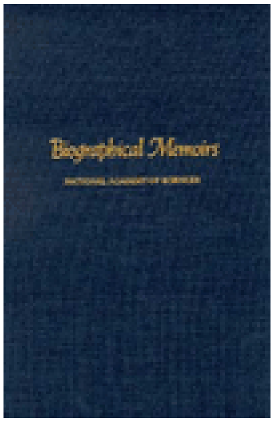

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Biographical Memoirs

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Volume 69

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The National Academy of Sciences was established in 1863 by Act of Congress as a private, non-profit, self-governing membership corporation for the furtherance of science and technology, required to advise the federal government upon request within its fields of competence. Under its corporate charter the Academy established the National Research Council in 1916, the National Academy of Engineering in 1964, and the Institute of Medicine in 1970.

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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.

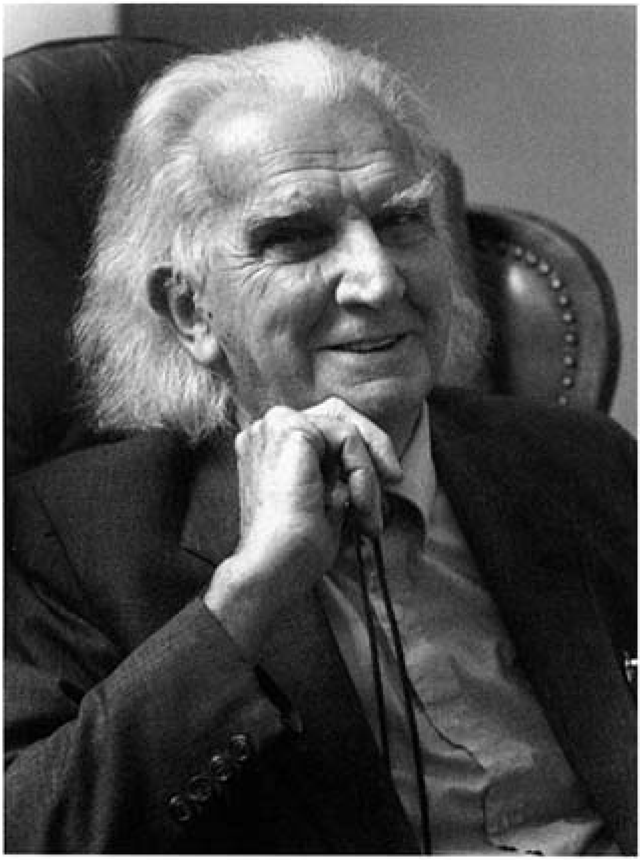
The *Biographical Memoirs*, begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work 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 69



Kenneth Ewart Boulding

January 18, 1910–March 18, 1993

NATHAN KEYFITZ

KENNETH EWART BOULDING—ECONOMIST, man of letters, ardent peace activist—made his contribution to the body of economic knowledge with a combination of humanistic values and technical proficiency. Reading his papers, written from the early 1940s to the 1990s, one sees a mastery and creativity not only of economics but of all the social sciences and ethics as well.

Boulding was born in Liverpool, England, on January 18, 1910, and died in Boulder, Colorado, on March 18, 1993. Five years after taking a degree at Oxford with first-class honors he left for the United States, where he spent the rest of his life, first as a U.S. resident and then as a citizen. Married in 1941, he and his wife, Elise Bjorn-Hansen, were together for the subsequent fifty-two years, in which they saw five children, John Russell, Mark David, Christine Ann, Philip Daniel, and William Frederic, into the world and into professional life. I first met the young couple in 1946, when Kenneth and I were teaching at McGill University in Montreal and Elise was a student in my sociology class. We have kept in touch ever since.

Boulding was awarded honorary doctorates by over thirty universities; he had prizes not only for economics but also for political science, peace research, and scholarship in the humanities. He was, in turn, president of the Society for General Systems Research (1957-59), president of the American Economic Association (1968), president of the International Peace Research Society (1969-70), president of the International Studies Association (1974-75), president of the American Association for the Advancement of Science (1979), and president of the section on economics of the British Association for the Advancement of Science (1982-83). He was a member of the National Academy of Sciences (elected in 1975), the Institute of Medicine, and the American Academy of Arts and Sciences.

