boundless gratitude that I had ten years to benefit from Hovland's wise and benevolent guidance and, especially, from his example. Yet, in preparing this memoir almost forty years later, I have gained an aching awareness of how much we and the whole range of the behavioral, social, and cognitive sciences lost back in 1961 as a result of the untimely death of this gifted researcher, statesman of science, and incomparable human being.

I THANK FORMER YALE students and Hovland associates for the many thoughtful and heart-warming reminiscences they shared with me, including those I have quoted in this memoir (the most extensive supplied by Hovland's former coworkers Harold Kelley and Herbert Kelman) and those, though not quoted here, that contributed helpful information, suggestions, or corrections (from Robert Abelson, Irvin Child, Earl Hunt, Kenneth Kurtz, Mark Lepper, Edith Luchins, George Mandler, George Miller, Lloyd Morrisett, John Pierce, and Burton Rosner). Finally, I thank Hovland's daughter Kathie Hovland Walvick, his son David A. Hovland, his brother C. Warren Hovland, and his cousin Mary Hovland Jenni (who generously provided me with the wonderful material she had previously obtained from still other of Hovland's family members and colleagues—many of whom are no longer living).

NOTES

1. The principal long-term researchers in Hovland's Experimental Section of the War Department's Research Branch were Frances Anderson, John Finan, Irving Janis, Arthur Lumsdain, Nathan Maccoby, Fred Sheffield, and M. Brewster Smith. A number of others worked in that section for briefer periods, including John Butler, David Grant, Donald Horton, Eugene Jacobson, Ansel Marblestone, Alice Schmid, and Adeline Turetsky. Still others (from the parallel Survey Section of the Research Branch) collaborated in projects of Hovland's Experimental Section—particularly Robert Ford, Edward Suchman, and Paul Wallin.

2. Researchers in Hovland's Yale attitude change project included Robert Abelson, Norman Anderson, Elaine Graham Bell, Jack Brehm, Timothy Brock, Enid Hobart Campbell, Arthur Cohen, Rosalind Lorwin Feierabend, Peter Field, Jonathan Freedman, Irving Janis, Harold Kelley, Herbert Kelman, Bert King, Doris Kraeling (now Rutz), Gerald Lesser, Howard Leventhal, Harriet Linton, Abraham Luchins, Arthur Lumsdaine, Wallace Mandell, William McGuire, Norman Miller, Jacob Rabbie, Donald Rife, Milton Rosenberg, Irving Sarnoff, David Sears, Fred Sheffield, Muzafer Sherif, Walter Weiss, and Philip Zimbardo.

3. Early long-term members of what became the Behavioral Research Center of the AT&T Bell Laboratories included the social psychologists Morton Deutsch, Harold Gerard, Robert Krauss, and Seymour Rosenberg, and the experimental psychologists Herbert Jenkins, Ernst Rothkopf, and Roger Shepard—later joined by a number of other now eminent quantitative and experimental psychologists. Long-term members of this center who have been elected to the National Academy of Sciences include Bela Julesz, Roger Shepard, George Sperling, Saul Sternberg, and the center's director Max Mathews. In addition, Edward E. David, John R. Pierce, and William O. Baker (also members of the Academy) played significant roles at high levels of the Labs in shaping and supporting its Behavioral Research Center. (For more about the history of this center, see the report prepared by Carroll, Julesz, Mathews, Rothkopf, Sternberg, and Wish, 1984).

4. Hovland's students and associates who worked on these cognitive processes included Daniel Berlyne, Albert Bregman, Hugh Cahill, Earl Hunt, Herbert Jenkins, Kenneth Kurtz, Lloyd Morrisette, Dean Pruitt, Roger Shepard, and Walter Weiss.

5. Students whose Yale doctoral dissertations on conditioning or verbal learning were supervised by Hovland were James Calvin (1939), Chester Hill (1941), David McClelland (1941), William Jenkins (1942), William Orbison (1945) Fred Sheffield (1946), and Virginia Voeks (1947). On social psychology or personality: Ethelyn Klatskin—née Elmer Potter (1948), Homer Wood (1948), and Russell Clark (1951). On attitude or opinion change: Herbert Kelman (1951) and Walter Weiss (1952). On human learning or generalization: Kenneth Kurtz (1953), William McGuire (1954), John Antoinetti (1955), Roger

Shepard (1955), Lloyd Morrisett (1956), and Hugh Cahill (1957). On prediction of performance times: Jerome Kornreich (1948). On human curiosity: Daniel Berlyne (1953). On decision making: Dean Pruitt (1957). On prediction of ratings of adjective meanings: Jonathan Freedman (1962). The dissertations of Orbison and Freedman were each jointly supervised by Hovland and another faculty member; and, following Hovland's death, other Hovland students completed their dissertations with still other members of the Yale faculty.

REFERENCES

Anonymous. 1958. Carl Iver Hovland. *Am. Psychol.* 13:158-67.
Carroll, J. D., B. Julesz, M. V. Mathews, E. Z. Rothkopf, S. Sternberg, and M. Wish. 1984. Behavioral science. In *A History of Engineering and Science in the Bell System*, ed. S. Millman, pp. 431–71. Indianapolis: AT&T Bell Laboratories.
Cohen, A. R. 1964. *Attitude Change and Social Influence*. New York: Basic Books.
Janis, I. L. 1968. Carl I. Hovland, 1912–1961. In *International Encyclopedia of the Social Sciences*, vol. 6, ed. D. L. Sills, pp. 526–31. New York: Macmillan.
Janis, I. L. 1973. Hovland. In *The McGraw-Hill Encyclopedia of World Biography*, vol. 5, pp. 372–73. New York: McGraw-Hill.
Jenni, M. H. 1974. An inventory and evaluation of source materials on Carl Iver Hovland. Unpublished manuscript.
Lovie, A. D. and P. Lovie. (To appear.) Carl I. Hovland. In *Encyclopedia of Psychology*. For the American Psychological Association, Washington, D.C.
Maccoby, N. 1963. The new "scientific" rhetoric. In *The Science of Human Communication*, ed. W. Schramm, pp. 41–53. New York: Basic Books.
McGuire, W. J. 1996. The Yale communication and attitude-change program in the 1950s. In *American Communication Research: The Remembered History*, ed. E. E. Dennis and E. Wartella, pp. 39–59. Hillsdale, N.J.: Erlbaum.
Miles, W. R. 1961. Carl Iver Hovland. *The American Philosophical Society Yearbook*, pp. 121–25.
Moscovici, S. Attitudes and opinions. *Annu. Rev. Psychol.* 14(1963):231–60.
Schramm, W. 1963. Communication research in the United States.

In *The Science of Human Communication*, ed. W. Schramm, pp. 1–16. New York: Basic Books.

Sears, R. R. Carl I. Hovland, 1912–1961. *Am. J. Psychol.* 74(1961):637–39.

Shepard, R. N. Stimulus and response generalization: Deduction of the generalization gradient from a trace model. *Psychol. Rev.* 65(1958):242–56.

Shepard, R. N. Toward a universal law of generalization for psychological science. *Science* 237 (1987):1317–23.

Shepard, R. N. and J.-J. Chang. Stimulus generalization in the learning of classifications. *J. Exp. Psychol.* 65(1963):94–102.

Smith, M. B. 1968. Attitude change. In *International Encyclopedia of the Social Sciences*, vol. 1, ed. D. L. Sills, pp. 458–67. New York: Macmillan.

Selected Bibliography

1937 The generalization of conditioned responses. I. The sensory generalization of conditioned responses with varying frequencies of tone. *J. Gen. Psychol.* 17:125–48.

1938 Experimental studies in rote-learning theory. I. Reminiscence following learning by massed and by distributed practice. *J. Exp. Psychol.* 22:201–24.

1939 Experimental studies in rote-learning theory. V. Comparison of distribution of practice in serial and paired-associate learning. *J. Exp. Psychol.* 25:622–33.

1940 With C. L. Hull, R. T. Ross, M. Hall, D. T. Perkins, and F. B. Fitch. *Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology*. New Haven: Yale University Press.

With R. R. Sears. Minor studies of aggression. VI. Correlation of lynchings with economic indices. *J. Psychol.* 9:301–10.

1948 Psychology of the communicative process. In *Communications in Modern Society*, ed. W. Schramm, pp. 59–65. Urbana: University of Illinois Press.

Social communication. *Proc. Am. Philos. Soc.* 92:371–75.

1949 With A. A. Lumsdaine and F. D. Sheffield. *Experiments on Mass Communication* . Princeton: Princeton University Press.

1951 Human learning and retention. In *Handbook of Experimental Psychology* , ed. S. S. Stevens, pp. 613–89. New York: Wiley.

1952 A "communication analysis" of concept learning. *Psychol. Rev.* 59:347–50.

1953 With I. L. Janis and H. H. Kelley. *Communication and Persuasion: Psychological Studies of Opinion Change*. New Haven: Yale University Press.

With W. Weiss. Transmission of information concerning concepts through positive and negative instances. *J. Exp. Psychol.* 45:175–82.

With H. C. Kelman. "Reinstatement" on the communicator in delayed measurement of opinion change. *J. Abnorm. Soc. Psychol.* 48:327–35.

1954 Effects of the mass media of communication. In *Handbook of Social Psychology*, vol. 2, ed. G. Lindzey, pp. 1062–1103. Cambridge, Mass.: Addison-Wesley.

1956 With K. H. Kurtz. Concept learning with differing sequences of instances. *J. Exp. Psychol.* 51:239–43.

1957 With others. *The Order of Presentation in Persuasion.* New Haven: Yale University Press.

1959 Reconciling conflicting results derived from experimental and survey studies of attitude change. *Am. Psychol.* 14:8–17.

With others. *Personality and Persuasibility*. New Haven: Yale University Press.

With L. N. Morrisett. A comparison of three varieties of training in human problem solving. *J. Exp. Psychol.* 58:52–55.

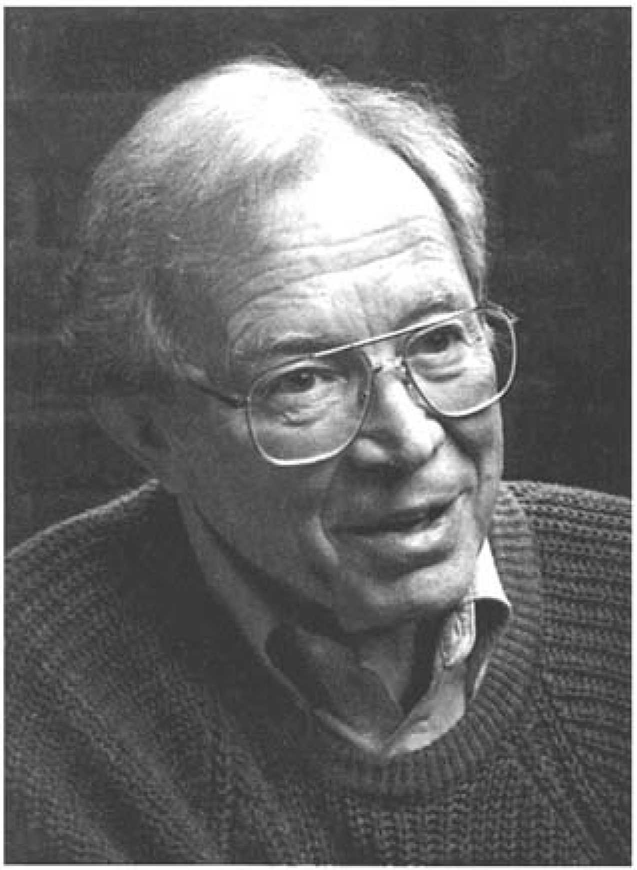
1960 Computer simulation of thinking. *Am. Psychol.* 15:687–93. With E. B. Hunt. Computer simulation of concept attainment. *Behav. Sci.* 5:265–67.

With E. G. Hunt. Order of consideration of different types of concepts. *J. Exp. Psychol.* 59:220–25.

With M. J. Rosenberg, W. J. McGuire, R. P. Abelson, and J. W. Brehm. *Attitude Organization and Change: An Analysis of Consistency Among Attitude Components*. New Haven: Yale University Press.

1961 With R. N. Shepard and H. M. Jenkins. Learning and memorization of classifications. *Psychol. Monogr.* No. 75, (13, Whole No. 517).

With M. Sherif. *Social Judgment: Assimilation and Contrast Effects in Communication and Attitude Change*. New Haven: Yale University Press.



Photograph by G. Paul Bishop, Jr., Berkeley, California

Carson Dunning Jeffries

March 22, 1922–October 18, 1995

BY WALTER KNIGHT, JOHN REYNOLDS, ERWIN HAHN, AND ALAN PORTIS

CARSON JEFFRIES MADE MAJOR fundamental contributions to knowledge of nuclear magnetism, electronic spin relaxation, dynamic nuclear polarization, electron-hole droplets, nonlinear dynamics and chaos, and high-temperature superconductors. These accomplishments involved the Ph.D. programs of thirty-five graduate students, numerous postdoctoral scholars, and resulted in more than a hundred significant publications between 1947 and 1992. As an accomplished artist he exhibited early examples of correlated sound and laser color displays. It is evident that unusual scientific and artistic abilities reinforced each other in a remarkably talented and productive man. He is also remembered for being an uncommonly kind and gentle human being.

PERSONAL HISTORY

Carson was born on March 22, 1922, in Lake Charles, Louisiana, where his father Charles William Jeffries was postmaster and his mother Yancey Dunning a Latin teacher. He had three brothers and a sister, all of whom have survived him. He considered attending a local junior college, but after interviewing the physics instructor (who told him that a rope draped over a frictionlessy pulley would move so as to equalize the length on the two sides of the pulley!), he decided to go elsewhere and earned his B.S. degree at Louisiana State University in 1943.

Education was important to Carson partly because his mother kept track of all the children in school matters. Beyond this, he possessed a strong fascination with what he imagined went on in the schoolhouse. For example, Carson accompanied his one-year-older brother walking to brother's first day at school. Brother immediately disliked it and wanted to return home. Carson, however, wanted to stay, but he was too young and he had to wait a year for the pleasure of attending school. Asked when he became interested in physics, Carson commonly replied, "by the age of four." When he was somewhat older, he was allowed to build his own shed to contain his tools and whatever apparatus he was working on. At any rate, there is no doubt that he wanted to become a scientist at an early age. Much later, when he was in the hospital being prepared for a surgical procedure on his hip, he was visited by a social worker, who informed him, "Mr. Jeffries, we have chaplains of every faith—Christian, Jewish, Buddhist, Muslim … Which would you like to come and talk with you?" Carson half sat up in bed and shouted, "Science is my religion!" The social worker beat a hasty retreat.

After receiving his undergraduate degree, he worked on radar countermeasures at Harvard. There his talents impressed Felix Bloch, who urged him to plan for graduate studies later at Stanford, where Bloch was to resume his professorship in 1946. In the meantime, Carson married Elizabeth Dyer, a native of Maine. Carson and Betty shared many interests—drama, for example. After they came to Berkeley (see below) they regularly attended the annual Christmas dinner and performed in the accompanying theatrical performance at the Ahwanee Hotel in Yosemite Valley.

Carson was a graduate student at Stanford from 1946 to 1950. During this period he witnessed the discovery and development of nuclear induction, now called nuclear magnetic resonance (NMR), which significantly influenced his subsequent research. His thesis project was to measure the ratio of the proton magnetic moment in water to the free proton orbital moment in an inverse cyclotron mode in the same magnetic field. The method of measuring related quantities in the same magnetic field can eliminate the need to achieve high accuracy in measuring the magnetic field itself. Several other important experiments performed in those days used a similar technique. For example, J. H. Gardner and E. M. Purcell at Harvard measured the NMR of the proton and the cyclotron frequency of the electron in the same magnetic field, which is equivalent to measuring the proton magnetic moment in units of the Bohr magneton.

PROFESSIONAL DEVELOPMENT

After he completed his Ph.D. in 1951 under Bloch's guidance, Carson and Betty departed for Zürich, Switzerland, where he became an instructor and assistant to Hans Staub at the University of Zürich Physical Institute. He built and operated the first NMR apparatus in Switzerland and taught the atomic physics lab on the magnificent salary of $125 per month. Carson, Staub, and associates published papers on the measurement of several nuclear magnetic moments, which before the advent of NMR had been known only imprecisely.[1]

When he joined the Physics Department at the University of California, Berkeley, in 1952 as instructor, his salary skyrocketed to $350 per month, and he became a part of the nucleus of the condensed matter physics group, which included

Charles Kittel, Arthur Kip, and Walter Knight.[2] In the following we summarize several important phases of his research in more or less chronological order.[1]

Nuclear Magnetic Resonance and Electron Spin Resonance, 1952–57

Carson measured several nuclear magnetic moments. In the course of this work he observed that resonance linewidths of the two silver isotopes in metallic samples were unusually broad. The effect was later analyzed theoretically by Ruderman and Kittel and is often called the Ruderman-Kittel effect. It is also called the indirect exchange interaction in which an itinerant electron interacts with a nuclear spin, which polarizes the electron spin. The reoriented electron spin subsequently interacts at random with some other nuclear spin, which consequently experiences a significant local hyperfine magnetic field. This sequential double interaction is a long-range effect which results in increased resonance linewidths of both isotopes of silver in silver metal. The effect was soon thereafter observed by Bloembergen and Rowland in thallium metal, which has a stronger hyperfine interaction, resulting in a correspondingly larger line broadening. Similar isotropic spin-spin interactions had also been observed for molecules in liquids.

Carson and his students went on to investigate electron spin relaxation and demonstrated for the first time the phonon bottleneck effect, confirming particularly the work of John Van Vleck, who was much impressed by Carson's work. Setting a pattern for the future, Carson worked out as much as he wanted to know and proceeded to a new investigation.

Optical Pumping and Dynamic Nuclear Polarization In Solids, 1956–57

Independent of A. Abragam, Carson formulated and implemented the methods of dynamic nuclear polarization. This made possible polarized targets in nuclear scattering experiments, such as were carried out with Chamberlain and Shapiro. In the early experiments he employed microwave techniques, but later experiments on rare-earth solids required optical pumping techniques, which Carson added to his repertoire to make it possible to explore a wider range of materials. He also worked out a method of spin population transfer by crystal rotation in a magnetic level crossing experiment, the "nuclear spin refrigerator."

Electron-Hole Droplets in Semiconductors, 1972–83

This was probably the most spectacular of his experiments. The phenomenon had been recognized as a possibility by Russian physicists, but not in the surprising form obtained by Carson, and the Russians were stunned by the results. Photographs of the luminescence of decaying excitons from the droplets taken by Carson and his students appeared on national television and the NBC evening news reported "a new state of matter." As a matter of interest we report here (according to Eugene Commins, department chair at the time) that Carson once took over the weekly colloquium and gave a superb ad lib talk on electron-hole droplets when the scheduled speaker canceled out.

Nonlinear Dynamics and Chaos in Solid State Systems, 1981–95

Here Carson investigated and displayed period doubling and routes to chaos in semiconductor junctions, helical plasma waves, and spin waves in ferromagnets. His experimental plots are often quoted as exemplary in the field.

Nonlinear Dynamics and Harmonic Generation in High TC Superconductors, 1987–95

There are several papers dealing with these effects, with remarkable oscilloscope displays of response patterns for, e.g., the yttrium barium copper oxide compounds considering granularity and intergranular links. A variety of similar experiments, along with electron-hole droplets, occupied Carson scientifically toward the end of his career.

One of the authors (JR) described Carson's mode of research in recommendations to the department chair regarding Carson's award of the Berkeley Citation:

> Jeffries is one of those happy scientists who enjoys working productively in his lab with his own hands more than just about anything else on earth. Moreover, he is one of the most skillful experimentalists I have ever known. He is particularly good at designing, building, and operating sophisticated electronic equipment. When he starts a new project he systematically clears a lab room for it, organizes his tools and test equipment, and proceeds to put circuitry together with incredible speed. His single-minded concentration on a new project is extraordinary, and one would find him in his laboratory day and night, almost oblivious to whatever else is going on in the world. Within weeks he is getting results, usually in a new field—his knowledge and proficiency in experimentation permit him to enter entirely new fields, competing effectively with scientists in well-equipped industrial or government laboratories who are not obliged to prepare and deliver regular lectures. In each new field he rapidly became a leading and innovative practitioner, winning international recognition of his contributions.

GRADUATE STUDENTS AND TEACHING

Carson excelled in teaching his research students. His ability to work in new fields made him a popular thesis adviser over the years. Starting with very able students, he worked with them side by side in the laboratory, stimulating them to develop their talents to a high level.

