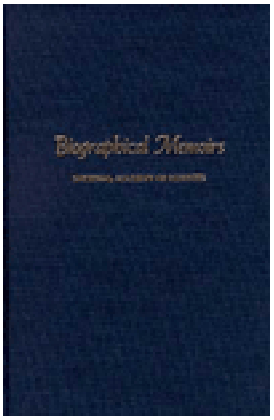

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Preface

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

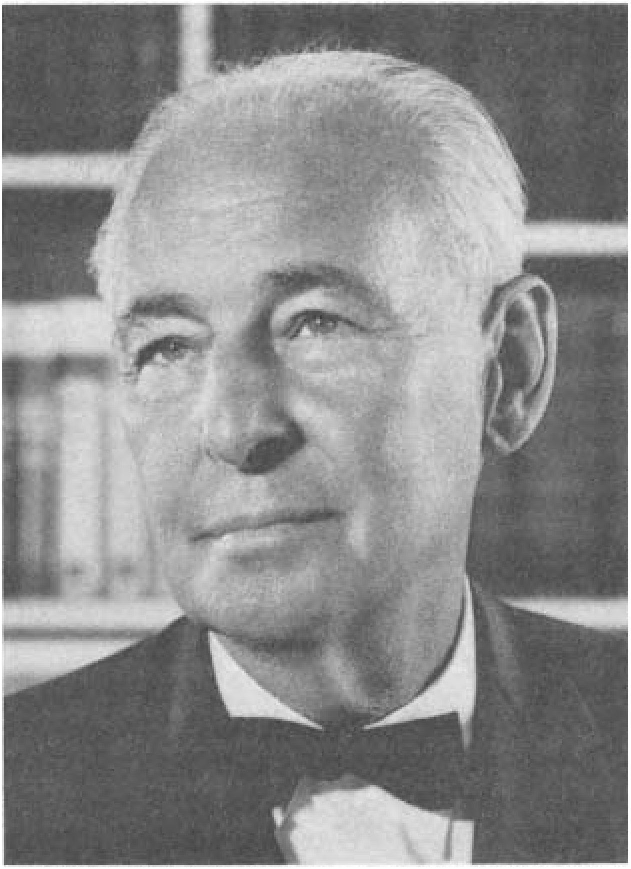
The *Biographical Memoirs,* begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work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

PETER H. RAVEN

HOME SECRETARY

Biographical Memoirs

VOLUME 61



Lloyd Viel Berkner

February 1, 1905-June 4, 1967

by Anton L. Hales

A Supreme Optimist, Lloyd Viel Berkner believed firmly that what *should* be done *could* be done. He had the ability, furthermore, to persuade others that this was so, thereby getting support for large, expensive projects. As his contemporary, Merle Tuve, wrote:

> The astonishing thing about [Berkner's] lifetime of varied activities is the frequency with which his large-scale views and proposals were accepted and worked out, to the mutual benefits of his colleagues and the public which supported them, usually with public funds.[1]

He presented his views with vigor, yet Vannevar Bush (another contemporary) was able to observe:[2]

> Lloyd V. Berkner was undoubtedly one of the best-liked men in the whole field of science and engineering.

He played a significant role in the scientific effort of World War II and, after it, in the explosive development of public funding for science and technology. At the same time he made major contributions to geophysics and to the development of international cooperation in science.

Frederick Seitz relates the following early illustration of the Berkner style:

> As a pall bearer at Berkner's funeral service in Arlington National Cemetery, I stood next to an air admiral who had been one of Berkner's boy

> hood companions. Like all of us present he was experiencing a profound sense of loss. During one of the interludes in the ceremony, my companion said: "It is because of Berkner that I am an air admiral. When we were in high school together he read a newspaper article stating that the Navy planned to give twenty high school students cadet training for the Air Reserve during summers and invited applicants. Berkner said to me, 'We are going to take them up on that.' I suggested that it was a pretty hopeless cause, but he felt the opportunity was important enough to be worth a try. Both of our applications were turned down, which led me to say 'I told you so.' Lloyd said, 'Look, we are going to that camp and going to do from the sidelines exactly what everyone else does. One day, two of the boys they picked will fall out, and you and I will fall in.' I followed Lloyd's lead and we did exactly what he proposed, getting up with the others at dawn and going through all the training routine. The officer in charge of the program came up to us one day after about two weeks had passed and said 'Hey! You two, come over here and get into line!' That was our admission to Naval Aviation!"[3]

Berkner served in the Naval Reserve until 1965, when he retired with the rank of rear admiral.

THE EARLY YEARS

Born in Milwaukee, Wisconsin, on February 1, 1905, Lloyd Berkner grew up in Sleepy Eye, Minnesota. Already at age fourteen he was an enthusiastic radio amateur with his own station, 9AWM, and before entering college set a distance-speed record—using only homemade equipment—for relay-radio communication from Connecticut to Hawaii and back.[4] At the University of Minnesota, where he studied electrical engineering, he worked with the university's experimental radio station to establish WLB, one of the Twin Cities' earliest broadcast radio stations. He also joined the Naval Aviation Reserve, took flight training, and devised, installed, and flight-tested a small VHF radio-communication system for small naval aircraft. (One wonders how he found time to do all this while completing his degree!)

For a year after graduating from Minnesota in 1927, Berkner worked on a radio-range system for the early airmail routes and on radio navigation beacons for the Airways Division of the U.S. Bureau of Lighthouses. Moving to the Bureau of Standards, he accepted an assignment as a radio engineer with the Byrd Antarctic expedition. Leaving behind his new bride, Lillian Fulks, he flew with Byrd on the first Antarctic flight—a reconnaissance mission to find a path through the ice for the ship to reach the base. Once in Antarctica, Berkner's major duty was to assist Malcolm Hanson in setting up the radio communication equipment on which the success of the expedition depended. Little America's seventy-foot antenna masts made radio communication possible with the expedition's aircraft and stations all over the world.

After the facilities at Little America were completed, Berkner returned with the ships to Dunedin to set up a station that would link Little America with the outside world. Extensive radio operations halfway round the globe to and from Antarctica represented an early epoch in long-distance radio communication and made the Byrd expedition an early "media event." Lillian joined Lloyd in Dunedin, and later the two took part in the Byrd expedition's triumphal tour of New Zealand.[5]

In addition to operating the relay link station for communications from Antarctica, Berkner monitored the strengths of the signals from stations in Great Britain and the United States. He reported his analysis of these data in his first scientific publication.[6]

On returning to the Bureau in 1930, Berkner continued his study of radio transmission conditions. He persuaded the Bureau[7] to initiate a half-million dollar project for studies of the ionosphere using radio-pulse transmissions—a technique developed five years earlier by Breit and Tuve at the Department of Terrestrial Magnetism (DTM), Carnegie Institution of Washington (CIW). This was the first of his many successes in getting order-of-magnitude funding increases for scientific research.

During this period he studied ways to determine the fine structure of the F-layer of the ionosphere, strove to develop a rationale for the prediction of long distance short-wave radio propagation conditions, and analyzed ionosphere data collected during the Second Polar Year (1932). But in the early years of the Depression, the Bureau of Standards cut the ionosphere project's funds, and the Carnegie group suggested that Berkner join DTM to continue ionospheric research on a smaller and more personal scale.

CARNEGIE INSTITUTION OF WASHINGTON, DEPARTMENT OF TERRESTRIAL, MANAGEMENT (1933-39)

In 1933 the activities of DTM centered on terrestrial magnetism. As acting director John Fleming recorded in CIW's *Year Book* 31: "As in the preceding year, the year July 1, 1931 to June 30, 1932 was given over largely to the statistical investigations of the accumulated observational material and to the development of the possible laboratory attack on problems in terrestrial magnetism and electricity." Research at DTM, however, was neither as circumscribed nor as pedestrian as this statement might imply.

Under Louis Bauer, DTM had completed a systematic magnetic survey of the Earth, including biological, physical, and chemical observations made at sea during the cruises of the nonmagnetic ship, *Carnegie.* It was the analysis of these observations and the magnetic field observations to which Fleming referred. Yet the staff published papers on all aspects of terrestrial magnetism and electricity, including the relation of magnetic phenomena to solar activity and the ionosphere. They also measured the direction of magnetism of rocks and even of sedimentary cores from the North Atlantic.

But in the thirties excitement in physics centered in the atomic or nuclear fields, and—not surprisingly—DTM physicists became involved in these fields. Begun in 1926, the atomic physics program was "a deliberate attempt to provide a new means of attack on some of the most basic problems in magnetism and physics by the development of artificial (high voltage) sources of high-energy particles and radiations" (CIW *Year Book* 31). These experiments included proton-proton, neutron-neutron, and proton-neutron interactions, as well as various "transmutation" phenomena. The most exciting results were achieved in 1939 when—following the discovery of nuclear fission of uranium by chemical methods (in the language of the *Year Books,* "atomic transmutation")—DTM nuclear physicists were able to confirm fission of uranium under bombardment by neutrons. The possibility of a chain reaction, and of whether atomic energy in amounts of practical importance could be developed, were mentioned in *Year Book* 39.

The staff of DTM was a small and coherent group, all well aware of what each other was doing. It was in this environment that Berkner's scientific interests developed. Their regular conferences on ionospheric research and theoretical physics brought visitors from all over the world, and during this period, Berkner developed his appreciation of geophysics as a global science dependent on worldwide observations.

At DTM, Berkner worked on the ionosphere-sounding program, and all his papers were concerned with the ionosphere (and its relation to terrestrial magnetism, solar activity, and radio transmission) or to the development of equipment for ionospheric observation (as, for example, the continuously recording, automatic ionosphere recorder).

In 1936 Berkner and his wife visited Germany and England. In England he met Sir Edward Appleton, who then led the British research on the ionosphere. He also attended his first international meeting, the General Assembly of the International Union of Geodesy and Geophysics at Edinburgh.

In 1938 the Berkner family spent some months at the Carnegie Magnetic Observatory at Watheroo, Australia, which proved to be a great family experience. Travelling by ship to England, they then sailed the Suez Canal route to Australia, with stops at many ports on the way. There followed nine months of life in the Australian outback while Lloyd first installed an automatic ionosphere-sounder, then worked on the records he received from it. On the way back to the United States, he lectured on aspects of ionospheric research at universities in Australia and New Zealand.

As Henry Booker wrote of this period of Berkner's career:

> In the 1930s only two or three people in the world were thinking about automatic ionospheric observatories. Of these few, the one who conceived, designed, built, installed, operated, and exploited scientifically the first group of successful ionospheric observatories was Lloyd Berkner .... While this period of Berkner's career was dominated by the creation of a new style of ionospheric sounding equipment, and while this type of equipment became of great practical importance for radio communications, his personal objective in the endeavor was to further the science of geophysics .... With these sounders many ionospheric phenomena now well known were clearly recognized for the first time.[8]

Berkner, Wells, and Seaton published more than fifteen joint papers describing these phenomena before the outbreak of World War II.[9]

It is clear that in the eyes of DTM's three ionosphere researchers and of other colleagues at DTM, their research was pure, or basic, science. Yet it would be hard to argue they never had practical ends in mind. Their work on the ionosphere may not qualify as basic science according to

Vannevar Bush's definition of basic research as an endless frontier, research "performed without thought of practical ends."[10] Yet basic and applied science are often linked more closely than Bush's definition implies, with the distinction left to the taste of the researcher.

In 1939 Vannevar Bush was president of the Carnegie Institution of Washington. He was also chairman of the National Advisory Committee for Aeronautics and thus had contacts with the White House staff. Convinced that war was coming and that science must play an important role in the impending conflict, he met with President Roosevelt in June 1940. Out of this meeting, the National Defense Research Committee (NDRC) was born. NDRC—later the Office of Scientific Research and Development—was to enlist civilian scientists to work in university laboratories on projects deemed by the Committee to be of importance to the war effort.

Naturally enough, some of the early projects were at DTM: development of a network of ionospheric-sounder stations in the western hemisphere, so that optimum communications frequencies could be predicted; development of a proximity fuse; and atomic fusion experiments using high-energy neutron bombardment of uranium. Long before Pearl Harbor, Berkner worked with Harry Wells on the ionosphere-sounder network and with Merle Tuve on the proximity fuse.

In 1941 Berkner spent a few months in Alaska with Lillian and his two daughters, Patricia and Phyllis, working on the installation of an ionosphere sounder at College, Alaska, which he envisaged as the first stage in the development of a complete geophysical observatory in Alaska.

When the Office of Scientific Research and Development (OSRD) was formed, Bush appointed Berkner as his assistant, but in September 1941, the Navy recalled him to ac tive duty to organize a radar section in the Bureau of Aeronautics. Soon thereafter he was given charge of all naval aviation electronics engineering. Berkner worked tirelessly to supply radar to Naval aircraft as fast as possible, vigorously supporting the development of airborne radar to make fighter aircrafts' protection of the fleet at night more effective.

Of Berkner's wartime work, Admiral Hall said: "The Navy benefited much from Lloyd Berkner's work then.... Although professionally demanding, he was a leader, not a cold driver. Ashore or afloat, he would suggest some idea or means of solving a problem not previously considered. He was persuasive and men liked working for and with him."[11] The Navy awarded him the Legion of Merit and the Secretary of Navy's Commendation with ribbon. The British, conferring on him the Order of the British Empire, called him "a cooperative friend and forthright critic."

THE POST-WAR YEARS (1946-51)

Berkner returned to DTM in 1946 as chairman of the Section of Exploratory Geophysics of the Atmosphere. For much of the period from 1946 to 1951, however, he was on leave of absence to study the interaction between government and science, and Harry Wells managed the Section as acting chairman. In 1946, Vannevar Bush became chairman of the joint Research and Development Board created by the Departments of War and Navy,[12] and Berkner was appointed executive secretary of the Board.[13] As such he was responsible for the creation of committees, panels, and other mechanisms for involving the scientific and technological community in military R&D.

After this assignment he returned to DTM, but was there barely a year when he was named special assistant to the Secretary of State and granted another leave of absence.

He headed a committee that planned and programmed the Military Assistance Program for the North Atlantic Treaty Alliance, saw this program through Congress, and put it into effect.

His next stint at DTM was broken by a study, carried out with a committee of the National Academy of Sciences, of the growing impact of science on foreign policy. The report issued by that committee, *Science and Foreign Relations*—often called the Berkner Report—recommended the appointment of a science advisor to the Secretary of State.

In 1950, during an after-dinner speech at a conference on ionospheric physics at State College, Pennsylvania, Berkner outlined another pressing area of ionospheric research.[14] He stressed that solving dynamical problems of the outer atmosphere required a major effort, suggested several directions such an effort should take, and then launched into a discussion of the evolution of the Earth and its atmosphere and the role oxygen had played in the emergence of life from the ocean. During the sixties, Berkner himself returned to his earlier interest in the study of the Earth's atmosphere.

ASSOCIATED UNIVERSITIES YEARS (1951-60)

In 1951 Berkner became the first full-time president of Associated Universities Inc. AUI is an autonomous, nonprofit corporation set up by nine northeastern universities to organize and operate a nuclear research center and provide large, complex research equipment—such as accelerators and nuclear reactors—for use by the community.

By 1951, Brookhaven National Laboratory was already well established, and the AUI trustees, in appointing a fulltime president, sought to insure a broader role for AUI in scientific management. Berkner chose to give Brookhaven only general oversight, leaving control of the laboratory in the hands of the director. At Brookhaven during this period, new high-energy particle accelerators were developed.

In his early years at AUI, Berkner was involved in a number of defense-related studies. One of the more important of these was the M.I.T. summer study program, Project Lincoln, that led to the creation of the Distant Early Warning (DEW) line in the Arctic.

In 1954 AUI undertook responsibility for the organization of the National Radio Astronomy Laboratory at Greenbank, West Virginia. The first telescope, an eighty-five foot dish, was completed in 1958. The construction of the high-precision, fully steerable, 140-foot-diameter telescope—although beset by both financial and technical difficulties—was well under way by 1960, though it was not completed until 1963. It was during his years as AUI president that Berkner's involvement in national and international science reached its peak.

BERKNER'S ROLE IN NATIONAL AND INTERNATIONAL SCIENCE

At the Edinburgh General Assembly of IUGG in 1936, Berkner became secretary of a joint committee of IUGG and URSI (the International Union of Radio Sciences). In 1946 he was active in the discussion that led to the reorganization of IUGG and in 1948 became a member of its executive committee.

Berkner's greatest contribution to international science was his suggestion (in April 1950 at a party at Van Allen's home in honor of Sidney Chapman) that a third International Polar Year should be arranged twenty-five years after the second, instead of after the customary fifty years. Berkner argued that technologies developed during the war years would make possible more effective study of Antarctica, that 1957-58 would be a year of sun spot maximum, and—

what J. Tuzo Wilson called "the most persuasive of all"—that those present at the party would be able to participate in 1957-58 but not in 1982-83.[15] Berkner envisaged a global program, not one restricted to the polar regions, and Sidney Chapman later suggested it be called the International Geophysical Year (IGY).

The International Council of Scientific Unions (ICSU) created a Special Committee for the International Geophysical Year (CSAGI) with Sidney Chapman as president, Berkner vice president, and Marcel Nicolet secretary-general. Of this group, it was Berkner who took the lead in promoting what would become the first of the great international programs in geophysics. Berkner served on the executive committee of the U.S. National Committee, recruiting Kaplan, Shapley, and Gould to act as chairman and vice chairman of the National Committee, and chairman of the Antarctic subcommittee, respectively.

As a result of the IGY and of resolutions passed at the URSI and IUGG General Assemblies in 1954 (endorsed by CSAGI later that year), which drew attention to the advantages of satellites for studying various aspects of solar activity and their effect on the ionosphere, the satellite programs of the United States and the USSR began.

When the resolutions were considered by the U.S. National IGY committee, there were differences of opinion within the committee. Berkner and Athelstan Spilhaus were vigorous advocates of a nonmilitary U.S. space program, but others doubted whether this was really science and whether it was wise for such a large expenditure to be undertaken under the aegis of the IGY committee. Eventually, in July 1955, President Eisenhower announced that there would be a U.S. space program, and not long thereafter the USSR announced a similar program.

As is well known, it was during a reception in honor of

CSAGI delegates at the Russian Embassy in Washington on October 4, 1957, that Walter Sullivan of the *New York Times* told Berkner TASS had announced the launch of *Sputnik I.* Berkner immediately announced this publicly, congratulating the Soviets on their achievement. The first U.S. satellite, *Explorer I,* was launched on January 31, 1958. Those who believed that the satellite program would make contributions to science were vindicated when Van Allen announced on May 1, 1958, the discovery of the first of the Van Allen radiation belts as a result of an analysis of observations from *Explorer I.*

The major and most enduring of IGY achievements was its demonstration that international cooperation in solving problems of global science could work. In 1959, the Antarctic program culminated with the signing of the Antarctic Treaty, which reserves the continent for peaceful and scientific purposes. By the end of IGY, furthermore, the satellite programs had also made major contributions to the understanding of the upper atmosphere. And in all of this, Berkner was highly influential.

Before the end of the International Geophysical Year, the NAS's IGY group recognized that a mechanism for scientific advice regarding the continuation of the space exploration program would be needed and made a proposal to Detlev Bronk, then president of the Academy, for the creation of a Space Science Board. The Board was created in 1958, before the establishment of NASA. As its first chairman, Berkner was, in the words of Frederick Seitz, "a key figure in the advisory structure which guided the evolution of the new agency."[16]

Berkner became president of ICSU in 1955 and was one of those responsible for the creation of the Committee on Space Research (COSPAR), the international committee for the promotion of international cooperation in space science. Professor W. J. G. Benyon, who was president of URSI in 1954 when the satellite resolution was passed, is quoted by Odishaw as paying tribute to Berkner's role. "Berkner was a man of considerable foresight—indeed in some ways he could be termed a scientific visionary. More than anyone else he brought new life and vigor into international cooperation."[17] Berkner became president of URSI in 1957.

Berkner was elected president of the American Geophysical Union (AGU) in 1959. The Union was a flourishing body but, at that time, its only publication was the *Transactions.* For a number of years DTM had published a journal called *Terrestrial Magnetism,* renamed *Journal of Geophysical Research* in 1950, with Merle Tuve as editor. By 1958 Tuve had grown weary of being editor,[18] and Berkner, who was president-elect, and Maurice Ewing, who was president, took the opportunity to bring the journal under the control of Union-appointed editors Philip Abelson and Jim Peoples. The financial support needed for this initial phase was obtained from the National Science Foundation.

From this small start the very strong AGU publication program of today has developed. Abelson remarks that, while president, Berkner "brought about changes that made the Union the vital organization it is today. .. . Lloyd Berkner was a man of energy and imagination. He had a liking for people and an ability to identify talent among them. He selected grand objectives and then moved decisively, working with others to achieve the goals. Lloyd was an organization man in the best sense of the word. He understood well how much people working together can accomplish."[19]

One of Berkner's other activities of this time was to chair the Panel on Seismic Improvement,[20] as a result of doubts about whether the monitor system proposed by the 1958

Geneva Conference was adequate. Their report, written in only three months, was the blueprint for seismology during the ensuing twenty-five years. Only recently, aided by instruments that give much more complete coverage of the seismic frequencies, have we gone beyond its original vision of processes within the Earth and what can be found out about them. Berkner's role on this panel was described to me by one of the participants: "Berkner had a larger vision of what was needed to make seismology into a modern science than many of the [other committee] members;" and by another: "Lloyd's contribution was to put things into perspective."

Berkner became a member of the Academy in 1948 and was involved in its affairs until his death in 1967. He worked through the Academy in all of his activities in international science. In 1960 he became treasurer and, according to NAS President Frederick Seitz, "revamped the Academy's investment and business operations."[21]

One of Berkner's Academy activities in the 1950s was to serve as co-chairman of the National Committee for Meteorology. Convinced of the need for a national center for atmospheric research, he and others made the case that led to the creation of the National Center for Atmospheric Research, supported by the National Science Foundation, in Boulder, Colorado.

From 1956 to 1959 Berkner served on President Eisenhower's Science Advisory Committee. In 1958 he returned to Antarctica, revisited the 1928-30 Byrd expedition base at Little America, and wrote a report for President Eisenhower. This report was a factor in the President's decision to continue a U.S. Antarctic program after the end of the IGY. An island in the Weddell Sea was named Berkner Island in recognition of his contributions to the development of research in the Antarctic.

GRADUATE RESEARCH CENTER OF THE SOUTHWEST

Berkner joined the board of Texas Instruments in 1957 and—in discussion with Erik Jonsson, Cecil Green, and Eugene McDermott—became aware that a number of leaders of the Southwest community, concerned about the

> serious problems facing the intellectual and economic growth of the Southwest in this technological age, . . . realized the imperative need for the generation of an intellectual and scientific climate in the region to ensure its healthy development.[22]

As Tuve describes Berkner's reaction to this problem:

> With his typical focus on [those] events and problems forming up to trouble us some years in the future and stimulated by the geographical imbalance so readily observable in higher educational establishments and research activities in the United States, Berkner undertook in the late nineteen-fifties to alert the Midwest and the South to their shortcomings in these areas.[23]

Leaders in the Dallas community had set up a Graduate Research Center at Southern Methodist University to provide broad support for graduate activities, but Berkner soon became convinced that so complex a problem required more direct support. As rapidly as qualified faculty were found for the graduate school, for instance, they came under intense pressure to go to more scientifically and technologically advanced regions of the country. Berkner envisaged the creation of a community of scholars in an institution devoted to postgraduate education and research. On February 14, 1961, with the support of the community, the Graduate Research Center of the Southwest—subsequently renamed the Southwest Center for Advanced Studies—came into being.

Berkner expected the Center, with grants from federal agencies, to become self-supporting within a few years. The Center had not yet reached this goal, however, when he suffered his first severe heart attack in June 1964, only two years after the first staff members had joined the Center. After two months in hospital and several more recuperating, Lloyd was forced to curtail his activities on behalf of the Center. Deprived of his full leadership, the Center still was not fully self-supporting in 1967, the year he died.

In 1969, after lengthy discussion, the Center became the University of Texas at Dallas, with undergraduate programs for juniors and seniors and a limited graduate program. Politically necessary though it may have been, it is my view that the decision to restrict the University to the last two years of an undergraduate program could only prevent its achieving the level of excellence Berkner and the founders of the Center envisioned.[24] It will never be known if Lloyd would have succeeded in achieving his goals for the Center and the region, or whether the problem of geographical imbalance was too intractable for a short-term solution. Clearly the long-term solution is still to come.

Although he curtailed his activities on behalf of the Center, Lloyd was constitutionally unable to take things easy. Together with Lauriston Marshall, he continued studying the evolution of Earth's atmosphere, the main theme of his after-dinner speech to the ionosphere conference at The Pennsylvania State University in 1950. Their view was that Earth was formed without an atmosphere, or had lost its initial atmosphere at an early stage of its history. The present atmosphere has formed as a result of the release of gases from the interior, the three most plentiful being water, nitrogen, and carbon dioxide—most of which were removed from the atmosphere to form carbonate rocks. It is not possible for the oxygen of the atmosphere, however, to have come from the interior as oxygen, and it must have been derived in some way from the water (perhaps by photodissociation) and from the carbon dioxide (by photosyn thesis). Once the water was broken down, its oxygen would have recombined with the hydrogen unless the hydrogen was able to escape, so that the major source of Earth's oxygen would have had to be carbon dioxide, with the carbon deposited as fossil fuel and in sedimentary rocks.[25]

These studies alone were not enough to keep Lloyd occupied. He continued to make speeches stressing the importance of technology in improving the lot of mankind, while at the same time stressing that "no technology, however powerful, can solve the problem of an exponentially increasing population." Berkner fully embraced Francis Johnson's observation that "it must be recognized that a biological population will expand until something limits it; this can be deliberate control or it may be hunger or pestilence."[26]

LLOYD BERKNER, THE MAN

At the dedication of the University of Texas at Dallas's Lloyd Berkner Hall, many tributes were paid to Lloyd Berkner as a colleague. Two of those tributes describe the man so well, they are worth quoting here. Odishaw said:

> For Berkner came upon you as though on a wave. A large man, large in physique, large in thought and deliberation, large in style and substance. . . One general trait of his . . . not generally known . . . [in addition to] his good judgment about people, [was] his warmth toward the young.

Or as Erik Jonsson put it:

> Now permit me to look at Lloyd, the man, as my colleagues and I know him in SCAS: brilliant, kindly, good-humored, far-seeing, determined, yet patient with his fellows and a beloved friend . . . . Large of body as well as intellect, Lloyd moved easily in any segment of the diverse society and environment between the two Poles. Always he seemed to be "at home" and enjoying himself.

As I have said before, Berkner believed that what *should* be done *could* be done, and in that sense he was an optimist. But this optimism was never ill-founded. His goals were always achievable and almost always achieved (one major exception being his unsuccessful proposal to the National Science Foundation for a national geophysical institute.[27])

Berkner liked to do things quickly. His accomplishments during his short spells as executive secretary of the Research and Development Board of the Department of Defense, and at the State Department setting up the Military Aid Program for the North Atlantic Treaty Organization, attest to this. That he was aware of this innate hastiness is shown by self-deprecatory comments he made while speaking to Dael Wolfe, as quoted by Milton Lomask[28] :

> It's a good thing they picked Alan [Waterman to direct the National Science Foundation]. He's been slow and cautious, and sometimes people have been irritated by the way he has handled things. But he has been a steady and constructive builder. Had I been director I would have moved too fast, and the Foundation would probably have been torn apart by now. But being aware that he sometimes moved too fast did not affect Berkner's style of achieving his ends.

One factor in his success was his great ability to present a cause so cogently that others found the case difficult to refute. Another was that he enjoyed committees. He was a good committee member and an excellent chairman. He always studied the relevant papers before a meeting. He allowed discussion to continue until he thought all points of view had been expressed, then presented his own summation of the consensus. His timing and judgment were good, and in most cases his conclusion was close enough to the views of the majority that the committee found it acceptable. It was at a committee meeting of the Council of the Academy that he suffered his last, fatal heart attack.

In the years between 1964 and 1967 Lloyd was busier than some of his friends thought was good for him, but when this was put to Lillian Berkner, she replied: "When

Lloyd is doing nothing he is miserable; when he is busy he is happy and will die happy. I would not have it otherwise."

Berkner never found time to complete a Ph.D., there having always been more urgent science on his agenda, but he was awarded twelve honorary degrees (the D.Sc. from the Brooklyn Polytechnic Institute [1955], Dartmouth College [1958], and the universities of Calcutta [1957], Notre Dame [1958], Columbia [1959], Rochester [1960], and Tulane [1961]; the Ph.D. from the University of Uppsala [1956]; the LL.D. from the University of Edinburgh [1959]; and the D.Eng. from Wayne State University [1962] and Nevada's Lafayette College [1965]), and numerous other awards. His field was geophysics, and he received both the Fleming and Bowie medals of the American Geophysical Union.

More importantly, he was—and is remembered by his friends as—a man of vision.

I am greatly indebted to Lloyd's daughter, Patricia Berkner Booth, for providing me with a copy of an essay, "Lloyd Viel Berkner—Man of Distinction," written by Lloyd's grandson, C. Arthur Booth, in 1978; and also to Francis Johnson and Al Mitchell of the University of Texas at Dallas for making available to me copies of materials from the University's archives. My especial thanks to Professor F. Johnson for sending me a copy of the speeches made at the dedication of Lloyd Berkner Hall in 1973.

NOTES

1. Merle A. Tuve, "Lloyd Viel Berkner," *Yearb. Am. Philos. Soc.* (1967):110.
2. Vannevar Bush, "Lloyd Viel Berkner—A Commentary," *IEEE Spectrum,* 1967.
3. Frederick Seitz, dedication of Lloyd Berkner Hall, University of Texas at Dallas, 1973.

4. Francis S. Johnson, "Lloyd Viel Berkner and His Research," memorial lecture, XVIth General Assembly of the International Union of Radio Science in Ottawa, Canada, 1969.

5. C. Arthur Booth, "Lloyd Viel Berkner—A Man of Distinction," unpublished essay, 1978. A copy of this essay by Berkner's grandson is available in the NAS archives in Washington, D.C.

6. Lloyd V. Berkner, "Some Studies of Radio Transmission Over Long Paths Made on the Byrd Antarctic Expedition," *U.S. Bureau of Standards Journal of Research* 8(1932):265-78.

7. Tuve, "Berkner," *Yearb. Am. Philos. Soc.,* 1967, p. 110.

8. Henry G. Booker, Berkner Hall dedication, University of Texas at Dallas, 1973.

9. The first of these was L. V. Berkner and H. W. Wells, "Report of Ionosphere Investigations at the Huancaljo Observatory (Peru) during 1933," *Proc. Inst. Radio Eng.* 22(1933):1102-1123.

10. See Milton Lomask, *A Minor Miracle,* National Science Foundation, p. 43. "Science—the Endless Frontier" was a report Vannevar Bush prepared at the request of President Franklin D. Roosevelt and submitted to President Harry Truman.

11. G. B. H. Hall, Berkner Hall dedication, University of Texas at Dallas, 1973.

12. The Department of Defense was not created until 1947.

13. Tuve, "Berkner," *Yearb. Am. Philos. Soc.,* 1967, p. 111.

14. Lloyd V. Berkner, part B, in *Proceedings of the Conference on Ionospheric Physics,* eds. L. Katz and N. C. Gerson (Cambridge, Mass.: Geophysics Research Division, Air Force Cambridge Research Center, 1952), p. 14.

15. Hugh Odishaw, Berkner Hall dedication, University of Texas at Dallas, 1973.

16. Seitz, Berkner Hall dedication, University of Texas at Dallas, 1973.

17. Odishaw, Berkner Hall dedication speech, University of Texas at Dallas, 1973.

18. Abelson, Berkner Hall dedication, University of Texas at Dallas, 1973.

19. Philip M. Abelson, Berkner Hall dedication, University of Texas at Dallas, 1973.

20. At the suggestion of the State Department, a panel of distinguished seismologists was named by the Special Assistant to the President.

21. Seitz, Berkner Hall dedication, University of Texas at Dallas, 1973.

22. Lloyd V. Berkner, *Graduate Education in the Southwest* (Dallas, Texas: Southern Methodist University Press, 1961).

23. Tuve, "Berkner," *Yearb. Am. Philos. Soc.,* 1967, p. 113.

24. In 1989 the Texas Legislature passed a bill authorizing the University of Texas at Dallas to accept freshman and sophomore students as well as juniors and seniors.

25. L. V. Berkner, "The Role of Oxygen," *Saturday Review* 49(1966): 30-34.

26. Francis Johnson, "Berkner and His Research," Memorial Lecture Series, URSI, August 27, 1969.

27. J. Merton England, *A Patron for Pure Science* (Washington, D.C.: National Science Foundation, 1982), pp. 307-09.

28. Milton Lomask, *A Minor Miracle* (Washington, D.C.: National Science Foundation, 1975), p. 109.

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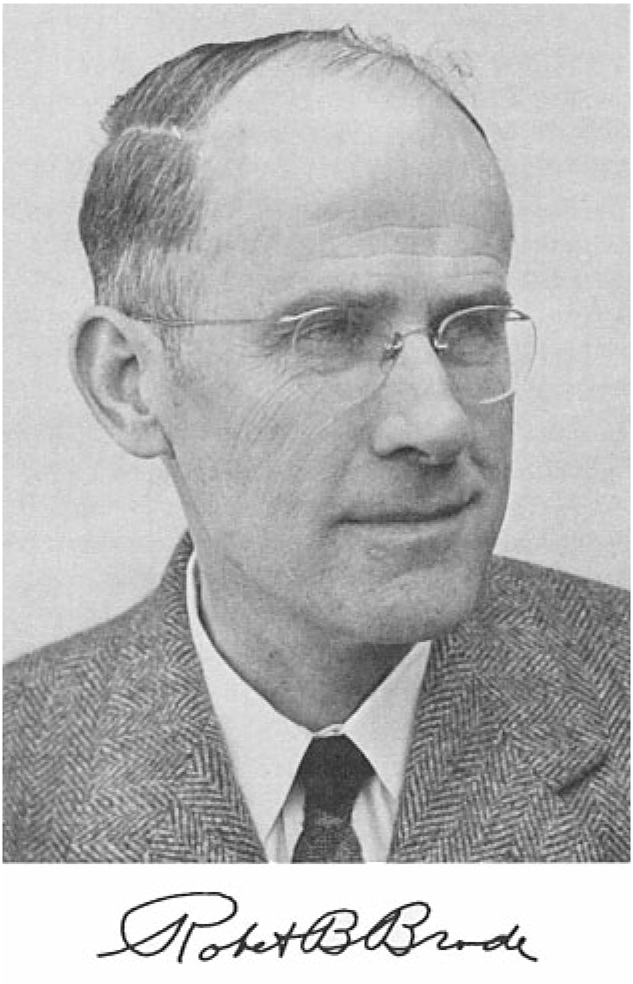

Robert Bigham Brode

June 12, 1900-February 19, 1986

by William B. Fretter

Assisted by David L. Judd

The Eminent Physicist, distinguished educator, public servant, academic statesman, and professor emeritus of the University of California, Robert Bigham Brode, died February 19, 1986, at his home in Berkeley, California. Robert was one of triplets born June 12, 1900, in Walla Walla, Washington. Following the example of their father, professor of biology at Whitman College, Robert and his brothers, Wallace and Malcolm, all became distinguished scientists.

EDUCATION

Bob Brode's young life included not only science but also a general cultural education. During those early years he became a proficient flute player, contributing enjoyment to others and gaining personal pleasure for many years.

Leaving Whitman with a bachelor's degree in 1921, Brode went to the California Institute of Technology, where he earned the Ph.D. degree in physics in 1924. By so doing he took his place at the head of a long line; 1924 was the very first year in which this degree was awarded by Caltech, which was then, under the direction of Robert A. Millikan, developing its scientific program.

PREWAR RESEARCH AND TEACHING

Brode's first research, published in 1925, showed that molecules such as nitrogen and carbon monoxide, or methane and argon—having similar arrangements of their external electrons—have very similar cross-sections for collisions with slow electrons, leading to results that are difficult to explain using classical physics. Not until 1966, when modern computers to evaluate numerically the wave-mechanical analysis became available, were his results completely understood. From then on his early work was widely used in analysis of data concerning the scattering of charged particles at low energies.

Brode started his professional experience as an associate physicist in the Bureau of Standards. He held a Rhodes Scholarship at Oxford in 1924-25, a National Research Fellowship at Göttingen in 1925-26, and a research appointment at Princeton in 1926-27. He married Bernice Hedley Bidwell on September 16, 1926.

Brode came to the University of California at Berkeley in 1927 as an assistant professor of physics and rose very rapidly to the rank of full professor in 1932. During these years he was energetically establishing his program of research in the growing department, continuing his work on interactions of slow electrons in various gases, a field now important in research on plasma physics. Workers at several European laboratories, in particular Dr. L. LePrince-Ringuet and Dr. P. Auger in Paris, were involved in similar research.

Brode was especially pleased to be awarded a Guggenheim Fellowship for study in Cambridge and London in 1934-35, as he had developed a great affection for England from his rewarding stay in Oxford as a Rhodes scholar ten years earlier. He, his wife, and their two small sons also enjoyed a warm friendship with the family of P. M. S. Blackett of Birkbeck College, London.

During this period Brode transferred his activity from very slow electrons to very fast particles, the heavy components of cosmic radiation. He became enthusiastic about the scientific results that could be obtained by applying Blackett's techniques involving counter-controlled cloud chambers to study the specific ionization and momentum of these particles.

Returning to Berkeley, Brode continued these studies with a succession of students, starting with M. A. Starr. He reported on the design and characteristics of a magnet for cosmic ray cloud chamber studies in which the mass of the mesotron was measured by using a drop-counting technique in the magnetic field. With his student, Dale Corson, he was able to separate electrons, protons, and mesons and to measure their masses. In 1938, Corson applied the drop-counting technique to confirm the theoretical prediction that specific ionization for electrons should increase with increasing energy for relativistic mass above about four times the rest mass.

Brode was always interested in teaching both undergraduates and graduates. He was well remembered by many students as the instructor in the upper-division course on electricity and magnetism. He participated regularly in the Monday Night Journal Club started by Ernest Lawrence in the 1930s, often contributing reports of his own research and keen comments on the work of others. In the years 1930-43 fifteen graduate students conducted their research under his direction. During the intensely active period of research and development before World War II, this work resulted in improved equipment and analysis, contributing substantially to the development at Berkeley of a world-class physics department.

WAR SERVICE

Brode's work with cosmic rays was interrupted by the war. Going first to the Applied Physics Laboratory at Johns

Hopkins, he played a leading role in the successful research and development effort on the amplitude-operated radio proximity fuse. When the Los Alamos project was established in 1943, among its many problems was the development of a fusing mechanism to detonate a bomb at a specific height above the terrain. New methods were needed because of the size of the explosion and the unprecedented level of reliability required, but the proximity-fuse problem had elements of similarity, so the project's leaders—many of whom had been Brode's colleagues at Berkeley before the war—sought his talents. Thus it happened that he joined the project at the start, and his wife and sons arrived a few months later. During his entire period "on the mesa" from 1943 until he returned to Berkeley in 1946, Bob Brode was in charge of the "fusing group.

Their early work was directed toward adapting proximity fuses and developing barometric-pressure switches. The latter program led to creation of a sizable organization that, in cooperation with the Air Force at distant air bases, used elaborate instrumentation to collect and interpret data on the evolving designs. Early in 1944 it became evident that the range of proximity fuses was inadequate for the increased estimate of detonation height, and that barometric switches were incapable of the required precision. Brode and his group then focused their efforts on a newly developed simple radar, which was tested extensively, using the experience and the organization from the earlier field tests. A large number of systems were dropped from barrage balloons in New Jersey and on dummy bombs from aircraft based at Wendover Field, Utah. An elaborate design evolved using several radars, together with banks of clocks with pull switches and barometric switches to arm the system after leaving its aircraft, all in series-parallel arrays to guard against trouble from premature operation or failure of any individual component.

The planning and design began in August 1944 and resulted in production of operating units almost a year later. The pace of the work, like every other activity at Los Alamos, was limited chiefly by the number of hours each leader was able to drive himself and his team. Brode had recruited a mixed group of fourteen civilians, twelve reserve military officers, and thirty-seven special-detachment army privates recently drafted after obtaining college degrees in the sciences. In most accounts of the Manhattan Project the work of this group has received little attention, yet it was essential. The quality of its field work was praised by the commanding officers at each of the four sites involved, and the fusing systems on both bombs used to end the war operated perfectly. The group's dedication and skill reflected Bob Brode's leadership and the example he set. One of its members wrote, "No one could have had a more inspiring, demanding, and sympathetic boss under those remarkable conditions. Bob and Bernice were outgoing and hospitable to young people. They enjoyed the New Mexico ambience when time allowed, folk-dancing to all hours on occasion. Bob played the flute in the local amateur symphony. They were a sincere, sophisticated but unaffected couple doing all they could to help others and to maintain a civilized environment during times of great stress."

POSTWAR TEACHING AND RESEARCH

After the war, Brode resumed his teaching and research on measurements of momentum, specific ionization, and range of cosmic ray mesons. Using a cloud chamber in the field of a permanent magnet, he made observations of cosmic radiation particles in a B-29 aircraft at 30,000 feet. He also returned to guiding the research of graduate students, supervising another twenty-two of them between 1946 and 1957, for a total of thirty-seven students, who have made significant scientific, academic, and administrative contributions. They included Dale R. Corson, president and chancellor of Cornell University (1969-79) and William B. Fretter, vice-president of the University of California (1978-83).

He continued to be active in undergraduate education; in the middle 1960s he assisted the U.S. Educational Commission in its mission to many European countries, and took particular interest in the awards of Hayes-Fulbright fellowships. From 1962 to 1965 he was the chairman of the Committee on Physics Faculties in Colleges (COPFIC) formed by the American Association of Physics Teachers and the American Institute of Physics, which reported on methods for improving the teaching of physics in colleges.

A Fulbright Fellow in 1951-52, Brode was again able to work in England, this time on research in Manchester, where he renewed ties with physicists he had known earlier.

Robert Brode was a member of a large number of scientific, academic, and honorary organizations; in many of them he was sought out for positions of service and leadership. These include the International Union for Pure and Applied Physics (vice-president); American Association for the Advancement of Science (president of the Pacific Division); National Research Council (chairman, Physics Division Executive Committee); American Association of University Professors (vice-president and other offices); American Physical Society (member of the Council); National Science Foundation(associate director for research); and International Council of Scientific Unions (U.S. delegate). He served on the board of editors of *Reviews of Modern Physics,* on selection panels for Rhodes Fellowships, John F. Kennedy Foreign Scholarships, and Fulbright Scholarships, and on panels for awards by the U.S. State Department, the U.S. Atomic Energy Commission, and the Institute of International Education. He was also an associate of the Carnegie Institution and a member of the American Academy of Arts and Sciences, the Washington Academy of Sciences, the Optical Society of America, the American Association of Physics Teachers, the American Institute of Physics, Phi Beta Kappa, Sigma Xi, Delta Sigma Rho, and other groups. He was acting director of the Space Sciences Laboratory at Berkeley in 1964-65 and was director, in London, of the University's Education Abroad Program in the United Kingdom in 1965-67.

ACADEMIC STATESMANSHIP

After the war Brode was increasingly sought out by his professorial colleagues at Berkeley to become a leading member of the Academic Senate. Brode was a firm believer in faculty self-governance at the University of California. During the difficult era of McCarthyism, when the university was rent by the oath controversy, he was a vigorous leader of the faculty, serving as chairman or member of its most important committees; his integrity, forthrightness, and deep concern for the welfare of the entire university community were widely respected. In these roles, and through the American Association of University Professors, for which he served as vice-president and in a variety of other posts, he exerted strong and ultimately successful efforts to restore full academic freedom.

Brode rendered long service to the university as academic assistant to two of its presidents, Clark Kerr (1960-65) and Charles J. Hitch (1972-73), as well as to the vice-president for academic affairs, Angus E. Taylor (1967-72). In this capacity, his wise counsel covered a wide range of subjects, including academic freedom, reorganization of the Academic Senate, admission and enrollment issues, standards of peer review for faculty, and the changing status of the ROTC programs on the campus. President-Emeritus Kerr has written of his service, "Bob Brode was one of the great statesmen of the Berkeley faculty. He gave me the best advice I got from anyone. He was always the wise, active, devoted, unselfish leader of faculty opinion. He had the rare ability to be, at one and the same time, both in and above the conflicts of the day. His only agenda was the welfare of the university and of its faculty and students. Might there be more like him!"

HONORS AND AWARDS

Robert Brode's talents and accomplishments were rewarded by a series of scholarships, fellowships, and honorary degrees:

Rhodes Scholarship, 1924-25
National Research Fellowship, 1925-26
Guggenheim Fellowship, 1934-35
Election to the National Academy of Sciences, 1949
Fulbright Fellowship, 1951-52
Honorary D.Sc. degree, Whitman College, 1955
Centennial Award, University of California, 1968
Honorary LL.D. degree, University of California, 1970

The citation for the final honor listed above serves to sum up his career:

> Brilliant physicist, talented teacher, scientific statesman, creative administrator, for over four decades you have inspired countless students. Your interest has ranged from slow electrons in atoms to swift mesons in cosmic rays. You have been many times chosen to guide the affairs of your discipline and have lent your wise counsel to your nation and to the world. You

> have given unflaggingly of yourself in service to your colleagues and to your University. You embody the best of your profession, and we salute you today in gratitude and affection, and confer upon you our highest honor.

Bob Brode had a great fondness for Christmas music. At this time of year he often invited students and others to his home for a sumptuous buffet, followed by singing from the Oxford Book of Carols. For over fifty years, beginning with his arrival at Berkeley in 1927, he was an enthusiastic singer in the Monks Chorus of the Faculty Club, a group of faculty members who gather each December to sing at the annual Christmas dinners of the club. He recruited several members from the physics department and elsewhere over the years, and enjoyed regaling the Monks with tales of the Christmas music at King's College at Cambridge.

This very tall, large-boned, gentle, modest, and considerate man was decidedly a presence—loved and respected by all who knew him. He and his diminutive wife Bernice resided happily for many years high in the Berkeley hills in a distinctive house of rustic redwood design, constructed in part by Bob with his own hands, and later in a similar house nearer the campus. These homes made perfect settings for their gracious and informal hospitality to an enormous circle of colleagues, visitors, students, and friends, who continue to cherish their memories.

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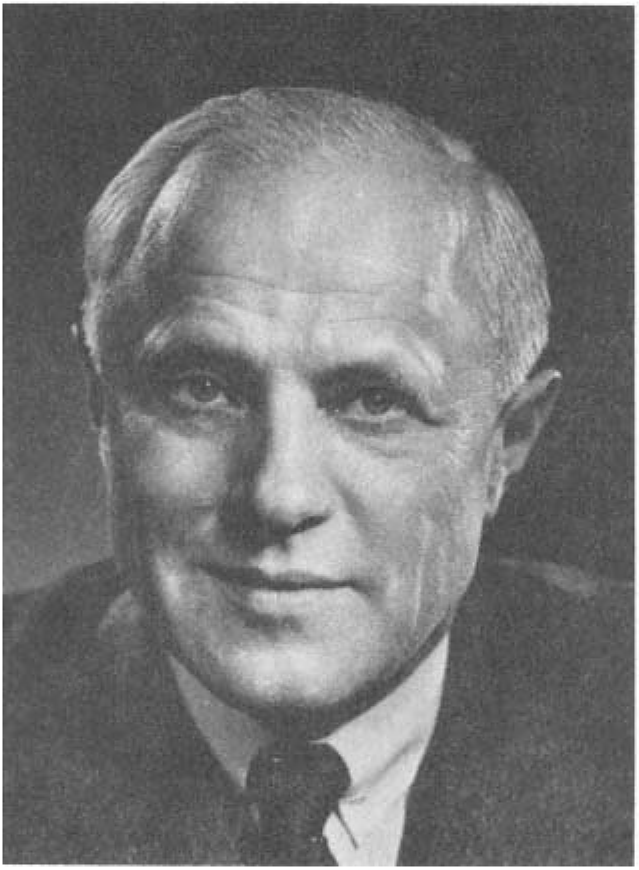

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Karl T. Compton

Karl Taylor Compton

September 14, 1887-June 22, 1954

by Julius A. Stratton

To do full. justice to Karl Compton is an impossible task, for the range of his interests and activities was virtually limitless, his effect upon the scientific community profound, and his service to our country in time of war—and in quieter days—far greater than a nation could expect from a single individual. He was a leader whose fine mind matched his radiant personality and understanding heart. He was a man of principle whose transparently honest goals moved men, a warm friend who inspired loyalty, and a mentor who engendered pride in achievement. Those of us who knew him closely felt the shining example of his own life, and the intervening years make us ever more conscious of the greatest of his legacies to us—his focused and unquenchable spirit.

THE EARLY YEARS (1887-1918)

Karl Taylor Compton was born in Wooster, Ohio, on September 14, 1887, the eldest child of Elias and Otelia Augspurger Compton. Mary, Wilson, and Arthur would follow. Elias's Anglo-Saxon Presbyterian forebears had come to America prior to the Revolution and eventually settled in Ohio, to which Otelia's family—Alsatian and Hessian Mennonites—came early in the nineteenth century.

Elias, who grew up on a farm, taught school before entering the University (now College) of Wooster, a Presbyterian institution, from which he was graduated with highest academic honors in 1881. He moved on to theological school, full of plans for the ministry and service in foreign missions. In his final year, however, he responded to a call from Wooster to substitute for an ailing professor. Ordination, on the basis of these nearly completed studies, was granted in 1897. He had performed so well at Wooster that he was urged to accept a regular appointment, and he remained there until his retirement, becoming professor of philosophy and serving for many years as Dean.

Elias Compton married Otelia Augspurger in 1886, and the pair proved to be remarkable parents of a remarkable family. The three sons, all with Princeton doctorates, eventually became college presidents. Two—Karl and Arthur—were distinguished physicists, and the latter became a Nobel Prize laureate in 1927. Their daughter, Mary, an excellent scholar and honor graduate of Wooster, fulfilled a parental dream by spending many years in India as the wife of C. Herbert Rice (also a college president) and as an active missionary worker.

The four young Comptons' lives and achievements are perfect testimony to the importance of childhood environment and the influence of home and family. There can be no doubt that the strength of Karl Compton's character and his supreme regard for the individual, as well as his innate ability and intelligence, stem from his background. It was a family reared in an atmosphere of stability and spirituality, of discipline and understanding, of shared interests and responsibilities and commitment to the common good. There was, in addition, always time for fun and the enjoyment of life.

A rough outline of "preliminary random ideas, to be refined" indicates that K.T., as he came to be known by many, intended to write his memoirs—which, had he been spared, surely would have presented a fascinating, honest account of a productive and eventful life. First on his list were his parents, their plans, and their home on College Avenue —always a haven for students and the central focus of his early days; then came his friends, the gangs, and the games; his public school days and skipping a grade; transfer in 1902 to Wooster's preparatory department for the last two years of high school, and his college days there. He would have emphasized the jobs he took from the age of eleven through college to earn spending money and also, as he once said, "to harden my muscles for athletics." He carried hods on construction projects, worked as a farm hand, mule skinner, and book canvasser, in tile and brick factories, and, following a brief introduction to surveying in a mathematics course, on the "first mile of state-paved road in Ohio." He enjoyed "the daily grind of the pick and shovel gang" and "the finesse to be acquired in pitching a shovelful of dirt onto a wagon." From the men he came to know who dug ditches, laid bricks, worked on farms, and whose "good qualities and special abilities" he appreciated, he learned "the joy of working with your hands."

Beginning in 1897, Karl's summer vacations were spent camping out at Lake Otsego in Michigan with friends and students from the college joining the family party. There he developed a lasting appreciation for the outdoor life, fishing, canoeing, hiking, hunting. For relaxation he liked nothing better than a strenuous canoe trip in the wilderness. It was an interest that provided more than an opportunity for quiet reflection and the testing of physical endurance. It also provided the challenge of leaving the camp site better than he had found it, and he brought to that task the same sense of responsibility that would guide him in every effort he undertook.

Both Karl and Arthur gave early evidence of an interest in science. Though Karl announced while still in the preparatory department that he wanted to be a scientist, he still had not decided on a specific field when he was a sophomore in college. At that time he was doing odd jobs in the biology laboratory, and looked forward to an assistantship for the following year. Funds did not become available, however, and he accepted an appointment in the Physics Department as an assistant in charge of arranging equipment and setting up laboratory experiments. But for this change in circumstances, he might well have become a biologist rather than a physicist. He would often cite this incident as an example of how seemingly minor events can alter the direction of one's life.

Karl was graduated cum laude in 1908 with a bachelor of philosophy degree. As an undergraduate he had been in every way an outstanding student leader and participant in extracurricular activities. He was, above all, a fine athlete. He was a left end on the football team and became captain in his senior year. As a member of the varsity baseball team (of which he also served as captain), he was a good hitter and fielder and, as a senior, served as pitcher. He also coached the girls' basketball team, was active in his fraternity, and taught a Bible class for the Y.M.C.A.

He remained at Wooster as a graduate assistant and laboratory demonstrator for elementary physics. It was a period in which he demonstrated a remarkable aptitude for teaching and—in the assessment of a later colleague—experienced an awakening of "genuine scientific understanding." His master of science degree was awarded in 1909, and his thesis, "A Study of the Wehnelt Electrolytic Interrupter," was published in the *Physical Review* the following year, the first of hundreds of publications.

The year 1909 brought an important decision with re spect to his future. Karl received an offer to head the science department of a missionary college in Korea—exactly the kind of life work Elias hoped his eldest son would choose to follow. But misgivings about his readiness for such a responsibility were reinforced by his father, who counseled that the best preparation for a teaching career would be further graduate work at a great university with superb library and laboratory facilities. Karl decided upon Princeton, attracted by its new Palmer Physical Laboratory and the presence of two physicists from Cambridge University: Owen W. Richardson and James H. Jeans. Still, he was forced to postpone his enrollment there until he could amass sufficient funds.

An appointment as instructor of chemistry at Wooster for 1909-10 was welcome—not only for the money it would provide, but also for the opportunity to continue to play baseball with a local "bush league" team. During that year, however, Karl rapidly increased his reputation as an outstanding teacher, a reputation he would maintain for many years to come.

Entering Princeton in 1910, Karl Compton was appointed to a half-time teaching assistantship and in 1911 was awarded the Porter Ogden Jacobus Fellowship, the most prized fellowship at the university, which recognizes the highest scholastic achievement in the graduate school. Because of O. W. Richardson's interest in experimental and laboratory work, Compton chose to work with him to complete his doctorate and through him was led to a deepening interest in electron theory. His thesis dealt with electrons liberated by ultraviolet light. It was followed by several papers—authored jointly with Richardson—on the photoelastic effect, as well as several experiments testing a theory that would later bring Richardson the Nobel Prize. Compton received his Ph.D., summa cum laude, in 1912, and he had already accepted an appointment to teach physics beginning in 1913 at Reed College, a new undergraduate institution in Portland, Oregon. A one-year postdoctoral appointment at Princeton allowed him to remain there to carry on the research undertaken with Richardson, which led to further publications on the photoelastic effect. During that year Karl's brother Wilson came to Princeton to take a doctorate in economics.

In June 1913, Karl Compton married Rowena Rayman, whom he had met at Wooster during his freshman year and to whom he had been engaged since 1908. They took up residence on the Reed campus, and so began his active professional life.

At Reed, Compton's title was instructor, yet he was solely responsible for the instruction in physics and worked hard to build up the department and its laboratory facilities. He inspired his students with an interest in research, collected apparatus, carried on experiments, and published a number of papers in the *Physical Review*. He looked upon these years as wonderful experience but was happy to return to Princeton in 1915 as assistant professor of physics.

PRINCETON YEARS (1915-30)

Karl's brother Wilson had received his Ph.D. from Princeton in June 1915 and was teaching at Dartmouth, but Arthur, who had begun work for a master's in physics in the fall of 1913, was still there, a year away from completing his doctorate. They enjoyed working together on research and, during this period, developed a device known as the Compton Electrometer. At this time also, Karl declined an offer to join the General Electric Company as a research physicist, while agreeing to act as a consultant under an arrangement that continued for many years.

Anxious to do his part in World War I, Karl began working on projects at Princeton, the Thomas A. Edison Labo ratories, and the Signal Corps in Washington, D.C. Associated with the Research Information Service in December 1917, he was sent to Paris and assigned to the American Embassy as an associate scientific attaché. Beyond the technical work involved, this experience afforded an opportunity to come in contact with many important scientists and engineers with whom he developed life-long relationships. Above all, he came to understand the urgent need in time of war for what he later called "combat scientists"—though he could hardly have foreseen the major role he would play in World War II.

Following the Armistice of November 1918, Karl Compton returned home to his wife and three-year-old daughter, Mary Evelyn, and to Princeton, to which he had become deeply attached. Sadly, Rowena died in the fall of 1919.

In June of that year, at the age of thirty-one, Compton's outstanding qualities as a teacher and experimental physicist were rewarded by promotion to full professor. His gift for teaching was to become almost legendary at Princeton, a gift marked not alone by the clarity of his presentation and a contagious enthusiasm, but also by his manifest concern for the well-being and progress of each student. Yet this dedication to teaching in no way detracted from his interest and devotion to research. He was soon recognized as the most distinguished member of the Palmer Laboratory, and graduate students came in increasing numbers to work under his direction.

Broadly, his field included electronics and spectroscopy, his research ranging over such subjects as the passage of photoelectrons through metals, ionization and the motion of electrons in gases, the phenomena of fluorescence, the theory of the electric arc, absorption and emission spectra in mercury vapor, and collisions of electrons and atoms. Over a hundred papers appeared in various scientific jour nals throughout the Princeton years, constituting an impressive testimonial to his vital energy and imagination and his generosity in sharing both work and credit with students and colleagues.

His reputation grew both nationally and internationally, and in 1927 he was named director of research at the Palmer Laboratory and appointed to the Cyrus Fogg Brackett Professorship. This new chair enabled him to concentrate on graduate work in the department, of which he was named chairman in 1929. He looked forward to years of teaching and research, determined to make Princeton's physics department the best in the world.

Then an invitation came that would profoundly alter the course of his life. Early in 1930, to his utter surprise—the greatest surprise, he once said, of his life—he was asked to become president of the Massachusetts Institute of Technology.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY (1930-54)

Compton was well acquainted with MIT's reputation as a distinguished school of engineering—an institution whose graduates had made enormous contributions to the progress of American industry and technology for nearly three-quarters of a century. But, with a few exceptions, MIT had (to use Compton's words) cut relatively little figure in scientific circles.

But enormous advances in science were rapidly transforming existing engineering practice and introducing a whole new range of future opportunities. An engineering education that focused largely on the techniques and procedures of current industrial practice was no longer adequate. Science lay at the heart of modern engineering, and MIT's scientific curriculum needed strengthening to prepare the school's students for the broadening horizons looming ahead. In those days the Institute was experienc ing a period of intellectual unrest—an old-fashioned confrontation between conservative forces rooted in the past and young rebels bent on change. I was one of those young rebels in the Department of Physics, and we were fortunate to have the support of many senior faculty members who shared our views and reinforced our beliefs.

What we were witnessing, in fact, was the end of an era. We were awakening to a whole new world of science—science in its fundamental sense, which was almost totally missing from the Institute of that time—and to a new awareness of how this modern science might transform engineering of the future. There were signs that things were beginning to stir. New facilities for chemistry were already in the planning stage, distinguished physicists from abroad had been invited to spend short periods in Cambridge, and younger members of the faculty were returning from graduate work in Europe. But progress was frustratingly slow.

To the members of the MIT Corporation, Karl Compton was the perfect choice to lead the Institute to a bright future. Never having thought of assuming the presidency of any institution, he was loath to leave Princeton, his students, and his research. In the end, however, he was inspired by the challenge, as he explained in a letter to the editor of *The Daily Princetonian:* "the magnitude of this opportunity to help science 'make good' in engineering education creates an obligation which transcends other considerations."

He took office in July 1930 in a time of great need and—unhappily—adversity, for his arrival coincided not only with the onset of the Depression with all its financial nightmares, but also with a period when science and the applications of science were under attack, viewed by many as the cause of social ills and national despair. Compton would prove to be a courageous spokesman in defense of science and technology, and despite terrible problems of day-by-day funding, his contributions to the strengthening of basic science, to the quality of an Institute education, and to the enhancement of its international reputation will never be forgotten.

The Institute, of course, did not change character overnight. But everywhere, after Compton's arrival, there was a change of spirit, and it was clear that a new road lay ahead. By the end of his first five years in office, he had given MIT both an administrative and an educational structure, established clear lines of communication between faculty and administration, encouraged research and graduate study, supported curricular revisions, and established a graduate school. Compton was deeply conscious that the basic sciences and the spirit and methods of scientific research must find representation in the education of those who would contribute most heavily to the technological advances of the future. He also understood that if MIT's scientific departments were to make meaningful contributions, science for its own sake must have a legitimate place in the curriculum.

In 1921 Karl Compton had married again, and he and the former Margaret Hutchinson, a graduate of the University of Minnesota, won the hearts of students, faculty, and alumni through their friendliness, good will, and genuine concern for others. During his years as president, student amenities on campus were greatly improved, and (as one might have imagined) the athletic program received his wholehearted support.

Everything he set out to do as president he measured against the charter of the institution under his care. He responded with an enthusiastic "Yes!" in reaffirming the principles of that charter, and "its truly great idea of public service." His views on MIT's obligation for public ser vice not only led to the Institute's extensive involvement in the government's scientific effort during World War II, but also helped create a model of cooperation between university and government that was emulated all over the country.

Under Compton's hand MIT underwent a revolutionary transformation, both of its intellectual temper and in the definition of its academic horizons. In this process he developed guidelines for a new approach to education in science and engineering, so that his influence extended far beyond the confines of a single institution.

Compton's active role in the Society for the Promotion of Engineering Education (now the American Society for Engineering Education), of which he was president in 1938, was, therefore, of special significance. As chairman of the Committee on Engineering Schools of the Engineers' Council for Professional Development he led the way in setting standards for the accreditation of engineering curricula. He believed that education, and particularly scientific and technical education, should be broadly based and responsive to the needs of the times, and that science should be put to work and could contribute significantly to industrial progress.

Despite his seemingly total immersion in teaching and research, Karl Compton—from the very outset of his career—took an active and constructive part in many of the affairs of the larger scientific community. In 1923 he was elected a member of the American Philosophical Society. The following year he became a member of the National Academy of Sciences, for which he served as chairman of the Section of Physics from 1927 to 1930. In 1925 the American Physical Society named him vice president, and two years later he succeeded to the presidency.

In the early 1930s, Compton joined with other leaders of the APS to organize the American Institute of Physics—a major achievement. Karl Compton guided this body—designed to bring together in federation a number of disparate societies relating to developing fields in physics, to serve as a spokesman for physics in relation to the general public, and to sponsor a program of publication for the dissemination of a burgeoning body of research results in the field—first as chairman of the board, from its inception in 1931 to 1936, and in succeeding years as a wise counselor. To this task he brought his characteristic energy, tact, vision, and wisdom. In his honor the Institute established, in 1957, the Karl Taylor Compton Medal Award "for distinguished service in the advancement of physics."

In 1935, Compton also served as president of the American Association for the Advancement of Science. He was a fellow of the Optical Society of America and a member of the American Chemical Society, the Franklin Institute, and several professional engineering societies.

WORLD WAR II: COOPERATION WITH THE MILITARY

In 1933 President Roosevelt asked Karl Compton to chair a new Scientific Advisory Board. Its creation was not accompanied by a clear mandate from the government, however, and it was discontinued two years later. But with the advent of World War II, he once more moved to the forefront of scientists who saw the need for reliable scientific advice at the highest level of government. When the National Defense Research Committee was created in 1940, under the chairmanship of Vannevar Bush, Compton was appointed a member and chief of Division D, which comprised those academic and industrial engineers and scientists responsible for detection—chiefly radar, fire control, instruments, and heat radiation. In 1941, the NDRC became part of a new Office of Scientific Research and Development, also headed by Bush, with Compton in charge of those divisions concerned with radar, radar countermeasures, guided missiles, optics, and physics.

Compton chaired the United States Radar Mission to the United Kingdom in 1943 as well as the committee that received a similar British mission to the United States in the following year. From 1943 to 1945 he was Chief of the Office of Field Service of OSRD, and in 1945 he was scientific adviser to General MacArthur and head of the Pacific Branch of the OSRD in Manila. With the official Japanese surrender on September 1, 1945, Compton left Manila for Japan as a member of a Scientific Intelligence Mission.

Throughout these busy years, he was called upon for committees and advisory boards on a variety of subjects—production of synthetic rubber, military training, weapons, and chemical warfare—and served on the Secretary of War's Special Advisory Committee on the Atomic Bomb.

Nor did demands from Washington cease when the war was over. President Truman appointed Compton to a committee on the atomic bomb test, and he was chairman of the Joint Chiefs of Staff's Evaluation Board for such tests. In 1946 he became chairman of the President's Advisory Commission on Military Training and from 1946 to 1948 was a member of the Naval Research Advisory Committee.

Despite continued demands from Washington, Compton managed to turn his attention once again to the affairs of MIT, whose needs had been profoundly affected by World War II. He also found time to serve as a trustee of the Ford, Rockefeller, and Sloan Foundations, the Sloan-Kettering Institute, the Brookings Institution, and Princeton University, to name but a few of a long list of philanthropic and government activities dating back to the thirties.

In 1948, answering still another call from Washington, he agreed to succeed Vannevar Bush as chairman of the Joint Research and Development Board, an agency designed to oversee military scientific research efforts in the postwar period. But a year later, for reasons of health, he was forced to relinquish the post. Having resigned the presidency of MIT and been elected chairman of its Corporation in 1948, he returned to Cambridge in November 1949.

HONORS AND AWARDS

The many awards that rightfully came to Karl Compton throughout his life honored all that he did for his country and his countrymen and recognized long and faithful service in the cause of science, engineering, and education. In awarding him the Marcellus Hartley Medal[1] in 1947, the Academy cited his "eminence in the application of science to the public welfare," both for his contributions—as a physicist and an administrator—to the nation's "wartime research effort . . . and in the reinforcing of collaboration and understanding between civilian scientists and military men."

In 1946 he received the highest civilian honor of the U.S. Army, the Medal for Merit, for personally "hastening the termination of hostilities," particularly by means of the radar research and development program he directed. In 1948 he was named Honorary Commander, Civil Division, of the Most Excellent Order of the British Empire, and Knight Commander of the Norwegian Order of St. Olaf. He was promoted to Officer in the French Legion of Honor in 1951.

The Washington Award of the Western Society of Engineers came in 1947, the Lamme Medal of the American Society for Engineering Education in 1949, and in 1950, the Hoover Medal—a joint award of the American Institute of Electrical Engineers, American Society of Mechanical Engineers, American Institute of Mining and Metallurgical Engineers, and American Society of Civil Engineers.

His contributions as a physicist were especially recognized by the Rumford Gold and Silver Medals of the American Academy of Arts and Sciences in 1931, the William Proctor Prize for Scientific Achievement of the Scientific Research Society of America in 1950, and the Priestley Memorial Award of Dickinson College in 1954 for his contributions to "the welfare of mankind through physics."

Compton was also the holder of thirty-two honorary degrees, the one from Princeton (in 1930) aptly stating that "he merits honors the more because he counts them less than the satisfaction of work well done."

IN CONCLUSION

In New York for a meeting in June 1954, Compton suffered a heart attack. Six days later, on June 22, a massive blood clot ended his life. Three children survive: Mary Evelyn (Mrs. Bissell Alderman) from his marriage to Rowena Rayman; and—from his marriage to Margaret Hutchinson, who died in 1980—Jean (Mrs. Carroll W. Boyce) and Charles Arthur. There are also several grandchildren and great-grandchildren.

A figure of great dignity and tremendous strength of character, Karl Compton made an enormous contribution to the intellectual and scientific development of our country in one of its most critical periods. His stature stems from his visionary ideas on science and education and his response to the great currents of thought that were stirring men's minds in his day.

To grasp the full range and depth of Compton's character, one must recognize how it molded the thinking and actions of those who shared his intellectual environment. A brilliant experimental physicist, an inspiring teacher, a great academic leader, a conscientious public servant, he was beyond all these a wise and dedicated statesman of science, vigorous in thought, deeply rooted in religious tradition, and utterly fearless in expressing what he believed to be true and right. To every effort he brought a full measure of extra strength—warmth of friendship and understanding, firmness of character, modesty, and effective administrative skill.

He has been described as a great American. He was a great, and responsible, human being as well.

NOTE

1. Today known as the National Academy of Sciences Public Welfare Medal, the Academy's highest honor, and the only award presented on behalf of the entire Academy membership.

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University of Michigan Information Services

Clyde H. Coombs

Clyde Hamilton Coombs

July 22, 1912-February 4, 1988

by Amos Tversky

Clyde Coombs will be remembered by his fellow psychologists and other social scientists for his seminal contributions to the analysis of qualitative measurement and multidimensional scaling, and for his innovative models of conflict and choice. Clyde will be remembered by his students and friends as an inspiring teacher and colleague, who stimulated and enriched the lives of those who were fortunate enough to know him. Clyde was endowed with enormous energy, genuine curiosity, and a deep commitment to research and testing. Research, for Clyde, was an exciting adventure in the realm of new ideas, and teaching provided him with an opportunity to share ideas with his students and to convey his contagious enthusiasm, as well as his personal warmth and unfailing sense of humor. Although Clyde was primarily a theoretician who developed mathematical structures for describing cognitive processes, he was also a gifted experimentalist who introduced several elegant and innovative designs and an ingenious data analyst who contributed some powerful and parsimonious methods for the analysis of psychological data. Indeed, much of Coombs' work may be characterized as an attempt to discover and articulate the formal structures that are hidden in psychological data.