He settled first at the University of Michigan in 1949. In 1967 he moved to the University of Colorado at Boulder, where he became distinguished professor emeritus in 1980. But there were many intervals of work and teaching elsewhere in the course of those years. The list of places where he visited for weeks or months is as long as the list of universities that gave him doctorates.

It is impossible to write a biography of Kenneth Boulding without speaking of Elise. They collaborated on many things, but their strongest common interest aside from bringing up their five children was the peace movement.

Few names are mentioned more often than the Bouldings among the founders of the international peace research movement that gained prominence in the 1960s. But Kenneth was also a founder of another movement that came to prominence about that time—systems analysis as a way of unifying the sciences, natural and social. The search for isomorphisms—propositions of the same structure valid in two or more disciplines—was part of what animated it, but many other propositions have turned up as well. And what may well be called a third movement, evolutionary economics, that he wrote about nearly twenty years ago, became prominent in the late 1980s and 1990s. While none of the three movements has quite fulfilled the brilliant prospects of its first youth, all have become a durable part of the academic and extra-academic research scene.

Kenneth Boulding was a judicious statesman of science. He speaks of "economics imperialism," the attempt on the part of economics to take over the other social sciences,[1] and, although his loyalty to his main profession was lifelong and unchallenged, still he didn't like imperialism even here. He was very conscious of the power of economics for dealing with economic problems, but he never tired of pointing out the large areas of life in which its techniques do not apply. He could be critical of features of the modern world that economics has brought into existence through the power of its ideas, just as he could praise the political and other dimensions of freedom that come with a free economy.

Kenneth Boulding would have nothing to do with the idea of a nonnormative social science. In the tradition of the great economists of the nineteenth century, one that has largely disappeared in the twentieth, he was not embarrassed to say of American power,

> Unless we "stand for" something in the world … that power will lose legitimacy and will eventually disappear.[2]

But that does not mean that he ceased to be an economist in dealing with values. Opposing a common ethical view that freedom and justice are absolute, so that we cannot permit any injustice whatever and certainly no infringement on freedom, he spoke of an "ethical market":

> [W]e have to ask ourselves how much justice are we willing to give up for so much liberty.[3]

Crowds in revolution do not think that way but in absolute terms; they are not aware that without compromise the great danger is social disorder. What follows disorder is tyranny—a dictatorship with neither liberty nor justice. A compromise is not perfect, yet only compromise is workable.

Though a leader in several major innovations, Boulding was no enthusiast for new fashions in social science just because they are new:

> One of the elements which easily may lead to a worsening of the decision-making process at present is the increasing fashionableness of gaming … [including] business gaming in the corporation. It can provide illusions of certainty about the future which may turn out to be quite disastrous. … A similar criticism can be leveled at techniques like the Delphi method.[4]

Boulding had many loyal students, who owed much to the inspiration his lectures provided. But the essential Boulding is inimitable. Those sparkling insights that brighten everything he wrote are not a methodology that can be taught by a master and then learned and practiced by others. One goes through some 3,000 densely packed pages in the collected edition of his papers and finds little repetition and in nearly every page striking ideas. Yet Boulding had no ambition to construct a system, a scheme, or schedule that would have a slot for each idea. Neither the profusion of brilliant ideas nor the seeming disorder in which they came forth was to the liking of those fellow professionals who want to standardize the field, to provide a package of knowledge and technique that all the qualified would share and apply, to certify economists as competent to apply that package, so that they will be the physicians to the economy.

That is not the only feature of Boulding that worried some schools of economics. Another was his recognition, especially from the 1970s onward, that growth—meaning increase of consumption without limit—could not possibly be the prime objective of society and the individuals in it.

> [E]conomics has been incurably growth-oriented and addicted to everybody growing richer, even at the cost of exhaustion of resources and pollution of the environment.[5]

In reading through Boulding's work one is astonished at how far he anticipated ideas that were reinvented years later and that many social scientists have not yet tumbled to. Back in 1958 he took up ecological questions:

> Are we to regard the world of nature simply as a storehouse to be robbed for the immediate benefit of man? … Does man have any responsibility for the preservation of a decent balance in nature, for the preservation of rare species, or even for the indefinite continuance of his race?