The list of his Ph.D. students and their postdegree positions prepared by him in 1990 is impressive:

Total number: 35

Number now professors: 13

At institutions: Harvard, 2; Illinois, 2, University of California, Santa Cruz, 2; Massachusetts, 2; Rochester, 1

Permanent staff members at industrial labs: IBM, Xerox, GTE, Raytheon, Dupont

Government labs: Oak Ridge, Goddard, JPL, LBNL, Naval Research Lab

SCIENTIST AS ARTIST

Carson had an artistic side. He engaged in periods of intense activity, reminding us of his scientific work style. Not the least of his accomplishments was the building of a solar house, which might well be called a sculpture because of its design and setting. The observer inside is aware not only of function but also artistic design. For example, some of the copper hot water heaters are hung flat on the wall to be seen and admired, as well as to keep him warm in winter. Outside one sees the house in an exotic setting amidst a group of power transmission pylons. The house won him a prize from Pacific Gas and Electric Company for its efficiency.

Carson's art progressed from an initial period of abstract painting to kinetic works. After visiting and revisiting an exhibition (Directions in Kinetic Sculpture) assembled by Peter Selz, director of the University Art Museum, Carson informed Peter that he thought he could contribute something in that genre. Art professor Karl Kasten recalls that Carson became intrigued with the aesthetic aspects of materials in motion. "He proceeded to produce a series of works primarily for the delectation of his family and friends.

He did not exhibit in public art galleries, which, to my mind, was unfortunate."

His first pieces were ones in which light patterns were modulated by sound. The sculptures had luminous parts, which were controlled microphonically, so that if you spoke to the object it would react to the sounds of your speech with a luminous display. These pieces were the forerunners of important pieces of performance art executed by Carson in collaboration with eminent composers, including John Cage. He had commissions for works exhibited at Expo 70 in Osaka, Japan, and for a 1977 outdoor display in Mexico City.

Late in life Carson created a series of wind-activated pieces on his property high in the Oakland hills. They were related in form and could be seen as individual units or as an orchestrated ensemble moving in the wind, like a ballet company on stage. These pieces varied in height from 5 feet to 8 feet. Each was comprised of colorful sheets of canvas stretched on metal tubing and wire struts, vaguely resembling the frame of an umbrella. The tubes were hinged, pivoted, and delicately balanced, so the slightest breeze would move them in graceful arcs. They also had many modes and degrees of freedom of motion, tending to be aperiodic, and thus related to Carson's interest in chaos. His gentle, sensitive nature was manifest in these creations.[3] [4]

Carson was a warm, gentle, modest friend to all who knew him. He bore many difficulties, particularly a disease that led to deterioration of his weight-bearing joints and required numerous surgical operations on his hips and knees. He endured the suffering with remarkable grace and adjusted his life style so that his disabilities had minor impact on his teaching, research, and service. The final tragedy was a malignant brain tumor, which robbed him of his ability to read and resisted all medical attempts to arrest its growth. Fortunately, until the final week of his life he was able to have rewarding conversations with old friends and to display still the gentle humanity we all had known so well. His final decline was swift, and he died peacefully at home on October 18, 1995.

Carson was divorced from his first wife Elizabeth, with whom he had two children, Andrew and Patricia. Andrew and his wife recently had a pair of fraternal twins, Christopher Daniel and Alexander Carson. Patricia and her husband have adopted a boy, making a total of three grandchildren in 1997. Carson's second wife Olivia Eielson gave him support and companionship in his later years. She is a painter, and shared Carson's interests in art and literature.[5]

HONORS

Carson received much recognition for his work. He held prestigious senior fellowships, which allowed him to make extended visits to Oxford (England), Saclay (France), and Harvard. He was elected to the National Academy of Sciences (1983) and the American Academy of Arts and Sciences. He was honored by his former students and postdoctoral associates at a symposium in Berkeley on June 27, 1992. At his retirement party on May 4, 1992, he was awarded the Berkeley Citation. In recommending this award to the chancellor, department chair Professor Steiner commented, "Professor Jeffries is one of our outstanding experimentalists. More than once he has been nominated for the Nobel Prize." His chief memorial is the lasting image in the minds of his family, friends, and colleagues of an inspiring human being.

NOTES

1. Based on notes of Erwin Hahn's after-dinner speech given at Carson's retirement party on May 4, 1992.
2. Recollections of Walter Knight at the time Carson was hired as a member of the Berkeley Physics Department. Professor Emilio Segrè made contact with Carson in Zürich. Subsequently, Carson made an inquiry about job prospects by writing to Professor Raymond Birge, who was department chair. Soon thereafter, when I arrived on the scene in the summer of 1950, Professor Birge asked me to contact Bloch to get his evaluation of Carson, who had completed his degree under Bloch. I reported that Bloch's comments were highly favorable, indicating that he judged Carson to be a genuinely superior candidate, whereupon Birge proceeded with the official appointment.
3. Based on a letter of Professor of Art Karl Kasten to John Reynolds concerning Carson's art work.
4. Video tapes of the wind-activated sculptures were made of them in motion, and it is hoped that they will be available in the Physics Library for the benefit of those who may be interested.
5. We are grateful to Olivia Eielson and Andrew Jeffries for providing personal information about Carson and his family.

Selected Bibliography

1950 With F. Bloch. A direct determination of the magnetic moment of the proton in nuclear magnetons. *Phys. Rev.* 80:305–306.

1951 A direct determination of the magnetic moment of the proton in units of the nuclear magneton. *Phys. Rev.* 81:1040–55.

1957 Polarization of nuclei by resonance saturation in paramagnetic crystals. *Phys. Rev.* 106:164–65. With M. Abraham and R. W. Kedzie. Gamma-ray anisotropy of Co^{60} nuclei polarized by paramagnetic resonance saturation . *Phys. Rev.* 106:165–66.

1960 Dynamic nuclear orientation by forbidden transitions in paramagnetic resonance. *Phys. Rev.* 117:1056–69. With M. Abraham and R. W. Kedzie. Dynamic nuclear orientation of Co^{60}. *Phys. Rev.* 117:1070–74.

1961 With O. S. Leifson. Dynamic polarization of nuclei by electron-nuclear dipolar coupling in crystals. *Phys. Rev.* 122:1781–95.

1962 With P. L. Scott. Spin-lattice relaxation of some rare earth salts at helium temperatures; observation of the phonon bottleneck. *Phys. Rev.* 127:32–51.

With R. H. Ruby and H. Benoit. Paramagnetic resonance below 1°K; the spin-lattice relaxation time of Ce^{3+} and Nd^{3+} in lanthanum magnesium nitrate. *Phys. Rev.* 127:51–56.

1964 With K. H. Langley. Operation of a proton spin refrigerator. *Phys. Rev. Lett.* 13:808–809.

1966 With G. H. Larson. Spin-lattice relaxation in some rare earth salts. I. Temperature dependence. *Phys. Rev.* 141:461–78.
With G. H. Larson. Spin-lattice relaxation in some rare-earth salts. II. Angular dependence, hyperfine effects, and cross relaxation. *Phys. Rev.* 145:311–24.
With K. H. Langley. Theory and operation of a proton spin refrigerator. *Phys. Rev.* 152:358–76.
1968 With W. B. Grant and L. F. Mollenauer. Achievement of significant nuclear polarizations in solids by optical pumping. *Phys. Rev. Lett.* 20:488–90.
1973 With T. K. Lo and B. J. Feldman. New phenomena in exciton condensation in germanium. *Phys. Rev. Lett.* 31:224–26.
1974 With R. M. Westervelt, T. K. Lo, and J. L. Staehli. Decay kinetics of electron-hole and free-exciton luminescence in Ge: Evidence for large drops. *Phys. Rev. Lett.* 32:1051–54.
With R. S. Markiewicz and J. P. Wolfe. Microwave dimensional resonances in large electron hole drops in germanium. *Phys. Rev. Lett.* 32:1357–60.
1975 With J. P. Wolfe, R. S. Markiewicz, and C. Kittel. Observation of large long-lived electron hole drops in germanium. *Phys. Rev. Lett.* 34:275–77.
With J. P. Wolfe, W. L. Hansen, E. E. Haller, R. S. Markiewicz, and C. Kittel. Photograph of an electron-hole drop in germanium. *Phys. Rev. Lett.* 34:1292–93.
1978 With J. P. Wolfe, R. S. Markiewicz, S. M. Kelso, and J. E. Furneaux. Properties of the strain-confined electron-hole liquid. *Phys. Rev. B* 18:1479–1503.

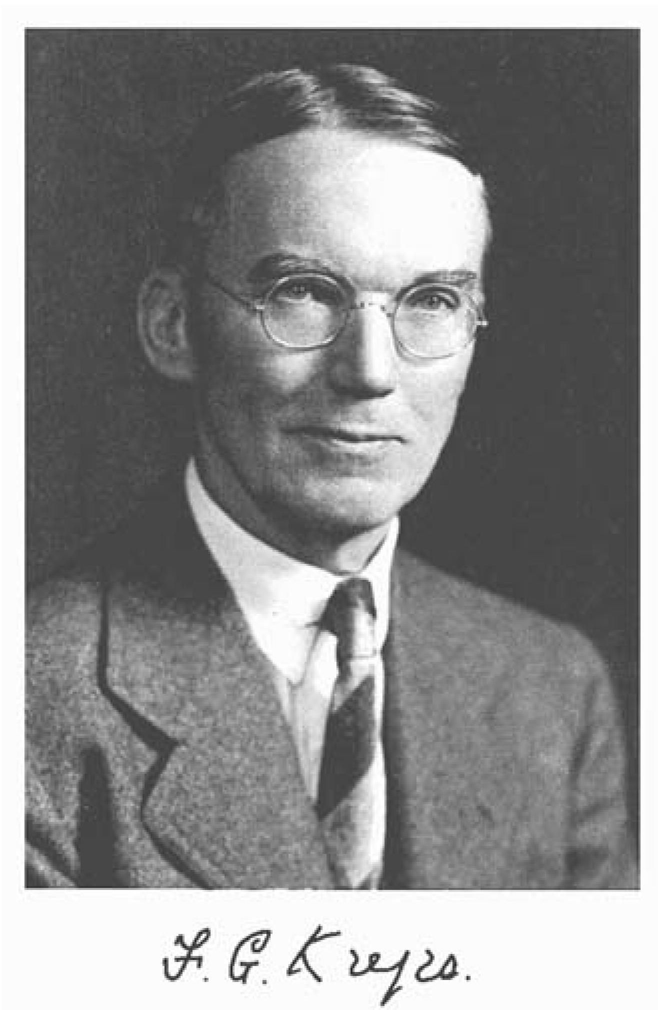
1982 With J. Perez. Direct observation of a tangent bifurcation in a nonlinear oscillator. *Phys. Lett.* A 92:82.

1984 With G. Gibson. Observation of period doubling and chaos in spin-wave instabilities in yttrium iron garnet. *Phys. Rev.* A 29:811–18.

1985 With R. Van Buskirk. Observations of chaotic dynamics of coupled nonlinear oscillators. *Phys. Rev.* A 31:3332–57.

1988 With Q. H. Lam, Y. Kim, L. C. Bourne, and A. Zettl. Symmetry breaking and nonlinear electrodynamics in the ceramic superconductor $YBa_2Cu_3O_7$. *Phys. Rev B.* 37:9840–43.

With P. H. Bryant and K. Nakamura. Spinwave dynamics in a ferrimagnetic sphere. *Phys. Rev.* A 38:4223–40.



Courtesy of the Massachusetts Institute of Technology Museum

Frederick George Keyes

June 24, 1885–April 14, 1976

BY JOHN ROSS

FREDERICK GEORGE KEYES was an outstanding physical chemist who made notable theoretical and experimental advances in thermodynamics, equations of state of gases, and thermodynamic properties, in particular liquid water and steam. His early professional years coincided with the establishment of physical chemistry as an important division of science. He established bridges from science to engineering, from both of these fields to industrial research, and from all of these to the creation of an American industry of scientific instruments. Further, he established early a center for low-temperature research in the United States. The treatise "Thermodynamic Properties of Steam," known as the "Steam Tables," coauthored with J. H. Keenan of the Department of Mechanical Engineering at MIT, has been used in the design and operation of steam generating power plants worldwide.

Keyes was born in Kingston, Ontario, Canada, on June 24, 1885. In his early childhood he received private instruction from his parents, and he rejected entering high school; he preferred spending his pre-college years in experimentation in his home. His formal education was begun in college where he received a B.S. degree from Rhode Island State College in 1907 and from Brown University an M.S. degree in 1907 and a Ph.D. degree in 1909. Keyes's doctoral thesis was in organic chemistry, but he also studied physical chemical research on bacterial metabolism. After leaving Brown University he went to the Massachusetts Institute of Technology on a postdoctoral research appointment with Professor L. J. Gillespie and then, after one year, with Professor A. A. Noyes, who was then the dean of American physical chemists. Noyes and Charles A. Kraus developed quartz-glass electric seals for mercury vapor lamps, and Keyes was employed as chief engineer for the Cooper-Yewett Electric Company during the years 1913–16.

In 1916 Keyes returned to MIT, now located at its new campus in Cambridge, Massachusetts. Keyes began research on the measurements for equations of state of gases and liquids, that is, pressure-volume-temperature relations. In 1917 this research was interrupted by the entry of the United States into World War I. Keyes joined what was later to be known as the Chemical Warfare Service. By February 1918 he had organized a complete laboratory for research and testing in chemistry, physics, and bacteriology and oversaw the shipping of that laboratory to France. In France the laboratory was located at Puteaux and was the only resource of its kind available to the American expeditionary forces in France. Keyes was cited by General Pershing for outstanding service, and he also received the Purple Heart.

After World War I Keyes returned to MIT's research laboratory for physical chemistry, then still headed by Noyes. In 1922 Noyes left MIT and joined the faculty of the Throop Institute of Technology, which later became the California Institute of Technology. In that same year Keyes became the director of the Physical Chemistry Laboratory and soon thereafter also took on the responsibility of the head of the Department of Chemistry, a position he held until 1945. Keyes established at MIT the Research Laboratory of Organic Chemistry in 1925 and the Research Laboratory of Inorganic Chemistry in 1930. He took a very active part in the development of the Eastman Laboratories, which was MIT's first building devoted to research and education in chemistry and physics.

Keyes's scientific investigations focused strongly, but far from entirely, on the thermodynamic properties of matter, predominately gases and liquid water. For example, in 1922 he coauthored a manuscript on the equation of state for methane. The work had scientific interest in that measurements of pressure-volume-temperature relations lead to information about intermolecular forces, a connection early recognized by Keyes. As typical of so much of Keyes's work, this study of methane had an important practical purpose, that of extracting helium from natural gas.

Keyes formed early a laboratory in the United States devoted to low-temperature research. He and Collins described instrumentation for such research, including the famous Collins helium cryostat.

In an extensive series of studies Keyes and his coworkers, among them Leighton B. Smith, Samuel C. Collins, and Harold T. Gerry, made highly precise measurements on pressure-volume-temperature relations for a unit mass of liquid and gaseous water. In addition, with Collins and Howard McMahon, he concentrated on the measurements of heat capacity and in particular the pressure variation of the enthalpy of steam, which is directly related to the Joule-Thomson coefficient and that in turn to molecular forces. From these and related researches Keyes and J. H. Keenan published in 1936 "The Thermodynamic Properties of Steam," known as the "Steam Tables." This authoritative and incisive work, brought up to date from time to time, has had extraordinary influence on the design and construction of steam engines and steam turbines in the entire world. The increased economies in steam generation due to the design of steam turbines based on accurate steam data have brought savings of millions of tons of coal per year.

A related area of interest to Keyes was the study of the dielectric constant of gases as a function of temperature and density. One of his students here was John L. Oncley, who later became a well known biophysical chemist, and the other graduate student was John G. Kirkwood, who later became one of the leading theoretical physical chemists in the world.

During World War II Keyes investigated the thermodynamic properties of hydrogen peroxide as a source of oxygen for submarines, and the scientific aspects of that work were published after the war.

In addition to equilibrium thermodynamic properties, such as the equation of state, Keyes had a longtime interest in transport processes of a variety of gases, including steam, nitrogen, the rare gases, oxygen, carbon monoxide, water, and others. He contributed here by making high precision measurements, correlation studies of the experiments from different laboratories and the connection of these properties to intermolecular forces.

Keyes published extensively from the first to the sixth decade of this century. He was deeply devoted to science as an intellectual discipline as well as an important aspect and contributor to our culture. He had the highest standards of scholarship and appreciated excellence.

During his tenure as chairman of the chemistry department at MIT, Keyes attracted several outstanding scientists, including I. Amdur, who pioneered in molecular beam research and transport processes; Walter H. Stockmayer, renowned for his work in statistical mechanics and polymers; George Scatchard, pioneer in electrolyte chemistry and biophysical chemistry; J. Willard Stout, later the longtime editor of the *Journal of Chemical Physics,* and James A. Beattie, who made lasting contributions to gas thermometry and equilibrium thermodynamic measurements of gases.

Keyes was active in founding several companies devoted to producing and marketing chemical laboratory equipment in the United States. He was a holder of many patents, and remained active until near his death in Keyes Scientific Co., Inc., an industrial consulting firm in Cambridge.

In 1923 Professor Keyes married Gabriel Alice Bowers of Paris, France. Mrs. Keyes was related to the impressionist painter Pierre Bonard. The family, at least until relatively recently, had large holdings of Bonard's paintings and at one time sponsored a Bonard exhibit at the Tate Gallery in London.

By present day standards of scientists, Keyes was extraordinarily wealthy. First, he inherited wealth. He said to me once with some wistfulness that in fact he had never looked at some of the bank deposit books left to him by his father. Second, he made considerable money on his own with his founding of scientific equipment manufacturing and sales companies, with his patents, and with his investments. Third, he married into great wealth. The Keyeses, in spite of their wealth, lived a relatively simple life, with a few exceptions. Keyes loved fine cars and he owned, in the latter part of his life, three cars. One was a quite ordinary Rolls-Royce, a city car, which he maintained in Cambridge. The second car was an extraordinary Rolls-Royce Roadster, a huge car that required a sixty-gallon gasoline tank to be sure to make the trip from Cambridge to Cape Cod, where the Keyeses had extensive land holdings.

The third car was an Hispania Suissa, which was built essentially only for royalty and heads of state. This car was maintained, as all of the others, in immaculate condition, but was stationed in Paris. After knowing Keyes for many years I once asked him why he maintained this car in Paris since he very seldom, if ever, got there anymore. He replied simply, "Yes, I know John, but it is so nice to have a car when you get there."

The Keyeses and a friend of theirs purchased about 2,000 acres on Cape Cod near Truro in the early 1920s. The Keyeses' share of this land was given as a gift to MIT and ultimately MIT had to sell the land to what is now a national seashore on Cape Cod.

I first met Fred Keyes when I was a graduate student at MIT in 1948. Since I worked for I. Amdur for my Ph.D. thesis, and Keyes had his laboratories just across the hall, somehow Keyes also considered me his graduate student. I left MIT in 1952 and returned in 1966 to be chairman of the Department of Chemistry. Keyes insisted that I inherit his office, which was one of the small extravagances that he permitted himself. The office was constructed at his expense in about 1932 of Canadian white oak paneling. It had a ceiling painted blue with silver fleurs-de-lis and a fireplace. Above the fireplace was an inscription from the works of Virgil: "Felix qui potuit rerum cognoscere causas" ("Happy he who knows the causes of things"). Many years later this inscription was to become the motto of Churchill College at Cambridge. Incidentally, the fireplace had next to it a wood bin, which could be restocked from the hallway. One of the panels in this great office, when lightly touched, opened up and revealed a discreet locker for sherry (or single malt whiskeys, depending on the occupant).

Keyes was gentlemanly, soft-spoken, always impeccably dressed, and had, if not an aristocratic, then at least a conservative authoritative demeanor. Politically he was also conservative; he stated to me on several occasions that the demise of Western Civilization could indeed be traced to the imposition of the income tax in the United States in 1913.

On December 11, 1970, MIT established the Frederick George Keyes Professorship of Chemistry and the announcement was made by James R. Killian, Jr., then chairman of the MIT Corporation. The announcement also came on the sixtieth anniversary of Keyes's association with the institute. Killian stated, "We wish to mark this anniversary in a proper way as an expression of our esteem for his distinguished career, and of our appreciation of his many different kinds of contributions to MIT." I had the privilege of being the first Frederick George Keyes professor.

Selected Bibliography

1917 A new equation of continuity. *Proc. Natl. Acad. Sci. U. S. A.* 3:323–30.

1918 With R. B. Brownlee. The vapor pressure of liquid ammonia up to the critical temperature. *J. Am. Chem. Soc.* 40:25–45.