Coombs' major contribution consists of his creative analysis of the qualitative structures that arise from similarity and preference data, and the insightful applications of this analysis to many psychological problems. In an early paper entitled "A Theory of Psychological Scaling" that appeared in 1952, Coombs developed his central ideas about qualitative measurement that served as the basis for the ideal point model and the unfolding technique. Coombs realized, as did other psychometricians, that the measurement of psychological attributes cannot be carried out using the standard logic of physical measurement that is based, in one form or another, on the concatenation of objects and is carried out by the counting of units.

How, then, can we quantify psychological attributes, such as similarity or preference? The traditional psychometric approach, applied by Spearman and Thurstone to the measurement of intellectual abilities, employs factor analysis and similar statistical techniques to construct abstract dimensions from observed intercorrelations among variables, such as test scores. An alternative approach, developed by Thurstone for the measurement of attitude and preference, posits an underlying probabilistic process whose location parameters, estimated from choice probabilities, are interpreted as psychological scale values. Coombs was not fully satisfied with either the correlational or the probabilistic approach to psychological measurement because they introduced strong, and largely unverifiable, assumptions. Instead, he approached the problem of psychological measurement from a purely ordinal perspective that does not presuppose a numerical structure. In this respect, Coombs' work pioneered the development of multidimensional scaling and axiomatic measurement theory, while Thurstone's work was the precursor of the modern theory of signal detection.

Coombs began his analysis with a classification of empirical relational structures based on (i) whether the observed relation is binary (e.g., *x* is brighter than *y*) or quaternary (e.g., *w* and *x* are closer than *y* and *z*), and (ii) whether the data refer to objects from one set (e.g., stimuli) or to objects from two distinct sets (e.g., individuals and stimuli). The two dichotomies yielded four kinds of structures that Coombs called preferential choice data, single stimulus data, stimulus comparison data, and similarities data. This classification scheme was used by Coombs to exhibit the distinctive features of different data structures that can nevertheless be embedded within a unified system.

The central elements in Coombs' system are the ideal point model for similarity and preference, the ordered metric scale derived from these data, and the unfolding technique for constructing this scale. Coombs' system, therefore, has three major components: a psychological model of choice, a measurement structure implied by the model, and a scaling technique for recovering the underlying structure. In the ideal point model, both individuals and stimuli are represented as points in some multidimensional space. The dimensionality of the space depends on the nature of the stimuli: rectangles are two-dimensional; paintings can have many dimensions. Each individual is represented in the space by an ideal point that corresponds to the most preferred position of that individual. According to this model, a person prefers option *A* over option *B* if and only if *A* is closer to *B* to his or her ideal point. Thus, voters order political candidates, or consumers order products, by the distances from their ideal points. This model does not restrict the representation of the options, and it allows different individuals to have radically different preference orderings. The force of the model stems from the assump tion that all individuals and stimuli reside in a common space, which constrains the set of permissible preference orderings.

Coombs noted that the preference orders generated by different individuals convey a great deal of information about the structure of the common space—even when the number of alternatives is relatively small as is often the case in many applications. These data give rise to what Coombs called an order metric scale, which consists of a partial ordering of the distances between stimuli. This scale represents a new type in the classification of scales introduced by Stevens; it lies between the purely ordinal scale obtained by a simple ordering of stimulus points and the stronger interval scale that requires a complete ordering of all interpoint distances.

Coombs and his students applied this model to a wide variety of choice problems, varying from marketing to politics and from risk to psychophysics. The unidimensional case illustrates the power of the model. Consider, for example, a group of voters who rank-order each of several candidates for office, and suppose the candidates differ along a single left-right dimension. Because the ideal points of all the voters lie on the same left-right dimension, the permissible orderings of the candidates are constrained. Clearly, an individual whose ideal point is to the left of all candidates will order them from left to right, whereas an individual whose ideal point lies to the right of all candidates will order them from right to left. An individual whose ideal point lies near the middle of the scale, however, will prefer the candidates in the center to those who lie on either extreme. This model can accommodate many, but not all, preference orders. In particular, it prohibits any ordering in which both left and right candidates are preferred to those in the center. Such an ordering is excluded in the unidimensional case because there is no point on the line that is closer to both the left and the right than to the center. The preference orders that are compatible with the unidimensional ideal point model, therefore, are those generated by single-peaked utility functions over the left-right dimension. A single-peaked utility function is an ordinal scale of preference that reaches its maximum somewhere along the dimension and falls off monotonically in either direction. This notion, which generalizes the standard notion of a monotone utility function, played a central role in Coombs' research on choice and conflict. The significance of Coombs' analysis stems from the fact that it provides a method for discovering an underlying common dimension, if one exists, even when there is no *a priori* (e.g., left to right) ordering of the points. Thus, Coombs' model generalizes the psychometric notion of a unidimensional ensemble of items (e.g., test questions or attitude statements) developed by his longtime friend Louis Guttman.

In dealing with attributes such as money or pain, it is natural to assume that everyone will prefers more to less, or less to more. In such cases, the utility scale is a monotone function of the attribute in question. For such attributes, such as temperature, risk, or conservatism, we normally prefer intermediate levels over extreme ones, and different people prefer different levels. This state of affairs gives rise to the single-peaked preference functions, which Coombs investigated both theoretically and empirically. Many years later, Coombs discovered an explicit statement of this idea in an eighteenth-century book by Joseph Priestley, the discoverer of oxygen, who discussed the flow of pleasure and pain associated with variation in temperature. Although the significance of single-peaked preference functions has been recognized by several scholars since Priestley, Coombs made two distinct contributions in connection to this concept.

First, he analyzed the qualitative structure that lies behind this representation and showed how to recover the underlying dimension (e.g., right-left) from individuals' preference orders, when the ordering of the stimuli is not known in advance. He also showed that some of the metric properties of the dimension (i.e., the ordering of intervals) can also be inferred from the data, using the unfolding algorithm. Second, Coombs did not accept the presence of single-peaked preferences as a blind fact to be treated as a primitive concept. Instead, he attempted to derive it from more basic principles of hedonic experience. He proposed that nontrivial choices involve a conflict between the upside and the downside, or between the benefit and the cost. To understand the making of decisions, therefore, we must understand the principles that govern the resolution of this basic conflict. Coombs proposed two such principles: "One is that good things satiate and the other is that bad things escalate" (Coombs, 1983, p. 21). With George Avrunin, Coombs formalized these assumptions and showed how the satiation of good and the escalation of bad give rise to single-peaked preference functions. These assumptions also served as a basis for their systematic analysis of the various types of conflict (e.g., approach-approach, approach-avoidance), which was summarized in *The Structure of Conflict,* Coombs' last monograph, which was published after his death.

Coombs' contribution to the analysis of preference, however, is not limited to the investigation of its formal structure and its psychological underpinnings. Coombs applied these notions to a wide array of psychological problems, ranging from judgments of the severity of crimes to the pattern of citation in psychological journals, and from preferences concerning family composition to the problems of risk perception and risk preferences, to which he devoted much of his research.

Coombs did not view expected utility theory as an adequate descriptive model, and throughout his career he developed alternative accounts of risky choice, and tested them in many experiments. Coombs departed from the traditional analysis of risk in two important respects. First, he distinguished clearly between the perception and the preference for risk and explored their interrelations, using the newly developed methods of conjoint measurement. He investigated the parameters that control the perceived riskiness of options and concluded that it is determined primarily by the undesirable outcomes and their likelihood, rather than by the desirability of a risky prospect can be decomposed into two components: its expected actuarial value and the value of the risk it entails. Contrary to the classical assumption of risk aversion according to which all people minimize the risk component, Coombs argued that different individuals have different ideal levels of risk at the same level of expected value, and that people choose between gambles in order to achieve the level of risk they desire.

The ideal point model is very typical of Coombs' style and character. On the one hand, he was fiercely individualistic and committed to the idea that people are entitled to their own views, tastes, and beliefs. At the same time, he held strong beliefs and uncompromising views on scientific and personal conduct. The ideal point model reconciles the tension between individual freedom and global order. Individuals are allowed to have different ideal points and different preferences, yet they must all coexist in a common space that imposes some higher-order constraints, which are needed to ensure the coherence of the system.

BIOGRAPHY

Clyde Hamilton Coombs was born in New Jersey on July 22, 1912, but he spent most of his early life in California, where he developed a lifelong love of outdoor activities, particularly camping, tennis, and swimming. His first two college years were spent at Santa Barbara State, where he studied mathematics and engineering in preparation for a military career that he intended to pursue largely because his father, who died before Coombs was born, had been in the Navy. A course in psychology, however, opened up an exciting new world for Clyde and convinced him to change direction. Coombs stopped his college education for a year to read psychology and physiology, and continued his education at the University of California at Berkeley, where he majored in psychology. He was strongly influenced by Wagner Brown, Edward C. Tolman, Robert Tryon, and Nathan Shock. His preference for hard science led him to pursue courses in chemistry and biology, aiming to study psychological problems from a physiological perspective. He completed a master's thesis on adaptation of the galvanic skin response, and he planned to do a dissertation on olfaction.

The second event that changed Coombs' academic course was the appearance of L. L. Thurstone's *Vectors of the Mind,* which first introduced Coombs to the new field of mathematical psychology. One of his teachers at Berkeley, Robert Tryon, formed a small group that went through Thurstone's book chapter by chapter. Coombs was very impressed with the possibilities of using mathematical models to study psychological processes, and he wrote to Thurstone, who offered Coombs a research assistantship at the University of Chicago. In 1937 Coombs went to Chicago and began a new phase in his intellectual development. Thurstone had created in Chicago a stimulating intellectual environment, full of fervor and excitement. The weekly seminars in Thurstone's home generated intense discussions of basic scientific and methodological problems. Coombs followed Thurstone's example and, for more than thirty years, he ran a weekly seminar in his home in Ann Arbor that became a major forum for the discussion of new, exciting ideas in measurement, scaling, and decision research.

At Chicago Coombs studied mathematical biophysics with Nicholas Rashevsky, who provided him with a different perspective on mathematical modelling. Coombs also met a graduate student in demography, Lolagene Convis, who later became his wife. Clyde and Lolagene Coombs had two children, Steven and Douglas. As a graduate student, Coombs was asked to teach an elementary course in social psychology, a field with which he was not acquainted. Approaching this task with his characteristic enthusiasm, Clyde put an enormous amount of effort into preparing the course, which made him appreciate both the significance of social psychology and the difficulty of applying the scientific method to the complicated problem of interpersonal relations. Much of his work in the following years dealt with the attempt to use formal models and deductive reasoning to illuminate complex psychological processes. This is the theme of his 1983 book *Psychology and Mathematics.*

After receiving his Ph.D. in 1940, Coombs became a personnel research psychologist for the U.S. War Department. Over the next six years, he rose to the rank of major and designed a separation counseling program for demobilized G.I.s that won him the Legion of Merit. In 1947, Coombs returned to academic life, joining the psychology department of the University of Michigan in Ann Arbor under the inspired chairmanship of Donald Marquis, who encouraged a broad-based department and was very supportive of interdisciplinary approaches and innovative research. At the invitation of Samuel Stauffer, director of the Laboratory of Social Relations, Coombs spent the academic year 1948-49 with Paul Lazarfeld and Fred Mosteller at Harvard Univer sity, where he began to develop his "ideal point" model and unfolding technique.

Upon his return to Michigan in 1949, Coombs began developing a mathematical psychology program that became noted for the quality of its staff, research, and students. Coombs initiated an interdisciplinary seminar that attracted people from philosophy, mathematics, economics, political science, sociology, and psychology—all interested in the development of a mathematical social science. In the summer of 1952, Coombs and R. M. Thrall, of the mathematics department, received a grant from the Ford Foundation for a summer institute on interdisciplinary approaches to measurement and decision making. The result of the summer institute, held in Santa Monica, was a book edited by Thrall, Coombs, and Davis entitled *Decision Processes,* which played an important role in shaping the emerging field of behavioral decision research and mathematical psychology.

While Coombs was developing and refining his theoretical ideas, he also carried out an innovative experimental program. A particularly insightful study that revealed some of the stochastic characteristics of choice behavior was conducted while Coombs was a Fulbright fellow at the University of Amsterdam in 1955-56. This experiment, which involved choice between various shades of gray, demonstrated elegantly the need to incorporate ideal points (in this case concepts of a perfect gray) into the measurement of sensation. The next sabbatical year, spent at the Center for Advanced Study in the Behavioral Sciences in Palo Alto in 1960-61, gave Coombs the opportunity to put together many of the ideas he had developed over the years. The logical interrelations among the various data-collection procedures and data-analytic techniques were explored in detail in Coombs' major book on scaling, *A Theory of Data,* which was published in 1964. Back at Michigan, Clyde continued to teach and train a new generation of mathematical psychologists. He took enormous pride in his students and followed their intellectual development throughout the years. His lively and exciting course on mathematical psychology attracted many students, who convinced him to write an elementary graduate text based on this course. This book, coauthored with former students Robyn Dawes and Amos Tversky, appeared in 1970, and it has been translated into six foreign languages.

Lolagene Coombs' research on fertility problems and gender preferences in Asia provided Coombs with a new domain for his scaling methods. Working together, Lo and Clyde utilized conjoint measurement and unfolding theory to develop scales for measuring people's preferences for family size and gender composition. These scales, and the models from which they derived, were applied and tested in many countries. Their research showed that despite the large variations among cultures, people's preferences in all countries are better described in terms of ideal points for the number of children and for the balance between genders than in terms of specific ideal points for the number of boys and the number of girls.

Much of Coombs' work on choice was concerned, directly or indirectly, with the question of conflict. How do people reconcile incompatible goals and inconsistent objectives, and how do they trade off risks against benefits? Shortly before his retirement, Coombs turned his attention and energy to this fundamental problem. In collaboration with George Avrunin, he explored the origin of the single-peaked preference function that played such a central role in his work and analyzed the structure of both intrapersonal conflict and interpersonal conflict from this perspective. This work culminated in a book entitled *Structure* *of Conflict,* which brought together many of the major themes from Coombs' work. With an impeccable sense of timing, Coombs was able to complete the revisions of the manuscript and send it to the publisher a day before his unexpected death.

Coombs' retirement from the University of Michigan did not slow him down in the least. With his characteristic exuberance and zest for life, he continued to pursue his research on the structure of conflict and to teach at the universities of Hamburg, Calgary, and Santa Barbara. The flexible "retirement" schedule permitted him and Lo to travel to far places, such as the Galápagos and the Amazon, to play tennis, to go camping in Idaho, and to continue his activity in the French Wine Club that had amply supplied his table for many years. The Coombs' spent more time at their second homes in Vermont and Florida, but Clyde always carried his portable computer from one setting to another, and that allowed him to continue the work he enjoyed so much. Until the very last day, he remained an active outdoorsman, a witty conversationalist, and a gracious host.

Clyde was a creative scientist, an inspiring teacher, and a beloved friend. But perhaps his most precious gift was his ability to enjoy life—people, research, scenery, and art—and to make others' lives richer and more enjoyable. In a field populated by models, Clyde provided the best model of all: himself.

He will be remembered and missed.

HONORS AND RECOGNITIONS

For many years, Clyde Coombs chaired the Mathematical Psychology Program at the University of Michigan. He served on numerous review committees for the National Science Foundation, the National Institutes of Health, the Canadian Research Council, and the Deutsche Forschungs gemeinschaft. He was president of the Psychometric Society (1955-56) and of the Division of Measurement and Evaluation of the American Psychological Association (1958-59), and he was the first head of the Society for Mathematical Psychology (1977-78). He was an honorary fellow of the American Statistical Association (since 1959), and he was elected to the American Academy of Arts and Sciences (1977) and to the National Academy of Sciences (1982). He received an honorary doctorate from the University of Leiden, The Netherlands, in 1975, and was awarded the Distinguished Scientific Contribution Award of the American Psychological Association in 1985.

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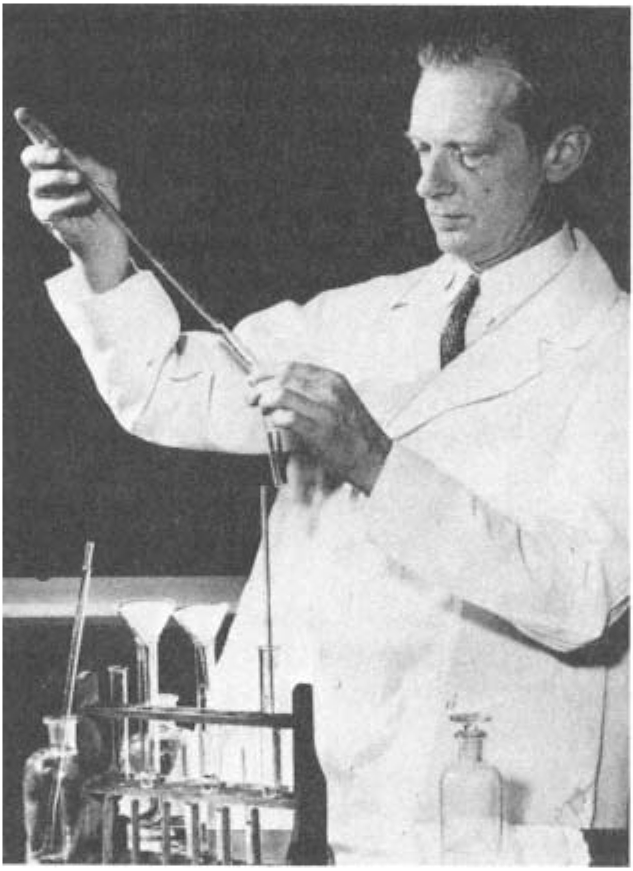


Archives, Washington University School of Medicine

Carl Ferdinand Cori

December 5, 1896-October 19, 1984

by Mildred Cohn

Unraveling the glycolytic and glycogenolytic pathways was a remarkable feat and a testament to the imagination and ingenuity of all those who participated. Carl and Gerty Cori contributed an essential part and in so doing were among the pioneers who showed that biochemical investigations of isolated enzyme systems could lead to an understanding of physiological processes. From their discovery of the first product of glycogen breakdown, glucose-1-phosphate, the Coris went on to isolate and crystallize the enzyme phosphorylase that catalyzed the reaction—the first of a class of reactions in which inorganic orthophosphate reacts to yield an organic phosphate ester.

Glycogen phosphorylase proved a treasure trove for biochemistry. By reversal of the phosphorylase reaction, it was shown that a macromolecule could be synthesized in a cell-free system. The enzyme was further found to exist in two interconvertible forms, though one was inactive in the absence of adenylic acid, which acted as an effector. The next generation of scientists trained in the Cori laboratory, using the Coris' fundamental discoveries, probed still further and discovered the two most widespread metabolic regulatory mechanisms: the cyclic AMP system and phos phorylation-dephosphorylation of enzymes accompanied by a cascade system of control.

EUROPE (1896-1922)

Carl Cori never regretted leaving Europe at the age of twenty-five to come to the United States and grew to be thoroughly at ease with the language, institutions, and customs of his chosen country. Yet he was, and would always be, a product of his cultured European background.

Carl Ferdinand Cori was born in Prague (then part of the Austro-Hungarian empire) on October 19, 1984. When he was two the family moved to Trieste, where his father took over as director of the Marine Biological Station. Carl spent his formative early years in Trieste, and his autobiographical essay, "The Call of Science" (1969), paints the picture of a happy childhood in a cultured and international milieu. In Trieste, Carl came into contact with a variety of ethnic groups. He was soon fluent in Italian, and the racial tolerance he developed there proved to be life long. Educated at the classical gymnasium from 1906 to 1914, furthermore, he obtained a grounding in Latin and Greek he would never forget.

Just as important, however, was the informal education he received at the Marine Biological Station. During field trips on the motorboat "Adria," his father, who captained the boat, lectured on the geology, botany, and early cultural history of the coastal region in addition to its oceanography and marine biology. Renowned for his own broad erudition in biology, archaeology, and history, the younger Cori attributed his abiding interest in them to his experiences on these field trips.

Summers with his extended family in the Austrian Tyrol rounded off Carl's education by giving him a love of mountaineering and music. His only rebellion, he records, was a certain penchant for practical jokes aimed at embarrassing his parents. His reference to a crystal of permanganate in the chamber pot, for example, caused consternation for a visiting maiden aunt with hypochondriac tendencies.

Carl's family boasted illustrious scientists and academics on both sides. Ferdinand Lippich, his maternal grandfather and professor of mathematical physics at the German University of Prague, had (in addition to making theoretical contributions to physics) developed the polarimeter as a precision instrument. Wilhelm Lippich, his great-grandfather, had been an anatomist at the University of Padua and a professor in Vienna. His uncle Friedrich Lippich was professor of chemistry in Prague, while his father, Carl I. Cori, was one of Europe's leading zoologists and marine biologists. Prominent scientists, furthermore, were frequent visitors to the house, and it is hardly surprising that young Carl chose to embark on a scientific career.

In 1914, at the age of seventeen, he entered the Carl Ferdinand University (the German university of Prague) to study medicine—at that time the customary route to a research career in the life sciences. He was fortunate to find there an intelligent and charming fellow student, Gerty Radnitz, who shared his interest in science and love of the outdoors. In 1916, while still medical students, they published their first joint research paper. Later, Gerty became Carl's wife and was, until her death in 1957, his dedicated scientific collaborator.

During World War I, in his third year at the university, Carl was drafted into the Austrian army. He was first stationed in a bacteriology laboratory, where—after a severe bout of typhoid fever he ascribed to his own carelessness—he taught himself a meticulous technique for handling pathogens. Later he served in a hospital for infectious diseases near the Italian front. Because of his knowledge of Italian, he was charged with caring for civilians as well as soldiers. While he could help some patients with the drugs then available, he found tuberculosis, malaria, pellagra, scurvy, and typhoid rampant in this poorly nourished population. His inability to help the victims of an influenza epidemic shocked him. This, along with the experience of a long and dangerous retreat amid a mass of undisciplined soldiers, made him skeptical about the practice of medicine and strongly averse to war ever after.

Cori returned to Prague in 1918, completed his clinical studies, and was awarded the M.D. degree. He and Gerty were married in Vienna, where both had gone to do postdoctoral work, in August 1920. He divided his laboratory work between the university's Pharmacology Institute and its internal medicine clinic but found his experiences in the latter so discouraging that he had no desire to continue in clinical medicine. The alternative—a career devoted entirely to research—was attractive both to him and to Gerty. Unfortunately, the postwar devastation in Austria made it highly unlikely that they would find paid positions; one could hardly get enough to eat. Gerty Cori, working at Karolinen Children's Hospital, developed symptoms of xerophthalmia from the inadequate diet provided for her there.

Fortunately, Carl Cori's research on the mechanism of seasonal variation of vagus action in the frog heart (1921) caught the attention of H. H. Meyer, who had just retired from the Pharmacology Institute. In the summer of 1921, Dr. Gaylord, director of the State Institute for the Study of Malignant Diseases (now the Roswell Park Memorial Institute) in Buffalo, New York, asked Meyer to suggest a biochemist for his institution. Meyer recommended Cori, who was interviewed for the position but was so certain that nothing would come of it that he accepted a position with

Otto Loewi in the Pharmacology Department of the University of Graz.

Cori stayed at Graz only six months, during which he learned a great deal. Loewi's enthusiasm and originality coupled with his wide knowledge of all aspects of biomedical science made him an invaluable mentor. It was during this intellectually stimulating period that Cori first thought of a way to study intestinal absorption, and the fate of sugar, in general, in the animal body. But other aspects of the Graz experience were decidedly unpleasant. To be eligible for employment at the university, Cori had had to prove his Aryan descent, and despite his scientific stature, Otto Loewi's future there was questionable. Living conditions and research facilities were woefully inadequate. When Gaylord offered Cori the position in Buffalo, therefore, both he and Gerty Cori welcomed the opportunity to leave Europe. Carl left early in 1922, and Gerty joined him six months later.

BUFFALO, NEW YORK (1922-31): STATE INSTITUTE FOR THE STUDY OF MALIGNANT DISEASES-ROSWELL PARK MEMORIAL INSTITUTE

In Buffalo, Cori initiated his life's work on carbohydrate metabolism and its regulation. Obliged to do routine laboratory tests for the hospital affiliated with the Institute, he used the opportunity to hone his analytical skills. His first paper on carbohydrate metabolism was published in 1922; in the ensuing decade, he published some eighty papers. After some initial problems in 1922-23 (1969,1), Gerty Cori, who had a position in the Institute's pathology department, was allowed to collaborate with Carl, and the majority of those eighty papers are joint publications. In the following discussion no attempt will be made to assess individual contributions because it was the work of two peers speak ing with one inseparable voice. As Carl Cori stated (1947,1): "Our efforts have been largely complementary; and one without the other would not have gone so far as in combination."

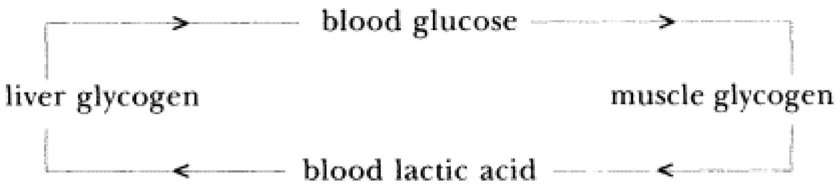
Regulation of Glucose Concentration in the Blood

The question the Coris wanted to answer was: What regulates blood glucose concentration? Their initial experiments were physiological, designed to determine the amount of sugar absorbed in the alimentary tract by measuring the unabsorbed sugar left in the gut of a rat after it had ingested a known amount (1925,5). This was followed by experiments with a new method devised to determine the glycogen content of liver and carcass separately.

But in order to assess changes in glycogen concentration after sugar ingestion or hormone administration, a control value had first to be established. It was found that the glycogen concentration was fairly constant in rats fasting for twenty-four to forty-eight hours. Higher and more uniform glycogen concentrations, furthermore, could be obtained by giving an amount of glucose that could be absorbed completely in three hours.

Having established conditions and analytical methods that were reproducible, it was then possible to do balance studies to determine the fate of the absorbed glucose in fasted rats and the influence insulin and epinephrine had on that fate. The researchers found that insulin increased oxidation of glucose and conversion to muscle glycogen but decreased conversion to liver glycogen (1926,1, 1928,1). Epinephrine, on the other hand, decreased muscle glycogen and increased liver glycogen (1928,2,3). Since it was known that muscle glycogen does not contribute glucose to blood, the Coris concluded that another intermediate must be formed from muscle glycogen and circulated through the blood to the liver to become the precursor of liver glycogen. Since it was known that lactic acid was formed when glycogen disappeared in muscle, the Coris postulated—and later demonstrated—that lactate was the intermediate in the "cycle of carbohydrates" (1928,2,3).

The "cycle of carbohydrates," which the researchers represented in the diagram given below (1929,1):



came to be known as the "Cori cycle." This scheme derived from research on the circulation of carbohydrate material in the intact animal was a milestone in the elucidation of carbohydrate metabolism and, along with other insights on blood homeostasis, helped clarify the action of epinephrine on blood glucose. Yet equally important were the reliable—and reproducible—techniques developed by the Coris to detect and quantify even small changes in liver glycogen, on which their research depended.

Influential Publications

During his stay at the Institute in Buffalo, Carl Cori also published a number of papers dealing with cancer. In 1923, Otto Warburg found that tumors display higher aerobic and anaerobic glycolysis than normal tissue. But Warburg's studies were carried out in vitro, and the Coris were the first to demonstrate that tumors in intact animals also showed abnormally high formation of lactic acid from glucose (1925,4).

In 1931, Cori wrote a masterly review on mammalian carbohydrate metabolism (1931,4). With more than 100 pages and some 500 cited references, the review included a critical evaluation of the literature, a summary of the pioneering work Carl and Gerty Cori had accomplished, and an indication of the direction of their future research on the mechanism of glycogenolysis. It established Carl Cori as a leader in the field and influenced the study of carbohydrate metabolism for many years to come.

WASHINGTON UNIVERSITY SCHOOL OF MEDICINE, ST. LOUIS (1931-45)

In 1931, Carl and Gerty Cori moved to St. Louis, where Carl became the chairman of the Pharmacology Department at Washington University and Gerty was given a position in the department, albeit with a token salary. In addition to his research, however, Cori was now expected to organize a department, equip a research laboratory, and spend considerable time teaching medical students. Understandably, there was a hiatus in the publication of papers.

During the last phase of the Buffalo period, however, the Coris had started working with isolated muscle preparations rather than intact animals. From balance-studies with epinephrine, they had concluded that the formation of a precursor of lactate from glycogen that accumulates in muscle is accompanied by the disappearance of inorganic phosphate (1930,5). They had developed a method, furthermore, for detecting hexose monophosphate simultaneously as both hexose and phosphate (1931,3). These experiments were a prelude to their important discovery of glucose-1-phosphate, for it was in St. Louis that their research progressed in the direction of biochemistry.

Phosphorylase—The Cori Ester

To study the concomitant disappearance of glycogen and phosphate, the Coris decided to use minced skeletal muscle from frogs. When the water extracted, muscle dispersions were incubated anaerobically in phosphate buffer, and they observed that—unless boiled muscle extract were added—only small amounts of hexose monophosphate were produced. In the first striking result from these experiments, they identified 5'-adenylic acid as an obligatory activator of the reaction (1936,4; 1938,1)—a finding that foreshadowed the idea that the allosteric activation of appropriate enzymes regulates metabolic processes.

At the 1935 International Physiological Congress in Moscow, the Coris presented their results on the formation of hexose monophosphate from the reaction of glycogen with stoichiometric amounts of phosphate in rat and frog muscle. Subsequently investigating the reaction in muscle extracts on his own, J. K. Parnas confirmed that, simultaneous with the cleavage of glycogen, phosphate disappears—a reaction he characterized with the term "phosphorolysis."

Because there was a large discrepancy in their analytical results for hexose monophosphate between phosphate- and reducing-power determinations, the Coris postulated the formation of a hitherto unknown phosphorylated, nonreducing precursor of hexose-6-phosphate. Having isolated this product as a crystalline brucine salt, they tentatively assigned it the structure of glucose-1-phosphate. They adduced, together with S. P. Colowick, final proof of this structure by synthesizing α-glucose-1-phosphate (1937,2), and it subsequently came to be known as the "Cori ester."

Discovery of the Cori ester bore the hallmark of the Coris' meticulous approach to scientific problems. Their success depended first and foremost on reliable methods of analysis whose results could be quantified—as seen in their analysis of phosphorylated hexose. They took particular care, furthermore, with the design of their experiments—as exemplified by the series that led to their identification of the activator 5-AMP. Add to this the element of luck, for the muscle dispersions the Coris' chose as a medium allowed glucose-1-phosphate to accumulate. In intact muscle, the presence of Mg^{2+} causes the enzyme phosphoglucomutase (1938,3) to convert glucose-1-phosphate to glucose-6-phosphate, so that its concentration is nineteen times that of glucose-1-phosphate at equilibrium. But happily for the Cori experiments, in their method of muscle preparation, much of the Mg^{2+} was "washed out." They suggested that, contrary to earlier notions, blood glucose in the liver is regulated by the sequential action of three enzymes: glycogen phosphorylase, phosphoglucomutase, and glucose-6-phosphatase—a sequence confirmed by their later work (1939,1).

The Coris named their new-found enzyme, which catalyzed the formation of glucose-1-phosphate from glycogen and inorganic phosphate, "phosphorylase," consistent with Parnas's term "phosphorolysis" for the reaction. This new intermediate in glycogenolysis, furthermore, was novel in two ways. First, it was the first known example of the esterification with inorganic orthophosphate at the reducing group of a hexose. It was also evidence of a phosphorolytic cleavage of glycogen by the enzyme.

Their 1936 discovery launched the Coris full-fledged into enzymology. Seeking to synthesize glycogen, they reversed the phosphorylase reaction, adding a small amount of glycogen when they observed a lag in its production (1939,3). This established the need of a primer in polysaccharide synthesis and produced as a product a large, starch-like polysaccharide (1940,2). For the first time, researchers working with a cell-free preparation had synthesized a macromolecule—an exciting demonstration that contradicted the long-held notion that the biosynthesis of macromolecules required energy-metabolism and could, therefore, occur only in intact cells.[1]

It would be difficult to overestimate the conceptual impact of the Coris' biosynthesis of glycogen. Because of their success, subsequent investigators used biochemical approaches to investigate how macromolecules are synthesized, approaches that served them well.

Polysaccharides and Phosphorylase a and b

In 1942-43, the Coris collaborated with Arda Green to publish a series of brilliant papers laying the groundwork in two important areas: 1) the structure, biosynthesis, and characterization of polysaccharides; and 2) the characterization of phosphorylase *a* and *b* and their role in metabolic regulation.

The Coris' purification and crystallization of muscle phosphorylase (1942,1) made large quantities of the polysaccharide it produced available for study, enabling them to investigate the synthesis and chemical structure of the product formed from glucose-1-phosphate. The polysaccharide formed in the phosphorylase reaction was not glycogen (a branched polysaccharide) but rather an unbranched polysaccharide similar to amylase. The necessity of adding liver or heart muscle extract to phosphorylase to induce the formation of glycogen led the Coris—in an ingenious series of experiments—to infer the existence of another enzyme, in addition to phosphorylase, in the formation of branched polysaccharides (1943,2).

In this same period they were investigating how phosphorylase activity was regulated in some detail. Describing, with Arda Green, two forms of muscle phosphorylase, *a* and *b,* they found that the *b* form was active only in the presence of 5'-AMP (1943,1). The formation of *b* from *a,* furthermore, was demonstrated to be enzyme-catalyzed. The researchers named this PR (prosthetic-group removing) enzyme, but 5'-AMP—the presumptive prosthetic group—

was not found in phosphorylase *a.* Establishing that the *a* form contained four times as much phosphate as the *b* form, the Coris postulated, with their usual astuteness, that interconversion plays a significant role in metabolic regulation, since the inactive form is found in resting muscle, while the active form is found in contracting muscle (1974,1).

WASHINGTON UNIVERSITY, ST. LOUIS (1945-66)

With the end of World War II, Carl Cori left the Pharmacology Department to become chairman of the Biochemistry Department, and scientists from all over the world flocked to St. Louis to work with the Coris. From 1946 to 1960 I, too, was privileged to be in Carl Cori's department. Both Coris trained young scientists, and they were consistently supportive, treating us with respect, offering encouragement, and expressing appreciation.

Though we were somewhat in awe of the breadth and depth of Carl Cori's knowledge, our daily departmental luncheons in the library were much enlivened by his wit. One of those rare individuals to whom all products of the human intellect are accessible, he was equally at home discussing archeology, music, or botany. He spoke only when he had something to say, was both logical and precise, and was, therefore, always listened to with great respect. Cori was intellectually and personally so compelling that even occasional contact with him left a tremendous impression. His direct, unornamented approach to the pursuit of his scientific objectives could make him seem aloof, even austere, but he was never solemn, and his high spirits often gave rise to a wonderful gaiety.

Cori's linguistic ability was legendary, and his scientific writing was exemplary in its economy and rigor. His psychological insight was uncanny. On one occasion, after an hour with a prospective graduate student, he discussed the student with me and predicted the kind of scientific career the student would have. Twenty years later, his prediction had been completely confirmed.

Cori's philosophy regarding the way to run a small department was to gather a group in which everyone would work on different aspects of the same general subject—in the case of his Biochemistry Department, carbohydrate metabolism. Some might be interested in the physical properties of enzymes or in their detailed mechanisms of action, others might be interested in more physiological effects of hormonal regulation, but all would benefit from interaction with each other. Under Cori, this system proved particularly effective, but whether it would have been so under anyone else's leadership is questionable.

The Coris lived with their son, Tom, born in 1936, in a pleasant house of modern design in a suburb of St. Louis. There they particularly indulged in gardening, a hobby Carl Cori pursued actively throughout his life. They extended hospitality to members of the department and visiting scientists and to their many non-scientist friends, who included sociologists, artists, and musicians. In the Cori home, the welcome was always warm and the conversation animated and intellectually stimulating.

In 1947, the Coris were awarded the Nobel Prize for Physiology or Medicine, which they shared with Dr. Bernardo Houssay of Argentina. Dr. Houssay was cited "for his discovery of the importance of the anterior pituitary hormone for the metabolism of sugar," while Carl and Gerty Cori were cited "for their discovery of the catalytic conversion of glycogen." The announcement of the award engendered great excitement and joy in the laboratory and among their friends everywhere. Gerty Cori was the first American woman to receive the Nobel Prize and the third from anywhere in the world, having been preceded only by Marie Curie and

Irène Joliot Curie. But 1947 was also the year that brought the devastating news that Gerty Cori was suffering from an incurable form of anemia, the disease that led to her death a decade later. Gerty's zeal was undaunted, and, if possible, her dedication to science was even more inspiring than it had been before her illness.

In 1956, their associates over the years at Washington University published a tribute to the Coris in a special issue of *Biochimica Biophysica Acta* entitled *Enzymes and Metabolism: A Collection of Papers Dedicated to Carl F. and Gerty T. Cori on the Occasion of Their 60th Birthday.* Contributors included five future Nobel laureates—C. de Duve, A. Kornberg, L. F. Leloir, S. Ochoa, and E. W. Sutherland—and the volume included many important and elegant papers. Several treated subjects particularly close to the Coris' interest, including Sutherland et al.'s treatment of phosphorylase *a* and *b* conversion; Krebs and Fischer's paper on the conversion of phosphorylase *b* to *a;* and Ochoa, Grunberg-Manago, and Ortiz's paper on a new enzyme, polynucleotide phosphorylase.

In addition to the 1947 Nobel Prize, Carl Cori received many other awards and honors. Among them were the Lasker Award of the American Public Health Association; the Squibb Award of the American Society for Endocrinology, which he shared with Gerty Cori; and the Willard Gibbs Medal of the American Chemical Society. He received honorary degrees from many universities, including Cambridge (England), Granada (Spain), Monash (Australia), and Trieste (Italy).

Cori was also elected to many prestigious intellectual academies and societies, including the American Academy of Arts and Sciences, the National Academy of Sciences (1940), and the American Philosophical Society. He was also a member of many foreign academies and societies, including the

Royal Society (London), the National Academy of Medicine (France), and the Royal Danish Academy of Sciences.

Glycogen Structure and the Regulation of Carbohydrate Metabolism

Because his research focused on enzymes from 1945 on, it was fitting that Cori head a department of biochemistry. But to him enzymes were always a means to understand the metabolic pathways in the cell. In his research he first attempted to isolate and characterize the mechanism of action of enzymes during glycogenolysis and glycolysis, then sought to understand the regulation of carbohydrate metabolism by hormones. Together with M. W. Slein, the Coris isolated and crystallized glyceraldehyde-3-phosphate dehydrogenase from rabbit muscle (1945,3); tightly bound NAD was found in the crystalline enzyme (1948,4). (J. Harting and S. Velick later elucidated this enzyme's mechanism of action, an example of substrate-level oxidative phosphorylation.) Next, with L. Berger, M. W. Slein, and S. P. Colowick, they isolated, from yeast, pure hexokinase, the enzyme that catalyzes the formation of glucose-6-phosphate from glucose and ATP, the first step in glucose utilization (1946,3). In 1948, Gerty Cori, J. F. Taylor, and A. A. Green crystallized aldolase. A year later, Carl Cori and V. Najjar purified phosphoglucomutase, which converts glucose-l-phosphate to glucose-6-phosphate, and investigated its mechanism of action (1949,1,3).

In 1951, Gerty Cori and J. Larner described a new enzyme they called "the debrancher." This was an amylo-1,6 glucosidase, and it catalyzed the hydrolysis of the 1,6 glucosyl bonds at the branch points in glycogen. Now—with the combined activities of phosphorylase and the debrancher enzyme—glycogen could be almost completely degraded in the presence of phosphate to produce glucose-1-phos phate from the α-1-4 linkages, and glucose from the α-1-6 linkages, respectively. It was possible, consequently, to decipher the structure of branched polysaccharides. As degradation with both enzymes proceeded and fewer branch points remained, glycogen was found to produce a decreasing amount of glucose, suggesting a tree-like, branched structure as a model for glycogen structure.

Hand in hand with the Coris' studies of the properties of the enzymes, they continued to research the problem of metabolic regulation. After demonstrating with Sutherland that insulin preparations brought about glycogenolysis in liver slices, Cori, Sutherland, Haynes, and Olsen isolated the impurity in the insulin preparations, which proved to be glucagon, the glycogenolytic factor in insulin preparations (1949,3). Cori and Sutherland subsequently showed that glucagon and epinephrine increase the rate of conversion of phosphorylase b to *a,* thereby controlling the rate of glycogenolysis in liver (1951,2). In the mid-fifties, Sutherland (with Wisolait) and Krebs (with Fischer), both trained in Cori's laboratory, proved that the conversion of phosphorylase *b* to *a* involved phosphorylation of the enzyme—Sutherland working with the liver enzyme and Krebs with the muscle enzyme. Sutherland's studies led to his discovery of cyclic AMP formation, while Krebs's work led to the elucidation of the complex cascade of regulating enzymes involved in the conversion of *b* to a.

The Action of Insulin

From his experiments with rat-muscle extracts, Cori concluded that insulin could increase glucose utilization by overcoming the inhibitory effects of adrenocortical fractions. He also suggested that insulin's first site of action was at the hexokinase-catalyzed reaction: glucose + ATP → glucose-6-phosphate + ADP by reversing the inhibitory ef fects of anterior pituitary and adrenocortical factors. This proved difficult to reproduce, however, and—after a good deal of research by a number of investigators—a consensus evolved that insulin acts first by affecting the permeability of muscle cells to sugars. This was confirmed some ten years later by Cori and Helmreich, who presented evidence that insulin increases the permeability of muscle to glucose.

Although Cori's attempt to establish the direct effect of insulin on the enzyme level was unsuccessful, the Coris' studies on phosphorylase, as well as later studies by Sutherland and others, validated the idea of explaining hormone action by proceeding experimentally from the whole cell to the pure, isolated enzyme. It should be noted that, to this day, the mechanism of insulin action is not completely understood.

Using intact diaphragms from rats, Cori and M. Krahl were able to show that glucose uptake in the diaphragms of diabetic rats was stimulated in the presence of insulin (1947,2). In later studies, they showed that insulin strongly affects the uptake of pentose (1957,2) and 2-deoxyglucose (1960). These in vivo studies demonstrated, furthermore, that hexokinase in diaphragm muscle from diabetic rats is inhibited.

Glycogen-storage Diseases

Throughout the fifties the Coris also investigated the nature of glycogen-storage diseases, and Carl Cori continued this research into the sixties. They were the first to pinpoint the defects in glycogen-storage diseases on the molecular level. Von Gierke's disease, for instance, is characterized by very low levels, or the complete absence, of the enzyme glucose-6-phosphatase in the liver (1952,2). This enzyme catalyzes the breakdown of glucose-6-phosphate to glucose, and, in its absence, glycogen cannot yield blood glucose. The Coris conducted further studies on glycogen storage disease in collaboration with B. Illingworth (1956,2), which were continued after Gerty Cori's death (1959,1, 1965,1).

THE LATER YEARS: BOSTON (1966-84)

At the end of the difficult decade of Gerty Cori's illness, which culminated in her death in 1957, Carl Cori was emotionally drained. Fortunately, this period was succeeded by a happy marriage in 1960 to Anne Fitzgerald-Jones, with whom he shared many interests in archeology, art, and literature. Carl Cori's wit and grace flourished in this last period of his life in the warm atmosphere of his and Anne Cori's home. After his retirement from the chairmanship of the Biochemistry Department of Washington University in 1966 at the age of 70, the couple moved to Boston.

Carl Cori left an indelible mark on Washington University by the example of his high standards and the outstanding productivity of his group. W. H. Danforth, a former Cori postdoctoral fellow who became chancellor of Washington University, acknowledged the strong influence Carl Cori, whose advice was often sought and heeded, on the university.

Enzyme Synthesis and Gene Expression

In 1966, Cori was appointed visiting professor of Biological Chemistry at Harvard Medical School and occupied a laboratory at Massachusetts General Hospital. He remained active in research until his final illness in 1983 and formed many deep friendships in Boston, as attested by the number of people who attended his memorial service held there.

Concerned about metabolic regulation for more than forty years, Carl Cori now struck out in a new direction—the study of the regulation of enzyme synthesis at the level of gene expression. This work was a collaborative effort with the eminent geneticist Salomé Glüecksohn-Waelsch of the Albert Einstein College of Medicine.

They investigated radiation-induced mutations in mice in which deletions in the chromosome region that included the albino locus served as a marker (1968,2). Because of the suppression of glucose-6-phosphatase, these deletions proved lethal in homozygous mutant mice (1968,2, 1969,2, 1970,1), since this liver enzyme (which the Coris had found to be deficient in von Gierke's glycogen-storage disease) is essential for the maintenance of blood sugar levels. The mutants manifested multiple biochemical defects (1973,1), including a deficiency of tyrosine aminotransferase and certain blood plasma proteins (1976,5). These multiple biochemical abnormalities, and the lack of gene-dosage effect in heterozygotes, suggested the involvement of other genes besides structural ones.

Yet, with many enzymes being synthesized normally, it was clear that this was not a defect in the general mechanism of protein synthesis. As it continued (1981,1, 1983), this research produced a striking result: unequivocal evidence that the structural genes for several of the missing enzymes were completely normal, but that regulatory genes were deleted. The DNA of the structural genes is encoded to specify the structure of the protein, but the regulatory gene determines the expression or non-expression of the structural gene. Furthermore, the Glüecksohn-Waelsch and Cori group's experiments demonstrated by experiments hybridizing mutant mouse liver cells and normal rat hepatoma cells that the regulatory genes for glucose-6-phosphatase and tyrosine aminotransferase were deleted from chromosome 7, and that structural genes are on other chromosomes.

This work broke new ground in demonstrating that both structural and regulatory genes are essential for the synthesis of individual proteins in a mammal, a phenomenon previously only known to occur in bacteria and yeast. It is impressive that Carl Cori in his ninth decade was making significant contributions to a problem at the frontier of molecular biology. His mind continued to absorb new knowledge and use it creatively to the end of his life.

Cori was often called upon to write review articles, and this did not cease during his years in Boston. He also wrote a considerable number of historical articles in this period, and philosophical treatises on the relation between science and the humanities. In addition to an autobiographical essay, he wrote articles about scientists he had known, including Francis Schmitt, Earl Sutherland, James Summer, Embden, and Gerty Cori. He ended his own memoir with:

> The frontiers of physics, astronomy and biology, and the instrument of their study, the human mind, fill one with wonder as to the great creations of art and architecture, past and present. From these, and from contact with nature, love and friends, spring the joy of living and the understanding of sorrow and of the human predicament. Humanism may be as important to mankind as competence in a particular field of science.

Carl Cori was an eminently civilized man.

The best source for Carl Cori's early life is his autobiographical essay, "The Call of Science," which appeared in *Annual Review of Biochemistry*. Other excellent sources include Herman M. Kalckar's historical account of the Cori's contributions in "Selected Topics in the History of Biochemistry: Personal Recollections," *Comprehensive Biochemistry* 35 (1983); a biographical memoir for the *American Philosophical Society Yearbook* (1985), by John T. Edsall; and the very detailed biographical memoir in *Biographical Memoirs of Fellows of the Royal Society,* 32 (1986), by Philip Randle.

NOTE

1. L. F. Leloir and his collaborators subsequently showed that, in vivo, a different enzyme—glycogen synthetase—catalyzes the formation the α-1,4 glucosidic bond from UDP-glucose in the biosynthetic pathway of glycogen.

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Gerty T Cori

Gerty Theresa Cori

August 8, 1896-October 26, 1957

by Joseph Larner

Gerty and Carl Cori's most significant contributions were the establishment of the cycle of carbohydrates known as "the Cori Cycle," the isolation of glucose 1-phosphate, and the discovery of phosphorylase and phosphoglucomutase. These discoveries established the enzymatic pathways of glycogenolysis and glycolysis.

In glycogen metabolism, Gerty Cori pioneered in the discovery of the debranching enzyme amylo-1,6-glucosidase and its use in the elucidation of glycogen structure by serial enzymatic degradation. This pioneering work led to the elucidation of the enzymatic defects in the glycogen storage diseases. Her studies, therefore, extended fundamental scientific discoveries into the clinical arena, most particularly in the field of pediatrics, her original area of clinical interest and specialization.

Gerty Theresa Radnitz was born on August 8, 1896, in Prague, at that time part of the Austro-Hungarian empire. Otto Radnitz, her father, was director general of a sugar refinery in Bohemia. Her mother's brother was professor of pediatrics at the University of Prague. Gerty studied at home until the age of ten, when she went to a girls' preparatory school, from which she graduated in 1912. In 1914, after passing her final examination (*matura*) at the Tetschen

Real Gymnasium, she enrolled as a medical student at the Carl Ferdinand University, the German university of Prague. There she met Carl Cori; they received M.D. degrees together in 1920 and were married in Vienna in August of the same year.

Carl described her as follows: "She was a fellow student, a young woman who had charm, vitality, intelligence, a sense of humor, and love of the outdoors —qualities which immediately attracted me." Their research together began as students and resulted in their first joint publication, on complement, in 1920.

The times were troubled. World War I had just ended, and the Austrian Empire had begun to disintegrate. Prague became the capital of the new country Czechoslovakia. Starvation and near starvation were widespread, and Gerty Cori developed symptoms of xerophthalmia—fortunately cured in time with an improved diet at her home in Prague.

During most of 1921, the Coris worked separately. Gerty was in pediatrics at Karolinen Kinderspital in Vienna under Professor W. Knoepfelmacher. Her research dealt with thyroid treatment for temperature regulation in a patient with congenital myxedema, and subsequently with studies in thyroidectomized rabbits. Several clinical research papers were published on hematological dyscrasias, including hemolytic crisis and essential thrombocytopenia.

Meanwhile Carl, also in Vienna, did laboratory work in the mornings and research at the University Pharmacological Institute in the afternoons. "My preceptor at the clinic in Vienna," he later wrote, "was a brilliant but amoral physician who was strongly antisemitic." Carl realized that because Gerty was a Jewess, their chances of obtaining academic positions in Europe were extremely slim, certainly an important factor motivating them to move to the United States.

In 1922 the Coris came to the United States—Gerty following Carl by about six months—and took up positions at the New York State Institute for the Study of Malignant Diseases (now Roswell Park). Gerty's first Buffalo publication, in 1923, compared thyroid extract and thyroxin on the rate of multiplication of paramecia, continuing her early interest in thyroid hormone action.

In Buffalo, the Coris' collaborative work rapidly became focused on carbohydrate metabolism in vivo and its hormonal regulation. In order to attack questions quantitatively, they developed careful methods for analyzing glucose, glycogen, lactic acid, and inorganic and organic phosphate. The Coris' studies on tumors in vivo confirmed the pathophysiological importance of Warburg's in vitro finding of increased tumor aerobic glycolysis, i.e., lactic acid formation. This early interest in lactic acid was of even greater importance in later work with epinephrine. In 1923 and 1924, Gerty published a series of four papers alone on the influence of X-rays on the skin and on the metabolism of body organs. She was interested in the possible differential sensitivity to X-rays of stained versus unstained skin. One cannot help but wonder whether this early exposure to radiation might have contributed to her later fatal bone marrow disease.

In Buffalo the pattern of developing superior analytical methodologies with great attention to experimental detail, coupled with the quantitative framing of questions, became a hallmark of the Coris' research. In a series of three elegant papers, they presented quantitative, in vivo balance studies involving short-term (3-hour) epinephrine administration that showed a small increase (+37 mg) in liver glycogen in the face of a larger decrease in total (chiefly muscle) glycogen (-57 mg). (From studies in hepatectomized animals, muscle glycogen itself was already known not to contribute directly to blood glucose.) Since epinephrine administration led to increased blood lactic acid, the increased liver glycogen most likely had come from lactic acid, a product of muscle glycogen breakdown carried by the blood from the muscles to the liver.

The Coris demonstrated that 40 to 95 percent of D-lactate (the isomer formed in muscle), whether eaten or injected, was retained as liver glycogen. L-Lactate, the unnatural isomer, was absorbed but not essentially retained as liver glycogen. Careful control experiments excluded vasoconstriction and hypoxia as the causes of the lactic acid increase produced by epinephrine administration. Arterio-venous difference measurements demonstrated that increased lactic acid in the blood arose from bodily (chiefly muscle) sources. The Coris termed this hallmark accomplishment the "cycle of carbohydrates," later fittingly called "the Cori Cycle."

The Coris' close attention to detail in developing superior analytical methodology proved even more important in the analysis of hexose phosphates, the next intermediates they scrutinized. They had previously shown that only 40 percent of the glycogen lost from the body with epinephrine action could be accounted for as lactic acid. This provided the motivation for developing a procedure for analyzing hexose mono-phosphates, a rigorous method based on measuring both the reducing power and organic phosphate content of water-soluble barium salts precipitated with ethanol. The Coris performed both determinations in order to characterize the product more precisely.

In the first of two notable papers—which lists Gerty Cori as the first author, suggesting that it was she who was primarily responsible for developing the team's quantitative analytical methodology—the Coris described their methodology. In the second they showed that hexose mono phosphate increased with epinephrine administration but not with insulin or with glucose. Thus began the work on the biochemical basis for the formation of hexose monophosphate in glycogenolysis and the discovery of glucose 1-phosphate.

In 1931 Gerty and Carl moved to St. Louis. Their work focused on the action of epinephrine evoking glycogenolysis in muscle. They increasingly simplified their experimental systems, working first with the whole animal, then with isolated muscle preparations, then with minces, and finally with broken cell preparations. In Buffalo they had shown conclusively that epinephrine administration increased hexose monophosphate in muscle in fifteen to sixty minutes, with a decrease to basal concentrations in four hours. Further, they had demonstrated a decrease in inorganic phosphate under these conditions and estimated that the hexose monophosphate accumulation was sufficient to account for the missing glycogen not accounted for as lactate.

From 1933 to 1936 they produced a set of papers on hexose monophosphate formation in frog and rat muscle, particularly with epinephrine administration and electrical stimulation, chiefly done anaerobically. Carefully measuring lactic acid, inorganic phosphate, creatine phosphate, and ATP, they concluded that the increased hexose monophosphate derived from glycogen esterification with inorganic phosphate in a stoichiometric reaction. The increase in hexose monophosphate occurred with an equivalent decrease in inorganic phosphate, with no change in creatine phosphate or ATP. In a paper with A. H. Hegnauer, Gerty states: "Of the three exothermic chemical reactions known to occur in anaerobic muscle (lactic acid formation, splitting of phosphocreatine, and splitting of adenosine triphosphate), only the first one is accelerated by epinephrine."

The Coris focused less and less on lactate and more on hexose monophosphate. They also obtained important data through studies of the reverse reaction, i.e., epinephrine removal (or cessation of electrical stimulation) followed by recovery under aerobic conditions. Hexose monophosphate was shown to disappear three times more rapidly aerobically than anaerobically. Increases in inorganic phosphate were accompanied by a decrease in hexose monophosphate but with increases in glycogen and lactic acid. Aerobically, glycogen was a major product, but anaerobically lactic acid predominated.

The Cori experiment on frog muscle poisoned with iodoacetate proved key, for it showed that the loss of hexose monophosphate was the same in poisoned and in unpoisoned muscles. Aerobic resynthesis of glycogen from hexose monophosphate, therefore, occurred directly, without first being converted to lactic acid.

Glucose 1-phosphate was first isolated from washed, minced frog muscle, incubated in inorganic phosphate buffer, in the presence of adenylic acid (1936). In the full publication of this work (1937), the Coris recorded that rabbit muscle was extracted with water, dialyzed extensively against water, and kept refrigerated under toluene. Phosphate buffer, glycogen, and adenylic acid were then added to the extract. The reaction mixture was incubated thirty minutes at 25ºC, deproteinized, and $Ba(OH)_2$ added to alkaline pH. The procedure developed for hexose phosphate analysis—namely, precipitation with alcohol—was then followed. The reducing power before acid hydrolysis represented the hexose 6-phosphate. The reducing power after acid hydrolysis represented the new hexose 1-phosphate. The researchers obtained about 500 mg of barium glucose 1-phosphate from 750 mg of glycogen.

In this paper, the chemical synthesis of glucose 1-phos phate was also ascribed chiefly due to the work of Sidney Colowick. The chemical properties, furthermore, including the acid dissociation constants, were carefully determined for both natural and synthetic compounds and shown to be identical. Very little can be added today to the chemical properties described by the researchers of the Cori lab, and the paper represents another milestone in enzymological research. It also included brief references to enzymological studies that would play such a large role in the Coris' later work, such as hydrolysis of the glucose 1-phosphate by an intestinal phosphatase and, in a note, the enzymatic conversion of glucose 1-phosphate to glucose 6-phosphate (Embden ester) in the presence of Mg^{2+}.

The years 1938 and 1939 were seminal, for, following their isolation of glucose 1-phosphate, the Coris shifted the emphasis of their work toward enzymology. Of ten papers published during that period, Gerty Cori was first author on seven, Carl on two, and Sidney Colowick on one.

One paper they wrote with Sidney Colowick studied the "migration" of the phosphate group of glucose 1-phosphate to the 6 position. Again, rabbit muscle extracts were prepared, exhaustively dialyzed, and even electrodialized to remove the Mg^{2+} required for the reaction. Among the series of metals studied, Mn^{2+} was found to be even more effective than Mg^{2+}. Mannose 1-phosphate and galactose 1-phosphate, synthesized by Sidney Colowick, were shown not to be converted to the corresponding 6 phosphates by the enzyme now named phosphoglucomutase. This was in keeping with the phosphoglyceromutase terminology previously used by Meyerhof and Kiessling. The muscle extracts contained no detectable phosphatase activity, but the conversion of glucose 6-phosphate to fructose 6-phosphate was established and the enzyme termed "phosphohexoisomerase."

A major error was the researchers' failure to recognize phosphoglucomutase as an equilibrium reaction. This was undoubtedly due to the presence of the isomerase, which distorted the mutase equilibrium. As noted in an addendum, no effect of insulin (Zn free) was detected either on the mutase reaction or on the formation of glucose 1-phosphate from glycogen. (This was in response to an earlier report by Lehmann describing an inhibitory effect of Zn insulin on the mutase reaction.)

A subsequent paper described the properties of the enzyme catalyzing the formation of glucose 1-phosphate from glycogen. This catalytic activity was now named phosphorylase.

With Gerhard Schmidt, Gerty and Carl began to examine the physiologic significance of their discovery of glucose 1-phosphate. They turned their attention to the liver—the organ responsible for the formation of blood glucose. It had been presumed that blood glucose was formed in the liver by the enzyme diastase (amylase). An alternate pathway via the action of phosphorylase and glucose 6-phosphatase had already been proposed by Gerty and Carl.

With Gerhard Schmidt, they now demonstrated the presence in liver of phosphorylase and phosphatase in the face of very weak amylase activity. Phosphorylase and phosphatase were separated from each other by alumina adsorption. By ammonium sulfate fractionation, phosphorylase was obtained free of mutase and phosphatase. This enzyme preparation catalyzed the formation of a polysaccharide in the test tube, from glucose 1-phosphate, which stained brown with iodine and was indistinguishable from glycogen. Adenylic acid was necessary for the phosphorylase reaction to proceed in either the degradative or synthetic direction.

The researchers further demonstrated with an extract from muscle the synthesis of glycogen in the test tube.

Here the synthesized polysaccharide stained blue with iodine, thus more closely resembling starch. Once again they had produced a hallmark paper.

In a brief but very important note, Gerty and Carl reported the activating effect of glycogen itself on the synthesis of glycogen from glucose 1-phosphate. Careful correlation of experimental results revealed an important difference when glycogen synthesis was carried out with phosphorylase preparations from liver and from other tissues. With preparations from other tissues, a lag period of variable length for glycogen synthesis was always observed. In contrast, with liver preparations, no lag period was observed. (No lag was observed with any enzyme preparation when the reaction was run toward glycogen degradation.) Since liver phosphorylase preparations always contained glycogen, while little or no glycogen was detected in the other enzyme preparations, the researchers decided to study the effect of adding glycogen during the lag period. Added glycogen abolished the lag period, and, Gerty and Carl reasoned, "one may conclude that this enzyme, which synthesizes a high molecular weight compound—glycogen, requires the presence of a minute amount of this compound in order to start activity." Thus began the concept of glycogen synthesis upon a preexisting primer.

Once again, the final publication by Gerty and Carl was an elegant description of the enzyme kinetics. Michaelis constants were determined for glucose 1-phosphate, for adenylic acid, and for glycogen with enzyme preparations from both brain and muscle. The reaction equilibrium was measured as a function of pH, and the reaction order was determined. It was further shown that glucose inhibits the reaction competitively with glucose 1-phosphate.

As already mentioned, phosphorylase preparations from brain, heart, or liver synthesized glycogen that stained brown with iodine, while phosphorylase preparations from muscle synthesized glycogen that stained blue with iodine. This extremely interesting observation eventually led to my subsequent work on the branching enzyme. In a paper published with Richard Bear, they compared the X-ray diffraction patterns of the two types of enzymatically synthesized glycogen with the pattern from plant starches and found that the pattern of the blue iodine-staining polysaccharide synthesized by the muscle phosphorylase was very similar to that from plant starches. The brown iodine-staining polysaccharide showed only a diffuse pattern, characteristic of amorphous material. With Zev Hassid, they demonstrated that the blue iodine-staining polysaccharide synthesized by the muscle enzyme was similar to the unbranched fraction of starch called "amylose." Digestion studies with B amylase, together with chemical methylation and hydrolysis studies, defined the synthetic polysaccharide as a 1,4-linked glucose polymer with an average chain length of 200.

Together with Earl Sutherland, Sidney Colowick, and Carl, Gerty established the chain of reactions from glucose 6-phosphate to glycogen, separating phosphorylase from mutase and isomerase. The researchers further corrected the previous failure to recognize the mutase equilibrium and quantified it. By precipitating the inorganic phosphate released by phosphorylase as $Ba_3(PO_4)_2$, they "pulled" the set of reactions—mutase and phosphorylase—toward glycogen synthesis against the unfavorable mutase equilibrium.

Gerty's experimental flair next became apparent in a note written with Arda Green and Carl describing the crystallization of muscle phosphorylase. The stage had been set by the prior fractionation of the enzyme, free of mutase and isomerase. Then in 1943, there appeared a definitive set of four papers on crystalline muscle phosphorylase.

In the first, Arda Green and Gerty Cori described the preparation and physical properties of phosphorylase, including the molecular weight. In the second, Gerty and Arda Green wrote on the prosthetic group with two forms of the enzyme, a and *b,* which were shown to be interconverted by a third enzyme termed "PR" to denote prosthetic-group removing. The prosthetic group removed, however, was thought to be adenylic acid, and this was later shown to be incorrect. The third paper—by Carl, Gerty, and Arda—described the kinetics of the reaction, while the fourth paper, by Gerty and Carl, dealt with the formation of glycogen. In it a new enzyme was described that permitted a conversion of the blue iodine-staining polysaccharide into the brown-staining glycogen. The new enzyme was thought to be a new phosphorylase that synthesized the 1,6 bond or an enzyme related to an amylase.

These are the papers on which I cut my scientific teeth, and—with the exception of the work on the PR enzyme—they remain classics in the field.

The crystallization of phosphorylase *a* from muscle and the recognition of a second *b* form, together with its subsequent crystallization by Carl and Gerty, initiated the era of control by covalent phosphorylation and the influence of allosteric effectors—since the two forms were recognized by their differential sensitivity to adenylic acid. The correct chemistry of the interconversion between the two forms *a* and *b* was subsequently clarified by Krebs and Fischer and by Sutherland and Rall and their collaborators. The Cori laboratory continued to crystallize additional glycolytic enzymes successfully. Gerty, with Carl and Milton Slein, crystallized from muscle extracts *d*-glyceraldehyde 3-phosphate dehydrogenase and, with John Taylor and Arda Green, aldolase.

While Carl, Earl Sutherland, and Theo Posternak pursued their interest in enzyme mechanisms (particularly study ing the mutase reaction), Gerty and I continued to study glycogen structure. The work on hormonal regulation by Carl, Win Price, and Sidney Colowick that had caused such a furor had been discontinued. This was later resumed along different lines by Mike Krahl, Joe Bornstein, Rollo Park, and their collaborators, who were studying insulin action, and by Sutherland, Rall, and their coworkers, studying epinephrine and glucagon action.

I arrived in St. Louis in 1949, having spent eighteen months in the Biochemistry Division of the Chemistry Department at the University of Illinois obtaining a master's degree in chemistry. Upon arrival I was assigned to work under Gerty Cori and immediately began a program of research.

At that time Gerty was particularly interested in the degradation of the 1,6 linkages, or branch points, in glycogen. Shlomo Hestrin, who had preceded me in St. Louis, found that highly recrystallized muscle phosphorylase degraded glycogen only partially (about 40 percent), whereas crude phosphorylase degraded glycogen completely. I was given the task of finding how the cruder enzyme bypassed or degraded the branch points.

Through the work of Allene Jeanes, Melvin Wolfrom, and their collaborators, the 1,6-linked disaccharide, isomaltose, had just become available. We had a small, precious sample, which we used as a standard in a paper chromatographic analysis of the products of enzymatic degradation of glycogen from crude or highly purified phosphorylase preparations. When the reaction mixtures were treated to remove the hexose phosphates, the analyses revealed the presence of free glucose only, and no isomaltose was found.

We proposed, therefore, a hydrolytic mechanism of cleavage of the 1,6 linkages, with the formation of free glucose as sole reaction product. The debranching enzyme was named amylo-1,6-glucosidase. Acting together with phosphorylase, amylo-1,6-glucosidase completely degraded glycogen to a mixture of about 90 percent hexose 1-phosphate and 10 percent free glucose. William Whelan subsequently showed that this enzyme has two activities: first, to reposition the several glucose residues on the product of the action of phosphorylase on glycogen, such that the singly 1,6-linked glucose residue was exposed (transferase activity); second, to catalyze the hydrolysis of the 1,6 linkage, releasing free glucose.

I still recall Gerty's excitement when I discovered free glucose as the only reaction product. She ran up the hall to Carl's office at the other end of the Department shouting "It's free glucose, it's free glucose!"

I continued to work with Gerty, using the new enzyme amylo-1,6-glucosidase and phosphorylase in a sequential manner to work out the arrangement of the branch points in glycogen and in amylopectin, the branched-starch fraction. Gerty also continued working with Pat Keller on the PR interconverting enzyme. They found that, in the conversion of phosphorylase *a* to *b,* the molecular weight was halved, and Carl immediately renamed the PR enzyme phosphorylase rupturing.[1]

I next worked out the mechanism of the branching enzyme, while Gerty carried out independent studies on hexokinases with Milton Slein and—with Severo Ochoa, Milton Slein, and Carl in 1951—on fructose phosphorylation and metabolism in liver.

By far her greatest interest in her remaining years was the nature of the enzymatic defects in the glycogen storage diseases, a return, in a sense, to her original interest in pediatrics. I first considered and then proposed to Gerty the possibility that the disease—then considered to be a single ailment termed Von Gierke's disease—might be due to a lack of the debranching enzyme, amylo-1,6-glucosidase.

But Gerty felt that the missing enzyme was glucose 6-phosphatase. Behind us in the laboratory stood a chemical cabinet that contained, among other things, a set of glycogen samples Gerty had isolated from tissues sent to her by numerous clinical investigators interested in this disease. I reasoned that, if I were correct, the glycogen itself would have an abnormal structure, with shortened outer chains, but with branch points intact. If Gerty were correct, the glycogen structure would be normal.

We made a wager on the outcome—a common event in the laboratory. With her permission I proceeded to take one of the glycogen samples from the cabinet, dissolve a small aliquot in water, and stain it with a few drops of iodine solution in a test tube. To both Gerty's and my amazement, the sample stained bluish-purple with iodine—more like a starch than a glycogen! This sample had been sent by Dorothy Anderson, my former pediatrics teacher from Columbia University College of Physicians and Surgeons. Serendipitously, it was the only sample in the collection with an abnormal iodine color.

I immediately rationalized an explanation, arguing that the branch points were intact but that the outer chains were elongated because the child had died in a state of good nutrition, so that the outer chains had been built up. Gerty became very excited, recognizing that with an abnormal glycogen structure, glycogen storage was a molecular disease. (The only example of a molecular disease known at that time was Pauling's sickle-cell hemoglobin.)

Gerty began to study this problem intensively. It soon became clear that it was in fact a disease with multiple enzyme defects. She was able to document four forms, one related to lack of glucose 6-phosphatase in liver, a second related to a lack of amylo-1,6-glucosidase with a generalized organ distribution, a third related to a lack of the branching enzyme responsible for the bluish-purple-staining glycogen, and a fourth of unknown etiology leading to a generalized organ disease.

Gerty Cori summarized these findings in a Harvey Lecture in 1952. Both of our hypotheses had been correct. Her last published work, in 1957, was a review on the glycogen storage diseases.

Gerty was a tireless scientific worker and an avid reader. She was at all times a superb experimentalist and analyst with the most demandingly high standards. Even though I arrived in St. Louis with a paper already published in the *Journal of Biological Chemistry,* she personally taught me how to pipette, watched over my shoulder as I performed my first standard curve for the analysis of glucose, taught me how to crystallize muscle phosphorylase, and had Earl Sutherland teach me how to crystallize the potassium salt of glucose 1-phosphate.

She was constantly in the laboratory, where we two worked alone. We washed our own laboratory glassware and she would occasionally complain bitterly to Carl about not having any dishwashing help. When she tired, she would retire to her small office adjoining the laboratory, where she would rest on a small cot. She smoked incessantly and dropped cigarette ashes constantly on the tar-covered laboratory benches. I often wondered if this helped in the enzyme crystallizations.

Gerty had a vivacity and a love of science and discovery that were infectious. She wanted to make the exciting discovery first, then do the necessary controls later. She and Carl had an instinctive "feel" for the right path to follow to solve the problem. She needed only one exciting experimental finding to jump into a problem with unbounded energy.

During these years her health was failing, and Carl was personally involved in monitoring her blood hemoglobins and administering her transfusions. Yet the illness never carried over emotionally to affect her attitude during her work in the laboratory.

Gerty was extremely broadly read. The Mercantile Library was an organization in St. Louis from which one ordered books by phone. Gerty would routinely order five to seven books per week, which were delivered to the laboratory or to the department office. By week's end she would have read them all, put in her next week's order, and have the old ones picked up. This occurred week after week.

She could speak authoritatively on a variety of topics, from political theory, to sociology, to art and the humanities, to grocery shopping. Her intellectual breadth never ceased to amaze me.

I feel most fortunate to have been trained by Gerty and Carl as a student and to have had the opportunity to soak up some of their approach to dealing with scientific problems.

THIS I BELIEVE [2]

I have been very fortunate to have been allowed to work freely in the field of . . . biochemistry, in which I have been intensely interested ever since I got a first glimpse of it as a medical student in 1914. I came to this country in 1922 and owe it the greatest debt of gratitude for having treated me and my husband with fine generosity, giving us wonderful opportunities for research work, security, and a happy life.

I believe that the benefits of two civilizations, a European education followed by the freedom and opportunities of this country, have been essential to whatever contributions I have been able to make to science.

I believe that in art and science are the glories of the human mind. I see no conflict between them. In the past they have flourished together during the great and happy periods of history and those men seem to me short-sighted who think that by suppressing science they will release other creative qualities. It may be, however, that the present period is more favorable to the development of science than of art.

Contemplation of the great human achievements through the ages is helpful to me in moments of despair and doubt. Human meanness and folly then seem less important. Humanity has but a short history of civilized life and the hope for greater wisdom must resign itself to a fairly distant future. Gone are the somewhat Utopian hopes of my youth, the belief in continuous progress. I still believe, however, that Western civilization is at least on the right track.

Cruelty and malice have decreased as the well-being of people in the Atlantic community has increased. An immense advance in this direction has taken place in the last hundred years, even if one considers the setbacks caused by the two terrible wars of this century. Modern medicine, aided by chemistry, has decreased human suffering. Throughout, science has thus conferred an immense boon on mankind.

The greatest achievements in art and science, I believe, have been made by men who had faith, or compassion, for their fellow men, and I like to think in this connection of the moving outcry in Beethoven's opera, *Fidelio:* "Es gibt eine Gerechtigkeit!" (There is justice!).

I believe that cynicism and despair and the straight jacket into which totalitarian systems try to force the human mind are inimical to first-rate achievements in art and science.

I believe that the excessive will to power of some men has been, and is still, the cause of great suffering of humanity. Science has given these ruthless men tools of great effectiveness and has vastly increased the domination they can exercise over their fellow men. This has created in some men's minds the misconception that science itself is evil.

My beliefs have undergone little change during my life, though I like to think they have developed into a somewhat higher plane.

Honesty—which stands mostly for intellectual integrity, courage, and kindness—are still the virtues I admire, though with advancing years, the emphasis has been slightly shifted, and kindness now seems more important to me than in my youth.

The love for and dedication to one's work seems to me to be the basis for happiness. For a research worker the unforgotten moments of his life are those rare ones, which come after years of plodding work, when the veil over nature's secret seems suddenly to lift, and when what was dark and chaotic appears in a clear and beautiful light and pattern.

NOTES

1. As mentioned above, the final solution of the interconversion mechanism was provided by Krebs and Fischer, and by Sutherland and Rall and their collaborators.

2. Gerty Cori submitted this short statement of her personal philosophy to the National Academy of Sciences in 1954.

HONORS AND DISTINCTIONS

Degrees

1914-20

M.D., Carl Ferdinand University, Prague, Czechoslovakia

Professional Appointments

1920-21

Clinical training in pediatrics, Karolinen Kinderspital, Vienna, Austria

1921-22

Department of Pathology, New York State Institute for the study of Malignant Disease (Roswell Park), Buffalo, New York

1931-47

Professor of pharmacology, Washington University School of Medicine, St. Louis, Missouri

1947-57

Professor of biochemistry, Washington University School of Medicine, St. Louis, Missouri

Honors

1946

Midwest Award of the American Chemical Society

1947

Squibb Award of the Association for the Study of Internal Secretion

1947

With C. F. Cori, Nobel Prize in Medicine or Physiology

1948

Frances P. Garven Medal, American Chemical Society

1948

St. Louis Award

1950

Sugar Prize of the National Academy of Sciences

1951

Borden Award of the Association of American Medical Colleges

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With C. F. Cori. The fate of sugar in the animal body. VI. Sugar oxidation and glycogen formation in normal and insulinized rats during absorption of fructose. *J. Biol. Chem.* 73:555-66 .

With C. F. Cori. The fate of sugar in the animal body. VII. The carbohydrate metabolism of adrenalectomized rats and mice. *J. Biol. Chem.* 74:473-94 .

1928 With C. F. Cori. The mechanism of epinephrine action. I. The influence of epinephrine on the carbohydrate metabolism of fasting rats with a note on new formation of carbohydrates. *J. Biol. Chem.* 79:309-19 .

With C. F. Cori. The mechanism of epinephrine action. II. The influence of epinephrine and insulin on the carbohydrate metabolism of rats in the post absorptive state. *J. Biol. Chem.* 79:321-41 .

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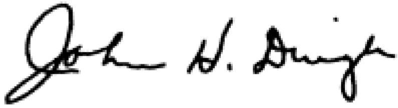

John Holmes Dingle

November 24, 1908-September 15, 1973

by William S. Jordan, Jr.

John Dingle's contributions to biomedical science and medical education were fully expressed in each of the components of the classic triad of research, teaching, and service. He pioneered studies of the epidemiology and etiology of acute respiratory infections in military and civilian populations, which set a standard for all such future studies; he created a new department of preventive medicine at Western Reserve University (now Case Western Reserve) and participated in the school's extensive review of medical education and subsequent curriculum revision; and he served with distinction on numerous national advisory groups, most notably as director of the Commission on Acute Respiratory Diseases and then as president of its parent body, the Armed Forces Epidemiological Board. With a remarkable group of like-minded contemporaries, John Dingle extended efforts in all of these areas to promote international cooperation in the study and control of infectious diseases.

John Holmes Dingle was born on November 24, 1908, in Cooperstown, North Dakota, where his father was a Methodist minister. He was the only child of his father's second marriage, joining six much older children borne by his father's first wife. His mother was from Honeoye, New

York, from which her family sent a barrel of apples each year to North Dakota. As Dingle reminisced in later years when reflecting on limited financial resources in his youth, it was his assignment to bring up apples from the barrel in the basement with instructions that rotten apples were to be eaten first. An apple without spots was a luxury delayed. His father died when Dingle was eight years old; when he was thirteen, the family moved to Seattle, Washington, to live with one of his older half-brothers.

Dingle's predoctoral education was obtained at the University of Washington—a certificate in pharmacology and a B.S. (summa cum laude) in 1930 and an M.S. in 1931. He then journeyed east for three days by train to the Johns Hopkins University School of Hygiene and Public Health, which had earlier granted him a scholarship, only to learn that his scholarship had vanished with the Depression. The school did wave tuition, but he had to do a number of odd jobs to pay for room and board, losing sixty pounds in the process. Observing this, bacteriologist Pearl Kendrick—who later pioneered development of the pertussis vaccine—helped by periodically inviting Dingle to lunch. Hopkins awarded him a Sc.D. in immunology in 1933. Subsequently, again largely for financial reasons, he worked as an assistant bacteriologist at the Maryland State Department of Health Laboratory and then joined the staff of Upjohn Company as a bacteriologist for two years before entering Harvard Medical School in 1935. There he was supported by a Cabot fellowship from 1936 until his graduation magna cum laude in 1939.

As a mature student with a strong background in bacteriology and immunology, Dingle naturally established a beneficial association with Hans Zinsser and the stars and soon to be stars of his department: Leroy D. Fothergill, John F. Enders, John H. Mueller, and F. Sargent Cheever.

He formed a productive bond with Fothergill during a series of studies (with C. A. Chandler) on the pathogenesis of *Haemophilus influenzae* infection in mice. During the first week of August 1938, at the beginning of his senior year, an epidemic of eastern equine encephalomyelitis (EEE) occurred in New England, principally in Rhode Island and southern Massachusetts. Coincident with the peak of the epidemic, encephalitis appeared in five children living within five miles of each other in Massachusetts and in the same area as the equine disease. Brain tissue from fatal cases was examined at Harvard by Sidney Farber, pathologist at the Children's Hospital, who joined Fothergill, Dingle, and M. L. Connerley in describing the cases, and by investigators at the Rockefeller Institute. The latter isolated EEE virus; the children died as secondary hosts of a virus that infects horses.

During the same epidemic, pigeon breeders in the area suffered unusual losses in their flocks. Fothergill and Dingle identified a virus from the brain of a dead pigeon as EEE virus, confirming the role of birds in the transmission cycle. Concurrently, Dingle contributed his immunological expertise to studies with Thomas Hale Ham of a rare form of hemolytic anemia, paroxysmal nocturnal hemoglobinuria, and interested Ham in studying the cold hemagglutinins associated with primary atypical pneumonia. Later, when both were at Western Reserve University—Ham at Dingle's urging—they provided advice to the author when he had the opportunity to study two cases of syphilitic cold hemoglobinuria in the same year. By the time of his graduation from medical school, Dingle had contributed to twenty published papers. It is not surprising that he competed successfully for an internship at Children's Hospital.

In 1940 he joined Maxwell Finland's group at the Thorndike Memorial Laboratory, Boston City Hospital, as Francis Weld

Peabody Fellow in medicine and instructor in Zinsser's Department of Bacteriology and Immunology. He participated in early studies of sulfonamides, establishing the efficacy of sulfadiazine in the treatment of meningococcal meningitis, and collaborated with Lewis Thomas on investigations of the bacteriological and immunological aspects of meningococcal infections. This was at a time when the United States was mobilizing for World War II, and meningococcal meningitis was to become a threat to military populations. Dingle's first "field assignment" was to lead a Harvard team to investigate simultaneous outbreaks of diphtheria, scarlet fever, and meningococcal meningitis at Halifax, Nova Scotia, in January 1941.

Another disease of military importance was to shape Dingle's career and occupy his attention for the next quarter century. Primary atypical pneumonia was recognized as an epidemic respiratory disease, perhaps an entity, distinct from then-known viral pneumonia, in the late 1930s. Finland, an investigator particularly interested in pneumonia, had been seeing an increasing number of cases of nonbacterial pneumonia at Boston City Hospital by the time Dingle arrived, and a number of members of the house staff on the two Harvard medical services were being hospitalized with this diagnosis. Similar illnesses were occurring in military personnel in expanding recruit camps. In 1941, having been named a consultant on epidemic diseases to the Secretary of War and a member of the Commission on Influenza under the Board for the Investigation and Control of Influenza and Other Epidemic Diseases in the Army—the predecessor of the Armed Forces Epidemiological Board—Dingle was asked to join a group of medical scientists to investigate an outbreak of atypical pneumonia at Camp Claiborne, Louisiana. This short-term study led to the recommendation that a separate Commission on Acute Respi ratory Diseases be created, its investigators to conduct long-term field and laboratory studies at an Army recruit base. The establishment of this commission at Fort Bragg, North Carolina, and the direction of its studies there for the next four years constitute one of the major achievements of Dingle's career.

By October 1942 he had assembled an outstanding group of investigators, including Theodore J. Abernathy (internist), George F. Badger (biostatistician), Alto E. Feller (virologist), Alexander D. Langmuir (epidemiologist), and Charles H. Rammelkamp (internist). These physicians, augmented by Army medical officers, enlisted personnel, and civilian staff, decided to publish collectively as the Commission on Acute Respiratory Diseases. Dingle promoted this designation to emphasize that theirs was a team effort. His own brilliance coupled with his respect for and encouragement of the talents of others established him as a persuasive leader in a stimulating, congenial, and productive environment.

Among the major contributions to the development of knowledge of respiratory diseases in military personnel was the demonstration that at least three then uncultivatable filterable agents were responsible for three clinically and epidemiologically defined illnesses: atypical pneumonia, the common cold, and an influenza-like illness of recruits characterized at Fort Bragg and labeled acute respiratory disease (ARD). This was accomplished by a series of carefully designed studies in volunteers.

Human subjects had to be used because the embryonated egg and animal systems available in this pre tissue culture era yielded little other than influenza viruses. Rammelkamp toured the country recruiting volunteers from among conscientious objectors of several religious groups, and the commission took over a resort hotel in nearby Pinehurst in which to house them. Lacking spe cific serologic tests, the investigators could select susceptible volunteers only on the basis of a history of recent illness. They were fortunate in that filtered respiratory secretions induced illnesses that resembled those from which they were taken. Subsequent cross-over challenge experiments confirmed differences in incubation periods of the common cold and ARD agents and demonstrated homologous but not heterologous immunity, indicating that the two agents were immunologically distinct and probably distinct from the agent of atypical pneumonia. It is now known that atypical pneumonia is caused not by a virus but by *Mycoplasma pneumoniae;* ARD is caused by several adenovirus types and can be prevented by a live oral vaccine; most, but not all, colds are caused by any of more than 100 rhinoviruses.

After the war, the military decided that it still needed the advice of the Army Epidemiological Board and the expertise of certain of its commissions. Dingle, who was to continue as director of the Commission of Acute Respiratory Diseases until 1955, conceived the idea of applying its epidemiological and laboratory methods to the study of civilian populations and sought an academic base at which to do so. Yale, with Francis G. Blake in medicine and a Division of Public Health headed by John R. Paul, was considered, but Joseph T. Wearn, dean of Western Reserve University's School of Medicine, persuaded him to come to Cleveland in 1946 to establish a new Department of Preventive Medicine. Dingle's decision was no doubt influenced by the willingness of local practitioners to cooperate by identifying families for pilot studies. He brought three members of the commission—Badger, Feller, and Rammelkamp—with him and recruited Richard G. Hodges, a pediatrician who had conducted studies of pneumococcal vaccine at an Air Force base with Colin M. MacLeod, to join them. This group was later augmented by others, including Floyd W. Denny, Jr., Harold S. Ginsberg, Sidney Katz, Lois P. McCorkle, Robert Oseasohn, and myself, as Dingle exercised his capacity to raise funds for the support of his department.

With the approval of the Board of Trustees of the Academy of Medicine of Cleveland, whose then-president, Chauncey W. Wyckoff, a pediatrician, encouraged participation by his patients, Dingle's staff eventually recruited eighty-six families, with 443 individuals, resident in middle-class suburbs within easy driving distance of the medical school to participate in what became known as the Cleveland Family Study. This longitudinal study of the occurrence of illness, all illnesses, in families with young children was largely descriptive because, apart from influenza, few respiratory viruses could be identified in those years. Although the bacteria that could be identified—*Haemophilus influenzae,* pneumococci, and streptococci—were comparatively infrequent causes of illness, the occurrence of multiple cases of acute glomerulonephritis in a family infected with the type 12 group A beta hemolytic streptococci was the clue that led Rammelkamp to postulate the existence of nephritogenic types, a postulate supported by studies of other populations. The high incidence of viral respiratory infections in the families, particularly in young children, was documented, and the relative importance of the home and school in their transmission was assessed. Similar data measured the passage of influenza epidemics, most notably Asian influenza in 1957, study of which necessitated reactivation of observation of the population, which had recently been discontinued after ten years of data collection. That this was possible is a tribute to the rapport Dingle had established with the parents and to the responsiveness of a staff devoted to his leadership.

When adenoviruses became available, it was shown that many children are infected by certain types early in life, often without symptoms. Curiously, the adenovirus types responsible for ARD in military recruits were shown not to be important causes of illness in civilians. Other studies documented the ineffectiveness of an antihistamine for the treatment of colds and provided evidence for the existence of two kinds of nonbacterial gastroenteritis. A final note: all of the data were processed under the guidance of Badger without benefit of computers! A summary of this team effort was published in 1964.[1]

Seldom, if ever, has a medical faculty paid more attention to undergraduate education than during Dingle's tenure. Dean Wearn, himself a product of Harvard Medical School and the Thorndike Memorial Laboratory, had capped his opportunity to recruit eleven new department chairmen in a short period of time with the decision to reexamine the objectives of medical education, the content of the curriculum, and pedagogical processes. Hale Ham was recruited to conduct this "experiment in medical education," and his friend and colleague Dingle became an active participant in the key committees and planning sessions that evolved. On occasion this led to faculty retreats at the dean's home, which became both serious working sessions and pleasant social events. To promote relaxation after a day of intense debate, a self-appointed vocal group composed and presented songs relevant to this collective effort. Such was Dingle's personality and warmth of fellowship that it was only natural this group should gather at his home, with him at the piano, to write its ditties.

The revised curriculum, introduced in 1952, incorporated subject committee teaching, early introduction of students to patients and families, project research, free time, increased emphasis on ambulatory care, and other approaches periodically rediscovered by medical educators. Dingle's department assured the incorporation of empha sis on the scientific method and consideration of biostatistics, epidemiology, preventive medicine, and health care, as appropriate, throughout four years by having its staff interact with both basic and clinical scientists. A number of these staff members, with Dingle's blessing, went on to become department chairman at other institutions.

During and after World War II, Dingle was closely associated with a group of scientists with broad knowledge of infectious diseases and a concern for expanded basic and clinical research both at home and abroad. This group included Colin M. MacLeod, Thomas Francis, Jr., Joseph E. Smadel, and Theodore E. Woodward, who, with others, spent many days traveling the world at the request of James A. Shannon, director of the National Institutes of Health, or the White House, or the Department of Defense to select a site for a laboratory for the study of cholera, to negotiate collaborative agreements with the Japanese, or to assess progress at strategic military research laboratories. They, and others like them in other fields, laid the groundwork for an expanded U.S. effort in international health research.

National and international travel became more and more difficult for Dingle because of the slow progression of an undiagnosed musculoskeletal disease that plagued him from the age of eighteen. Weakness of his left leg, attributed without documentation to poliomyelitis, caused him to limp, and weakness of the left shoulder and arm appeared later. This was not sufficient to disqualify him from being commissioned a major, M.C. AUS in May 1944, with the rank of lieutenant colonel when deactivated in July 1946, nor did he later allow increasing disability to limit his participation in the activities of professional societies and advisory committees during twenty-four years as chairman of the Department of Preventive Medicine. His spirit is sym bolized by his determination to spend a year in Geneva as a WHO consultant in 1965-66 despite his need by this time for braces on both legs and a wheelchair for long stretches. By 1969 he was completely dependent on a wheelchair and made arrangements to have himself forklifted on and off airplanes in the days before jetways. Such was the affection felt for him by his colleagues that he never lacked companionship and a helping hand. Despite muscle biopsies and enzyme studies, his illness remained undiagnosed before and after his death from cardiopulmonary failure at the age of sixty-five.

During his years in Cleveland, Dingle enjoyed the blessings of a loving family. He had previously been married to Cornelia Eddy, Ph.D., a fellow bacteriologist, whom he met at Hopkins; they were married in October 1933 and divorced in June 1945, having had no children. At Fort Bragg, Dingle met Doris (Dottie) V. Brown, a native of nearby Fayetteville, while she was working as a secretary for the Commission on Acute Respiratory Diseases. They were married on January 18, 1946, and set up housekeeping in Cleveland Heights the following August. They had two children, Eva Meredith and David Rufus, with whom they delighted to return each summer to a rambling family cottage on the Intracoastal Waterway at Gause's Landing near Shallotte, North Carolina. Even in later years when his disability limited him to the front porch, Dingle looked forward to this family vacation.

At home John and Dottie kept open house for the world. He loved to entertain and she was a marvelous hostess. They were generous with invitations to friends and faculty associates, attracting many to marathon sessions of duplicate bridge. Interest both in the Cleveland Family Study and in the curriculum experiment assured a steady flow of visitors, a high percentage of whom enjoyed the warm fellowship of the Dingle household where dry martinis were a specialty and tasty food was assured, even with a different visitor or two every night.

Dingle served his community for twelve years as a member of the Cleveland Health Council and as its chairman during 1954-57. He was a member of the advisory boards of the Cleveland Health Department, the Maternal Health Association, and the Cleveland Diabetic Fund. He was an active member of the National Research Council's Subcommittee on Infectious Diseases and its successor, the Subcommittee on Infectious Diseases and Chemotherapy, for ten years, serving as chairman in 1950-53. He was elected to the National Academy of Sciences in 1958.

Dingle was a member of fifteen professional societies and served as president of three of them: American Association of Immunologists, American Epidemiological Society, and Central Society for Clinical Research. His numerous awards included the Legion of Merit and the Outstanding Civilian Service Medal from the Department of the Army, the Albert Lasker Award, the James D. Bruce Memorial Award of the American College of Physicians, and the Bristol Award of the Infectious Diseases Society of America.

Those who were privileged to know and work with John Dingle remember him as a creative medical scientist and teacher, a remarkably effective collaborator, an able administrator, a warm and thoughtful friend and colleague, and, above all, a noble citizen.

I am indebted to Mrs. John H. Dingle and to George F. Badger and Theodore E. Woodward for supplementing my personal knowledge of John Dingle's life and career.

NOTE

1. Dingle, J. H., G. F. Badger, and W. S. Jordan, Jr, *Illness in the Home. A Study of 25,000 Illnesses in a Group of Cleveland Families* (Cleveland: Western Reserve University Press, 1964), 398 pp.

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Harvey Fletcher

Harvey Fletcher

September 11, 1884-July 23, 1981

by Stephen H. Fletcher

To children of famous people, a parent is just a parent. So it is oftentimes surprising to grow up and realize that your father was an important and well-known scientist. When I was a boy, I was frequently told that my father, Harvey Fletcher, was a great man, but it wasn't until I was an adult that I learned he had made enormous contributions to the field of acoustics and atomic physics as a teacher, researcher, and administrator.

My father was born on September 11, 1884, in Provo, Utah, of pioneer parents. Provo was then a small town in Utah Valley near a freshwater lake surrounded by high mountains. As my father recalled:

> As I looked across Utah Valley, I thought that the tops of the mountains that I could see in any direction marked the end of the world where people live. On the other side of these was the great ocean. There were cracks in the wall that held the ocean back, so that the water from the ocean leaked through and formed the various streams that come down from the mountains.[1]

This musing and philosophizing about the world around him characterized my father's life. However, he had no ambitions to be a scientist. His father, whose trade was building houses, had only four months of formal school ing. His uncles ran grocery stores in Provo. My father labored with his father building houses and with his uncles delivering groceries, and as a youth he wished nothing more than to follow in their footsteps.

In 1901 he entered Brigham Young Academy. This was the only school in Utah Valley offering education beyond the eighth grade, which he had then obtained. He wanted to enroll in the Commercial Division but opted for the Normal Division because the tuition was somewhat cheaper. In the Normal Division courses in mathematics, physics, chemistry, and so forth were offered to prospective teachers, and he was thus exposed to these subjects quite by accident. He was shocked when he received a failing grade in his first course in physics. In retrospect he felt he had not taken the course seriously enough and repeated it, this time earning an A^+, the highest grade in the class. Years later his students were delighted and comforted when he told them that he had failed his first physics course.

Brigham Young Academy at that time was a continuation of high school and college courses, looking toward a bachelor's degree in six years. While he was enrolled in the school, the curriculum was changed to provide four years of high school and three years of college. In 1907, at the end of seven years, my father graduated with a B.S. degree; prior to graduation he was hired by the school to teach physics and mathematics courses. In the summers he put his mathematical and engineering skills to practical use by working on such projects as running a government survey in the unexplored mountain country of eastern Utah and supervising the building of water mains to supply the town of Provo.

After a year of teaching at Brigham Young University (Brigham Young Academy became Brigham Young Univer sity with the above-noted change in curriculum), his ambition was completely reoriented, from business to science. He decided to pursue a teaching career in physics and felt he needed to obtain a Ph.D. if he was to be successful. He believed the University of Chicago would offer him the best training. However, he felt that he could not face the big city alone, so persuaded the girl he had been courting, Lorena Chipman, to marry him and join in this great adventure. The marriage took place on September 9, 1908, and soon after the couple moved to Chicago.

They were, perhaps, a little naive to move to Chicago before being assured of his admission to the University of Chicago's graduate program. Indeed, admission proved to be difficult, because, despite his B.S. degree, he had completed only three years of college work, much of which was not accredited by Chicago. The Admissions Office informed him that he must take four years of undergraduate work at Chicago before he could be admitted to the graduate program. Such an extended educational program was beyond his means. Fortunately, Robert A. Millikan, then an assistant professor, came to his rescue. He suggested that my father enroll as a special student in the courses that were usually taken in the first year of the graduate physics program, and, if these were successfully passed, he might then be admitted to the graduate school. He did this and was admitted on the condition that he make up one year of undergraduate work in the college. In the following three years he not only made up the one year of college but also taught high school science in the School of Education at Chicago, assisted in college lectures by operating the projection equipment, took care of his young family, and earned his Ph.D. summa cum laude in physics—the first summa cum laude degree in physics ever awarded at the University of Chicago. He was also elected to Phi Beta Kappa.

This was a remarkable achievement after such an uncertain beginning.

At that time, Professor Millikan, together with Professor Louis Begeman, was conducting research into the nature of the electric charge. After inquiring about a subject for his Ph.D. thesis, Professor Millikan outlined the research that he and Professor Begeman had been conducting at Chicago and also described similar work being done at Cambridge University by J. J. Thomson and Regener. My father described the Chicago experiment as follows (Autobiography, p. 30):

> They had arranged a little box having a content of 2 or 3 cubic centimeters which was fastened to the end of a microscope. A tube was attached from an expansion chamber to the little box. By opening suddenly a petcock, a sudden expansion of the air in the little box was made which caused a cloud of water vapor to form. Then viewed through a microscope this cloud was seen to be composed of a large number of tiny water drops. The droplets would soon drop from the top to the bottom of the box under the influence of gravity. A conducting plate was arranged at the top and another one at the bottom of the box so that an electric field could be produced.
>
> When this electric field was turned on it would retard the fall of some droplets. They were trying to make the field just right so that the droplet would be suspended in the air between the plates. From the speed of the droplet, that is the fall speed, and the intensity of the field to stop the droplet, one could calculate the electrical charge on the droplet. This was essentially repeating the experiment that Regener did in England. However, the water forming the droplet evaporated so fast that the little droplet would only stay in view for about 2 seconds. So it was difficult to get more than a rough estimate of the charge.

After discussions with Professors Millikan and Begeman, my father decided to build equipment similar to the Millikan-Begeman equipment but using oil instead of water. He described the experiment as follows (Autobiography, pp. 31-32):

First, an arc light with two condensing lenses in front of it was set up. The combination made a bright beam of light. The experience which I had with projection lanterns for lectures made it possible to get this together very quickly. I then used the atomizer and squirted some oil spray so that it fell through the beam of light. The light made these tiny drops of oil look like tiny stars. This indicated this part of the experiment would probably work. I then went down to the student shop and found some brass sheets about one-eighth of an inch thick. From them I cut two circular plates about 20 centimeters in diameter. Then I fastened (soldered) a stem onto each one so that they could be held by an ordinary laboratory stand with clamps. A small hole was then bored in the center of the top plate. These plates were then set up horizontally being about 2 centimeters apart. In this first set-up the air between the plates was not enclosed. So I moved the stands holding the two plates over into the beam of light. I then put a large cardboard between the light and the plates and cut a hole just large enough to permit the light to go between the plates without touching them. I then found a cathetometer (an instrument commonly used around a physics laboratory) and placed it so the telescope on it was turned and raised and lowered until its line of sight went between the two plates and at about 120° from the direction of the light beam. The distance from the telescope to the plates was about one meter. I then tried out the apparatus. I turned on the light; then focused the telescope; then sprayed oil over the top of the plate; then came back to look through the telescope. I saw a most beautiful sight. The field was full of little starlets, having all colors of the rainbow. The larger drops soon fell to the bottom, but the smaller ones seemed to hang in the air for nearly a minute. They executed the most fascinating dance. I had never seen Brownian Movement before—here was a spectacular view of them. The tiny droplets were being pushed first that way and then this way by the actual molecules in the air surrounding them. I could hardly wait until I could try an electrical field upon them to see if they were charged. I knew there [were] two or three banks of small storage cells in the laboratory. A large number of these small storage cells had been connected in series and mounted in storage compartments on a small trunk. Each of these units would produce 1,000 DC volts at its terminal. So I soon rolled these into place near my crude apparatus. Insulated wires were attached electrically to each of the plates. The other ends of these wires were attached through a switch to the two terminals of the 1,000 DC battery. I finished most of this that first afternoon. The next morning I spent some time adjusting it and installing a meter to read the volts ap

> plied by the big storage battery. I was then ready to try the battery on these tiny oil drops.
>
> The atomizer was used to spray some of the oil across the top plates. As I looked through the telescope I could see the tiny stream of oil droplets coming through the hole. Again I saw beautiful stars in constant agitation. As soon as I turned on the switch, some of them went slowly up and some went faster down. I was about to scream, as I knew then some were charged negatively and others positively. By switching the field off and on with the right timing one could keep a selected droplet in the field of view for a long time.

Both my father and Professor Millikan realized immediately the importance of this discovery to an understanding of the basic nature of electricity, and he and Millikan received considerable publicity at the time for the discovery. Many noted scientists came to observe the experiment. My father recalled the visit of Charles P. Steinmetz, the "wizard" from General Electric Company (Autobiography, pp. 33-34):

> He was one who did not believe in electrons. He could explain all the electrical [phenomena] in terms of a strain in the "Ether." After watching these little oil droplets most of one afternoon, he came and shook my hand and said, shaking his head, "I never would have believed it. I never would have believed it," and then left.

A paper describing the experiment and its results was prepared and published in *Science* (September 18, 1910). My father described his part in the preparation of this pioneering paper as follows (Autobiography, p. 35):

> I wrote more of it than he did, particularly about the modification of Stokes Law and the arrangements of the data. He went over it all and changed the phrasing somewhat to make it read better. All the time I thought we were to be joint authors.

However, the paper bore only the name of Professor Millikan. My father describes how this came about (Autobiography, p. 36):

> Phyllis was born May 21, 1910, and as you will see that is about the time we finished this paper. When she was about one month old, I was babysitting with her, as Lorena had gone out somewhere with some of her friends there. Answering a knock I went to the door and was surprised to see Professor Millikan. I wondered why he had come to our humble apartment. I soon found it was to decide who was to be the author of the paper referred to above. There were four other papers in the formative stage that were coming out of these oil drop experiments, and I expected they would all be joint papers.
> He said that if I used a published paper for my doctor's thesis that I must be its sole author. The five papers on which we did the experimental work together were:

1. The Isolation of an Ion, a Precision Measurement of its Charge and the Correction of Stokes Law. Science, September 30, 1910—Millikan.
2. Causes of Apparent Discrepancies and Recent Work on the Elementary Electrical Charge. Phys. Zeit, January, 1911—Millikan and Fletcher.
3. Some Contributions to the Theory of Brownian Movements, with Experimental Applications. Phys. Zeit, January, 1911—Fletcher.
4. The Question of Valency in Gaseous Ionization. Phil. Mag., June, 191 1—Millikan and Fletcher.
5. A Verification of the Theory of Brownian Movements and a Direct Determination of the Value of Ne for Gaseous Ionization. Phys. Rev., August, 1911—and Le Radium, July 1—Fletcher. This was my thesis.

> It was obvious that he wanted to be the sole author on the first paper. I did not like this, but I could see no other out and I agreed to use the fifth paper listed above as my thesis and would be listed as the sole author on that paper.

Although my father was disappointed at not being included as a joint author on the pioneering paper, he had no ill will against Professor Millikan. He said (Autobiography, p. 36):

> People have frequently asked me if I had bad feelings toward Millikan for not letting me be a joint author with him on this first paper. . . .My answer has always been no. It is obvious that I was disappointed on that first paper as I had done considerable work on it, and had expected to be joint author. But Professor Millikan was very good to me while I was at Chicago. It was through his influence that I got into the graduate school.

> He also found remunerative jobs for me to defray all my personal and school expenses for the last two years. Above this was the friendship created by working intimately together for more than two years. This lasted throughout our lifetime. Remember, when we worked together he was not the famous Millikan that he later became. When he wrote his memoirs shortly before he died, he had probably forgotten some of these early experiences.[2]

H. D. Arnold was also a graduate student at the University of Chicago working under Professor Millikan at the time of the oil drop experiment and was acquainted with the new knowledge uncovered about Stokes Law. Researchers at the American Telephone & Telegraph Company and the Western Electric Company became very interested in the research being done at the University of Chicago and offered my father a position with them, which he refused. The position was then offered to Arnold, who accepted, and within three years, building on the research at Chicago, developed the Arnold vacuum tube. My father said later, somewhat wistfully, "It might have been my invention instead of his, if I had accepted the job offering there."

Instead of going to the Bell System, my father returned to Provo in 1911 to resume his teaching career at Brigham Young University. While equipment and facilities were more limited than at Chicago, he persisted in his research with the oil drop and built another apparatus to continue his measurements. He published two more papers in this field while at Brigham Young University.

Teaching performance is always difficult to evaluate, but from conversations I have had with some of his former students, my father was an innovative and inspiring teacher. He lists as some of his outstanding students from this period the following:

Vern O. Knudsen, chancellor of University of California, Los Angeles

A. Ray Olpin, president of the University of Utah

Dilworth Walker, head of Business College, University of Utah

Wayne B. Hales, chairman of Physics Department, Brigham Young University

Joseph Nichols, chairman of Chemistry Department, Brigham Young University

Milton Marshall, chairman of Physics and Math Department, Brigham Young University

Carl F. Eyring, dean of College of Arts and Sciences, Brigham Young University

By the time my father finished a five-year teaching stint at Brigham Young University, his family had increased to five—he and his wife and three children: Phyllis, born in Chicago, and Stephen and Charles, born in Provo.

Every year the offer from the Bell System to join its research effort was renewed. And, because of his loyalty and commitment to building the physics faculty and attracting potential scientists to Brigham Young University, every year the offer was rejected. But after awhile my father began to feel that he was not realizing his full potential as a research scientist at Brigham Young University. So after much soul searching he accepted the offer made in 1916 and moved his family to the New York City area, where he began work in the research and development department of the Western Electric Company.[3]

Entering the Bell System organization started a new chapter in my father's scientific life. This was a relatively large organization with specialists in many diverse fields. A good part of the organization was spending its time helping the United States gear up its defense effort as World War I continued to drag on in Europe and the United States came closer to being involved. He was too new to the sys tem to make much of a contribution to this war effort, although he did devise a capillary microphone that worked under water.[4]

My father's first year with the Bell System was spent getting acquainted. He climbed telephone poles, operated switchboards, and installed telephones. At the end of the year he began formulating a plan for research that he felt was needed by his new employer. Since the business at that time was largely the transmission of speech from the mouth of one person to the ear of another, he felt that a thorough understanding of the characteristics of speech and hearing should be fundamental to the Bell System. He thus embarked on a study of the physical mechanisms of the body used in this form of communication. I can remember as a boy seeing my father with a bellows under his arm connected by a tube to a vibrating reed, which he placed in his mouth to form various speech sounds. He would repeat them until he was sure he understood the position of the tongue and lips that characterized the sound. His first publication on this new research was "On the Relative Difficulty of Interpreting the English Speech Sounds," appearing in *Physical Review,* in August 1920. At Chicago he had been elected a member of the American Physical Society. After his move to New York, meetings were more accessible, and he became a frequent and active participant, eventually becoming its president in 1945.

Many of his friends who knew of his important work in the measurement of the electric charge attempted to dissuade him from leaving that promising field to research the much more prosaic field of acoustics. They warned him that all there was to know about acoustics had already been discovered. However, he felt the same sense of commitment he had experienced at BYU urging him to explore acoustics and more specifically the production, transmission, and recording of sound.

The tools that the modern acoustical beginner uses, such as oscillators and amplifiers, were just being developed. The rudimentary methods for testing hearing extant in those early days are illustrated by an anecdote my father told (Autobiography, pp. 51-52):

> The very wealthy Duponts had a history of deafness in their family. One of them, Alfred Dupont, was a friend of Mr. Gifford, president of AT&T Company. Mr. Gifford asked me if we could help his hearing. So I made an appointment to see him. By this time we had oscillators, amplifiers and attenuators, so we could produce a known frequency and intensity of a tone. In other words we had the essential elements of what later was used as an audiometer.
>
> When Mr. Dupont came to my office I asked him about his hearing and he said his ability to hear fluctuated. It was better after a treatment from a doctor he had been going to frequently. But as soon as he got home it returned to the same old level. I then asked him to tell me what was the treatment. The treatment was an X-ray beam directed at his ear. I became very skeptical and asked him if I might accompany him for one of these treatments and he consented. Before leaving the laboratory we made a careful audiogram of his hearing with our embryo audiometer. The doctor's office was in a fashionable part of Manhattan. We took an elevator to the 5th floor. The elevator opened into a large room which was filled with all kinds of electrical gadgets. Large Whimhurst and static machines with large brass balls across which large sparks would fly, X-ray machines and giessler tubes, etc. The doctor said he would test Mr. Dupont's hearing before the treatment and then after the treatment. The following was the method used in the test.
>
> There was a path along the floor marked off in feet. It was about 20 feet long. Mr. Dupont was asked to stand at one end of this. The doctor stood at the other end and said in a very weak voice, "Can you hear now?" Mr. Dupont shook his head. He kept coming closer and asking the same question in the same weak voice until he came to about two feet from his ear, where he said he could hear. His hearing level was found to be two feet.
>
> Mr. Dupont then was asked to stand four or five feet in front of an X-ray tube with his ear facing the tube. The X-ray was turned on two or three

> times. He then turned his other ear toward the tube and had a similar treatment. He then stood in the 20 foot path and another hearing test was made. But this time as he started to walk toward Mr. Dupont he shouted in a very loud voice. "Do you hear me now?" As the doctor reached the 10 or 15 foot mark, Mr. Dupont's eyes twinkled and he said he could hear.
>
> I could hardly keep from laughing because what happened was so apparent. The intensity of the loud voice from 15 feet was just the same as the weak voice at 2 feet. The doctor must have been aware of this but the patient wasn't. Mr. Dupont was quite elated until we got back to the laboratories to make another audiogram and it turned out to be exactly the same as the one made before he had the treatment. I explained to Mr. Dupont what had happened. This was tried again in about a week with the same result. After that Mr. Dupont never paid a visit to this doctor.

Although my father is credited with the invention of the audiometer, he pointed out that, while the 2-A audiometer was patented in his name, it was not the **first** audiometer. There was a device made by Robert H. Seashore, dean of the Graduate School of Iowa State University, and also one by Lee Wallace Dean and C. C. Bunch at the same school. There was also a contemporaneous work on audiometers by Vern Knudsen at UCLA and Dr. Jones, a practicing otologist in California, and a preceding audiometer in the Bell System—the 1-A devised by R. L. Wegel and E. P. Fowler.

However, it was the 2-A audiometer that proved most practical and was used most thereafter by audiologists and schools for hearing measurement.

My father became well known for his work for the hearing handicapped. As a consequence, he met many interesting and some famous individuals. He tells this story of a meeting with Thomas A. Edison (Autobiography, pp. 62-64):

> He came to the laboratory with one of his Chief Assistants. We made tests of his hearing and found that he was very hard of hearing. If one talked very loudly into his ear he could understand. His hearing loss was about the same for high and low frequencies, which is very unusual for one

having as great a hearing loss as he had. The fact that he could understand speech when one shouted into his ear encouraged me to think we could make a hearing aid with which he could hear ordinary conversation and we agreed to do this. The necessary equipment was placed in a box about 16 x 8 x 8 inches; the microphone was on the end of a cord and [the] headpiece on a head band. Instead of a battery for a source it had [a] power pack which operated on AC current.

When I took this hearing aid over to his office I had this interesting experience. When I came into his office he greeted me with the following, "I understand you are a fine mathematician." I admitted that I had studied a little mathematics. He then replied, "You know I started to study mathematics, but I got only as far as Algebra. When I struck the plus and minus signs in this, I became all confused and quit. And I haven't done anything with it since." This shows that at least in his day the lack of mathematical ability did not handicap inventability. He was a fantastic inventor but no scientist.

While I chatted with him in his office, I noticed that he had one of these old fashioned roll top desks. There were small drawers in the top part having labels on them. I noticed one was labeled taxes, another coal bills, and other such items like these. I was very much surprised that a man as noted as Edison did not delegate such matters to someone else. I understood from some of his close associates that he would not let anybody else do the work. I also noticed that he had a peculiar elevator connecting his three or four floors in his laboratory. He would not let an elevator company install a modern one. He made the one that they were using.

Then he said, "I understand you have a hearing aid there, that might help me." I said, "Yes, we would like to try it." Then he said, "I hope the gadget does not work on AC current because we have none of it in the place." As he said this one of his assistants in back of him started to wave his hands at me and whispered, "Yes, we have but the old man doesn't know it." So I only answered, "I guess we can make it work." It was well known that he had a great controversy concerning AC and DC especially for running street cars and always insisted on using DC. Because of this, the majority of the old hotels around New York were nearly all wired for DC and the street cars were using DC. We found he could hear very well with the hearing aid and he used it for a long time after that. He took it to some of the dinners at which he was a special guest. He was a much wanted celebrity. A few years later I met him and asked how it was working. He said it was O.K. "When I used to attend these dinners I sat in silence wondering what the after-dinner speaker was saying and wishing I could hear him, but I was content to turn my thoughts toward some of my inventions. But now with the hearing aid I can hear and understand the speaker but usually find it so dull I turn it off and turn my thought to my inventions." He did tell me the story of how he lost his hearing.

He said when he was just a boy he worked on a railroad train in the express postal car. While the train was stopped he was fooling and kidding the other man in the car who was much older and stronger than he was. Edison played some prank on the older man. To get quickly away he jumped out of the car door to the ground. The older man grabbed at his head and caught both ears and pulled Edison back up into the car. Edison felt a terrific pain in both ears and knew something had ruptured. Since then he had been hard of hearing. This is the story that Edison told. This incident may have been a primary cause, but there must also have been a secondary cause that acted upon the auditory nerve, then or later, to produce the profound deafness that he had. He said it had not handicapped him very much, particularly when it was necessary to solve a perplexing problem.

I had another experience with Edison as follows. His first assistant (the foreman) in the research laboratories came to my office one day and said he would like to have an audiogram of his hearing to compare it with Edison's. He said Edison insisted upon passing upon every phonograph record that went into stock for sale. He would hold one end of a morning glory horn against the phonograph loud speaker. The other end was fitted with a rubber tube which he fitted into his ear. In this way he passed or rejected every record that was sold. The foreman said, "You know and I know that he doesn't hear these records like a person with normal hearing, but I just cannot persuade him to let us do this inspection work. Now, if you make an audiogram for me, we will compare it with Edison's audiogram which you made and probably this will convince him." Well, I made the audiogram for him. It was normal through most of the speech frequency range but dropped very badly above 2000 cycles. Edison's audiogram was much lower but about the same through the whole frequency range. The foreman sort of frowned when he saw the audiogram but thanked me and left. I think he discussed the audiogram with Edison, but I think Edison kept on testing the records in the same old way.

My father's work in aiding the hearing handicapped was not confined to his work on the audiometer; he also worked to improve hearing aids. Mr. DuPont, after his disillusionment with his audiologist, pleaded for something to help his hearing at meetings of his Board of Directors. He asked my father for assistance, and a binaural hearing set was devised. Two microphones were placed in the middle of the board-room table, and the telephone receivers were placed in a headband for Mr. DuPont to wear. The amplifiers, transformers, and condensers were housed in a cabinet under the table. The binaural system enabled him to sense the direction of the speaker, as well as to clearly understand the speech. This was the first hearing aid using vacuum tubes and was in use for many years in the DuPont board room. It was not practical for ordinary use, since it carried a price of $5,000. However, it did lead to the manufacture of simple monaural versions by Western Electric Company.

In 1921 my father began publication of some of his findings about speech and hearing. The first article was "The Frequency Sensitivity of Normal Ears," published in the *Proceedings of the National Academy of Sciences,* November 1921. This was followed by numerous articles throughout the 1920s and culminated in the publication of his book, *Speech and Hearing,* in 1929 by D. Van Nostrand & Company. This book became widely used throughout the world as the definitive work in the field. A review of the book in the *Journal of Speech and Hearing Disorders* stated:

> His book is an embracing summary of a unified research program without equal in the field. The description it offers of the physical characteristics of speech and the analysis it presents of auditory phenomena, particularly in regard to the hearing of speech, have become classical. . . .The intervening years have brought added respect for both the stability and the scope of the pioneering contributions made by the author and his coworkers.

By the mid-1920s, my father had attracted enough attention to the importance of his research work in acoustics that a small group in the research department of Western Electric Company was formed, and in 1928 he was made director of acoustical research, part of the Physical Research Department in the newly formed Bell Telephone Laboratories. By 1935 he was made director of all physical research at Bell Laboratories.

He was also attracting attention outside the Bell System. In 1920 he was elected a fellow of the American Physical Society and in 1921 a fellow of the American Association for the Advancement of Science. In 1922 he became a member of the American Institute of Electrical Engineers and in 1926 a member of the Board of Managers of the American Federation of Organizations of the Hard of Hearing. For the latter organization, most of whose members were hard of hearing, he devised a multiple hearing aid, so that attendees at meetings of the federation could hear. He described the first use of this equipment as follows (Autobiography, p. 57):

> As I remember it small strips were made with five or six jacks and attenuators wired in parallel. Any number of these strips could be connected together by simply plugging connecting cords into the end jacks. There were about 20 of these strips so that about 100 head pieces could be connected in parallel to the transformer coming out of the amplifier. These were arranged according to the seating in the hall. As the hard of hearing person sat down, he could reach for a head set and then turn the attenuator until he obtained the loudness with which he could best understand. The first installation was at Hotel Astor at the annual convention. When everybody became quiet for the beginning of the meeting, they asked me to talk and explain how to use these sets. Some of the people could hear and helped the others to adjust the head bands. When they all had finished fussing with the head band I started to talk slowly in a normal volume and greeted them and told them to adjust the attenuator if they needed more volume. Most did not but sat with tears running down their

> cheeks crying, "I can hear! I can hear!" It was the first time most of them had ever heard a public speech. After they quieted down I turned the microphone over to the president and he carried on the conference. It was a memorable occasion and these hard of hearing persons never forgot it. The Bell Labs donated the system to the national organization and it was used for many years after that time. The officers of the Federation took their group hearing aid all over the USA for meetings in various localities.

In 1929, my father was elected president of this organization.

In the same year he was asked by Mayor James Walker of New York City to be a member of the city's Noise Abatement Commission, headed by C. C. Burlingham. This commission studied and measured the sources of noise in the city and issued a report on their findings. Although many of the sources of noise identified in the report were thought to be unavoidable, steps were taken to eliminate or ameliorate some of them, and antinoise ordinances were adopted by the city.

In May 1929 the Acoustical Society of America was formed, and my father became its first president. This brought together teachers, engineers, and others interested in the broad field of acoustics. The first issue of the *Journal of the Acoustical Society* was published in 1929. In recognition of his work in the field, my father was named an honorary member of the Acoustical Society in 1949. At that time, only Thomas A. Edison had been awarded such an honor.

Most of those who are speech handicapped have hearing difficulties. However, it came to my father's attention that some individuals, who had learned to talk normally, were deprived of this ability by the removal of the larynx, usually as a result of cancer surgery. Since my father had already devised a vibrating reed mechanism to study speech sounds, it was a relatively simple matter to devise an artificial larynx to help those who had lost their larynx through surgery.

The Bell Systems, therefore, manufactured and offered for sale to such individuals an artificial larynx. My father received much publicity around the world for this invention, and, though designed for a relatively small class of handicapped people, it was almost miraculous to those who regained their speech through it. I remember a chance meeting with such an individual while with my father on a tour. The gratitude shone in his eyes as he thanked my father over and over again through his artificial larynx for making the resurrection of his speech possible.

Another outgrowth of this basic research into acoustics was the development of recording and reproductive apparatus that would faithfully transmit the entire audible frequency range. This made it possible to synchronize sound with a motion picture and produce talking pictures to take the place of the prevalent silent ones. When the motion picture companies were ultimately convinced that the public wanted talking pictures, my father was besieged with attractive offers from Hollywood to help engineer their introduction, but he resisted all offers and remained with the Bell Telephone Laboratories. However, for many years most of the sound pictures were made under Bell System patents. Initially, these were projected on narrow screens already in use in the theaters, and no attempt was made to give them a spatial effect, but my father knew that a spatial effect could be created by the use of a binaural system, such as he had provided to Alfred DuPont for his board meetings.

My father was prepared to demonstrate this spatial effect, which he initially called auditory perspective but that has become better known as stereophonic sound or simply stereo. The principal exhibit of the Bell System at the 1932 World's Fair in Chicago was such a demonstration. Twenty or thirty earphone headsets were placed in a semicircle around a glassed-in stage, where a dummy called Oscar sat. Microphones were placed in each ear of the dummy and were connected to the corresponding receivers in each of the headsets. A person would be walking around the dummy on stage and talking. The glass prevented any communication through the air, but those individuals who had the headsets on were startled. It seemed to them that someone was walking around them and talking to them. They would turn and look over their shoulders to see who it was.

At about this time, my father contacted Leopold Stokowski, conductor of the Philadelphia Orchestra, concerning the possibility of utilizing a stereophonic sound system in a concert of the orchestra. He had approached many of the conductors of the leading orchestras of the time as to the feasibility and desirability of such a demonstration, but he had received a very cool response. Only Mr. Stokowski was enthusiastic. My father described these early tests in his autobiography (Autobiography, pp. 90-93):

> It was about 1931 when I first met Stokowski and we made tests of stereophonic sound down at the Academy of Music in Philadelphia where the Philadelphia Orchestra held its concerts. There was a spare room in the Academy building which was large enough to house the orchestra so that we could have them play on the stage and listen to it up in this large room. In this way we tried experiments until we felt we had developed a stereophonic system. Originally, the theory of this system was that it should have an infinite number of loud speakers at one end and the same number of microphones at the other end. However, we found that in stage productions, three microphones, three transmitting lines and three loud speakers were sufficient. I'll not go into the details of the development work that was necessary to produce this. There are several printed papers on it. However, we did make nine loud speakers expecting that we might have to use three across and three up and down. However, we found that most of the action was horizontal and consequently, three loud speakers were sufficient.
>
> For this demonstration the Philadelphia orchestra was in Philadelphia. It was conducted by the Assistant Director Smallen, and Mr. Stokowski was

at the controls in Washington, where the orchestra pieces were reproduced. I'll not go further into the details except to say that there was a very select audience there. The Cabinet to the President, Senators, and Representatives, and other officials of the Government were invited guests, and it was under the direction of the National Academy of Sciences. The President of the Academy at that time was Dr. W. W. Campbell. He introduced me, and I will quote from the little booklet which tells about this. "With the assistance of the orchestra in Philadelphia, Dr. Fletcher then performed several experiments to demonstrate the important characteristics of the new apparatus. On the stage of the Academy of Music in Philadelphia where the pickup microphones were installed, a workman busily constructing a box with a hammer and saw was receiving suggestions and comments from a fellow worker in the right wing. All the speech and accompanying sounds were transmitted over the cable circuits to the loud speakers on the stage of Constitution Hall in Washington. So realistic was the effect that to the audience the act seemed to be taking place on the stage before them. Not only were the sounds of sawing, hammering, and talking faithfully reproduced correctly, but the auditory perspective enabled the listeners to place each sound in its proper position, and to follow the movements of the actors by their footsteps and voices. For another demonstration the audience heard a soprano singing 'Coming Through the Rye,' as she walked back and forth in an imaginary rye field on the stage in Philadelphia. Here again her voice was reproduced in Washington with the exact auditory perspective. The singer appeared to be strolling on the stage of the Constitution Hall.

An experiment which demonstrated both the complete fidelity of reproduction and the effect of auditory perspective was performed by two trumpet players, one in Philadelphia at the left of the stage of the Academy of Music, and the other in Washington at the right of the stage of Constitution Hall but invisible to the audience. Alternately they played a few phrases of the same selection. To those in the audience there seemed to be a trumpet player at each side of the stage before them. It was not until after the stage was lighted that they realized only one of the trumpet players was there in person. The music of the other was transmitted from Philadelphia with such perfect fidelity and reproduced into such true prospective [sic] that it was impossible to tell that one of the players was absent. The auditory perspective effect is not restricted to placing sounds in their correct position across the stage but is 3-dimensional. This was shown by having several sources of sound move around the stage in Phila

> delphia. Not only back and forth but high up in the center of the stage as well. The movement of each sound was faithfully reproduced by the loud speakers in Washington. Even when the sounds were carried high above the level of the floor." I think that's sufficient to indicate what this first demonstration was. I might say that it was a tremendous success.

This demonstration was with a live orchestra, instrumentalists, and singer. The next step was to record and reproduce stereophonically. This was accomplished and demonstrated in 1939 in Carnegie Hall. I was in the audience for this demonstration, and I remember vividly one aspect of it. A dancer appeared before the curtain in the middle of the stage and began a tap dance. After a few minutes, the dancer danced off to the left apparently still tapping, but the sound of the tapping went off to the right, confusing the audience. The sound was actually coming from a recording, not from the live dancer.

I also remember that the volume on some of the orchestral numbers became so high that, not the rafters, but some of the fixtures in the old building began to ring.

This demonstration was repeated at the Eastman School of Music and in a Hollywood theater.[5] The records of these early demonstrations of stereophonic sound have been preserved at Columbia University. They are on film rather than the magnetic tape or metal disks currently in use in the recording industry. It was on the basis of these demonstrations and the scientific papers describing them that my father came to be dubbed the "Father of Stereophonic Sound."

As noted above, in 1933 my father's responsibilities at Bell Laboratories were broadened to include all physical research. As such, he supervised the work of William A. Shockly, Walter H. Brattain, and John Bardeen in the development of the transistor; Dean Wooldridge in the development of magnetic tape; James Fiske; Charles Towne; and other outstanding physicists.

Just as Bell Laboratories had recognized his organizational ability and put him in charge of physical research, outside organizations in the scientific community were recognizing and calling on the same ability. In 1931 my father was elected a member of the Executive Committee of the American Institute of Physics, which he had helped organize. In 1933 he was elected to the Board of Directors of the American Association for the Advancement of Science. In 1945 he was elected president of the American Physics Society. In 1947 he was appointed to the Committee on Hearing, Division of Medical Sciences, National Research Council. In 1948 he became a member of the National Research Council assigned to the Division of Engineering and Industrial Research for a three-year period. In 1949 he was appointed to a three-year term as a member of the Standing Committee on Meetings of the National Academy of Sciences. He had been elected to membership of the Academy in 1935.

During the period of U.S. involvement in World War II, from 1941 to 1945, my father was again called upon to work for the war effort. He was very circumspect about his involvement. All he states in his autobiography is (Autobiography, pp. 103-104):

> During the war period, my research was in the general field of acoustics and was under the general supervision of O.S.R.D. I had charge of groups working at:
> Harvard University,
> California Institute of Technology,
> University of Pennsylvania,
> North Carolina State College,
> Rutgers University,
> Stevens Institute of Technology,
> and three or four groups at the Bell Labs.
> Most of this work was secret and a group effort. I received several citations for this work, one signed by President Truman.

It is now presumed that his principal work involved the detection and identification of underwater sounds.

After the war my father devoted most of his efforts to fulfilling his various executive responsibilities, but he also worked strenuously to complete two projects before his impending retirement from Bell Laboratories. The first was "Empirical Theory of Telephone Quality," a project that had captured his attention off and on for twenty-five years. The second project was "The Dynamics of the Cochlea." This research, and other developments since the original publication of *Speech and Hearing* in 1929, were incorporated into a new book brought out in 1953, *Speech and Hearing in Communication,* also published by D. Van Nostrand.

Upon retirement from Bell Laboratories at the age of sixty-five, my father accepted a position as professor of electrical engineering at Columbia University, a position he held for three years. During that time he organized a department on acoustics and persuaded Bell Laboratories to donate some of the stereophonic apparatus, which had been used in his demonstrations, to Columbia. Cyril Harris, who worked with my father during these years, succeeded him as head of the acoustical engineering department at Columbia and made many valuable contributions to the field of acoustics.

My father moved again in 1952. He accepted a position as director of research at his old school, Brigham Young University. He was able to increase the amount of grants for research being given the university and to get the university to use part of these grants to fund the salaries of faculty members and students engaged in the various research projects.

In 1953, at the behest of the president of the university, my father began organizing an engineering course for the university and became chairman of the Department of En gineering Science. This ultimately became the College of Physical and Engineering Sciences. In 1958 he was able to induce Armin J. Hill to be dean of this college, and thus relieved him of the administrative responsibility. After two years, during which he taught mathematics at the university, he again turned his attention to research. From 1960 until his death in 1981, he actively pursued his study of musical tones. He had a vision that, as more was learned about the characteristics of the various musical instruments, completely new instruments would be invented, which would broaden the listening pleasure of music lovers. He analyzed the sounds of the piano, organ, cello, violin, and bass drum, etc.

Although intensely interested in his scientific pursuits, my father did not neglect his recreational opportunities. As a boy he was an ardent fisherman, and this interest lasted throughout his life. He enjoyed all sports, playing tennis and basketball and watching other sports. He enjoyed fine music. I remember as a boy listening to Metropolitan Opera stars, such as Caruso and Galli-Curci, and instrumentalists like Heifetz on our old phonograph. He was active in promoting camping and boy scout activities.

My father was also a devoted family man. He and my mother, Lorena, who was named American Mother of the Year in 1965, had seven children, six of whom lived to maturity. Of the six, he inspired five to follow in his footsteps. His daughter, Phyllis, worked on the editorial staff of the American Institute of Physics. James, Robert, and Paul all earned Ph.D.s in physics, and Harvey J. earned a Ph.D. in mathematics. All have had successful careers in teaching and in industry and government. Although I was the oldest son, and therefore somewhat closer to my father than the younger children, I did not pursue a scientific career, but with his help and encouragement became a lawyer.

After my mother's death in 1967, my father married her sister, Fern Eyring. This marriage lasted until 1981, when both died.

A memoir of Harvey Fletcher would be incomplete without mention of the inspiration he gave his children, his students, his associates, and the thousands who have read and still read his publications. He had a deep religious conviction, which he expressed in books and articles and in countless sermons. Indeed, his life was so full and rich that he wrote a manual for Latter-Day Saint Sunday schools, entitled *The Good Life* (1963). He never felt any insolvable conflict between science and his religious beliefs. He will be remembered fondly by those who knew him and with respect by those who know of his contributions.

He was a working scientist until the very year he died at the age of ninety-six. The curiosity that propelled him as a boy, a young student, a researcher, and leader of others continued to the very end. His influence will continue to be felt for years to come by those who knew him and for those following in his very large footsteps.

NOTES

1. Autobiography of Harvey Fletcher. Unpublished.

2. My father was reluctant to publish anything related to his part in the now famous oil drop experiment for fear that it might detract in some measure from the credit given Professor Millikan, whom he admired and esteemed as a friend. This is the principal reason that his autobiography was not published. After my father's death, an article by Mark B. Gardner, an associate at Bell Telephone Laboratories, concerning the oil drop experiment was published in *Physics Today,* June 1982, pp. 43-47.

3. In 1927 this department was combined with the much smaller research and development group at AT&T to form Bell Telephone Laboratories.

4. A detailed description of this device is to be found in his autobiography (Autobiography of Harvey Fletcher, unpublished, pp. 48-49).

5. The Mormon Tabernacle Choir's music was also recorded in stereophonic sound for use in these demonstrations.

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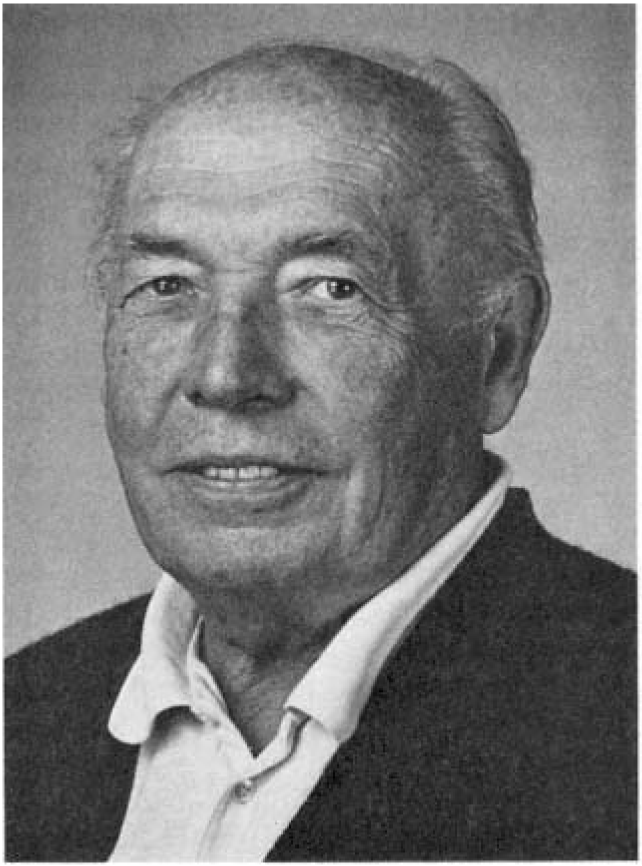

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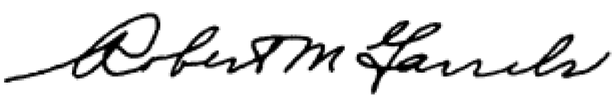

Robert Minard Garrels

August 24, 1916-March 8, 1988

by Robert A. Berner

With the passing of Robert M. Garrels, the world has lost a unique individual. He is among the handful of persons that over the past half century truly altered the course of geochemistry, which was his specialty, as well as that of earth science in general. Hidden within this modest, affable, kind, and considerate man was the soul of a revolutionary, and it is the hope of this biographical memoir to document the revolution that he led.

Bob Garrels was born in Detroit, Michigan, on August 24, 1916, the second of three children of John Carlyle and Margaret Anne Garrels. His father was a successful chemical engineer who, in his youth, was an outstanding athlete both as an All-American football player and as an Olympian who placed second in the 110-meter hurdles and third in the shotput in the 1908 Olympics. (Can you imagine the same individual being able to successfully compete in both events today?) Bob inherited the love of athletics from his father and was athletically active all his life until he was felled by cancer during his last year. In fact, for a few months Bob was the holder of the world's high-jump record for fifty-seven-year-old men.

Bob's childhood years, from age six through twelve, were spent in Saltville, in the mountainous southwestern part of

Virginia, where his father worked for a chemical company that used local salt and limestone as raw materials. His boyhood was spent mainly in outdoor activities such as hunting, exploring, and swimming. The formative factors that motivated him toward a scientific career are best summarized in his own words taken from his unpublished autobiography (written for the National Academy). He states (words in brackets are my additions):

> There were three factors, I think, that pushed me toward a scientific career. First, of course, my father's interest; second, the richness of the area [southwestern Virginia] in natural lore. The presence of salt deposits attracted animals, and fossils of Pleistocene forms were often uncovered at construction sites. The rocks of the hills surrounding the town contain abundant Paleozoic fossils. The third factor was the presence of James Moore, a middle-aged bachelor who was a first-rate amateur astronomer, and who delighted in teaching me and my friends about the universe.

At age twelve, Bob and his family moved to Grosse lie, Michigan, where he attended high school. Although he excelled in mathematics and chemistry, his principal interests seemed to be reading and athletics. Also, his mother saw to it that he became a "reasonably good pianist." He mentions in his autobiography that his first scientific thrill came from building a crude stroboscope to determine the rate at which an automobile wheel was turning. He used the device to win a prize at a Detroit department store for the person who guessed the number of turns made by the wheel in a shopping day. In fact, he was able to show that the department store was not holding the rotation rate constant, thus demonstrating scientific skepticism at an early age.

Garrels entered the School of Liberal Arts at the University of Michigan in 1933. Although his original intention was to become a chemist like his father or a novelist, by the end of his sophomore year because of a bad teacher he hated chemistry (this attitude would change in graduate school) and realized that creative writing was not his forte. This led to geology, of which he knew little other than the fact that he loved the outdoors and had a general inclination toward science. He enjoyed majoring in geology and graduated with a B.S. degree with honors in 1937.

His graduate school experience is best summarized in his own words (and autobiography):

> In 1937 I entered the Graduate School at Northwestern University, only because they needed a teaching assistant at $50 per month and the best job I could find paid $75. The Department of Geology at Northwestern was small but excellent; my fellow graduate students were compatible, competitive, and capable. I soon ran out of geology courses, and took chemistry courses to fill in my program; to my amazement [considering his undergraduate experience] I found them fascinating and useful. Professor John T. Stark of the Department of Geology was a tremendous influence on me during my graduate years, and remains to this day my best friend. He was very close to the students, and a man of remarkable intellectual breadth. His teaching method was that of being the Devil's advocate, and I have adopted and used it with great pleasure. He gave me the first real discipline of my career; no statement went unquestioned, and I began to try to apply some of the principles relating to methodology and epistemology, that I had learned in [undergraduate] philosophy [courses], to earth science.
>
> Professor C. H. Behre, Jr. was my sponsor for a thesis. He suggested a geochemical problem related to the formation of lead and zinc deposits, and arranged with Professor F. T. Gucker, Jr. of the Chemistry Department for my guidance in the laboratory aspects of the work. To Gucker I give the credit for any ability I may have in chemistry. We met regularly, and each week he suggested what I should accomplish. His standards were high, he was sympathetic but firm, and I worked harder than I have before or since. He led me into problems that required more chemical training than I possessed, and somehow made me understand that it was unthinkable for me not to solve them. So, of course, I did.

Garrels received the M.S. degree from Northwestern in 1939, based on a thesis concerning some iron ores of New foundland that he had studied during the summer of 1938. In 1941 he received the Ph.D. degree; the thesis research was largely a laboratory study, using electrochemical techniques, of complex formation between lead and chloride ions in aqueous solution. The thesis won the Sigma Xi award for the best one submitted that year at Northwestern. He presented his results, in the broadened context of the role of complex ions in the transport of metals in underground waters, to the Boston meeting of the Geological Society of America. This was his first scientific contribution and one of a pathfinding nature. Unfortunately, as he once told me, almost no one attended his talk because its significance was unappreciated by geologists at that time.

After obtaining his Ph.D., degree Garrels stayed on at Northwestern as a replacement for Charles Behre, who had left. Then in 1944, due to the war he became affiliated with the Military Geology Unit of the U.S. Geological Survey in Washington. Shortly thereafter he was assigned to the Beach Erosion Board of the Corps of Engineers in Hawaii, working on beaches that were to be invaded by the U.S. military attempting to gain control of Pacific islands from the Japanese. He once confided to me that had it not been for the atomic bomb dropped on Japan he would have had to do *on site* mapping and reconnaisance to prepare for a U.S. invasion of the Japanese home island of Honshu, a very dangerous task.

After the war, in 1945, he returned to Northwestern and taught there until 1952, holding the positions of assistant and then associate professor. He continued his research on ore deposits and taught introductory geology and introductory physical science. His teaching of geology led to the writing of a book, *A Textbook of Geology,* published in 1951. This underappreciated book was not a big seller (because it contains liberal doses of physics and chemistry) but demonstrated Garrels's original approach to earth science and his unusual propensity for being ahead of his time. Some noteworthy papers published by Garrels during this "first Northwestern period" (see Selected Bibliography) were concerned with diffusion in water-saturated rocks, the study of fluid inclusions as a guide to geologic thermometry, activity coefficients of lead and chloride ions in aqueous solution, the origin of Clinton iron ores as revealed by laboratory experiments, and a study of crystal growth. In 1952 he also published, along with W. C. Krumbein, a paper that was to become a classic, "Origin and Classification of Chemical Sediments in Terms of pH and Oxidation-Reduction Potentials." This work was to show how the study of sediments and sedimentary rocks could be approached from a physical-chemical standpoint, and was required reading for anyone (like myself) studying sedimentary rocks during the 1950s. One couldn't think of Garrels without thinking of Eh-pH diagrams.

In 1952, Garrels left Northwestern (he was to return two more times) to become head of the Solid State Group, Geochemistry and Petrology Branch, of the U.S. Geological Survey, where he was to remain for three years. There he concentrated on experimental and theoretical aspects (more Eh-pH diagrams) of uranium and vanadium geochemistry. He states in his autobiography:

> [At the survey] it was a fascinating experience to work with a large number of people on every aspect of geochemistry of just two elements.
> After the University, in which I represented the entire discipline of geochemistry, and in which my collaboration with others was on the basis of other disciplines, it was like changing one's view from 10 fold to 1000 fold magnification.

Bob never liked being an administrator and found that his personnel-related duties at the USGS were getting to be too much. Thus, in 1955 he returned to academic life, accepting the position of Associate Professor of Geology at Harvard University. (He was promoted to professor in 1957.) This, as it turned out, was one of the best appointments Harvard could have made at the time, for it was there that his research and teaching talents flourished. Because of Garrels, graduate students could obtain invaluable training in theoretical and experimental aspects of geochemistry, practically unobtainable elsewhere. I know because I was one of these fortunate graduate students (although Garrels was not my primary advisor; I worked instead under Raymond Siever). Garrels's lab was a hub of activity, with my contemporaries measuring Eh and/or pH in mineral equilibrium experiments (e.g., Paul Hostetler, Owen Bricker, and Don Langmuir), doing theoretical calculations (e.g., Hal Helgeson and Al Truesdell), forcing water through clay packs (Bruce Hanshaw), and turning sulfide minerals into electrodes (M. Sato).