1920 On the establishing of the absolute temperature scale. *J. Am. Chem. Soc.* 42:54–59.

1921 The Joule-Thomson effect for air. *J. Am. Chem. Soc.* 43:1452–70.

1924 Evidence of association in carbon dioxide from the Joule-Thomson effect. *J. Am. Chem. Soc.* 46:1584–92.

1927 The Sutherland viscosity constant and its relation to the molecular polarization. *J. Phys. Chem.* 130:709–14.

1928 With H. G. Burks. The equation of state for binary mixtures of methane and nitrogen. *J. Am. Chem. Soc.* 50:1100–1106.

1930 With J. G. Kirkwood. Dielectric constant of carbon dioxide as a function of temperature and density. *Phys. Rev.* 36:754–61.

1933 Methods and procedures used in the Massachusetts Institute of Technology program of investigation of the pressures and volumes of water to 460°C. *Proc. Am. Acad. Arts Sci* 68:505–64.

1934 With L. B. Smith and H. T. Gerry. The vapor pressure of water. Part II. Steam research program. *Proc. Am. Acad. Arts Sci.* 69:137–68.

With L. B. Smith. The volumes of unit mass of liquid water and their correlation as a function of pressure and temperature. Part Ill. Steam research program. *Proc. Am. Acad. Arts Sci.* 69:285–314.

1936 With J. L. Oncley. The relation between the dialectic constants of some compressed gases and the density. *Chem. Rev.* 19:195–212.

With L. B. Smith and H. T. Gerry. The specific volume of steam in the saturated and superheated condition together with derived values of the enthalpy, entropy, heat capacity and Joule-Thomson coefficients. *Proc. Am. Acad. Arts Sci.* 70:319–64.

With J. H. Keenan. *Thermodynamic Properties of Steam, Including Data for the Liquid and Solid Phases.* New York: J. Wiley and Sons.

1937 With H. T. Gerry and J. F. G. Hicks, Jr. The production of liquid hydrogen without expensive equipment. Low temperature studies no. I. *J. Am. Chem. Soc.* 59:1426–37.

1938 With S. C. Collins. The heat capacity and pressure variation of the enthalpy for steam from 38° to 125°C. Part V. Steam research program. *Proc. Am. Acad. Arts Sci.* 72:283–99.

1939 With S. C. Collins. Note on the year's progress in the precise measurement of the effects of intermolecular potential in gases. *J. Phys. Chem.* 43:5–14.

1948 I-Introductory comments to a series of contributions on gas properties. *Trans. ASME* 70:621–22.

With C. E. Huckaba. The density of aqueous hydrogen peroxide solutions. *J. Am. Chem. Soc.* 70:2578–83.

1949 The consistency of the thermodynamic data for water substance vapor phase to 550°C. Part VII. *J. Chem. Phys.* 17:923–34.

1962 The second virial coefficient for steam. *J. Intern. Heat Mass Transfer* 5:137–42.

1964 With R. G. Vines. The thermal conductivity of steam. *J. Intern. Heat Mass Transfer* 7:33–40.

1965 With R. G. Vines. The thermal conductivity of nitrogen and argon. *J. Intern. Heat Transfer* 87:177–83.

1969 With J. H. Keenan, P. G. Hill, and J. G. Moore. *Steam Tables: Thermodynamic Properties of Water, Including Vapor, Liquid, and Solid Phase*. New York: J. Wiley and Sons.

Heinrich Klüver

Heinrich Klüver

May 25, 1897–February 8, 1979

BY FREDERICK K. D. NAHM AND KARL H. PRIBRAM

HEINRICH KLÜVER WAS AN influential figure in the field of animal behavior and is said to have brought the Gestalt psychology movement to the continental United States. Joining rigorous experimental methods with a phenomenological spirit of investigation, he became one of the foremost experimental psychologists of his time and helped to shape the field today known as neuroscience.

He is known mostly for his work with the neurosurgeon Paul Bucy and the description of the Klüver-Bucy syndrome, which was described in non-human primates following large bilateral lesions of the temporal lobe. Later in his career he turned to neuroanatomy and developed staining techniques that are still in use.

He was by many accounts a man devoted to the practice of science. He successfully refused the pull of administrative duties and continued to carry out experiments until his mandatory retirement from the University of Chicago in 1963. Klüver's work had an important influence on the growth of the neurobiological sciences in the United States. His experimental practices were both conceptually rich and methodologically sound. He subjected his ideas to strict experimental validation and had no patience for arm-chair psychology. His studies furthered our understanding of the brain mechanisms involved in vision and emotion. Klüver's papers are still frequently cited and continue to engender debate and stimulate research into the functions of the human and non-human primate's occipital and temporal lobes.

PERSONAL HISTORY

Heinrich Klüver, son of Wilhelm and Dorothes (Wübbers) Klüver, was born on May 25, 1897, in Schleswig-Holstein, Germany. He arrived in the United States in 1923, married Cessa Feyerabend on February 4, 1927, and was naturalized as a U. S. citizen in 1934. On February 8, 1979, at the age of eighty-one, he died in Oak Lawn, Illinois, and is survived by his second wife Harriet Schwenk Klüver. After reluctantly serving as a private in the German army at the age of seventeen, he entered the University of Berlin and then the University of Hamburg where, in 1920 as a graduate student, he spent the next three years working with one of the fathers of Gestalt psychology, Max Wertheimer.

After leaving Germany, he boarded a freighter and, via the Panama Canal, traveled to the United States. His first year was spent in Palo Alto, California, as a student in the department of psychology at Stanford University. In 1924 he was granted his Ph.D. in psychology for his work on eidetic phenomena—unusually strong visual imagery in young children. Next, Klüver headed for the University of Minnesota where he spent two years (1924–26). It was there that he met psychologist Karl Spencer Lashley, who became both a friend and a lifelong colleague.

After leaving Minnesota, Klüver spent two years at Columbia University as a fellow of the Social Science Research Council and then moved to Chicago to join Lashley at the Institute for Juvenile Research. Thereafter, he moved to the University of Chicago, holding appointments in the

Division of Psychiatry and the Division of Biological Sciences. Although he was formally retired at the age of sixty-three, he continued to visit his Culver Hall laboratory up to the year before his death.

Upon arrival at the University of Chicago as an associate professor of experimental psychology, he joined the "Neurology Club," a collection of outstanding neuroscientists that included among others Karl Lashley, Percival Bailey, A. Earl Walker, Ralph Gerard, Stephen Polyak, Charles Judson Herrick, and Roy Grinker. As remarked by Paul Bucy, another member and later to be Heinrich Klüver's most notable collaborator, "Even in this constellation of brilliant stars, Heinrich Klüver shone". [1]

Throughout his years at the University of Chicago, he never taught undergraduate or graduate students, and after his twenty-four hours of chairmanship in the Department of Psychology took no part in any further administrative duties. Furthermore, Klüver, toward the latter part of his career, was by choice left alone in his Culver Hall laboratory, without a secretary. He refused to see visitors unless their interests were exceptionally close to his own. When visitors were admitted they had to stay at least a half day behind the locked door to his laboratory. One of us (K.H.P.) enjoyed such a visit for a full day—lunch was not even considered. Towards the end of his retirement, as recognition of his work came, he ventured out more often, attending meetings and presenting himself as a cordial colleague.

At the time of his retirement, a plumbing leak destroyed most of Klüver's papers; the remainder are now under the care of the Department of Special Collections at the Joseph Rejenstein Library, University of Chicago.

PROFESSIONAL HISTORY

Visual Psychology And Psychoparmachology

As a student of Max Wertheimer, Klüver began his scientific career studying the nature of visual perception in children. He continued these phenomenologically motivated psychological investigations during his period as a graduate student at Stanford University, studying 'eideteker,' young children with unusually strong visual imagery. These children experienced persistent visual imagery, and the goal of these early studies was to understand how such visual phenomena related to normal mechanisms of perception. In addition, he knew the value of studying the varieties of perceptual phenomena in both the normal and pathological state. His systematic review of the clinical neurology literature on brain-damaged patients, mostly German soldiers after World War I (1927), is one example of his lifelong interest in visual processes in the context of pathophysiological states.

Years before psychoactive compounds became popular, Klüver's interest in mescal "buttons" or peyote (the dried tops of the cactus *Lophophorus Williamsii*) can be traced back to his earlier publications on eidetic visual phenomena, for mescal visions were thought to resemble visual eidetic imagery (1928, 2). This phase of Klüver's career is rather colorful from an historical perspective, for he provided an intimate analysis of these mescaline-induced visual changes. With the aid of an assistant in a laboratory setting at the University of Minnesota around 1924, he ingested mescal buttons and compulsively documented the nature of his own experiences during intoxicated states. Though he found the value of mescal in the study of eidetic vision to be minimal, he did notice that during his mescal state the boundaries between subjective and objective world tended to disappear and concluded that the use of mescal may shed light on the loosening of subject-object relations in schizophrenics. He also experienced recurring visual forms such as those used by Miro in his painting and suggested that their existence might be of some interest to anthropologists studying visions and symbolic art of various tribes (1928). Klüver always recognized the importance of his data for other fields, and he confidently pointed out that psychoactive compounds were an important tool in the study of visual abilities such as color and space phenomena, dreams, illusions, and hallucinations.

Animal Behavior and Occipital Lobe Lesions

After his self-experimentations with mescal, he began to study the behavioral effects of mescal on the non-human primate. For Klüver the use of non-human primates and the study of visually guided behavior provided a means by which to objectify and test his views. The aim of his studies on monkeys that had been administered mescaline was to determine how this psychoactive compound affected monkeys' ability to judge the similarity or differences between objects. During this period, Klüver developed new techniques that extended his experimental methods and helped him to document the sensory capabilities of monkeys in both normal and drug-induced state.

One of these innovations was the "pulling-in" technique that consisted of a horizontal platform on which lay two strings, at the ends of which were attached various weighted, colored, and textured forms. Using this set-up, monkeys could be trained, for example, to choose the darker of two colored disks for a reward. The "method of equivalent and non-equivalent stimuli" was then used to determine the range of stimuli which were from the monkey's perspective similar to the positively rewarded training stimuli. Eventually, this apparatus was further developed and provided Klüver the means by which to test monkeys on a wide battery of stimuli in various conditions (1935, 1-3). With an introduction by Karl Lashley, Klüver's 1933 book entitled *Behavior Mechanisms in Monkeys* contains the data collected using these behavioral methods. After fully acquainting himself with the behavioral characteristics of nonhuman primate behavior, he then embarked on his lesion studies, which would add a new dimension to his experimental arsenal.

Klüver's first attempt to apply the ablation method was conducted with the aid of Karl Lashley. These experiments studied the influence of occipital lobe lesions on visually mediated behavior (1936; 1937, 1). The studies showed that, in the absence of occipital cortex, monkeys could still respond to changes in light flux. Later, in one of our laboratories (K.H.P.) a more accurate resection of visual cortex was undertaken by Lawrence Weiskrantz as a graduate student. His lifelong experiments led to the discovery of blind-sight in humans: the ability to respond to the location and form of objects in the absence of subjectively seeing them.

Temporal Lobe Lesions and the Klüver-Bucy Syndrome

With Paul Bucy, Klüver would extend the scope of his ablation studies to include lesions of the temporal lobes. The temporal lobe experiments of Klüver and Bucy were initially motivated by Klüver's previous findings that injection of mescaline in monkeys produced chewing and licking movements, as well as convulsions. Klüver made the observation that these oral behaviors were reminiscent of the "uncinate" fits first described by Hughlings Jackson in certain patients with temporal lobe epilepsy. If surgical removal of the uncal region could abolish the oral behaviors produced by mescaline administration in monkeys, a strong case could be made regarding the underlying neuroanatomical site at which mescaline exerted its effect.

In collaboration with Paul Bucy, the first unsuccessful attempts to identify the locus of action of mescal in monkeys began with dissections of the trigeminal and facial nerves. These were then followed by lesions of the temporal lobes. Paul Bucy's contribution was invaluable at this point as Klüver had no surgical experience with temporal lobe resections. Despite Klüver's desire to perform more restricted lesions of the uncal region, Bucy felt he couldn't perform such an operation and decided to take out the whole temporal lobe.

On the afternoon of December 7, 1936, Dr. Bucy removed a large portion of the left temporal lobe in the aggressive adult female Rhesus monkey named "Aurora." This monkey had been an experimental subject of Prof. George W. Bartelmes, but due to its viciousness was offered to Klüver who was recognized for his monkey handling skills. As recounted by Bucy, on the morning after the left temporal lobe was removed, Klüver called him on the phone and exclaimed, "What did you do to my monkey?" [1] Hastening to the laboratory, Bucy saw that this preoperatively aggressive monkey had by all accounts become "tame." It was unbelievable. [2] This formerly vicious, unmanageable beast was indeed tame. After a second surgery that removed the right temporal lobe, the full extent of Aurora's behavioral disturbances became manifest.

Klüver's pursuit of the locus of action of mescaline would end here, as the monkeys continued to exhibit mescaline-induced lip-smacking behavior even after temporal lobe lesions. The unexpected behavioral findings redirected

Klüver's main research program to the study of the temporal lobe and diseases of the temporal lobe in non-human and human primates. Though temporal lobe lesions had failed to abolish the oral behaviors (as Klüver had initially hypothesized), it was not without reward, for as Paul Bucy wrote, "…it may come as a surprise that the discovery of the syndrome of bilateral destruction of the temporal lobes came by chance and without prior planning—but not by accident. This discovery was the result of the action of a well-prepared, active, alert mind, which perceived the unexpected and recognized its importance."[1]

The syndrome that Klüver and Bucy described was initially referred to as the "temporal lobe syndrome";[3] according to Klüver, "…the most striking behavior changes ever produced by a brain operation in animals" (1951). This syndrome has since come to bear their name and is comprised of six categories of symptoms:

1. "Psychic blindness"—the inability to recognize objects by sight in the absence of any impairment in visual acuity;
2. "Hypermetamorphosis" (of Wernicke)—a condition characterized by repetitive and persistent responses to small visual objects;
3. Oral tendencies such as the oral examination of objects consisting of licking, biting, and chewing;
4. Taming; and
5. An increase in the manipulation of genitalia and in heterosexual and homosexual behaviors.

Klüver and Bucy never attempted to localize their behavioral findings to any particular neuroanatomical structure. This was left to one of us (K.H.P.) to accomplish. Lashley doubted that Bucy's temporal lobe lesions accounted for the changes in aggressive and sexual behavior. He surmised (erroneously) that the lesions had invaded the hypothalamus. Klüver was reluctant to sacrifice his now tame monkeys.[4]

By contrast, Klüver's motive was not to determine the functional significance of the structures damaged. Rather, his aim was to understand what the constellation of symptoms he referred to as the "temporal lobe syndrome" revealed about the psychological structure and the phenomenal determinants of visual and emotive experience. It is this "behavior with phenomenal determinant" to which Klüver devoted his life, and as he wrote early in his career:

> …the question we wish to answer is: What is it that determines the directions and turns of behavior? More specifically, what are the factors which impart certain directions to the animal's behavior in situations in which reactions to sensory stimuli are performed? What are, briefly speaking, the determinants of sensory responses? We are not interested in the fact that there is such a thing as "behavior"; we are interested in the factors responsible for certain kinds of behavior (1933, p. 332).

What Klüver and Bucy reasoned from their observations of temporal lobectomized monkeys was that damage to both cortical and subcortical structures of the temporal lobe had disrupted the processes by which the meaning of a sensory precept is "appreciated." Supported by solid behavioral testing of monkeys with circumscribed neurosurgical ablations, Klüver, along with Bucy, proposed that between the stimulus and the response lay an essential psychological process; namely the ability to understand what is perceived. The experiments of Klüver and Bucy were a catalyst for the psychosurgical movement of the mid-twentieth century, for at the end of one of Klüver's presentations in the late 1930s, Egas Moniz stood up and publicly queried Klüver whether such methods could be used to treat intractably violent individuals. Klüver later remarked that he was mortified by this interaction. Moniz performed the first frontal lobe ablation only a few years later and subsequently received a Nobel Prize for his accomplishments.

Porphyrins and the Klüver-Barrera Stain

Though Heinrich Klüver is best known for his temporal lobe experiments with Paul Bucy, his work during the following two decades with Elizabeth Barrera was an important contribution to the field of neuroanatomy. During this period he embarked on the last major phase of his scientific career and began his neuroanatomical studies investigating the brain tissue of many animals. With these experiments, Klüver mastered an entirely new set of laboratory techniques. Using fluorescence spectral analysis, he made the important discovery that the white matter in brains of warm-blooded animals contained a high concentration of porphyrin.

Using spectrochemical methods and data on solubility, Klüver and Barrera later found that the 625mm band of porphyrins was composed of both protoporphyrin and coproporphyrin. Characteristic of Klüver's active mind, he astutely reasoned that certain neurological and psychiatric disorders might be associated with a disturbance in the metabolism of porphyrins—a condition he termed "cerebral porphyria" (1944).

The concentrations of these porphyrins was too minute to allow the differentiation of structures at a cellular level, as Klüver had originally hoped. Klüver and Barerra then conducted in vitro studies to investigate whether the differential uptake of particular cells or fibers to exogenous applied phthalocyanine derivatives (porphyrin-like compounds) could help to visualize the microarchitecture of brain slices. Although they were unable to show any effect at a microscopic level, they did observe incidentally that at a more macroscopic level, the white matter compared with gray matter had a greater affinity for erogenous porphyrins. Like his studies with Bucy, the unexpected had arisen, and as he had done before, he grasped its significance and went on to develop a new histological method for staining brain slices.

After experimenting with a number of synthetic organic pigments, they discovered that the Luxol fast blue MBS stain provided a remarkable means by which to stain the myelin sheaths of nerve cells. This Luxol fast blue MBS stain could also be used in conjunction with the Nissl stain cresyl violet. This combination allowed both the myelin and the cell to be visualized on the same brain section, and continues to be a widely used histological method in neuroanatomy and neuropathology.

An "incident" that occurred at the beginning of this research typifies Klüver's character. Trying out a great number of tissue stains without success, he dropped the discards into a slop jar for later disposal. One morning, glancing down as he deposited another failure, Klüver was astounded to find some slides at the top of the slop jar with contrasting blue and violet stained tissue. He spent the next two years combining chemicals used during the previous years (as catalogued in his records) to discover the successful combination.

CONCLUSION

Heinrich Klüver did not train any graduate students, and is thus without a living legacy of pupils. Nonetheless, he had an undeniable impact on the growth of biological psychology as editor to ten journals spanning the fields of psychology, neurology, and biological psychiatry. He was a member of twenty-eight scientific societies, consultant to numerous advisory committees, and lectured widely throughout the world. Klüver left little in the form of autobiographical material—his sparing comments inextricably associated with documents pertaining to his scientific work.

Upon his nomination, however, to the National Academy of Sciences in 1957, Klüver wrote the following:

> I have always felt greatly in debt to the country to whose shores I came about a third of a century ago and at this moment feel particularly pleased and honored that the outstanding scientists representing the foremost scientific academy of this country have asked me to join them. WE THANK ROBIN ANNE O'SULLIVAN of the Joseph Regenstein Library, University of Chicago, for providing archival materials.

HONORS AND DISTINCTIONS

Professional Appointments

1924–26	Instructor in psychology, University of Minnesota
1926–28	Fellow, Social Science Research Council, Columbia University
1928–33	Research psychologist, Behavior Research Fund, Chicago
1933–35	Research associate, Department of Pathology, University of Chicago
1935–36	Associate professor of experimental psychology, University of Chicago, Division of Psychiatry
1936–38	Assistant professor of experimental psychology, University of Chicago, Division of Psychiatry
1933–38	Associate member, Otho S.A. Sprague Memorial Institute, University of Chicago
1938–57	Professor of experimental psychology, University of Chicago, Division of Biological Sciences
1957–62	Sewell L. Avery distinguished service professor of biological psychology, University of Chicago, Division of Biological Sciences
1963	Sewell L. Avery distinguished service professor emeritus, University of Chicago, Division of Biological Sciences

ACADEMIES, HONORS, AND AWARDS

1954	American Academy of Arts and Sciences
1957	National Academy of Sciences

1960	Karl Spencer Lashley Award in Neurobiology, American Philosophical Society
1963	Samuel W. Hamilton Award, American Psychopathological Association
1964	Gold Key Award, Medical Alumni Association, University of Chicago
1965	Honorary M.D., University of Basel
1965	Gold Medal Award, American Psychological Foundation
1966	Honorary member, American Neurological Association, International Brain Research Organization, American College of Neuropsychopharmacology, Society of Biological Psychiatry
1969	Distinguished Achievement Award, *Modern Medicine*
1969	Gold Medal Award, Eastern Psychiatric Research Association
1969	Honorary Ph.D., University of Hamburg
1971	Honorary M.D., University of Kiel
1975	Honorary member, Society of Biological Psychiatry

NOTES

1. P. C. Bucy. Heinrich Klüver. In *Neurosurgical Giants: Feet of Clay and Iron*, ed. P. Bucy, pp. 349–53. New York: Elsevier Science Publishers, 1985.
2. It still is. Unilateral temporal lobectomices have little if any grossly observable effects. The observation most likely followed the *second* surgery.
3. Klüver and Bucy were unaware that this temporal lobe syndrome had been accurately described in 1888 by Sanger-Brown and Schäffer in a monograph entitled: An investigation into the functions of the occipital and temporal lobes of the monkey's brain. Philosophical Transactions of the Royal Society of London, Vol. 179, pp. 303–27.
4. As Bucy's first resident and Lashley's associate, K.H.P. was admitted to the Klüver sanctuary to attempt to persuade Klüver to "do the anatomy," which he finally did some years later. As K.H.P. entered the laboratory (for the day), he was greeted warmly by both Klüver and Aurora, who was sitting on Klüver's desk. K.H.P. had already started the program of dissecting the temporal lobe syndrome and a fruitful exchange was engendered regarding the

types of quantitative behavioral tests that needed to be done. As a result, some years later K.H.P. and his students showed a double-dissociation between "psychic blindness" produced by the cortical resection of the temporal lobes due to a flattening of generalization gradients and disabilities on the Klüver equivalence task, which is disturbed by resections of the amygdala.

Selected Bibliography

1924 The determination of types with an experimental study of the eidetic type. Dissertation, Leland Stanford Junior University.
1926 Mescal visions and eidetic vision. *Am. J. Psychol.* 37:502–15.
1927 Visual disturbances after cerebral lesions. *Psychol. Bull.* 24:316–58.
1928 Studies on the eidetic type and on eidetic imagery. *Psychol. Bull.* 25:69–104.
Mescal: The 'Divine' Plant and Its Psychological Effects. London: Kegan Paul, Trench, Trubner, and Company.
1931 The equivalence of stimuli in the behavior of monkeys. *J. Genet. Psychol.* 39, 3–27.
1933 *Behavior Mechanisms in Monkeys*. Chicago: University of Chicago Press.
1935 A tachistoscopic device for work with sub-human primates. *J. Psychol.* 1:1–4.
Use of vacuum tube amplification in establishing differential motor reactions *J. Psychol.* 1:45–47.
An auto-multi-stimulation reaction board for use with sub-human primates. *J. Psychol.* 1:123–27.
1936 Analysis of the effects of the removal of the occipital lobes in monkeys. *J. Psychol.* 2:49–61.

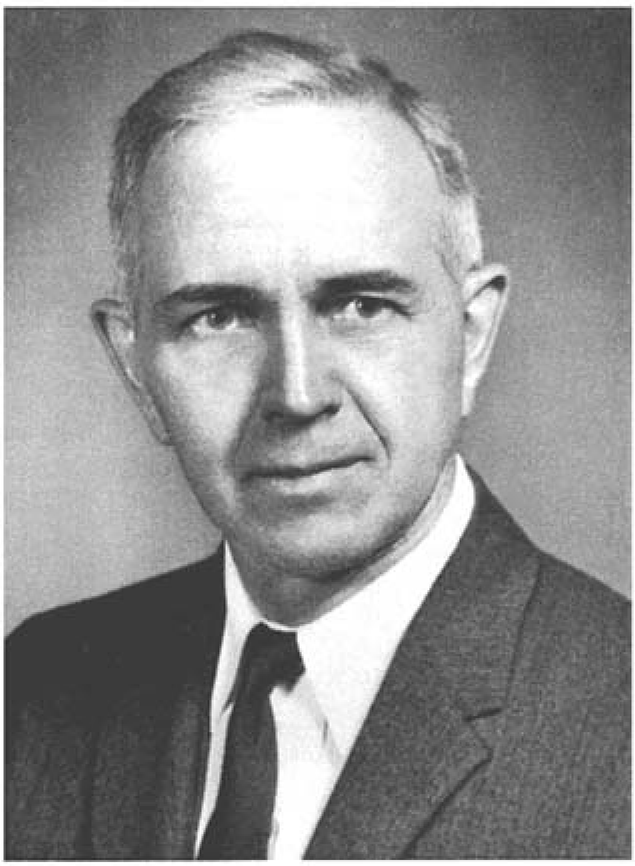
1937 Certain effects of lesions of the occipital lobes in macaques. *J. Psychol.* 4:383–401.
With P. C. Bucy. "Psychic blindness" and other symptoms following bilateral temporal lobectomy in rhesus monkeys. *Am. J. Physiol.* 119:352–53.
1938 With P. C. Bucy. An analysis of certain effects of bilateral temporal lobectomy in the rhesus monkey, with special reference to "psychic blindness." *J. Psychol.* 5:33–54.
1939 With P. C. Bucy. A preliminary analysis of the functions of the temporal lobes in monkeys. *Trans. Am. Neurol Assoc.*, 65th Meeting, pp. 170–75.
With P. C. Bucy. Preliminary analysis of functions of the temporal lobes in monkeys . *Arch. Neurol. Psychol.* 42:979–1000.
1940 With P. C. Bucy. Anatomic changes secondary to temporal lobectomy. *Arch. Neurol. Psychiatr.* 44:1142–46.
1944 On naturally occurring porphyrins in the central nervous system. *Science* 99:482–84.
1951 Functional differences between the occipital and temporal lobes with special reference to the interrelations of behavior and extracerebral mechanisms. In *Cerebral Mechanisms in Behavior,* ed. L. A. Jeffress, pp. 147–99. New York: Wiley.
1953 With E. A. Barrera. A method for the combined staining of cells and fibers in the nervous system. *J. Neuropathol. Exp. Neurol.* 12:400–403.

1955 With P. C. Bucy. An anatomical investigation of the temporal lobe in the monkey (Macaca mulatta). *J. Comp. Neurol.* 103:151–251.

1958 "The temporal lobe syndrome" produced by bilateral ablations. In *Neurological Basis of Behavior*, eds. G. E. W. Wolstenholme and C.M. O'Connor, pp. 175–82. Boston: Little, Brown and Company.

1965 Neurobiology of normal and abnormal perception. In *Psychopathology of Perception* , eds. P. H. Hoch and J. Zubin, pp. 1–40. New York: Grune & Stratton.

1966 *Mescal and Mechanisms of Hallucinations*. Chicago: University of Chicago.



Lewis G. Longsworth

Courtesy of the Rockefeller University Archives, New York, New York

Lewis Gibson Longsworth

November 16, 1904—August 8, 1981

BY ROBERT A. ALBERTY

THE FOCUS OF Lewis Longsworth's scientific research was the measurement of mobilities of ions and molecules in liquid media. Initially, he studied small ions under the influence of an electric field by the moving boundary method, and then he extended his research to the study of proteins and nucleic acids and then complexes. He made major contributions to the analysis of mixtures of proteins by electrophoresis.

Later in his career Longsworth studied the diffusion of neutral molecules in solution, where the gradient of the chemical potential provides the driving force. Diffusion affords a means for the study of the mobility of a nonelectrolyte in any medium in which it is soluble. He provided an early model of separation methods in laboratories to obtain purified biological substances. His experimental work was characterized by great care, high precision, and innovation of new experimental methods.

Longsworth spent his whole career at the Rockefeller Institute, where MacInnes, Shedlovsky, and Longsworth created one of the world's outstanding centers of electrolyte research and brought it to bear on biological problems.

THE EARLY YEARS

Lewis Longsworth was born in Somerset, Kentucky, on November 16, 1904, and spent his first dozen years in this community. His father operated a lumber mill, using hardwoods, especially the local hickory, until his death in an industrial accident in 1916. Lewis moved with his mother to Lexington, Kentucky (1916–18), and then to Winfield, Kansas (1918–25). This living in Winfield increased the warmth of the personal relationship I had with Lewis Longsworth, starting thirty years later, because I had been born in Winfield, and had left in 1925 when my family moved to Lincoln, Nebraska. Lewis mentioned in his autobiographical sketch[1] that Winfield "had been awarded a prize for being the best community in Kansas in which to raise children."

Lewis was salutatorian of his high school class when he graduated in 1922. His interest in science was kindled by his high school physics teacher. Lewis attended Southwestern College in Winfield, where he came under the influence of Larry Oncley, head of the Chemistry Department and father of a member of the National Academy of Sciences with the same name. Lewis concentrated on his studies, graduated from college in three years, and went to Kansas University for graduate work, where he earned his Ph.D. in three years.

Lewis did his doctoral research under the direction of H. P. Cady, the discoverer of helium in the natural gas from Dexter, Kansas, that wouldn't burn. His thesis was on the measurement of ion mobilities in liquid ammonia. He used the moving boundary method in which the movement of an interfacial region between solutions in a U-shaped tube is measured when an electric potential difference is applied across the tube. Upon completion of his doctoral work in 1928, Lewis was awarded a two-year term as a National Research Council fellow. In 1929 he was married to Helen Francis Cady, the youngest daughter of his research advisor.

FORTY-TWO YEARS AT THE ROCKEFELLER INSTITUTE

As a National Research Council fellow, Lewis went to the Rockefeller Institute to join the electrochemistry group started by Duncan A. MacInnes.[2] MacInnes had moved there from the Massachusetts Institute of Technology in 1926 at the suggestion of Dr. W.J.V. Osterhout, a physiologist, who felt that physiology could benefit from the advancement of fundamental electrochemistry. The laboratories of MacInnes and Osterhout were adjacent. MacInnes had gone to MIT in 1917 to join the Research Laboratory of Physical Chemistry, and had been appointed a professor in 1925. At the time he moved he was measuring transference numbers and the potentials of galvanic cells containing liquid junctions. One of the first things he did at the Rockefeller Institute was to hire Theodore Shedlovsky,[3] who had been his student at MIT. Longsworth and Shedlovsky were closely associated with MacInnes throughout nearly the entire period of his career at the Rockefeller Institute and Rockefeller University.

Longsworth stayed at the Rockefeller Institute until he retired as a professor emeritus in 1970. From 1930 to 1939 he had the rank of assistant, from 1939 to 1945 associate, and from 1945 to 1949 associate member, becoming a member in 1949 and a professor in 1954, when the Rockefeller Institute became Rockefeller University.

In his first years at the institute, Longsworth used the moving boundary method for increasingly precise measurements of transference numbers and ionic mobilities. These studies form the basis for current tables of these properties in aqueous electrolyte solutions. Many of the papers were published under his name alone, but he also published papers with Duncan MacInnes and Theodore Shedlovsky, who was making precise measurements of electric conductivities of electrolyte solutions and testing the applicability of the interionic attraction theory of Debye and Hückel in 1923, as later modified by Onsager in 1927 to include the relaxation effect and the electrophoretic effect. In addition, Lewis was involved in improving Ph measurements and using them in physiologic studies. Longsworth and MacInnes published on bacterial growth with automatic pH control and on the apparent oxidation-reduction potential, acid production, and population studies of *Lactobacillus acidophilus*.

In 1937 an important new development occurred when Arne Tiselius published his method for studying the electrophoresis of proteins. The study of protein mixtures by the moving boundary method is more difficult than the determination of mobilities of small ions because the boundaries are more easily disrupted by the convection resulting from the temperature gradient across the cell due to ohmic heating. When the moving boundary method is applied to strong electrolytes, the electric forces within the moving boundary keeps it sharp, but when protein molecules move in buffer solutions the moving boundaries were easily disrupted by convection. Tiselius's solution to this problem was to carry out the electrophoresis experiments at about 2°C, the temperature of maximum density of the buffer solution. Tiselius received a Nobel Prize in 1948. Tiselius used a schlieren band method for photographing boundaries between colorless solutions. A knife edge in the focal plane of the schlieren lens was used to cut off light from the cell that had been deflected by the refractive index gradient of a boundary. Actually the displaced slit "images" produce Gouy interference patterns in the focal plane of the schlieren lens, which I will refer to later. In 1938 Philpot showed that by adding a diagonal slit in the focal plane of the schlieren lens and a cylindrical lens to focus the plane of the slit on the photographic plate, a plot of refractive index gradient versus height in the electrophoresis cell could be obtained. This is referred to as the astigmatic camera. The term electrophoresis was originally introduced to mean the migration of charged colloidal particles in an electric field. Later it was generally applied to the movement of charged particles under the influence of an electric field.

Longsworth was well prepared to make contributions to the study of proteins by electrophoresis because he had been making precise measurements on electrolytes and understood the underlying theory of moving boundaries. He made a number of improvements in the Tiselius apparatus. His first experiments with proteins were in collaboration with Dr. Karl Landsteiner, an immunochemist, who was puzzled by the inability of the physical measurements then available to distinguish between proteins that could be readily identified by the antigen-antibody reaction. In an article published in 1938 Landsteiner and Longsworth found that the egg albumins of several closely related birds and the hemoglobin of a variety of animals had different electrophoretic mobilities. Longsworth also used electrophoresis to obtain purified biological substances.

During World War II Longsworth's electrophoretic studies were interrupted by an Office of Scientific Research for Defense project that led to a patent assigned to the U.S. government and some unclassified research on the electrochemistry of uranyl salt solutions.

In his research Longsworth obtained more accurate analyses of mixtures of proteins, such as blood plasma. In 1945 he published analyses of maternal and fetal plasmas and sera with R. M. Curtis and R. M. Pembroke. It was in this period that I became acquainted with Longsworth. I was working on a wartime medical research project on the isolation and purification of plasma proteins under the direction of Prof. J. W. Williams at the University of Wisconsin. This was part of a large project of Prof. Edwin J. Cohn at Harvard Medical School. It was Cohn's idea to separate and purify human blood proteins (selected ones) and use them in the treatment of medical and surgical conditions in the Armed Forces and also in the civilian population. Prof. John T. Edsall was the senior associate to Cohn, and Larry Oncley, Larry Strong, and John Ferry were important leaders in the project. One of my activities was to determine the purity of serum albumins and gamma globulins isolated from blood collected by the Red Cross. I used the University of Wisconsin's Tiselius apparatus, which had come from Sweden. The number of samples to be analyzed was so great that Lewis Gosting, who was also in the Wisconsin project, and I built the improved optical systems for the electrophoresis of proteins.

The 1940s was a very active time both for the development of the theory required for the quantitative interpretation of experiments in moving boundary electrophoresis and the improvement of equipment. Longsworth made major contributions both to theory and experimental techniques.

THEORY OF MOVING BOUNDARY ELECTROPHORESIS

When an interface is formed between two homogeneous electrolyte solutions, and an electric current is passed through the system, moving boundaries are formed. When the current has been passed for a long enough time, there are homogeneous regions between the boundaries. The theory of moving boundaries in electrolyte solutions starts with

Kohlrausch in 1876. He showed that the sum of ratios of ion concentrations to ion mobilities is constant throughout a moving boundary system, and this sum is called the regulating function. Longsworth made the next contribution to the theory of moving boundaries in 1945 by deriving the moving boundary equation, which relates the concentrations of an ion on either side of a boundary to the volume moved through by the boundary and the relative mobilities on either side of the boundary. The general solution to the problem of predicting the compositions of new solutions and the displacements of the separated boundaries was provided by Vincent Dole,[4] also of the Rockefeller Institute. Vincent Dole was a young medical doctor who had come to the Rockefeller Institute to work with D. D. Van Slyke and became acquainted with Longsworth, Shedlovsky, and MacInnes because of his mathematics background as an undergraduate and his interest in using the electrophoresis of plasma in the clinic. Vincent Dole understood that moving boundary systems are constrained by electroneutrality and mass balance. He showed that these constraints led to a relation between the volumes moved through by the various boundaries and the concentration changes across boundaries, subject only to constant relative ion mobilities. His demonstration dramatically changed the understanding of complicated moving boundary systems. According to Dole's equations, a system containing n ions will, in general, form a maximum of n - 1 boundaries, one of which is a stationary boundary. If a system contains p anions and q cations, there will generally be p - 1 boundaries with negative velocities and q - 1 boundaries with positive velocities. Dole showed that the volumes moved through by boundaries can be obtained as solutions of polynomials.

Starting in 1946 I corresponded with Longsworth a great deal about the moving boundary method and in 1947 I wrote my thesis with Prof. J. W. Williams on electrokinetic characterization of the gamma globulins from normal human blood plasma. I vividly remember several trips to the Rockefeller Institute to consult with Longsworth about the equipment and the interpretation of electrophoretic schlieren patterns. I was very much impressed with his knowledge about the moving boundary method, the high standards that he set for himself, his kindness and helpfulness, and his having lunch with me in the dining room of the Rockefeller Institute. I want to acknowledge my gratitude to Lewis Longsworth for his wise and patient counsel during the time (1946–60) that I was at the University of Wisconsin-Madison and was in frequent communication with him. Longsworth was basically a gentle man, and I can remember that he never told me that I was wrong, although he did tell me when he was skeptical. When he told me he was skeptical, I took it very seriously. Longsworth visited Madison in June 1952 to give an All University Lecture.

ELECTION TO THE NATIONAL ACADEMY OF SCIENCES

Longsworth became a member of the National Academy of Sciences in 1947. In 1953 he was appointed by President Bronk to serve on an Academy committee on battery additives. The formation of this committee had been requested by Secretary of Commerce Sinclair Weeks shortly after the head of the National Bureau of Standards A. V. Astin had been dismissed, thereby disturbing the scientific community. The head of a private company was promoting a certain battery additive, but on the basis of the Bureau's tests Astin thought the claims were false and repudiated them. Secretary Weeks fired Astin and forced him out of the Bureau. The scientists were outraged because this seemed to them a blow to honest science, and they petitioned the

Academy to carry out an investigation. The four months' interval from the time of appointment of the committee on June 24, 1953, to the release of its report on October 30 was devoted to the reading and appraisal of the many reports on the battery additive AD-X2 and the meetings of the committee under the direction of Zay Jeffries. The release of the report supporting Astin was not the end of the committee's duties. The subsequent action of the Federal Trade Commission against Pioneers, Inc., makers of AD-X2, involved Lewis Longsworth and two other members of the committee as witnesses in January 1955. Although the charge of false advertising made by the Federal Trade Commission against Pioneers, Inc., was dropped on May 16, 1956, the work of the committee had been a factor in the reinstatement of Astin as head of the National Bureau of Standards in October 1953. It was not until December 1961 that the suit Pioneers, Inc., brought against the U.S. Court of Claims in 1959 was rejected and science finally prevailed over politics.

Longsworth also served the Academy as a member of a committee that President Seitz appointed in August 1962 to evaluate the merits of a classified project of the Bureau of Ships.

DIFFUSION OF ELECTROLYTES AND PROTEINS

In the mid-1950s Longsworth's interests shifted from moving boundary measurements with electric fields to measurements of diffusion constants in aqueous solutions. The reason for this was that it was becoming increasingly clear that moving boundary measurements in free solution for analyses of biochemical mixtures of both low and high molecular weights were going to be replaced by zone techniques in stabilized media. Longsworth's interests in diffusion had been kindled in 1944 by the organization of a conference on this subject under the auspices of the New York Academy of Sciences, for which he prepared a historical survey. In this survey he included a photograph he had taken of the interference fringes that had been obtained by G. L. Gouy in 1880, but which had not been subsequently used. When a boundary is formed between two solutions and a substance is diffusing from the lower solution to the upper solution, the image of an illuminated horizontal slit is spread out into a rectangular pattern of interference fringes. The lower edge is formed by the light that has passed through the solution where the gradient of a refractive index is the steepest. Interference fringes are formed by rays that pass through layers of equal gradient above and below the center of the boundary for which the path difference gives constructive interference. Thus, Longsworth revived the use of Gouy fringes for determining diffusion coefficients, which was brought to its highest level by Prof. L. J. Gosting and associates in the Enzyme Institute at the University of Wisconsin.[5] [6] Gosting had spent a year at the Rockefeller Institute in the MacInnes-Longsworth-Shedlovsky group.

Longsworth was a member of the American Chemical Society, Electrochemical Society, Harvey Society, and Sigma Xi. He was the recipient of the 1968 American Chemical Society Award in Chromatography and Electrophoresis. In 1978 he was cited by *Lab World* for his "important contributions to the ever-increasing and expanding adaptations of electrophoresis and chromatography in laboratory medicine." In 1978 he received an honorary doctorate of science degree from Rockefeller University at its commencement ceremony.