It was while he was at Harvard that Garrels's book *Mineral Equilibria at Low Temperature and Pressure* was published in 1960. This book was one of the first of its kind to show earth scientists how to directly apply chemical thermodynamics to geology with emphasis on the construction of stability diagrams from thermodynamic data. It and its successor, *Solutions, Minerals, and Equilibria* (coauthored with C. L. Christ) have been enormously influential since that time. Proof of this was the designation of the latter book as a Citation Classic, because of its high frequency of citation, by the organization that publishes *Science Citation Index.*

Also while at Harvard, Garrels was involved in a very profitable collaboration with Mary E. Thompson and Ray Siever. Such classic papers were published by these three, in various combinations, as "Oxidation of Pyrite by Iron Sulfate Solutions" (1960,2), "Stability of Some Carbonates at 25°C and One Atmosphere Total Pressure" (1960,3), "Control of Carbonate Solubility by Carbonate Complexes" (1961), and "A Chemical Model for Sea Water at 25ºC and One Atmosphere Total Pressure" (1962,2). This last paper, by Garrels and Thompson, showed how ion-pairing and complex formation could be used to calculate activities of the principal ions of seawater. It has been so influential that it was also cited by *Science Citation Index* as a classic paper.

Eventually Garrels became chairman of the Geology Department at Harvard and again disliked the administrative duties so much that it eventually contributed to his leaving to return to Northwestern in 1965. This time he stayed at Northwestern for four years. It was during this "second Northwestern period" that he undertook a close and fruitful collaboration with F. T. Mackenzie. Garrels's enthusiasm for the ideas of Lars Gunnar Sillen led him to the idea that the chemical composition of seawater is buffered by reactions with silicate and, to a lesser degree, carbonate minerals. This led to publication with Mackenzie of papers dealing with the chemical mass balance between rivers and oceans and the silicate-bicarbonate balance in the ocean. A key concept in this work was that of reverse weathering, whereby fine-grained cation-free silicates (e.g., clays) carried to the oceans are reconstituted by the uptake of cations and silica, resulting in the conversion of bicarbonate to CO_2. Such reactions were needed at the time to explain the chemical balance of seawater composition; however, they have been rivaled subsequently by processes accompanying basalt-seawater reaction.

Also during his second Northwestern period, Garrels was author or coauthor of pathfinding papers on the genesis of groundwaters, the origin of the chemical composition of springs and lakes, and the theoretical treatment of irre versible reactions in geochemical processes, the latter representing collaboration with his former student H. C. Helgesen, who was at Northwestern at the time. At this time Garrels began a long and successful collaboration with Roland Wollast of the University of Brussels. (He had previously met Wollast during a sabbatical in Belgium during 1962-63.) Also, he completed with Fred Mackenzie the book *Evolution of Sedimentary Rocks,* which was published in 1971. In this book, whose major influence is only recently being felt, Garrels and Mackenzie demonstrate the importance of the chemical recycling of sediments by way of weathering, dissolution, erosion, transport, sedimentation, burial, and diagenesis followed by uplift and restarting of the sedimentary cycle. Such sediment recycling is a fundamental concept which was originally emphasized by Hutton but which, until the work of Van Nieuwenkamp and Barth in the 1950s and 1960s, had laid practically dormant for over 150 years.

During the second Northwestern period there came a change in Garrels's personal life. He divorced his wife of twenty-nine years, Jane (Tinen), and married Cynthia A. Hunt in 1970. By then his children by the first marriage, Joan F., James C., and Katherine G., were essentially grown up. Not only was his new wife, Cynthia, an inspiration, but in fact she was eventually a coauthor of two books with him: *Water the Web of Life* (1972,1) and *Chemical Cycles and the Global Environment* (1975,1), the latter also with F. T. Mackenzie. He remained happily married to Cynthia until the end of his life in 1988.

While on the subject of human interest, I'd like to recount a humorous episode relating to the pleasant times our family and I had with Bob and Cynthia in Bermuda during the summers of 1970 and 1971. Bob had been doing research in Bermuda during summers of the previous several years and, as a spare-time activity, had organized the informal group called BBSAC, the Bermuda Biological Station Athletic Club. The BBSAC-ers all ran, swam, rowed, and, after these labors in the heat, enjoyed a few drinks. One Garrelsian rule was that a swim from the raft to the research vessel, *Panulirus,* and back, a distance of about 200 yards, entitled the swimmer to one gin and tonic. There were strong but variable currents in the swimming area, however, and some swimmers complained that a greater effort was expended when the currents were stronger. Garrels immediately realized that the current was not simply additive in one direction and subtractive in the other but was, instead, a true hindrance to performance. After all, the swimmer couldn't return to the raft if the current against him was faster than his still-water swimming speed. Garrels constructed a nonlinear plot of current speed versus extra effort in terms of a "gin and tonic factor," with the swimmer eligible for an infinite number of gin and tonics when the current speed equaled his still-water swimming speed! This episode exemplifies both Bob's clever wit and his ability to bring scientific order to even the most mundane of topics.

In 1969 Bob moved from Northwestern to the Scripps Institution of Oceanography, where he remained for just two years, and then to the University of Hawaii, where he spent another two years. During this time, 1969-74, he continued his collaborative researches with Fred Mackenzie and Roland Wollast and began an additional collaboration with Yves Tardy of the University of Strasbourg on theoretical calculation of the thermodynamic properties of silicate minerals. At both Scripps and Hawaii, he also worked with the late Ed Perry, which culminated in their paper, published in 1974, entitled "Cycling of Carbon, Sulfur, and Oxygen through Geologic Time." This has been a very in fluential paper which spawned a number of studies in the 1980s on the evolution of atmospheric oxygen.

In 1974 Garrels returned to Northwestern for the third time (actually the fourth time if you include World War II) and remained there until 1979. This constitutes his third, or last, "Northwestern period." During this time he continued his previous collaboration with Mackenzie, Wollast, and Tardy and added Abraham Lerman, of Northwestern, as an additional collaborator. With Abe's expertise they constructed computer models for geochemical cycles with emphasis on phosphorus, carbon, and sulfur. By means of some ingenious reasoning, Garrels, Mackenzie, and Lerman were able to predict that the sulfur and carbon isotopic records for Phanerozoic rocks should essentially correlate with one another, and this was later found to be correct.

The last phase of Garrels's career began in 1979, when he was appointed to the Marine Science Department of the University of South Florida at St. Petersburg with the title of Research Professor, and was present there from 1980 until his death in 1988. Although by now he was well into his sixties, his mind and accomplishments hadn't diminished a bit. I had the good fortune of collaborating with him when he visited Yale University during the fall of 1979 and several summers thereafter. (He held then the position of Adjunct Professor at Yale.) His enthusiasm for geochemical cycling was contagious, and he drew me into this field. Together the two of us, along with Antonio Lasaga, also of Yale, published a paper in 1983 entitled "The Carbonate-Silicate Geochemical Cycle and Its Effect on Atmospheric Carbon Dioxide over the Past 100 Million Years." In this paper we tried to show how one might calculate the change of atmospheric CO_2 over time, how it was affected by plate tectonics, and how CO_2 could have acted as a "greenhouse gas" as a control of past global climates.

Later in the St. Petersburg period, Bob Garrels wrote a brilliant paper with his student Lee Kump on "Modeling Atmospheric 02 in the Global Sedimentary Redox Cycle" (1986,1). He was still active right up to the time of his death. In May of 1987 he was diagnosed as having cancer which had metastasized from his colon. For the rest of 1987 Bob fought his disease and continued to work. During the year a book summarizing thermodynamic data on minerals was published (coauthored by Terri Woods), as was his last major paper, entitled "A Model for the Deposition of the Microbanded Precambrian Iron Formations." Although severely debilitated by his disease and the effects of chemotherapy and radiation therapy, he was able to present a talk at the fall meeting of the Geological Society of America and to write a reply to some critical comments served up on his model for banded iron formations. This was true heroism and, characteristic of Garrels, done in good humor with no health complaints.

During the early months of 1988 Bob quickly grew worse, and he finally expired on March 8, 1988. The news of his death arrived the next day at a scientific meeting that I was attending, and which he had so much wanted to attend himself, on the GAIA hypothesis. The meeting was being held in honor of his good friend James Lovelock, and he had many ideas he wanted to air at it. Needless to say, the news created a pall over the meeting, and a number of us found it hard to keep our eyes dry. His wife and now widow, Cynthia, characteristic of her selflessness, urged that Bob's close friends at the meeting not rush to St. Petersburg but stay at the meeting and give our talks, as Bob would have wished. This we did.

Over his long career Bob received many honors, too numerous to mention here. The more notable ones are election to the National Academy of Sciences (1961); presi dent of the Geochemical Society (1962); the Arthur L. Day Medal of the Geological Society of America (1966); the Goldschmidt Award of the Geochemical Society (1973); the Penrose Medal of the Geological Society of America (1978); the Wollaston Medal of the Geological Society of London (1981); the Roebling Medal of the Mineralogical Society of America (1981); and honorary doctorates from the Université Libre de Bruxelles (1969), the Université Louis Pasteur, Strasbourg (1976), and the University of Michigan (1980). He also served on many committees, including those of the Geological Society of America, Society of Economic Geologists, the Mineralogical Society of America, the National Academy of Sciences, and the National Science Foundation.

In summary, Bob Garrels, as I said at the beginning, was a unique revolutionary. He not only brought physical chemistry to earth science but also made it understandable to ordinary geologists so that they could apply it to a wide variety of problems. He made us think in terms of cycling and recycling of rocks and impressed on us the importance of trying to treat geochemical cycles in a quantitative manner, with a combination of both chemical and geological insight. He showed us how to look at the composition of seawater, spring water, and lake water, and deduce all sorts of wondrous things from a little data. He pointed the way to how ore deposits form. Above all, he set an example of good humor, humility, optimism, and consideration for the feelings of others. He never attacked or criticized his opponents; he only disagreed with them. His time was always available for discussions with students, and he was patient with both the dullard and the smart show-off. He was never pompous and always scrupulously honest. His death has surely been a loss to the world; I wish there were more like him.

The author thanks Cynthia H. Garrels and C. Bryan Gregor for helpful comments on the manuscript.

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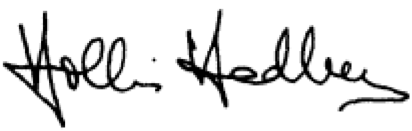

Hollis Dow Hedberg

May 29, 1903-August 14, 1988

by Georges Pardo

On the occasion of Hollis Dow Hedberg's retirement from Princeton University in 1972, he was honored by a conference on petroleum and global tectonic. Robert F. Gohen said of him:

> In the course of a lifetime of distinguished service to his discipline, service which has earned him international recognition and honor, he has attained first-rank standing in academic circles while simultaneously pursuing a successful career in the petroleum industry, most notably, since 1946, with Gulf Oil as a senior officer concerned with exploration. In short at once a man of thought and a man of affairs, he has built bridges between the theoretical and the practical, contriving lines of communication that reach from the realms of teaching and research to provide a basis for decision making and action in the world of affairs.

I will attempt to describe the life that is so aptly summarized by this quote.

In the summer of 1940, after my first year at the Instituto de Geología, in Caracas, Venezuela, I was invited by my professor of stratigraphy, Dr. Ely Mencher (later of MIT) to join him an a field trip to eastern Venezuela. We stopped in San Tomé, the Mene Grande Oil Company (a subsidiary of Gulf Oil Corporation) main camp in the area. There we were greeted and guided through the geologic section along the recently built San Tomé-Puerto la Cruz highway by a humble-looking, soft-spoken gentleman with a deep voice, strong bony hands, somewhat of a twisted posture, long steps, and always a ready smile. At the time I spoke no English and still understood very little about the profession I was entering, but he made every effort to communicate in Spanish with me. His general appearance and attitude made it difficult for me to believe that he was "El Doctor Hedberg," at the time a well-known and respected geologist in Venezuela, in charge of all geological operations for Mene Grande Oil Company in eastern Venezuela, which were considerable. I certainly could not have guessed then the impact-this man was going to have on my life and the geological world.

Hollis Dow Hedberg was born on May 29, 1903, on the crowded second floor of a small stone farmhouse in the Swedish community of Falun, Kansas, during one of the worst floods in Kansas history. A midwife was rowed in, but she arrived too late to be of any assistance. His father, Carl August Hedberg, was born in Sweden and brought to the United States when he was four years old. He married Hollis's mother, Zada Mary Dow, of Scottish-English parents. Hollis has an older brother, James, and a younger sister, Carol. Strong family bonds and Swedish tradition dominate his childhood. He is raised in the midst of hard work, love, and a deep sense of values. By the age of eight, he is plowing fields behind a horse. Plowing the long rows, he learns to whistle the music that he hears at home. "What else can you do behind a horse and a plow?" he answers when asked where he learned to whistle so clearly. Whistling while concentrating on a problem will become one of his trademarks in life. The evening's entertainment consists of the family playing music together, Hollis playing the cello, his father the violin, his mother the piano, and brother James the viola, or of reading books to each other.

Throughout his life Hollis Hedberg remembered, and was fond of, the books they shared on such occasions. This tradition of family reading continues with his own family.

Young Hollis enters the Falun elementary school in 1909 and attends the Falun Rural High School from 1916 to 1920. After graduating from a class of four, he is admitted to the University of Kansas in Lawrence in 1920. His original intention is to obtain a degree in journalism, but his interest shifts to geology. In 1921 his father dies, and he has to interrupt his schooling to return home and run the farm. In 1922 he resumes his studies and in 1925 receives a B.A. in geology with a Phi Beta Kappa key. During the summers of 1924 and 1925 he was a field assistant for the Kansas State Geological Survey. In 1925 he is admitted to Cornell University in Ithaca, New York, under H. Ries, head of the Geology Department, and is awarded an M.S. in geology in 1926. His first paper, "The Effect of Gravitational Compaction on the Structure of Sedimentary Rocks" (1926), is the result of investigations carried out by him while at Cornell. Here he advances the theory that porosity in shales is an index of pressure metamorphism and could also be used as an indicator of the oil potential of a section. Only in recent years has the idea of a correlation between rock porosity and oil maturation reappeared in the literature. In March 1926 he applies for membership to the American Association of Petroleum Geologists (AAPG) and is elected associate in August of the same year. It is his first membership in a technical society. Many were to follow, twenty-four in all. During his lifetime, Hedberg serves this society on many committees and contributes frequently to the *Bulletin.*

Hedberg's first employment is with the Lago Petroleum Company, a Venezuelan subsidiary of the Standard Oil Company of Indiana, as a petrographer in their Maracaibo labo ratory. He leaves New York for Maracaibo on June 1926. It is his first trip outside the United States. The extent of Hollis's attachment to his family is demonstrated by the fact that since the day he leaves home he writes his mother several letters a week, every week he is away, until her death in 1945. In addition to his work, he travels on his own. He is fascinated by this tropical country and its inhabitants, and his candid remarks are fortunately preserved in the correspondence to his mother. He always remembered the time he spent in Venezuela with fondness. In July 1927 he is nearly killed during one of his excursions by himself. He gets lost during a rainstorm in the Misoa River area, on the east side of Lake Maracaibo, and is swept away by the swollen waters of the Raya River while trying to ford it. Although he is a powerful swimmer, he eventually goes under exhausted. Later he writes his mother ". . . strange to say, I didn't feel frightened at all, although I was certain it was the end. I felt more surprised than anything else. It seemed such an unexpected way to end up there by myself . . . ." Somehow he manages to crawl along the bottom of the river and emerges, hanging onto a boulder sticking out of a steep bank. He is mostly upset, of all things, by his camera being ruined and the loss of a brand new Stetson hat that he saw last floating away down the river. During that period of time he uses some of his work to publish "Some Aspects of Sedimentary Petrography in Relation to Stratigraphy in the Bolivar Coast Fields of the Maracaibo Basin, Venezuela" (1928). There he expresses, for the first time in print, his belief that criteria other than fossils can be used for correlating and dating rocks. However, he is dissatisfied in general with his job. He corresponds with K. C. Heald, of the National Science Foundation, whom he had met while at the University of Kansas, regarding a possible NSF grant and meets M. N. Bramlette, in charge of the geological laboratory of Venezuelan Gulf Oil (subsidiary of Gulf Oil Corporation), who suggests that he join Gulf. By mutual agreement, his contract with Lago Petroleum is not renewed and he returns to the United States in March 1928. On the recommendation of K. C. Heald and M. N. Bramlette, E. S. Bleeker, chief geologist of the Venezuelan Gulf Oil Company, offers him a job in the geological laboratory in Maracaibo as a stratigrapher.

He shortly returns to Maracaibo, leaving New York on May 18, 1928, arriving there on June 3 after the first ship, a tanker, is diverted to Port Arthur. He refuses to take a pilot boat back to New York, leaving behind all his possessions, and he has to take the train back from Port Arthur to New York to start the trip all over. Fate has it that on board the second ship he meets Frances Murray, a freshman in college, whose father works in Maracaibo, on her way to spend a vacation with her family. He starts his duties as stratigrapher and on Bramlette's departure he takes charge of the geological laboratory in October 1928. He writes "Cretaceous Limestones as Petroleum Source Rock in Northwestern Venezuela" (1931), in which he proposes that the La Luna Formation, of Cenomanian/Turonian Age, is the main source for the petroleum accumulated in tertiary sediments in the Lake Maracaibo area. This thesis has stood the test of time. He carried out field trips to many areas of Venezuela and Colombia with the purpose of establishing a solid network of measured, consistently described and dated stratigraphic sections, among them the Rio Querecual in eastern Venezuela. In addition to the normal laboratory duties, he performs further analytical work in connection with his studies of compaction of clays and shales. He lives a very arduous life and has on-and-off bouts with malaria, pleurisy, and dysentery. His attitude toward people is exemplified by the following: While doing some field work in the Perija Mountains, he used the help of local Indians. He notices that one of them, a young Indian wearing only a loincloth, sits under a lamp with a worn-out book after work, while the rest of his companions talk or play games. Hollis asks him what he is doing, and Inocencio Sanchez replies that he is trying to teach himself to read. When the field work is finished and the local help dismissed, he offers Inocencio a job as assistant in the laboratory in Maracaibo. Inocencio accepts, learns how to read and much more, raises a nice family, and remains, until his death many years later, one of the best technicians Gulf has ever had. This pattern of helping deserving people in all walks of life will be repeated countless times during Hollis Hedberg's lifetime. He starts lifelong friendships with prominent geologists such as M. N. Bramlette and Hans Kugler from Trinidad Leaseholds Ltd. He dates Frances Murray when she visits Venezuela, and during a vacation trip to the States in September 1932 he is engaged to her. They get married on November 11 of the same year. He returns to Venezuela in December, and Frances joins him in June of 1933.

In the summer of 1934, the Hedbergs come back to the United States on a leave of absence and move to Palo Alto, California, and he completes with flying colors the Stanford University curriculum requirements for Ph.D. in geology between September 1934 and May 1935. His first son, Ronald, is born there in 1934. He meets and is influenced by Eliot Blackwelder, H. G. Schenck, from Stanford, and J. A. Cushman, among others. While in school he presents "Gravitational Compaction of Clays and Shales" (1936,2) and, with H. G. Schenck and R. M. Kleinpell, "Stage as a Stratigraphic Unit" (1936,1) at a Geological Society of America meeting in Palo Alto in April 1935. This last paper is the first public manifestation of his lifelong concern about stratigraphic nomenclature.

They return to Venezuela in September 1935, where he resumes his duties as director of Gulf's geological laboratory. A Ph.D. in geology is granted to him in absentia in June 1937. His dissertation "Stratigraphy of the Rio Querecual Section of Northeastern Venezuela" (1937,3) is published and includes "Foraminifera of the Carapita Formation of Northeastern Venezuela" (1937,4). In the Rio Querecual paper he makes a clear distinction between age (Time), stage (Time-Stratigraphic), formation (Lithogenetic), and zones (Faunizone, Mineral-zone, etc.) independent of the former units. This forms the bases of his concepts on stratigraphic classification that he will strongly support and defend later in life. On February 1936 he is elected Fellow of the Geological Society of America. The Hedbergs' second son, James, is born in 1936.

In June 1937 he is appointed stratigrapher and director of the new geological laboratory in Oficina (El Tigre) and is put in charge of geological operations in eastern Venezuela for Mene Grande Oil Company (former Venezuelan Gulf), where the first wildcat Oficina No. 1, spudded in February 1933, 170 kilometers from the nearest production, is brought in as a producer in the same month as his arrival. He writes "Evaluation of Petroleum in Oil Sands by Its Index of Refraction" (1937,2), in which he calls attention to a little known, or understood, relationship between density and refractive index of crude oils and to its practical importance in evaluating the content of oil reservoirs. Today it is a standard technique. He becomes associate editor for the AAPG in October 1937 and will hold this position for forty-six years! The Hedbergs' third son, William, is born in 1938.

In December 1939 Hollis Hedberg is appointed assistant chief geologist in charge of all geological operations in eastern Venezuela, and the family moves to the nearby new camp of San Tomé. He has reservations about accepting a management position. Should he rather dedicate his life to science? During a trip to Trinidad he seeks the advice of his very good friend and mentor Hans Kugler. He is told, "The one way you may become a great geologist (who knows). But the other way, you may be the making of *many* great geologists through the favorable environment you may create for their development. Which do you think is the greater?" He managed to do both. He assumes very heavy responsibilities; between June 1937, the date of the completion of Oficina No. 1, and January 1946, shortly after his departure from Venezuela, fifty-five wildcats and exploratory stepouts and nearly 550 development wells are drilled, resulting in the discovery of twenty new fields with total reserves, estimated at the time, of over half a billion barrels and a production of nearly 150 million barrels of oil. And all this during the wartime shortage of technical personnel. Geologically the situation is a complex one, consisting of heavily faulted multiple linear sand bodies. He is the prime force behind every aspect of this exceptional operation, in which he makes full use of sound geological and geophysical methods and is not afraid to experiment with new techniques. It is a rational and thorough exploration and development program of a major petroliferous province. Today it has produced several billion barrels of oil. However, Hedberg never relaxes in his scientific objectives. In December 1940 he participates in a discussion of C. W. Tomlinson's on "Techniques of Stratigraphic Nomenclature" (1941,1), in which he further expands his ideas and proposes flexibility in the usage of "stage." During this period of time he meets Walter Bucher of Columbia University, for whom he develops great respect.

In July 1945 the family moves to Caracas, where he becomes assistant chief geologist in charge of all geological operations in Venezuela. Their fourth son, Franklin Augustin, is born there in 1946.

In August 1946 he is appointed chief geologist, Foreign Exploration Division of the Gulf Oil Corporation, in charge of all geological activities in foreign countries (except Venezuela and Canada) and is transferred to New York. He is promoted to exploration manager of the same division in 1951. However, before leaving Venezuela he fulfills what he considers his obligation toward the geological community by writing with former colleagues H. J. Funkhouser, A. Pyre, and L. C. Sass three papers describing the eastern Venezuela oil province. They are published in the *AAPG Bulletin* in January 1944, December 1947, and October 1948. In his new position he becomes particularly interested in Africa, where no oil has as yet been discovered. He personally digests all the literature and information that he can find about the continent and, alone, goes on a field trip for several weeks to visit all the localities that he considers pertinent. He comes back convinced of the petroleum potential of West Africa and initiates exploration programs that extend from the Spanish Sahara to Southwest Africa. These programs are in large part responsible for Gulf Oil's important position in West Africa and discoveries in Nigeria and Cabinda. Further, he personally negotiates favorable concession agreements with the governments involved. He writes the yearly "Petroleum Developments in Africa" for the *AAPG Bulletin* until 1960. The Hedberg family is living now in Summit, New Jersey. The fifth child and only daughter, Mary Frances, is born there in 1952.

In September 1952 he is appointed chief geologist, responsible for all geological activities of Gulf Oil Corporation worldwide. The family moves to Oakmont, a suburb of Pittsburgh. He is named exploration coordinator in 1953 and vice-president for exploration in 1957. In these positions he becomes increasingly discouraged by management's negative attitude toward petroleum exploration while emphasizing short-term returns. He is also strongly opposed to the decentralization of exploration that is taking place. He believes that fragmenting exploration and putting the pieces under the control, and budget, of arbitrarily defined profit centers will damage the entire exploration effort of the corporation. The future proves him right. Taking advantage of the rule, in effect in Gulf at the time, that employees with foreign service must retire at age fifty-five, he requests retirement in 1959. The chief executive, W. K. Whiteford, will not accept it, and he stays on in the capacity of vice-president until 1964, when he is appointed exploration advisor to the executive until 1968, the year of his retirement from Gulf. In the meantime, encouraged by friends, notably Harry Hess, and expecting to be retired, he has accepted the position of full professorship of geology, on a part-time basis, at Princeton University starting in 1959. He also had planned for him and the family to move to Princeton. They move and there he maintains his primary home and commutes between Princeton and Pittsburgh, where he keeps a small apartment across the street from the office. He obtains a Gulf grant for Lamont-Doherty Geological Observatory and meets Maurice Ewing, with whom he develops a strong friendship.

In 1958 he is elected vice-president of the Geological Society of America and is elected president in 1959. He is elected to the National Academy of Sciences in 1960, where he participates in many committees, and president of the American Geological Institute in 1962.

The period of time from 1946 until his death is one of incredible variety of endeavor and intensity of work. Only through exceptional will, discipline, determination, and ability can a man produce what he did. None of his activities are pro forma. When he goes to scientific society's meetings, he attends the sessions, listens to the papers and is always fully prepared for discussions, often knows well the speaker's background, and can quote his earlier statements if needed. He is asked to participate in countless committees, seldom refuses, and always does his homework. He travels extensively in the United States and abroad either on Gulf business or to attend conferences. Much of his travel expenses are out of his own pocket, and all the Hedbergs' vacation trips are centered around some geological activity such as visiting type sections and universities or meeting with geologists.

During this period Hollis Hedberg undertakes one of the most difficult tasks of his career. In addition to his workload, in 1946 he becomes a member of the American Commission on Stratigraphic Nomenclature and will remain in this capacity until 1960. In response to a request for comments by R. C. Moore, chairman of the commission, he writes "Time-Stratigraphic Classification of Sedimentary Rocks" (1948,1), in which he fully develops his thesis of a clear distinction between Time, Time-Stratigraphic, and Rock-Stratigraphic units. Time units are conceptual, while others are material. The properties of Rock-Stratigraphic units are used to establish Time-Stratigraphic units and determine their position in the Time scale. He advocates a more widespread use of "stage" and pleads for simplicity and flexibility. He also points out, having considerable experience in paleontology himself, that although fossils have been the most commonly used property to determine the age of a rock, they are not the only or most infallible property, and therefore biostratigraphic units should never be considered Time-Stratigraphic units. This last point becomes a great source of controversy and argument, mostly with paleontologists. He becomes chairman of the commission from 1950 to 1952 and reiterates his position in "Nature of Time-Stratigraphic Units and Geologic Time Units" (1951). At the 19th International Geologic Congress in Algier, in 1952, in "Procedure and Terminology in Stratigraphic Classification" (1954,2), he proposes to the International Commission on Stratigraphy the creation of an International Commission on Stratigraphic Nomenclature. As a consequence, what is to become the International Subcommission on Stratigraphic Classification (ISSC) is created. Hedberg is appointed chairman from 1952 to 1976. It is the opportunity to bring to fruition one of his lifelong ambitions: to bring forth his concepts on stratigraphic procedures and have them agreed on, accepted, and used as "guide" (not law) by the international geological community. To him stratigraphic classification, terminology, and procedures are the fundamental international language of geologists, and without clear understanding of the meaning of the terms, an unequivocal representation of the earth's history is impossible: "Uniformity in the procedure and terminology used in classification is essential to world wide understanding among geologists" (1954,2). This endeavor costs him an incredible amount of effort and a large amount of money. He prepares forty-six circulars, some two thousand pages, resulting from discussions with some fifty-five individual members, thirty-five individual ex-officio members, and forty organizational members around the world (these numbers varied somewhat from time to time). They are deposited in a number of libraries. In addition, there are seven published ISSC reports. He submits to the members many questionnaires, and there are endless discussions, even arguments, bringing out the fundamental differences in philosophy between stratigraphy schools. For instance, the representatives of the Federal Republic of Germany maintain that the goal of stratigraphy is to classify rocks in terms of Time *only* and therefore all other formalization is unnecessary, and the USSR has a similar position in addition to arguing for the existence of worldwide *natural* time breaks that must be the basis of stratigraphic classification. Hedberg's efforts culminate with the publication in 1976 of the *International Stratigraphic Guide* (1976,1), of which he is the editor. It is interesting to note that on the vote on whether or not to publish the *Guide* there are eighty-five yeses and three noes—the USSR and the Federal Republic of Germany naturally among them. The beauty of the *Guide* is its simplicity and general applicability, making it unnecessary to fundamentally modify it if new techniques or geological concepts were to appear. Hedberg is a strong influence on the ISSC until his death, and between 1954 and 1976 he publishes twenty-five papers pertinent to stratigraphic classification. In 1983 the North American Commission on Stratigraphic Nomenclature publishes *North American Stratigraphic Code*,[1] which, although it has some new terms, follows the guidelines set the by ISSC *Guide.*

During the 1960s he becomes increasingly interested in the geology of the oceans, and in 1962 he is appointed chairman of the AMSOC Mahole Committee of the National Academy of Sciences. He, and the majority of the committee members, are skeptical on whether the information obtained by drilling one hole through the Mohorovicic discontinuity would justify the enormous cost. He is much concerned with wisely utilizing public funds. He proposes, instead, to start with a more feasible and cost-efficient worldwide program of shallower holes as a prerequisite to the Mohole. However, in 1963 he resigns, and the committee dissolves itself, as a protest to the political pressures to drill immediately a Mohole "spectacular" with out the benefit of experience in deep water operations or "ultra-deep" hole technology. He and Creighton Burk, chief scientist of the committee, summarize the circumstances in "Drilling the Ocean Crust" (1964,2). The Mohole is never drilled, and a few years later the AMSOC Committee proposal becomes, in effect, the highly successful Joides Deep Sea Drilling Program, of which Hedberg becomes chairman of the Panel on Safety and Pollution Prevention from 1970 to 1977. Also during the same time he begins to stress the economic potential and strategic importance of offshore exploration and convinces Gulf Oil management to build a ship for the purpose of evaluating and exploring offshore sedimentary basins around the world. In late 1966 the project is approved, and the R/V *Gulfrex,* a multisensor ship, is built. It operates around the world from 1967 to 1975, logging 160,000 miles of surveys, and is replaced in 1974 by the R/V *Hollis Hedberg,* which logs over 200,000 miles around the North American Continent and the Caribbean before being decommissioned in 1985. He writes "Why Explore the Deep Offshore?" (1967,2), in which he expresses the thought that the petroleum industry needs to know "the other side of the basin" and should cooperate with governments and academic institutions in noncompetitive activities aimed at understanding the limits of potential economic petroleum occurrences. Between 1979 and 1981 he writes several articles expressing his concern that the United States is neglecting its offshore petroleum resources and proposes a multi-company/ government/ academic institutions consortium to evaluate the offshore potential. To stimulate exploration he also suggests the replacement of cash bonus bidding by work commitment bidding and government participation in exploration. Predictably this proposal meets with a lack of enthusiasm from the government and the petroleum industry. Neverthe less, he is appointed to an Advisory Energy Task Force for Ronald Reagan's 1979 presidential campaign. His interest in worldwide exploration of the oceans leads him naturally to the problems of the ownership of mineral resources under the sea. At that time there is no internationally accepted law governing the offshore extension of countries, and whatever practice in effect is hopelessly embroiled with the problems of surface activities such as fishing and sailing rights. Further, proposals were presented to the Third United Nations Conference for the Law of the Sea for a draft that could seriously affect the future of natural resources allocation. In 1968 he is appointed chairman of the Technical Subcommittee on Petroleum Resources of the Ocean Floor of the National Petroleum Council and remains in that post until 1973. He strongly objects to limits of national jurisdiction proposed in 1969 by a report of the President's Commission on Marine Sciences, Engineering and Resources, and he proposes instead to use a "boundary line" drawn between the physiographic base of the slope and that of the rise. He calls it the "base of the slope" method. This line would be fixed by some international technical commission. This idea is presented at the 1969 annual meeting of the American Association of Petroleum Geologists in Dallas. To prove the feasibility of the method, he personally draws from bathymetric maps a "boundary line" for all the continents and islands. The proposed technique is well explained and exemplified in *National-International Jurisdictional Boundary on the Ocean Floor.* [2] Unfortunately, the Third United Nations Law of the Sea Conference seems to be unable to reach an agreement due to conflicting self-interests of nations and moves toward a compromise boundary definition that he considers a "hodgepodge and travesty." In numerous papers and articles written between 1973 and 1983 he asks the United

States not to sign the treaty. He uses as an argument the signing of a draft treaty between the United States and Mexico in 1978 based, in large part, on the arbitrary 200 nautical miles limit. Under this treaty the United States would lose 25,000 square miles of deep-water, oil-prospective Gulf of Mexico as compared to his proposed "base of the slope" method. In "Evaluation of U.S.-Mexico Draft Treaty on Boundaries in the Gulf of Mexico" (1980,2) he strongly criticizes the U.S. policy regarding the Law of the Sea and urges the Senate not to ratify the treaty. Thanks to his actions, the Senate requests from the USGS an evaluation of the oil potential of the deep waters of the Gulf of Mexico, and as a consequence of the results, the treaty is never ratified. Further, President Reagan announces on July 9, 1982, that he will not sign the Law of the Sea Convention, and the United States walks out of the Third United Nations Conference on the Law of the Sea. This chapter is not closed yet, and let's hope that Hedberg's patriotic and rational opinion will influence the final outcome.

From 1959 to 1972 Hedberg teaches at Princeton University. He gives one course, "Stratigraphic Systems," for graduate students. It has the format of a seminar wherein each system is analyzed in historical sequence. Through discussions and examples, he leads the students so they become aware of past and present stratigraphic problems. He is as thorough as usual and invites visiting prominent geologists to participate and meet the students. He is very informal, and most of the seminar sessions are held in his home, in the living room, with pretzels and beer always appearing toward the middle of the sessions. He never complains about beer spilled onto the Oriental rugs, which happens with alarming frequency. He serves in an advisory capacity to the Department of Geology. After his retirement from Princeton University in 1972, he becomes professor emeritus.

As a matter of fact, Hollis Hedberg, in his lifetime, has been advisor to many universities and other institutions: New York University, Columbia, Western Reserve, Pittsburgh, Kansas, Woods Hole Oceanographic Institution, MIT, Lamont-Doherty Geological Observatory, and Stanford.

Having reached a professional summit in both academic and industry circles, he is a living example of his strong belief that a close interaction between the two is essential for the progress of both. He expresses his thoughts on the subject at the 24th William Smith Lecture, in London in 1970: "Petroleum and Progress in Geology" (1971,1).

He still manages to have time to do his own research on problems that have been on his mind for a long time. He writes "Geologic Aspects of the Origin of Petroleum" (1964,1), in which he summarizes the nature of petroleum occurrences in terms of rock types, fluids, age, temperature, etc., discusses the sedimentary environment, processes, timing, and sources, and suggests some lines of investigation, all based on observed relationships. In "Significance of High Wax Oils with Respect to Genesis of Petroleum" (1968,3), he supports the thesis, from field evidence, that oils with a high wax content must have been sourced in beds with a high non-marine plant content. Later he writes "Relation of Methane Generation to Undercompacted Shales, Shale Diapirs, and Mud Volcanoes" (1974,2), in which he proposes that pressures higher than hydrostatic, sometimes higher than geostatic, can be generated in shales due to the decomposition of organic matter, thus causing diapirism and sedimentary volcanism. He presents "Methane Generation and Petroleum Migration" (1980,1), a related subject, at the 1978 annual AAPG convention. In these studies he emphasizes his long held belief that geology is an inductive science and that theories should always be based on solid observations of nature.

After his retirement from Gulf in 1968, he remains associated with the company as a consulting exploration advisor. In this capacity, in the late 1970s he recommends to management the investigation of an area, somewhat neglected by the industry, where seeps and mud volcanoes are abundant: northwestern Colombia. He personally travels to Bogota, meets with ECOPETROL high officials and, against all odds, obtains for Gulf excellent terms over essentially the entire prospective area in exchange for a joint ECOPETROL-Gulf systematic exploration program. It is a marvelous expression of personal confidence from the Colombians. Only Hollis could do it, but he also had a feeling of heavy responsibility toward them. Hedberg actively participates in the program from 1982 to 1986, even after the acquisition of Gulf by Chevron in 1984, going to the field many times, advising, and following closely the progress of the studies, even though his health is beginning to fail toward the end of his involvement. Unfortunately he cannot see the completion of the project due to the increase in guerrilla warfare and drug trafficking, which renders the whole area unsafe for Americans and Colombians alike. From 1976 to 1984, he is a member of Gulf Oil Corporation's Science and Technology Council, whose main objective is to monitor and advise Gulf Research & Development Company. It is ironical that in 1981, after several generations of Gulf executives ignoring his advice on organizational matters, the chairman of the board asks him, seventy-eight years old, and another retired Gulf executive, I. G. Davis, to make a study of the Exploration Department organization, personnel, and programs and recommend improvements. Recommendations are presented in November of the same year. One of them is the creation of a Gulf Exploration Council. Under his guidance, it operates from 1982 to 1984. Hedberg is a member, and he takes a very active part in it in an attempt to stimulate and improve the quality of the exploration program. Unfortunately, Gulf Oil Corporation runs out of time. In retrospect, Hollis Hedberg's life was closely intertwined with that of Gulf. Except for the early, short period with Lago Petroleum, it was the only company that he was ever associated with, and he strongly believed that Gulf's health depended on a solid exploration effort. In spite of frustrations and disappointments, he was extremely loyal to it and, to the end, was always prepared to be of assistance. Although he was accused of having orange blood, the color of Gulf's logo, he always emphatically stood for what he believed was right, even if it did not agree with management views. He derived much satisfaction from the fact that he was admired and respected by everyone he came in contact with in exploration, especially the younger generations, and he was able to guide the careers of many of them.

During his entire life Hollis Hedberg was interested in the history of geology, and he became quite a scholar on it. His fine collection, gathered over the years, of old editions of books by early geologists and explorers, as well as of old maps, testify to it. Due to his heritage he was naturally interested in the role of Swedish geologists. His first paper on the subject, "Influence of Torbern Bergman (1735-1784) on Stratigraphy" (1969,1), is published in 1969. He participates in the translation from Swedish of "A Description of a Cliff. . ." by Göran Vallerius,[3] published in 1970, and *Hermann Karsten, Pioneer Geologist in Northern South America, 1844-1856* (1974,1), a paper on a German, is published in 1974. Finally, *The 1780 Description by Daniel Tilas of Stratigraphy and Petroleum Occurrence at Osmundsberg in the Siljan Region of Central Sweden* (1988) is published. This is to be his last work. It is an extensively annotated translation from the Old Swedish of one of the earliest published sets of stratigraphic correlations and a description of the occurrence of petroleum with suggestions of its source rock. He receives the first published copy less than two months before his death. The letter accompanying the copy said, "On a personal note, I am particularly gratified to be able to make this presentation to someone who has had such an important impact on my career as a petroleum geologist—not to mention the fact that the same someone was the first dinner guest of a newly married couple in Oberlin, Ohio, who so much appreciated his stopping by to say hello," signed Larry Funkhouser, President of AAPG.

All in all, during his lifetime, Hedberg published 177 papers and fifteen reviews. However, the bulk of his writings is in countless private reports and memoranda which unfortunately might never be published. Many of them are probably lost by now. It is a pity because some, such as the ones describing Colombian and Venezuelan geologic sections or his very unconventional heavy mineral zonation in eastern Venezuela (an early attempt at multivariate analysts), were classics.

Hollis Hedberg received many honors and awards during his lifetime: the Medalla de Honor de la Instrucción Pública by the Venezuelan Government in 1941 (first foreign recipient and his first award) in recognition to his contribution to Venezuelan geology; the AAPG Sidney Powers Medal and the University of Kansas Distinguished Service Award in 1963; the Geological Society of London William Smith Lecture in 1970; the AAPG President's Award, and is honoree of Princeton University Conference on Petroleum and Global Tectonic in 1972; the National Academy of Sciences Mary Clark Thompson Award and the AAPG Human Needs Award in 1973; the Offshore Technology Conference Distinguished Achievement Award and the Geological Society of London Wollaston Medal in 1975; Doctor

Honoris Causa, University of Uppsala, Sweden, 1977; the Geological Society of America's Penrose Medal, the highest award for a geologist in the United States in 1980; honoree at the AAPG 1981 Hedberg Research Conference in Galveston, Texas; the AGI Ian Campbell Medal and the Louisiana State University Hollis D. Hedberg Award in Energy in 1983; and the AGI William Heroy Jr. Award in 1987.

In addition, he was awarded honorary memberships in the Sociedad Scientífica Matemática, Física y Natural de Venezuela in 1941, the Geological Society of London in 1957, the Asociación Venezolana de Geología, Minería y Petróleo in 1959, Geological Society of Stockholm in 1960, and The Danish Royal Academy of Sciences and Letters in 1970.

Hollis Hedberg had a personal life as successful and rewarding as his professional life. Frances was his close and understanding companion for fifty-six years. In addition to the normal load of housework, under all kinds of circumstances and frequent moves, she ran plane-table for him in the Rio Querecual, coauthored a paper with him, kept geological files in San Tomé, accompanied him on countless trips, and drove the car to their summer home in Cape Cod so he could type on the back seat. They raised their family with the same sense of values with which he had been raised, and today three sons have Ph.D.'s in geology, one has two M.A.'s, in classics and journalism, and the daughter has a Ph.D. in molecular biology. They are all successful professionals and have given them fourteen grandchildren and five great-grandchildren. More important, the family bonds are as strong as they were between Hollis and his parents. Although his work was his principal "hobby," he was also a fine athlete for much of his life, a good swimmer and outstanding tennis player. He and Frances enjoyed square dancing, and occasionally he played the accordion, but most of all he cherished his vegetable gardens, of which the most extensive and complete was at their vacation home in Cape Cod.

With the death of Hollis Hedberg on August 14, 1988, came an end to an astonishing career full of accomplishments. He is unquestionably one of the great geologists of our time, but he made significant contributions in areas that required more than just being a great geologist. To succeed in convincing a corporation management to invest in areas with no previous success, to bring about an international consensus in the language of stratigraphic classification and procedures, and to help keep the United States out of the Law of the Sea Convention as drafted by the United Nations required an incredible amount of determination, persistence, hard work, discipline, self-confidence, courage and patience. To be admired, respected, and cared for by everybody who came in contact with him required kindness, humility, understanding, and love. Incredibly, he did all of this in addition to having a wonderful, close-knit family.

It is very difficult to come to the realization that the man in the perennial gray suit with the somewhat crooked posture, the big smile, and the handshake that almost tore your arm out of its socket is no longer here. We, the many who in one way or another had the luck to come in contact with him and the geological community as a whole, will miss him very much.

In preparing this memorial I have had the assistance of Mrs. Frances Hedberg and access to Hollis Hedberg's letters and personal files and drew from my personal experiences, having had the good fortune to be a close friend and professional associate for forty-eight years.

NOTES

1. *American Association of Petroleum Geologists Bulletin* 67 (1983): 841-75.
2. Law of the Sea Institute, University of Rhode Island, Occasional Paper 16 (1972), 19 pp.
3. *Proceedings of the Bristol Naturalists' Society* 31 (1970):615-24.

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Courtesy, Department of Public Relations, University of Chicago

Dwight J. Ingle

Dwight Joyce Ingle

September 4, 1907-July 28, 1978

by Maurice B. Visscher

It is difficult to compress into a memoir of reasonable length the story of a scientist who was born on an Idaho ranch, educated in an ungraded elementary school and small town high school, attended a newly established state university, and yet went on to become a first-rank, pioneering scientist in a new and uncharted field. Yet even from this short account it will be apparent that his life was of importance to the advancement, philosophy, and ethics of science.

The period of Dwight Ingle's active life was the time of rapid development in endocrine science, to which Ingle himself contributed greatly. His career included a series of statistically improbable successful ventures, chronicled in his autobiography, *I Went to See the Elephant.* [1] The title comes from John Godfrey Saxe's story of "the six blind men of Indostan," who describe an elephant according to the part of the animal they can touch with their hands, and Ingle displayed genuine modesty in choosing it, for, as he explains in the book's preface, "Science does involve looking at some specific properties of systems that are too big to get into perspective." With a simple and genuine modesty, he continues:

> This may be the first time that a non-famous scientist has written an autobiography. I hope it will show that an ordinary fellow can have fun being a scientist. There has been some self-direction in my life, but it may well be that the origin of some of my drives and faiths are hidden. Fortuity has played an important role in determining my interest, opportunities, and what has become of me.[1]

EDUCATION AND EARLY LIFE

Dwight Joyce Ingle was born September 4, 1907, on a ranch near Kendrick, Idaho. He attended a small country elementary school and graduated from high school in Kendrick. At eighteen he entered the University of Idaho with a career as a physical education director in mind. Interested in weightlifting and wrestling since he was a boy, he had not yet acquired any real interest in science though he was a voracious reader. This situation changed very soon under the influence of the faculty.

Ingle's first serious scientific interest was in the field of psychology, which perhaps intrigued him because he engaged in summer work in state mental hospitals in Idaho. As he tells the story in his autobiography, he soon became disillusioned with the treatment and methods of care then available for the mentally ill, coming early to the conclusion that psychoses are organic illnesses. His interests, therefore, shifted to physiological psychology. Earning the Bachelor of Science degree at the University of Idaho in 1929, he enrolled there as a graduate student and earned a Master's degree in psychology in 1931. During his years at the University of Idaho, Ingle began reading the literature of endocrinology and studying adrenalectomized animals. He supported himself as a graduate student by working part time as a laboratory animal caretaker and by grading examination papers.

The Great Depression was then under way in the United States, and life was hard for most graduate students. Ingle was offered a small stipend as a teaching assistant at the Uni versity of Minnesota that allowed him to continue his graduate studies. He knew of Karl Lashley and others' basic work in physiological psychology at Minnesota and planned to emulate them. As it happened, however, his interests and his work shifted from psychology to fundamental endocrinology.

The author of this memoir was a member of the University of Minnesota Graduate School Committee that conducted Dwight Ingle's final examinations for the Ph.D. degree. He had majored in psychology and minored in physiology, of which I was the representative. My first experience with the candidate was in connection with his petition to the Graduate School to be exempted from the foreign language requirements for the Ph.D., which had never been waived before that time, and I remember vividly the consternation this request raised in the Graduate School community. Ingle's examining committee took account of the sixty high-quality scientific papers that he had already published and concluded that Ingle had demonstrated his fitness for a doctorate. There was no dissent among the committee members.

The Ingle case was a landmark decision for the Graduate School at Minnesota and was followed by many more examples of breaking traditional rules and regulations at that institution. I was somewhat surprised, therefore, that Ingle himself made no mention in his autobiography of his stubborn refusal to bother with things like foreign language requirements, which he undoubtedly considered much less important than learning how to remove part or all of a rat's pituitary gland at the age of weaning. He was, of course, right, but in his mention of his Ph.D. examination he refers only to the fact that he was asked only one question relating to psychology, his major field, the examination being dominated by questions on biology and physiology.

After completing his graduate studies at the University of Minnesota Ingle moved to the Mayo Clinic, where he was a Mayo Fellow for four years and collaborated with E. C. Kendall's group in studies of the hormones of the adrenal cortex. He then became a Cox Medical Research Fellow at the University of Pennsylvania, where he worked for three years under the sponsorship of Dr. F. D. W. Lukens.

After the termination of the Cox Fellowship, Ingle worked for the Upjohn Company in Kalamazoo, Michigan. In his twelve years there he became a senior research scientist and conducted a basic research program in endocrinology without pressure to work on problems of specific importance to the company. Yet he became restive, and when he was offered a professorship of physiology in The University of Chicago's Ben May Institute, he left Michigan for Chicago, remaining there from 1953 until his retirement twenty years later. Chairman of the Department of Physiology for nine years, he was not happy as an academic administrator and relinquished the post before he retired.

INGLE AND THE NEW FIELD OF ENDOCRINOLOGY

As noted above, Ingle came onto the scientific scene when endocrinology was emerging as a major field of science. He offered the new field freshness of approach and imagination, strict critical thinking, and boundless energy for painstaking work. Perhaps his greatest asset was his ability to define problems in a way that could be answered by direct observation in experimental situations. In reviewing his very large scientific output, it is obvious that he excelled at asking specific questions and spared no pains in devising methods to answer them.

Ingle is responsible for at least three signal advances in endocrine science —development of a bioassay for adrenal cortical hormones that facilitated the purification of cortisone, documentation that the adrenal cortex and the pituitary gland interact by a negative feedback mechanism, and characterization of the permissive role of adrenal hormones in homeostatic control mechanisms. His contributions are described in detail in his own autobiography[1] and in E. C. Kendall's autobiography.[2]

The first of these advances stemmed from a 1934 paper in which Ingle (together with W. T. Heron and W. M. Hales) reported that repetitive contraction of skeletal muscle requires an intact adrenal cortex. Ingle suggested to E. C. Kendall that this phenomenon could be utilized as a bioassay for the activity of adrenal cortical extracts. Kendall immediately realized the potential of such a bioassay in the purification of adrenal hormones and invited Ingle to join his group at the Mayo Clinic, an appointment that Kendall described as a turning point in the purification of the glucocorticoid hormone cortisone.[2] The assay provided reliable data as to which purified fractions contained the active principle and gave the Mayo group an advantage over competing laboratories that lacked such an assay. During these early studies he characterized the relation between the adrenal gland and salt and water metabolism, a phenomenon that subsequently became the basis of another bioassay system that led to the recognition that the adrenal cortex also secretes a mineralocorticoid hormone, later characterized as aldosterone.

Another side observation of the studies at the Mayo Clinic led to his second major contribution, namely in 1937 Ingle observed that the administration of adrenal cortical extracts or purified glucocorticoids to intact rats causes atrophy of the adrenal glands. Adrenal atrophy could be prevented by the simultaneous administration of pituitary extracts, and he subsequently established in collaborative work with Herbert M. Evans and Choh H. Li that the changes in adrenal size and activity are mediated by the pituitary hormone adrenocorticotropin. The elucidation of the feedback relation between the adrenal cortex and the pituitary gland served as a paradigm for similar control mechanisms in other endocrine systems.

His third major contribution evolved from his studies of the physiological actions of adrenal steroid hormones, a train of thought that led to excursion into diverse aspects of physiology, including studies of the effects of adrenal steroids on carbohydrate physiology, investigations on the pathogenesis of hypotension (shock) in animals with adrenal insufficiency, and the effects of severe stress on animals. The damaging effects of stress were widely believed to be due to hypersecretion of the adrenal cortex. However, Ingle showed that the characteristic damaging effects of stress are produced when adrenal steroids are supplied to adrenalectomized animals at a constant but not excessive rate of administration. He thus deduced that the role of the adrenal cortex in the stressed state appears to be due to a subtle "permissive" or supporting role rather than as the primary mediator of the stress reaction. This paradigm has had an enormous impact on the analysis of endocrine systems.

PERSPECTIVES IN BIOLOGY AND MEDICINE AND THE ETHICS OF RESEARCH

At Chicago Ingle founded a hybrid journal of a type that had never existed before. *Perspectives in Biology and Medicine* published papers from the interface between the biomedical sciences and the humanities. To Ingle *Perspectives* was the forum where the "ordinary fellow" could explore and integrate "two cultures"—a term he coined long before E. P. Snow applied it as a catch phrase for the gap between humanists and scientists. With the encouragement and help of The University of Chicago Press, Ingle provided a single publication medium to serve the two groups, demonstrating in a practical way that differing points of view could have a commonality of objectives.

Perspectives published various types of speculative essays on basic science and the ethics of biomedical research—particularly useful at a time when serious problems so often involve situations where biological science and ethics intersect. Having first provided a forum for the rational presentation of varying viewpoints, Ingle used the journal frequently to air his own analyses of such problems.

Ever careful to give credit to his own research staff, Ingle felt particularly strongly about the impropriety of senior research directors who report the results of students without acknowledging their ideas and implementation. At the Endocrine Society's annual banquet in 1959, his presidential address included an elaborate fable about a scientist born with "a mutation that enhanced the ability of the brain to function creatively." This mutation had come about when a clergyman grandfather was struck by a single neutron while "bent over an ice cream freezer at a church social." Skipping the son, who became a mail carrier, it manifested itself in the grandson.

Tongue in cheek, Ingle went on to describe how the grandson became a promising graduate student and made an entirely independent discovery that greatly pleased his advisor. The advisor asked the young man if he might disclose the finding publicly in an important address he was about to deliver. Receiving consent, he dutifully reported that his graduate student had made an exciting discovery. To no one's surprise but the student's, however, the next day's headlines read, "Professor X announces a major discovery." Professor X then became a noted participant in scientific seminars and a frequent guest lecturer on the subject of the student's discovery.

According to Ingle's fable, Professor X rode to fame and fortune while the young student, if not ignored, was recognized only by serious students of his field. Promoted rap idly in prestigious academic institutions, he soon became a full professor with productive graduate students of his own. One day, a student reported to him a highly significant result gained from entirely independent studies. As it happened, the young professor was just about to give a talk on the work going on in his department, and he asked the graduate student's permission to report his unpublished results. He felt a little qualm when he remembered his own dismay when credit for his work as a student went to his academic sponsor but reminded himself that he provided the intellectual climate in which his students worked and the stipends to live and develop into creative scientists. And so it was that the grandson announced his student's innovative study, and the next day the headlines read, "Professor Y announces spectacular new scientific results"—with predictably favorable consequences to his career.

This tale occupies eight pages of Ingle's autobiography, where he details several stages of the successful scientific career: appointments to journal editorships, membership on foundation and government grants committees, administrative posts. An inveterate storyteller but never abrasive, he used a gently satirical tone to criticize people of his own ilk. He poked fun at human frailties but was generous and forgiving.

AWARDS AND HONORS

Ingle received much honor and acclaim for his contributions to endocrinology. He was invited to lecture many times at home and abroad. He was awarded the Honorary Doctor of Science degree by the University of Idaho in 1962, received the Koch Award of the Endocrine Society, and was elected to the National Academy of Sciences in 1963. He was given the Outstanding Achievement Award of the University of Minnesota in 1964, the Roche-Organon

Laurentian Hormone Conference Award, and the Upjohn Prize. He was elected a fellow of the American Academy of Arts and Sciences and was given the Modern Medicine Achievement Award. He was a member of Phi Beta Kappa, Sigma Xi, Alpha Omega Alpha, the American Association for the Advancement of Science, the American Physiological Society, the Endocrine Society (president, 1959-60), and the Society for Experimental Biology and Medicine (president, 1965-67).

"My research efforts have been driven by curiosity," he once wrote, when asked what motivated him to do scientific research. "The joy of the daily search has been enhanced by occasional discovery."

He will long be remembered as a perceptive, careful, and creative scientist of broad intellectual interests—and as an unusually decent human being; definitely not, as he himself put it, "an ordinary fellow."

NOTES

1. D. J. Ingle, *I Went to See the Elephant* (New York: Vantage Press, 1963).
2. E. C. Kendall, *Cortisone. Memoirs of a Hormone Hunter* (New York: Charles C. Scribner's Sons, 1971).

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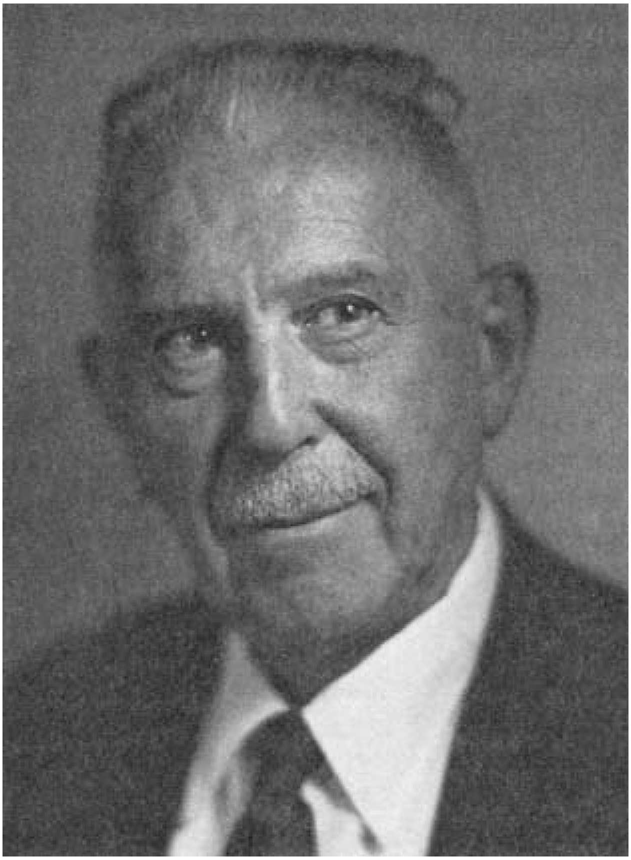



Solomon Lefschetz

September 3, 1884-October 5, 1972

by Phillip Griffiths, Donald Spencer, and George Whitehead[1]

Solomon Lefschetz was a towering figure in the mathematical world owing not only to his original contributions but also to his personal influence. He contributed to at least three mathematical fields, and his work reflects throughout deep geometrical intuition and insight. As man and mathematician, his approach to problems, both in life and in mathematics, was often breathtakingly original and creative.

PERSONAL AND PROFESSIONAL HISTORY

Solomon Lefschetz was born in Moscow on September 3, 1884. He was a son of Alexander Lefschetz, an importer, and his wife, Vera, Turkish citizens. Soon after his birth, his parents left Russia and took him to Paris, where he grew up with five brothers and one sister and received all of his schooling. French was his native language, but he learned Russian and other languages with remarkable facility. From 1902 to 1905, he studied at the École Centrale des Arts et Manufactures, graduating in 1905 with the degree of mechanical engineer, the third youngest in a class of 220. His reasons for entering that institution were complicated, for as he said, he had been "mathematics mad" since he had his first contact with geometry at thirteen.

Since he was not a French citizen, he could neither see nor hope for a career as a pure mathematician. The next best thing was engineering because, as he believed, it used a lot of mathematics.

Upon graduating in 1905, Lefschetz decided to go to the United States, for a time at least, with the general purpose of acquiring practical experience. First, he found a job at the Baldwin Locomotive Works near Philadelpia. But he was particularly attracted to electrical engineering, which, at that time, was a nonexistent specialty at the École Centrale. In view of this, in January 1907 he became an engineering apprentice in a regular course at the Westinghouse Electric and Manufacturing Company in Pittsburgh. The course consisted of being shifted from section to section every few weeks. He wound up in the transformer testing section in the late fall of 1907, and in mid-November of that year, he was the victim of a testing accident, as a consequence of which he lost both hands.[2] After some months of convalescence, he returned to the Westinghouse Company, where, in 1909, he was attached to the engineering department in the section concerned with the design of alternating-current generators.

Meanwhile, Lefschetz had become increasingly dissatisfied with his work there, which seemed to him to be extremely routine. So he resumed, first as a hobby, his mathematical studies that had been neglected since 1903. After a while he decided to leave engineering altogether and pursue mathematics. He left the Westinghouse Company in the fall of 1910 and accepted a small fellowship at Clark University, Worcester, Massachusetts, enrolling as a graduate student. The mathematical faculty consisted of three members: William Edward Story, senior professor (higher plane curves, invariant theory); Henry Taber (complex analysis, hypercomplex number systems); and Joseph de Perott (number theory). At the École Centrale there were two professors of mathematics, Émile Picard and Paul Appel, and each had written a three-volume treatise: *Analysis* (Picard) and *Analytical Mechanics* (Appel). Lefschetz plunged into these and, with a strong French training in basic mathematics, was all set to attack a research topic suggested by Professor Story, namely, to find information about the largest number of cusps that a plane curve of given degree may possess. Lefschetz made an original contribution to this problem and obtained his Ph.D. summa cum laude in 1911. In the Record of Candidacy for the Ph.D., it is stated by Henry Taber that it was an "excellent examination, the best ever passed by any candidate in the department," and signed by him under the date June 5, 1911.

Clark University had a fine library with excellent working conditions, and Lefschetz made good use of it. By the summer of 1911 he had vastly improved his acquaintance with modern mathematics and had laid a foundation for future research in algebraic geometry. He had also become more and more closely associated with another mathematics student at Clark, Alice Berg Hayes, who became his wife on July 3, 1913, in North Brookfield, Massachusetts. She was to become a pillar of strength for Lefschetz throughout the rest of his life, helping him to rise above his handicap and encouraging him in his work.

Lefschetz' first position after Clark was an assistantship at the University of Nebraska in 1911; the assistantship was soon transformed into a regular instructorship. In 1913 he moved to the University of Kansas, passing through the ranks to become a full professor in 1923. He remained at the University of Kansas until 1924. Then, in 1924 came the call to Princeton University, where he was visiting professor (1924-25); associate professor (1925-27); full professor (1927-33); and from 1933 to 1953, Henry Burchard

Fine Research Professor, chairman of the Department of Mathematics 1945-53 and emeritus from 1953.

The years in the Midwest were happy and fruitful ones for Lefschetz. The almost total isolation played in his development "the role of a job in a lighthouse which Einstein would have every young scientist assume so that he may develop his own ideas in his own way."[3] His two major ideas came to him at the University of Kansas.

The first idea is described by Lefschetz as follows. Soon after his doctorate he began to study intensely the two-volume treatise of Picard-Simart, *Fonctions Algébriques de Deux Variables,* and he first tried to extend to several variables the treatment of double integrals of the second kind found in the second volume. He was unable to do this directly, and it led him to a recasting of the whole theory, especially the topology.[4] By attaching a 2-cycle to the algebraic curves on a surface, he was able to establish a new and unsuspected connection between topology and Severi's theory of the base, constructed in 1906, for curves on a surface. The development of these and related concepts led to a Mémoire, which was awarded the Bordin Prize by the French Academy of Sciences in 1919. The translated prize paper is given in the Bibliography (1921,3). The first half of the Mémoire, with some complements, is embodied in a famous monograph (1924,1).

The general idea for the second most important contribution also came to Lefschetz in Lawrence, Kansas, and it is the fixed-point theorem which bears his name. Almost all of Lefschetz' topology arose from his efforts to prove fixed-point theorems. In 1912, L. E. J. Brouwer proved a basic fixed-point theorem, namely, that every continuous transformation of an n-simplex into itself has at least one fixed point. In a series of papers, Lefschetz obtained a much more general result for any continuous transfor mation of a topological space X into itself where the restrictions on X were progressively weakened. In 1923, he proved the theorem for compact orientable manifolds and, by introducing relative homology groups, he extended it in 1927 to manifolds with boundary; his theorem then included Brouwer's. In 1927, he also proved it for any finite complex and, in 1936, for any locally connected topological space.

In the 1920s and 1930s, as a professor at Princeton University, Lefschetz was wholly occupied with topology, and he established many of the basic results in algebraic topology. For example, he created a theory of intersection of cycles (1925,1; 1926,1), introduced the notion of *cocycle* (which he called *pseudo-cycle*) and proved the Lefschetz duality theorem (see 1949,1 for an exposition of the fixed-point theorem and the duality theorem). His *Topology* was published in 1930 (1930,1), and his *Algebraic Topology* was published in 1942 (1942,1). The former was widely acclaimed and established the name *topology* in place of the previously used term *analysis situs;* the latter was less influential but secured the use of the name *algebraic topology* as a replacement for *combinational topology.* [5]

Lefschetz was an editor of the *Annals of Mathematics* from 1928 to 1958, and his influence dominated the editorial policy that made the *Annals* into a foremost mathematical journal.

In 1943 Lefschetz became a consultant for the U.S. Navy at the David Taylor Model Basin near Washington, D.C. There he met and worked with Nicholas Minorsky, who was a specialist on guidance systems and the stability of ships and who brought to Lefschetz' attention the importance of the applications of the geometric theory of ordinary differential equations to control theory and nonlinear mechanics. From 1943 to the end of his life, Lefschetz' main interest was centered around ordinary nonlinear differential equations and their applications to controls and the structural stabilities of systems. Lefschetz was almost sixty years old when he turned to differential equations, yet he did original work and stimulated research in this field as a gifted scientific administrator.

In 1946, the newly established Office of Naval Research funded a project on ordinary nonlinear differential equations, directed by Lefschetz, at Princeton University. This project continued at Princeton for five years past Lefschetz' retirement from the university in 1953. Meanwhile, the Research Institute for Advanced Study was formed in Baltimore, Maryland, as a division of the Glen L. Martin Aircraft Company, and in 1957, Lefschetz established the Mathematics Center under the auspices of the institute and was entrusted with the recruitment of five mathematicians and about ten younger associates. He obtained the cooperation of Professor Lamberto Cesari of Purdue University and appointed Professor J. P. LaSalle of Notre Dame and Dr. J. K. Hale of Purdue to the group, the former as his second in command. After some six years it was necessary to transfer the center elsewhere, and the move, carried out by LaSalle, resulted in their becoming part of the Division of Applied Mathematics at Brown University. The group was later named the Lefschetz Center for Dynamical Systems. LaSalle was director and Lefschetz became a visiting professor, traveling there from Princeton once a week. Lefschetz continued his work at Brown until 1970, two years before his death.

In 1944, Lefschetz joined the Institute de Mathematicas of the National University of Mexico as a part-time visiting professor, and this connection continued until 1966. At the Institute, he conducted seminars, gave volunteer courses, and continued his research. He found a number of ca pable young men there and sent several of them to Princeton University for further advanced training up to the doctorate and beyond. From 1953 to 1966 he spent most of his winters in Mexico City.

Lefschetz received many honors. He served as president of the American Mathematical Society in 1935-36. He received the Bôcher Memorial Prize of the American Mathematical Society in 1924, and in 1970 he received the first award of the Steele Prize, also of the American Mathematical Society. He received the Antonio Feltrinelli International Prize of the National Academy of Lincei, Rome, in 1956; the Order of the Aztec Eagle of Mexico in 1964; and the National Medal of Science (U.S.) in 1964. He was awarded honorary degrees by the University of Prague, Prague, Czechoslavakia; University of Paris, Paris, France; the University of Mexico; and Brown, Clark, and Princeton universities. He was a member of the American Philosophical Society and a foreign member of the Academie des Sciences of Paris, the Royal Society of London, the Academia Real de Ciencias of Madrid, and the Reale Instituto Lombardo of Milan.

A symposium in honor of Lefschetz' seventieth birthday was held in Princeton in 1954,[6] and in 1965 an international conference in differential equations and dynamical systems was dedicated to him at the University of Puerto Rico. The international Conference on Albegraic Geometry, Algebraic Topology and Differential Equations (Geometric Theory), in celebration of the centenary of Lefschetz' birth, was held at the Centro de Investigación del IPN, Mexico City, in 1984.

LEFSCHETZ AND ALBEGRAIC GEOMETRY

In order to discuss Lefschetz' contributions to algebraic geometry, I shall first describe that field and its evolution up until the period during which Lefschetz worked. Then I will give a somewhat more detailed description of some of his major accomplishments. I will conclude with a few observations about the impact of his work in algebraic geometry.

In simplest terms, algebraic geometry is the study of algebraic varieties. These are defined to be the locus of polynomial equations

$$P_1(x_1, \ldots, x_N) = 0, \ldots, P_r(x_1, \ldots, x_N) = 0. \qquad (1)$$

Here the x_i are coordinates in an affine space and the P_a are polynomials whose coefficients are in any field *K* For our purposes, it will be convenient to take *K* to be the complex numbers, as this was the case in classical algebraic geometry and in almost all of Lefschetz' work. It is worth noting, however, that he was one of the first to consider the case where *K* is an arbitrary algebraically closed field of characteristic zero. In fact, the so-called Lefschetz principle as expanded in his book *Algebraic Geometry* (1953, 1) roughly states that any result from the complex case remains valid in this more general situation.

In addition to using complex numbers, it is also convenient to add to the above locus the points at infinity. This is accomplished by homogenizing the polynomials P_a and considering the resulting locus V in the complex projective space $\mathbf{P}^N$ defined by the homogenized equations. Two algebraic varieties V and V' are to be identified if there is a rational transformation

$$T: x_i' = R_i(x_1, \ldots, x_N)$$

that takes *V* to *V*' and is generically one to one there. These are called birational transformations, and *T* estab lishes an isomorphism between the fields $K(V')$ and $K(V)$ of rational functions on V' and V, respectively.