Longsworth was very dedicated to his research and did not have much time for hobbies. He and Helen had three children (Anne Louise, Ralph Cady, and Stella Caroline). The Longsworths spent their summer vacations in Estes

Park, Colorado, hiking in the Rocky Mountain National Park and visiting other parks and natural forests in the west. They worked at hiking at increasing elevations, and the ascent of Long's Peak at an elevation of 14,256 feet was frequently the climax of their vacation. Helen climbed Long's Peak on fifteen different occasions, whereas Lewis's count was thirteen. Lewis Longsworth died of a stroke during a vacation in Estes Park.

Longsworth provided a standard of excellence in the measurement of mobilities of ions and molecules from low molecular masses to those of proteins and nucleic acids. His first contributions were to the transference numbers of inorganic ions, but this led to improvements in both the theory and experimental methods for the electrophoresis of proteins. In his later years he turned to the measurement of mobilities using diffusion in free solution. He supported and encouraged many others working on these problems, and his criticisms and suggestions were welcomed by many people working on the determination of the properties of proteins and nucleic acids and the analysis of mixtures.

I WANT TO ACKNOWLEDGE the assistance of John T. Edsall and Vincent Dole in writing this biographical memoir. Renee D. Mastrocco, archivist at the Rockefeller University, provided Lewis Longsworth's autobiographical sketch and other information.

NOTES

1. L. G. Longsworth. Autobiographical sketch. Archives of the Rockefeller University.
2. L. G. Longsworth and T. Shedlovsky. Duncan A. MacInnes, 1885–965. In *Biographical Memoirs*, vol. 41, pp. 295–317. New York: Columbia University Press for the National Academy of Sciences, 1970.
3. R. M. Fouss. Theodore Shedlovsky, 1898–1976. In *Biographical Memoirs* , vol. 52, pp. 379–408. Washington, D.C.: National Academy Press, 1980.

4. V. Dole. Theory of moving boundary systems formed by strong electrolytes. *J. Am. Chem. Soc.* 67 (1945):1119–26.
5. L. J. Gosting. Measurement and interpretation of diffusion coefficients of proteins. In *Advances in Protein Chemistry*, eds. M. L. Anson, K. Bailey, and J. T. Edsall, pp. 429–554. New York: Academic Press, 1958.
6. P. J. Dunlop and L. J. Gosting. Use of diffusion and thermodynamic data to test the Onsager reciprocal relations for the isothermal diffusion of the system NaCl-KCI-H_2O at 25°C. *J. Phys. Chem.* 63(1959):86–93.

Selected Bibliography

1929 With H. P. Cady. A modification of the moving boundary method for the determination of transference numbers. *J. Am. Chem. Soc.* 51:1656–64.

1932 Transference numbers of aqueous solutions of potassium chloride, sodium chloride, lithium chloride and hydrochloric acid at 25°C by the moving boundary method. *J. Am. Chem. Soc.* 54:2741–58.

With D. A. MacInnes and T. Shedlovsky. Limiting mobilities of some monovalent ions and the dissociation constant of acetic acid at 25°C. *Nature* 130:774–75.

1936 With D. A. MacInnes. Bacterial growth at constant pH: Quantitative studies on the physiology of lactobacillus acidophilus. *J. Bacteriol* . 31:287–300.

1937 With D. A. MacInnes. Transference numbers and ion mobilities of some electrolytes in deuterium oxide and its mixtures with water. *J. Am. Chem. Soc.* 59:1666–70.

1938 With K. Landsteiner and J. van der Scheer. Electrophoresis experiments with egg albumins and hemoglobin. *Science* 88:83–85.

1939 With T. Shedlovsky and D. A. MacInnes. Electrophoretic patterns of normal and pathological human serum and plasma. *J. Exp. Med.* 70:399–413.

1940 With D. A. MacInnes. The interpretation of simple electrophoretic patterns. *J. Am. Chem. Soc.* 62:705–11.

1942 With D. W. Wooley. Isolation of an antibiotic factor from egg white. *J. Biol. Chem.* 142:285–90.
With D. A. MacInnes. An electrophoretic study of mixtures of ovalbumin and yeast nucleic acid. *J. Gen. Physiol.* 25:507–16.
1943 A differential moving boundary method for transference numbers. *J. Am. Chem. Soc.* 65:1755–65.
1945 With R. M. Curtis and R. M. Pembroke. The electrophoretic analysis of maternal and fetal plasmas and sera. *J. Clin. Invest.* 24:46–53
1947 The quantitative interpretation of electrophoretic patterns of proteins *J. Phys. Colloid Chem.* 51:171–83.
Experimental tests of an interference method for the study of diffusion. *J. Am. Chem. Soc.* 69:2510–16.
1949 With C. F. Jacobsen. An electrophoretic study of the binding of salt ions by beta-lactoglobulin and bovine serum albumin. *J. Phys. Colloid Chem.* 53:126–35.
1951 Interferometry in electrophoresis. *Anal. Chem.* 23:346–48.
1952 Diffusion measurements at 1°C of aqueous solutions of amino acids, peptides, and sugars. *J. Am. Chem. Soc.* 74:4155–59.
1953 Diffusion measurements at 25°C of aqueous solutions of amino acids, peptides, and sugars. *J. Am. Chem. Soc.* 75:5705–5709.
1957 Exchange diffusion of ions of similar mobility. *J. Phys. Chem.* 61:244–48.

1959 Moving boundary electrophoresis—theory. In *Electrophoresis*, ed. M. Bier, pp. 91–136. New York: Academic Press.

Moving boundary electrophoresis—practice. In *Electrophoresis*, ed. M. Bier, pp. 137–77. New York: Academic Press.

The concentration and temperature dependence of the Soret coefficient of some aqueous electrolytes. In *The Structure of Electrolytic Solutions* , ed. W. J. Hamer, pp. 183–99. New York: Wiley.

1960 The mutual diffusion of light and heavy water. *J. Phys. Chem.* 64:1914–17.

1966 The diffusion of hydrogen bonded solutes in carbon tetrachloride. *J. Colloid Interface Sci.* 22:3–11.

1968 Diffusion in liquids. In *Physical Techniques in Biological Research,* ed. D. H. Moore, pp. 85–120. New York: Academic Press.

Courtesy of the Rockefeller University Archives, New York, New York

Alfred Ezra Mirsky

October 17, 1900–June 19, 1974

BY SEYMOUR S. COHEN

ALFRED EZRA MIRSKY, SON OF Michael David Mirsky and Frieda Ittelson Mirsky, graduated from the Ethical Culture School in New York City and from Harvard College, obtaining a B.A. degree in 1922. He studied at the College of Physicians and Surgeons of Columbia University for two years. On receipt of a fellowship from the National Research Council in 1924, he worked at Cambridge University under Joseph Barcroft during the academic year 1924–1925, and completed his graduate studies under Lawrence J. Henderson at Harvard. He wrote a dissertation titled "The Haemoglobin Molecule" and received a Ph.D. from Cambridge in 1926.

The molecularity of haemoglobin and the molecular weight of the protein were established by Theodor Svedberg and Gilbert Adair in 1925. Their results demonstrated that proteins are rigorously definable species of large molecules, and were important in showing that proteins, despite their size, should be described in the molecular terms of the chemist. The initial postulates of protoplasmic components as being essentially undefinable, dispersible, and colloidal aggregates were eventually replaced by the view that the

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major constituents of protoplasm contain protein molecules whose shape, charge, and state of aggregation are markedly affected by the response of their ionizable groups to the hydrogen ion concentration (pH). Mirsky's early papers demonstrate that he had adopted a rigorously chemical orientation from the beginning of his career.

In 1924 and 1925 Mirsky published eight papers on hemoglobin with Mortimer L. Anson, of which the first two had Barcroft as coauthor. Anson was a fellow student in Barcroft's laboratory, as well as a later colleague at the Biophysics Laboratory of the Cancer Commission at Harvard. The collaboration of Mirsky and Anson on hemoglobin continued until 1935. As presented in preliminary form in Mirsky's dissertation, the disruption of protein organization and precipitation (denaturation) had been observed to be reversible. The mechanisms of these phenomena were the major problems studied by these young investigators for the next decade.

On 25 May 1926 Mirsky married Reba Paeff; they had a daughter and a son. In 1927 Mirsky was appointed an assistant in the laboratory of Alfred F. Cohn at the hospital of the Rockefeller Institute for Medical Research. Anson was appointed to the laboratory of John Howard Northrop at the Princeton branch of the institute. Cohn was engaged in the quantitation of activities of the human heart. Mirsky began his work in Cohn's laboratory with studies of pH in the blood of developing chicks, using a glass electrode developed with Anson. The increase in pH simulated the curve of decrease in oxygen consumption during development. The resulting paper marked the end of Mirsky's association with Cohn's research program, since Cohn had become more interested in humanistic studies than in those of laboratory science.

Mirsky and Anson resumed the study of hemoglobin and its denaturation and renaturation. In a second series of papers written between 1929 and 1935, they showed that protein coagulation takes place in two steps, in which unfolding can be separated from precipitation. Horse hemoglobin, coagulated by various methods, including heat or acid, can be solubilized and its unfolded state converted by a cyanide solution to a state indistinguishable from native hemoglobin; this product can then be crystallized to a denaturable hemoglobin. The denaturable portion of the hemoglobin was shown to be the globin; it was found that other denaturable proteins, such as serum albumin, can be renatured. These results were extended to the formation of an active trypsin from an inactive denatured enzyme.

Free sulfhydryl groups appeared in the denaturation of egg albumin and serum albumin. Although other researchers had proposed that such groups are generated from disulfide bonds during denaturation, another hypothesis was formulated in 1936, during Mirsky's sabbatical year at the California Institute of Technology. Mirsky and Linus Pauling then proposed that native proteins are coiled in specific configurations whose parts are stabilized largely by hydrogen bonds, and that unfolding and denaturation reveal groups previously obscured and protected by the originally folded chains. This paper was an important early statement of now-current views of protein structure and of the mechanism of denaturation and renaturation. Both views of the appearance of sulfhydryl groups are now believed to be correct, since in many proteins native structure is also maintained by disulfides whose reduction generates sulfhydryls and opens the structure.

In 1937 Mirsky studied changes in muscle proteins in an attempt to correlate them with functional alterations in the muscle as a result of elevated temperature. An irreversible muscle shortening, known as thermal rigor, was associated with the appearance of sulfhydryl groups, and he concluded that this phenomenon was due to a denaturation. He also believed that denaturation was part of muscle contraction generally, although he realized that not enough was known about muscle proteins at the time.

An interest in structural proteins led Mirsky to attempt the isolation of a protein complex that had been described in 1938 as derived from the cytoplasm. The fibrous material was found to contain large amounts of deoxyribonucleic acid (DNA) and was clearly nuclear in origin. He then began a new line of investigation, attempting to understand the structural and functional significance of nuclear nucleoproteins. In the mid 1930's the discovery that the plant viruses are ribonucleoproteins and that ribonucleic acid (RNA) is present in cytoplasmic particulates had begun the growth of biochemical interest in the nucleic acids, culminating in the 1944 discovery by O. T. Avery and his colleagues that DNA is the pneumococcal transforming agent. By 1941 enough was known of the relation of genes to chromosomes to pose the problem of the chemical nature of the hereditary determinants. In approaching this question, Mirsky, who in 1940 had become an associate member of the Rockefeller Institute, established a collaboration with the cytochemist Arthus W. Pollister, of the department of zoology at Columbia University. For the next decade Mirsky's laboratory at Rockefeller Institute was a leading center for structural and functional studies of cell nuclei.

Mirsky's first formal paper with Pollister describes the extraction of nucleoproteins from a wide variety of animal cells. Their major approach was the differential use of neutral sodium chloride solutions of varying concentrations: physiological saline removed protein and cytoplasmic constituents; concentrated saline (1M to 2M) extracted DNA and other proteins that were precipitated in physiological saline. (It was shown later than concentrated saline dissociated DNA and proteins.) These soluble components reassociated and reprecipitated at lower salt concentrations. Hence, the mild procedure, often suitable for the isolation of DNA, nevertheless introduced restructuring of the original cellular complex. Some thirty years later it was shown that chromosomal subunits of DNA and various proteins, known as nucleosomes, are isolable without dissociation, and hence represent a better approach to the isolation of a more native DNA-protein complex. Nevertheless, the DNA-protein complexes isolated in 1941 were shown to come from cell nuclei and were believed to be components of chromosomes. Mirsky's review of the status of this field in 1943 is a useful summary of this early period of the biochemistry of genetic material. Mirsky did not speculate as to the specific chemical nature of the gene; he suggested that the newly isolated DNA proteins were either "the genes themselves or were intimately related to genes."

Mirsky and Pollister then attempted to isolate chromatin from the nuclei of certain types of animal cells and thought that they had obtained threads of this material, which might even have been intact chromosomes. From 1946 to 1951 these efforts were extended with the collaboration of Hans Ris. Threads possessing the main cytological features of chromosomes were isolated from calf thymus lymphocytes and analyzed. Over 90 percent were found to consist of nucleohistone containing DNA. An insoluble residue contained protein determining the form of the "chromosome" as well as some RNA and DNA. These studies were extended to the isolation of similar "chromosomes" from many kinds of cells, including more voluminous structures from interphase nuclei. In his last papers, however, Mirsky became more circumspect about problems of isolating active chromatin.

In 1948 Mirsky became a member of the Rockefeller Institute; occupying new laboratories, he enlarged his group, which in addition to Ris included Vincent Allfrey, Marie Daly, and Herbert Stern. Foreign visitors such as Alberto Monroy began to work on problems of chemical embryology. With Ris, Mirsky showed that diploid somatic cells of an organism contain identical amounts of DNA, twice that of haploid germ cells. In 1950 Hermann J. Muller congratulated Mirsky and referred to the "grand discovery" of DNA constancy, which supported the concept of DNA as the hereditary material.

In 1950, in a symposium commemorating the fiftieth anniversary of the rediscovery of Mendel's work, Mirsky presented a paper titled "Some Chemical Aspects of the Cell Nucleus." Noting the constancy of DNA, in contrast with the variability of RNA, he concluded that DNA is part of the gene substance. Nevertheless, some six years after Avery's discovery, he was still unconvinced that DNA itself was the sole genetic material, pointing out the insensitivity of the assay and difficulty of assuring that minute quantities of protein are not attached to the DNA. As noted by Norman W. Pirie, who had similar reservations in later years concerning the infectivity of viral RNA, "Scepticism and objectivity are near neighbors." The chemical evidence of the purity of transforming DNA or of an infectious RNA was little better in the late 1980's than it was in the 1950's. Nevertheless, new bodies of data demonstrate the validity of the views that certain nucleic acids themselves may determine genetic continuity, and that sequences of bases in these polymeric nucleates determine the specificity of the genetic units.

In the early 1950's Mirsky and his colleagues turned to the problems of the regulation of gene expression and other metabolic activities of cell nuclei. Much work was done on the enzymatic content of nuclei and on their capacity to generate energy and to effect various syntheses. They demonstrated glycolytic systems in nucleic, as well as the nuclei's ability to synthesize and utilize adenosine triphosphate (ATP) in the synthesis of RNA. As it became clear that nuclei contain many proteins and enzymatic functions, many laboratories joined in this work, which had now merged with the broad front of the advance of knowledge of cellular and organelle structure and metabolic function. By the end of the 1960's the concluding work of the laboratory was concerned with problems of embryological development (with H. Naora and E. Davidson), observations on the modification of histones by acetylation and methylation, and on the effects of such substitutions on gene expression (with Vincent Allfrey and B. G. T. Pogo), and many other aspects of the contents and activities of cell nuclei. Mirsky's last papers, published in the early 1970's, were on the role of the histones in the structure of chromatin and in its replication and transcription.

In 1954, when the institute became the Rockefeller University, Mirsky's title was "professor." He became quite active in university affairs, particularly on committees concerned with the graduate program. In 1959 Mirsky initiated a series of lectures for high school students, now named in his honor. Following retirement from his laboratory in 1964, he served as librarian of the Rockefeller University from 1965 until 1972. His wife died in 1966, and he married Sonia Wohl in 1967. Mirsky became professor emeritus in 1971, after forty-four years at the Rockefeller Institute and University.

Mirsky's most active laboratory investigations occurred in the first thirty years at the Rockefeller Institute. His early studies on protein structure had enabled him to develop a new line of work that both pioneered in an understanding of cell organization and genetic chemistry and merged with the major biochemical advances of the period. The significant accomplishments of his laboratory led to his election to the National Academy of Sciences in 1954.

From 1951 to 1961 Mirsky served as an editor of the *Journal of General Physiology*. From 1959 to 1965 he was a coeditor, with Jean Brachet, of the compendium *The Cell*. Between 1954 and 1964, he was awarded honorary degrees by the University of Gothenburg, the University of Santiago de Chile, and the University of Palermo. The breadth of his interests and accomplishments, and his extensive writings of reviews and historical essays, led to his election in 1964 to the American Philosophical Society. Mirsky traveled widely and was quite knowledgeable in archaeology and art history; his fine collection of art and historical objects is at the Rockefeller University.

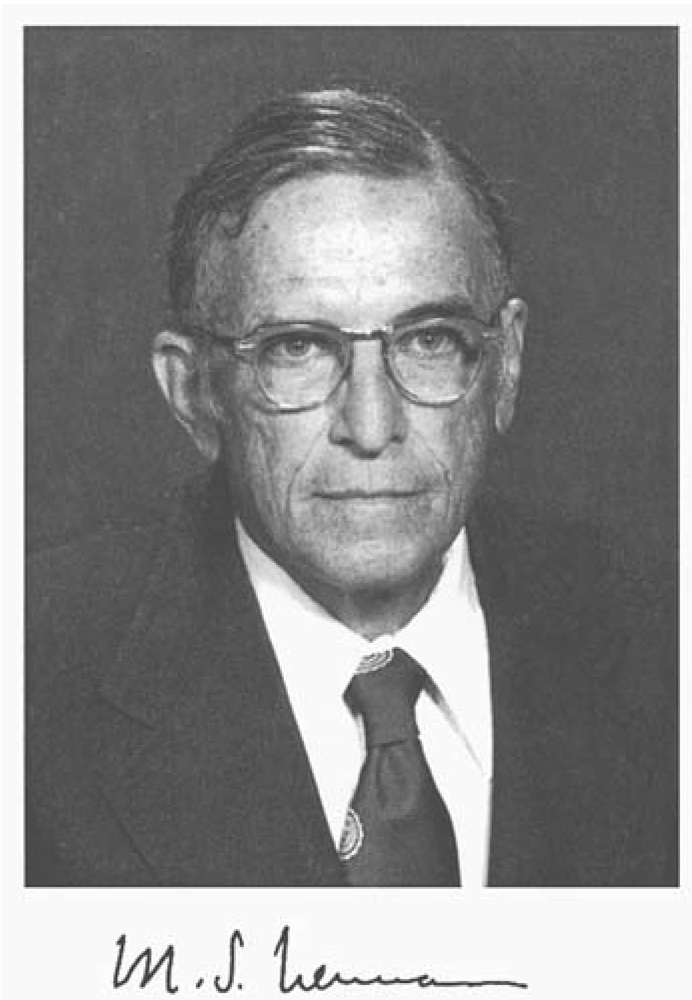
Bibliography

II. ORIGINAL WORKS. A chronological list of Mirsky's publications is available from the Rockefeller University archives. Key publications include "On the Correlation Between the Spectra of Various Haemoglobins and Their Relative Affinities for Oxygen and Carbon Monoxide," in *Proceedings of the Royal Society of London,* **B97** (1924), 61–83, with Mortimer L. Anson, Joseph Barcroft, and S. Oinuma; "On Some General Properties of Proteins," in *Journal of General Physiology*, **9** (1925), 169–179, with Mortimer L. Anson; "A Description of the Glass Electrode and Its Use in Measuring Hydrogen Ion Concentration," in *Journal of Biological Chemistry* , **81** (1929), 581–587, with Mortimer L. Anson; "Protein Coagulation and Its Reversal: The Reversal of the Coagulation of Hemoglobin," in *Journal of General Physiology* **13** (1929), 133–143, with Mortimer L. Anson; "Protein Coagulation and Its Reversal: Serum Albumin," *ibid.*, **14** (1931), 725–732, with Mortimer L. Anson; "The Equilibrium Between Active Native Trypsin and Inactive Denatured Trypsin," *ibid.*, **17** (1934), 393–398, with Mortimer L. Anson; "Sulfhydryl and Disulfide Groups of Proteins: II. The Relation Between Number of SH and S-S Groups and Quantity of Insoluable Protein in Denaturation and in Reversal of Denaturation," *ibid.*, **19** (1935), 427–438, with Mortimer L. Anson.

"On the Structure of Native, Denatured, and Coagulated Proteins," in *Proceedings of the National Academy of Sciences*, **22** (1936), 439–447, with Linus Pauling; "Protein Denaturation," in *Cold Spring Harbor Symposia on Quantitative Biology*, **6** (1938), 150–163; "Nucleoproteins of Cell Nuclei," in *Proceedings of the National Academy of Sciences* , **28** (1942), with Arthur W. Pollister; "Fibrous Nucleoproteins of Chromatin," in *Biological Symposia*, **10** (1943), 247–260, with Arthur W. Pollister; "Chromosomes and Nucleoproteins," in *Advances in Enzymology* , **3** (1943), 1–34; "The Chemical Composition of Isolated Chromosomes," in *Journal of General Physiology*, **31** (1947), 7–18, with Hans Ris; "The State of the Chromosomes in the Interphase Nucleus," *ibid.*, **32** (1949), 489–502, with Hans Ris; "The Deoxyribonucleic Acid Content of Animal Cells and Its Evolutionary Significance," *ibid.* **34** (1951), 451–462, with Hans Ris; "Some Chemical Aspects of the Cell Nucleus," in Leslie C. Dunn, ed., *Genetics in the 20th Century* (New York, 1951), 127–153; "Some Enzymes of Isolated Nuclei," in *Journal of General Physiology*, **35** (1952), 559–578 , with Herbert Stern, Vincent G. Allfrey, and Hans Saetren; "The Chemistry of the Cell Nucleus," in *Advances in Enzymology*,

16 (1955), 411–500, with Vincent G. Allfrey and Herbert Stern; "Protein Synthesis in Isolated Cell Nuclei," in *Journal of General Physiology* , **40** (1957), 451–490 , with Vincent G. Allfrey and Syozo Osawa; "Mechanisms of Synthesis and Control of Protein and Ribonucleic Acid Synthesis in the Cell Nucleus," in *Cold Spring Harbor Symposia on Quantitative Biology*, **28** (1963), 247–262, with Vincent G. Allfrey; "Changing Patterns of Histone Acetylation and RNA Synthesis in Regeneration of the Liver," in *Proceedings of the National Academy of Sciences*, **59** (1968), 1337–1344, with B. G. T. Pogo, A. O. Pogo, and Vincent G. Allfrey; "The Structure of Chromatin," *ibid.*, **68** (1971), 2945–2948; and, with Jean Brachet as coeditor, *The Cell: Biochemistry, Physiology, Morphology*, 6 vols. (New York, 1959–1964). The Rockefeller University archives have extensive holdings of Mirsky's notebooks, correspondence, and other documents, and supplies a curriculum vitae.

II. SECONDARY LITERATURE. See George Washington Corner, *A History of the Rockefeller Institute (1901–1953): Origins and Growth* (New York, 1965); Carolyn Kopp, "The Alfred E. Mirsky Papers at the Rockefeller University Archives," in *The Mendel Newsletter*, no. 23 (November 1983), 1–5; and Bruce S. McEwen, "Alfred Ezra Mirsky (1900–1974)," in *American Philosophical Society, Year Book 1976* (1977), 100–103.



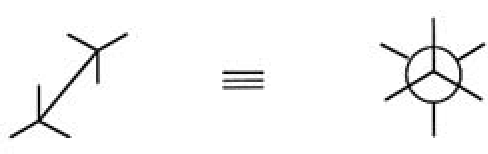
Courtesy of the Ohio State University Archives

Melvin Spencer Newman

March 10, 1908–May 30, 1993

BY LEO A. PAQUETTE AND MILTON ORCHIN

EVERY UNDERGRADUATE STUDENT enrolled in a course dealing with organic chemistry is quickly made familiar with the Newman Projection, named after Melvin Newman, because this method of representing the geometry of organic molecules appears in practically every text of organic chemistry. As originally suggested by Newman in 1952, this technique involves looking down the bond between two connected (sp^3-hybridized) carbon atoms and projecting the three-dimensional structure around this bond onto a plane surface. For example, the simple two-carbon compound ethane is represented in a line formula by CH_3-CH_3 and conventionally by the side-on perspective (sawhorse) formula (see A below). The arms of the Y and the inverted Y are understood to represent the six hydrogen atoms, the points at the intersection of the arms represent the carbon atoms, and the diagonal line connecting the Y's represents the carbon-carbon bond. In the Newman projection (see B below) the view of the molecule is not



A B

from the side but rather down the C-C bond axis so that the front carbon eclipses the rear carbon. Then to bring the formula onto the plane of the paper as shown, one imagines that the two Y's of (A) are squashed together, that is, the front inverted Y (CH_3) is projected backward and the rear Y (CH_3) is projected forward to the plane of the paper to give the plane projection formula (B), the Newman Projection. Here the front carbon is represented by the intersection of the three arms of the front Y and the rear carbon, which is eclipsed by the front one imagined to be at the center of the rear circle. Hence the spatial relation between the two carbons (which is in front and which is in back) is defined and thus a major problem with plane projections is finessed. The Newman projection is not only admirably suited for assessing steric effects but is also useful for analyzing symmetry properties. Thus the formula (B) readily reveals that the (inferred) carbon-carbon bond axis in ethane is not only a three-fold rotational axis but more importantly is a six-fold alternating axis (S6-in symmetry notation) as well, thus accounting for the equivalency of all six hydrogen atoms and the fact that only one mono-substitution product of ethane is possible.

PERSONAL HISTORY

Melvin S. Newman, a member of the National Academy of Sciences since 1956, died on May 30, 1993, in Columbus, Ohio, at the age of eighty-five. Thus ended the long and productive career of an enthusiastic and inspiring teacher and an outstanding pioneer in the fundamental development of organic chemistry for more than five decades. His lasting contributions to research and to Ohio State University with which he was associated for fifty-seven years were instrumental in maintaining excellence at his adopted institution. Mel's enormous impact was prominently recognized by naming the most recent building addition to the chemistry department the Newman-Wolfrom Laboratory.

Melvin Spencer Newman was born on March 10, 1908, in New York City, the fourth and last child of Jacob Kiefer Newman and Mae Polack Newman. His father made a specialty of reorganizing companies in financial difficulty to satisfy the claims of different stock and bond holders. Shortly after Mel's birth, the family moved to New Orleans, Louisiana. There he attended the Isadore Newman Manual Training Academy, a school founded by his paternal grandfather. Mel's chief interest as a child was in sports, except for membership in a short-lived chemistry club, where his chief activity, as he put it, "was to mix gun powder and deposit same in envelopes on street car tracks."

When he was fourteen years old, his family moved back to New York City where he missed his friends and street play. For his high school education, he attended Riverdale County School. Recognizing Mel's precocity, his parents secured a private chemistry tutor for him who did much to stimulate Mel's interest in the field. After graduating from Riverdale in 1925, he enrolled at Yale University and received his B.S. degree magna cum laude in 1929. He excelled in mathematics and chemistry, and particularly enjoyed a semester reading and studying Shakespeare. During this time, he developed an absorbing interest in music both classical and jazz. He was particularly fond of New Orleans jazz and a great admirer of Louis (Satchmo) Armstrong, with whom he became very friendly later in his career. His early life in New Orleans left indelible impressions, including an accent and way of speaking associated with that fascinating city.

One of Mel's loves while at Yale was golf, a sport at which he was notably skillful, having once played in the National Intercollegiate Tournament. Although discouraged from continuing his graduate work at Yale, he insisted on advancing his studies there, one reason being that no other university that he considered going to had a golf course. He even offered to pay his own tuition at Yale rather than receive a fellowship, an offer he soon rescinded after learning that his father had lost his money in the Wall Street crash of 1929 and could no longer support him.

Mel Newman completed his graduate studies in three years and received his Ph.D. degree at Yale in 1932 (Fleischman fellow, 1930–32) for research conducted on the chemistry of lipids under the direction of Professor Rudolph J. Anderson. Thereafter he accepted three postdoctoral fellowships in sequence. The first, supported by the National Tuberculosis Association was held at Yale (1932–33). He then joined Columbia University as a National Research Council fellow (1932–33) and then Harvard University as an Eli Lilly fellow (1934–36). This last position, spent in the laboratories of Professor Louis Fieser, initiated Mel's long-lasting interest in the field of polycyclic aromatic hydrocarbons.

His independent academic career began in the fall of 1936 as an instructor at Ohio State University, where he remained during his entire professional career. In 1940 he became an assistant professor and in 1944, in an unprecedented promotion, was awarded full professorship status.

Melvin Newman was married on June 30, 1933, to Beatrice Naomi Crystal of New Rochelle, New York, the daughter of a very successful retail merchant of women's fashion wear. Bea's reputation as a most gracious hostess caused the Newman home to be a popular meeting place for visiting scientists, faculty, and students. The Newmans had four children: Anthony Kiefer, Susan Crystal, Beth Clair (deceased), and Robert Melvin.

PROFESSIONAL HISTORY

Newman's interest in steric effects and what he called "overcrowded" molecules was a central theme during his entire research career and resulted in many seminal contributions to the elucidation of reaction mechanisms and an appreciation of the chiral properties of various types of molecules. The book *Steric Effects in Organic Chemistry,* which he edited in 1956, integrated a great variety of phenomena whose understanding was facilitated by the consideration of steric effects and was thus of great service to the chemical community.

One of the most enduring of Newman's research interests involved the synthesis and properties of substituted 1,2-benzanthracenes and related compounds. His very first publication from Ohio State University (1937) was entitled "The Synthesis of 1,2-Benzanthracene Derivatives Related to 3,4-Benzpyrene." One of the last of his many papers written approximately half a century later (1986) was a contribution in the same series entitled "An Unusual Synthesis of 5-Methoxy-7,12-dimethyl-1,2-benzanthracene." His interest in polynuclear aromatic compounds was sustained by the practical importance of these substances to the understanding of their carcinogenic activity, as well as by the scientific drive to understand the profound influence of steric interactions on structure and properties.

Newman's interests were not confined to the carcinogenic properties of the polycyclic hydrocarbons. Such compounds also provided a venue for some of his most impressive work on steric effects. One of the early triumphs of Mel's work was the synthesis of 4,5-dimethylchrysene. Predictions had been made that this hydrocarbon would not be amenable to synthesis because of the extreme overcrowding of the methyl groups and the necessity that the four benzene rings remain coplanar because of pi electron delocalization. Mel proceeded to demonstrate that the rings in this and related compounds were indeed bent and furthermore that such structures should be chiral.

The developments to follow attracted international acclaim. In 1955 Newman demonstrated the capacity to sustain optical activity by synthesizing and resolving hexahelicene. The absence of functionality on this structurally unusual hydrocarbon required that resolution be accomplished in a novel manner. Through charge transfer complexation with a tetranitrofluorenyl derivative, diastereomeric complexes were generated and then successfully separated. The enantiomers proved to have unusually high specific rotation ([a] > 3700°). Hexahelicene became the first member of a new class of aromatic hydrocarbons that coil to form a helix.

Once it became clear that the metabolic dihydrodihydroxy epoxides of the aromatic polynuclears rather than the hydrocarbon precursors themselves were the carcinogens, and that the activity was associated with certain regions of these hydrocarbons, Mel was stimulated to synthesize 7,11,12-trimethyl-1,2-benzanthracene. This specifically targeted compound proved to be one of the most potent hydrocarbon precursors to carcinogenic activity ever synthesized.

During the course of developing required synthetic methodology, Newman encountered some unusual esterification behavior of *o*-benzoylbenzoic acids and their analogs. He found that normal acid-catalyzed esterification gave cyclic esters. About this time, L. P. Hammett published his seminal book *Physical Organic Chemistry*. Stimulated by some of the results described therein involving 100% H_2SO_4, Mel undertook a series of studies in which a variety of oxygen-containing structures were dissolved in this solvent and the number and kind of various ionic species present were deduced from the cryoscopic behavior. With characteristic enthusiasm, he addressed the question of how this information could be usefully applied to synthetic problems with the result that anyone wishing to esterify highly hindered acids, which are resistant to normal esterification, now uses his methodology of dissolution in 100% H_2SO_4 followed by quenching with the alcohol of choice.

Newman was among the first to propose the existence of vinyl carbocations, as he was seeking to rationalize the course of reactions of oxazolidinones with base. When Jack Hine suggested that the results could also be explained by the intermediacy of unsaturated carbenes, the end products of vinyl carbocation deprotonation, Mel proceeded to show that Hine was correct by preparing such intermediates and demonstrating their utility.

Melvin Newman had a long and productive career. His articles are concise, clear, and direct. His writing and lectures were the embodiment of Albert Einstein's admonition: "Everything should be made as simple as possible but not simpler." As a consequence, he was a popular and much sought-after lecturer and gave hundreds of invited seminars in the United States and around the world. For his scientific contributions, Mel received many high honors, including the American Chemical Society Award for Creative Work in Synthetic Organic Chemistry (1961), the Morley Medal of the Cleveland Section of the American Chemical Society (1969), the Wilbur Lucius Cross Medal of Yale University (1975), an honorary doctor of science from the University of New Orleans (1975), the Columbus Section American Chemical Society Award (1976), and the Sullivant Medal, the highest award bestowed by Ohio State University (1976).

REMINISCENCES

Mel was known and respected for his warm openness, his infectious enthusiasm, and his complete integrity. In addition to his passion for golf and chemistry (he often remarked how fortunate he was to be paid for something that he loved to do), Newman took very special interest in family, students, music, food, and humor. His beloved children vividly recall the pungent chemical odors that they detected coming from his ties and accordingly his Hawaiian shirts were never viewed by them as smelling of exotic flowers. Mel always encouraged his children to hard work and implanted in them a strong pursuit of their personal interests without being intrusive.

He was held in great affection by his many students, and deservedly so because he cared deeply about them both professionally and personally. Mel had a fervent love for hands-on laboratory work, at which he was very proficient, and he inspired his associates along these lines. He would take great delight in watching his students mature. Sometimes he took over at the bench with his own hands with phrases such as "If I were running that reaction, I'd do it this way." At other times, he could be heard saying "The crystals are great, but what about the yield?" He was fascinated by crystals and continued work in the laboratory to the very end.

The sounds that came from his study were either classical or jazz-oriented. His order of preference was Louis Armstrong, Prokofiev, and Bach; the Newman collection of Armstrong recordings was unrivaled. In mid-1953 Armstrong was playing in Columbus, the date overlapping with a visit by Robert Woodward to Ohio State to give a lecture series on the tetracyclines. Mel made certain that Woodward and lan Scott, a postdoctoral in the Newman group at the time, attended an engagement, after which they were escorted backstage for a personal encounter with Satchmo. The next day, Mel in his introduction to Woodward's final lecture described the encounter: Mel: "Louis - I'd like you to meet Professor Woodward from Harvard. He is to chemistry what you are to jazz." Louis: "Gee! Mr. Newman - this cat must really be something!"

Newman loved good jokes, and he remembered many. He particularly delighted with clever twists of words. His laugh was especially memorable.

Over the years, he developed a laboratory course for advanced students and incorporated the series of recommended experiments in a book (1972). Among the dedications was one to his beloved wife Bea, "who can always control my reactions."

THE AUTHORS ARE very grateful to Beatrice Newman, Kiefer Newman, Susan Crystal Newman Katz, and Professor A. I. Scott for providing relevant background material for this biography.

Selected Bibliography

1937 The synthesis of 1,2-benzanthracene derivatives related to 3,4-benzpyrene. *J. Am. Chem. Soc.* 59:1003.

1940 The synthesis of 5-methylchrysene and related compounds. *J. Am. Chem. Soc.* 62:870.

1941 A new method for the esterification of certain sterically hindered acids. *J. Am. Chem. Soc.* 63:2431.

1945 With H. G. Kuivila and A. B. Garrett. Normal and complex ionization of organic molecules in solvent sulfuric acid. (I). Methyl 2,4,6-trimethyl-benzoate; o-benzoylbenzoic acid and its normal and pseudo methyl esters. *J. Am. Chem. Soc.* 67:704.

1947 With A. S. Hussey. Optical activity from a new type of steric hindrance. *J. Am. Chem. Soc.* 69:978.

1948 With W. B. Wheatley. Optical activity of the 4,5-phenanthrene type: 4-(1-methylbenzo[c] phenanthylacetic acid and 1-methylbenzo[c] phenanthrene. *J. Am. Chem. Soc.* 70:1913.

1949 With R. A. Craig and A. B. Garrett. The behavior of oxygenated compounds in acid media. *J. Am. Chem. Soc.* 71:869.

1950 With H. K. Connor. Steric hindrance: Some reactions of mesitylacetylene. *J. Am. Chem. Soc.* 72:4002.

1951 With N. C. Deno. Aryl carbonium ions in sulfuric acid. *J. Am. Chem. Soc.* 73:3644.

1952 A useful notation for visualizing certain stereospecific reactions. *Rec. chem. prog.* 13:111.
1953 With H. V. Anderson and K. H. Takemura. The synthesis of polynuclear aromatic hydrocarbons. II. Methylbenzo[c]phenanthrenes. *J. Am. Chem. Soc.* 75:347.
1954 With W. M. Edwards. New reactions involving alkaline treatment of 3-nitroso-2-oxazolidones. II. *J. Am. Chem. Soc.* 76:1840.
1955 With W. B. Lutz and D. Lednicer. A new reagent for resolution by complex formation: The resolution of phenanthro[3,4,c] phenanthrene. *J. Am. Chem. Soc.* 77:3420.
1956 With others. Synthesis of hydroaromatic compounds containing angular methyl groups. V. Equilenin series. *J. Am. Chem. Soc.* 76:2331.
With W. B. Lutz. α-(2,4,5,7-Tetranitro-9-fluorenylidine aminoxy)-propionic acid. A new reagent for resolution by complex formation. *J. Am. Chem. Soc.* 78:2469.
With D. Lednicer. The synthesis and resolution of hexahelicene. *J. Am. Chem. Soc.* 78:4765.
1957 With A. W. Weinberg. Pyrolysis of 3-nitroso-5,5-disubstituted-2-oxazolidones. *J. Am. Chem. Soc.* 79:2814.
1958 With R. J. Harper, Jr. Kinetic and equilibrium studies of cyclic ketal formation and drolysis. *J. Am. Chem. Soc.* 80:6350.
1962 With D. Pawellek and S. Ramachandran. Synthesis and reactions of 4-trichloromethyl-2,4,5-trimethyl-2,5-cyclohexadienone. *J. Am. Chem. Soc.* 84:995.

1964 With J. Blum. The synthesis and ionization constants of the six aminobenzo[c]phenanthrenes. *J. Am. Chem. Soc.* 86:1835.

1966 With C. Courduvelis. Reactions proceeding by the [3.2.1] bicyclic path. *J. Am. Chem. Soc.* 88:781.

1968 With others. Synthesis and polarographic reduction of strained phenanthrenequinones. The buttressing effect. *J. Am. Chem. Soc.* 90:458.

1974 With D. R. Olson. A new hypothesis concerning the reactive species in carcinogenesis by 7,12-dimethylbenz[a]anthracene. The 5-hydroxy-7,12-dimethylbenz[a] anthracene-7,12-dimethylbenz[a] anthracene-5(6H)-one equilibrium. *J. Am. Chem. Soc.* 96:6207.

1979 With V. K. Khanna and K. A. Kanakarajan. A novel synthesis of 7-fluorobenzo[a]pyrene involving two new molecular rearrangements. *J. Am. Chem. Soc.* 101:6788.

1986 With A. Kumar. An unusual synthesis of 5-methoxy-7,12-dimethylbenz[a]anthracene. *J. Org. Chem.* 51:2379

A H Sturtevant

Courtesy of the Institute Archives, California Institute of Technology, Pasadena

Alfred Henry Sturtevant

November 21, 1891—April 5, 1970

BY EDWARD B. LEWIS

STURTEVANT WAS THE youngest of six children of Alfred Henry Sturtevant and Harriet Evelyn Morse. His grandfather Julian M. Sturtevant graduated from Yale Divinity School and was a founder and later president of Illinois College. Sturtevant's father taught mathematics for a time at Illinois College but subsequently turned to farming, first in Illinois and later in southern Alabama, where the family moved when Sturtevant was seven. His early education was in Alabama in a one-room country school, but for the last three years of high school he went to a public school in Mobile.

In the fall of 1908 Sturtevant entered Columbia University. The choice, a crucial one, was made because Sturtevant's oldest brother, Edgar, was then teaching Latin and Greek at Barnard College; Edgar and his wife made it possible for Sturtevant to attend the university by taking him into their home. Sturtevant was greatly influenced by Edgar, from whom he learned the aims and standards of scholarship and research.