In the nineteenth century the intensive study of algebraic curves—that is, algebraic varieties of dimension one—was undertaken by Abel, Jacobi, Riemann, and others. On an algebraic curve *C* given by a single affine equation,

$$f(x, y) = 0, \tag{2}$$

in the plane, special objects of interest were the abelian integrals

$$\int R(x, y)dx, \tag{3}$$

where $R(x,y)$ is a rational function. For example, the hyperelliptic integrals

$$\int \frac{R(x)dx}{\sqrt{(x-a_1\ldots(x-a_n)}} \tag{4}$$

are abelian integrals on the hyperelliptic curve $y^2 = (x\text{-}a1) \ldots (x\text{-}a_n)$ In addition to the indefinite integral (3), abelian sums

$$\sum_i \int^{p_i} R(x, y)dx$$

and periods

$$\int_\gamma R(x, y)dx,$$

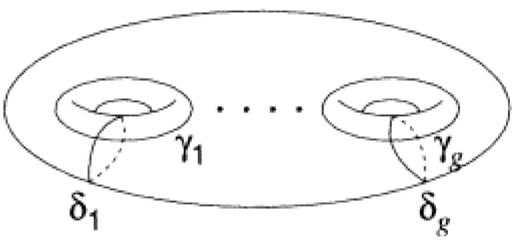
where *y* is a closed path on *C*, were of considerable interest. A major reason for studying abelian integrals and their periods was that these provided an extremely interesting class of transcendental functions, such as the elliptic function *p*(*u*) defined up to an additive constant by

$$u = \int^{p(u)} \frac{dy}{\sqrt{(x-a_1)(x-a_2)(x-a_3)}}.$$

It was Riemann who emphasized that studying C up to birational equivalence is equivalent to studying the abstract Riemann surface $\tilde{C}$ associated to the curve (2). Assuming that f is irreducible, in modern terms $\tilde{C}$ is a connected, complex manifold of dimension one for which there is a holomorphic mapping

$$\pi: \tilde{C} \to \mathbf{P}^2$$

whose image is C and where π: $\tilde{C} \to C$ is generically one to one. Viewed as an oriented real two-manifold, the Riemann surface $\tilde{C}$ has a single topological invariant, its genus g, and we have the familiar picture



where $\delta_1 \ldots, \delta_g, \gamma_1 \ldots, \gamma_g$ form a canonical basis for $H_1(\tilde{C}, \mathbf{Z})$.

The introduction of $\tilde{C}$ greatly clarifies the study of abelian integrals. For example, in terms of a local holomorphic coordinate z on $\tilde{C}$, the rational differential $\omega = R(x,y)dx$ above is given by the expression

$$h(z)dz,$$

where

$$h(z) = \sum_{k=-N}^{\infty} c_k z^k$$

is a meromorphic function. We then say ω is of the first kind if $h(z)$ is holomorphic in all local coordinate systems, of the second kind if all residues $c_{-1} = 0$, and of the third kind otherwise. For example, assuming that the a_i are distinct in (4), the holomorphic differentials on the hyperelliptic curve $y2 = (x-a_1) \cdot (x-a_n)$ are given by abelian integrals (4) where $R(x)$ is a polynomial of degree at most [n -1/2]. If the poles of ω are a finite set of points D, then the cycle γ is understood to be a class in $H_1(\tilde{C} -D, \mathbf{Z})$ and ω is of the second kind whenever its residues

$$\int_\gamma \omega = 0, \ \ \gamma = 0 \text{ in } H_1(\tilde{C}, \mathbf{Z})$$

are all zero. If $S(x,y)$ is a rational function on C, then clearly the exact differential $dS(x,y)$ is of the second kind.

Two results that relate the algebraic geometry and topology of an algebraic curve are these:

the vector space of differentials of the first kind has dimension equal to g; (5)

the vector space of differentials of the second kind modulo exact differentials has dimension equal to 2g = rank $H_1(\tilde{C}, \mathbf{Z})$. (6)

As we shall see, by understanding the topology of an algebraic variety, Lefschetz was able to give far-reaching extensions of these results.

Toward the end of the nineteenth century, the study of algebraic curves was extended by Max Noether and others, especially the Italian school led by Castelnuovo, Enriques, and Severi. In addition, these mathematicians and others began the systematic study of algebraic curves and algebraic surfaces from a projective or geometric point of view. To them, an algebraic surface S was the generic projection into $\mathbf{P}^3$ of a smooth algebraic surface $\tilde{S}$ lying in a $\mathbf{P}^N$. Thus, S is given by the single affine equation

$$f(x, y, z) = 0 \tag{7}$$

and has singularities from a very short list of special types. A major result was that any algebraic surface has such a smooth model $\tilde{S}$, and indeed an understanding was developed of all smooth models (in the case of curves, the Riemann surface $\tilde{C}$ is unique up to a biholomorphic transformation). Studying C or S by geometric methods operationally meant analyzing the various linear systems of divisors on the curve or surface. The specifics of these are not particularly relevant here; suffice it to say that little use was made of the topology and/or analysis on the variety.

Meanwhile, Picard and Poincaré in France had undertaken to extend results such as those in equations (5) and (6) to algebraic surfaces. Thus, they attempted to classify both single and double rational integrals

$$\int P\,dx + Q\,dy \tag{8}$$

$$\iint R\,dx\,dy \tag{9}$$

on the surface (7) in much the same way as had been done for curves. Here P, Q and R are rational functions of x, y, and z and it is assumed that the 1-form $P\,dx + Q\,dy$ is closed, i.e., that

$$\partial P / \partial y = \partial Q / \partial x .$$

In this case we may speak both of the indefinite integral (8) and of its periods, as in the curve case. For the double integral (9), however, only its periods are defined. These are expressions

$$\iint_{\gamma} R\, dx\, dy,$$

where γ is a closed 2-cycle in $\bar{S}$ supported outside the singular locus of *R dx dy*.

It is clear that the introduction of homology theory by Poincaré was essential for an understanding of rational integrals on a surface, and Poincaré's work on "analysis situs" was done while Picard was midstream in his own investigations. In what remains one of the "tour de forces" in the history of mathematics, over a period of about twenty years, Picard was able to arrive at a preliminary understanding of both single and double rational integrals on an algebraic surface. These investigations are detailed in the two volumes of *Traité des Fonctions Algébriques de Deux Variables* by Picard and Simart. There one may find a fairly complete extension (completed by Poincaré) of equations (5) and (6) to the rational integrals (8), an extension made possible by the understanding of the 1-cycles on an algebraic surface that was developed by Picard and by the theory of so-called normal functions due to Poincaré. For example, (5) becomes the statement that the number of closed, linearly independent holomorphic 1-forms on the surface is equal to the irregularity q, the irregularity being an algebro-geometric character that was later identified by Lefschetz as $b_1/2$, where $b_1 = \text{rank } H_1(\bar{S}, \mathbf{Z})$. In addition, by very lengthy arguments Picard was able to prove a number of results concerning the double integrals (9). One of these may be explained as follows: We consider the affine algebraic surface S_0 obtained by removing the hyperplane at infinity from S. Thus, S_0 is the surface in $\mathbf{C}^3$ given by the polynomial equation (7). We then consider rational 2-forms

$$\omega = R(x, y, z)dx\,dy$$

that are everywhere holomorphic on S_0 and, therefore, that have their poles at infinity. These are then given by such expressions where R is a polynomial that has certain specified properties relative to the singularities of S_0. Among such ω**'s** are the exact differentials

$$\omega = d\eta, \qquad (10)$$

where $\eta = P\,dx + Q\,dy$ is a rational 1-form on S that is holomorphic on S_0. Picard then proved that the pairing

$$< \omega, \gamma > \to \int_\gamma \omega$$

between equivalence classes of such ω**'s** modulo exact forms (10) and 2-cycles $\gamma \in H_2(S_0, \mathbf{Z})$ modulo torsion cycles was nondegenerate. This is essentially equivalent to what is now known as the algebraic deRham theorem for algebraic surfaces.

As far as Picard and Poincaré were able to push things, certain difficulties remained. One was the relationship of the "curve at infinity"—i.e., $S \backslash S_0$—to the topology of S. In the third paragraph of his "A Page of Mathematical Autobiography" (1968, 2), Lefschetz refers to this when he says:

> From the $\boldsymbol{\rho}_0$ formula of Picard, applied to a hyperelliptic surface $\boldsymbol{\Phi}$ (topologically the product of 4 circles) I had come to believe that the second

Betti number $R_2(\Phi) = 5$, whereas clearly $R_2(\Phi) = 6$. What was wrong? After considerable time it dawned upon me that Picard only dealt with *finite* 2-cycles, the only useful cycles for calculating periods of certain double integrals. Missing link? The cycle at infinity, that is the plane section of the surface at infinity. This drew my attention to cycles carried by an algebraic surve, that is to *algebraic* cycles, and . . . the harpoon was in!

Another difficulty, one that is totally unlike anything encountered in the study of rational integrals on curves, is that we may have a relation (10) where, for *any* η satisfying that equation, the poles of η are strictly longer than those of ω. In modern terminology, ω defines a class in $H^2(S_0)$ that is nonzero but whose image vanishes in $H^2(S_0 \backslash D_o)$ for a suitable divisor D_o on S.

It was at this stage that Lefschetz entered the picture. By systematically studying and understanding the topology of an algebraic surface, and also of a general *n*-dimensional algebraic variety, Lefschetz was able to solidify and considerably extend the results of Picard and Poincaré. In so doing, he once and for all established the principle that the understanding of the topology of an algebraic variety was central and essential in algebraic geometry. According to Hodge,[7] "our greatest debt to Lefschetz lies in the fact that he showed us that a study of topology was essential for all algebraic geometers."

Before discussing the work of Lefschetz in more detail, I would like to remark that it was seeking a better understanding of Lefschetz' results on the topology of an algebraic variety that led Hodge to his work on harmonic integrals as detailed in his book *The Theory and Applications of Harmonic Integrals* (Cambridge, 1988). Subsequently, it is fair to say that algebraic geometry has been central to the development of mathematics in the last forty years, and that the strand of what might be called "transcendental algebraic geometry" as represented by Lefschetz and

Hodge is now completely interwoven into algebraic geometry and into the rest of mathematics.

Turning to Lefschetz' work in algebraic geometry, his approach to the study of the topology of an algebraic variety was typically direct and intuitive. He treated an algebraic variety as a concrete object and analyzed its topology directly. In modern terms, he realized a smooth n-dimensional $\tilde{V}$ by generally projecting it to a hypersurface V in $\mathbf{P}^{n+1}$ given by the affine equation

$$f(x_1, \ldots, x_n, y) = 0. \tag{11}$$

Questions of singularities were by and large circumvented by suitably lifting various constructions on V up to $\tilde{V}$. Now it was Picard who systematically analyzed the algebraic surface (7) by considering it as "fibred" by the ∞^1 algebraic curves C_y given by

$$f(x, y, z) = 0, \quad y = y.$$

This, in turn, was an extension of the classical method of studying the algebraic curve (2) by considering it as a branched covering over the x- axis. The analogue of branch points are then the singular curves $\mathbf{C}_y$ where the plane y = y is tangent to the surface $f(x,y,z) = 0$. Picard thus considered $\tilde{S}$ to be made up of the ∞^1 curves $\tilde{C}_y$ all but the singular ones having the picture above with retrosections $\delta_1 \ldots, \delta_g, \gamma_1 \ldots, \gamma_g$. The behavior of this topological picture was then analyzed. This method was then formalized and extended by Lefschetz, who proceeded to study the smooth variety $\tilde{V}$ inductively by closely examining its hyperplane sections given by (11), together with $\Sigma c_i x_i =$ constant.

More specifically, choosing coordinates generically, we consider the Lefschetz pencil of (n-1)-dimensional varieties W_t given by

$$\begin{cases} f(x_1, \ldots, x_n, y) = 0 \\ \qquad\qquad x_n = t. \end{cases}$$

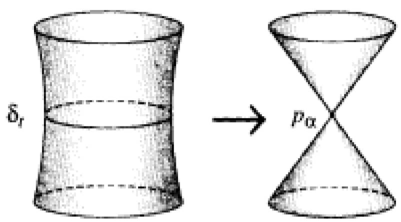
The lifts $\tilde{W}_t$ of W_t to $\tilde{V}$ are then smooth except for a finite number of critical values $t = t_1, \ldots, t = t_N$, where the hyperplane $x_n = t_\alpha$ is simply tangent to W_t at one finite point p_α. In a suitable local analytic coordinate system $z_1, \ldots, z_{n+1}$ in $\mathbf{P}^{n+1}$ centered around p_α, the Lefschetz pencil has the analytic equation

$$z_1^2 + \ldots + z_{n+1}^2 = t - t_\alpha,$$

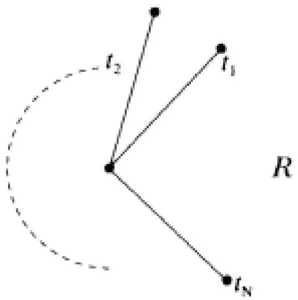
and from this a complete and explicit analysis of the topology of the $\tilde{W}_t$ as $t \rightarrow t_\alpha$ is possible. The result is that locally $\tilde{W}_t$ retracts onto the real n-sphere δ_t given by

$$\begin{cases} z_1^2 + \ldots + z_{n+1}^2 = t - t_\alpha \\ \operatorname{Im} z_i = 0 = Im\ (t - t_\alpha) \end{cases}$$

This cycle then shrinks to a point or vanishes as $t \rightarrow t_\alpha$. All this may be pictured in the case $n = 1$ by

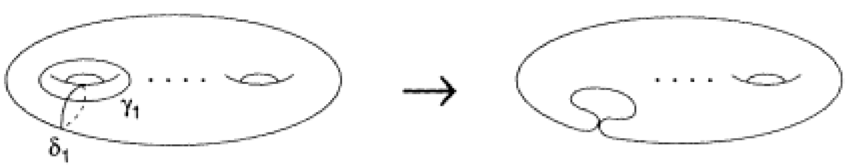


Assume now that, by an inductive procedure, the topological structure of the $\tilde{W}_t$ is known in a manner to be made more explicit below. Next, picture the t-plane with slits drawn to the critical values



Over the simply connected region R on the t-sphere minus the slits, the family $\tilde{W}_t$, $t \in R$ is topologically trivial and is thus a product. More precisely, if we assume that inductively $\tilde{W}_t$, $t \in R,$ has been represented as a real $2(n-1)$ cell e_t^{2n-2} , with suitable identifications on the boundary, then the family e_t^{2n-2} , $t \in R,$ describes a $2n$-cell e^{2n}. The boundary of e^{2n} consists of the family of boundaries of the e_t^{2n-2} , which are assumed to be known inductively, together with the identifications that result from the identifications across the slits $t_0 t_\alpha$, as depicted above. These latter identifications are then known from a local analysis of the singularities around the points p_α .

In the case $n = 1$, the cells e_t^2 are obtained by cutting the Riemann surface $\tilde{W}_t$ along the retrosections $\delta_1 \ldots, \delta_g, \gamma_1 \ldots, \gamma_g$. As $t \to t_\alpha$ we have a global picture



where the cycle δ_1 vanishes. An explicit local analysis of this picture around $t = t_\alpha$ gives the identification

$$\delta_1 \sim \delta_1 + \gamma_1$$

to be made across the slit $t_0 t_\alpha$.

I have given this description not so much to attempt to reproduce Lefschetz as to illustrate his way of working, which, as mentioned, was concrete and direct. He had marvelous intuition, and so far as I know, all of the results he claimed in algebraic geometry have now been proved. When I was a graduate student at Princeton, it was frequently said that "Lefschetz never stated a false theorem nor gave a correct proof." In the case of the method of Lefschetz' pencils, it was later recognized that he was using t as a complex Morse function, log $|t-t_0|$ *t01* being the real Morse function, and this then led to the very beautiful derivation of Lefschetz' theorems, as given by Andreotti-Frankel (in *Global Analysis,* Princeton University Press, 1969).

These theorems may be summarized as follows: denote by $\tilde{W}$ a general smooth hyperplane section $\tilde{W}_{t_0}$ of $\tilde{V}$ and consider the inclusion mapping

$$j: H_i(\tilde{W}, \mathbf{Z}) \rightarrow H_i(\tilde{V}, \mathbf{Z}). \qquad (12)$$

Then, to begin with, we have:

the mappings (12) are isomorphisms for $i \leq n-2$ and are subjective for $i = n-1$; (13)

for $i = n-1$, the kernel of j is generated by the vanishing cycles d_α associated to the various critical points $t = t_\alpha$. (14)

These two results have the effect of reducing the topology of a variety below the middle dimension to that of its hyperplane sections. To understand the crucial middle ho mology group from the exact homology sequence of the pair ($\tilde{V}$, $\tilde{W}$) and assuming an inductive understanding of H_n ($\tilde{W}$,**Z**), we need to understand the relative group H_n ($\tilde{V}$, $\tilde{W}$;**Z**) and this is given by the statement:

the group H_n ($\tilde{V}$, $\tilde{W}$;**Z**) is generated by the relative cycles Δ_α described by the locus of the vanishing cycle δ_t as t traverses the slit $t_0 t_\alpha$. (15)

In addition to giving the δ_α and Δ_α as generators of suitable groups, Lefschetz also gave the generators for the relations among these cycles and thereby arrived at a complete understanding of the H_i ($\tilde{V}$, **Z**) for $i \leq n$. For $i > n$ we consider the mappings

$$L^k : H_{n+k}(\tilde{V},\mathbf{Q}) \to H_{n-k}(\tilde{V},\mathbf{Q}), \qquad (16)$$

obtained by intersecting a cycle with k general hyperplane sections. Then we have

the mapping (16) is an isomorphism. (17)

This result, the so-called Hard Lefschetz Theorem, has an interesting history. To see what it means, we consider the case of algebraic surfaces. Over the punctured t-sphere $B = \mathbf{P}^1 \backslash \{t_1, \ldots, t_N\}$, we have a family $\tilde{C}_t$, $t \in B$, of smooth algebraic curves. By transporting cycles $\tilde{C}_t$ around closed loops on B, we obtain the so-called monodrony action of $\pi_1(B)$ on H_1 ($\tilde{C}$,**Z**). The effect of moving cycles around a critical point t_α is given by the Picard-Lefschetz transformation

$$\gamma \to \gamma + (\gamma \cdot \delta_\alpha)\delta_\alpha,$$

where $\gamma \cdot d_\alpha$ is the intersection number. It follows from (13) and (14) that H_1 ($\tilde{S}$) is isomorphic to

$$\{\gamma \in H_1(\bar{C}) : (\gamma \cdot \delta_\alpha) = 0 \text{ for } \alpha = 1, \ldots, N\}.$$

This is just the space of invariant cycles, i.e., classes in $H_1(\bar{C})$ that are invariant under the action of $\pi_1(B)$.

Given a 3-cycle Γ on $\bar{S}$ that is in general position, the intersections

$$\Gamma \cdot \bar{C}_t = \gamma_t$$

give a family of cycles invariant under $\pi_1(B)$ and the map $L: H_3(\bar{S}) \rightarrow H_1(\bar{S})$ is just $\Gamma \rightarrow \gamma_{t_0} = \gamma \in H_1(\bar{C})$ followed by the natural mapping $H_1(\bar{C}) \rightarrow H_1(\bar{S})$. Conversely, if γ is an invariant cycle, then the locus Γ of its translates γ_t is a 3-cycle with $\Gamma \cdot \bar{C} = \gamma$. Thus, the mapping L is subjective, and to prove that it is injective, we must show that

an invariant, vanishing cycle is zero. (18)

It is easy to see that (18) is equivalent to the assertion the intersection form on the space of vanishing cycles is nondegenerate, (18)'

and that, in turn, is a consequence of the action of $\pi_1(B)$ on $H_1(\bar{C})$ is semi-simple. (18)"

Clearly, (18)-(18)" are global statements about monodrony, and although (18)" is true, the only existing proofs require the use of Hodge theory or its effective equivalent. This is a case in which Lefschetz' intuition was right on target, but the direct, geometric approach was insufficient to give a complete proof.

As a consequence of the results (13)-(17), Lefschetz deduced the properties

$$\begin{aligned} b_{2i+1} &\equiv 0 \bmod 2 \\ b_{2i} &\geq 1, \quad i \leq n \end{aligned} \qquad (19)$$

on the Betti numbers b_i of a smooth n-dimensional variety. The first inequality in the case $i = 0$, $n = 2$ is a consequence of (18)', since an alternating bilinear form can be nondegenerate only on a vector space of even dimensions; the general case is proved by similar considerations.

The second inequality above arises from the following considerations. First, any k-dimensional subvariety $\tilde{U} \subset \tilde{V}$ carries a fundamental cycle $[\tilde{U}] \in H_{2k}(\tilde{V}, \mathbf{Z})$.(Typically, Lefschetz considered this as obvious.) Second, for subvarieties $\tilde{U}_1$ and $\tilde{U}_2$ of complementary dimensions and meeting in isolated points, the algebro-geometric and topological intersection numbers are equal, namely

$$\tilde{U}_1 \cdot \tilde{U}_2 = [\tilde{U}_1] \cdot [\tilde{U}_2].$$

Finally, a general k-fold hyperplane section $\tilde{U}$ and a general (n-k)-fold hyperplane meet in a positive number of points, and consequently $[\tilde{U}] \neq 0$. The argument just given is clearly a special case of the Hard Lefschetz Theorem (16); the point of putting it here is that it explains how Lefschetz' attention was drawn to the part $H^{alg}_{2k}(\tilde{V}, \mathbf{Z})$ of the homology of a smooth variety that is represented by algebraic cycles, i.e., by linear combinations of fundamental classes of k-dimensional algebraic subvarieties.

Lefschetz was especially interested in the part $H^{alg}_{2n-2}(\tilde{V}, \mathbf{Z})$ given by the fundamental classes of the divisors on $\tilde{V}$. Here, his results were definitive. To state them we shall specialize to the case of algebraic surfaces, although every thing extends to *n*-dimensions. Two divisors D_1, D_2 on an algebraic surface are said to be algebraically equivalent in case there is an algebraic family $\{D_l\}$ of divisors parameterized by an auxilary variety Λ and such that $D_i = D_{\lambda_i}$ for suitable points λ_1, λ_2 on Λ. It is clear that $[D_1] = [D_2]$ if D_1 and D_2 are algebraically equivalent, and Lefschetz proved the converse. This then established once and for all the so-called theory of the base and the finiteness of the rank p of the group of divisors modulo algebraic equivalence.

Now, this number p = $H_2^{\text{alg}}(\bar{S}, \mathbf{Z})$ had been discussed by Picard in his attempt to classify the double integrals (9). Lefschetz was now able to give the generalization of (6) to double integrals by completing Picard's 'algebraic deRham' result to

> the dimension of the space of rational differentials $\omega = R(x,y)dx\,dy$ of the second kind modulo exact forms is equal to b-p. (20)

Put another way, Lefschetz was able to show that the occurrence of the relation (10) where poles (η) > poles (ω) was attributable to the nonhomologous algebraic 1-cycles on $\bar{S}$.

A related result, the famous Lefschetz (1,1) theorem, characterized the algebraic part $H_2^{\text{alg}}(\bar{S}, \mathbf{z})$ **of** $H_2(\bar{S}, \mathbf{z})$. The result is:

> A necessary and sufficient condition that a class $\Gamma \in H_2(\bar{S}, \mathbf{Z})$ be of the form $\Gamma = [D]$ for (21)

some divisor *D* is

$$\int_\Gamma \omega = 0$$

for all holomorphic two-forms ω on $\bar{S}$. The conjectured extension of (21) to higher dimensions and codimensions is due to Hodge, and it remains one of the major unsolved problems in algebraic geometry.

To conclude this section on Lefschetz and algebraic geometry, I would like to briefly comment on Lefschetz' subsequent influence on algebraic geometry. In the first forty or so years of the twentieth century there were two principal strands in algebraic geometry; one was the geometric approach of the Italian school, and the other was the transcendental approach as represented by Lefschetz and Hodge and continued by Kodaira and Spencer. In the last half-century a third strand, the algebraic approach of Weil, Zariski, and Grothendieck was added, and all three strands have now become intertwined. There is no better illustration of this than the Weil conjectures. These purely arithmetical statements were formulated by Weil, who also understood that they could be proved if a "suitable" cohomology theory could be developed for varieties defined over a field of finite characteristic. A *suitable* cohomology theory is essentially one for which Poincaré duality and the various Lefschetz theorems stated above, in particular the Hard Lefschetz Theorem, could be established. Such a cohomology theory was introduced by Grothendieck, and as is well known, the Weil program was completed by Deligne, who in effect used an inductive procedure reminiscent of the Lefschetz pencil method to prove an arithmetic analogue of the Hard Lefschetz Theorem.

When we look at algebraic geometry today, we not only see the intertwining of the historical strands within the field, but equally, we see algebraic geometry intertwined with the rest of mathematics and central to the ongoing developments of the field. One illustration of this is the use of algebraic geometry to generate solutions of special differential equations, both ordinary and partial. Here I mention the work originating from the Russian school that constructs commuting integrals of special Hamiltonian dynamical systems from the Jacobian variety of algebraic curves, and the Atiyah-Hitchin-Drinfeld-Manin construction of special solutions to the Yang-Mills equations from suitable algebraic vector bundles. To some extent it may be said that we have come full circle to the historical roots of algebraic geometry in the study of special transcendental functions arising from abelian integrals, abelian sums, and periods as explained above. In all of these developments, the topological properties of algebraic varieties, as part of the infrastructure of algebraic geometry, play a central role.

I would like to mention a very beautiful recent development that exemplifies both a style and subject that are direct descendents of Lefschetz. This is the intersection homology theory of algebraic varieties due to Goresky-MacPherson, a theory that was designed to retain Poincaré duality and that reflects the topological properties of singularities in much the same way as ordinary homology, in the hands of Lefschetz, reflected the topology of smooth varieties. On reading the original paper of Goresky-MacPherson (*Inventions Math.,* Vol 71, 1983) one cannot help but be struck by the similarity in viewpoint to Lefschetz.

Lefschetz' influence in algebraic geometry clearly places him in the tradition of Abel, Jacobi, Riemann, Poincaré, and Picard, whose works he drew so much from and extended in such a significant way. This influence is manifest today in his theorems, some of which were stated above, in the intertwining of topology and algebraic geometry, and in his overall approach to mathematics.

LEFSCHETZ THE TOPOLOGIST

Much of Lefschetz' work in topology is concerned with the notion of "fixed point." If f is a continuous mapping of a space X into itself, a fixed point of f is a point x such that $f(x) = x$. The first important result of fixed-point theory was proved by L. E. J. Brouwer in 1912. It asserts that, if E is a closed n-cell, then *every* mapping of E into itself has (at least) one fixed point. This result becomes false if E is replaced by a space with a more complicated topological structure. For example, if S is the unit sphere in Euclidean $(n + 1)$-space and f is the antipodal map, given by the formula $f(x) =-x$ for all points x of S, then f has no fixed points. On the other hand, any mapping of S into itself has a well-defined *degree* $d(f)$ [intuitively, $d(f)$ is the number of times that f maps S around itself; a more precise definition is given in the next paragraph]. It can be proved that any map whose degree is different from the degree $(-1)^{n+1}$ of the antipodal map has a fixed point.

If X is a compact polyhedron and q is a non-negative integer, the q th homology group of X with rational coefficients is a rational vector space $H_q(X)$ of finite dimension; and a map f: $X \to X$ induces a homomorphism $f_q : H_q(X) \to H_q(X)$, which can be represented by a square matrix with rational (in fact, with integral) entries. If t_q is the trace of this matrix, the *Lefschetz number* of f is the alternating sum $L(f)$ of the integers t_q. The homomorphisms f_q depend only on the homotopy class of the map f and therefore the same is true for the Lefschetz number. In particular, if g is a sufficiently close approximation to f, then $L(f) = L(g)$ When X is the n-sphere, the only nontrivial homology groups of X occur in dimensions o and n, and the latter groups are one-dimensional. Thus, t_o and t_n are integers; in fact, $t_o = 1$ and $t_n = d(f)$, so that

$$L(f) = 1 + (-1)^n d(f).$$

Thus, the above result on self-maps of the n-sphere is subsumed in the Lefschetz Fixed-Point Theorem: *If X is a* *compact polyhedron and* f: X → X *a map such that* L(f) ≠ 0, *then* f *has a fixed point.*

In his first proof of the fixed-point theorem in 1923 (1923, 1), Lefschetz made the additional assumption that X is an orientable closed *n*-manifold. One can approximate the map *f* by a map *g* that has only a finite number of fixed points and that is well-behaved near each fixed point *x* in the sense that *g* maps some neighborhood of *x* homomorphically upon another neighborhood. The index *i*(*x*) is then defined to be +1 or -1 according to whether *g* preserves or reverses orientation about *x.* The Cartesian square of *X* is an orientable (*2n*)-manifold *Y,* and the diagonal *D* and the graph *G* of *g* can be regarded as *n*-cycles in *Y*. Their intersection consists of all points (*x,x*) such that *x* is a fixed point of *g*; it is a zero-cycle *y* of *Y* whose Kronecker index *I*(*y*) is easily seen to equal the sum of the indices of the fixed points of *g*. But *I*(*y*) can be calculated in a different way, using the Künneth theorem on the homology groups of a product space and the Poincaré duality theorem in *Y*, with the result that *I*(*y*) = *L* (*g*). If *f* has no fixed points, we may assume that *g* has none either; then *L*(*f*) = *L* (*g*)= *I*(*y*) = 0.

While this proof is attractive, it suffers from the disadvantage that it fails to include the Brouwer theorem as a special case. This is because the n-cell is not a closed manifold, but rather a manifold with a boundary; and the above proof breaks down because of the failure of Poincaré duality for manifolds with a boundary. It was to remedy this situation that Lefschetz invented relative homology.

Like many other results of the time, the Lefschetz Duality Theorem was awkward to state because the correct concepts had not yet been developed. Expressed in modern language, it asserts that, if *X* is a (compact, oriented) *n*-manifold with regular boundary *A,* then the relative ho mology group $H_q(X,A)$ is isomorphic with the cohomology group $H^{n-q}(X)$. For the purpose of proving the fixed-point theorem, it suffices to know that the homology groups $H_q(X,A)$ and $H_{n-q}(X)$ are dual vector spaces. This was sufficient to modify the proof above to cover the case of manifolds with regular boundary, and this was done by Lefschetz in 1927 (1927, 2).

The importance of the Lefschetz duality theorem was not limited to this application. Of course, if A = Ø it reduces to the Poincaré duality theorem. On the other hand, if A is a subcomplex of a triangulation of the n-sphere S, and if U is a regular neighborhood of A, then S-U is a manifold with regular boundary $b(U)$, to which we may apply the Lefschetz duality theorem to conclude that $Hq[S\text{-}U,b(U)]$ and $H^{n-q}(S\text{-}U)$ are isomorphic. Using standard (by now!) techniques of homology theory, we recover the Alexander duality theorem. Thus, the Lefschetz duality theorem appears as a unifying factor, connecting two important but apparently unrelated results.

Not content with this version of the fixed-point theorem, Lefschetz continued to seek generalizations. In 1928 Hopf had proved the theorem for arbitrary compacy polyhedra, but with some restriction on the map f. By 1934 Lefschetz had succeeded in removing the latter restrictions (1934,2), and by 1937 he was able to remove the hypothesis of triangulability of X, requiring instead that X be a compactum which is homologically locally connected in a suitable sense (1937,4).

The extensions of the fixed point theorem to more general spaces are not simply generalizations for their own sake. Indeed, fixed point theorems often appear in analysis as tools for proving existence theorems. To mention a very simple example, consider the ordinary first-order boundary problem

$$y' = F(x, y), \quad y(x_0) = y_0,$$

or, equivalently, the integral equation

$$y(x) = y_0 + \int_{x_0}^{x} F[x, y(x)]dx.$$

The correspondence that associates to each function y the function defined by the right-hand side of the latter equation may be regarded as a mapping *f* of a suitable function space into itself. And a solution of the equation is nothing but a fixed point of *f*. To be sure, the function spaces appearing here and in other places in analysis are far from being compact, and so the Lefschetz theorem does not apply directly. Nevertheless, this point of view has been a very powerful one in modern analysis.

The importance of Lefschetz' work, however, is not limited to the study of fixed-point theorems. The notion of a fixed point of a map of a space into itself can be thought of as a special case of that of a *coincidence*. If f, g: $X \rightarrow Y$ are maps, a coincidence of f and g is a point x such that $f(x) = g(x)$ If $Y = X$ and f is the identify map, a coincidence of f and g is simply a fixed point of g. The graphs $G(f)$ and $G(g)$ of f and g are subspaces of $X \times Y$, and the coincidences of f and g correspond to the intersection of their graphs. If X and Y are manifolds of dimensions m and n, respectively, then $G(f)$ and $G(g)$ may be regarded as m-cycles in the $(m + n)$-manifold $X \times Y$. Now the theory of intersections in algebraic geometry was an old one; if U and V are subvarieties of an algebraic variety W, in general position, their intersection is a subvariety of dimension $p + q$-n, where p, q, and n are the dimensions of U, V, and W, respectively. This suggested to Lefschetz the idea of defining intersections in an orientable manifold M. This he succeeded in doing with the result that the graded homology group of M is a ring, the *intersection ring* of M (1926,1).

All this took place before the discovery of cohomology. While Lefschetz did not define cohomology groups, he introduced *pseudo-cycles* in 1930 (1930,4). They were not defined intrinsically but, rather, were defined in terms of an embedding of the space in question in a sphere, and were used only as a tool for the proof of one of the versions of his fixed-point theorem. It was not until the late 1930s that the modern treatment of cohomology and cup products was given.

Other of Lefschetz' ideas that by now have thoroughly permeated the subject include singular homology theory and relative homology. While Lefschetz was not the first to use singular chains, his Colloquium Lectures (1930,1) gave the first formal treatment of the theory. His theory had some mild defects (the chain groups turned out not to be free), but these were corrected by Eilenberg in 1944, and the resulting theory has been of the greatest importance. As for relative homology groups, they are principal ingredients in the axiomatic treatment of the homology theory by Eilenberg and Steenrod, which has been so influential in the development of the subject in the last thirty or so years.

LEFSCHETZ AND ORDINARY DIFFERENTIAL EQUATIONS

Lefschetz was nearly sixty years old when he turned to differential equations, and he devoted the last twenty-five years of his life to the subject. He wrote over forty papers, articles, and books in this field and formed around him a vigorous and distinguished school, guiding and encouraging students and young mathematicians to work on problems of significance. In fact, he rekindled interest in a subject that had been nearly totally neglected in the United

States, and he recognized its mathematical importance and practical implications.

Although Lefschetz' own contributions to differential equations, control theory, and dynamical systems are not comparable to his great work in algebraic geometry and topology, he nevertheless wrote noteworthy original papers in these areas. His main interests centered around the theory of dissipative (as distinct from conservative) dynamical systems, including structural stability, and the resolution of singularities of critical points and bifurcating periodic orbits.

Dissipative dynamical systems are important in engineering problems where friction and resistance are essential ingredients. Such dynamical systems can be represented as vector fields on the phase-space manifold. Let S be the set of all C^1 vector fields on a compact differentiable manifold M without boundary and assign to S the C^1-topology. Two systems V_1 and V_2 of S are said to be qualitatively equivalent if there is a homomorphism of M mapping the unparameterized solution curves of V_1 onto those of V_2 . A dynamical system V is called *structurally stable* if there is a neighborhood N of V in S such that each $V_1 \in \mathrm{N}$ is qualitatively equivalent to V. Structurally stable differential systems are important in applied problems where the parameters of the physical process are known only approximately.

Lefschetz stimulated and guided work on these qualitative problems of global analysis. H. F. DeBaggis improved results of Pontryagin for the sphere $S^2 = M$, and M. Peixoto proved that the structurally stable systems on a compact surface form an open dense subset of S. L. Markus proved that, on arbitrary n-dimensional manifolds M, a structurally stable system must necessarily have isolated and elementary critical points and periodic orbits.

Lefschetz was the first person from outside the former Soviet Union to recognize the importance of Liapunov's stability theory, and he made an important contribution to the Lurie stability problem, one of the first applications of the Liapunov theory to a nonlinear control problem. He opened up the field of the mathematical theory of control, and in 1951, one of his students, Donald Bushaw, gave the first complete solution of a nontrivial problem in optimal control.

Among his other original contributions was his work on the behavior of solutions of analytic differential equations near an isolated singular point. He gave a complete characterization and a constructive procedure for obtaining all the solution curves of a two-dimensional system near an isolated critical point that pass through this critical point (1968,1). For a two-dimensional analytic system for which the coefficient matrix of the linear variational equation of an isolated critical point has both roots zero but is not identically zero, he proved that there can be at most a single nested oval of orbits (1958,1). He gave one of the best treatments of the method of determining the stability of an isolated equilibrium point of an n-dimensional system for which the linear variational equation has some zero roots(1961,1). He also studied the existence of periodic solutions of second- and higher-order nonlinear systems of differential equations (see 1943,2; 1954,2).

NOTES

1. Phillip Griffiths wrote the section on algebraic geometry, Donald Spencer wrote the sections on personal history and ordinary differential equations, and George Whitehead wrote the section on topology.

2. The date of the accident has been given incorrectly by several authors. The account of it here is taken from a communication by Lefschetz to the Academy dated January 8, 1954, and entitled "A Self Portrait," an unpublished document that was requested by A. Wetmore on behalf of the Academy.

3. From "A Self Portrait" (see note 2).

4. Topology can be described as the study of *continuous* functions, and it is customary to use the work "map" or "mapping" when referring to such functions.

5. F. Nebeker and A. W. Tucker, "Lefschetz, Solomon," in *Dictionary of Scientific Biography,* Supplement II, 1991.

6. *Algebraic Geometry and Topology, a Symposium in Honor of S. Lefschetz,* edited by R. H. Fox, D. C. Spencer, and A. W. Tucker, Princeton University Press, 1957, pp. 1-49.

7. *Ibid,* note 6.

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Arthur Weever Melton

August 13, 1906-November 5, 1978

by Michael I. Posner

Arthur Melton was already a full professor of psychology, a brigadier general in the Air Force Reserve, and the editor of the *Journal of Experimental Psychology* when I first met him as a new graduate student at the University of Michigan in 1959. It was only the second year of his return to academia after having served as technical director of the Air Force effort in human personnel for eight years. Of course, as is typical of graduate students, I assumed he had always been at Michigan, although I remember now references to his time at Ohio State. I mention this because I was neither a contemporary nor, still less, in a position equal to Melton's, and thus I could not know him as those who were his close friends did. Rather, I knew him as an imposing force in the field, as a teacher of considerable influence, and as a role model even for those who were not direct students of his.

More personal memoirs of Melton have been written by Benton Underwood, who was Melton's student at Missouri; by Robert Daniel, who also knew Melton at Missouri; by Frank Geldard, who knew Melton during the period of his Air Force work; and by Jim Greeno and Wilbert McKeachie, who were colleagues and coworkers at Michigan. These writers divide Melton's work into his early academic period at Missouri, his Air Force period, and the later period in which the Human Performance Center was built at Michigan. Perhaps because my daily involvement with Melton was much less extensive than theirs or because fourteen years have passed since his death, it seems possible to view these separate periods more as a whole and see the continuity of his developing ideas about psychology in relation to changes in the field.

Recent histories of experimental psychology have often identified the study of information processing entirely with developments stemming from the efforts to use computers to stimulate higher-level human functions. Indeed, the work of Herbert Simon and Allen Newell, among others, has been very influential in developing cognitive science. The Michigan Human Performance Center, together with the Applied Psychology Unit in Cambridge, Massachusetts, represented a different tradition that also contributed to the view of the human as an information processor. The Michigan approach arose from the functionalist tradition. It emphasized quantitative empirical relationships. These included Fitts' law relating movement speed to its distance and accuracy of termination, the Hick-Hyman law relating reaction time to the number of alternatives, and Melton's own empirical relation between the recallability of an item and the spacing between its repetitions.

Analogies between the human mind and technology change over the years. We are currently undergoing a shift from very strong identification of mental processes with serial computer systems to an emphasis on parallel activation. It is quite interesting to see that these new theoretical proposals are being tested against the quantitative empirical generalizations that were developed in the functional tradition that Melton practiced. The fourteen years that have elapsed between Melton's passing and this memoir make it possible to apply to his influence the kind of empirical check that was so characteristic of his approach. The 1987 social science citation index listed ten of Melton's publications. His effort to summarize the influence of the second presentation of an item on its memory was the most frequent, with thirteen citations nearly twenty years after its publication. I hope this memoir will provide some background for appreciating the nature of Melton's contribution.

EDUCATION

Arthur Melton came to Washington University in St. Louis, Missouri, in 1924 as an eighteen-year-old undergraduate from Arkansas. He found there then, as he would now, a strong emphasis on the functional analysis of human behavior. Functionalism held that a detailed analysis of the tasks of daily life could help us understand how humans relate to their environment and how the environment might be improved to accommodate human nature. John A. McGeoch, with whom Melton worked as an undergraduate, performed studies exploring how the distribution of practice, rest, and interpolated learning affects the formation and loss of associations. This work led to McGeoch's textbook *Psychology of Human Learning*. It was largely a compendium of experimental findings of functional relationships between variables that could be manipulated by the experimenter and the changes in performance that they caused. There was, however, the start of a theory of forgetting embedded in this material. Information was not lost merely through a period of disuse, but rather as a function of the amount and type of learning between the original learning and testing. Ten years later Melton would make a substantial experimental contribution to the "interference theory" of forgetting.

Melton received his B.A. degree in 1928 and went on to graduate study at Yale University. Four years later he took his Ph.D. degree with Edward S. Robinson. Robinson had come to Yale from Chicago and was himself a contributor to the functional tradition of verbal learning.

At the height of the depression, even a psychology degree from Yale University was no guarantee of a scarce academic position. However, Robinson had been involved since 1925 in a project supported by the Carnegie Corporation and the American Association of Museums, and Melton was employed for three years in studies leading to important monographs on the actual behavior of visitors to museums and a comparison of methods of instructing children in science museums.

Two things of great importance for Melton's future work emerged from that assignment. First, he learned that psychology could make contributions to real-life problems. Second, he began to explore what process might produce the functional relationships he observed. Consider as an example Melton's measurement of the time that visitors remained in front of the displayed paintings (dwell time). He plotted the dwell time for each month of museum operation. The dwell time was at a minimum in the summer months and reached a peak in the winter. This was true both for weekday and Sunday visitors. The reason for this, Melton speculated, was not in the behavior of individuals, as would be the case if, during the summer season, each person reduced the time he or she stayed indoors, but rather in the type of visitor attracted to the museum in the winter and summer seasons. While Melton was frustrated in not being able to prove this speculation in the museum setting, he was able to show that the visits of relatively uninterested visitors increased toward summer, presumably because of the ease of getting there, while those of relatively interested visitors declined, possibly because of competition from other activities. These studies illustrated both the functional approach of looking at consistent generalizations from systematic empirical data and also an effort to go beyond the functional relationship toward the development of a process that might underlie the functional relationship. The scarcity of such process explanations was one reason why the functional approach often seemed so uninteresting. In addition, these studies illustrated that systematic observation of natural behavior could be applied to the actual implementation of museum exhibits.

PRE-WAR RESEARCH

In 1932 Melton's former mentor John McGeoch left the chairmanship of the Psychology Department at the University of Missouri, Columbia. With the strong recommendation of Robinson, Melton assumed the position as head of the Psychology Department at the University of Missouri at the age of 29.

It was the development of the M.A. program at Missouri that ensured Melton's reputation as an administrator. Melton and the four members of the Psychology Department at the University of Missouri produced a small and thriving intellectual home for a number of outstanding psychologists, including Alvin Liberman (a member of the National Academy of Sciences), Benton Underwood (a member of the National Academy of Sciences), Robert Malmo, David C. McClelland, Richard Q. Bell, and Moncrieff Smith.

During this time, Melton also showed how a functional approach to human learning might be combined with process ideas to reveal the operations that take place in the mind of the subject but that are not directly observable. The importance of Melton's paper on the role of interpolated learning can only be understood in light of the ex treme inhibition within functionalism of proposing internal states that might lie behind regularities of behavior. Despite the efforts of Donald Hebb and Clark Hull to develop detailed process theories, the accepted approach in studies of human learning was to explore functional relationships between variables and not to postulate hidden processes. Even in 1969 this view was so strong that Lyle Bourne, a contemporary functionalist, in presenting research on the formation of concepts, said, "There might not be anything missing in an account of behavior which fails to mention underlying psychological processes." This led Allen Newell to reply "to believe that we should proceed only with descriptions of regularities and avoid any attempt to see in them the processing that is involved seems to me almost a failure of nerve." If these powerful emotions (since I attended the meeting, I know they were) about the legitimacy of inferring underlying process could be invoked in 1969, how much more controversial was their use by Melton, a scion of the functionalist movement in 1940?

Melton and Irwin had subjects learn two lists of associated word pairs, and then studied the recall of list one associations after varying amounts of list two learning. They plotted the amount of interference on list one memory as a function of the number of trials on list two. All of this was very consistent with the functionalist tradition of relating an independent variable (number of list two trials) to a dependent variable (amount of retroactive interference). Then, however, Melton took a step beyond functionalism into the internal world of process. The standard way to interpret retroactive interference was that when subjects attempted to produce associations to list one, they intruded the new learning developed during the list two trials. Melton and Irwin measured the number of such overt intrusions as a function of the number of list two learning trials.

Then they *subtracted* overt intrusions from the total amount of retroactive interference they had measured. What was left was a new hidden factor they called factor *X*. Not only was factor *X* large, but it also showed a systematic increase with the amount of interpolated learning of list two. At low levels of list one learning, most of the interference was due to overt intrusions, but as list one learning increased, the interference appeared to be caused mainly by the internal process called factor *X*. Melton tried to identify factor *X* with the unlearning of an association that had been described by Clark Hull during extinction in classical conditioning. Although the very label factor X indicates the care with which Melton took to identify the theoretical nature of this hidden factor, it is clear that he was using the careful functional approach to make visible a process that was otherwise hidden from view. The use of subtraction methods to reveal underlying process was to become a major part of new studies of short-term memory and cognition that emerged after World War II, but Melton's own research was a clear forerunner of the newer process orientation.

MILITARY PSYCHOLOGY

The combination of Melton's administrative skills as Psychology Department head at Missouri and his encyclopedic knowledge of psychological methods (as revealed by his papers on methodology) and applications (as revealed by his museum studies) must have made him a nearly irresistible attraction to the growing war effort by psychologists. Even before Pearl Harbor he had worked on a battery of psychomotor tests for pilot selection. Those tests were instrumental in the smooth functioning of pilot selection after the war began. Melton wrote a long monograph on those test batteries. The success of those tests was important to the prestige of experimental psychology in the military.

Melton's research operation was successful in performing a valuable function in pilot selection during and following the war. It also served as a training ground for a large number of psychologists who were to become prominent leaders in the field of experimental psychology after the war. Melton worked on military projects from 1941 until 1957, with the exception of a brief period at Ohio (1946-49) that was interrupted for a one-year visit to Stanford in 1948. Melton's administrative leadership helped to create and cement an important role for psychology within the military that has lasted until the present time.

HUMAN PERFORMANCE CENTER

Melton returned to academia at the University of Michigan in 1957. One year later he attracted his close colleague in military psychology, Paul Fitts, from Ohio State University. Together they built a major center for experimental psychology and its applications. Melton and Fitts regarded the field of human performance as the appropriate basic science out of which would flow applications to the design of a wide range of civilian and military technologies. Much current work in the design of technology that can be used by humans flows from the concepts that were developed and the students who were trained at the Human Performance Center. Human performance theory emphasized the development of functional relationships that might be explained in terms of transformations of internal representations. It became a part of the more general cognitive psychology that developed in the 1960s.

Melton's unique research contribution was in the area of short-term memory. This topic allowed Melton to combine his long-standing interest in verbal learning with the newer information processing approach that emphasized memory within the context of cognitive tasks. Melton's work stressed the continuity between short-term and long-term memory. His 1963 paper on short-term memory was far more consonant with current views of short-term memory as an active state of long-term memory than it was with the then popular buffer models. An example taken from that paper is the improvement in recall of a nine-digit number when its repetitions were separated by two to eight other sequences. Even a single repetition of a nine-digit list was sufficient to improve its second recall after eight intervening lists. This could occur even when subjects had no idea that lists were repeated. Melton concluded that "the structural memory trace established by a single occurrence of an event must be extraordinarily persistent." The evidence for continuity between short- and long-term memory provided by that paper was influential and remains widely cited as a source of information on the basic characteristics of short-term memory.

Another finding made by Melton (1967) supported the idea of continuity between short- and long-term memory. In studies of successive items within a single list, he found that the more widely spaced two occurrences were, the more likely an item was to be recalled on a subsequent occasion. This effect was similar to the advantage of spaced practice found in long-term memory skills. However, it was possible to demonstrate the advantage of spaced repetition very quickly and reliably within the short-term memory context. This made possible studies of the underlying process that was at work to produce improved recall with increased lag between repetitions (lag effect). One could ask, for example, whether the lag effect depended on the more varied context of presentation as the lag increased, whether there was reduced attention to the second item when it followed immediately after the first, or whether the additional practice involved in reinstating a delay item helped the subsequent recall. Once again Melton's exploration of a systematic function between two variables opened the way for studies of the underlying psychological and physiological mechanisms that are still continuing.

The Human Performance Center produced many of the major figures in contemporary experimental psychology. Edwin Martin, Robert Bjork, and Judith Goggen were among the younger associates who began their academic careers by working with Melton in the research programs of the center.

A number of very important doctoral dissertations were also developed there. Probably the most influential was Gerald Reicher's demonstration that every letter within a word is seen at a lower duration threshold than the same letter when presented in isolation or within a nonsense string. This finding was at first widely doubted, but it was then replicated innumerable times and later was a major contributor to the development of multilevel parallel processing models of cooperative computation that have been so widely influential in recent years. Robert Crowder's very convincing demonstration that nonverbal interpolated mental effort reduces short-term memory for verbal items was a basic contribution to capacity models of short-term memory. Barbara Tversky's study of picture word matching were among the earlier objective experimental demonstrations of the development of a visual image from a verbal presentation. Harvey Shulman demonstrated the role of semantic codes in short-term retention, thus providing an additional demonstration of the continuity between short-and long-term retention. Richard Pew, Irving Biederman, Edward Smith, and Howard Egeth were among other graduates of the Center's program during Melton's tenure.

Melton's contribution to the field of psychology included twelve years as editor of the *Journal of Experimental Psychol* *ogy* (1951-63). Melton was a believer in thorough experimental designs that explored multiple levels of a given variable and that used counterbalancing to avoid confounding influences. His years as editor helped to increase the concerns of a generation of young investigators with the importance of a high level of experimental control. He later served as chief editorial adviser to the American Psychological Association. Honors were also numerous, including election to the National Academy of Sciences in 1969 and receipt of the Gold Medal Award of the American Psychological Association in 1976.

These contributions gave Art Melton a level of authority with the experimental psychologists of his era that is difficult to describe. Melton's sense of dedication to psychology was so clear that one felt a greater debt because of it. It was a rare person who could refuse a request from Melton to serve on a committee, review a paper, or perform some other service that Melton felt was needed by the field.

Melton's studies of retroactive interference, spacing, and short-term memory helped to steer the field from the descriptive concerns he learned from the functionalists to an interest in the underlying mental operations that might serve as the empirical basis for a deeper theory of the human mind. Even more than his individual studies, the environment for training that he worked to create at the University of Missouri, in the Air Force, and most of all at the University of Michigan provided several generations of researchers with the methods to assist in the development of this new process-oriented view of human abilities. Among Melton's many awards was recognition for his graduate teaching from students at Michigan for his many contributions to their education. Through their work as well as his own, Melton's ideas and values continue to contribute to our understanding of psychology and to its application.

I was greatly assisted by three more personal published biographies concerned with Melton's work. One of these was an obituary written by Melton's long-time colleague Benton Underwood (*American Psychology* 34 [1979]:1171-73). Another was written jointly by Robert S. Daniel, Frank Geldard, James Greeno, and Wilert McKeachie (*American Journal of Psychology* 95 [1980]:153-58). Another biography appeared in conjunction with his reception of the Gold Medal Award of the American Psychological Association (*American Psychologist* 32[1977]:98-99). Dr. Irwin Pollack, who has spent many years at the University of Michigan, collected material that was of very substantial aid in developing the bibliography that appears following this memoir, and I am very grateful to him. Mary K. Rothbart helped me to clarify this memoir. Her editorial skill is greatly appreciated.

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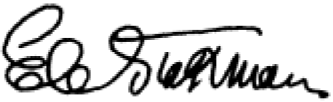

Elvin Charles Stakman

May 17, 1885-January 22, 1979

by C. M. Christensen

Elvin Charles Stakman was born May 17, 1885, on a farm near Ahnapee, Wisconsin, a small town on the west shore of Lake Michigan, the eastern terminus of the twenty-three-mile-long Green Bay and Ahnapee Railroad. Later the name of the town was changed to Algoma, which is listed officially as his birthplace. His parents were Frederick and Emelie Eberhardt Stakman, and he was the youngest of four children: Arthur, born in 1878, Lawrence, born in 1880, and Edna, born in 1883. While he was still a babe in arms the family moved to Brownton, Minnesota, a town of some 350 inhabitants about seventy-five miles west of Minneapolis, just where the Big Woods abruptly gave way to the prairie. All his life he remembered Brownton and the surrounding countryside, and the people there, with affection, and late in life wrote a letter to the editor of the Brownton paper telling of his love for the town.

His schooling through the tenth grade was in Brownton, but some courses necessary for entrance to the university were not available there, and his last two years of high school were in St. Paul, and in Glencoe, not far from Brownton. He was a natural-born scholar; learning came easy to him. Even as a youth he had wide-ranging interests, and his memory, all his life, was phenomenal. For two summers in

Brownton as a youth he studied German in a church school, and later continued the study of it on his own, so that by the time he matriculated at the university he already was fairly competent in spoken and written German. He began the study of Latin in the tenth grade, in Brownton, continued it during the last two years of high school in St. Paul and Glencoe, and during all his four years at the university. He also continued his study of German at the university, and by the time he graduated, German was a usable second language to him. German literature had a strong appeal to him, and he could and sometimes did quote long passages from Goethe's "Faust"—not to show off, especially, but because he loved the sonorous, rolling phrases, and the expansive thoughts they expressed.

He graduated from high school in the spring of 1902 and matriculated at the University of Minnesota in the fall. After he had paid his tuition of fifteen dollars for the semester and bought his books, he had sixty-five cents left. He worked at various odd jobs to help pay his way, and worked summers in Brownton and, in the harvest season, on threshing crews on nearby farms to help earn college money; he also received some needed financial help from his sister, who at age seventeen had begun teaching school. He must have lived frugally in college, as many other students of limited means have done before and since, and probably had few funds for extracurricular entertainments and amusements.

There were no formal "majors" then, but rather "group subjects," and Stakman chose botany, German, and political science as his special fields in which to concentrate. He graduated with a B.A. degree in 1906, and was elected to Phi Beta Kappa, although he seldom wore the key in later life—or any other tokens of membership in special or select groups or organizations or societies. Upon gradua tion he was offered an assistantship in botany, and also one in German, the assistantship in German being unusual in that it permitted him to continue graduate work in any field he chose. He also was encouraged to continue graduate work in political science. Obviously, his professors recognized his abilities. However, he needed to recoup his finances, and evidently felt an obligation to repay his sister for the help she had given him, and so he chose to accept a teaching position in a high school at Red Wing, Minnesota, an attractive town on the Mississippi River below St. Paul. After a year at Red Wing, he taught another year at the high school in Mankato, in south central Minnesota. At both schools, there had been problems with discipline among some of the boys, and Stakman evidently took care of those very effectively, in part by organizing athletic teams, which he also coached. In 1908 he was chosen, at age twenty-three, to serve as superintendent of the high school at Argyle, Minnesota, in the far northwestern corner of the state, in the Red River Valley. There, in addition to his administrative duties, he taught several courses, and again organized and coached athletic teams that, according to his account, excelled in competition with the other county schools.

In 1909 he accepted an assistantship in the newly (1908) organized Department of Vegetable Pathology, on the St. Paul campus of the University of Minnesota (the name of the department was soon changed to Plant Pathology and Agricultural Botany, and later to Plant Pathology). This was the beginning of a long and fruitful association that continued for almost seventy years; he continued to occupy his office and take part in seminars and discussions until shortly before his final illness in 1977.

Stakman received the M.A. degree in 1910; the head of the Graduate School gave him the choice of receiving ei ther an M.S. or an M.A. degree, and Stakman chose the M.A. According to Stakman's account, the head of the graduate school asked him, "Did you study particularly for technological purposes, or for the purpose of an education?" and Stakman answered, "Well, since I'm not completely sure that I can be a professional, I guess you'd have to say I studied particularly for educational purposes." He then was told he could have an M.A. degree if he wanted it, and replied, "Give me a M.A. degree. Okay, I'll take that degree." Graduate education was considerably simpler then.

His research for the M.A. degree dealt with germination patterns of spores of cereal smuts, among which he found numerous deviations from the standard textbook versions that had been taught for some decades.

For his Ph.D. work he undertook a study of "bridging hosts" in the black stem rust fungus, *Puccinia graminis.* The idea of "bridging hosts" stemmed from work in England in the late 1800s, and by the time that Stakman began his work on it, the "bridging host" concept was practically a tenet of faith among plant pathologists in general and of students and researchers of the stem rust fungus in particular. According to this concept, varieties of the stem rust fungus that could attack rye, say, but not wheat could, after growing for a time on barley (which they could infect to some extent as well as rye), become adapted to wheat and could then readily attack wheat. Barley thus served as a "bridging host" for the fungus to enable it to change its parasitic capabilities. If this were true, then any attempt to breed varieties of wheat resistant to the rust were hopeless, because the fungus could readily, by means of various bridging hosts, become adapted to attack wheat.

Stakman and his coworkers attacked this problem, begun for his Ph.D. thesis work but continued afterwards. By means of well-planned, meticulously carried out, and very extensive work, they (mainly Stakman) within a few years killed this widely accepted but, as it turned out, fallacious bridging host theory—killed it, pounded nails in its coffin, and buried it forever. This was the start of his approximately forty years of work with black stem rust of wheat—work which eventually reduced this disease from a major plague of wheat throughout the world to a relatively minor problem that can be and is being coped with effectively. Black stem rust still is with us, and probably always will be, but it no longer is the fearsome and widespread destroyer it once was. And the principles and practices developed during this work by Stakman and his many students turned out to be applicable to many other plant diseases, and, indeed, to just about all diseases of living things, both plants and animals, including humans, everywhere. Stakman had the vision essential to developing wide-ranging principles, coupled with the practicality and persistence to accumulate the necessary evidence to establish them as valid. His pioneer work on parasitic races of the stem rust fungus was enlarged to become a generally applicable principle that made reasonable and understandable order where before confusion had reigned.

When Stakman received his M.A. degree in June 1910, he already was or then was made assistant in plant pathology, with responsibility for undergraduate and graduate teaching, research, and extension in plant pathology. At times he taught other courses also, as diverse as one in wood structure to the undergraduates in the College of Forestry, and in household bacteriology to the women in home economics. On July 1, 1913, shortly after receiving his Ph.D. degree, he was made head of the Section of Plant Pathology, in the Department of Plant Pathology and Agricultural Botany. Just six weeks past his twenty-eighth birthday, he was well established in a position of leadership in what was to be his lifetime profession, in the department where he was to spend most of the rest of his professional career.

He was head of the Department of Plant Pathology from 1940 until his retirement in 1953.

As section head, Stakman in the 1920s and 1930s attracted outstanding graduate students from all over the world, who came to Minnesota for their advanced degrees in plant pathology, and who carried his teachings and some of his inspiration with them when they returned to their own countries. He was an unusually gifted and stimulating teacher, and the department under his leadership was pervaded by an atmosphere of excitement, accomplishment, and intellectual adventure. Stakman had a way of making his staff and students feel that they were on the cutting edge of scientific progress, that their work was important, that their contributions were significant, and that they were on an outstanding team. At any one time the department might have graduate students from many states in the United States and from Canada, England, Germany, Hungary, Australia, New Zealand, China, and India; it was a cosmopolitan department, and the students felt that they were contributing to the development of a new field, one with important basic principles and a multitude of practical applications in world agriculture. Thanks in good part to Stakman, they were caught up in an exciting adventure, converts to a newly revealed gospel expounded by its major prophet; of course it was exciting.

Stakman had an important part in getting a postgraduate curriculum established at the Agricultural College in Chapingo, Mexico, to which, from the early 1970s on, graduate students from Mexico and other Latin American countries could obtain M.S. and Ph.D. degrees in plant pathology and related fields. He taught one semester at the Univer sity of Halle (1930-31), where he lectured in German. He also lectured occasionally at several colleges in Mexico, and there he spoke in Spanish.

In 1917 Stakman married E. Louise Jensen, who since 1913 had been a mycologist in the Section of Plant Pathology that he headed. She had assisted him in teaching several courses, and she was a joint author with him of a research paper on timothy rust; she evidently was a productive member of the staff. They were a congenial couple, and she accompanied him on a number of his trips to foreign countries. They had an attractive home within easy walking distance of the plant pathology building on the campus where Stakman commonly put in twelve- to sixteen-hour working days. Louise died suddenly, without any advance warning, one evening in 1962, and Stakman deeply missed her companionship. They had no children.

In 1918 Stakman was primarily responsible for organizing, and for a time was leader of, a campaign to gain financial and legal support from the federal and state governments to eradicate barberries, the alternate host of the black stem rust fungus, from the major spring wheat growing areas of the United States from Michigan and Ohio on the east to the Dakotas, Montana, Wyoming, and Colorado in the west—an area amounting to about one-third of the United States. Barberry eradication had long been accepted in Europe as a necessary ingredient in the program of control of black stem rust, but nothing of the magnitude that Stakman planned had even been tried before, and no one knew whether it could be done, or whether, if it was done, it would contribute significantly to control of the rust. Stakman enlisted the solid support of men in the U.S. Department of Agriculture, of those in state governments, and of business tycoons in the railroad, milling, and farm equipment industries and in banking—no small accomplishment for a thirty-three-year-old professor in a new and small department in a midwestern university. Stakman not only got the program under way, he headed it for its first year. It was successful in every sense of the word—by 1954, more than 450 million barberry bushes had been eliminated in eighteen states, and long before that the often regular and destructive local epidemics of black stem rust were a thing of the past. Many men were involved in this program over the years, but its inception and its successful establishment were due primarily to Stakman; when he strongly believed that a given course of action was necessary to achieve a desired and beneficial goal, he could be an untiring and extremely persuasive promoter of it.

In 1941 Stakman, along with Dr. Paul Mangelsdorf of Harvard University and Dr. Richard Bradfield, head of Agronomy and Soils of Cornell University, was asked by the Rockefeller Foundation to make a survey of farms and farming, of agricultural research and teaching, and of agricultural extension and administration, in Mexico, to provide the foundation with the basis of information on which to judge whether it would be desirable to establish a joint project of agricultural research with the Mexican Department of Agriculture. Their report was uniformly favorable to the undertaking of such a program, and in 1943 the joint Mexican Department of Agriculture-Rockefeller Foundation program officially got under way. It turned out to be phenomenally successful, other countries became interested, and similar programs were initiated in Colombia and Chile in South America, in the Philippines (in collaboration with the Ford Foundation), and in India. Subsequently, these efforts led to the establishment of a worldwide network of research centers in developing countries around the world, all of them devoted to agricultural development and economic improvement of the countries they serve, and to the training of competent research and administrative staffs capable of pursuing the research necessary for agricultural and economic improvement. As of 1981, thirteen such international centers of research were in operation, under the general aegis of the Consultative Group on International Agricultural Research (CGIAR), with support from many different governments, agricultural agencies, and foundations. In most of these Stakman had little or no part, but all of them were an outgrowth of the Mexican Government-Rockefeller Foundation cooperative program in agricultural research that he helped get under way in the early 1940s, and for which he served as a consultant for many years after his retirement from the University of Minnesota in 1953.

As Stakman matured, he became more and more convinced that science as exemplified by agricultural research in its best manifestations could be a powerful force for the improvement of the lot of mankind. Through research came control of diseases and improved agricultural practices, with an increased abundance of food and fiber to furnish the necessities of life, to provide health and physical and mental well being for the peoples of the ever more crowded earth, and to provide education—if the peoples of the earth had the knowledge and the wisdom to take advantage of it. Coupled with this ability to produce the materials and goods necessary for a better life for humankind was the necessity to restrict the population to numbers that the various countries themselves could support. Stakman expounded this doctrine of improvement of production by means of research, and of the need for population control, from the mid-1950s on. The Cosmos Club, in its award to him in 1964, called him a "statesman of science," a happy and accurate designation. He spoke of these topics often with conviction and with evangelistic fervor.

Some of these talks were published, and were classics. Brief excerpts from two of them will serve to illustrate his themes.

From his acceptance speech on receiving the citation from the Cosmos Club, as "Statesman of Science," in 1964:

> Science has contributed much to the material welfare and to the intellectual enlightenment of society. Can it also contribute to spiritual refinement and the improvement of human relations? Can it help promote the general use of the scientific code of scrupulous intellectual integrity within each society and between all of them? Can it help to eradicate intolerance and bigotry from societies of the world as it has helped eradicate malaria, cholera, and yellow fever from many areas of the world? It can do its part, but its part is only part of what is needed. Society and all its civilizing agencies must do their part also, and may good sense, and even better sense, prevail in all their efforts!

From, "Education: Needs and Virtues; Crimes and Misdemeanors" in *New Concepts in Agricultural Education in India,* 1969:

> Eventually every country must adjust its population to its means of subsistence, it must produce enough food for its people or develop the purchasing power to buy it—provided enough food is available for purchase. But will enough always be available? Some thoughtful people say that it will not, unless the rate of population increase can be reduced rapidly and dramatically or food production can be increased quickly and substantially. If present food-population trends continue, the world food situation will be critical and may be catastrophic within two decades (he was very close to the mark, here), according to many competent students of the problem. Among neo-Malthusians there appears to be a growing conviction that Malthus probably was more right than wrong.
>
> Man has become the potential master of his own fate in his struggle for subsistence. Thanks to science and technology, he has the means to restrict his numbers simply and humanely and to increase his food supplies quickly and substantially. But to become the actual master of his fate, he must have the will and the wisdom to utilize fully his present means and the wit to devise better ones for the future.

That his message has not been heeded by some of the Third World countries who most needed to hear and to heed it does not detract from the soundness of his vision and of his prophecies.

One of his last public appearances was on January 30, 1973, when he was feted by the Plant Pathology Department of the Colegio de Postgraduados at Chapingo, Mexico, which he had long labored to help get established and which now dedicated to him a special reading room, named in his honor.

On July 19, 1977, at his home in St. Paul, he suffered a paralytic stroke and congestive cardiac arrest. Although a pacemaker was installed and helped him resume some activities, his power of speech was never recovered, and he gradually declined. He was well enough, on his ninety-third birthday, to attend in a wheelchair a dinner given in his honor by the graduate students of the Plant Pathology Department of the University of Minnesota, and which he seemed to thoroughly enjoy. He died January 22, 1979, about four months short of his ninety-fourth birthday. As specified in his will, his remains were cremated and the ashes interred at a nearby cemetery, near the graves of his mother and of Mrs. Stakman. The bulk of his estate, amounting to approximately $500,000, was left to the Department of Plant Pathology of the University of Minnesota, the income to be used to further various worthy causes for which public funds are not available. Even in death, Stakman contributed to the advancement of the profession, the department, and the university that he had served so long and so well.

PUBLICATIONS

Stakman published more than 300 papers, as well as several books. Several of his papers, dealing with science and society and with education, are classics.

MEMBERSHIP IN SCIENTIFIC AND LEARNED SOCIETIES, AND HONORS AND AWARDS

Dr. Stakman held regular or honorary memberships in thirteen scientific and learned societies in the United States and in Canada, Great Britain, Germany, India, and Japan. He was president of the American Phytopathological Society in 1922 and of the American Association for the Advancement of Science in 1949. In 1948, he was a member of the Scientific Mission to Japan under the auspices of the Supreme Command for Allied Powers. He was a member of the National Commission of UNESCO, 1950-56; the Executive Committee, National Science Board, 1951-54; and the Advisory Committee on Biology and Medicine, United States Atomic Energy Commission, 1948-54 (chairman, 195354, and consultant, 1954-59). He held various offices in the National Research Council in 1931-34, 1937-38, 194748, and 1950-58.

He received honorary degrees from the University of Halle-Wittenberg, Halle-an-der-Saale, Germany, 1938; Yale University, 1950; the University of Rhode Island, 1953; the University of Minnesota, 1954; the University of Wisconsin, 1954; and Cambridge University, England, 1964.

Among the special honors given him were the Emil Christian Hansen Gold Medal and Prize, 1928 (for his contributions to the knowledge of physiologic specialization in fungi); the Medalla de Merite Agronómico, Colombia, South America, 1955; the Centennial Award, Michigan State College, 1955; the Certificate of Merit, Botanical Society of America, 1956; the Otto Appel Medal, 1957; the first Cosmos Club Award, 1964; and La Cruz de Boyaca, Colombia, South America, 1966.

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Photo by Carl Bigelow, *Oakland Tribune*, Oakland, California

Otto Struve

Otto Struve

August 12, 1897-April 6, 1963

by Kevin Krisciunas

"Work was the motto of the whole of life. . . . In a letter [we find] the following passage: . . . The Struves cannot live happily without unceasing work, since from the earliest youth we have been persuaded that it is the most useful and best seasoning of human life."[1] Easily counted as one of the prominent astronomers of his century, Struve left a standard that many sought to emulate but few achieved.

The Struve I have just described is *not* the Otto Struve of this memoir, but his great-grandfather Wilhelm Struve (1793-1864). Yet the words apply equally well. The first of *seven* Struves in *five* generations to obtain a Ph.D. (or its equivalent) in astronomy,[2] in 1839 Wilhelm Struve founded Pulkovo Observatory near St. Petersburg, which has played a major role in positional astronomy ever since.[3] Wilhelm was one of the first three astronomers to measure the trigonometric parallax of a star—the final proof of Copernicanism. He published 272 works[4] and had eighteen children.[5]

Of the six Struves who pursued a career in astronomy,[6] four won the prestigious Gold Medal of the Royal Astronomical Society: great-grandfather Wilhelm in 1826, grandfather Otto Wilhelm in 1850, uncle Hermann in 1903, and our Otto in 1944. Such a level of recognition in astronomy is unique, and the Struves must be counted with the Cassinis and Herschels as one of the most significant astronomical family dynasties.