As a boy Sturtevant had drawn up the pedigrees of his father's horses and of his own family. He pursued this interest

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as a hobby while he was at Columbia. Edgar encouraged him to read works on heredity and to learn more about the meaning of pedigrees. As a result Sturtevant read a book on Mendelism by Punnett that greatly stimulated his interest, since he saw how Mendel's principles could be used to explain the pattern of inheritance of certain coat colors in horses. Edgar suggested that Sturtevant work out the genetic relationships, write an account of his findings, and submit it to Thomas Hunt Morgan, who held the chair of experimental zoology at Columbia and from whom Sturtevant had already taken a course. Morgan clearly was impressed, since he not only encouraged Sturtevant to publish the account, which appeared in *Biological Bulletin* in 1910, but also, in the fall of that year, gave Sturtevant a desk in his laboratory, which came to be known as the "fly room." Only a few months before, Morgan had found the first white-eyed mutant in *Drosophila* and had worked out the principles of sex linkage.

After completing his doctoral work with Morgan in 1914, Sturtevant remained at Columbia as a research investigator for the Carnegie Institution of Washington. He was a member of a research team that Morgan had assembled a few years earlier and that consisted principally of two other students of Morgan's, C. B. Bridges and H. J. Muller. The "fly room" in which they conducted all of their experiments was only sixteen by twenty-three feet, and at times as many as eight people had desks in it. According to Sturtevant, the atmosphere was one of high excitement, each new idea being freely put forth and debated. Morgan, Bridges, and Sturtevant remained at Columbia until 1928; Muller left the group in 1921 to take a position at the University of Texas.

In 1922 Sturtevant married Phoebe Curtis Reed; and in the same year they made their first trip to Europe, visiting museums and laboratories in England, Norway, Sweden, and Holland. They had three children.

In 1928 Sturtevant moved to Pasadena to become professor of genetics in the new division of biology that Morgan had established in that year at the California Institute of Technology. Much of the same stimulating atmosphere and unpretentious way of conducting science that Morgan and his students had practiced at Columbia was transferred to the new Kerckhoff Laboratory at Caltech. Sturtevant became the acknowledged and natural leader of the new genetics group established there. He maintained an active research program in which he often collaborated with other members of the genetics staff, including George W. Beadle, Theodosius Dobzhansky, Sterling Emerson, and Jack Schultz. He gave lectures in the general biology course and taught elementary and advanced courses in genetics and, on occasion, a course in entomology. He remained at Caltech until his death except for a year in England and Germany in 1932, as visiting professor of the Carnegie Endowment for International Peace, and shorter periods when he held visiting professorships at a number of American universities. He received many honors, including the National Medal of Science in 1968.

In addition to his principal publications dealing with the genetics and taxonomy of *Drosophila*, Sturtevant contributed papers on the genetics of horses, fowl, mice, moths, snails, iris, and especially the evening primroses (*Oenothera*). Although his chief contributions are in genetics, he was also a leading authority on the taxonomy of several groups of Diptera, especially the genus *Drosophila*, of which he described many new species. He was much interested in the social insects and published several papers on the behavior of ants.

Sturtevant had a prodigious memory and truly encyclopedic interests. He had a natural bent for mathematics but little formal training in it. He especially enjoyed, and was expert at solving, all kinds of puzzles, especially those involving geometrical situations. For him scientific research was an exciting and rewarding activity not unlike puzzle-solving. a common theme of his investigations was an effort to analyze and explain exceptions to established principles.

Sturtevant knew how to design and execute simple, elegant experiments, describing the results in concise, lucid prose. He set high standards for his own research and expected others to do the same.

Sturtevant's discoveries of the principle of gene mapping, of the first reparable gene defect, of the principle underlying fate mapping, of the phenomena of unequal crossing-over, and of position effect were perhaps his greatest scientific achievements. The account of these and some of his other major contributions to science is arranged in approximate chronological order.

Mendel had found that all of the hereditary factors with which he worked assorted independently of one another at the time of gamete formation. Exceptions to this second Mendelian law began to accumulate in 1900–1909. Morgan was the first to provide a satisfactory explanation for such exceptions in terms of a hypothesis, which assumes that genes tending to remain together in passing from one generation to the next must be located in the same chromosome. He further postulated that the extent to which such linked genes recombine at meiosis is a relative measure of their physical distance.

Sturtevant introduced the concept that the frequency of crossing-over between two genes furnishes an index of their distance on a linear genetic map. He proposed that 1 percent of crossing-over be taken as equal to one map unit. He then reasoned that if the distance between two genes, *A* and *B*, is equal to *x* map units and the distance between *B* and a third gene, *C*, is equal to *y* map units, then the distance between *A* and *C* will be $x + y$ if *B* is the middle gene; $x - y$ if *C* is the middle gene, and $y - x$ if *A* is the middle gene. The germ of this idea occurred to Sturtevant in conversation with Morgan. In his *History of Genetics*, Sturtevant recorded that he "went home, and spent most of the night (to the neglect of my undergraduate homework) in producing the first chromosome map, including the linked genes, y, w, v, m, and r, in that order, and approximately the relative spacing, as they still appear on the standard maps" (p. 47).

Sturtevant devised a crucial test of the principles of mapping genes by constructing crosses in which all three genes were segregating simultaneously. In the progeny of such "three-factor" crosses, Sturtevant discovered that double crossing-over can occur and that its frequency is equal to, or less than, the product of the two single crossing-over frequencies. Conversely, the frequency of double crossing-over can be used to deduce the order of the three genes. Sturtevant showed that the order obtained from two-factor crosses was fully confirmed and that the three-factor crosses provided a more powerful method of ordering and mapping genes than did two-factor crosses. He published these findings in 1913. His principles and methods of chromosome mapping have enabled geneticists to map the chromosomes of a wide variety of higher organisms, including man.

Sturtevant was as much concerned with the role of genes in development as with the laws governing their transmission from one generation to the next. In 1915 he published an account of the sexual behavior of *Drosophila* that included a study of sexual selection based on the use of specific mutant genes that altered the eye color or body color of the fly. This work was the forerunner of an extensive line of research by others and constituted one of the first examples of the use of specific mutant genes to dissect the behavior of an organism.

One of the more conspicuous roles that genes play in development is their control of the processes of sexual differentiation. In 1919 Sturtevant reported the first case in which intersexuality could be shown to result from the presence of specific recessive genes. Years later he found a similar type of gene that resulted in the virtually complete transformation of females into males. Mutants of still other "sex genes" have been found in *Drosophila* and in many other organisms, including man. As a result, sex has come to be viewed as a complex trait controlled by a number of different genes, mutants of which can be expected to produce various grades of intersexuality.

Sturtevant pioneered in providing experimental approaches to a central problem in biology—how genes produce their effects. An important break-through came in 1920, with his discovery of the first reparable gene defect. In studying gynandromorphs of *Drosophila* in which there was somatic mosaicism for the vermilion eye-color mutant, he noticed that the eyes developed the dark red color of the wild type instead of the bright red color of the vermilion mutant, even when the eye could be shown to be genetically vermilion. Evidently, vermilion eye tissue lacked some substance that could be supplied by genetically nonvermilion tissue from another portion of the body. As G. W. Beadle pointed out, much of modern biochemical genetics stems directly from this early work.

Sturtevant had shown in 1913 that for each of the major chromosomes of *Drosophila* there is a corresponding linkage map. He and others had noticed, however, that excessive variation in the amount of crossing-over sometimes occurs. The factors responsible were isolated by Sturtevant and by Muller around 1915 and were shown to act as dominant cross-over suppressors. The first clue to the nature of these factors came in 1921, when Sturtevant compared the chromosome maps of *Drosophila melanogaster* with those of *D. simulans*, a closely related species that he had first described in 1919. These maps closely paralleled one another except for a region of the third chromosome, in which it appeared likely that the two species differed by an inversion in their gene sequences. It was only later that sufficient numbers of mutants were obtained in the various inversion-containing chromosomes of *D. melanogaster* for Sturtevant to establish that the dominant cross-over suppressors were indeed inversions. What had first been a disturbing exception to the generality of Sturtevant's principles of chromosome mapping became, in his hands, another demonstration of their validity. In 1935, after the discovery of the giant salivary gland chromosomes of the Diptera by Emile Heitz and H. Bauer (1933), T. S. Painter, C. B. Bridges, and others demonstrated the existence of inversions and their points of rearrangement by direct microscopic analysis. These cytological studies fully confirmed the standard and inverted sequences that Sturtevant had deduced by purely abstract genetic analysis.

In 1923 Sturtevant provided the first satisfactory explanation of the puzzling pattern of inheritance that others had found for direction of shell-coiling in snails. He showed that it was sufficient to assume a simple Mendelian gene, with dextrality being determined by the dominant allele and with the direction of coiling in the individual being determined not by its own genetic constitution but by that of its mother. He pointed out that such characters are "fundamental," in the sense that they are impressed on the egg by the action of genes in the mother. In 1946 he showed that intersexuality in a species hybrid—that of the *repleta* and *neorepleta* species of *Drosophila*—is an unusually subtle case of maternal inheritance conditioned by an autosomal dominant gene.

In the early 1920's Sturtevant and Morgan had begun a study of the unstable *Bar* mutant of *Drosophila* in order to learn more about the nature of mutations and the mechanisms by which new ones arise. It was known by then that mutations, in the sense of simply inherited changes, could take the form of changes in numbers of chromosomes (such as trisomy or polyploidy), changes involving several genes at a time (deficiencies or duplications), or changes that appeared to be within the gene (point mutations). Efficient methods for the experimental induction and detection of mutations had yet to be worked out, however. Moreover, spontaneous mutants were too rare, for the most part, to permit practical study of specific genes, except in the case of *Bar*. This small-eye mutant had already been found by C. Zeleny to mutate occasionally to either revered *Bar*, with eyes of normal size, or *Ultra-Bar*, with eyes distinctly smaller than those of *Bar*. In 1925 Sturtevant demonstrated that these derivatives of *Bar* arise at meiosis and are associated, at the time of their origin, with an unusual type of recombination process that he termed "unequal crossing-over." He postulated that the reverted type had lost the *Bar* gene and the *Ultra-Bar* was a tandem duplication for the *Bar* gene.

After the discovery of the giant salivary gland chromosomes, it was shown by H. J. Muller and A. A. Prokofieva, and by C. B. Bridges, that *Bar* itself is a small tandem duplication of a short section of the sex chromosome of *Drosophila*. The exact nature of the unequal crossing-over process then became evident. If the chromosome containing the *Bar* mutant is symbolized as ABCBCDE …, where BC is a small segment that has become tandemly repeated, then, in the germ cells of an individual homozygous for such a chromosome, the leftmost BC region of one chromosome may occasionally come to pair with the rightmost BC region of the other chromosome. If a crossing-over then occurs within such unequally paired BC regions, it is evident that two new types of chromosome sequences will be produced: ABCDE … and ABCBCBCDE. … The former sequence corresponds to reverted *Bar* and the latter to *Ultra-Bar*. Thus, orthodox crossing-over within unequally paired, tandemly duplicated chromosomal segments accounts for the instability of the *Bar* mutant and provides a mechanism for progressively increasing the number of genes in a chromosome.

The process of unequal crossing-over has come to assume increasing prominence in biology as possibly one of the main forces of evolution. To illustrate, this process may have been involved in the evolution of the cluster of closely linked genes controlling the production of the β, γ and δ polypeptide chains of the human hemoglobin molecule. The extremely close similarity of these chains at the molecular level strongly implies that the genes determining them all arose from a single ancestral gene, presumably by repeated unequal crossing-over. In turn, the resultant duplicated genes evidently diverged gradually from one another by mutation until the gene cluster acquired its present form.

Sturtevant realized that *Bar* and its derivatives provided a unique opportunity to determine whether the position of a gene in the chromosome can affect its function. He devised a critical test that consisted of comparing the sizes of the eyes of two types of flies: homozygotes for *Bar*, the genetic composition of which can be symbolized as BCBC/BCBC; and heterozygotes for reverted *Bar* and *Ultra-Bar*, of composition BC/BCBCBC. He compared the eye sizes by counting the number of facets and showed that the second type of fly had significantly fewer facets than did the first. Since the total content of genic material in the two chromosomes is the same in both cases, the observed differences in eye size constitute a demonstration that the effect of the *Bar* gene (or *Bar* region) does indeed depend upon the position of that gene in the chromosome. Sturtevant's discovery of this phenomenon of "position effect" was the first demonstration that primary genic interactions occur at the site of the genes in the chromosome, as opposed to elsewhere in the nucleus or cytoplasm.

The position effect was shown by H. J. Muller, J. Schultz, and others to take many forms. Moreover, the more primitive the organism, the more prominent (apparently) is the role played by position effect. Thus, in bacteria the chromosome consists of a series of gene clusters, or operons, that are examples par excellence of the position-effect phenomenon, in the sense that the order of the genes in an operon, as François Jacob and Jacques Monod first showed, directly determines the order in which those genes are expressed.

As is often the case in basic science, Sturtevant's discovery of the position effect of *Bar* was a by-product of another study, in this case of the mutations of *Bar*. His accounts of the quite separate phenomena of unequal crossing-over and of position effect were published in 1925 in a paper that bore the modest title "The Effects of Unequal Crossing Over at the Bar Locus in *Drosophila*."

Sturtevant was able to exploit his early use of somatic mosaics to study the developmental effects of genes when he discovered a way of producing them in large numbers.

He found that females homozygous for the claret eye-color mutant of *D. simulans* produce a high proportion of gynandromorphs and other mosaics in their offspring. With the aid of this mutant he showed in 1929 that the degree of resemblance in genetic composition between two tissues in a somatic mosaic can serve to measure the degree to which those tissues have a common embryological origin. This principle, which underlies a kind of embryological-genetic mapping process now known as "fate mapping," has been widely exploited and has become a powerful tool of developmental and behavioral genetics.

In 1929 Sturtevant and S. Emerson showed that much of the extraordinarily complex genetics of the evening primrose (*Oenothera*) could be interpreted on a translocation hypothesis that had first been elaborated by John Belling for the jimsonweed, *Datura*. Many of the puzzling and bizarre "mutations" that Hugo de Vries and others had found in this organism remained disturbing thorns in the side of established genetic theory until Sturtevant and Emerson provided a detailed demonstration that they were not genuine mutations but, rather, the expected segregation products from the complex translocations of chromosome arms that are peculiar to, and widespread in, *Oenothera*.

Sturtevant and Dobzhansky collaborated in studying the plethora of inversions that occur in wild strains of many species of *Drosophila* , especially *pseudoobscura*. This work culminated in a paper (1936) that propounded an ingenious method by which inversions could be used as probes to trace phylogenetic relationships. They then successfully applied the method to constructing a detailed phylogeny of various strains and races of *pseudoobscura*.

In 1936 Sturtevant and Beadle published the results of an exhaustive study of the effects of inversions in *Drosophila* on crossing-over and disjunction. In this work they provided the first satisfactory explanation of the frequency and fate of certain aberrant chromosome types that arise as the result of crossing-over in inversion heterozygotes.

Sturtevant always maintained a keen interest in evolution and constantly examined the consequences for evolutionary theory of each new discovery in the rapidly developing science of genetics. He was an excellent naturalist and, as already noted, a taxonomist in his own right. In 1937 he published three "Essays on Evolution" in *Quarterly Review of Biology*. The first dealt with the effect of selection for mutator genes on the mutation rate of a species. The second pointed out some of the special problems of selection that are presented by the existence of sterile workers among the social insects. In the third essay he formulated a general scheme for interpreting one of the great puzzles of evolutionary theory—the origin of the sterility of hybrids.

In 1941 Sturtevant and Edward Novitski brought together the then-known mutational parallels in the genus *Drosophila*. Their results showed that the major chromosome arms of this organism tend to remain intact throughout the speciation process, although the specific order of genes within an arm gradually becomes scrambled, evidently by successive fixations of inversions.

The tiny fourth chromosome of *Drosophila* for many years resisted all efforts to map it genetically, until Sturtevant discovered special conditions that stimulated recombination to occur in that chromosome. His map of that chromosome appeared in 1951.

After 1951 Sturtevant's publications consisted mainly of original contributions to the genetics of iris; general articles on such topics as genetic effects of high-energy irradiation on human populations, the social implications of human genetics, and the theory of genetic recombination;

and several taxonomic studies, including a major monograph, written with Marshall R. Wheeler, on the taxonomy of the Ephydridae (Diptera).

In 1954, in his presidential address before the Pacific Division of the American Association for the Advancement of Science, Sturtevant warned of the genetic hazards of fallout from atmospheric testing of atomic bombs. He felt that although there might be a need for bomb testing, the public should be given the best scientific estimate of the biological hazards of fallout.

Sturtevant's last published work on *Drosophila* (1956) was an account of his discovery of a remarkable mutant gene that was without any obvious effect on the fly by itself but that, in combination with another specific mutant gene (determining a prune-colored eye), killed the organism at an early stage of development. In addition to posing a challenging problem in developmental genetics, such highly specific complementary lethal systems provide an opportunity for effecting the self-destruction of certain undesirable classes of flies.

Sturtevant's last major work, *A History of Genetics* (1965), was the outgrowth of a series of lectures given at several universities and of a lifelong interest in the history of science. True to his early love of pedigrees, he presents, in an appendix to that book, detailed intellectual pedigrees of geneticists of his day.

Bibliography

I. ORIGINAL WORKS. A complete bibliography of Sturtevant's publications through 1960 can be found in the appendix to *Genetics and Evolution* (see below). A collection of thirty-three of his more important papers, reprinted in 1961 to honor Sturtevant on his seventieth birthday, includes brief annotations written by him in 1961 for several articles: *Genetics and Evolution, Selected Papers of A. H. Sturtevant*, E. B. Lewis, ed. (San Francisco, 1961).

Sturtevant was the author of *An Analysis of the Effects of Selection*, Carnegie Institution Publication no. 264 (1918); and *The North American Species of Drosophila*, *ibid.*, no. 301 (1921); and *A History of Genetics* (New York, 1965). His other works include *The Mechanism of Mendelian Heredity* (New York, 1915; New York, 1972), written with T. H. Morgan, H. J. Muller, and C. B. Bridges; *The Genetics of Drosophila* (Amsterdam, 1925), written with T. H. Morgan and C. B. Bridges; and *An Introduction to Genetics* (New York, 1939), written with G. W. Beadle.

Background material can be found in Sturtevant's *A History of Genetics* and his articles "Thomas Hunt Morgan," in *Biographical Memoirs, National Academy of Sciences*, **33** (1959), 283–325; and "The Early Mendelians,' in *Proceedings of the American Philosophical Society*, **109** (1965), 199–204.

II. SECONDARY LITERATURE. There are biographical accounts of Sturtevant by G. W. Beadle, in *Yearbook, American Philosophical Society* (1970), 166–171; and by Sterling Emerson, in *Annual Review of Genetics*, **5** (1971), 1–4. For a discussion of Sturtevant's work in Morgan's laboratory, see G. E. Allen, "Thomas Hunt Morgan," in *DSB*, IX, 515–526.

Wm G Young

William Gould Young

July 30, 1902—July 5, 1980

BY JOHN D. ROBERTS

WILLIAM G. YOUNG was born in Colorado Springs on July 30, 1902, and died July 5, 1980, in Laguna Hills, California. Bill Young was a physical organic chemist whose name is not now much of a household word among the current fraternity of workers in the field. There are two reasons for this. One is the fact that the major research he did was so basic to organic chemistry that it was subsumed into "what every organic chemist knows," an arena where it is not usual for individual contributors to be identified by name. In addition, much of the work that Bill Young initiated is often now associated more with his brilliant student and fellow member of the National Academy of Sciences, Saul Winstein.[1]

The second reason that Bill Young is not as well known as he might be is that he devoted enormous energy and steadfastness to making the University of California, Los Angeles, a first-rate institution in teaching and research. In this effort, another Young, Charles E. Young, UCLA's long-serving and energetic chancellor is generally given major credit, but as I shall relate, Bill Young provided critical impetus in the early days of UCLA's relatively brief history. Whether or not Bill Young would have wanted to see UCLA continue to expand to its present status as almost the epitome of the modern multifaceted megauniversity is of course unknowable, but it seems unlikely given his character, at least as I knew it.

Bill's father, Henry A. Young, was a self-educated man who was a jewelry salesman in Providence, Rhode Island. At age thirty he made a successful move to Colorado Springs to improve his health. There he first entered the insurance business and later became a stock and investment broker. Bill described his father as "a man of broad cultural and civic interests, who reared his children to appreciate good music, the theater and a well-chosen library. They were encouraged then to take advantage of the educational opportunities that he missed." [2] He supplied no information about his mother, Mary Ella (Salisbury), but he had brothers, Ralph, who followed his father as an investment broker, Ray, and Harry.