One might also note that Otto Struve's great-uncle Carl was the Russian ambassador to Japan from 1874 to 1882, then ambassador to the United States in the 1880s.[7] His uncle Alfred was a noted geologist, and his uncle Wilhelm was president of the Imperial Geographical Society of Russia.[8]

EARLY YEARS

Otto Struve was born on August 12, 1897, on his family's estate in Kharkov, in the Ukraine, where his father Ludwig was director of the Kharkov University Observatory. In the Bancroft library at the University of California, Berkeley, there are a few pictures relating to Struve's early years. One shows a corner of the house in which he was born, looking somewhat like Leo Tolstoy's house at Yasnaya Polyana. Another picture shows Otto's five-year-old brother in a sleigh hitched to a dog, with ten-year-old Otto and his seven-year-old sister standing behind. Yet another picture shows the three children, a nursemaid, the janitor, the son of the groundskeeper, and a cow.

Otto grew up as the eldest child in an upper-class Russian family whose relatives had ably served the Tsar and Russian science. Great-grandfather Wilhelm had fled Altona (near Hamburg) to escape being drafted into Napoleon's army and settled in Dorpat (now Tartu) in Estonia. The Struve heritage was thus as a family of Baltic German origin that lived and worked in the Russian Empire.[9] Otto was the first member of his family to attend Russian-speaking, not German-speaking, schools. His "first language" was equally German and Russian.[10]

At the time of the beginning of World War I, Otto graduated from the gymnasium in Kharkov and entered the univer sity there to study mathematics and astronomy. In 1916 he interrupted his studies and enlisted in the Russian Imperial Army. He became an artillery officer and served on the Turkish front.[11] After the Treaty of Brest-Litovsk (March 1918), which ended Russia's involvement in the war, Struve returned to the University of Kharkov, where he obtained his degree in 1919.

Russia was in turmoil. The Tsar had abdicated in March 1917, the Provisional Government was overthrown by the Bolsheviks in October-November of that year, and the country was embroiled in a civil war. Struve enlisted in the White Russian Army of General Anton Denikin in June 1919, a move he later called "the most self-sacrificing act of my life."[12] He adds: "I have no doubt that the time will come when the Russian people will recognize that patriotism was not the exclusive privilege of those who fought on the winning side." Struve was wounded in action in July 1919.[13] It was presumably during his army years that he contracted diphtheria, scarlet fever, typhoid fever, and rheumatic fever.

Very little information concerning Struve's activity in the Russian Civil War has come down to us. Apparently, once his horse was shot under him, and on another occasion he got a bullet through his sleeve.[14] For a while he was part of a cavalry unit that "would jump up and gallop off at a moment's notice whenever there was rumour that Trotsky was in the neighborhood. But they did not capture the leader of the opposition."[15]

This is not the place to discuss the Russian Civil War, but Struve was very much part of the dynamic events of the two stages of evacuation ahead of the Bolsheviks.[16] ,[17] In March 1920 thousands of soldiers, women, and children were trying to get out of Novorossisk on the Black Sea. Roads to the port were blocked for miles. The route was strewn with dead bodies, stripped naked and frozen. There were dead horses, mules, camels, abandoned guns, and vehicles. At the water's edge Cossacks shot their horses. Starving refugees tore the corpses apart for food. Struve was with a detachment of about 300 men. The families of several officers were with them. When it was learned that no women or children were to be allowed to board the ship, a ring of people was made, and the women donned men's uniforms. A commander of a White Army destroyer agreed to take 150 men. Otto Struve was to count off that half of the detachment and remain behind, but just then there was shell fire. The gangplank was unguarded and all 300 scrambled aboard. Seeing that the ship was listing, a British cruiser took on the 300 people and carried them to the Crimea.

General Denikin, leader of the Whites, was replaced by the inspirational General P. N. Wrangel.[18] But the Whites were on their last legs. Wrangel had commandeered enough ships and stockpiled sufficient fuel for the eventual evacuation from Russia altogether. In mid-November of 1920 a fleet of over 100 ships, carrying nearly 150,000 men, women, and children, left the Crimea for Turkey.[19] The Russian officers, Struve included, were sent to Gallipoli, where they were cared for by relief agencies. Struve eventually obtained permission to go to Constantinople to look for work.

During Struve's year and a half in Turkey, he ate at soup kitchens and worked at whatever jobs he could find. For some time he and other fellow Russian ex-officers worked as lumberjacks, sleeping six to a tent. One night during a severe thunderstorm, a tent nearby was hit by lightning and all of its occupants were killed.

Enter fate. Edwin Frost, director of Yerkes Observatory in Williams Bay, Wisconsin, had written to Struve. We read in Frost's autobiography that it was by mere chance that his letter reached Struve. Struve was sitting on a park bench when another Russian ex-officer walked by. The other officer had opened the letter, hoping that it contained money. Instead, it was an offer of a job in America.[20]

Struve himself told this story many times, but under careful scrutiny the long arm of coincidence is much shorter. On December 25, 1920, the director of the Berlin-Babelsberg Observatory, Paul Guthnick, had written to Frost, relating the sad case of the Struve clan.[21] Hermann Struve, Guthnick's predecessor, had died on August 12. Ludwig Struve, having fled the war-torn Ukraine, had died in Simferopol on November 4. The younger of Otto's sisters had drowned, and Otto's brother had died of tuberculosis. Otto's mother had a position at the University of Simferopol. Another sister was with the mother, but it was impossible for Otto to communicate with them.[22] As Frost had been willing to try to obtain a position for Ludwig or Otto previously,[23] could Frost try again for the young Otto? Thus asked Guthnick.

On January 27, 1921, Frost wrote back to Guthnick to say he would do his best. There began months of sometimes daily activity on Frost's part to get Otto Struve to the United States. The famous job offer letter was sent by Frost on March 2, 1921,[24] and received by Otto Struve on April 27.[25] But Otto had already known that his situation was likely to change, as he had been in contact with Guthnick and his Aunt Eva in Berlin. On March 11, 1921, Struve wrote to Frost (in German) that he had heard of the job offer from Guthnick.[26] On April 12, Struve wrote a curious letter to Frost in English words but with entirely German syntax, in which he thanked Frost again in advance for the job offer.[27]

Struve took the letter to the YMCA to make sure it said what he thought it did. There he met a man named Areson, who came from southern Wisconsin, knew Yerkes Observatory, and also knew Frost.

It is interesting to note what Frost said to others to strengthen the case for Otto Struve to enter the United States. To the president of the University of Chicago he wrote, "[Otto] is the son of the late professor of astronomy at Kharkov, and grandson and great-grandson of the two greatest astronomers of Russia, Otto and Wilhelm Struve. . . . I am perfectly willing to take him on his lineage."[28]

We regard Otto Struve as a first-class spectroscopist and astrophysicist. But in the letter of March 11, 1921, he says this to Frost:

> I feel it is my duty to confess openly that I am only marginally familiar with the area of astronomical spectral analysis and that I practically have never worked in that area. . . . Should this prove to be no hindrance to my appointment at Chicago [for the position of assistant for stellar spectroscopy], I would gladly answer your call.

Thus, Struve's training in astrophysics was almost entirely at the University of Chicago.

It took all spring and summer to arrange for Struve's visa and passage to the United States. He arrived in New York on October 7, 1921.[29] Three days later he arrived in Williams Bay,[30] wearing not the tattered Russian officer's uniform he had had on the ship, but an outfit he had bought at a flea market in New York, consisting of orange shoes, purple trousers, and a green jacket.[15,31]

YERKES YEARS

Struve was to spend the next twenty-nine years associated with Yerkes Observatory. He obtained his Ph.D. in December 1923 from the University of Chicago, with a dissertation on short-period spectroscopic binaries, and subsequently became instructor (1924), assistant professor (1927), and associate professor (1930), and on July 1, 1932, he succeeded Frost as director. Struve became an American citizen in 1927.[13]

Otto's mother came by ship to the United States in early 1925. As Frost tells it:

> When I realized that her steamer would be crossing the line of totality of the eclipse of January 24, 1925, I prepared quite an elaborate document appointing her commissioner on the high seas of the Yerkes Observatory of the University of Chicago. I forwarded this to the chief officials of the steamship company in Hamburg and received from the commodore of the fleet the assurance that Madame Struve would have every opportunity to observe the eclipse. At the critical moment, with due ceremony, the captain escorted her to the bridge in spite of the fact that there was a snowstorm at the time.[32]

There is no account of the elder of Otto's sisters, who had been with Madame Struve in Simferopol. Presumably, she died between 1921 and 1925.[33]

Otto Struve married Mary Martha Lanning in May 1925. According to Chandrasekhar, it was her second marriage.[34] For three years prior to this, Mary had worked as a secretary at Yerkes. (Thus, she knew about observatory business.) "When [Mary] and Otto were first married they had such romantic feelings about their lives at Yerkes.... They skated on Lake Geneva in the wintertime while Otto sang Russian songs, very romantic songs to her."[35] At one point Otto and Mary almost adopted a four-year-old boy, but apparently Mary then decided against it. In the 1950s Struve lamented that the biggest disappointment in his life was not to have had a child (one would presume a son) to whom to pass on the Struve legacy.[36]

It is difficult to guess how the dynamics of Otto's marriage affected his scientific endeavors. Apparently, Mary enjoyed the status of being the wife of a famous observa tory director, and she would go to McDonald Observatory with Otto for months at a time, where she would help him with his data reduction tasks by copying down numbers that Otto called out while his eye was glued to a microscope eyepiece as part of reducing spectra.[37]

Otto, Mary, and Madame Struve (Otto's mother) formed a triumvirate. They lived together essentially the entire thirty-eight years of Otto and Mary's marriage. Both Otto's wife and mother survived him, but only by a few years.[38]

Otto Struve's first published paper, in Russian, concerned "Aid to Russian Scientists."[39] In the aftermath of the Russian Civil War, many people in Russia were starving. Frost, Struve, and George van Biesbroeck served as the Committee for Relief to Russian Astronomers.[40] Packages of food and clothing were sent to Russian astronomers in many cities. The funds came from astronomers and their families all across the United States.

Struve's scientific publications begin in 1923. The previous autumn the American Astronomical Society had met at Yerkes Observatory. Struve presented a paper on the spectroscopic binary 13y Ursae Minoris, and the two-page abstract was published in *Popular Astronomy.* Struve's publication list grew at an average rate of twenty-two items per year, reaching a total of at least 907 items.[41] This places him near the top of astronomical productivity,[42] as measured by the total number of published items. (Thus, it is difficult to do justice to Struve's work in an article of this length.) About half of Struve's papers were what we call technical contributions. He published thirty-nine articles (and ten other items) in *Popular Astronomy* (1923-51), 154 in *Sky and Telescope* (1942-63), and eighty-three reviews of books and other astronomers' work. His popular articles kept many astronomers, both amateur and professional, abreast of the rapidly developing field of twentieth century astronomy and astrophysics.

Struve's most important work was published in several books and in the *Astrophysical Journal,* the premier research journal of astrophysics, which Struve edited from 1932 to 1947. According to my count, there were 223 such papers.

As a junior astronomer, Struve collaborated on projects with the more senior Yerkes astronomers. There were seven papers with G. van Biesbroeck containing positional measurements of asteroids (1923-28). With Frost and Storrs Barrett, he published "Radial Velocities of 368 Helium Stars" in 1926, and with the same coauthors, "Radial Velocities of 500 Stars of Spectral Class A" in 1929.[43] Such velocity data on stars allowed the determination of the sun's motion in the local portion of the galaxy. Also, the data bank of spectra with sufficient resolution to allow radial velocity measurements was also a data bank for Struve's subsequent work on the "peculiarities" of stellar spectra.

One of Struve's early and ongoing interests was interstellar calcium, whose presence was known from the absorption lines in the spectra of many stars.

It is now well known that most stellar atmospheres have about the same composition, but that the presence or absence of certain spectral lines is due to the pressure and temperature in these atmospheres. One of Struve's most important contributions to stellar spectroscopy involved the observed widths of spectral lines. These are affected by the abundances of the elements, broadening due to the rotation of the stars, and the effect of electric fields on the atoms (the Stark effect). The resulting widths (or dispersions) add quadratically:

$$\sigma^2_{observed} = \sigma^2_{abundance} + \sigma^2_{rotation} + \sigma^2_{Stark}$$

Struve noted in a 1929 paper that it was R. d'E. Atkinson who, in 1922, first considered Stark broadening in stellar spectra. Theoretical analyses by E. O. Hurlbert, H. N. Russell, and J. Q. Stewart, and then M. Vasnecov, led to predictions of a measurable effect, a broadening of 1 Å for the sun, but 42 Å for a Lyrae, a much hotter star. Cecilia Payne had noted the Stark effect in actual spectra in 1925,[44] but it was Struve and C. T. Elvey (one of his two principal collaborators in the 1930s) who published the first proof. They investigated the widths of spectral lines in hot B and A stars and found that neutral atoms of low atomic weight show wider lines than the heavier atoms. They found that stars with narrow lines were more luminous than stars of the same spectral subdivision with broad lines, in agreement with theory. Theory stipulated, however, that hydrogen lines should increase in width due to Stark broadening from Hß to He, and the observed increase was less than expected.

With the Russian astronomer G. A. Shajn, Struve showed, in 1929, that B- and A-type stars rotate much faster than the cooler G, K, and M stars. The hot stars can have equatorial rotation speeds in excess of 200 kilometers per second. By comparison, the sun's value is 2 kilometers per second. This led Struve to conjecture that rapidly rotating stars could fission into rapidly revolving close binaries, or perhaps rapidly revolving close binaries might fuse into rapidly rotating single stars.

Because stellar spectral lines contain information on abundance, rotation, and the Stark effect, one must try to separate their relative contributions. Struve states that the rotational dispersion can be separated statistically in short-period binaries. The dispersion due to abundance is best investigated in cooler stars (for which the Stark effect is small). Pinning down the Stark component involves a strong combination of theory and high dispersion observations of stars, something Struve specialized in.

A fourth contributing factor to the stellar spectral lines is turbulence in the stellar atmospheres. In 1934 Struve and Elvey showed that stars such as 17 Leporis had lines with broad wings, attributable to turbulent motion on the order of 67 kilometers per second. This paper was an important contribution to the curve of growth method of spectral analysis (then only five years old), whereby the normalized line strengths (i.e., equivalent widths) are correlated with the abundances of the atoms present.

One of Struve's most important accomplishments was the organization of the construction of McDonald Observatory in west Texas. This has been described in some detail by Evans and Mulholland in their book *Big and Bright: A History of the McDonald Observatory.* [45] The funds for the observatory came from the estate of a wealthy Texas banker, W. J. McDonald (1844-1926), who left the bulk of his money ($800,000) to the University of Texas for the construction of an astronomical observatory. The will was contested by the banker's relatives, partially on the grounds that the desire to endow astronomy was a clear demonstration of insanity. The problem for the University of Texas was that it had the money but no astronomers. One of the chief testimonials to the usefulness of endowing an observatory came from E. B. Frost at Yerkes, and a thirty-year partnership between the University of Chicago and the University of Texas commenced in 1932, the year Struve became director. As Struve moved up the Yerkes ladder, he thus knew that at the top was the directorship of not one, but two, observatories.

Plans for the observatory called for a reflecting telescope of aperture 80 inches. (It turned out to be an 82-inch, and at the time of its dedication in 1939 was second in size only to the 100-inch at Mount Wilson.) It is situated at the 6,800-foot summit of Mount Locke in Jeff Davis County, a site likely to remain free from light pollution forever, given the desolation of west Texas and the observatory's remoteness from the nearest large city (160 miles).

Before the eighty-two inch telescope at McDonald was ready, important results had already been obtained at this new observing station for the Yerkes astronomers. Using a 150-foot nebular spectrograph set up on a hillside, Struve and his colleagues demonstrated that there are large areas of diffuse emission in the galaxy, showing lines of hydrogen and forbidden lines of ionized oxygen and nitrogen, which are characteristic of interstellar gas excited by ultraviolet light from hot stars. Today we call the ionized regions around hot stars Strömgren spheres, but Strömgren himself admits that it was Struve's observations in the late 1930s that stimulated his own theoretical work on questions relating to the interstellar medium.[46]

From 1932 onward Struve coauthored more papers with Pol Swings (1906-83) than with anyone else. Their prime occupation was spectroscopic studies of peculiar stars.

What constitutes a peculiar star? W. W. Morgan recalls: "[Struve] made the remark once that he never looked at the spectrum of a star, any star, where he didn't find something important to work on."[47] E. A. Milne adds: "[I]t is usually easy to recognize the characteristic Struve touch, the touch of one to whom the individual stars, with their individual peculiarities, were personal friends."[48]

To Struve, stars that had variable spectral lines were the most interesting, and the more complex those variations were, the better. Some of his favorites included unstable close binaries whose spectra were complicated by streams of gas, such as 27 Canis Majoris and ß Lyrae. Struve contributed greatly to the study of the unusual binary ε Aurigae, an F-type supergiant that undergoes a two-year eclipse every 27.1 years. Struve participated in two observing campaigns on this object, in 1928-30 and 1955-57. Curiously, the absorption lines of the system are almost identical during the eclipse and out of the eclipse. In 1937, G. Kuiper,

B. Strömgren, and Struve concluded that the eclipsing body is a very tenuous mass of gas which is partly transparent to optical light.[49] Their model was criticized by others. Until recently, it could only be agreed upon that the eclipsing body was either a semitransparent shell or a flat, opaque disk that covers half of the surface of the primary. Recent data obtained with the International Ultraviolet Explorer allow us to update the Kuiper-Strömgren-Struve model: the eclipsing body is a star of temperature 10,000 Kelvin, about four times the size of the sun. Infrared data obtained during the 1982-84 eclipse show that the companion star is surrounded by a ring of dust of radius 10 Astronomical Units, with a temperature of 500 Kelvin.[50] There are still various uncertainties in modeling ε Aurigae, and it is to the credit of Kuiper, Strömgren, and Struve that they got the basics of the model correct from optical wavelength data only.

As the director of the Yerkes and McDonald Observatories, Struve was both dedicated and demanding. He was invariably the first one to arrive in the morning, and he spent many evenings at the office.[51] After an evening staff meeting he might go off to the forty-inch and observe all night.[52] That he could be considered a twenty-four-hour-a-day astronomer was almost true.[53] "He had only one interest and one concern, namely, that astronomy should be developed and pushed to the maximum that was possible."[54] "On one theme alone he was completely inflexible, that a scientist should think first of science and only third or fourth of himself."[55]

Because of Struve's duties and observational interests there was insufficient time for him to be very involved with teaching, at least while he was at Yerkes. Chandrasekhar recalls that Struve's frequent trips to Texas caused him to miss up to two-thirds of his lectures. No one was assigned to teach them in Struve's stead; students were given some plate to measure and analyze in his absence.[56] Still, when it came to Ph.D. qualifying exams, Struve had a great interest in a high level of achievement. W. H. McCrea recalls that he was astonished to see how exacting Struve's standards were. He would not "lower the hurdle" but would give the student "the fullest possible opportunity to surmount it."[57] We can characterize Struve's attitude toward his colleagues and students thus: "It was unforgivable in his eyes for anyone to fall short of full commitment."[52]

In spite of Struve's successes in building McDonald and pushing astrophysics ahead, a case could be made that his greatest accomplishment was making the Yerkes Observatory staff the brain trust it was in the 1930s and 1940s. Elvey we have already mentioned. W. W. Morgan finished his Ph.D. under Struve in 1931[58] and recalls, "His [Struve's] influence was far and away the most important in my formative years as an astronomer. He had an intense enthusiasm that he translated to me, and was the sound basis on which I was able to build."[59] In the summer of 1936 Gerard Kuiper was hired, and also Bengt Strömgren (later Yerkes director, 1950-57). In 1937 S. Chandrasekhar, the brilliant mathematical astrophysicist, was hired. Other notables who were staff members include Jesse Greenstein, W. A. Hiltner, and Gerhard Herzberg. There were many visitors, such as Swings, Jan Oort, M. G. J. Minnaert, H. C. van de Hulst, and Albrecht Unsöld.

Struve worried about appearances, rank, and position. During the Depression he was concerned about hiring foreign astronomers when many Americans were out of work. When the wife of his deputy George van Biesbroeck wrote a letter to Belgium which was excerpted in a popular astronomy magazine and noted how Yerkes was being run by "the two Europeans," Struve was particularly angry.[60] That is how Morgan, who was born in America, replaced van Biesbroeck as deputy.

Not all these luminaries stayed at Yerkes forever. Strömgren returned to Denmark after only a year and a half, though later (1947-48) he was a visiting professor.[61] World War II threatened to take all the staff away for war work. (Elvey went and never came back.) The very existence of the observatory was threatened. Subscriptions to the *Astrophysical Journal* declined, and the number of issues sent abroad was reduced, because of censorship, to a trickle. Struve did his best to keep things together and founded the Yerkes Optical Bureau, which did wartime optical designs.[62]

One of Struve's most progressive ideas from the war years was the proposal to organize a scientific consortium, which would run McDonald Observatory and even build a new telescope, a Schmidt camera with a seventy-two-inch spherical mirror and a fifty-inch corrector. If the consortium were to be organized, its member universities could provide their respective astronomers access to first-class telescopes at a first-class site.[63] While nothing came of the idea at the time, the 1950s saw the establishment of the Association of Universities for Research in Astronomy (AURA), which runs Kitt Peak National Observatory. Otto Struve served on the original organizing committee for building KPNO.[64]

After the war, science worldwide underwent a major phase of expansion that has continued until today. The University of Chicago hired many new famous faculty and made them Distinguished Service Professors: Marshall Stone (mathematics), Enrico Fermi (physics), Harold Urey (chemistry), and Gustav Rossby (meteorology). But President Robert M. Hutchins had overlooked upgrading Struve's rank. Chandrasekhar sensed Struve's disappointment and went to Hutchins to suggest making Struve a Distinguished Service Professor. According to Chandrasekhar's later memory, Hutchins said, "I can kick myself for not thinking of it. I'm ashamed that I never thought of it. Thank you, Chandra, for telling me." Struve became a Distinguished Service Professor within a month.[65]

The years 1947-50 were a stress-filled time for Struve and Yerkes, and this was due in part to a miscalculation by Struve. Struve's ideal was George Ellery Hale (1868-1938), the founder of Yerkes Observatory, then of Mount Wilson Observatory.[66] Hale can be seen looking over Struve's shoulder in the photograph accompanying this article. Chandrasekhar recalls, "[Struve] imitated his life, I think even consciously. . . . [H]e wanted very much to be treated the way Hale was treated in the latter part of his life. It was his great disappointment to him that he felt he was not so treated."[67]

In 1947 Struve became chairman of the astronomy department, and Kuiper became observatory director. Apparently Struve expected Kuiper to seek his approval on all major observatory matters. Kuiper, who had a mind of his own, could not function in this manner, and developed an ulcer.[68] Struve's junior colleagues were renowned in their own right and Struve "perhaps felt a little as a masterful conductor might whose choir all wanted to be soloists."[69]

In 1949 Struve wrote to his wife that he felt he was being squeezed out of Yerkes by Chandrasekhar and Kuiper.[70] Archival material shows that there were a lot of bitter feelings about the situation at Yerkes at that time, but in reality astronomy *always* came first with Struve, and Struve's estimate of a person's contributions to astronomy was the most important part of Struve's relationship with that person. For example, Struve was responsible for Chandrasekhar being nominated as the Russell lecturer of the American Astronomical Society for 1949. Struve nominated Chandrasekhar as his successor as president of the AAS (a post Struve held from 1946-49). Also, on the first occasion that Struve participated in the selection of a Bruce Medalist, the award was made to Chandrasekhar in 1952. Chandrasekhar told me in 1987, "I am concerned with the prevalent misunderstanding that Struve and I did not continue with the warmest personal relations during the later years at Yerkes or after his departure to Berkeley and elsewhere. "[71]

Kuiper recalls this period as follows:

> [Struve] was often overworked, suffered from insomnia which caused him sometimes to be in somewhat of a daze following two or three hours of sleep.... In time he decided on a complete change, accepting a professorship at the University of California in 1950. To his former Yerkes associates and friends, Struve appeared a different man after he had moved to Berkeley. . . . He seemed casual and even somewhat light-hearted about his new duties and involvements, although he took his scientific observing programs, now carried out at the Mt. Wilson Observatory as a guest investigator, as seriously as ever.[10]

POST-YERKES YEARS

At Berkeley, Struve was chairman of the astronomy department and director of the Leuschner Observatory. There he devoted much more time to graduate students than was ever possible at Yerkes. He worked closely with postdoctoral fellow Su-Shu Huang, Argentinian astronomer-in-exile Jorge Sahade, and assistant Velta Zebergs.

During the 1950s Struve devoted a great deal of energy to the study of ß Canis Majoris (or ß Cephei) stars, a field "which he was thought to have discovered, has since been brought to fruition, and progress in it has been gratifying."[72] There are only about two dozen of these stars known, perhaps due to the small fraction of time massive stars are found in one portion of the Hertzsprung-Russell Diagram. These stars are all of spectral type B0 to B3 and are giants, subgiants, or bright giants (luminosity class II to IV). They exhibit light and spectral variations on time scales of hours and show multiple periods in their light curves. Their behavior is attributed to pulsations regulated by a combination of the mass and rotational speed.

Having served as a vice president of the International Astronomical Union from 1946 to 1952, Struve was honored with election to its presidency in 1952 at the Rome assembly. The Korean War had caused the cancellation of the IAU meeting scheduled for Leningrad in 1951. Viktor Ambartsumian, spokesman for the Soviet delegation, had expressed official displeasure regarding the cancellation of the Leningrad meeting. It fell to Struve to reassure the Russians that a meeting would take place in the Soviet Union soon. Struve's comments were made to the assembly in Russian and electrified the audience.

In 1955 the IAU met in Dublin. Struve's opening address, reprinted in the October 1955 issue of *Sky and Telescope,* was full of anecdotes and was not of the serious tone one usually associates with him. Struve reminded his listeners that the purpose of the IAU was to pursue international cooperation whenever necessary or useful, as opposed to whenever possible. He related his own perspective that astronomy research requires the participation of all civilized nations, that the most important tools of astronomy were the astronomers. (Thus, training is paramount.) He quoted de Sitter that "astronomy is the most beneficial of the sciences for serving the ideal aims of mankind." He said that the IAU should advise various national committees regarding their scientific priorities, but added, "As a believer in evolution rather than revolution, I do not advocate sudden changes."

In 1958 the IAU meeting was held in Moscow, but Struve did not attend. A couple of years previously he had received a copy of a book that dealt with astronomy at the University of Kharkov (Struve's alma mater) from 1805 to

1955. Struve noted that the book was devoid of a great amount of political propaganda. However, he found out that the Library of Congress had a previous version of the book that contained derogatory remarks about him. The copy that had been sent to Struve had had four pages removed, with others pasted in as replacements. This involved changes to the first appendix, the index, the table of contents, and even the errata slip.[73]

The undoctored version discusses Otto's father, Ludwig, then says:

> His son Otto Struve, having been instructed in his own time at Kharkov University, [and] having betrayed his native land [*izmenniv Rodine*] went abroad and settled in the USA. Over the course of a long time he was in the service of the American imperialists in the capacity of director of the Yerkes Observatory, near Chicago.[74]

In the book Struve was also taken to task for his article "Freedom of Thought in Astronomy," published in 1935.

Struve was not amused that his activities were called "traitorous," and did not want to be subjected to any further attacks in Moscow.

In 1956 Struve suffered a fall while observing. Broken bones kept him from being at the University of Pennsylvania to receive an honorary degree in June of that year. About 1958 he contracted hepatitis, and its aftermath was cirrhosis of the liver, the eventual cause of his death.[75]

While Struve's published papers still dealt with optical stellar spectroscopy, he also became convinced of the importance of developing radio astronomy, which he pushed at Berkeley. In 1959 he took a new job as the first director of the National Radio Astronomy Observatory in Green Bank, West Virginia, in part because he was on the search committee for a director, and the committee could not find anyone to accept the position.[76] Struve lent prestige to NRAO at that time, and he served as a mentor because of the astronomical research tradition he represented. Some feel he was not the right person for the job, and it was a disappointment to all that the construction of the 140-foot telescope, well under way at the time of Struve's arrival, was slowed beyond expectations. Still, Struve's prestige lent credence to the proposal for the 300-foot transit telescope, which recently finished twenty-five years of service,[77] and Struve was supportive of Frank Drake's Project Ozma, the first search for extraterrestrial life.

After two and one-quarter years as the director of NRAO, Struve decided to resign. He wrote to I. I. Rabi, the president of Associated Universities, Inc., which managed NRAO, that he had insufficient time for his research, and that all the meetings "leave me in a state of continuous fatigue which is the cause of other health problems."[78]

Struve's final positions were as a visiting professor of astronomy at Caltech and, simultaneously, a visiting staff member at the Institute for Advanced Study at Princeton. He died on April 6, 1963, in Berkeley, California, and his remains were cremated.

We list below Struve's academic awards and honorary degrees.

1944	Gold Medal, Royal Astronomical Society
1948	Bruce Gold Medal, Astronomical Society of the Pacific
1950	Draper Medal, National Academy of Sciences
1950	Decoration: Chevalier, Order of the Crown, Belgium
1954	Rittenhouse Medal
1954	Janssen Medaille, Société Astronomique de France
1955	Medaille Jules César Janssen, French Academy of Sciences
1955	G. Bruce Blair Award, Astronomical Society of the Pacific

Honorary doctorates:	
1939	Sc.D., Case School of Applied Sciences
1946	Sc.D., University of Copenhagen
1948	Sc.D., University of Liège
1951	Doctor Honoris Causa, National University of Mexico
1956	Sc.D., University of Pennsylvania
1960	Sc.D., Wesleyan University
1960	Dr. Phil., Kiel University
1960	Doctor Honoria, National University, La Plata
1961	Doctor of Laws, University of California

CONCLUDING REMARKS

According to Struve's own hand, when he was in his early fifties he was six feet tall, weighed 192 pounds, had grey hair and grey eyes. (I mention this because of some recollections that Struve was of *very* great size, which in fact was not all *that* great.) It has been said by a number of independent sources that he was walleyed because of spending too much time squinting into a microscope eyepiece measuring spectra, which ruined his binocular vision. His physical appearance and demeanor were such that people were intimidated by him, although he did not try to be intimidating.

Others speak of his European formality. In 1963 Struve sent a copy of *Astronomy of the 20th Century* to Morgan, which was inscribed, "To Bill Morgan." This was the only time Struve had ever called him by his first name. Yet with astronomers such as Bart Bok, Cecilia Payne-Gaposchkin, Sergei Gaposchkin, Bengt Strömgren, Nicholas Bobrovnikoff, and others, his letters began with greetings, "Dear Bart," Dear Cecilia," etc. Letters to his wife start simply, "Dearest" and end "Love, Otto."

Struve was a very, very serious person, but not one hundred percent so. The brief obituary of Struve by Chandrasekhar in the *Astrophysical Journal* [79] is accompanied by a picture of

Struve laughing. Chandrasekhar chose that picture on purpose. In a 1941 article on interstellar matter, Struve recalls something said by Eddington: people talk a lot about interstellar dust and gas, though they do not know much about it. This is like the man who refuses to sleep in a supposedly haunted room and says, "I do not *believe* in ghosts, but I am *afraid* of them."

What is Struve's legacy? It is not really that things have been named after him, like asteroid 2227 Struve,[80] or the eighty-two-inch telescope at McDonald, named in his honor in 1966.[81] One Festschrift was published in 1966,[82] and another in 1973.[83] In 1970 a collection of ten of his best papers was published with then-modern updates by experts in each subspecialty.[84] If we focus on Hearnshaw's book *The Analysis of Starlight,* the three most mentioned names are those of Struve, W. S. Adams, and H. C. Vogel.

In addition to the founding of observatories, the Struve family legacy involves the observation of double stars. Otto's great-grandfather and grandfather observed the positions of double stars. Otto observed them spectroscopically. Otto Struve was one of the first to study the processes of mass transfer between hot stars and their companions. "The fact is that mass transfer in interacting binaries is behind all the work now going on in such objects as novae, recurrent novae, and by generalization to more massive objects to the infall of material into white dwarfs, into black holes, etc."[85] Struve and other pioneers in this field gave us "information that was not merely gravitational but also entered the region of atomic physics."

> Another whole field that this touches on therefore is the nature of the spectroscopic peculiarities of matter in rapid motion at relatively low densities travelling from one star to another. Furthermore, in cases where mass is lost to an individual star alone or in a binary expanding shells

> occur which are recognizable by spectroscopic peculiarities due to the dilution of radiation. Struve and Wurm were among the first to compute the effect of dilution of the radiation field on the spectroscopic evidence. The idea of non-local thermodynamic equilibrium (non-LTE) now dominates solar envelope astrophysics, theory and observation, and became a useful tool for the recognition of stellar winds before observations from space and the ultraviolet made them directly observable. Similarly, the phenomenon he calls large scale turbulence which affects the curves of growth of disturbed stars is connected with the heating of the outermost envelope or the wind itself, so that the characteristic temperature inversion, with the chromosphere hotter than the reversing layer and the corona hotter than either seems to be a general characteristic of dilute gases in rapid motion.[86]

Struve's contributions to the growth of American astrophysics cannot easily be overestimated. He had a hand in the renaissance at Yerkes and the establishment of McDonald Observatory, Kitt Peak National Observatory, and the National Radio Astronomy Observatory. He built the Yerkes brain trust of the 1930s and 1940s and provided visiting European astronomers with scientific opportunities unattainable in Europe. Struve was one of the founders of modern consortium research. He also helped hold the International Astronomical Union together in the politically charged 1950s.

Struve was exceptionally dedicated to astronomy and worked very hard, many would say too hard, to advance it. His legacy is the influence he had on his colleagues and associates worldwide, personally and in print. His life and his dedication were exemplary, one might say even legendary.

This memoir could not have been written without input from people who knew Otto Struve, particularly S. Chandrasekhar, W. W. Morgan, Sarah Kuiper Lansberg, and Jesse and Naomi Greenstein. An earlier draft of this paper was read by a number of interested parties, among them A. Batten, D. Osterbrock, D. DeVorkin, J. Sweitzer, B. T. Lynds, V. Zebergs, I. Biermann, L. Sisson, and G. Herbig. The last mentioned made useful suggestions for the selected bibliography. Invaluable material was obtained from the Bancroft Library (University of California, Berkeley), the Yerkes Observatory archives, and the Gerard Kuiper archives at the University of Arizona.

NOTES

1. Otto Wilhelm Struve, *Zur Erinnerungan den Vater den Geschwistern dargebracht* (Karlsruhe, 1895):9. Unpublished 87-page translation by Alan Batten.

2. Alan H. Batten, "The Struves of Pulkovo—A Family of Astronomers," *Journal of the Royal Astronomical Society of Canada,* 71 (October 1977):345-72. See also Alan H. Batten, *Resolute and Undertaking Characters: The Lives of Wilhelm and Otto Struve* (Dordrecht: Reidel, 1988), and the series of articles about the Struves by Z. N. Sokolovskaya in the *Dictionary of Scientific Biography.*

3. Kevin Krisciunas, *Astronomical Centers of the World* (Cambridge: Cambridge University, 1988):chapter 5, and references therein.

4. Z. N. Novokshanova (Sokolovskaya), *Vasilii IAkovlevich Struve* (Moscow: Izdatel'stvo "Nauka," 1964):249-73.

5. O. W. Struve, note 1, pp. 29-30, 56.

6. Otto Struve's cousin Georg (1886-1933) was an astronomer in Germany whose son Wilfried (1914-) obtained a Ph.D. in astronomy in 1939, but after World War II became an acoustical engineer.

7. Otto Wilhelm Struve to Simon Newcomb, 13/1 January 1882, Newcomb Archives at the Library of Congress. There are eleven letters of Carl Struve to Newcomb and his wife in this archive.

8. Otto Struve, "Abridged Record of Family Traits," National Academy of Sciences, Washington, D.C. 1954?.

9. On one of Otto's immigration applications to the United States in 1921 he lists his nationality as Estonian. He may have feared that American officials would not give him a visa if they thought all Russians were Communists. Edwin B. Frost to Henry de Bach, July

26, 1921, Yerkes Observatory Archives. Also, Struve Archives at Bancroft Library, ID number 67/135, identity papers.

10. Gerard P. Kuiper to T. G. Cowling, August 7, 1963, Kuiper Archives, University of Arizona.

11. Lieutenant Struve is pictured on p. 5 of James S. Sweitzer, "A Most Exceptional Star: The Life of Otto Struve," *Griffith Observer,* 51 (September 1987):3-11.

12. Otto Struve, "Footnote to History," *Science,* 129 (1959):60.

13. Otto Struve, Autobiographical Materials, Bancroft Library, ID number 67/135.

14. T. G. Cowling, "Otto Struve 1897-1963," *Biographical Memoirs of the Fellows of the Royal Society,* 10 (1964):283-304, p. 284.

15. Sarah Kuiper Lansberg, "Stories told by or about Dr. Otto Struve," unpublished notes, 1963?.

16. F. D. M. (only author's initials given), "New Honors for Dr. Struve, the Wisconsin Astronomer Who Once Was Russia's Man without a Country," *Milwaukee Journal,* May 9, 1937. Obtained from Yerkes Observatory Archives.

17. Richard Luckett, *The White Generals: An Account of the White Movement and the Russian Civil War* (New York: Viking, 1971):349-54, 381-84.

18. By coincidence, a student and colleague of Wilhelm Struve was Wilhelm Wrangel, who became a Russian admiral. See Joseph Ashbrook, "The Crucial Years of Wilhelm Struve," *Sky and Telescope,* 25 (June 1963):326-27. Peter Wrangel was a descendent of Wilhelm Wrangel.

19. Luckett, note 17, p. 384.

20. Edwin Bryant Frost, *An Astronomer's Life* (Boston and New York: Houghton Mifflin, 1933):255-56.

21. Paul Guthnick to Edwin B. Frost, December 25, 1920, Yerkes Observatory Archives. Original in German.

22. Eva Struve to Edwin B. Frost, April 11, 1921, Yerkes Observatory Archives.

23. Edwin B. Frost, "A Family of Astronomers. Hermann Struve, 1854-1920; Ludwig Struve, 1858-1920," *Popular Astronomy,* 29 (1921):53641, p. 539.

According to Gleb Struve (Otto's second cousin), Peter Struve (Gleb's father) may have played a role in putting Otto in touch with Frost. Peter Struve was an advisor to Generals Denikin and

Wrangel and traveled back and forth from Berlin to southern Russia during the Civil War (letter of Alan Batten to K. Krisciunas, July 27, 1988). He was also a prolific writer on economic and political matters. See two-volume biography by Richard Pipes, *Struve: Liberal on the Left* (*1870-1905*) and *Struve: Liberal on the Right* (*1905-1944*) (Harvard University Press, 1970 and 1980). Peter Struve's collected works, numbering 663 items, were published in a fifteen-volume edition in 1970 by University Microfilms.

24. Edwin B. Frost to Otto Struve, March 2, 1921, Bancroft Library, ID number 67/135. A copy exists in the Yerkes Observatory Archives.

25. Otto Struve to Edwin B. Frost, April 28, 1921, Yerkes Observatory Archives.

26. Otto Struve to Edwin B. Frost, March 11, 1921, Yerkes Observatory Archives. Original in German.

27. Otto Struve to Edwin B. Frost, April 12, 1921, Yerkes Observatory Archives.

28. Edwin B. Frost to Harry Pratt Judson, April 14, 1921, Yerkes Observatory Archives.

29. Edwin B. Frost, note 23, pp. 540-41.

30. Edwin B. Frost to Alexander Kaznakoff, October 10, 1921, Yerkes Observatory Archives.

31. Cowling, note 14, p. 285. According to Cowling, who is probably basing his version on notes from Mary Struve, Otto arrived in Wisconsin wearing a green hat, blue coat, brown trousers, and tan shoes. In any case, he must have been quite a sight!

32. Edwin B. Frost, note 20, p. 256.

33. There is some confusion as to the number of Otto's sisters. Did he have one or two? In Guthnick's letter to Frost, note 21 above, it states, "eine jüngere Schwester vor den Augen des Vaters und eines Bruders beim Baden ertrunken," meaning "a younger sister [of Otto] drowned right within view of [her] father and brother." This could mean "a sister younger than Otto" or "the younger of Otto's two sisters." Given that the drowning was known to Guthnick, it must have been known to Eva Struve in Berlin (Otto's aunt). Another sister must have existed, for in the letter of Eva Struve to Frost (note 22), dated April 11, 1921, it says, "My nephew's mother and sister are at Simferopol in Crimea." Also, in Sokolovskaya's article on Ludwig Struve in the *Dictionary of Scientific Biography,* she

refers to a six-year-old sister of Otto dying in 1919, and refers to Otto's brother Werner and another sister.

34. S. Chandrasekhar, interview by Kevin Krisciunas, October 6, 1987, American Institute of Physics, Oral History Project, p. 20.

35. Naomi Greenstein, "Reminiscences of Otto and Mary Struve," cassette tape monologue of January 21, 1988, as suggested by a list of questions prepared by Kevin Krisciunas. American Institute of Physics, Oral History Project.

36. Letter of Donald E. Osterbrock to Kevin Krisciunas, July 9, 1988. Osterbrock attributes this to Su-Shu Huang, who worked with Struve in Berkeley.

37. Interview of Paul and Helen Jose, Fort Davis, Texas, January 15, 1988, by Kevin Krisciunas and James Sweitzer. The Joses did graduate work in astronomy at the University of Michigan in the early 1930s. Paul Jose worked at McDonald Observatory in the late 1940s and the 1950s. They still own the Struves' incredibly long dining room table and eleven of the original twelve chairs, which Mary and Otto Struve had at House A at the observatory. More on Paul Jose is to be found in Evans and Mulholland's book (note 45).

38. Otto's mother died on October 1, 1964, at the age of ninety. Mary Struve was discovered to have died on August 5, 1966. The estimated date of death was July 19. The cause of her (natural) death could not be determined. (Death certificates obtained from the Alameda County, California, recorder.) I am told by a number of sources that after World War II Mary Struve was very much a recluse.

39. *Dernières Nouvelles* (Paris), No. 614, 1922, in Russian.

40. Materials pertaining to the Astronomers Relief Committee were obtained from the Yerkes Observatory Archives.

41. The most complete published list is by A. Unsöld in *Mitteilungen der Astronomischen Gesellschaft* (1963, pp. 5-22), which lists 444 papers and abstracts, with references to data published in Harvard Cards, and observatory reports. However, in the Bancroft Library at Berkeley there is Struve's own list compiled in early 1962, which is 876 items long. We must subtract two from that list because they are errata, and four more because they are the second halves of articles published in *Sky and Telescope.* But we must add three books, twenty-one items in Unsöld's list, one from *Popular Astronomy,* and twelve articles from *Sky and Telescope* (May 1962 to April 1963),

hence the number 907. About 8 percent of these are abstracts and observatory reports.

42. The most prolific astronomer, according to the number of published items, was Ernst Öpik (1893-1985), who published 1,094 items. Letter of John McFarland, librarian at Armagh Observatory, to K. Krisciunas, February 27, 1986.

43. Full references to Struve's published work are included in the selected bibliography at the end of this memoir.

44. Cecilia Payne-Gaposchkin, *An Autobiography and Other Recollections,* ed. Katherine Haramundanis. (Cambridge: Cambridge University Press, 1984):169.

45. David S. Evans and J. Derral Mulholland, *Big and Bright: A History of the McDonald Observatory* (Austin: University of Texas Press, 1986).

46. Bengt Strömgren, interview by Lillian Hoddeson and Gordon Baym, May 6 and 13, 1976, American Institute of Physics, Oral History Project, pp. 26, 46.

47. W. W. Morgan, interview by David DeVorkin, August 8-9, 1978, American Institute of Physics, Oral History Project, p. 13.

48. E. A. Milne, "On the Award of the Gold Medal to Professor Otto Struve, Director of the Yerkes and McDonald Observatories," *Monthly Notices of the Royal Astronomical Society,* 194 (1944):112-20, p. 117. One is reminded of the twentieth century Indian mathematician Ramanujan, for whom "every positive integer was one of his personal friends." See James R. Newman, "Srinivasa Ramanujan," in *The World of Mathematics,* ed. James R. Newman (New York: Simon and Schuster, 1956):368-376, p. 375.

49. Otto Struve and Velta Zebergs, *Astronomy of the 20th Century* (New York and London: Macmillan, 1962):305-12.

50. Margherita Hack, "Epsilon Aurigae," *Scientific American* (October 1984):89-105. See also *1982-1984 Eclipse of Epsilon Aurigae,* ed. Robert E. Stencel (Washington, D.C.: NASA, 1985), NASA Conference Publication 2384.

51. Chandrasekhar interview, 1987, note 34, p. 9.

52. G. H. Herbig, "Introduction: A Personal and Scientific Appreciation of Otto Struve," in *Spectroscopic Astrophysics: An Assessment of the Contributions of Otto Struve,* ed. G. H. Herbig. (Berkeley and Los Angeles: University of California Press, 1970):1-3, p. 2.

53. W. W. Morgan, interview by Kevin Krisciunas, October 7, 1987.

54. S. Chandrasekhar, interview by Spencer Weart, May 17-18, 1977, American Institute of Physics, Oral History Project, p. 70.

55. Jesse Greenstein, "Otto Struve," cassette tape monologue [July] 1988, American Institute of Physics, Oral History Project, p. 6.

56. Chandrasekhar interview, 1987, note 34, pp. 14-15.

57. W. H. McCrea, "Clustering of Astronomers," *Annual Review of Astronomy and Astrophysics,* 25 (1987):1-22, p. 13.

58. J. B. Hearnshaw, *The Analysis of Starlight: One Hundred and Fifty Years of Astronomical Spectroscopy* (Cambridge: Cambridge University Press, 1986):337.

59. Morgan interview, 1978, note 47, p. 13.

60. Evans and Mulholland, note 45, p. 31. See also *Ciel et Terre* (1934):97-100 and (1935):170.

61. Struve had wanted Strömgren to come for three years, but they agreed that it would be half that. It was agreed from the start to be a temporary appointment. Strömgren interview, 1976, note 46, pp. 29, 48.

62. David H. DeVorkin, "The Maintenance of a Scientific Institution: Otto Struve, the Yerkes Observatory, and Its Optical Bureau during the Second World War," *Minerva* 18 (Winter 1980):595-623.

63. Otto Struve, "Cooperation in Astronomy," *Scientific Monthly* 50 (1940):142-47; DeVorkin, note 62, pp. 603-4; Evans and Mulholland, note 45, pp. 98-100.

64. Leo Goldberg, "The Founding of Kitt Peak," *Sky and Telescope,* 65 (March 1983):228-32.

65. Chandrasekhar interview, 1987, note 34, p. 25.

66. After his success in building the 100-inch telescope at Mount Wilson, Hale gave up the directorship in 1922. W. S. Adams succeeded him in this role, but as the person in charge of operations. Hale continued on as "honorary director in charge of policy." See Helen Wright, *Explorer of the University: A Biography of George Ellery Hale* (New York: E. P. Dutton, 1966):345.

67. Chandrasekhar interview, 1977, note 54, p. 71.

68. Sarah Kuiper Lansberg, interview by Kevin Krisciunas, January 16, 1988.

69. Cowling, note 14, p. 292.

70. Otto Struve to Mary Struve, January 5, 1949, Bancroft Library.

71. Chandrasekhar interview, 1987, note 34, p. 26.

72. A. van Hoof, "The Beta Canis Majoris Stars," in Herbig, note 52, pp. 343-63, 361.

73. Ruth S. Freitag to Kevin Krisciunas, February 23, 1988.

74. A. I. Slastenov, *Astronomy at the University of Khar'kov over 150 Years* (in Russian) (Khar'kov: Khar'kovskogo gosudarstvennogo universiteta imeni A. M. Gor'kogo, 1955):64-66, p. 64. See also Vladimir Kourganoff, "Otto Struve: Scientist and Humanist," *Sky and Telescope,* 75 (April 1988):379-81; and Kevin Krisciunas, "More About Otto Struve," *Sky and Telescope* 76 (September 1988):229-30.

Let us consider the grammatical structure of the first sentence of the quote ". .. .having betrayed his native land, he went abroad and settled in the USA." Given the temporal ordering of these three clauses, "having betrayed his native land" came before his emigration, and so it must refer to Struve's activity as an officer in the White Russian Army. (I thank Prof. Kourganoff for his help in clarifying the original Russian.)

75. Death certificate obtained from Alameda County, California, recorder.

76. Beverly T. Lynds, interview by Kevin Krisciunas, August 12, 1988.

77. David S. Heeschen, letter to K. Krisciunas, August 12, 1988. The 300-foot transit telescope at NRAO met its demise on November 15, 1988. See Gerrit L. Verschuur, "Reminiscences of the 300-Foot," *Sky and Telescope,* 77 (March 1989):252-53.

78. Otto Struve to I. I. Rabi, October 31, 1961, Bancroft Library.

79. S. Chandrasekhar, "Otto Struve. 1897-1963," *Astrophysical Journal,* 139 (February 15, 1964):423.

80. *Sky and Telescope,* 68 (October 1984):312.

81. Bart J. Bok, "Otto Struve Memorial Symposium," *Sky and Telescope,* 32 (August 1966):68-71.

82. M. Hack, ed., *Modern Astrophysics: A Memorial to Otto Struve* (Paris: Gauthier-Villars, and New York: Gordon and Breach, 1967).

83. A. H. Batten, ed., *Extended Atmospheres and Circumstellar Matter in Spectroscopic Binary Systems* (Dordrecht: D. Reidel, 1973). IAU Symposium 51 (Struve Memorial Symposium).

84. See G. H. Gerbig, ed., note 52.

85. J. Greenstein, note 55, p. 4.

86. J. Greenstein, note 55, pp. 4-5.

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Technology Portrait Studio

Norbert Wiener

Norbert Wiener

November 26, 1894-March 18, 1964

by Irving Ezra Segal

Norbert Wiener was one of the most original mathematicians and influential scientists of the twentieth century. He developed a new, purely mathematical theory, an integral calculus for functions of infinitely many variables known as functional integration. It has been of great importance for probability and theoretical physics. Wiener made huge strides in the harmonic analysis of functions of real and complex variables. In a unified way, this resolved old problems, produced new challenges, and provided a prototype for key aspects of harmonic analysis on topological groups. In part concurrently, he developed applications of his mathematical ideas in engineering, biology, and other fields. In later life he developed a synthesis of such applications with diverse ideas represented by central parts of the work done in the twenties and thirties by Vannevar Bush, Walter B. Cannon, Alan Turing, and others.

This synthesis, which he called "cybernetics," has since been a productive unifying philosophy in science and engineering. In the United States, it primarily epitomized his earlier contributions to communication engineering; in Britain, it had a notable impact on neurophysiology, and its delayed, but eventually enthusiastic, acceptance in the Soviet Union stimulated important mathematical developments in control and ergodic theory. Before and while cybernetics was being developed, Wiener was a prime mover in multidisciplinary groups in these subjects. As much as anyone, he showed the importance of *higher* mathematics for fundamental applications, and the general scientific effectiveness of the mathematical way of thinking. At the same time, in association with his work, he elaborated philosophical and social ideas that influenced world culture.

ORIGIN

Norbert's paternal grandfather, Solomon Wiener, was a journalist and teacher of German background who worked in Poland. Norbert wrote of him that he sought to encourage the replacement of Yiddish by German among the Jews there. Norbert's father, Leo Wiener, was born in Bialystok, Poland, in 1862. Leo was related through his mother, Freda Wiener, to Leon Lichtenstein, a well-known German mathematician, as first cousin. Norbert later met Lichtenstein in Europe, and it is interesting that Lichtenstein's central interests of applied mathematics and potential theory came to be important ones for Norbert. Leo Wiener studied engineering in Berlin and medicine in Warsaw. At the age of eighteen he emigrated to the United States. He had had a plan to join in an undertaking to found a utopian community along Tolstoyan lines in Central America. which fell through when his partner backed out. However, in this connection, apparently, he disembarked at New Orleans. After a succession of employments and travels, he became professor of modern languages at the University of Missouri.

Norbert's maternal grandfather, Henry Kahn, had a department store in Missouri, to which he had emigrated from Germany. Kahn's wife came from a family named Ellinger, which had been settled in the United States for some time. Their daughter, Bertha Kahn, married Leo Wiener in 1893. Their first child, Norbert, was born a year later on November 26, 1894, in Columbia, Missouri. The name Norbert was taken from a work of Robert Browning, the couple being thought to have met at a Browning club. Within the following year, Leo Wiener lost his position, apparently as a result of university politics. He decided to move to the Boston area in search of employment, and found an apartment in Cambridge. After a variety of positions, he obtained a part-time instructorship in Slavic languages at Harvard University. In conjunction with ancillary positions at Boston University and the New England Conservatory, among others, this provided a livelihood during the earlier years in Cambridge. Eventually he became a tenured professor of Slavic languages at Harvard University, a position he held until his retirement.

In the first volume of his unusually intimate autobiography, Norbert gave a portrait of his parents, and especially of his father. He was highly adulatory, but at the same time displayed some intellectual but principally emotional reservations He indicated that his father had placed somewhat excessive pressure on him and had not given him sufficient credit for his intrinsic merits. Instead, he felt, his father attributed his son's precocity and brilliance to his upbringing in accordance with the educational and social ideas he had espoused.

By other accounts, Leo Wiener was exceptionally original, imaginative, and productive intellectually. At the same time, he was a fine teacher and socially very broadly involved. His primary profession was that of a linguist and philologist, and he attained very high distinction in these fields. However, he developed original ideas in quite different areas, for example geology, but his theories had few followers in his day.

There is no question that Leo Wiener was quite concerned about the intellectual development of Norbert and his other children. Early on, he taught Norbert mathematics, languages, and other subjects. He put Norbert in touch with many outstanding intellects that later influenced him. A typical example of this was his taking Norbert to visit the laboratory of his friend, Walter B. Cannon. Cannon's concept of homeostasis was later to form one of the crucial pillars of Wiener's *Cybernetics.*

In 1898 the family had a second child, Constance. She later married Professor Philip Franklin, a mathematician at the Massachusetts Institute of Technology. A boy was born in 1900, but died in infancy. In 1901, the family visited Europe, following which Norbert entered third grade in a public school in Cambridge. However, after quickly advancing to the fourth grade, he was removed from the school by his father until he entered high school, two years later. Meanwhile, his second sister, Bertha, was born in 1902. She later married Professor Carroll W. Dodge, a botanist at Washington University in St. Louis, Missouri. Besides these two girls, the family eventually included four boys, of whom two died in infancy.

EDUCATION

In 1906, Norbert graduated from high school in Ayer, Massachusetts, and entered Tufts College to study biology and mathematics. His brother Frederick was born in the same year. In 1909, Norbert was graduated from Tufts College with a *cum laude* A.B. degree. He then entered Harvard Graduate School with the intention of studying zoology. However, the emphasis on laboratory work in this subject turned out to be inappropriate for him, and a year later he transferred to Cornell University, where he had been given a scholarship in the Sage School of Philosophy. He stud ied there with Frank Thilly, a friend of his father's from the Missouri days who had facilitated Norbert's transfer to Cornell, and Walter A. Hammond and Ernest Albee. But the work there again did not proceed really well, and a year later Norbert transferred back to the Harvard Graduate School.

However, he stayed with the subject of philosophy. He studied with Edward V. Huntington, G. H. Palmer, Josiah Royce, and George Santayana, some of the well-known philosophers of the time. He received an M.A. degree in 1912, but because of Royce's diminishing health, worked for his doctorate with Karl Schmidt of Tufts, who as a young professor was interested in mathematical logic. In 1913, Norbert was graduated from Harvard with a Ph.D. in philosophy.

In the meantime, the last child to be born to the family (in 1911) died in infancy. Norbert was living at home while at graduate school, and had responsibility for the care of his brother Frederick. Family pressures were strong and burdensome for Norbert. He was quite pleased when the traveling fellowship for which he had applied was awarded to him by Harvard. He contemplated going to Cambridge, England, to work with Russell, and to Turin to work with Peano. Hearing of Peano's decline in scientific activity and considering Russell to be quite active, he decided to go to Cambridge to pursue studies in mathematical logic.

POSTDOCTORAL YEARS

Norbert was not quite as satisfied with Russell's lectures and their meetings as he had expected. At the same time he found that despite his limited background in mathematics, he was able to pick up quickly on the mathematics lectures with which he supplemented his philosophical studies. This was particularly true of the lectures of G. H. Hardy, which Norbert found absorbing. Hardy was probably the leading

English mathematician of his day. Hardy showed less of the reserve that Wiener was sensitive to in some of his other teachers and fellow students, The support and encouragement given by Hardy to the ambitious but uncertain young man seeking a challenging direction in which he could display his intellectual prowess was probably an important factor in Norbert's becoming a mathematician.

Hardy's lectures and writings were virtual works of art as much as of science, displaying personal enthusiasm, richness of content, and unsurpassed lucidity. He showed a sincere and effective interest in talented young mathematicians, and he was not put off by eccentricity—most notably in the case of the Indian mathematician, Ramanujan, but also that of Wiener. In effect, he converted Norbert from a relatively diffuse interest in issues of broader relevance to one of concern for mathematical penetration and perfection. Hardy became somewhat in *loco parentis* to Norbert, and played this role intermittently for two decades afterward. However, Hardy seems never to have understood the side of Wiener that was deeply attracted to the broad issues of science and to possible applications; he even raised the question of whether Wiener's apparent concern about the latter was not a pose. From Hardy's position, somewhat that of a gentlemanly, latter-day scientific aesthete, such a pose would have been acceptable, while a true interest in applications would have been quite irrelevant. But however much Wiener's professional career depended on his prowess in pure mathematics, his later work was to display quite convincingly and effectively a profound concern for issues of external relevance. With Hardy's support, Wiener was to become better known for his work on Tauberian theorems than for his earlier and probably more innovative work on Brownian motion, which was outside the mainstream of mathematics during the twenties.

During the second semester of Norbert's fellowship year, Russell was away. As a consequence, he went to Göttingen for an extended visit. There Wiener attended the lectures of David Hilbert and Edmund Landau in mathematics, and those of Edmund Hüsserl in philosophy, before returning to Cambridge. The outbreak of World War I led to his return to the United States, where he completed a second traveling fellowship he had been awarded at Columbia University. He studied there with John Dewey, among others.

Following this, Norbert received a junior position at Harvard University. During 1915-16 he lectured there on the logic of geometry. During 1916 he served with Harvard's reserve regiment at the Officer's Training Camp in Plattsburg, New York. In 1917 he served with the Cambridge R.O.T.C. During these years he also worked variously as an instructor in mathematics at the University of Maine in Orono (1916-17); as an apprentice engineer in the Turbine Department of the General Electric Corporation in Lynn, Massachusetts (1917); and as a staff writer for the *Encyclopedia Americana* in Albany, New York (1917-18).

Both Norbert and his father were strongly and publicly for the Allied cause, despite their ties to German culture. Norbert expressed opposition to "Prussian militarism." He wrote frequently to individual members of his family, and in January 1918 wrote to his father as follows (in part):

> I think the time has come for me to make a last try to get into military service, and I am writing to ask you for permission.... It is not that I am dissatisfied with my work nor that I have any particular love for a military career, but I hate to think of myself as less of a man than those of my friends who are in the army, and I do not care after this war to look back on myself as a slacker. I cannot be anything but ashamed of myself when I advocate a war that I do not share in.

In 1918, Norbert accepted an invitation from Oswald Veblen, who was then an officer in the Army in charge of the computation of ballistic tables, to join him in this work at the Aberdeen Proving Ground, Maryland, as a civilian employee. Veblen's letter noted, in apparent response to Norbert's expressed inclinations, that should he prefer to do the work in a military capacity, such an arrangement might later prove possible. Indeed, Norbert enlisted in the Army as a private some months before the war ended, and continued in the same work. Two months after the war ended, Norbert felt that he was no longer needed. He sought his discharge, and this came through in February 1919.

Veblen, who was already a leading mathematician, returned to Princeton University, bringing with him various younger mathematicians who had displayed talent. Norbert would have liked to go with him but did not receive the call. Instead, he was recommended for a position in the Department of Mathematics at the Massachusetts Institute of Technology by Professor W. F. Osgood of Harvard University. Although the Institute had world renown as a school of engineering in 1919, it was then far from being a leading center of mathematical research. Norbert did not regard the recommendation as evidence of attainment of much standing as a mathematician, but he accepted the position that was offered. He remained at the Institute up to the time of his death, and his scientific interaction with it was to prove a great mutual benefit.

CENTRAL DECADE I: 1919-29

Wiener's early mathematical papers concerned mathematical logic and its relations to space, time, and measurement. In their physical and empirical concerns they foreshadow some of his mature interests. They display notable seminality and independence, and are quite interesting from a historical perspective. Their publication, in considerable number in the *Proceedings of the Cambridge Philosophical Society,* was facilitated by Hardy, whose lectures Wiener was attending at the time of his first publication (in mathematics) in 1913.

Whether despite or in part because of the turbulence of the war years, the seed of Wiener's scientific innovation began to sprout vigorously soon after. His first major mathematical salient, in what is now called functional integration, began around 1919. In August 1920, in one of his many intimate letters to his sister Constance, he wrote from Paris as follows:

> I have not been able so far to get in touch with Frechet. I have wired him that I am here and awaiting an answer. I find that I am making a little headway with my problem—integration in function space—and in a way that may have practical application. I define the measure of an interval in it in a way that hitches up with probability theory as it is applied in statistical mechanics, and I have been living in hopes that the Lebesgue integral which I can get from it will be good for something. At any rate, when I meet Frechet, I shall have a peach of a problem to work on.

This was the beginning of his work on a mathematical theory of Brownian motion, essentially the theory of "Wiener space," as others have called it, and the prime example for the modern theory of functions of functions, or for functions of infinitely many variables. The physical theory of Brownian motion had earlier been studied by Einstein and Smoluchowski and was proposed to Wiener as a topic for investigation by Russell; coincidentally and again serendipitously, the problem of integration in function space had been proposed to Wiener by I. A. Barnett, a former student of E. H. Moore, one of the founders of modern American mathematics, who had initiated research on this problem. These earlier approaches were different from Wiener's and had no special relation to Brownian motion, which Russell had earlier suggested as a topic for investiga tion. Wiener was quick to see that the conjunction of the integration in function space idea with the normal probability law established in the physical theory of Brownian motion led to an extremely incisive and interesting mathematical development, which at the same time dovetailed beautifully with the qualitative aspects of the physical theory noted by Perrin.

During the early 1920s, Wiener sank his roots deeper into functional integration, while at the same time making significant contributions in a variety of other parts of analysis. The novelty of his Brownian motion theory was such that it was not at all widely appreciated at the time, and the few who did, such as H. Cramer in Sweden and P. Levy in France, were outside the United States. He became somewhat better known for his work in potential theory. This was a traditional field, unlike functional integration, and although his work connected most closely with work of Lebesgue, Perron, and others in Europe, it seems to have been precipitated by his attendance at the lectures of O. D. Kellogg at Harvard, which both informed Wiener and aroused his interest. In a remarkably short period of time, of the order of two years, Wiener made a series of brilliant contributions that fundamentally altered the subject, which was never the same thereafter. He developed a fruitful concept of generalized solution to the Dirichlet problem (that of the solution of the Laplace equation attaining prescribed values on the boundary of the region in question). He was led thereby to a general notion of capacity that has been essential for modern potential theory. In a beautiful epilogue to this work, he gave a precise geometrical criterion for the regularity of a boundary point relative to the Dirichlet problem.

The theory of almost periodic functions burst on the scene in the twenties with the work of H. Bohr in Copenhagen;

it seemed potentially a promising approach to the intractable Riemann hypothesis and interested Wiener for its relation to the study of vibrations. He was awarded a Guggenheim Fellowship in 1926-27 to work on the subject and arranged through Max Born, who had worked with Wiener on quantum mechanics during a visit to MIT, to spend one semester in Göttingen, and arranged with Bohr to spend the other in Copenhagen.

The eight years beginning in 1926 were to be the most eventful in Wiener's personal as well as scientific life. In 1926 he married Margaret Engemann, a graduate of Utah State College and Radcliffe, who had earlier emigrated with her family from Germany. Their first daughter, Barbara, was born in 1928, and the second, Margaret, in 1929. Although he encountered some unpleasantness in Göttingen arising from the family's espousal of the Allied cause during World War I, the visit was scientifically extremely stimulating. In particular, he gave a course on trigonometric series in this major center and was exposed to ideas of R. Schmidt, which influenced him toward his group-theoretic treatment of Tauberian theory, one of his major scientific achievements. Schmidt's approach complemented that of Hardy and Littlewood on Tauberian theorems, the two together providing the groundwork for Wiener's brilliant synthesis, "Tauberian Theorems," a 100-page article which appeared in 1932. An even longer memoir, "Generalized Harmonic Analysis," which appeared in 1930, reflected in part his work with Bohr, provided an alternative approach to his Brownian motion theory, and connected this with the spectral analysis of functions on the line.

CENTRAL DECADE II: 1930-40

The Wiener family visited Cambridge University during 1931-32, and at Hardy's invitation, Wiener lectured on har monic analysis. Although the Fourier transform was a classic subject, Wiener's approach in his later book reflecting these lectures, *The Fourier Integral and Certain of Its Applications,* was a distinctive and seminal one. In it one can see the seed of important relations between harmonic analysis in euclidean space, a theory invariant under rigid displacements in this space, and analysis in function space, which relates to Hermite functions much as euclidean harmonic analysis relates to complex exponentials and is ultimately seen to be invariant under the group of unitary operators on a Hilbert space. The connection was to lead to "The Homogeneous Chaos," one of Wiener's most seminal papers, which facilitated harmonic analysis in Wiener space and related to the mathematical theory of Bose-Einstein quantum fields. A decade later, it led to the work of his disciples R. H. Cameron and W. T. Martin on analysis in Wiener space.

During the years 1932-33, Wiener was fortunate in having the collaboration of a brilliant young English mathematician, R. E. A. C. Paley. Following Paley's accidental death, Wiener combined their researches in an important and influential book, *Fourier Transforms in the Complex Domain,* published in 1934. This concerned several aspects of harmonic and stochastic analysis, especially Laplace as contrasted with Fourier integrals, an extension paralleling his earlier extension of Schmidt's work in Tauberian theory using complex methods. Comparing his own relation to Paley as somewhat similar to that of G. H. Hardy to his collaborator, J. E. Littlewood, Wiener wrote:

> My role was primarily that of suggesting problems and the broad lines on which they might be attacked, and it was generally left to Paley to draw the strings tight.

In his obituary of Hardy he wrote:

> I think it is fair to say that throughout their long collaboration the extremes of technical facility belong to Littlewood, but that much of the nexus of leading ideas and the philosophical unity is that of Hardy.

In general, Wiener's interest and thrust was to be primarily ideational and only secondarily technical, and enhanced precision and clarity was brought to his articles by the suggestions of a variety of mathematicians who became interested in his ideas.

Wiener's collaboration in the early thirties with E. Hopf, who came to MIT at Wiener's invitation, produced significant work for applied as well as pure mathematics, on an integral equation that bears their names. The topic could also be construed as one in complex harmonic analysis, and is exposed in Wiener and Paley's book, *Fourier Transforms in the Complex Domain* (1934,3). The extension of real harmonic analysis to the complex domain was one of Wiener's major secondary themes. His study of the work of Heaviside and background in communication theory made this most natural. The applications of harmonic analysis to communication theory largely concern networks and similar mechanisms. As physical objects, these have causality features not logically essential for real harmonic analysis, but which translate into complex analyticity features on Fourier transformation. The book with Paley was in significant part a rigorous and coherent treatment of the basic mathematical phenomena behind this connection. It had considerable pure as well as applied influence, being developed further in works of S. Bochner, E. Hille, and J. D. Tamarkin, among others. On the applied side, the method of factorization in the complex plane used in the treatment of the Wiener-Hopf equation has been useful for a variety of problems, including diffraction and prediction theory.

Wiener's influence was propagated by a number of students and disciples. S. Ikehara, a doctoral student from

Japan in the early thirties, developed a variant of the Wiener Tauberian theorem that was adapted to the treatment of the Riemann zeta function and led to one of the simplest proofs of the prime number theorem, to the effect that the number of primes less than n is asymptotic to $n/\log n$. An outstanding doctoral student was Norman Levinson, who worked initially in harmonic analysis in extension of the line developed by Paley and Wiener and later made significant contributions to the theory of ordinary differential equations. R. H. Cameron and W. T. Martin were already at the postdoctoral level when they began working with Wiener in the late thirties on complex and Fourier analysis. The English mathematician H. R. Pitt came to Cambridge and worked with Wiener on analytic functions of absolutely convergent Laplace-Stieltjes transforms, in extension of the core of Wiener's Tauberian theory. Wiener also worked with R. H. Cameron on the same subject. In part this involved continuous singular measures, on which Wiener worked with Aurel Wintner, a mature German mathematician who had emigrated to the United States. Wintner shared Wiener's interest in probability, and they worked together intermittently for two decades on issues in harmonic analysis and ergodic theory.

The later thirties also saw the beginning of Wiener's espousal of what he later termed "cybernetics, or control and communication in the animal and the machine." The meaning and role of such concepts as memory and learning in machines—a precursor to the field of artificial intelligence—was explored by Wiener in association with students and colleagues. The cofounder of modern information theory, Claude Shannon, took his doctorate at MIT during this period, as did Wiener's student Brockway McMillan, who contributed to Shannon's later theory. While Wiener's ideas concerned information in the broadest context, Shannon's work treats specifically the production and communication of information in a machine context. The work of the Shannon school has provided probably the major concrete exemplification and indication of practical relevance for Wiener's ideas. It also influenced ergodic theory, which was applied by McMillan, and led in particular to the introduction by Kolmogorov of the concept of the entropy of a flow, which has played an important role in ergodic theory ever since.

The respective ergodic theorems of G. D. Birkhoff and J. von Neumann in the early thirties established ergodic theory as a mathematical subject. Both Wiener and his colleague Eberhard Hopf at MIT were vitally interested in the physically fundamental applications of higher mathematics and became involved in the new subject. In the late thirties Wiener made significant contributions to it, including his Dominated Ergodic Theorem. This strengthened the theorems of Birkhoff and von Neumann, and illuminated the types of convergence involved in ergodic theory.

Probably Wiener's most important mathematical work of later years, and the only one comparable in depth and originality to his earlier work on Brownian motion, on real harmonic analysis, and on potential theory, was on what he termed "the Homogeneous Chaos." This work in the late thirties related ergodic theory to Wiener space Wand to harmonic analysis. The Hilbert space $L_2(W)$ (i.e., the space of all square-integrable functionals defined on Wiener space) was shown to be the direct sum of a sequence of orthogonal subspaces K_n ($n = 0, 1, 2,. . .$), each of which was invariant under the induced unitary action on $L_2(W)$ of the measure-preserving transformations $\Gamma(T)$, arising in a natural way (first observed by B. O. Koopman) from an arbitrary orthogonal transformation T on $L_2[0,1]$. Even nonlinear transformations could be represented by measure preserving transformations on *W,* as Wiener emphasized in later applications, although the latter did not in general leave invariant the K_n. This work exhibits in nascent form a major aspect of the equivalence of the particle and wave representations of a quantized Bose-Einstein field—the K_n are what are known as the n-particle subspaces in this connection—as mathematically formulated in the fifties but ideationally going back to the beginnings of quantum field theory in the highly heuristic form given by Dirac, according to which "a Bose-Einstein field is equivalent to an assembly of harmonic oscillators." It was characteristic of Wiener's extraordinary scientific intuition that he was able to construct a basic part of quantum field theory on the basis of pure thought, starting from his theory of functional integration. However, in this work, as well as in later, related work with A. Siegel in the direction of quantum theory, the relation to physics is argued quasi-philosophically rather than objectified analytically; in particular, the quantized field itself is not modeled. Indeed, Wiener seems generally to have eschewed noncommutative operator theory—a major scientific difference between him and the man who is otherwise most similar scientifically, John von Neumann. Relatedly, whereas von Neumann systematically deployed abstract algebraic methods in the direction of modern analysis and its applications, Wiener left undeveloped some of his own insights in the algebraic direction and preferred a classical and concrete approach that had a measure of continuity with the ethos of the Hardy-Littlewood school.

During the academic year 1935-36, Wiener was visiting professor at Tsing Hua University in Peiping (now Beijing), China. He spoke Chinese and many other languages with unusual fluency, even after modest exposure to them. He published several papers in China on analysis of the Hardy-Littlewood type.

PRIORITY TO APPLICATIONS (1940-64)

Wiener had always been inclined toward applications of mathematics in science and engineering, but before World War II his central contributions had been of an essentially general mathematical nature. The war focused his interests into concrete directions, and from that time onward his contributions were primarily in the direction of applications.

The first of these was to prediction theory, which was involved in anti-aircraft fire control. This was a natural and fairly straightforward application of the theory of stochastic processes, which J. L. Doob and others in the United States had developed on a rigorous basis, following Kolmogorov's mathematical formulation of the foundations of probability theory in 1933. Unsurprisingly, in view of the exigencies of the war, Kolmogorov himself had begun to publish on prediction theory, but his work was unknown to Wiener until I chanced to mention it to him at a meeting. However, after incisive early work, Kolmogorov left the subject, while Wiener developed it rather fully, including its engineering aspects, during and after the war. His mathematical theory, which modeled deviations from the signal, or "noise," as a stationary multivariate Gaussian stochastic process, was developed jointly with the younger mathematicians E. J. Akutowicz and, especially, P. Masani. The engineering implementation was developed in collaboration with Julian Bigelow. The basic theory was given in a report published during the war; this was effectively a draft of his monograph, "Extrapolation, Interpolation, and Smoothing on Stationary Time Series," published in 1950.

This work represents a special case of the study of mechanisms as devices that effect an input-output transfer, with regard to smoothing, feedback, and stability, independently of internal dynamics. In part, cybernetics emerges naturally from this study, for which the prediction theory was an important prototype. In principle, as Wiener stressed, similar considerations apply to the biological and social sciences as well as to the engineering and physical sciences. However, the former systems are relatively complex and lacking in symmetries, so that a general theory cannot be expected to apply to them with the same specificity as in the case of the temporally or spatially invariant models used in electrical engineering and physics. The latter were the systems to which Wiener's mathematical investigations before the war related. Correspondingly, it was the breadth and coherence of the cybernetic philosophy, and its usefulness as a guide to innovative development and experiment, that were its main contributions, rather than any difficult or incisive technical accomplishment. However, its permeation of scientific thought has been so extensive that its novel and stimulating character before and during the war cannot now be readily appreciated.

The origin of Wiener's almost uniquely comprehensive scientific identity can be traced along the following lines. He started out in biology, but felt himself to be too clumsy in experimentation and turned to philosophy. He took his doctorate in this subject, and undertook postdoctoral work directed toward logic. He seems to have turned to mathematics largely because he found he could do it relatively easily and well, and thereby make his mark in the intellectual world more readily than in other subjects. But important factors in his becoming a mathematician were Hardy's leadership and, probably, Russell's declining interest in mathematical logic.

By the middle and late thirties, Wiener had attained pure mathematical eminence, indeed a virtual world preeminence in a major part of mathematical analysis. But his concern with applications had not lapsed and indeed had been nurtured during his years at MIT by interaction with the Electrical Engineering Department and through it with Bell Telephone Laboratories. The Bush Differential Analyzer, for example, engaged his attention and was an early prototype of the kind of development that would concern him in the forties and thereafter. His work on Fourier analysis, and especially that in the complex domain, provided a general mathematical theory that was clearly most relevant to theoretical network and filter design issues. The work of physiologist Walter B. Cannon, whose contributions revealed and concretely exemplified the importance of feedback and control in the biological context, and Wiener's joint work with Cannon's associate, Arturo Rosenblueth, would naturally have turned Wiener's thoughts toward a unified approach to these matters—and the related ones of memory and learning—in all types of time series, whether generated by physical or biological processes.

Indeed, the time was ripe for such a synthesis. Claude Shannon, who had gone from MIT to Bell Laboratories, had developed the information theory in the context of coding theory and cryptography. This important work provided a compelling illustration of cybernetic philosophy and, together with Wiener's work, served to establish information theory as a field in its own right. About a decade earlier, Alan Turing had developed an illuminating approach to constructive mathematical logic based on a computing machine tape analogy. Still earlier, some of the groundwork for a mathematical theory of information had been laid with the work of L. Szilard in statistical physics, that of R. A. Fisher on statistical estimation, and, in the context of electrical communication theory, work of Nyquist, Kupfmuller, and especially Hartley. Behavioristic psychology, developed by John B. Watson around the same time, provided a biological example of cybernetics, in addition to the theory of homeostasis already mentioned.

CYBERNETICS

Even before World War II, Wiener had begun to think in these directions, while at the same time developing his earlier mathematical work in Fourier analysis and Brownian motion. This work led to important mathematical papers, but when the war came he was quick to turn to applications along cybernetic lines, such as prediction theory and fire control. He never again returned to mathematical work at the intense and profoundly innovative level of his prewar contributions.

In the postwar period Wiener was especially interested in working along multidisciplinary lines encompassing all of physiology, psychology, communication engineering, and the like. He sponsored a seminar that included a number of the most active similarly minded scientists and engineers in the Boston area and covered a broad spectrum of questions, from theory to hardware. In particular, his postwar collaboration with the Mexican physiologist Arturo Rosenblueth led to a series of important papers in biology and medicine. In engineering this approach was exemplified in collaborations with Julian Bigelow and Y. W. Lee involving engineering development of ideas growing out of Wiener's mathematical theory.

Among others who on occasion attended the Wiener seminar were W. S. McCulloch, Walter Rosenblith, and Jerome Wiesner, as well as the brilliant but short-lived Walter Pitts. Wiener's *Cybernetics,* published in 1948, was in essence both a report on these multifaceted activities and a program. It made a synthesis of ideas and applications that had been set forth in a more limited and technical way in the previous decade. It proved highly stimulating in areas where these ideas had not yet penetrated and remains especially influential in fields involving the conjunction of biology and psychology with engineering and mathematical modeling. By 1948, however, related ideas had been advanced, in part quite independently, by a number of scientists in diverse fields, from Walter B. Cannon in physiology to Dennis Gabor in optical engineering.

Wiener's later works largely consolidate, amplify, and popularize his earlier relatively theoretical work. Probably the most important was his book *Nonlinear Problems in Random Theory,* which made the ideas of his theory of Brownian motion and the homogeneous chaos accessible to engineers concerned with time series. He continued his earlier mathematical collaborations with E. J. Akutowicz, P. Masani, and Aurel Wintner at somewhat reduced levels.

His last works increasingly emphasized the biological and social applications of cybernetics. Homeostasis, sensory prosthesis, and the mechanism of the brain were among his favorite themes. So also was moral philosophy. His final collapse took place, fittingly enough, in a speech laboratory, representing a conjunction of several of his scientific interests. He was then in the midst of a lecture tour in Scandinavia, accompanied by his wife —whose steady and understanding support had been of incalculable benefit to him.

In perspective, cybernetics as a field in its own right has receded as its ideas were gradually absorbed in more specific forms in particular fields. It remains a universal metaphor indicative of parallels and relations between a very broad range of scientific and engineering theory and applications. As a crystallization of positivistic attitudes and initiatives concerning temporally evolving systems, it represents a significant contribution to philosophy, Wiener's first love.

BROWNIAN MOTION IN PERSPECTIVE

Wiener's most original and influential work was his theory of Brownian motion, one of the most striking mathemati cal developments of the twentieth century, whose implications are still being actively investigated. The idea of installing a countably additive measure in function space brought together central currents in mathematical analysis at the same time that it provided a definitive model for an important physical phenomenon. In more recent years, functional integration has played a major part in quantum field theory—although in a quite heuristic form in the physical literature—as in the path integral formalism originated by Feynman. In any event, Wiener's work in the early twenties appears, with some hindsight, to foreshadow somewhat the important and influential formulation in rigorous mathematical terms of the theory of probability by Kolmogorov in 1933. This correspondingly appears in considerable part as a synthesis of Wiener's prototypical initiative with the abstract integration theory developed during the twenties. Moreover, Wiener space remains the key example for the theory of stochastic processes and its many applications, although the current approaches to the subject are much simpler and more powerful than the original ones.

In its original and perhaps simplest form, Wiener space consists of the space $C_0[0,1]$ of all real continuous functions on the interval [0,1] that vanish at 0. The variable continuous function (or path) in this space, which will be denoted W for brevity, is usually denoted as $x(t)$ in connection with Wiener's theory. To begin with, Wiener measure is defined on all subsets of W that are obtainable by restricting the values $x(t_1), x(t_2), \ldots, x(t)$ at a finite number of arbitrarily given times to lie in a given region in n-dimensional space. It is uniquely determined by the assumptions, which were implicit in heuristic physical theory, that (1) the joint distribution of $x(t_1), x(t_2), \ldots x(t_n)$ is normal (it is assumed the measure of all W is 1, so that the language of probability theory is applicable); and (2) if

$s < t < u$, then $x(u)$-$x(t)$ has vanishing mean and variance u-t and is stochastically independent of $x(s)$. What was new here was the idea of treating the entire trajectory as a point of a measure space, and, to a lesser extent, the demonstration, which was essential for the applicability of the Daniell integral, that the measure was countably additive. General theory then permitted the unique extension of the measure to a much wider class of subsets of W so as to remain countably additive, and validated all major features of Lebesgue integration theory. In particular, it became meaningful to ask the probability that a function selected at random from W was in a specified function class, e.g., continuous or differentiable.

Wiener's proof that the probability that a Brownian motion trajectory modeled in terms of W would be differentiable was 0 could not have dovetailed more closely with the qualitatively observed character of Brownian motion. To quote from Wiener:

> The Brownian motion was nothing new as an object of study by physicists. There were fundamental papers by Einstein and Smoluchowski that covered it, but whereas these papers concerned what was happening to any given particle at a specific time, or the long-time statistics of many particles, they did not concern themselves with the mathematical properties of the curve followed by a single particle.
>
> Here the literature was very scant, but it did include a telling comment by the French physicist Perrin in his book *Les Atomes,* where he said in effect that the very irregular curves followed by particles in the Brownian motion led one to think of the supposed continuous non-differentiable curves of the mathematicians. He called the motion continuous because the particles never jump over a gap, and non-differentiable because at no time do they seem to have a well-defined direction of movement.

It is interesting that when first established, such phenomena as continuous, nondifferentiable functions were regarded by Poincaré and some other physically oriented mathematicians as irrelevant pathology of negligible import.

The theory may appear at first glance to be somewhat special, being, e.g., nonrelativistic and involving only the elementary differential operator *d/dt.* However, it can be adapted so as to be invariant with respect to any given group, which may be operative on an *n*-dimensional manifold, and in further modified form, with *d/dt* replaced by an arbitrary quasi-elliptic differential operator. In some ways the theory is most cogently and invariantly formulated as the theory of a Gaussian probability in Hilbert space, the connection with Wiener's formulation deriving from the use of his stochastic integral.

More specifically, the nondifferentiability of $x(t)$ with probability 1 meant that formal expressions such as $\int f(t)dx(t)$ where $f(t)$ is a given smooth function on the interval [0,1], had no *a priori* meaning in terms of conventional definitions of the integral. This led to Wiener's introduction of the simplest prototype of the "stochastic integral," which defined $\int f(t)dx(t)$ as a random variable, or measurable function, on *W*. Such integrals arise in generalized form in the modern theory of stochastic differential equations, which has been principally developed by K. Ito. In intuitive terms, the Wiener process $x(t)$ is the solution of the "stochastic" differential equation dx/dt = white noise; this serves indirectly to give precise mathematical meaning to the latter concept, which intuitively represents the resultant of uncorrelated random Gaussian effects in time or space.

Two decades would pass before Wiener's disciples R. H. Cameron and W. T. Martin would report that Wiener measure had strange absolute continuity or differentiability properties. They presented an analog to the theorem of Plancherel, applicable to the space $L_2(W)$ of all square-integrable functions on Wiener space, but it too had a strange appearance.

The puzzling features of analysis in W were to be clarified a decade later by work originating in the mathemati cal theory of quantum fields. In purely mathematical terms, this showed that in essence analysis in W could more invariantly and effectively be regarded as a disguised version of analysis in Hilbert space H, which in the case of a finite-dimensional Hilbert space was equivalent to conventional harmonic analysis of square-integrable functions in euclidean space. The invariant Gaussian measure g on a real euclidean space H is *not* countably additive when H is infinite dimensional, i.e., a Hilbert space. Nevertheless, it makes correspondence to any polynomial $p(x)$ defined on H (i.e., the usual type of polynomial applied to a finite number of coordinates in H) a linear functional $E(p) = \int f(x)dg(x)$ defined by reduction to integration over these coordinates. This functional is positive on positive polynomials and has restricted growth properties. Algebraic theory of the Stone-Gelfand type then shows that there exists a Daniell-Lebesgue-type countably additive probability space Ω and an integral-preserving algebraic isomorphism between the algebra P of all polynomials on H and an algebra of random variables that is dense in $L_2(\Omega)$. This legitimizes the application of abstract Lebesgue integration theory, and in particular the Lebesgue spaces $L_p(H,g)$ are well defined, by a process of completion of P. The connection with analysis in W is now that $L_2(W)$ is equivalent to $L_2(H,g)$ with H taken as $L_2[0,1]$, via a transformation that is uniquely determined by the property that it carries the Wiener stochastic integral $\int f(t)dx(t)$ into the linear functional $p(h) = \langle h,f \rangle \equiv \int f(t)\, h(t)dt$.

The absolute continuity results of Cameron and Martin are subsumed in simple and invariant form by the result that the transformation on H, $y \rightarrow Tx + a$, where T is a continuous invertible linear transformation on H and a is a given vector, is absolutely continuous (i.e., has a bona fide Jacobian) *if and only if* T^*T-I is a Hilbert-Schmidt operator. The apparently strange form of the Fourier transform *F* in *W* becomes intelligible on computation of the effect on *F* of a transition from Lebesgue to Gaussian measure in euclidean *n*-space (by the unitary transformation consisting of multiplication by the square root of the ratio of the Lebesgue and Gaussian measure densities), followed by letting $n \to \infty$, and then making the transition from Gaussian measure in *H* to Wiener measure in $C_0[0,1]$.

These results, which I developed in the fifties, and which have been extended by L. Gross, show that in essence L_2 analysis in Wiener space can be regarded as the infinite-dimensional form of conventional L_2 harmonic analysis. Gaussian measure is invariant under the orthogonal group $O(H)$ on *H*. This implies that for any orthogonal transformation *T* on *H,* there is a corresponding measure-preserving transformation $\Gamma(T)$ on the measure space corresponding to (*H,g*), a correspondence emphasized by Wiener in special cases in the form of an action on *W.* For example, if Brownian motion is considered on the entire real line instead of the interval [0,1], as is no essential change, then *W* becomes invariant under the action of the translations: $x(t) \to x(t + s)$. The spectrum of this "flow" (i.e., one-parameter group of measure-preserving transformations), by which is meant the spectrum of corresponding one-parameter unitary group on $L_2(W)$, was determined by H. Anzai and S. Kakutani, starting from the homogeneous chaos work of Wiener, exemplifying a relation to ergodic theory.

Ultimately it was seen that not only *orthogonal* transformations on the space of *real* square-integrable functions but also *unitary* transformations on the corresponding *complex* space act via an extension of Γ, which leaves invariant the homogeneous chaos decomposition of $L_2(H)$ This complex extension can be correlated with the theory of Bose-Einstein quantum fields in such a way that the invariant subspaces in question of $L_2(H)$ become just the so-called *n*-particle subspaces. However, it was not until more than a decade after Wiener's work that quantum field theory was subsumable under a clear mathematical theory, especially in its wave aspects, to which the Wiener space formulation corresponded. A heuristic treatment of particle aspects had been given by V. Fock in 1932. The mathematical formulation in the dissertation of J. M. Cook and its equivalence with the "wave" representation (which, e.g., diagonalized the quantum fields at fixed times) has been fundamental in the mathematical theory of quantum fields and especially in work on the construction of nonlinear fields.

Wiener thus intuited a major feature of Bose-Einstein quantization apparently without the stimulus of the physics of the production of particles, which he did not treat. He considered at length the mathematical treatment of light, particularly in his *Generalized Harmonic Analysis,* but stopped short of the quantized radiation field, which had earlier been introduced by Dirac to explain the emission and absorption of light. The path-space formulation of quantum theory proposed by R. P. Feynman around 1950 is, however, formally closely related to Wiener space. The heuristic Feynman integral was connected with the Wiener integral by R. H. Cameron and by M. Kac. In a more advanced form, this work underlies the influential "euclidean" two-dimensional model for quantum field theory established principally by E. Nelson.

HARMONIC ANALYSIS IN PERSPECTIVE

It was primarily the symmetry of temporal homogeneity that underlay Wiener's central work in stochastic and harmonic analysis, as well as his strong interest in ergodic theory, which connected with both subjects. The original Tauberian theory of Hardy and Littlewood, largely directed toward applications in analytic number theory, has no such connection. Wiener's work in developing it foreshadowed, as he was aware, the generalization of harmonic analysis to a wide class of commutative groups, of which the groups of temporal and spatial displacements that he treated were quite special cases. He and Paley were among the first to note possibilities for such generalization, but in line with his general attitude, he found it more interesting to treat more directly applicable and less abstract matters.

The development of the theory of harmonic analysis on general locally compact commutative groups, basically complete by the mid-forties, confirmed this insight. In a way the analog of the Plancherel theory on Wiener space, which was connected with Wiener's Gaussian approach to finite-dimensional Plancherel theory, could be construed as a generalization of harmonic analysis to a different group. There is no direct analog to Lebesgue measure in a Hilbert space, but as indicated above, there is an analog to Gaussian measure. Although vector displacements do not leave this measure invariant, they change it in a simple way, and the germ of harmonic analysis based on this measure can be extracted from Wiener's treatment in Gaussian terms of harmonic analysis on the real line in his book on the Fourier integral. Thus from an abstract mathematical point of view, both of Wiener's central areas of research—integration in function space and harmonic analysis on the line—can be regarded as prototypical instances of analysis on commutative groups, of two extreme types. Moreover, this point of view has even technical cogency, as shown by the refinement of classical inequalities on the line by using ideas from analysis in Hilbert space developed by L. Gross.

Wiener's work in harmonic analysis derives from his early imprinting in the Hardy-Littlewood school of "hard" analysis, although its ultimate effect was to "soften" it consider ably. In essence it dealt with L_1 harmonic analysis on the real line *R,* in contrast to L_2 harmonic analysis in infinitely many dimensions, or analysis on Wiener space. L_2 , the space of all square-integrable functions, is simple in that it is invariant under Fourier transformation, but L_1 is not; this is the nub of the difficulty for Wiener's prototypical result on the invertibility of an absolutely convergent Fourier series that nowhere vanishes. On the other hand, the convolution of two functions in L_1 is again in L_1, which is not the case for the functions in L_2. Thus $L_1(R)$ forms an algebra, essentially a subalgebra of the convolution algebra *A* of all countably additive complex-valued measures on *R.* The basic results of Wiener's *Tauberian Theorems,* and of later collaborations with Cameron and Pitt, are to the effect that an element *a* of *A* has an inverse (in *A*) provided its Fourier-Stieltjes transform vanishes nowhere (an obviously necessary condition) and if, in addition, the continuous singular component of *a* is small relative to the other components. In more classical terms, this amounts to the solubility of an integral equation of convolution type.

Wiener gave an alternative formulation of this result as it applied to L_1. If $f(x)$ is a given element of L_1, then the finite linear combinations of its translates $f(x + c)$, where *c* ranges over all of *R,* are dense in L_1 if and only if the Fourier transform off is nowhere vanishing. Wiener showed the same was true for L_2 if "nowhere vanishing"—which was meaningless for L_2 functions, since their Fourier transforms are ambiguous on sets of measure zero—was changed to "non-vanishing almost everywhere." He raised the question, which became known as "Wiener's conjecture," of whether the L_2 result also applied to *Lp* for other values of *p*. This was a natural direction of refinement of his Tauberian theory paper, but a decade later I showed that the conjecture was false for $1 < p < 2$, although essentially trivially true for $p > 2$.

Somewhat later, A. Beurling gave an necessary and sufficient condition for the translates of a given function f to span *Lp* in the sense indicated, provided *f* was in all the L_q spaces for $1 < q < \infty$. Shortly afterward he reduced the Riemann hypothesis to a plausible question of spanning in L_p. This remains unresolved, but the reduction confirms the potential of the direction that Wiener established for dealing with the issues in analytic number theory with which Hardy and Littlewood were most deeply concerned, and in which Wiener had a lifelong interest.

Ultimately the treatment of Tauberian theory became quite algebraic, a development foreshadowed by aspects of the work of Wiener and Beurling, but completed by the Gelfand school, myself, and others. With the realization that L_1 was a natural generalization of the group algebra of a group to the case in which the group *G* was the additive group of *R,* general locally compact groups (essentially only such admitting well-defined L_1 spaces) were studied along similar lines. The analogs of the results of Wiener and his colleagues were valid for arbitrary abelian or compact groups, but not in general.

Another algebraic direction derived from the reformulation of Wiener's basic theorem in terms of ideal theory: The closed ideal generated by a given function *f* in L_1 (as a convolution algebra) is all of L_1 if and only if the Fourier transform of *f* is nowhere vanishing. For an arbitrary function *f* in L_1, every function in the closed ideal it generates evidently vanishes wherever the Fourier transform of *f* does; but are all such functions in this ideal? Alternatively, one may ask whether the finite linear combinations of the $f(x + c)$ are dense in the subset of L_1 consisting of functions whose Fourier transforms vanish where that of *f* does—the "spectral synthesis" question. Algebraic methods show that vanishing of the Fourier transform on an open set including the zeros of the Fourier transform of *f* is sufficient, and from this it is deducible that if this zero set is sufficiently simple in structure then the indicated question has an affirmative answer. But in general the answer is negative, and no explicit necessary and sufficient condition for a given set of zeroes to imply spectral synthesis for functions whose Fourier transforms have such zeroes is presently known.

Wiener's responsiveness to colleagues and scientific trends was in conjunction with his versatility the source of many shorter papers in a variety of areas not yet mentioned. Some of these include gems of insight that are still cited, such as his early work in logic. But he will probably be remembered chiefly for his work in functional integration and real and complex harmonic analysis, and aspects of ergodic and potential theory, on the pure mathematical side, and for cybernetics and rigorous excursions into statistical mechanics and the theory of light on the applied side.

PERSONAL AND SOCIAL LIFE

Wiener was at the opposite end of the spectrum from ivory tower scientists or academic philosophers. All his life he remained an intellectual whose vocation and responsibility was to contribute to civilization and society as a whole. He felt that it was his bent and duty to remain an individualist who stood apart from institutional establishments, but he faced the world steadfastly. He was elected to the National Academy of Sciences in 1933, but resigned a decade later, giving as his primary reason his opposition to prizes, special honors, and exclusivity in science. In *Ex-Prodigy* he wrote:

> . . . my early rejection by Phi Beta Kappa [while an undergraduate at Tufts College] has strengthened me in a policy on the basis of which I have resigned from the National Academy of Sciences and have discouraged my

> friends in attempts to obtain for me similar honors elsewhere. . . . My reaction is essentially the same at the present day as it has been for nearly forty years—that academic honors are essentially bad, and that other things being equal, I choose to avoid them.

His reasons for resigning from the Academy are amplified in his letter of September 22, 1941, to Dr. Frank B. Jewett, then president of the Academy:

> The academy operates in at least three distinct roles, and to my mind these roles are not compatible with one another....
>
> As to the third purpose of the Society—the conveying of honors—I have no sympathy at all. I have always regarded exclusiveness as an attribute chiefly of use in selling unwanted junk to parvenus. I do not wish to belong to any scientific organization which has more than one grade of membership . . . .
>
> As to medals, prizes, and the like, the less said of them the better. The heartbreak to the unsuccessful competitors is only equalled by the injury which their receipt can wreak on a weak or vain personality, or the irony of their reception by an aging scholar long after all good which they can do is gone. I say, justly or unjustly administered, they are an abomination, and should be abolished without exception. With these convictions I can only resign from the National Academy of Sciences and rectify the error, committed under the well-meaning appeals of my friends, which I committed in accepting membership in it. . . .

President Jewett's reply, dated September 24, is as follows:

> While I still feel you are making a mistake and that you can render better service by staying inside the Academy and using your influence to make it conform more nearly to what you think it should be, I realize that you alone must judge your desires.
>
> I am sorry I have not been able to dig up a problem which would show you the value I see in a body like the Academy, even though it is not all I myself should like to have it. However, one cannot always produce white rabbits out of a hat on demand.
>
> Whatever your final decision, believe me to be your friend.

On October 14, 1941, the Council of the Academy met and later telegraphed Wiener: "Resignation accepted with regret."

Wiener also thought twice about accepting the National Medal of Science, which he received two months before his death, worrying that it would erode the independence and consistency of his position. His persistent uneasy relations to authority in one form or another tend to bring to mind the complex and exceptionally close relations with his father, detailed in his autobiographical volumes. In the introduction to the first volume he wrote:

> There is a great temptation to write an autobiography in the Freudian jargon, more especially when a large part of it is devoted to the very Freudian theme of a father and son conflict. Nevertheless I shall avoid the use of this terminology. . . . Yet I cannot deny that Freud has turned over the stone of the human mind and shown a great population of pale and emotionally photophobic creatures scuttling back into their holes.

Those who knew Wiener could hardly help but be struck by the applicability of his description of his father to himself, both as regards temperament and general intellectual tendencies, and he himself wrote that ". . . my father, . . . notwithstanding all the elements of conflict between us, was my ideal and closest mentor."

All his life Wiener remained essentially a youthful figure, fraternal rather than paternal in his interactions, in *de facto* respects, apart from his scientific seminality. He was reactive rather than judicial, instructive rather than accommodating, deeply devoted to the highest ideals of scientific detachment and truth at the same time that he was personally quite concerned about the relative quality of his achievements, future as well as past.

Wiener would on occasion become absorbed in intricate questions of technique, as shown for example by the counterexample he developed with Pitt to the invertibility of an arbitrary nonvanishing absolutely convergent Laplace-Stieltjes transform. But this was not his main concern or forte. He preferred the challenge of a qualitatively new issue, or the synthesis of new relations between existing developments to technical perfection and organization—as, he wrote, had his father also.

Moreover, he preferred a concrete incision to an abstract envelopment, other things being equal, even though his true calling was ideational rather than technical. An instance of this was his anticipation independently of Banach of the concept and some of the theory of complete linear normed vector spaces. Following his initial paper, he seemed content to leave it to others to develop this subject, preferring more structured and applicable, if no less penetrating, areas of research. Another instance was his apparent disinterest in the algebraic methods that developed partly from his work in harmonic analysis.

The algebraic approach, initiated by M. H. Stone in the United States, greatly advanced by Gelfand and his school in the Soviet Union, and applied by myself and others to harmonic analysis and general groups, enormously simplified and greatly extended Wiener's work on the invertibility of absolutely convergent Fourier-Stieltjes transforms, including the basic Tauberian theorem. Wiener's work was catalytic in the development of this approach, and Wiener was apparently satisfied to have acted as such and was not deflected from ongoing research.

Wiener's drive, flexibility, breadth, and vision made it possible for him to make significant original contributions in subjects that he came to largely *en passant.* On the other hand, no one of Wiener's scientific range could be an expert on all of the subjects in which he took an interest. His work on relativity, quantum theory, light, and statistical mechanics for the most part display topical imagination more than mature scholarship. But some of this work, such as his theory of the coherency of light, has been quite significant.

His reactive and fraternal nature facilitated his personal scientific interactions, and his ideas often developed from groundwork and salients by colleagues developed shortly before he appeared on the scene. It was his unique capacity to sense the potential importance of such salients when appropriately grouped together, and to quickly envision and develop a synthesis that only in retrospect can be seen to carry earlier developments to a logical conclusion.

His autobiographical books, *Ex-Prodigy* (1953) and *I Am a Mathematician* (1956), largely mark the end of the innovative phase of his scientific career, apart from his continuing work on prediction theory and some unsystematic excursions. His main motive in writing his unusually personal yet philosophical autobiographical books was that

> I wish to think out to myself what my career has meant and to come to that emotional peace which only a thorough consideration and understanding of one's past bring.

Psychosocially, Wiener was *sui generis.* His personality reflected passionate individualism, a broad and active involvement in society and civilization, and a restless intellectual drive. He was a colorful figure and wit, and he became the center of a large accumulation of anecdotes, which he rather enjoyed. An example, which may be well known because it occurred on more than one occasion, was that of his meeting a colleague midway between his office, where he worked regularly even after retirement, and the faculty club, where he took lunch. On disengagement following an intense conversation, Wiener turned back and said to his colleague, "By the way, which way was I going when I met you?" "Why, that way," said the puzzled colleague. "Oh good," Wiener replied, "in that case, I've already had lunch!"

ACADEMIC CAREER

Wiener was a generally extroverted man with many friends, and, for the most part, enjoyed exceptional scientific respect. Still, his independence from conventional disciplinary and other categories and especially outspoken perceptions, insights, and devotion to principle as he saw it fostered reservations and misunderstandings on occasion. Many awards and honors came to him, but he received relatively few invitations to outstanding academic positions such as his early work amply merited and that would have facilitated his research and increased his influence. Vestigial antisemitism may also have played some part in this, as he believed.

Among the prizes he did receive were the Bowdoin Prize (1914) from the Harvard graduate school; the Bocher Prize (1933) of the American Mathematical Society, for outstanding research in analysis, jointly with Marston Morse; the Lord and Taylor American Design Award (1949); and the ASTME Research Medal (1946). He received honorary Sc.D. degrees from Tufts University (1946), the University of Mexico (1951), and Grinnell College (1957). He was one of the first to be awarded the National Medal of Science. On this occasion, January 13, 1964, President Lyndon B. Johnson made the following citation:

> For marvelously versatile contributions, profoundly original, ranging within pure and applied mathematics and penetrating boldly into the engineering and biological sciences.

Following his death, President Julius A. Stratton of the Massachusetts Institute of Technology wrote of him:

> One of the world's great mathematicians, he was also one of MIT's most distinguished professors. During his forty-five years of association with this

> faculty he was a symbol of fine scholarship, and indeed of the highest goals of MIT. We respected him not alone for his productive and creative mind but equally for his warmth of understanding and for his humanity.

EPILOGUE

Wiener vitalized analysis, the branch of mathematics that primarily originates in external issues, at the medium level of abstraction that held together the concrete questions that flow into it from outside with an inner, concentrated logic of its development. He was conscious of this role, and while appreciative of the trend of emphasis on internal issues (topology and algebra) in American mathematics that he largely ascribed to Oswald Veblen, expressed concern that it had gone too far, not only for relevance outside of pure mathematics but for optimal growth of this subject itself. In 1938, at the height of his pure mathematical achievements, he expressed himself as follows:

> It is a falsification of the history of mathematics to represent pure mathematics as a self-contained science drawing inspiration from itself alone and morally taking in its own washing. Even the most abstract ideas of the present time have something of a physical history. It is quite a tenable point of view to urge this even in such fields as that of the calculus of assemblages, whose exponents, Cantor and Zermelo, have been deeply interested in problems of statistical mechanics. Not even the influence of this theory on the theory of integration, and indirectly on the theory of Fourier series, is entirely foreign to physics. The somewhat snobbish point of view of the purely abstract mathematician would draw but little support from mathematical history.

After the distraction of the war, during which he developed as his major contribution to the war effort his treatment of stationary Gaussian processes, he preferred to pursue potential applications of mathematics to a variety of fields in which he felt a special challenge and interest. Further systematic pure mathematical work along the lines he had initiated he was largely content to leave to his disciples and others. In his later work, modeling life as a system, he showed how the concepts of feedback, smoothing, spectrum, and the like that were familiar in engineering and physical systems are relevant to biology and social science.

His influence reinforced and anticipated many trends in science and technology that were organized by him under the rubric of cybernetics. He was as an active proponent of the development of large-scale, high-speed computers long before the need for and potential of them was broadly recognized. Computer modeling of the brain and artificial intelligence developed in substantial part from his influence, applied within collaborative groups in the Boston area. As a scientifically charismatic figure with a considerable literary flair and, above all, a remarkable capacity for relevant theoretical innovation and synthesis, he may have been unsurpassed in his impact on the general scientific scene of his day.

His scientific career and personality were unique. Yet his works stand overall as an outstanding model in this century for a life of synthesis of pure intellectual penetration with external relevance.

I thank J. L. Doob, P. Elias, W. T. Martin, B. McMillan, W. A. Rosenblith, and M. H. Stone for valuable communications about aspects of Wiener's works and life. This biography was based in part on biographical data prepared by Wiener for the National Academy of Sciences and supplied by the Office of the Home Secretary. Thanks are also due the MIT Archives, and especially to Ms. Kathleen Marquis for the provision of copies of Wiener's letters and other material. I thank the MIT Museum for Wiener's photograph and background information.

Wiener's collected works, exclusive of his books, have been published with commentaries by MIT Press, Cambridge, Massachusetts, edited by P. Masani, in four volumes, 1976-85. A special issue of the *Bulletin of the American Mathematical Society,* vol. 72, no. 1, part II (1966), was dedicated to Wiener and includes reviews of his works, organized by field.

CHRONOLOGICAL SUMMARY

1894	Born November 26 in Columbia, Missouri
1906	H.S. diploma, Ayer High School, Ayer, Massachusetts
1909	B.A. *cum laude* in mathematics, Tufts College
1909-10	Harvard University
1910-11	Cornell University
1911-13	Harvard University—M.A., 1912; Ph.D., 1913
1913-14	Travelling Fellow, Harvard University; study with Bertrand Russell, Cambridge, England, and with David Hilbert, Göttingen, Germany; awarded Bowdoin Prize (1914)
1914-15	Travelling Fellow, Harvard; study with Bertrand Russell and G. H. Hardy, Cambridge, England, and at Columbia University
1915-16	Harvard University, Docent Lecturer, Department of Philosophy
1916-17	University of Maine, Instructor in Mathematics
1917-18	General Electric Corporation, Lynn, Massachusetts
1918	Staff Writer, *Encyclopedia Americana,* Albany, New York
1918-19	U.S. Army Aberdeen Proving Ground, Maryland
1919	Editorial Writer, *Boston Herald*
1919-24	MIT, Instructor in Mathematics
1925-29	MIT, Assistant Professor of Mathematics
1929-32	MIT, Associate Professor of Mathematics; Bocher Prize, American Mathematical Society (1933)
1932-59	MIT, Professor of Mathematics; Lord and Taylor American Design Award (1949); Hon. Sc.D., Tufts College (1946), University of Mexico (1951), and Grinnell College (1957)

1959-60	MIT, Institute Professor; ASTME Research Medal (1960)
1960-64	MIT, Institute Professor Emeritus; National Medal of Science (1963)
1964	Died in Stockholm, Sweden, March 18

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DOCTORAL STUDENTS OF NORBERT WIENER

1930	Shikao Ikehara	Ph.D.
	Sebastian Littauer	Sc.D.
	Dorothy W. Weeks	Ph.D.
1933	James G. Estes	Ph.D.
1935	Norman Levinson	Sc.D.
	Henry Malin	Ph.D.
1936	Bernard Friedman	Ph.D.
1939	Brockway McMillan	Ph.D.
1940	Abe M. Gelbart	Ph.D.
1959	Donald G. Brennan	Ph.D.

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Perry W. Wilson

Perry William Wilson

November 25, 1902-August 17, 1981

by Robert H. Burris

Perry Wilson, more than any other individual, turned studies of biological nitrogen fixation from a descriptive to a quantitative and analytical emphasis. Nitrogen deficiency more frequently limits plant growth than does any other deficit except water. Certain procaryotic organisms can convert nitrogen from the atmosphere to a form that plants can use. Wilson's research laid the groundwork for the phenomenal increase in studies on the biochemistry, genetics, and physiology of biological nitrogen fixation, a process vital to maintenance of the nitrogen cycle on earth.

Perry William Wilson was born in Bonanza, Arkansas. The family moved from Bonanza to Oklahoma and thence to Terre Haute, Indiana, when Perry still was very young. The possessions and income of the family were modest.

Perry Wilson, in an autobiographical sketch introducing the 1972 *Annual Reviews of Microbiology* about "Training a Microbiologist," said,

> My thesis is that one's training comes from many sources, none of which should be overlooked or overemphasized. . . . . A widely held belief is that one's career often reflects early influences. . . . As the twig is inclined. . . . My own early training can hardly furnish a test case since I never attended a school long enough to become inclined toward anything. A member of a

> somewhat mobile family, I attended elementary schools in Arkansas, Oklahoma and Indiana, none for a period of more than two years. . . . The curriculum in such schools was based largely on the three Rs—I became proficient in only two—reading and arithmetic. . . . Biology consisted of courses in human physiology decorously taught from a text suitable for a mixed audience. . . . The text and illustrations were studiously neutral with respect to reproduction. However, such deficiencies mattered not at all; to a student body made up largely of farm children, the facts of life were a part of their daily experience. . . . My favorite course was arithmetic. . . . The problems were oriented toward the practical. . . . In a culture where a mortgage on crops and land was another fact of life, a great deal of attention was given to the calculation of partial payments on bank loans .... High school was different. Our family finally settled in Terre Haute, Indiana, and I had the unique experience of attending all four years in an excellent high school. . . . It was staffed by a group of well-trained, young, enthusiastic teachers; they inspired many of us to dream of such a career. . . . In my senior year I took chemistry, but the experience did not alter my plans for a career: attend the local teacher's college and become a professor of high school mathematics.

After completing his high school work in 1920, Perry Wilson received a college scholarship, but it was inadequate to cover expenses, so he took a job as lab boy at the Commercial Solvents Corporation (CSC) in Terre Haute. There had been a great demand during World War I for acetone for **use** in explosives and in "dope" for airplane wings, and Commercial Solvents had erected a butanol-acetone fermentation plant in Terre Haute. They used Chaim Weizmann's culture of *Clostridium acetobutylicum* which produced butanol, acetone and ethanol in the proportions 60:30:10. Although the demand for acetone slackened after the war, the demand for butanol expanded as a market was created for it in automobile lacquers, so the plant continued to operate.

Perry's first job at CSC was to collect samples periodically from the various tanks being used for production of inoculum for the 40,000-gallon fermentation tanks of corn mash. The distillation of solvents was done from copper pots, and Perry enjoyed keeping them bright. Mr. Bogin, the supervisor, took an interest in Perry, loaned him books, and taught him analytical methods.

After a period in the fermentation lab, Perry's career goals shifted away from teaching high school mathematics, so in the fall of 1922 he enrolled as a chemical engineering student at Rose Polytechnic Institute in Terre Haute. He continued to work weekends at CSC. The butanol fermentations often would become sluggish, and the yield of solvents would drop precipitously; the basis of the difficulty was baffling. Because of their backgrounds in microbiology and fermentation, E. B. Fred and I. L. Baldwin, members of the bacteriology staff at the University of Wisconsin, were invited by CSC to serve as consultants. They uncovered the fact that some of the difficulties with the fermentations arose from contamination with *Lactobacillus leichmannii.* The plant was a remodeled distillery and had a maze of pipes, valves, and dead ends that never were adequately heated by the steam used for sterilization. When the cul-de-sacs were eliminated in a new plant designed by industrial engineers, this problem with contamination was eliminated. However, some fermentations still became sluggish, and it was only later that the source of the problem was defined.

Wilson was offered a position as analytical chemist at the CSC plant, so he dropped out of school for a year to take the job. This gave him a chance to become acquainted with consultants to the company, as he was responsible for analyses of the experimental fermentations. The company sold fermentation residues as an animal feed, and Perry did the nitrogen analyses to establish that the protein content of the material met standards.

Perry studied for another year at Rose Polytechnic Institute and then returned to CSC in the lacquer research department. There he became involved further in analytical organic chemistry. In the fall of 1925 he was told that the company was going to establish some research fellowships at the University of Wisconsin under the direction of Professors Fred and Peterson. This interested him immediately because he had been indoctrinated on Wisconsin by three Wisconsinites on the research staff, and he was well acquainted with Fred and Peterson from their consulting work at the Terre Haute plant. As Perry still was an undergrad, Fred and Peterson did the necessary bending of the fellowship rules so he could complete his undergraduate work at the University of Wisconsin. So he transferred for the second semester in 1926 and was granted fifty credits for his work at Rose Polytechnic Institute. His record shows that he later was permitted to substitute organic chemistry for physiology, physical chemistry for animal husbandry, and math for an agriculture option. His agriculture course record shows that he took bacteriology, agricultural chemistry, agronomy, botany, chemistry, economics, English, German, and veterinary science. It is evident that he designed a program to give himself a solid background in basic sciences.

Although Perry's previous training had included little biology, the course in bacteriology by W. H. Wright captured his imagination, and he wondered how effectively he could integrate his bent for chemistry with a career in bacteriology. The broad treatment of bacteriology in E. B. Fred's soil bacteriology course convinced him that microbiology was broad enough to cover a whole spectrum of interests. His minor was biochemistry (agricultural chemistry at that time), and he took courses from Hart, Steenbock, Peterson, and Tottingham. For research, Wilson was assigned the task of identifying crystals recovered by Elizabeth McCoy from milk that had been fermented by butyric acid organisms. The crystals were calcium citrate, but when they determined the water of crystallization, they found that the literature value was incorrect. So in 1927 Peterson, Wilson, McCoy, and Fred published a paper, Perry's first, in the *Journal of the American Chemical Society* to correct the value.

In the summer, Perry returned to CSC and was assigned to the bacteriological research division. There he worked with D. A. Legg, who had diagnosed that the sluggish fermentations had been caused by a bacteriophage, then described as the d'Herelle phenomenon. A phage-resistant strain solved the difficulties.

Perry received his B.S. in 1928 and submitted an undergraduate thesis project, "Production of Acetylmethyl Carbinol by *Clostridium acetobutylicum."*

In September 1928, Perry Wilson returned to Madison to initiate his graduate studies. He continued work in bacteriology and agricultural chemistry under the direction of E. B. Fred and W. H. Peterson, and investigated the nitrogen metabolism of *Clostridium acetobutylicum,* the CSC organism. He intended to return to CSC the next year, but his career was altered at this point. In the spring of 1929, the Frasch Foundation, through the American Chemical Society, awarded $40,000 to the departments of Agricultural Chemistry and Bacteriology to do research over a period of five years on the biochemistry of microorganisms. Half the award was for investigating the biochemistry of symbiotic nitrogen fixation. Professors Fred and Peterson asked Perry, when he had finished his M.S. work in the summer, to shift to the Frasch grant and start working on biological nitrogen fixation. This would delay Perry's Ph.D. for a year but would carry an increased stipend. He reasoned that he could finish his Ph.D. and then go back to industrial fermentation, so he accepted. He neglected to take into account how an intriguing research problem and its many ramifications can keep one bound and occupied for life—he never returned to industrial fermentation.

Perry Wilson completed his M.S. degree in August 1929. He married Helen Evelyn Hansel on September 4, and they settled in Madison so Perry could complete his Ph.D. Theirs was a happy marriage. Perry was inherently a nervous and constantly active person, and it was up to Helen to try to keep things on an even keel. As Helen opined later, "Life with Perry was always exciting, sometimes a little hectic, but never dull."

Back in the laboratory in the fall of 1929, Perry started to learn about growing plants, as his research was to focus on fixation in leguminous plants. To keep occupied during this learning period, he wrote a theoretical paper on the energetics of heterotrophic bacteria with W. H. Peterson and published it in *Chemical Reviews*—his first review. Perry had taken an undergrad course with Warren Weaver on probability and had found it a thoroughly exciting experience. He also had taken a stimulating course on the mathematical foundations of statistics from Mark Ingraham, and this had prompted him to read the work of R. A. Fisher and others. He teamed up with Ethel Kullman, who was examining methods for counting the rhizobia, and they published a statistical inquiry into methods for estimating rhizobia.

With the plant methods under reasonable control, Perry Wilson launched into his Ph.D. studies on the relationship between the concentration of carbon dioxide and the fixation of nitrogen by alfalfa and clover. He completed his Ph.D. in 1932 and submitted a thesis on the biochemistry of nitrogen fixation by the legumes. It is apparent that he had lived up to the expectations of E. B. Fred, W. H. Peterson, and I. L. Baldwin, and after his Ph.D. was awarded, the

Bacteriology Department appointed him as an instructor in 1932, followed by advancement to assistant professor in 1934, associate professor in 1938, and professor in 1943.

The Chemistry Department had wanted a course to acquaint their students with biological science. Bacteriology was willing to accommodate, and Perry was assigned to develop a lecture and lab course that was designed primarily for senior chemistry majors. This course, which appeared under various numbers during forty years, was "Perry's baby." It was more demanding than the usual general course in bacteriology and attracted students who wanted a challenge. After some years, it was taken by most seniors in pharmacy as well as chemistry, and it drew many other students with good science backgrounds. It is interesting that Ed Tatum, future Nobel Laureate, early volunteered to aid in the lab to gain teaching experience. From time to time Perry also taught soil microbiology, bacterial physiology, history of bacteriology, and a course in writing scientific reports.

To back up a bit, biological nitrogen fixation had received considerable attention, because the importance of nitrogen as a major fertilizer element for plants had been recognized widely. Mixing a leguminous crop with a non-leguminous crop was practiced as a beneficial operation in the time of the Romans, but the basis of the benefit was not clear. Boussingault in the 1830s performed careful field experiments that convinced him that leguminous plants, such as peas, accumulated considerably more nitrogen than non-leguminous controls, and he suggested that the nitrogen was derived from air. Liebig, who was the leading organic chemist of the day, assailed the findings of Boussingault without bothering to do any experiments to check their validity. This voice of authority from Germany convinced many, but not all; the French continued to support Boussingault. Lawes, Gilbert, and Pugh in England attempted to resolve the issue with very carefully performed experiments, but their careful preparations destroyed the nitrogen-fixing bacteria upon which the legumes depend. Finally, in 1886, Hellriegel and Wilfarth in Germany reported convincing evidence that nodulated leguminous plants can utilize molecular nitrogen. The information was rather quickly reduced to agronomic practice, and in time it became an accepted practice to inoculate leguminous seeds with suitable root nodule bacteria at the time of planting, so that their roots would become properly infected to form nodules. The period 1886 to 1932 was marked by the isolation of root nodule bacteria (*Rhizobium* sp.) in pure culture, demonstration of their specificity for certain leguminous plants, study of the physiology of the organisms, investigation of the infection process, and attempts to get the rhizobia to fix nitrogen apart from the host plant.

The biochemistry of nitrogen fixation was largely neglected. Dean Burk had the idea that this would be a fascinating area of study, so as a postdoc in Meyerhofs lab, he launched studies on the free-living, aerobic nitrogen fixer *Azotobacter chroococcum.* He utilized manometric techniques to investigate the respiration of the organism and attempted to establish its response to changes in the pO_2 and pN_2. Burk stayed with these studies for about a decade and then shifted his research to an even more elusive subject, cancer. Although the constants reported by Burk were not very accurate, he established a new approach for studies of nitrogen fixation.

The unity of biochemistry was being stressed, and Perry was intrigued by the possibilities in developing the comparative biochemistry of nitrogen fixation. He chose the leguminous plant system rather than a free-living nitrogen fixer for investigation. Perhaps this was because the Frasch grant specified work with legumes. The complex symbi otic system is much more difficult to manipulate experimentally than an organisms such as *A. chroococcum,* but as Perry stated on the occasion of receiving the Pasteur Award in his lecture "Chance Favors the Prepared Mind,"

> The first piece of luck arose because we chose the wrong experimental material with which to make the study—the symbiotic system of leguminous plants and the root nodule bacteria. Today we realize that this system is far too complicated for an initial survey and that we should have used species of the free-living soil bacteria, either *Azotobacter or Clostridium.* But had we done so, we undoubtedly would have missed the significant observation that gave us the break we needed.

The break to which he referred was the discovery that hydrogen is a specific and competitive inhibitor of nitrogen fixation.

Red clover inoculated with *Rhizobium trifolii* was chosen as the experimental plant. The seeds were surface sterilized, and after being germinated aseptically they were inoculated and transferred to 9-liter Pyrex serum bottles containing sand with plant nutrient minus nitrogen. The cotton stoppers that had been in place for sterilization of the units were replaced with stopper assemblies that allowed evacuation and gas addition through cotton filters. Units were evacuated, and gases were added to desired pressures. An internal indicator showed when it was necessary to add carbon dioxide. Other gases were changed weekly, and plants were grown about six weeks before harvesting.

Although the technique of growing plants in closed containers under controlled gas atmospheres was time-consuming, it yielded interesting results, most of which were summarized by P. W. Wilson in his 1940 monograph "The Biochemistry of Symbiotic Nitrogen Fixation." In approximately a decade, Perry and his research group had defined the growth substance requirements of the rhizobia, studied the respiration of the organisms, investigated and disproved claims that aseptic germinating legume seeds fix nitrogen, investigated the carbohydrate-nitrogen relationship, established the effects of carbon dioxide and light intensity on fixation, checked the claims for excretion of nitrogen from legume roots, established the pN_2 function and the pO_2 function in nitrogen fixation, determined the Michaelis constant for nitrogen fixation in red clover, studied the associated growth of legumes and non-legumes, and reported on the energetics of nitrogen fixation. Perry's monograph summarized this research, and its publication became a milestone in biological nitrogen fixation and a worthy successor to the 1932 monograph of Fred, Baldwin, and McCoy entitled "The Root Nodule Bacteria and Leguminous Plants."

The decade of the thirties included a year when Perry Wilson did research abroad on a Guggenheim fellowship. In his letter supporting Perry's application for the Guggenheim fellowship, E. B. Fred stated, "Perry Wilson . . . possesses an unusual capacity for productive scholarship. . . . He is a clean-cut young man of sterling qualities. . . . In my opinion he is the most promising young man in the field of the biochemistry of microorganisms which we have ever had at Wisconsin."

In 1936, the Wilsons went to Cambridge, England, on the Guggenheim fellowship. There Perry worked with Marjory Stephenson's group to test whether hydrogenase was somehow associated with biological nitrogen fixation. His stay there also provided an opportunity to learn about enzymology and enzymological methods. Perry utilized manometric techniques in his studies, and upon his return to Madison acquired a Warburg respirometer unit for his lab. Cambridge was a hotbed of activity in enzymology, both in the biochemical laboratory and in the Molteno Institute where Keilin had his group. Hopkins, Dixon, the Piries, Dorothy Needham, Lwoff, David Green, Baldwin, D. D. Woods, and Perry's colleagues Stare and Baumann from Wisconsin all were there at the time. At the Second International Congress for Microbiology in London, he met Kluyver, Van Niel, Schoen, Stapp, Orla-Jensen, Krebs, and Virtanen. Perry read a paper of his own and one for Dean Burk at the meetings.

At the congress, Perry had an opportunity to discuss his research with A. I. Virtanen. He had not had any success in demonstrating hydrogenase in free-living rhizobia at Cambridge, so he thought he should explore bacteria in the root nodules. Virtanen's Helsinki lab was well equipped for plant work, so he accepted Virtanen's invitation, and he, Helen, and their two-year-old daughter, Gwenn, took a ship to Helsinki in October.

Virtanen had published observations on excretion of nitrogen from leguminous plants. He not only attributed significance to excretion in mixed cropping between legumes and non-legumes, but he reported the recovery of specific compounds among those excreted. The occurrence of aspartic acid among the excreted compounds was a cornerstone in Virtanen's hypothesis that hydroxylamine was the key intermediate in biological nitrogen fixation. Work in Perry's lab and other labs had never shown appreciable excretion, and as a result it had not been possible to verify Virtanen's results or to test his speculations. During his stay in Helsinki Perry didn't find hydrogenase in root nodules, but he did observe a meager level of excretion.

Upon Wilson's return to Madison, the excretion experiments were repeated and were negative. Perry had brought some of Virtanen's peas, barley (non-leguminous indicator plant), and rhizobia for inoculum, and he even had shipped some of Virtanen's sand. None of these helped. George Bond also had no success in getting excretion in Glasgow, Scotland, or in Madison, where he spent a year in Perry's lab. So the controversy on excretion and its significance continued, but Virtanen and Perry always remained on friendly personal terms. As Perry has stated, "Although . . . Virtanen and I disagreed in the public prints regarding the excretion data for the biochemical mechanism of nitrogen fixation, our differences of opinion never affected our warm friendship. We routinely exchanged data before publication, with perfect confidence that no advantage would be taken of the advance information."

When the Frasch grant expired in 1939, the foundation gave another year of support so Perry could write up his results and publish his monograph "The Biochemistry of Symbiotic Nitrogen Fixation." The data were masterfully presented in the monograph, and like the earlier book of Fred, Baldwin, and McCoy, it had an immediate and lasting impact on the field. The scientific literature is now so vast that one can read but a small fraction and assimilate even less. However, a skillfully presented and carefully reasoned treatment such as given in Wilson's monograph still can have a notable influence on a field of research.

After the Frasch grant, Perry's research was supported for years by the Rockefeller Foundation and the Wisconsin Alumni Research Foundation. Emphasis was shifted from leguminous plants to the more easily manipulated free-living nitrogen fixers. The lab group soon established that hydrogen was a specific, competitive inhibitor of nitrogen fixation in *Azotobacter vinelandii* as well as in the red clover studied earlier. *Azotobacter* also proved to have a hydrogenase.

I came to Perry's lab in 1935, took my Ph.D. in 1940, and spent a year during 1940-41 with Harold Urey, Rudolf

Schoenheimer, David Rittenberg, and Sam Trelease at Columbia University absorbing the fundamentals of using the stable isotope ^{15}N as a tracer. Back in Madison, Wahlin and Eppling in the Physics Department helped us build an isotope-ratio mass spectrometer so we could analyze ^{15}N. Perry's group adopted the technique, and we worked jointly until we had accumulated sufficient data to feel confident in supporting ammonia as the key intermediate in nitrogen fixation. The data were far more convincing than those put forward for hydroxylamine by Virtanen, and this marked the demise of hydroxylamine as a candidate for key intermediate and the rise of ammonia to a virtually unchallenged position.

Kamen and Gest in 1949 reported that *Rhodospirillum rubrum,* a photosynthesizing bacterium, fixes nitrogen. They came to Madison to check their observations with ^{15}N, brought active cultures, and within a day we had jointly verified that the organism fixed nitrogen. This was an interesting development, because *R. rubrum* had been a favorite organism for study, but its capability for fixing nitrogen had been missed. It was particularly embarrassing to researchers in the field of nitrogen fixation who had used *R. rubrum* for other types of investigations. Another interesting aspect of the discovery was that Kamen and Gest had been prompted to test the organism, because they had found *R. rubrum* had a hydrogenase, and Perry Wilson for years had been speculating that there must be an association between nitrogen fixation, hydrogenase, and hydrogen inhibition. Perry's group promptly demonstrated that the other types of photosynthetic bacteria fixed nitrogen.

Today, the research on biological nitrogen fixation has expanded greatly, and it is difficult for the work of one individual and his associates to have an impact comparable to that of Perry Wilson's. Perry always had an inquiring mind, and in the thirties he was curious about how the literature on nitrogen fixation had grown. So he investigated this point, and in 1935 he and Fred wrote a paper entitled "The Growth Curve of a Scientific Literature—Nitrogen Fixation by Plants." They found that the number of papers on biological nitrogen fixation had shown a modest growth until the 1886 report of Hellriegel and Wilfarth shifted it into an exponential phase that broke somewhat during World War I. Their extrapolation beyond 1935 was a woeful underestimate, for they stated, "In conclusion, it appears from the 'smoothed' data that the research student of the future can look forward to an annual production of approximately 100 publications a year in this field. This limit of production seems likely to occur about 1965 to 1970. Likewise, the total number of pages to be mastered each year will be from 1,500 to 1,600 before the harassed student may look for relief from an ever-increasing annual load." I counted the nitrogen fixation papers and their pages in the 1965 *Chemical Abstracts,* and in memory of E. B. Fred and P. W. Wilson I will not record the numbers here. Suffice it to say that these giants, who were authors of two of the most definitive books in the field, were highly perceptive individuals but exhibited no psychic insight into how overwhelming our scientific literature would become.

Perry Wilson directed a highly productive research group until the time in 1972 when he had a serious stroke while lecturing to a large bacteriology class. The stroke destroyed his speech and left partial paralysis on his right side. He recovered quite well and led a cheerful life. The tragedy was that the attack that occurred only about a year before his scheduled retirement destroyed his ability to write. Perry was an unusually talented writer. While most of us struggle with transferring thoughts to paper, he ap peared to do it effortlessly and with style. He had looked forward to retirement as a time for writing. Fortunately, he had an opportunity earlier as author of his 1940 monograph and his textbook with Werkman on bacterial physiology, as editor of other books, as editor of *Bacteriological Reviews,* and as author of many scientific papers to leave us the benefit of his skills.

Perry Wilson was a good academic citizen and participated in many functions at the University of Wisconsin. He enjoyed teaching and regularly gave a lecture and lab course in bacteriology for advanced chemistry and pharmacy students. He and C. A. Elvehjem led a seminar on respiratory enzymes, and in 1939 the two edited a book entitled *Respiratory Enzymes.* This seminar and book were the genesis of the Enzyme Institute at the University of Wisconsin, and Wilson and Elvehjem were instrumental in its founding. Later Perry did joint research with several investigators at the Enzyme Institute and took leave to work there one year. Wilson served on the National Research Council as representative from the American Society for Microbiology. He was elected to the National Academy of Sciences in 1956 and gained great satisfaction from this. His son recalls that on the day of election Perry left work early and picked him up at West High, and when Richie asked what the excitement was, Perry replied, "Richie, I've been elected an All American."

This comment emphasizes Perry's continuing interest in sports. He was not an athlete himself, but he was always informed about the performances of teams from the University of Wisconsin, the local high schools, and the Green Bay Packers. He vicariously enjoyed the triumphs of his son in cross-country competition.

When Perry was disabled in his later years, he received the loving care of his wife, Helen. They were married in

1929, raised two children, Gwenn and Richard, participated in many activities together, and had a wide circle of friends. Although the years after 1972 were difficult, Perry seemed to enjoy them thoroughly. The absence of earlier responsibilities appeared to give him a lift, and he delighted in seeing people and they enjoyed his company.

Perry Wilson was an unusually fine scientist. He had an excellent background in mathematics and an intuitive grasp of statistics. I have never been associated with a colleague with a better sense of experimental design. His research was well conceived, definitive, and interpreted with honesty and insight. Perry made very few errors in his reported investigations. His submitted papers must have been frustrating to editors looking for something to criticize and correct.

Research support was very limited during the thirties, and Perry Wilson was fortunate to have the Frasch Foundation grant. The combined grant to bacteriology and biochemistry was about $10,000 a year, and those of us who were grad students in Perry's lab were the envy of others in the department because of our affluence. As grad students we received excellent training from Perry, and there was good morale in the lab. Perry was not a hard taskmaster, and we didn't resent that fact that our journal discussion meetings were held in Perry's office at 10:00 a.m. on Sundays.

Perry's talents were recognized by his microbiological colleagues, and he not only served as editor of *Bacteriologal Reviews* but also was elected president of the American Society of Microbiology. It was during the period of his presidency that the organization founded the American Academy of Microbiology. Perry gained particular satisfaction from his role in the NSF-backed project to prepare a set of new high school textbooks in biology, as he always was interested in the development of youth. When he gave the lead lecture in 1969 at a symposium on nitrogen fixation before the Royal Society in London, he wrote Graduate School Dean Bock, "I was flattered to be introduced as the dean of biological nitrogen fixation." But that was how he was regarded in the field, and few would have challenged that characterization.

Perry Wilson had a humble background, but he was highly talented and used his opportunities effectively to establish a special place for himself in the areas of biological nitrogen fixation. He planned his research with imagination and created a solid base upon which he, his students, and current investigators have been able to build an enduring structure.

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Clark Wissler

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September 18, 1870-August 25, 1947

by Stanley A. Freed and Ruth S. Freed

Wissler lived his professional life when anthropological theory in the United States was dominated by the Boasian paradigm of historical particularism. Franz Boas (1858-1942) was chiefly concerned with studying particular cultures as distinctive units without comparing them. Wissler's theoretical ideas provided a basis for going beyond the bounds of Boasian anthropology and developing a nomothetic approach to ethnological data.

Wissler's major theoretical contributions are: 1) a noteworthy development of the concept of culture; 2) a detailed formulation, in which environmental factors are given prominence, of the nature and meaning of the culture area; 3) the age-area concept as a basis for inferential historical reconstruction; 4) the culture pattern; and 5) the universal pattern of culture, which encompasses ideas about the origin of culture, the psychological nature of mankind, and the relation of psychology and anthropology.

Wissler's contributions to the definition of culture have become standard, if largely unacknowledged. His concept of the culture area as a descriptive and classificatory device has been widely used, and the dynamic aspect of the concept was picked up thirty years later by Kroeber in his influential *Cultural and Natural Areas of Native North America.* [1] Wissler's ideas concerning culture and the environment have been assimilated into modern anthropological theory. Some of his influence has been largely unperceived, particularly concerning the culture pattern, later made famous by Ruth Benedict in *Patterns of Culture.* [2] His innovative and sophisticated concept of the universal pattern of culture was misunderstood and largely overlooked for decades after he proposed it, although its classificatory aspect was routinely used in ethnographies. However, analogues of the theory can be identified today in sociobiology.

Wissler's important contributions to ethnological theory account for only part of his formidable scientific reputation. He also did ethnographic fieldwork; directed research projects of major scope, both in ethnology and archeology; built up impressive museum collections; planned exhibitions; did psychological research and founded the first psychological laboratory at The Ohio State University in 1897; encouraged the early development of dendrochronology; and during much of his career was the principal organizational figure in American anthropology. His work was of considerable interest to geographers and psychologists as well as to anthropologists.

EARLY LIFE

The eldest of seven children of Benjamin F. Wissler and Sylvania (Needler) Wissler, Clark Wissler (christened Clarkson Davis Wissler) was born in a rural house near Cambridge City in Wayne County, Indiana, on September 18, 1870. His maternal relatives came from England in the early 1700s and settled in east-central Indiana soon after the American Revolution. His paternal ancestors reached the same general area in 1808, having migrated to Pennsylvania early in the 18th century from a part of Swabia, now in Switzerland.

Wissler's father was at various times a farm laborer, farmer, carpenter, school superintendent, and editor of a country newspaper. He served in the Civil War. Wissler's first two or three years of schooling were in his father's school. His father had only a little training above the elementary school level, but he spent much of his spare time studying one subject after another. Wissler recalls that at one time his father tried to make a naturalist of him, but he was too much interested in historical events, heroes, and antiquities to spend much time on birds and animals.

Wissler remembers his mother as a handsome woman with wonderful hair. She had little schooling but was quick, hard-working, sensible, moral, exacting, capable, and everything a mother should be. Every year Wissler made a pilgrimage to the little country churchyard where she was buried. Although his mother was not particularly religious, finally ceasing even to attend church, Wissler was brought to church in infancy and childhood. It made a strong impression on him, especially the revival meetings where some people temporarily entered altered states of consciousness, "shouting and tearing around." Wissler "steeled [himself] against yielding to the urge, in which I was encouraged by my parents. To this day I dislike mass emotionalism." Nevertheless, he closely observed the behavior of the chief actor and later when he "read about abnormal behavior and religious extravagance, it all sounded very real and natural."[3]

One of the strongest influences in Wissler's life was a neighboring farmer, a man of high intelligence but little schooling, who aroused Wissler's interest in anthropology. Wissler worked for him as a boy. An ancient Indian village site was located on his property. The farmer was interested in archeology and collected artifacts, and he encouraged Wissler to do likewise. On rainy days and Sundays

Wissler visited him. His neighbor would take one artifact after another, explain its use, and give it a technical name. "Little that I learned from him in this, my first museum, was afterward found incorrect."[4] From then on, Wissler always felt at home with a collection.

Wissler received his elementary education in Cambridge City. He graduated from nearby Hagerstown High School when he was seventeen years old. He taught in local rural schools for five years (1887-92), studying at Purdue University after the six-month school term ended. The next summer Wissler and a local school superintendent under whom he had worked opened a normal school for the training of teachers. Sixty students enrolled, giving Wissler "a real thrill, for I had a chance to teach on a higher level."[5] He spent a year as principal of Hagerstown High School (189293) and then, having made up his mind to follow an educational career, boldly resigned and entered Indiana University. He majored in experimental psychology, receiving his A.B. in 1897 and his A.M. in 1899. He spent one summer at Clark University working under G. Stanley Hall. During his last two undergraduate years, he held an assistantship and conducted experiments on individual differences in mental and muscular abilities. He combined his graduate study and research at Indiana University with teaching, serving as instructor in psychology and education at The Ohio State University from 1897 to 1899.

By this time, Wissler's university experiences had made him aware that his future depended on a higher degree. Also, he married Etta Viola Gebhart on June 14, 1899, and the responsibility of marriage was an additional incentive. The couple eventually had two children, a son, Stanley Gebhart Wissler, and a daughter, Mary Viola Wissler, who for many years was a librarian at the American Museum of Natural History.

Wissler was appointed assistant in psychology (1899-1900) at Columbia University in the same year that he received his A.M. from Indiana University. A year later, he became university fellow in psychology (1900-01). He continued his graduate studies under the psychologist James McKeen Cattell, receiving his Ph.D. in psychology in 1901. From 1901 to 1903, he pursued his laboratory research on individual mental and physical differences under Cattell and also served a year (1901-02) as instructor in pedagogy at New York University.

Wissler regarded his early psychological work as important and took pride in it. He was interested in the mathematical methods used in psychology and claimed to be the first to use Pearson's correlational formula in psychological testing. His psychological work clearly influenced his approach to anthropology, which is peppered with psychological insights, as in the concept of the universal pattern of culture, the culture pattern, and the culture area. Wissler "had a feeling that [anthropology and psychology] belonged together."[6] His background in psychology also influenced his later career, playing a part in his appointment to the Institute of Psychology at Yale.

Shortly after graduating from Columbia, Wissler abandoned psychology for anthropology. In later years, he was at some pains to explain his rather abrupt change of careers. He recalled his boyhood when he fell under the influence of his archeologically inclined neighbor and read everything that came to hand about Indians and antiquities. He mentioned that during his summer at Clark University he took a course from the anthropologist A. F. Chamberlain. At the University of Indiana, Wissler studied under William L. Bryan and George E. Fellows. Once Fellows took his class to Chicago to visit the University of Chicago and the Field Museum. They were given a talk by Professor Starr, an anthropologist, and saw the anthropological collection at the Field Museum. The visit made a deep impression on Wissler. During Wissler's last year at Columbia, he took three full-term courses in anthropology taught by Boas and Livingston Farrand. Although Wissler emphasizes the intellectual influence of his mentors, especially Fellows, practical considerations were probably decisive in his choice of careers. At the time, there were more openings for anthropologists than psychologists "owing to a burst of expeditionary activity at the American Museum. Wissler received opportunities there first for field work, and then of a curatorial position."[7]

PROFESSIONAL LIFE

Wissler joined the American Museum in 1902 as assistant in ethnology under Franz Boas. At that time, the Department of Anthropology had been reorganized into two departments: ethnology and archeology. In 1904, Wissler was advanced to assistant curator of ethnology; by 1905, when Boas resigned from the American Museum, Wissler was listed as acting curator of ethnology. In 1906, he was named curator of the Department of Ethnology, and in 1907, when archeology and ethnology were administratively recombined, curator of the Department of Anthropology, which rank he held until his retirement in 1942. In short, he was virtual head of the Department of Anthropology at the American Museum for thirty-seven years.