Bill went to grade and high school in Colorado Springs, graduating in 1920. Even at an early age, he was interested in all areas of science, but less so in other areas and had what he characterized as "a genuine dislike for foreign languages."[2] Bill enjoyed outdoor sports particularly baseball, basketball, trout fishing, and golf. The latter two sports he participated in until late in life, along with gardening and color photography. He was especially proficient at golf and was a Colorado state intercollegiate golf champion.

Bill entered Colorado College and obtained a B.A. in 1924 and an M.A. in chemistry in 1925. For reasons unclear to me, rather than going to graduate school, he then became a research assistant, working at the Carnegie Institution of Washington's plant physiology laboratory at Carmel, California, with H. A. Spoehr and James H. C. Smith. It must have been a productive period, because it resulted in five publications on four quite different subjects, probably the most significant being on the preparation and properties of carotene. This may have sparked Bill's lifelong interest in unsaturated compounds. During this period, he married Helen Graybeal, a woman of intelligence and vivacity with whom he was very compatible. They had no children.

In 1927 Bill entered the California Institute of Technology as an American Petroleum Institute fellow and began his doctoral research with Howard J. Lucas.[3] At this time, Howard Lucas was the only organic chemist on the Caltech faculty and was to become probably the only chemist to be elected to the National Academy of Sciences in the modern era without a Ph.D. degree. Howard's chemical career hardly seemed to qualify him for a professorship at Caltech in the era of such early giants of chemistry at that institution as A. A. Noyes, Linus Pauling, Richard Tolman, Richard Badger, and Don M. Yost. However, Lucas was hired as an instructor of Throop College somewhat before A. A. Noyes and George Ellery Hale began to exert their notable effects on Throop, which metamorphosed it into the California Institute of Technology. Lucas received his M.S. degree at Ohio State, then became associated with an agricultural station in Puerto Rico, where he published papers on the milk of Puerto Rican cows and the adulteration of peanut oil with, of all things, nitrobenzene. Despite this, by 1927 Howard Lucas had become a leader in the field of what in 1940 was to be labeled physical organic chemistry. He had a knack for picking significant research problems and he had the intelligence and persistence to learn qualitative structural theory from Linus Pauling.

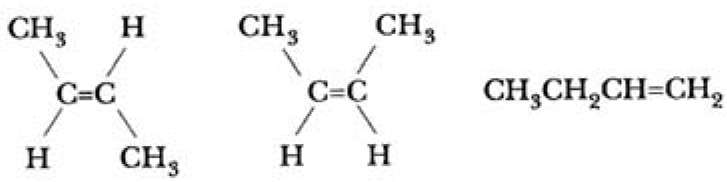
With Lucas and his prior training at Carnegie, Bill Young flourished, and completed his Ph.D. with six publications in two years, each concerned with preparation and characterization of the isomeric butenes in pure form, surely projects of interest at that time to the Petroleum Research Institute. One should remember that this was many years before the advent of chromatographic or spectroscopic means of establishing the composition of butene mixtures, and one of the projects Young worked on was a chemical procedure for analysis of such mixtures. It was a nontrivial task, involving first conversion of the butenes to dibromide, distillation, and then analysis of the three-component dibromide mixture by density, refractive index, and determination of the second-order rate constants with potassium iodide in acetone.

Young's promise as a researcher led to an award of a prestigious National Research Council postdoctoral fellowship at Stanford, where he again worked with H. A. Spoehr. From this came one publication on the preparation of glyceric aldehyde, the stereochemical prototype of the carbohydrates according to the convention of Emil Fischer.

At the end of his Stanford period, he was appointed instructor at the University of California, Los Angeles. The year marked a turning point for this new university, which was founded to provide higher public education in the rapidly growing Los Angeles area. The program was set up under the thumb of the Berkeley administration and no competition was desired on the doctoral level. However, a number of excellent faculty members were acquired in the hope that the situation would ultimately change and also because a wonderful new campus of Romanesque buildings was being constructed in a bucolic setting in West Los Angeles. Even in its earliest days, UCLA had notable undergraduates, the most prominent being Glen Seaborg of transuranic-element fame and Saul Winstein, a world leader in physical organic chemistry, to be discussed further shortly. The chemistry department was a mix of young, bright instructors and assistant professors with a small older group inherited from a prior, essentially teachers college at a campus in west-central Los Angeles. Chairman William

Conger Morgan, who had done no chemical research since his Ph.D. thesis, was regarded as a curmudgeon by the students, but he had excellent taste in selecting new faculty members. A master's degree was offered in chemistry and, as is not uncommon for strong departments with no doctoral program, master's theses were usually the equivalent of Ph.D. theses at many other institutions. This was so at UCLA where Bill Young's students Saul Winstein, Stanley J. Cristol, and Jerome Vinograd, all future Academy members, had significant master's theses.

Young's major chemical achievements stemmed from his desire to be able to prepare pure *trans*-2-butene **1** in substantial laboratory quantities from of *cis*-2-butene **2** and 1-butene **3**.



trans-2-butene **1** *cis*-2-butene **2** 1-butene **3**

The sequence was simple and seemed to offer no difficulties.

$CH_3CH{=}CHCHO \rightarrow \rightarrow CH_3CH{=}C \quad CH_2Br \rightarrow$

4 5

$CH_3CH{=}CHCH_2MgBr \rightarrow CH_3CH{=}CHCH_3$

6 1

The starting material, 2-butenal **4**, was shown to be the all-*trans* isomer, but the succeeding steps would up giving an unexpectedly serious mixture of **1**, **2**, and **3**. Here Saul

Winstein produced a magnificent master's thesis showing that the procedures to make **5** were such as to produce mixtures of the isomers **5** and **7**.

$CH_3CH{=}CHCH_2Br \rightarrow CH_3CHBrCH{=}CH_2$

5 7

Furthermore, **5** and **7** were more or less unstable and rather easily converted to an equilibrium mixture. Winstein and Young unraveled the mechanisms for the rearrangement reactions (often known as allylic rearrangements) and showed how pure **5** and **7** could be prepared. Then to the surprise of all concerned, it was found that the above sequences **5** → **6** → **1** gave not **1** but the same mixture as from mixtures of **5** and **7** or from pure **7**. The resolution of this problem has never been completely achieved, but much more is known today. After completion of his master's thesis, Winstein did a path breaking Ph.D. at Caltech with Howard Lucas, went off on a National Research Council fellowship to Harvard, an instructorship at the Illinois Institute of Technology, and finally back to UCLA for an illustrious career until his untimely death in 1969.

I arrived on the UCLA scene as a freshman in 1936, when elementary chemistry was being taught by William Conger Morgan. I had my first exposure to Bill Young when he was brought in to the freshman class to demonstrate and explain electrical conductance of ionic solutions, a subject on which Professor Morgan apparently was not up to speed. I started research in 1938 in analytical chemistry and, intrigued by organic chemistry as taught by master teacher G. Ross Robertson, I asked Robertson if I could do research with him in my senior year. He said no and that I should be working with someone at the forefront and recommended Bill Young. When I talked with Bill, he suggested that I work on the allylic rearrangements occurring in the conversion of **5→7**. But before I could get started, Saul Winstein came to visit fresh from his postdoctoral at Harvard. He convinced Bill that I should work on the mechanism of a different type of allylic rearrangement, which was of substantial theoretical interest during that period

At about this time (1940) Professor Morgan died and Bill Young was the popular choice to succeed him as department chairman. So, when he moved upstairs to the chairman's office, he installed me, a senior undergraduate, in his private office-laboratory downstairs. I shared this with another senior, William G. McMillan, later to be a professor of physical chemistry at UCLA. Besides research, we did most of our physical chemistry laboratory experiments in Young's laboratory. The allylic-rearrangement research went well, and Bill Young, with characteristic generosity and knowing I was to start graduate work at Penn State, invited me to present the results at the September 1941 meeting of the American Chemical Society in Atlantic City.

After the attack on Pearl Harbor, I was quite uncertain of the future, and Bill arranged for me to come to work on a war project starting at UCLA. Bill and T. A. Geissman (a new faculty member from Illinois) were the principal investigators on the project, which was concerned with extracting oxygen from the air with a cobalt-containing organic chelate called salcomine.[4] It was an ostensibly efficient process intended to be used in the field or on aircraft in flight. The brown chelate absorbed oxygen rather quickly from the air and turned black. When heated to 100°C, the oxygen was released. The cycle could then be repeated. The problems were to synthesize new chelates to find ones that were more rapid in combining with oxygen, had higher oxygen capacities, or would degrade more slowly when recycled.

For me it was a marvelous learning experience in all three areas.

For a brief period the peons of the project were also involved in assembly work for the famous bat-incendiary bomb project.[5] [6] Bill Young was also in the testing phase, and this proved quite effective when by accident the testing group burned down a small military airport in New Mexico. Bill very much enjoyed telling about this ill-fated venture, which has been marvelously documented by L. F. Fieser.[5]

During Bill Young's chairmanship, a powerful group of young faculty were brought in; besides T. A. Geissman and Saul Winstein, there were T. L. Jacobs (Cornell via Harvard), C. D. Coryell (Caltech), and D. J. Cram (Harvard). Coryell was lost to MIT via Oak Ridge, but the department flourished and soon became one of the strongest in the country, especially after a Ph.D. program was started in the early forties. Bill was able to expedite the process by serving several critical years on the university budget committee. In 1948 Bill became dean of physical sciences and in 1957 vice-chancellor for planning, a position he held until he retired in 1970. Here he played an important role in bringing about the Center for Health Sciences, now one of UCLA's crown jewels.

My own research connection with Bill Young, besides the undergraduate research done in collaboration with Saul Winstein, had to do with the nature of the butenylmagnesium bromide **6** and was started immediately after the war project was closed down. In seeking a solution to this difficult problem in the absence of such techniques as nuclear magnetic resonance spectroscopy, Bill was for me an ideal doctoral supervisor. [7] He was willing to spend a substantial sum for a state-of-the-art fractionating column, encouraged my creativity, allowed me to participate extensively in teaching, and developed a close and warm friendship with my wife and me. The department was generally close-knit, with a monthly dance group and other social activities. Bill often invited me to lunch with other faculty and administrative officials to understand better how universities work. Along with his continued commitment to understanding allylic rearrangements, Young extended his early interest in polyenes by embarking on a program to devise a practical synthesis of vitamin A. Although the effort led to eight publications, it was unsuccessful; but, it did bring back one of Bill's strongest students, Stanley J. Cristol, an earlier, successful M.S. candidate for a UCLA Ph.D. under Young's supervision.

Bill Young was active in the American Chemical Society (ACS), first, in the Southern California Section, where he served as chairman for the 1940–41 term and nine years as national councilor. He was on the Executive Committee of the Organic Division for five years and chairman from 1948 to 1949. From 1958 to 1960 he was a member of the ACS Board of Directors. However, his most important work for the ACS was on the Committee on Professional Training, where he served for seventeen years (1943–60) and as chairman (1948–58). This committee established an influential accreditation system to certify institutions based on the quality of their undergraduate chemistry programs and to certify students who met the minimum requirements for a chemistry degree. The program had its controversial aspects. Many universities and colleges resented having another accreditation program piled on those already in place and particularly for a scientific rather than a professional major. The penalty, hardly severe, for noncertified graduates was exclusion from full ACS membership for two years. Nonetheless, lack of accreditation was used by department chairmen as a club on their administrations to strengthen their course offerings, facilities, and faculties often with signal success. Of course, the chemistry program of the major research universities had little difficulty in becoming accredited, even if, in some cases, the menu of their chemistry courses differed from the standards. A certain amount of experimentation was not only allowed but was encouraged.

Certification of graduates was a different matter. Many institutions, including Harvard, had premedical students that were chemistry majors, but these students did not take the full measure of courses required by the Committee of Professional Training, and the schools usually reported large discrepancies between their total number of chemistry graduates and their certified chemistry graduates.

Suggestions for accreditation of graduate programs were considered, but they were never implemented. Instead, the committee developed and kept updated a very useful directory of U.S. and Canadian graduate programs in chemistry, biochemistry, and chemical engineering. This directory contains brief vitae of faculties, lists of their publications, and an indication of which publications came about from the work of graduate students and which from postdoctoral fellows. Bill Young sacrificed much of his research effort as the result of his resolve to make this and other parts of the accreditation program successful.

Bill Young received many honors for his scientific and administrative achievements. He was the first of the UCLA faculty to be elected to the National Academy of Sciences (1951). UCLA honored him as faculty research lecturer (1947), accorded him its Distinguished Service medal (1964) and an honorary LL.D. (1972), and named the chemistry building Young Hall (1970). He also received D.Sc. degrees from Colorado College (1962) and the University of Colorado (1975). He was selected for a Distinguished Alumnus

Award from Caltech (1968). The American Chemical Society honored him with the first Richard C. Tolman Medal (1961), the Chemical Education Award (1962), and the society's highest award, the Priestley Medal (1968).

William G. Young was an exceptionally modest and gentle man, who focused his attention far more on helping others than advancing his own interests. Always fun to be with, he worked out difficult situations with dogged patience and always with his eyes on the future. He was enormously effective in teaching, research, and administration, and he was a person you liked to deal with. He brought enormous credit to his profession.

NOTES

1. W. G. Young and D. J. Cram. Saul Winstein. In *Biographical Memoirs* , vol. 43, pp. 320–53. New York: Columbia University Press for the National Academy of Sciences, 1973.
2. W. G. Young. Autobiographical statement, p. 1. National Academy of Sciences archives, Washington, D.C., 1950.
3. W. G. Young and S. Winstein. Howard Johnson Lucas. In *Biographical Memoirs,* vol. 43, pp. 163–76. New York: Columbia University Press for the National Academy of Sciences, 1973.
4. T. A. Geissman, M. J. Schlatter, I. D. Webb, and J. D. Roberts. The synthesis of some intermediates for use in the preparation of analogs of salicylaldehyde ethylenediamine cobalt ("Salcomine"). *J. Org. Chem.* 11(1946):741–50.
5. L. F. Fieser. *The Scientific Method. A Personal Account of Unusual Projects in War and Peace*, pp. 121–34. New York, Reinhold, 1964.
6. S. M. Hersh. *Chemical and Biological Warfare*, pp. 62–63. New York:Bobbs-Merrill, 1968.
7. J. D. Roberts. *The Right Place at the Right Time*, pp. 29–44. Washington, D.C.: American Chemical Society, 1990.

Selected Bibliography

(The selection of references given below is based on the author's judgment and may not reflect in more than a qualitative way the range or importance of William G. Young's research.)

1927 With J. H. C. Smith and H. A. Spoehr. The preparation and properties of leaf pigments. *Carnegie Inst. Washington, Yearb.* 26:156–57.

1929 With R. T. Dillon and H. J. Lucas. The synthesis of the isomeric 2-butenes. *J. Am. Chem. Soc.* 51:2528–34.

1930 With H. J. Lucas. The composition of butene mixtures resulting from the catalytic decomposition of the normal butyl alcohols. *J. Am. Chem. Soc.* 52:1964–70.

With R. T. Dillon and H. J. Lucas. The reaction rates of potassium iodide with 1,2- and 2,3-dibromobutane. The analysis of mixtures of the normal butenes. *J. Am. Chem. Soc.* 52:1953–64.

1932 With A. N. Prater. The allylic rearrangement of crotyl bromide and crotylmagnesium bromide. *J. Am. Chem. Soc.* 54:404.

Investigations on the stereoisomerism of unsaturated compounds. I. The composition of crotonaldehyde. *J. Am. Chem. Soc.* 54:2498–2503.

1936 With S. Winstein. Allylic rearrangements. I. Crotyl and methylvinylcarbinyl bromides. *J. Am. Chem. Soc.* 58:104–107.

With S. Winstein and A. N. Prater. Allylic rearrangements. II. Crotyl and methylvinylcarbinyl-magnesium bromides. *J. Am. Chem. Soc.* 58:289–91.

1937 With F. E. Blacet and J. G. Roof. Studies of absorption spectra. I. Crotonaldehyde and acrolein. *J. Am. Chem. Soc.* 59:608–14.

1938 With J. F. Lane. Allylic rearrangements. V. The mechanism of the reaction of crotyl alcohol and methylvinylcarbinol with solutions of hydrogen bromide. *J. Am. Chem. Soc.* 60:847–53.

1939 With G. Ballou and K. Nozaki. Allylic rearrangements. VIII. The action of magnesium on cinnamyl chloride. *J. Am. Chem. Soc.* 61:12–15.

1942 With J. D. Roberts and S. Winstein. Allylic rearrangements. XIII. Kinetics and mechanisms of the conversion of crotyl and methylvinylcarbinyl chlorides to acetates and ethyl ethers. *J. Am. Chem. Soc.* 64:2157–64.

1944 With L. J. Andrews and S. J. Cristol. Polyenes. I. The synthesis and absorption of the ionylideneacetones and related compounds. *J. Am. Chem. Soc.* 66:520–24.

With J. F. Lane and J. D. Roberts. Allylic rearrangements. XV. The carbonation of butenylmagnesium bromide. *J. Am. Chem. Soc.* 66:543–45.

With J. D. Roberts. Highly-branched compounds. The preparation of triisopropylcarbinol and diisopropyl-s-butylcarbinol. *J. Am. Chem. Soc.* 66:1444–45.

With J. D. Roberts. The reaction of butenylmagnesium bromide with acetomesitylene. *J. Am. Chem. Soc.* 66:2131.

1946 With A. C. McKinnis, I. D. Webb, and J. D. Roberts. Allylic rearrangements. XIX. Studies of the ozonization of allylic systems. *J. Am. Chem. Soc*. 68:293–96.

With J. D. Roberts. Allylic rearrangements. XX. Some addition reactions of butenylmagnesium bromide. *J. Am. Chem. Soc*. 68:649–52.

With J. D. Roberts. Allylic rearrangements. XXI. Further studies related to the nature of the butenyl Grignard reagent. *J. Am. Chem. Soc*. 68:1472–75.

1947 With T. W. Campbell. Allylic rearrangements. XXIII. The reaction of the sodium derivative of allylbenzene with carbonyl compounds. *J. Am. Chem. Soc*. 69:3066–68.

With R. L. Meier, J. Vinograd, H. Bollinger, L. Kaplan, and S. Linden. Investigations on the stereoisomerism of unsaturated compounds. VIII. The catalytic hydrogenation of butadiene. *J. Am. Chem. Soc*. 69:2046–50.

1949 With R. E. Kepner and S. Winstein. Allylic rearrangements. XXIV. Abnormal bimolecular substitution. The condensation of butenyl and pentenyl chlorides with sodium malonic ester. *J. Am. Chem. Soc*. 71:115–18.

1950 With K. W. Wilson and J. D. Roberts. Allylic rearrangements. XXVIII. The reactions of butenylmagnesium bromide with hindered ketones. *J. Am. Chem. Soc*. 72:218–19.

1951 With I. D. Webb and H. L. Goering. The reaction of butenyl chlorides with diethylamine and triethylamine. *J. Am. Chem. Soc*. 73:1076–83.

With S. Winstein and H. L. Goering. Allylic rearrangements. XXXII. The solvolysis and intramolecular rearrangement of α,α-dimethylallyl chloride. *J. Am. Chem. Soc*. 73:1958–63.

1957 With R. H. DeWolfe, D. E. Johnson, and R. T. Wagner. Allylic rearrangements. XXXVIII. The reactions of cinnamyl Grignard reagents and alkali metal derivatives of allylbenzene with acetophenone. *J. Am. Chem. Soc.* 79:4798–4802.

1958 With S. H. Sharman, F. F. Caserio, R. F. Nystrom, and J. C. Leak. Allylic rearrangements. XLIII. Some studies of the thermal decomposition of allyl chlorosulfinate-1-C^{14}. *J. Am. Chem. Soc.* 80:5965–71.

1959 With R. E. Ireland, T. I. Wrigley, C. W. Shoppee, B. D. Agashe, and G. H. R. Summers. Allylic rearrangements. XLII. Solvolytic reactions of 3β-chlorocholest-4-ene and 3α-chlorocholest-4-ene. *J. Am. Chem. Soc.* 81:1452–54.

1966 With J. S. Franklin. The acid-catalyzed isomerization of α- and *cis* and *trans*-γ-methylallyl alcohols. *J. Am. Chem. Soc.* 88:785–90.

1969 With R. A. Benkeser, W. E. Broxterman, D. A. Jones, Jr., and S. J. Piaseczynski. The prevalence of *cis*-addition products in the reaction of the butenyl Grignard with sterically hindered ketones. *J. Am. Chem. Soc.* 91:132–37.

1971 With H. E. Green and A. F. Diaz. The acid-catalyzed isomerization of the butenyl acetates. *J. Am. Chem. Soc.* 93:4782–87.

1972 With Z. Rappaport and S. Winstein. Allylic oxidation of olefins by mercuric acetate. *J. Am. Chem. Soc.* 94:2320–29.