At the beginning of his career in anthropology at the American Museum, he also held a position at Columbia University. He was assistant (1903-05) and eventually lecturer (1905-09) in anthropology. However, he had a short teaching career, whether because of a grievous (unspecified) illness that he contracted in 1907 or because of unpleasantness with Boas that was the residue of Boas's resignation at the American Museum. Although there were short relapses, Wissler's health took a turn for the better about 1912. "With good medical care and the heroic efforts of my wife, I was able to hold my own....It has been a sore trial, but I accepted it as inevitable and tried to do the best I could. Really there is no just ground for complaint."[8]

After his health improved, Wissler wanted to do some lecturing, but an offer of an honorary appointment at Columbia came to nothing as the American Museum refused permission. However, in 1924, in the "greatest surprise" of his life, Yale University offered Wissler a research appointment in the newly founded Institute of Psychology.[9] Wissler's training and interest in psychology made him the logical anthropologist for the position. Within a few years, the Institute of Psychology was expanded into the Institute of Human Relations, and Wissler maintained an association with it until he left Yale. Wissler became professor of anthropology when the Department of Anthropology was established in 1931 under Edward Sapir. Harry L. Shapiro, Wissler's colleague at the American Museum, recounts that Wissler would go to New Haven Saturday mornings, lecture and meet with students during the day, and return to New York in the evening.[10] William N. Fenton, who was a student of Wissler, recalls that at least later in his affiliation with Yale, Wissler was at New Haven two days a week and that he had anthropology students before the Department of Anthropology was founded. His teaching in later years emphasized acculturation studies and ethnohistory. Fenton emphasizes Wissler's humanity, remembered by colleagues and former students, such as J. C. Ewers, W. W. Hill, F. M. Keesing, D. G. Mandelbaum, S. Mekeel, W. Z. Park, and F. Rainey, all of whom became prominent anthropologists.[11] Wissler became Professor Emeritus at Yale in 1940.

Wissler made his principal scientific and administrative contributions during the thirty-seven years that he was head of anthropology at the American Museum. His field research, lasting only from 1902 to 1905, concerned the Dakota, Gros Ventre, and especially the Blackfoot. He published eleven monographs based on his fieldwork. He believed that a fieldworker should attempt to provide as comprehensive an ethnography as possible, and his seven publications on the Blackfoot approximate that ideal. Thereafter, Wissler directed a number of other field projects on the northern plains which made that region the best known ethnographic area in the New World at the time. In addition, he sponsored both ethnological and archeological fieldwork in other areas of the New World, especially the southwestern United States, encouraged physical anthropology, built up collections of worldwide scope, planned exhibitions, and oversaw the publication of about thirty-eight volumes of the *Anthropological Papers of the American Museum of Natural History.* He himself published at least 365 titles, chiefly in ethnology, but including a few in pedagogy, psychology, physical anthropology, and archeology.

Wissler developed his major theoretical ideas against the background of his Plains Indian research. His first field trip, in 1902 to the Dakota, stimulated his interest in the concept of culture. Although from reading during his student days, he "began to sense what a culture was like....the real experience came when I was offered a summer field trip.... to the Dakota Indians. . . . I fell into the spirit of the culture [and] now had the feel of the phenomena. . . . Some of the finest human experiences came while engaged in this work: they have always remained the most treasured."[12]

Definition of Culture

Wissler's interest in culture led to attempts to expand and refine its definition. He was the first anthropologist to perceive the normative aspect of culture, to define it as learned behavior, and to describe it as a complex of ideas, all characteristics of culture that are today generally accepted. His background in psychology and pedagogy and his interest in statistics are apparent in his contribution to the definition of culture. A definition of culture as rigorous as possible was essential for a cross-cultural nomothetic approach to ethnological data. If aspects of culture are to be compared cross-culturally, culture itself must be defined.

Culture Area

Although the culture area, the best known of Wissler's concepts, was around long before Wissler, it was he who developed the full potential of the concept and advanced it to the point where it could be used analytically. In Wissler's hands, the culture area became more than just a geographical grouping of social units with similar cultures; it was a significant theory of culture change and, as he was well aware, offered an alternative to the Boasian style of anthropology. What the concept did was to shift analytical focus from the culture and history of the specific social unit to a concern with the trait-complex viewed in cross-cultural perspective. Although the usefulness of the culture-area concept has been very great for ethnology, its theoretical significance, especially for the period in which it was formulated, has been generally unappreciated.

The correspondence of a well-defined geographical area with a group of cultures that share many features is the basis of the concept of the culture area. The tribes of one culture area are prevented by physical barriers, cultural habits, and psychological characteristics from close relations with the tribes of other culture areas. The principal barriers that preserve the distinctiveness of a culture area are physical: surface, climate, fauna, and flora. Culture areas are internally dynamic. In each culture area there is a culture center where innovation takes place and from which cultural influences diffuse to the margins of the culture area. One result is a group of typical tribes and another of marginal tribes. Wissler's view of the relation of culture and environment with its emphasis on the culture center, environment as the medium of culture growth, the selection of a limited number of the possibilites offered by the habitat, and the stability derived from social habits, attracted favorable attention from human geographers.

Culture Pattern

Wissler proposed the concept of the pattern phenomenon, or tribal pattern, to explain the rejection or the incorporation and modification of diffused trait-complexes by receiving societies. Wissler alluded to the basic idea of the pattern phenomenon in 1912. Four years later, he substantially developed the concept in his paper on the shamanistic and dancing societies of the Plains Indians and adjacent tribes, dealing especially with the processes of incorporation and modification. The pattern phenomenon refers to the fact that in a tribe or region there are dominant concepts that retard diffusion or serve to modify borrowed trait-complexes. "The conception is that in certain phases of culture each social unit develops a style, or pattern, for its traits and that borrowed traits will be worked over to make them conform to this pattern."[13] Although the concept of the culture pattern, like the culture area, may have been in the air at the time that Wissler wrote, we have found little evidence that any other anthropologist had established clear priority. In any case, Wissler appears to have done with the culture pattern approximately what he did with the culture area: he developed the idea, examined its meaning, applied it to a large body of data, and incorporated it into a theory of culture change. Reviewing

Wissler's *General Discussion of Shamanistic and Dancing Societies* (1916), A. L. Kroeber observed, ". . studies such as this are uncovering principles of broad applicability—principles of a psychology that is truly social, and that. . . must ultimately be. . . recognized by every historian or analyst of human civilization."[14]

Age and Area

The culture-area concept, as a theory of culture change, offers the possibility of making historical inferences. In general, new traits repeatedly originate at the culture center and diffuse outwards to the margins of the culture area in more or less concentric circles. If traits tend to spread at generally similar speeds then there will be a relation between the geographical extent of traits and their age. A widely spread trait within a culture area will in general be older than a more localized one. In his *Introduction to Social Anthropology* (1929), Wissler referred to the relation of time and space as "age and area"; it is often called the age-area concept. Although the age-area concept was severely criticized, much of the criticism was misdirected. While noting that the concept must be used carefully, A. L. Kroeber wrote, "[Wissler] has done enough with the age-area concept to show that it is not a mere instrument of speculation but a legitimate means of inferential reconstruction when other data fail."[15]

Universal Pattern of Culture

The major importance of the universal pattern of culture at the time it was proposed was probably in the context of particularism versus comparison. The culture-area concept had furnished a historical basis for cross-cultural comparison; trait-complexes could be abstracted from cultural context and compared because they were historically connected. Historically unrelated traits can be compared because they are various manifestations of the universal pattern of culture. Although the universal pattern of culture lays a theoretical basis for the comparison of historically unrelated trait-complexes, it deals with many other matters as well, such as the origin of culture, its nature, its acquisition, and the relation of psychology and culture. It is a complex sophisticated theory, and sixty-five years after it was proposed in *Man and Culture* (1923), it still merits attention in any consideration of the relation of biology, psychology, and culture.

The concept of the universal pattern of culture distinguishes between culture content and pattern. Content consists of trait-complexes. Cultures differ concerning their inventories of trait-complexes, but, viewed as wholes, they are all seen to conform to a basic general plan or pattern. This plan, or outline, features nine principal subdivisions, called culture complexes, such as language, material traits, religion, and government. Each culture complex can be subdivided to the extent that one wishes, but the general scheme fits all cultures known to history.

The universal culture pattern is the essence of humanity, for culture appears only in the form of the universal pattern. By the phrase, human being, we usually mean a primate that possesses a culture conforming to this basic pattern. Thus, the universal pattern is deeply involved in what Wissler considered the fundamental problem in education, psychology, zoology, and anthropology, namely, the analysis of humanity's original biological equipment for culture. The capacity for culture, that is, for the universal pattern, is rooted in the genes. The universal pattern is an expression of inborn behavior, but the specific content is not so determined, depending instead on the nature of the environment and fortuitous events. Cultural evolution must proceed along the lines of the universal pattern of culture. Each culture complex can be enriched or made more complex, but it cannot be transcended. All cultures are predestined to keep within the bounds of the universal pattern. Yet the universal pattern has within it the potential for wonderful flexibility and tremendous expansion, qualities which, in Wissler's view, make it nature's masterpiece.

There is a general psychophysical basis for culture, namely, the making of responses and the ability to learn, which is essentially a conditioning of responses. Besides this general basis for culture, Wissler proposed more specific psychophysical functions (responses) that are related to various culture complexes. For example, the use of tools is based on the grasping response and the power of reflection; language, on the production of vocal sounds and reflection. Reflection (thought) is the conditioning of the cause-effect response. It is basic to all culture complexes. Wissler asserts that "The universal pattern for culture is then largely determined by the number and kind of these inborn responses. . . ." The details of the trait-complexes, in turn, "are largely variants in the conditioning of inborn responses."[16]

Archeology and Physical Anthropology

Wissler made noteworthy contributions to archeology through his long vigorous promotion of field research over much of the New World. Among the more noteworthy investigations that he backed was work in and near New York City by A. Skinner, L. Spier, M. R. Harrington, and others; prolonged surveys and excavations in the southwestern United States by L. Spier, A. L. Kroeber. N. C. Nelson, E. H. Morris, and G. H. Pepper; the excavations and explorations of R. Olsen, W. C. Bennett, and J. B. Bird in several regions of South America; the excavations of

G. Vaillant and G. F. Ekholm in Mexico; the Ipiutak (Alaska) excavations by H. Larsen, F. G. Rainey, and H. L. Shapiro; and V. Stefánsson's work in the Arctic. Physical anthropologists were active in the Department of Anthropology during Wissler's tenure as head, most notably L. R. Sullivan, H. L. Shapiro, W. K. Gregory, M. Hellman, F. Weidenreich, and Wissler himself.

The archeological work in the Southwest deserves special attention. Jonathan E. Reyman notes that "although the Southwest was neither his area of special interest or expertise, Wissler was responsible, to a significant extent, for the early prominence of the Southwest in American archeology."[17] Reyman points out that during Wissler's tenure as editor of the *Anthropological Papers of the American Museum of Natural History,* N. C. Nelson, A. L. Kroeber, and L. Spier published studies in that series from 1914 to 1919 that revolutionized American archeology. It is also noteworthy that Kroeber's monumental study of Zuñi social organization was published during the same period.[18] Other work of great importance in Southwestern archeology was carried out by Earl Morris at Aztec Ruin and George H. Pepper at Pueblo Bonito, the reports of which were published by the American Museum under Wissler's editorship.

Wissler was responsible for the application of dendrochronology to the problem of dating sites in the Southwest. He asked A. E. Douglass if it would be possible to apply dendrochronology to wood from archeological ruins. Douglass was enthusiastic and asked for some samples. Ultimately Douglass constructed a 250-year chronology of relative dates for Aztec Ruin and Pueblo Bonito and another separate chronology of 160 years for Chaco Canyon. Douglass could not cross-identify his chronologies with living trees and therefore could not establish calendar dates.

Clark Wissler died in New York City on August 25, 1947. He received little scholarly attention after his death owing, in all likelihood, to the fact that his principal base was in a museum where he did not have the chance to build a large following among students. However, in the last decade, his work has been reappraised and his contributions more properly evaluated. Two of the greats of American anthropology, A. L. Kroeber and C. Kluckhohn, credit Wissler with "an exploratory and pioneering mind," and he left an enduring legacy.[19] He was one of the three or four leading American cultural anthropologists of his time.

SOURCES AND ACKNOWLEDGMENTS

This memoir draws heavily on our article, S. Freed and R. Freed, "Clark Wissler and the Development of Anthropology in the United States," *American Anthropologist,* 85(4) (December 1983):800-25. The various obituaries listed in that article are useful, especially, G. P. Murdock, "Clark Wissler, 1870-1947," *American Anthropologist,* 50 (1948):292-304. The biographical note by William W. Speth, "Clark Wissler 1870-1947," *Geographers: Biobibliographical Studies,* 7 (1983):151-154, which was written for an audience of geographers, places Wissler's scientific ideas in the context of geographical thought. J. S. Reed's doctoral dissertation, "Clark Wissler: A Forgotten Influence in American Anthropology," Ball State University, 1980, is useful. The Department of Anthropology at Ball State University maintains the Clark Wissler papers. We thank W. N. Fenton, H. L. Shapiro, and L. Williamson for kindly reviewing and checking the manuscript, and D. D. Fowler for calling our attention to Wissler's role in the use of dendrochronology in archeology.

HONORS AND DISTINCTIONS 20

Honorary Degrees

Indiana University (LL.D., 1929)
Yale University (M.A., 1931)

Offices in Professional Societies

American Anthropological Association (vice-president, 1915, president, 1919-20)

American Association for Adult Education (vice president, 1937)

American Association for the Advancement of Science (vice president, 1914)

American Association of Museums (president, 1938-45)

American Museum of Natural History (dean of the council of the scientific staff, 1935-42)

Bernice P. Bishop Museum (consultant)

Carnegie Institution of Washington (advisor, research associate, 1924-33)

National Park Service (member of the Committee on the Study of Educational Problems in the National Parks, superceded by the National Park Service Educational Advisory Board, superceded by the Advisory Board on National Parks, Historic Sites, Buildings, and Monuments 1929-47)

National Research Council, Division of Anthropology and Psychology (vice chairman, 1920; chairman, 1920-21); Committee on Scientific Research on Human Migration (1926-27); Division of Medical Sciences (member); Committee for Research in Problems of Sex (1925-36).

New York Academy of Sciences (president, 1930 and 1931)

Rockefeller Foundation (advisor)

Social Science Research Council (various committees; member of the Board of Directors, 1925, 1936-38)

Memberships (Honorary)

American Philosophical Society
Educational Research Association
Fellow of The Royal Anthropological Institute of Great Britain and Ireland
Michigan Academy of Sciences
National Academy of Sciences (elected 1929)
Phi Beta Kappa
Sigma Xi

Memberships (Additional)

American Archaeological Society
American Psychological Association
American Society of Physical Anthropology
Association of American Geographers

NOTES

1. A. L. Kroeber, *Cultural and Natural Areas of Native North America* (Berkeley: University of California Press, 1947).

2. R. Benedict, *Patterns of Culture* (Boston: Houghton & Mifflin, 1934).

3. Clark Wissler, autobiographical statement, National Academy of Sciences, Archives, Washington D.C., 1943, pp. 3-4.

4. Wissler, autobiographical statement, p. 2.

5. Wissler, autobiographical statement, p. 7.

6. Wissler, autobiographical statement, p. 8.

7. A. L. Kroeber, in *Memorial Service for Dr. Clark Wissler,* unpublished data, American Museum of Natural History, Archives, p. 1.

8. Wissler, autobiographical statement, p. 9.

9. Wissler, autobiographical statement, p. 9.

10. Harry L. Shapiro, personal communication, 1983.

11. William N. Fenton, personal communications, 1988.

12. Wissler, autobiographical statement, pp. 8-9.

13. *The American Indian* (New York: Douglas C. McMurtrie), p. 344.

14. A. L. Kroeber. Review of *Societies of the Plains Indians,* ed. Clark Wissler, *Science* 47 (1918):243.

15. A. L. Kroeber, The culture-area and age-area concepts of Clark

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16. Wissler, *Man and Culture* (New York: Thomas Y. Crowell, 1923), pp. 267, 269.

17. Jonathan E. Reyman, "Note on Clark Wissler's contribution to American archeology," *American Anthropologist* 87(1985):390.

18. A. L. Kroeber. "Zuñi kin and clan," *Anthropological Papers of the American Museum of Natural History* 18(1917):39-207.

19. A. L. Kroeber and C. Kluckhohn. "Culture: A critical review of concepts and definitions," *Papers of the Peabody Museum of American Archaeology and Ethnology* 47(1952):151, Harvard University.

20. Wissler was somewhat casual about details such as dates. We have, therefore, checked the information about his honorary degrees and offices (other than "advisor" and "consultant") in professional societies that he gave in his autobiographical statement by telephoning the societies or from published sources. We were, however, unable to verify the item about the American Association for Adult Education.

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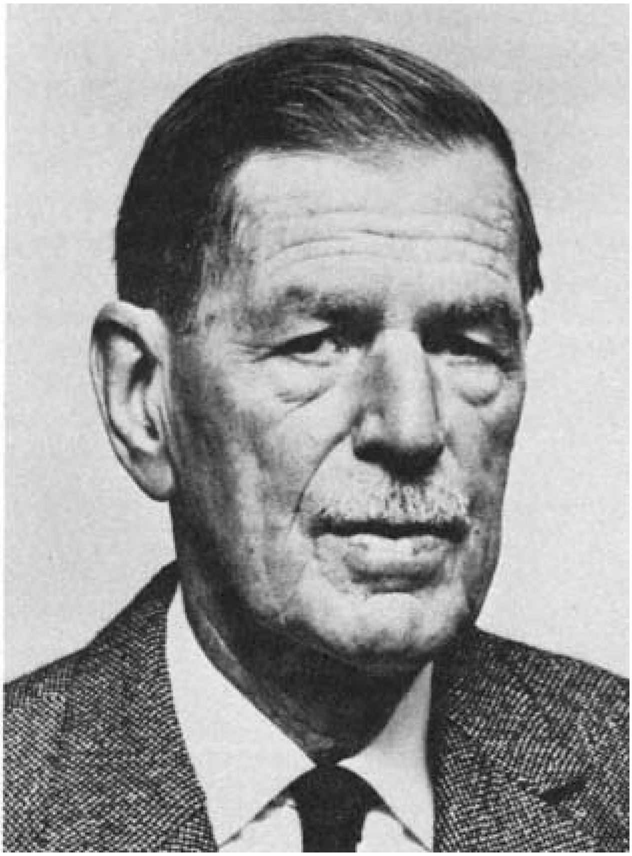

W. P. Woodring

Wendell Phillips Woodring

June 13, 1891-January 29, 1983

by Ellen J. Moore

Wendell Phillips Woodring worked for the U.S. Geological Survey (USGS) almost continuously for over forty-five years. His first job with the USGS was as a field assistant (or roustabout, as he called it) during the summer of 1912. This may have been the time he set his party chief adrift in a boat on a fast-moving river with the oars aboard but without the oarlocks in place. In spite of this youthful blunder, he was given a second chance, and he became an internationally recognized authority on Tertiary fossils of the Caribbean, Central America, and California.

Woodring's recognition of time-equivalent dissimilar lithologic facies in the California Coast Range brought order to a near-chaotic complexity, and it is the basis for all subsequent studies in that area. His painstaking and probing studies of Cenozoic molluscan faunas in the Caribbean and the adjacent eastern Pacific led to his estimate of almost precisely at the Pliocene-Pleistocene boundary for the completion of the Panamanian sea barrier and land bridge. This estimate also established the time of initiation of the great Pleistocene mammal migrations between North and South America.

Wendell Woodring was born June 13, 1891, in Reading, Pennsylvania, and was named for the abolitionist Wendell Phillips, whom his father admired. His great-great-great-grandfather Samuel Wotring changed his name to Woodring when he landed in Philadelphia in 1749. The family name originally was Vautrin in Lorraine, France, but was teutonized to Wotring after the Massacre of St. Bartholomew Day, when the family fled to Alsace, then part of Germany. His father, James Daniel Woodring, a minister in the Evangelical (now United Methodist) Church, was installed as president of Albright College, the educational institute of the church. Woodring's mother, Margaret Kurtz Hurst, was of German-Swiss ancestry. His father died in 1908, leaving his mother the task of supporting their six children on meager resources. Woodring graduated from Albright College in 1910 at the age of nineteen and taught high school science classes at St. James, Minnesota.

In 1912 he went to the Department of Geology at Johns Hopkins University as a graduate student. Although Woodring felt he was "woefully unprepared," the chairman of the department, William Bullock Clark, merely recommended that Woodring take undergraduate courses in geology and mineralogy during his first year. This he did under Professor Charles Schwartz, whom he found inspiring. He was also influenced by Professor Harry Fielding Reid, who had just published his elastic-rebound theory of earthquakes, and by Edward Berry, professor of paleontology. Woodring was awarded his Ph.D. in 1916.

Woodring worked for the USGS at the same time he pursued his doctorate at Hopkins. His dissertation dealt with Miocene marine bivalves and scaphopods from Jamaica. Under an informal agreement between the U.S. Geological Survey and the Carnegie Institution of Washington, he later expanded this work to include the gastropods. Pub lished as a two-volume set, it is modestly titled *Miocene Mollusks from Bowden, Jamaica.* But Woodring was not content only to describe the species and discuss their stratigraphic significance. He devoted a significant portion of the work to the origin and ecology of the fauna, setting a precedent in depth of inquiry.

Woodring was a leading contributor to systematic paleontology, which he called "old-fashioned paleontology." His series of Professional Paper chapters on "Geology and Paleontology of Canal Zone and Adjoining Parts Panama, a Contribution to the History of the Panama Land Bridge" (1957-82) stand as a testimony to his scholarly approach to the subject. But Woodring was no ivory-tower systematist. He was also a highly skilled field geologist who believed in covering foot by foot the terrain that he was mapping. Using Cenozoic marine mollusks as a guide to time-equivalent beds, he was able to separate and delineate lithologically similar strata, and his now classic maps have yet to show need of major revision. In fact, Ernst Cloos in presenting Woodring with the Geological Society of America's Penrose medal in 1950, said, "He is not a specialist—his interest and knowledge include the entire field of geology. He is a geologist's geologist."

In 1917 Woodring was hired as a geologist and paleontologist by Sinclair Oil Corporation for work in Costa Rica and Panama. Then, having volunteered for wartime military service, he served as second lieutenant in the 29th Engineers on the Marne River in France from 1918 to 1919. His battalion commander was Major Theodore Lyman, professor of physics at Harvard, whose name is immortalized in stellar spectroscopy as the discoverer of the Lyman line, and his regimental commander was Colonel Roger L. Alexander, professor of physics at Princeton.

After the war Woodring resumed work with the USGS, where he briefly studied the Elk Hills Naval Petroleum Reserve, in the San Joaquin Valley of California. In 1920 he was designated geologist-in-charge of the Geological Survey of Haiti. This was done according to treaty terms imposed on the Haitian government in July 1915 by the United States government during occupation by the marines following a political crisis. He traveled over much of Haiti in pack trains of horses and mules mapping the geology, and returned to the United States in April 1922. He then was appointed paleontologist in the Tropical Oil Company to work in the Caribbean coastal part of Colombia.

Woodring became professor of invertebrate paleontology at the California Institute of Technology in 1927. During his teaching years, he became a close friend of Chester Stock, professor of vertebrate paleontology, of Ralph Reed, who sharpened his knowledge of the geology of California, and of his own student, diatom specialist Kenneth Lohman. During this time, much to his great amusement in later years, he and his wife employed Linus Pauling, later two-time Nobel laureate, as an occasional baby sitter for his two daughters.

He returned to the Geological Survey in 1930, saying that three years of teaching were enough, and was assigned to map the Kettleman Hills, California. The field work was done in 1930-32. His careful mapping in the Kettleman Hills laid the groundwork for the stratigraphic classification and nomenclature of the California marine Tertiary. It contributed significantly to later interpretation of the Cenozoic geologic history of the Coast Range and to our understanding of deformation related to the San Andreas fault. Among the most valuable results of the Kettleman Hills study were the descriptions of outcropping Tertiary formations from Coalinga to Taft and how these formations relate to subsurface units, particularly to those oilbearing units informally named by drillers. The Kettleman Hills Oil Field became one of California's most productive.

In the summer of 1934, in association with Milton (Bram) Bramlette, Woodring began work on the Palos Verdes Hills, an uplifted peninsular block on the southwestern border of the Los Angeles basin. He has said that this was the most satisfying of his field experiences, partly because he so admired Bramlette, whom he considered an exceptionally skilled field geologist, but mostly because so many aspects of geology were involved and because the problems were so challenging. The Palos Verdes Hills work emphasized data that might aid in the study of the subsurface of the Los Angeles basin and the discovery of oil. But the Palos Verdes Hills paper went out of print quickly because it was in great demand by engineering geologists working to save structures threatened by the serious landslides around the seaward slopes of the peninsula. Woodring himself was amused by this development and dubbed the report a "best-seller."

From 1938 to 1940, again in collaboration with Bramlette, Woodring mapped the geology of the Santa Maria district in coastal southern California. Topographic maps of suitable scale and accuracy were not available, so aerial photographs were used for field work. The geologic maps were published on a base of mosaics of these photographs and are the first colored maps published on such a base by the USGS. In this paper, as in two others dealing with the Cenozoic stratigraphy of southern California, he presented the systematic paleontology in narrative rather than formal style, because the primary emphasis was on the geology and the relationship of the molluscan and foraminiferal faunas to the stratigraphy. These papers today remain the standard reference on paleontology, geologic names, and lithostratigraphy for geologists who work on the giant oil fields in the Santa Maria, San Joaquin, and Los Angeles basins, both onshore and offshore.

From 1941 until the end of World War II, Woodring was engaged in government oil investigations in California and was headquartered at the University of California at Los Angeles. He then returned to Washington, D.C., and the main office of the Paleontology and Stratigraphy Branch of the USGS, housed in the Smithsonian Institution's National Museum of Natural History. One of his most memorable experiences during this time was mapping the geology of Barro Colorado Island, Panama, which he said was "like working in an unfenced zoological park and botanical garden." This island now holds the headquarters of the Smithsonian Tropical Research Institute.

In the late 1940s, Woodring directed his attention to continental tropical America and began his monumental work on the geology and fossil mollusks of the Canal Zone and adjoining parts of Panama, something he had wanted to do since his student days. He began field work in the Republic of Panama in 1947 and made his tenth and last visit there in 1977 at the age of eighty-five. The first chapter of the Professional Paper series based on this work was published in 1957, and the sixth and last chapter was published in 1982. During the 1960s and into the 1970s, he devoted most of his time to this project, knowing it would be his last contribution to Tertiary molluscan paleontology. This olympian work describes and records 964 species and subspecies of mollusks in nine marine formations ranging in age from Eocene to Pliocene. He planned the formal systematic paleontology to be a basic foundation for all subsequent work in tropical America, as indeed it is and will be for many years to come. Of particular interest to him were the zoogeographic relations of the Panamanian faunas, which showed that a strait separated North and South

America during most of Tertiary time. He concluded that the mammalian faunal interchange across the Isthmus of Panama began near the boundary between the Pliocene and Pleistocene and was at its zenith during the early Pleistocene. When he spoke of the animals crossing this land bridge for the first time, his voice would change to express awe, and his eyes would light up the with wonderment of the scene.

In addition to these weighty tomes, Woodring published many shorter papers of superlative quality. Preston Cloud (1983) particularly admired one such paper and said:

> It was a brief two-page note (1960), musing pointedly on the significance of the pelecypod *Astarte,* found in modern subarctic waters and also in the subtropical Eocene London Clay Sea—an example, as he put it, of paleoecologic dissonance. A mere abstract, questioning the basic assumption underlying paleoecology.

In 1961, to honor his work on the occasion of his retirement, Preston Cloud, then with the USGS, and Philip Abelson, then with the Geophysical Laboratory, Carnegie Institution of Washington, organized the "Woodring Conference on Major Biologic Innovations and the Geologic Record" attended by his colleagues and friends from the United States, France, Belgium, Canada, and England.

Woodring's impact on the U.S. Geological Survey was unique. Refusing to accept administrative duties himself, he served as counselor to many administrators. He was an active participant in and contributor to two of the basic ruling guides of geology and paleontology: the Code of Stratigraphic Nomenclature and the International Zoologic Code. Serving as a role model for a host of younger paleontologists, his influence through the structure of his papers had a profound effect on others who strove to emulate him. In addition to being scholarly, his papers are of high literary quality and, rather than measured by quantity of printed pages, every page counts. His advice to one young author was, "Though nobody reads paleontologic papers for delectation or amusement, the reading should be as painless as possible."

Woodring officially retired from the USGS in 1961, because at that time no one could be employed by the Survey past the age of seventy. But he continued his work at the National Museum, as a Smithsonian Research Associate, occupying the same office as before. Woodring retired more fully in 1979 and went to Santa Barbara, California, where he lived in a retirement home, used the library at the University of California, Santa Barbara, and interacted with members of the Department of Geological Sciences. He died in Santa Barbara on January 29, 1983.

During the 1930s and '40s most of Woodring's colleagues at the National Museum were his age or older, and all were world-renowned specialists in their fields. Questions in any area of the natural sciences could be answered by a short walk down the hall. These scientists occupied large offices furnished with single light bulbs hanging from the ceilings and with microscopes of ancient vintage basically valuable today only as antiques. Mostly, his colleagues were male, and they wore somber suits to work with white shirts and ties. Some placed elastic bands on their shirt sleeves to keep them out of the way and wore green visors to protect their eyes from the glare of the naked light bulbs. Originally, the offices in the museum were all on the third floor, and one wit, a renowned scientist himself, said that the third floor housed the most interesting exhibits.

Woodring certainly shared in this glory and eccentricity, although he eschewed the eyeshades and shirt garters. He always walked erect, and his presence was austere. Socializing took place only during lunch or during one of his two precisely scheduled fifteen-minute coffee breaks. He was meticulous in every way, and his office was always impeccably neat. At the close of the day, he cleared his desk and neatly stacked and covered the wooden half-trays of fossils. He wrote his papers in longhand on pads of blue-lined paper and sent the sheets directly to the typist, usually without a need to add or change a single word.

As a young man at the National Museum, Woodring had held William Healey Dall in awe, and said later, in a memorial to Dall (1958,3), that "to a novice he was a fabulous tradition rather than a man," and so Woodring himself grew to be regarded. His appearance and reputation kept many people at bay, yet he could be most kind and compassionate, even forgiving gross accidental errors if the appropriate apology was forthcoming. He never expected more of others than he asked of himself. But he was intolerant of imprecision, and it was hard to meet his level of precision and thoroughness. Woodring set high scholarly goals, and he loved to be challenged intellectually and to argue with admired colleagues, but few had the fortitude to rise up and disagree with him. Still, he disliked obsequiousness and would become angry when his ideas were accepted without thought, simply because of his reputation.

When Preston Cloud became Woodring's chief in the late 1940s he brought the branch into the 20th century with fluorescent lights and new microscopes. He also started hiring young Ph.D.'s to train under the old timers, and obtained budgets sufficient to support field work. The changes were stunning, and many in the group old enough to have fathered Cloud took umbrage. But not Woodring. He supported Cloud and his innovations, and the two became close friends. Cloud reciprocated by challenging him at every turn, and Woodring was delighted. On special occasions during a two- or three-martini lunch, the glasses would bounce on the table as the two pounded away, arguing on the cutting edge of geology. Those, including the new crop of young scientists who worked with Woodring and saw beneath the tradition and dignity, cherished Woodring as a colleague and friend who was deeply caring and had a delightful sense of humor.

Woodring was awarded membership in the National Academy of Sciences in 1946, served as chairman of the Geology Section and coordinator to the Biology Section, and in 1950 organized an Academy conference on paleoecology and biogeochemistry. From this conference emerged new interdisciplinary efforts related to the biosphere, atmosphere, and crustal evolution within the larger Earth history. He was president of the Paleontological Society in 1948 and received the Penrose medal of the Geological Society of America in 1949 and an honorary Doctor of Science from Albright College in 1952. He was president of the Geological Society of America in 1953 and was elected to membership in the American Philosophical Society in the same year. He received the Distinguished Service Award and Gold Medal of the U.S. Department of the Interior in 1959. In 1967 Woodring received the Thompson Medal of the National Academy of Sciences, and in 1971 he was honored by the President of Costa Rica on behalf of Central American geologists. In 1977 he received the medal of the Paleontological Society.

In 1918 he married Josephine Jamison, who died in 1964. In 1965 he married Merle Crisler Foshag, who died in 1977. His daughter Jane died in 1954. At the time of his death he was survived by his daughter, Judy Armagast, three grandchildren—David Woodring Armagast, Marilyn Armagast Martorano, and Susan Jane Armagast (now Susan A. Moison, M.D.)—and two sisters, Margaret Brillhart and Mary Hangen.

A final tribute is the establishment by his daughter Judy of the W. P. Woodring Memorial Fund, for aid to graduate students in the Department of Geological Sciences at the

University of California, Santa Barbara. A legacy which Woodring himself bestowed was his donation of over 2,000 volumes and some eight boxes of reprints to the Escuela Centroamericana de la Universidad de Costa Rico to show his admiration and respect for the people.

For this memorial, I drew on biographic data prepared by Woodring for the National Academy of Sciences and furnished to me by the Office of the Home Secretary, as well as on memorials previously published by Preston Cloud (American Philosophical Society, 1983) and myself (Geological Society of America, 1984). Also used were the Penrose medal presentation by Ernst Cloos (1950), the Paleontological Society medal presentation by Preston Cloud (1978), and personal data from Woodring's daughter, Judy Armagast. And finally included are my own recollections of working with Woodring at the National Museum from 1951 to 1959, and information from correspondence that is now housed in the archives of the Smithsonian Institution.

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Photo by Anne Plettinger

Frederik William Holder Zachariasen

February 5, 1906-December 24, 1979

by Mark G. Inghram

Frederik William Holder Zachariasen's contributions to science have been rich and varied. He was a leader in the determination of the crystal structure of inorganic crystals using x-ray diffraction. Though primarily an experimentalist, he contributed to theory whenever he found the theory inadequate. In over 200 published papers he included experiments on the crystal structure of minerals, on the structure of inorganic crystals, on the structure of anionic groups, on atomic and ionic radii, on the glassy state, on the liquid state, on actinide crystal chemistry, on high-pressure phases, on crystal structure and superconductivity, on the melting process, and on the variation of bond strengths with bond lengths. His contributions to theory include papers on temperature diffuse scattering of x-rays, on stacking disorder, on the phase problem, and on extinction including the Borrmann effect. Each of these theoretical efforts was followed by careful experimental investigations to establish the correctness of the theory he had developed.

Linus C. Pauling of the University of California at Berkeley, who also concentrated on the determination of crystal structure during his early years, said of Zachariasen's work (1975), "I feel that he is to be classed among the outstand ing scientists of the twentieth century, and at the top in the field of inorganic crystal structures." Robert Penneman of the Los Alamos Scientific Laboratory said (1975), "The breadth of his contributions is enormous; there is no major advance in crystalography in one half century that does not bear his mark." Bernd T. Matthias of the University of California, San Diego, said (1975), "His was a monumental achievement in understanding the detailed nature of the whole periodic system." Such accolades abound.

THE MAN

Zachariasen had absolutely no use for pretense or titles. His friends and associates always called him by one of his two nicknames, "Willie" or "Zach." It would give completely the wrong impression of "The Man" if I were to refer to him as "Zachariasen" in this memoir. I will therefore use the name he preferred: "Willie." He would want it no other way.

Willie was born in Langesund, at the mouth of the Langesundfjord, in southeastern Norway. Willie's father was a sea captain. Langesund is about 15 kilometers from Brevick, which is at the center of the nepheline-synetic pegmatite veins which have yielded over thirty new species of minerals. The islands of the Langesundfjord are also rich in deposits of rare silicates and other well-crystallized minerals. Raymond Pepinsky, one of Willie's early Ph.D. students, relates a story in which Willie, decades after his youth, correctly identified on sight a Langesund eudidymite specimen which had been mislabeled by the Krantz firm in Germany. X-ray examination proved Willie correct. "Willie, how could you have known?" he was asked. "I played on the Langesund islands when I was a boy," he replied with his usual warm grin, "and I remember those crystals." Such mineral crystals so interested Willie that when he went to

Oslo University in 1923, he studied in the Mineralogical Institute under the guidance of the great geochemist Victor Moritz Goldschmidt.

Goldschmidt (1888-1947) was one of the first to recognize that crystal structure data as determined using x-rays could reveal the distribution of elements within crystalline minerals. Among the crystals which interested Goldschmidt, and which Willie studied while still in Oslo, were some rare earth crystals. Crystals containing these elements abound on the Langesundfjord islands. As Pepinsky tells us, "In order to protect the richest of such deposits for his own leisurely study of its mineral species, Goldschmidt purchased one of the islands Willie knew best."

Willie published his first paper, "Uber die Kristallstrucktur von BeO," in 1925 at the age of nineteen. He published nineteen more papers before publishing his Ph.D. thesis in 1928 at the age of twenty-two. He was awarded the Ph.D. that same year. He was then appointed assistant professor at the University of Oslo, but was granted a leave for 1928-29 to accept a Rockefeller Foundation Fellowship to study with Sir Lawrence Bragg at Manchester University in England.

After his postdoctoral at Manchester, Willie returned to Oslo. Early in 1930 Willie received an offer from Arthur Holly Compton, Nobel Prize winner for the discovery of the Compton effect, to join the faculty of the Department of Physics of the University of Chicago as an assistant professor. Willie had been recommended to Compton by Bragg. Willie accepted that appointment and stayed at the University of Chicago until he retired in 1974.

One week before Willie sailed for New York he married Ragni (Mosse) Durban-Hansen, granddaughter of the pioneer Norwegian geochemist W. C. Brogger (1851-1949). Brogger, among other things, had discovered and first described the minerals of the Langesund region. In many of Willie's publications on the structure of minerals he refers to "Brøgger's well-known monograph on the pegmatite minerals of the Langesundfjord." Clearly Willie loved and was immersed in the study of minerals and crystallography while young and in Norway.

The invitation to the University of Chicago in 1930 was an effort on Compton's part to build up x-ray studies at Chicago. It is of interest to note that, at that time, Compton was not chairman of the Department of Physics. He built up the group in x-rays almost on his own. Willie arrived on campus on the same day as another new assistant professor active in x-ray studies: Samuel King Allison. Allison had also been invited to join the faculty by Compton. Allison already knew the campus since he had grown up in the University area and had received his Ph.D. from the University seven years earlier. The Zachs and the Allisons became the closest of friends—a friendship that lasted the rest of their lives. Also added to the faculty in 1930 as an instructor was Ernest O. Wollan. In addition to these four faculty members active in x-ray studies there were three postdoctorals: Marcel Schein from 1929-31 as a Rockefeller Foundation fellow, Elmar Dershem from 1929-42 as a research associate, and John H. Williams from 1931-33 as a National Research Council fellow. Williams had earlier worked with Allison at the University of California, Berkeley. In addition to the very close friendship with the Allisons, the Zachs also became close lifelong friends with the Scheins and John Williams. Willie had little respect for Compton as a person. Three of these seven—Allison, Compton, and Williams—worked on the physics of production of x-rays and on their interaction with matter. Dershem and Schein worked in similar areas, except with very soft x-rays. Wollan worked on the scattering of x-rays by gasses. Willie was alone in crystal structure determination. During the few years this group was together, almost all of their research was published independently. There were three exceptions, two papers by Dershem and Schein and one by Wollan and Compton. Compton and Allison did publish jointly an extensive and excellent reference book, *X-Rays in Theory and Experiment* (1934). In spite of the extensiveness of that book, it contained nothing on Willie's love, crystal structure. Shortly after that book was written, all except Willie changed their fields of research: Compton, Dershem, and Schein to cosmic rays; Allison, Williams, and Wollan to nuclear physics. Willie continued, single-mindedly, to determine what he could about the structure of matter using x-rays as a tool.

Willie had very little financial support for his work during the thirties. Aside from funds to support Compton's work (supported in part from the outside) and operating A. A. Michelson's two grating ruling engines (maintained during that period by the chairman of the department, Henry Gordon Gale), there was very little money left for others. Willie had to use homemade x-ray tubes built by his associate Dershem. When in 1938 his kenotron rectifier burned out, he had to shut down his research for six months until the department could find the $75 necessary to buy a new rectifier. All trips to meetings of the American Physical Society were at participants' expense. Willie had to make his own slides. A typical procedure for such trips was for Willie and Sam Allison to take one car, add as many of their students as possible and head for the meeting. If the meeting was to be in Washington, D.C., for example, they would stop overnight at the Gamma Alpha house (scientific fraternity) at the University of Ohio, and then at a cheap tourist house in Washington. This lack of support from the department of which he was a member continued until 1943.

In 1943 Willie joined the Manhattan Project. During the next two years he helped to unravel the chemistry, and to determine the crystal structure, of the transuranic elements and compounds. Many who were involved in that project feel that it would have taken much more time to do these jobs if it had not been for Willie's insight and genius in crystal structure determination.

Late in 1945 Willie first accepted administrative duties. His influence and effectiveness in these positions has positively affected many lives. In 1928, just two years before Willie went to the University of Chicago, a national survey had rated the Department of Physics of that university number one in the country. This was due in large part to the presence at that time of Michelson, Milliken, and Compton, three Nobel Prize winners. During the thirties, under the guidance of Gale and Compton, that rank slipped badly. This, according to Willie, was due primarily to the autocratic rule within the department, and to the hiring by the department of its own students as junior faculty, largely to assist the faculty member under whom that student had received the Ph.D. degree.

The changes Willie made were momentous and lasting. He immediately ended the domination of the department by Michelson's grating ruling engines by giving them away, one to Bausch and Lomb, and one to the Massachusetts Institute of Technology. He immediately turned the department from autocratic to democratic. The then tenured staff of the department met for the first time in many years to consider departmental affairs. Without much delay they voted to terminate the appointments of ten nontenured staff members. They also voted to reject as a member of the Department of Physics a new professor whom Compton had just hired, as usual without consulting anyone else in the department—a ticklish situation which Willie had to handle. A position for this professor was finally found in another part of the university.

Next, the staff voted to appoint Enrico Fermi and Edward Teller as professors, Robert F. Christy and Walter H. Zinn as associate professors, and John H. Simpson as instructor. It was Willie's job to invite these men to join the staff and to persuade them to accept. Willie did invite them, and all accepted. With this success, the Chemistry Department, partly due to Willie's needling, invited Willard F. Libby, Joseph E. Mayer, and Harold Urey. This enabled Willie, with the support of the physics faculty, to invite Maria G. Mayer to become a volunteer professor of physics, since the university's nepotism rules at that time forbade two members of one family holding faculty positions. She accepted. A bit later, with the support of the physics faculty, Willie invited Gregor Wentzel and others. The fact that all these outstanding physicists accepted positions at the University of Chicago is testimony to Willie's persuasiveness and the confidence which people put in his work and leadership. With this staff there was no trouble in attracting the most outstanding students in the country. By 1949 the department was once again rated tops in the nation, and among the Ph.D.'s graduated between 1945 and 1950 were five who were later awarded Nobel Prizes in physics.

As soon as Willie took over, with the unanimous support of the faculty, Willie introduced bylaws by which the department was to operate. According to these bylaws the department was no longer to be administered by a chairman who acted as a head, but by a true chairman. To Willie "chairman" meant that the person administering the department could do only those things that the faculty voted while the chairman occupied the chair at faculty meetings. All faculty were to vote on new appointments;

all faculty of higher rank on promotions. A policy committee, a budget committee, a curriculum committee, a services committee, etc., were established made up of faculty, by vote of the faculty. With this reorganization the old autocratic procedures of Gale and Compton were gone. As far as graduate students were concerned, Willie persuaded the faculty to accept the rule that all Ph.D. theses had to be published under the students' names alone. He felt that if a piece of research had not been done independently enough to justify publication by the student alone, it was not acceptable as establishing that the student could do independent research. Willie's standards were always very high. This rule held until the late 1960s when exceptions were made for students in high-energy physics, where a staff member's participation was required before that student could get access to a large accelerator.

Willie had a heart attack in January 1949 and a second attack four months later. With the second heart attack Willie resigned the chairmanship of the Department of Physics and slowed down a bit. He had published twenty-seven papers in 1948-49. Willie, however, was not one to walk on eggs. In 1954-55 he published fifteen papers. With the department once again going downhill, Willie was drafted once again to take over the chairmanship in 1956. Again he worked his magic. In 1959 the faculty of the Division of Physical Sciences persuaded him to take over as dean of the division. Again he did his magic, and the caliber and support of the faculty throughout the division improved. Having accomplished what he thought he could, he resigned as dean in 1962, two years before his term was up, and returned to his research. In 1970 Willie had a bad case of phlebitis. His best friend Sam Allison had died of complications from phlebitis a few years earlier. Two heart attacks and phlebitis were enough to cause him to rethink his future. He was just at retirement age. He was offered a special postretirement appointment but decided to accept that appointment only part time, so that he could spend some time "living." Since Willie and his wife, Mossa, had many friends from the Manhattan Project living in Los Alamos and its neighbor Santa Fe, New Mexico, they decided to move to Santa Fe. There they purchased the first home they had ever owned. Willie did as he had agreed; he and Mossa returned to the University of Chicago for one quarter of each year, for three years, to give one course. He kept quite active in research through his contacts with associates in the Los Alamos scientific laboratories, mainly his friends Finley H. Ellinger and Robert A. Penneman. He also worked with his friend and associate Bernd Matthias at the University of California, San Diego. He published several papers with each. He also spent time enjoying food, music, art, mystery stories, Indian lore, his home, and "living."

Willie was a superb teacher both at the graduate and undergraduate levels. I well recall one course I took from him in graduate-level classical mechanics in the late thirties. This course was considered one of the very best in the department at that time. Willie would enter the lecture room, place his notebook on the desk, and proceed to give a beautiful, understandable, and rigorous lecture. Then, after answering questions he would pick up his still-closed notebook and leave. Only once in that particular eleven-week course did he open that notebook to check on a formula he had derived. He decided that it was correct, though in a different form than his notes. He then closed his notebook and finished the lecture. He just never made mistakes. It was not until years later that I learned that such lectures were the result of careful preparation on his part. Some years later, when I was chairman of the De partment of Physics, I asked Willie to give an introductory course in physics (physical science) for nonscience students at the undergraduate level. He agreed and did a superb job in this difficult assignment. Student evaluations were enthusiastic. He developed a close personal relationship with the roughly 150 students in his class. The course was given during the period of student protests in the late 1960s. The University of Chicago students were no different from any other college students, and students, including students from his class, took over the administration building of the university. Willie was one of a few faculty whom the students would let into that building. They obviously enjoyed conferring with him. He helped calm troubled waters.

Willie sponsored a number of students for their Ph.D. degrees, all of these in the period 1930-40. His Ph.D. students were John Albright, Donald A. Edwards, Ssu Mien Fang, Jane (Hall) Hamilton, Dorothy Heyworth, Richard C. Keen, Raymond Pepinsky, Stanley Siegel, Rose (Mooney) Slater, and G. E. Ziegler. He had two assistants while lie was working for the Manhattan Project: Wallace Koehler and Ann Plettinger. Koehler stayed with Willie for a few years after World War II working toward a Ph.D., but he did not finish. Plettinger stayed with Willie as an assistant until he left Chicago. She was coauthor with Willie on nine of his post-World War II papers. Willie accepted no students seeking advanced degrees during or after World War II. He felt that the work he was doing was chemistry, not physics, and that it was not a suitable field of research for physicists. He was in a physics department. Willie did accept several postdoctorals who came to him with outside named fellowships. He used the criterion of outside support as one indication of the independence, competence, drive, and real interest of these people in the work he was doing. He felt that if he provided the support, he would get people who simply wanted a job and weren't good enough to get their own support, or a faculty position elsewhere.

During Willie's first twenty years at the University of Chicago his favorite vacation was a stay with his friend Sam Allison at Three Lakes in Wisconsin or, more frequently, a fishing-canoeing trip into the Lake of the Woods area in northern Minneosta with his friends from his early years in Chicago, Sam Allison and John Williams. Williams had gone to the Department of Physics at the University of Minnesota. The fourth person on these trips was generally Buddy Thorness, also from the University of Minnesota. Willie loved the woods, the water, the portages, the fishing, the cooking, and most of all the repartee with these close friends.

After Willie's two heart attacks in 1949 the frequency of these canoeing trips dropped off. For relaxation Willie then became a devotee of the billiard room of the faculty club of the University of Chicago for a short game after lunch. The favorite game soon became a frustrating game called "Cowboy Pool." It was the game of choice for Willie because it is a game that cannot be played by a person without a sense of humor. As his ofttimes partner in these games Julian Goldsmith has said:

> Willie was the leader of the group, made up of people of diverse and unrelated interests. He set the tone and developed refined rules, designed to eliminate the trivial and make the game more challenging—typical of Willie. He set the standard for gamesmanship, repartee, razzing, hexing, friendly insult, amateurism, and comradery that made winning or losing of little importance. His influence made the game a subtle Rorschach test, and the real nature of the players became quickly evident. Willie's humor and strength of personality pervaded the room. He had a rapier-like wit. He added to what may sound like a sterile activity. With Willie it wasn't.

THE SCIENCE

Willie was always interested in the structure of matter that x-rays could reveal. He was not a developer of new x ray instrumentation or techniques. The techniques he learned while a student of Goldschmidt at Oslo University were the Laue (single crystal), the Debye-Scherer (powder), and the rotating crystal techniques, all of which used photographic recording. During his postdoctoral period with Sir Lawrence Bragg in Manchester University, Willie learned the Bragg technique of measuring the intensities of reflections from single crystals by means of ionization chambers, and the use of those measurements to derive Fourier diagrams (two-dimensional representations) of electron distributions within crystals. After World War II Willie adopted, and contributed to the development and use of, the single fixed crystal spectrometer using proportional counters for measurement of spatial intensity measurement. He did on a few occasions write papers in which he used neutron diffraction to determine the position of light elements within crystals, e.g., the rare earth hydroxides (1955) and MgH (1963). In these few cases Willie did the interpretation of the data, and his associates determined the neutron diffraction patterns.

Willie was also a very good chemist. His mentor Goldschmidt had prepared a number of the compounds and crystals that Willie used while a student in Oslo. Willie later prepared a number of compounds and crystals for his own use. After World War II, Willie had his own small chemistry laboratory. In that facility, among other things, he prepared a number of fluoride compounds and metaborates. He also grew crystals, e.g., of the metaborates, of sufficient size and perfection to do single crystal studies.

Mineralogical Crystals

As detailed earlier, Willie was born and grew up in Langesund, Norway, an area rich in well-crystalized minerals. He was intrigued from the very beginning with the structure of these minerals. His first published paper was on the crystal structure of BeO, which as a mineral is called Bromellite, named for the Swedish mineralogist who discovered it. His second paper was on Wurtzite (ZnS) and the related crystals α and ß CdS. Most of Willie's first thirty-four papers concerned minerals or compounds of interest to mineralogists. Specifically, and in order, the minerals he studied after Wurtzite included Phenacite (Be_2SiO_4), Willemite (Zn_2SiO_4), Montroydite (HgO), GeO_2 isolated from Argyrodite, Bixbyite ($[Fe,Mn]_20_3$), Titanite ($CaTiSiO_5$), Eudidymite and Epididymite ($NaBeSi_3O_7[OH]$), Thortveitite ($Sc_2Si_20_7$), Benitoite ($BaTiSi_3O_9$), Hambergite ($Be_2BO_3[OH]$), and Colusite ($[Cu,Fe,Mo,Sn]_4[S,As,Te]_{3-4}$). Along with these structure determinations, Willie discussed the structure of a number of minerals having analogous structures and formulas. As a survey of these minerals shows, as time went on, Willie determined the structure of more and more complex minerals.

Inorganic Crystals

Willie's interest in the structure of inorganic crystals in general and the reasons for variations in those structures become apparent in his years in Oslo. He did not just study the mineral Wurtzite (ZnS), he also investigated the chemically similar crystals ZnSe and ZnTe; not just BeO but also BeS, BeSe, BeTe, and MgTe; not just CdS but also CdSe and CdTe; not just HgO but also HgS, HgSe, and HgTe. He also made systematic studies of sesquioxides (X_2O_3) and crystals of the form AXO_3 (1928,3). Such studies enabled him to make correlations and generalizations. One important piece of work of this type was his publication of tables of atomic radii. His first publication of a paper specifically on this topic was in 1931.

Some of Willie's extensive general and systematic studies of inorganic crystal structure are sufficiently distinct to be listed separately.

Groups in Crystals

As Willie said in the introduction to a number of his papers, "I have been interested in the determination of the shape and accurate dimensions of inorganic groups in crystals." His interest was not only in the shape and dimensions, but variations in those parameters for the same group, in different crystals, and the reasons for those variations. This interest first appears in print in his thesis where, among other things, he studied the shape of XO_3 groups. The interest continued throughout his career. His first paper specifically on the subject, "The Structure of Groups XO_3 in Crystals," was published in 1931. He published thirty-three more papers, which in large part were studies of groups. He distinguished groups from radicals in the sense that an XO_3 group in a long string of XO_3 groups in which O's are shared between all adjacent groups is a group, it is not a radical. He correlated the structure of these groups with the number of valence electrons in the group. In his 1931 paper on XO_3 groups he showed that XO_3 groups having twenty-four valence electrons, e.g., $(NO)^{-1}$, $(CO_3)^{-2}$, and $(BO_3)^{-3}$, have coplanar structure, while those having 26 valence electrons, e.g., $(PO_3)^{-3}$, $(SO_3)^{-2}$, $(ClO_3)^{-1}$, $(AsO_3)^{-3}$, $(ScO_3)^{-2}$, $(BrO_3)^{-1}$, and $(SbO_3)^{-3}$, are pyramidal. In a 1932 paper entitled "Note on a Relation Between the Atomic Arrangement in Certain Compounds, Groups and Molecules and the Number of Valence Electrons," he refined these ideas. He made predictions based on valency for XO_2 groups and, for example, predicted that the $(NO_2)^{-1}$ group in $NaNO_2$ would not be linear. He put his student G. E. Ziegler to the task of checking this prediction. It proved correct. Willie also pointed out that crystals whose chemical formulas are AXO_3

do not necessarily have XO_3 groups. He demonstrated this to be the case in $LiIO_3$, $NaIO_3$, and $CsIO_3$, where iodine occurs in IO_3 octahedra which share corners with one another.

In another series of systematic investigations Willie studied the structure of S_nO_m groups. Specifically, he determined the structure of sulphite $(SO_3)^{-2}$, pyrosulphite $(S_20_5)^{-2}$, persulphate $(S_20_8)^{-2}$, and trithionate $(S_30_6)^{-2}$ during the period 1931-34. The structure of sulphate $(SO_4)^{-2}$ had been determined earlier by others, but controversy still existed about its structure in Na_2SO_4 and $AgSO_4$. Willie redetermined these structures. The dithionate group $(S_2O_6)^{-2}$ had been determined by others earlier. Based on such studies Willie showed that the pyrosulphite group should be written as SO_3SO_2 linked by a covalent bond between the two sulphurs, not as SO_2SSO_2, as had been previously assumed. Willie also showed that the persulphate group could be described as two SO_4 groups linked together by a covalent bond between two of the oxygen atoms SO_3OOSO_3 and that the trithionate group could be described as two SO_3 groups bonded to a common sulphur atom SO_3 $SSO_{3.}$ Such generalizations had obvious implications for later investigators who investigated other compounds containing these groups.

Borate Groups

The most extensive series of investigations of groups that Willie undertook was the determination of the structure of borate groups and of borates. This series of investigations continued through sixteen papers extending over thirty-three years. He introduced some of these papers in the later part of the series with the phrase, "This investigation is part of a systematic study of borate structures being carried on in this laboratory." As one might guess from Willie's history, the first borate Willie investigated was of a mineral, Hambergite, Be_2 (BO_3) (OH) (1931,1), in which he found the borate group to exist as an almost perfect O_3 triangle with the boron atom at its center. He reexamined this crystal in more detail in 1963. Willie also investigated boric acid H_3BO_3. Again he found an identifiable BO_3 triangle to exist, this time with a hydrogen bonded to each oxygen. The structure is thus better written as $B(OH)_3$. He then investigated a series of metaborates, i.e., crystals in which chemically the borate group is BO_2. The metaborates he studied included in order $Ca(BO_2)_2$, $KH_2(H_3O)_2,(BO_2)_5$, $K(BO_2$, ßH(BO_2), ßH (BO_2), $Na(BO_2)$, and $Li(BO_2)$. Again the breadth and depth of Willie's interest in each problem are obvious. Willie had suggested quite early, i.e., after his study of $Ca(BO_2)_2$, that BO_2 groups do not exist in crystals. It surprised many to learn that no identifiable $(BO_2)^{-1}$ groups were found. The structures Willie did find in these crystals were varied and beautiful, and they clearly intrigued Willie. In $Ca(BO_2)_2$ he found the structure to consist of an endless chain of almost perfect BO_3 triangles with two oxygen atoms in each triangle shared with an adjacent triangle. He later showed how these chains were bound together by calcium atoms. He showed that the same structure existed in Li (BO_2). In $K(BO_2)$, which Willie studied in 1931 and later in $Na(BO_2)$, he found the borate to again exist as a BO_3 triangle, but this time three triangles were formed into a six-membered ring of BO_3O_3 with a third oxygen bound to each boron to complete the three BO_3 triangles making up the ring. In 1940 H. Tazaki found metaboric acid ßH(BO_2), orthorhombic, to have the same borate structure Willie had found for $K(BO_2)$. Willie later studied the other two forms of metaboric acid. He found ßH(BO_2), monoclinic, to consist of chains of borate groups, two-thirds BO_3 triangles, and one-third BO_4 tetrahedra. In ßH(BO_2) cubic, he found all borons to be in tetrahedral configuration. Willie then went on to still more complicated metaborate. In $KH_2(H_3O)_2(BO_2)_5$

he found the borate structure to be a chain made up of pentaborate groups consisting of one BO_4 tetrahedron bound to four BO_3 groups, the groups sharing oxygens. He found $K_2B_40_7(H_20)$ to consist of $[B_4\ 0_5(OH_4)]^{-2}$ radicals made up of two BO_4 tetrahedra and two BO_3 triangles having only corners in common. In Willie's last paper on metaborates, $LiBO_2$ (1964,2), he gave preliminary results on $LiBO_28H_2O$ and promised to give details later. Given more time he certainly would have done so. This example of one of Willie's areas of interest well illustrates the depth and breadth of each of Willie's investigators.

Atomic and Ionic Radii

In 1932 Willie published a paper entitled "A Set of Empirical Crystal Radii for Ions with Inert Gas Configuration," in which he compared empirically calculated values for these parameters with x-ray measurements. This paper was a needed improvement of work done by Goldschmidt and by Pauling. In the paper Willie takes into account coordination number (number of nearest neighbors in the lattice), valence, and radius ratio. Starting with this paper, Willie returned again and again in later papers to refine his self-consistent table of atomic ionic radii. Such data are important in correlating the behavior of differential chemical elements and in making predictions of the properties of substances whose crystal structure has not yet been determined. Willie's last paper specifically on the subject was published in 1973, "Metallic Radii and Electron Configuration of the 5f-6d Metals."

Glass

In 1932, i.e., during Willie's first years at the University of Chicago, he departed from his study of crystals to give the first correct description of the structure of glass. He never considered this work as very important and, on at least one occasion, had to be reminded of the work before he recalled that at one time he had written about the subject. Those working on glass, however, consider his 1932 paper, which he expanded somewhat in a German version in 1933, to be the starting point of the real understanding of glass. Charles H. Green in a 1961 article on glass in *Scientific American* said, "The present day understanding of glass rest heavily on a single lucid paper, only twelve pages long, written in 1932 by William H. Zachariasen." Alfred R. Cooper in his introductory paper to the 1980 Borate Glass Conference said, "We dedicate this session on glass structure to Frederik William Holder Zachariasen because his single contribution to glass literature, 'The Atomic Arrangement of Glass,' may be the most influential paper on glass structure in this century."

Before Willie's paper on glass it was said that glass consists of crystalline materials. More precisely, the main features of the x-ray patterns of glass could be explained on the basis that vitreous silica consists of cristobalite crystallites, having dimensions of about 15Å and a lattice constant about 6.6 percent greater than in crystalline silica. Willie argued that this description could not explain the properties of glass. He then proposed a very different structure using oxide glasses as an example. Specifically, he suggested that glasses are made up of oxide groups AO_n satisfying the following four rules: (1) An oxygen atom is linked to not more than two atoms A; (2) The number of an oxygen atoms surrounding atoms A must be small (refined in the next paragraph to triangular or tetrahedral configuration for known glasses); (3) The oxygen polyhedra share corners with each other, not faces (this leads to random orientation of adjacent groups and hence no long-range order, i.e., no crystalline structure); (4) At least three corners in each oxygen polyhedron must be shared. Based on these rules, Willie was able to predict compounds which would produce a glass as well as explain many of the properties of glass.

Willie's paper on glass has appeared to many who have written about it to have come out of the blue. However, Willie had discussed crystals of the rutile type XO_2 in his 1926 paper with Goldschmidt, and some silicates in his 1930 paper with Bragg. From this work he had some concept of how SiO_4 groups associate. As I have detailed in the previous section, he had also studied how borate groups fit together in a variety of ways. Thus Willie's paper on the structure of glass was simply an insightful extension of his earlier studies of groups.

Liquid Structure

Willie published one paper on "The Liquid 'Structure' of Methyl Alcohol" one year after his classic paper on the vitreous state (glass). Methyl alcohol was particularly interesting to Willie since alcohols have a tendency to form a glass. The results were consistent both with Willie's papers on the structure of glass and with Bertrum Warren's x-ray studies of the liquid state. Warren had been a postdoctoral with Sir Lawrence Bragg at the same time as Willie.

The 5f Elements

Perhaps Willie's most celebrated work was the elucidation of the nature and the chemistry of the transuranic elements. His work in this area, started in 1943 when he joined the Manhattan Project, was extremely important to the Manhattan Project. He continued to do some work in this area throughout the rest of his life. Robert Penneman of the Los Alamos Scientific Laboratory has said of this work, "No other crystalographer has done so much to ex pand our knowledge of heavy element chemistry, or had such a central role in the early development of atomic energy." The initial problem faced in the understanding of the chemistry and structure of the transuranics becomes clear when one recalls that during the early stages of the Manhattan Project, only microgram quantities of these artificially produced elements were available. This meant that the chemistry of these elements, including the important separation processes for plutonium from its host materials, had to be determined using only these amounts. It is indeed difficult to determine the chemical composition of a sample using ordinary chemical techniques when only microgram amounts of that chemical are present. The procedure adopted involved the chemists doing microchemistry on these samples, and then sending them in capillaries to Willie to find out what they had produced. As it turned out during the early stages of the Manhattan Project, in many cases the compounds produced were a complete surprise. As these studies continued, and more detailed information was obtained, the concept of these elements being a 5f series of elements, analogous to the 4f series of elements in the rare earths, grew. The experimental evidence for this concept rested in large part on Willie's work. He found from his x-ray studies that the radii of successive transthorium elements in isostructural compounds decreased slowly, i.e., by about 0.03A per successive element, much as the radii of the rare earth elements decrease slowly, by about 0.03A per successive element. This was the first strong evidence for the 5f character of these elements. Willie's studies were crucial in the development of the metallurgy of the transuranium elements, particularly in the important case of plutonium. Within a few months of the preparation of the first milligrams of metal, Willie recognized that the metal had several phases stable at near-normal temperature and pressure (now a total of six). He solved the extremely complicated structure of α-plutonium with only slide rule and insight. The x-ray pattern is complicated since the first seventeen x-ray reflections are absent. Others had failed to solve this structure using the largest computers then available.

In 1952 Willie put forward one suggestion concerning these elements which has proved to be incorrect. He suggested that the 5f elements should be called thorides rather than actinides. The argument is in essence, are the elements predominantly trivalent, or are they tetravalent? In a 1961 paper that discussed the question, he said, "It is frankly admitted that the conclusions presented in this paper are somewhat speculative, and that the results as to electronic configuration ought to be based on physical properties which depend more directly on electronic interactions." His original reasoning in making this suggestion was based on consideration of the atomic radii exhibited by these metals. He had determined these radii, and they were just the ones one would expect for tetravalent metals with five or six electrons *if* the 5f electrons do not contribute to bonding, an assumption generally held at the time. Later theoretical work using large computers showed that this assumption was not generally valid. That work and later experimental results on superconductivity of these elements appear to have convinced Willie shortly before his death that this one suggestion was incorrect.

One of the most beautiful verifications of the similarity in electronic structure of the 4f and 5f elements was the discovery in 1974 by Willie and Penneman that above 56 kbars, cerium has the same crystal structure as α-uranium. Before their insightful discovery, α-uranium had a structure that was unique.

Diffraction Theory

Willie first felt the lack of adequate theory when in 1940 he wrote a paper on "A Theoretical Study of the Diffuse Scattering of X-rays by Crystals." He followed this paper with five other papers on the subject, including careful experimental checks. He assigned his student Stanley Siegel the job of completing these checks. Siegal's work was published in 1941. Partly as a result of this work, Willie took on the task of going over the then extant theory. This effort resulted in his classic 1945 monograph, *Theory of X-ray Diffraction in Crystals.* The succinctness of this book, as with many of Willie's publications, is illustrated by the story related by Wallace C. Koehler, Willie's student at the time. According to Koehler, after spending considerable time trying to get from one equation to the next, separated by the phrase "from this it follows," he asked Willie how he did it. Willie replied, "Don't worry, it took me two days to make that step." As this story illustrates, the book is used mainly by experts in the area. Many more recent papers are little more than direct expansions of paragraphs from Willie's book.

One problem that for many years made precise crystal structure determinations difficult was the lack of a method for determining the phase of structure factors. In 1952 Willie developed a method for determining the phases directly from the measured intensities. He immediately tested his method experimentally by applying it to metaboric acid. From that time his technique was refined until by 1975 over half of the structures being determined by x-ray techniques were solved by the "direct method" traceable to Willie's work.

Another problem that long plagued precise structure determination was a discrepancy between the calculated and measured intensities of diffracted x-ray beams. In 1963 Willie started looking at this problem in more detail. As one might guess from his history, his careful look at the problem began with a mineral, Hambergite, in which he found his carefully measured intensities to be incompatible with theory. In a reconsideration of the theory, he showed that C. G. Darwin's formula for the secondary extinction correction, which had been universally accepted and extensively used, contained an appreciable error when applied to x-rays. The error was in the treatment of the polarization of the x-ray beam. In 1963 Willie published a first-order approximation for the extinction correction for a mosaic crystal of arbitrary shape. In 1965 he published two more theoretical papers in which he derived more precise formulae for extinction and multiple diffraction. Shortly thereafter he published an experimental test of his new theory using a small quartz sphere. These papers also took into account corrections necessary in highly absorbing crystals for the Borrmann effect. Extinction becomes more serious as the scattered intensity increases. Scattering close to the incident beam is generally the most intense. It is just these low-angle beams which are important in the determination of valence electron distribution. Without a precise method for taking extinction into account, determination of outer electron distributions are unreliable. As Pepinsky said in 1975, these papers on extinction and the Borrmann effect are "a landmark in diffraction theory and broke the dam which had held back structure determination. The pathway is now open to new attacks on the problem of bonding electron distribution in simple structures, to far more accurate complex structure analyses, and eventually to bonding electron structure in these complex structures."

Melting Points

Willie published one paper in 1967, jointly with his friend Matthias and two of Matthias's associates, on "Melting-Point

Anomalies." As these authors said in their paper, "At present there is not even a satisfactory beginning of a macroscopic theory of melting. Speculative discussions, such as those given in this paper, are hence justified because they may suggest directions for fruitful theoretical exploration." Their suggestions involved the "partial f character of the hybridized wave function of the valence band," in essence the effective number of free electrons. Willie and his friend Matthias continued to think about this problem until Willie's death, but published nothing more.

Superconductivity

With his friend Bernd Matthias and some of Matthias's associates, Willie was coauthor of seven papers on superconductivity. During their years together at Chicago (194648), while Matthias was an assistant professor and Willie chairman, they had many fruitful discussions, some of which contributed to Matthias's later correlations of the superconductivity critical temperature to the effective number of valence electrons per atom. In later collaborations, among other things, Willie "interpreted correctly the nonstoichio-metric phases present in superconducting multicomponent mixtures." As these authors conclude, "The position of the elements in the periodic table, the valence electron concentration and the crystallographic structure exhibit a strong influence on the superconducting behavior." Willie's influence is clear.

SUMMARY

Willie's contributions to science, and to the scientific community, have been rich and varied.

In preparing this memoir I have drawn freely from memorials and accolades written by S. Chandrasekhar, A. R. Cooper, J. R. Goldsmith, M. Marezio, B. T. Matthias, P. B. Moore, Linus Pauling, R. A. Penneman, Raymond Pepinsky, D. H. Templeton, and Anthony Turkevitch, and from a recorded conversation between Willie and Edward Wolowiec in which Willie recalls some of his history while in Chicago.

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