And even in his early conventional years one detects a note of irony in his couplet:

> The wise economist is loath
> To give up anything for growth.[6]

I cannot believe that Boulding's sole objection to unlimited growth was the ability of the environment to stand it. His deeper objection comes from other sources. Perhaps the same source that made Keynes say that once the economic problem has been solved for all classes the motives (self-seeking, preoccupation with work and production) that achieved this would be seen at their true value, as something to be ashamed of.

> The love of money as a possession—as distinct from the love of money as a means to the enjoyment and realities of life—will be recognized for what it is, a somewhat disgusting morbidity, one of those semi-criminal, semi-pathological propensities which one hands over with a shudder to the specialists in mental disease.[7]

Keynes was optimist enough to believe that this would occur by itself and in this century. Boulding, because he was younger and lived longer, saw further history unfold—including numerous wars, persistent inequalities, injustice of every kind that showed too little tendency to diminish with rising GDP, that made such optimism more difficult for him. The best he could do was to express a hope—far short of a forecast—that we can learn

> to fly the great engine of change … that it may carry us not to destruction but to that great goal for which the world was made.[8]

Yet with a different image his thought here is the same as that of Keynes:

> The spurt [of growth] from the 1930s to the 1960s bears some resemblance to human adolescence, even to the production of a slightly pimply youth culture.[9]

When Boulding discussed progress he distinguished its measurable economic aspect, efficiency in the workplace:

> a rise in the amount of any commodity that can be produced with one man-hour of labor time.[10]

With that definition there is no danger of confusing economic progress with progress overall. We are told again and again throughout his work that as we learn to make things faster we do not necessarily move toward a more satisfying life in a tolerable environment.

It is to be expected that one as rich in ideas as Boulding will be interpreted differently by readers of different generations and differently by professional economists and others. What precedes is an attempt to portray his style of thinking as seen by a nonsectarian social scientist of his own generation.

NOTES

1. K. E. Boulding. *Conflict and Defense: A General Theory*. New York: Harper, 1962. Lanham, Md.: University Press of America, 1988, p. viii.
2. K. E. Boulding. *From Abundance to Scarcity: Implications for the*

American Tradition. Columbus: Ohio State University Press, 1978. Collected Papers, vol. VI, p. 525. Boulder, Colo.: Associated University Press, 1971.

3. K. E. Boulding. Prices and values: infinite value in a finite world. In *Value and Values in Evolution*. New York: Gordon and Breach, 1979. Collected Works, vol. VI, p. 600.

4. K. E. Boulding. Social risk, political uncertainty, and the legitimacy of private profit. In *Risk and Regulated Firms*, ed. R. H. Howard, pp. 82-93. East Lansing: Michigan State University Graduate School of Business Administration, 1973.

5. K. E. Boulding. Toward a modest society: the end of growth and grandeur. In *Economic Perspectives of Boulding and Samuelson*. Durham, N.H.: Whittemore School of Business and Economics, University of New Hampshire, 1971. Collected Works, vol. VI, p. 85.

6. K. E. Boulding. *Principles of Economic Policy*, p. 21. Englewood Cliffs, N.J.: Prentice-Hall, 1958.

7. J. M. Keynes. *Essays in Persuasion*, part V, 1931. Reprinted in *The Collected Writings* vol. IX. London: Macmillan, 1972.

8. K. E. Boulding. *Conflict and Defense*, p. 343. New York: University Press of America, 1988.

9. K. E. Boulding. *Collected Papers*, vol. VI, p. 87. Boulder, Colo.: Associated University Press, 1971.

10. K. E. Boulding. *Principles of Economic Policy*, p. 21. Englewood Cliffs, N.J.: Prentice-Hall, 1958.

Selected Bibliography

1941 *Economic Analysis*. New York: Harper & Row.

1945 *The Economics of Peace*. Englewood Cliffs, N.J.: Prentice-Hall.

1950 *A Reconstruction of Economics*. New York: John Wiley & Sons.

1953 *The Organizational Revolution: A Study in the Ethics of Economic Organization*. New York: Harper.

1958 *The Skills of the Economist*. New York: Clarke Irwin. *Principles of Economic Policy*. Englewood Cliffs, N.J.: Prentice-Hall.

1960 With W. A. Spivey et al. *Linear Programming and the Theory of the Firm*. New York: Macmillan.

1961 *The Image: Knowledge in Life and Society*. Ann Arbor: University of Michigan Press.

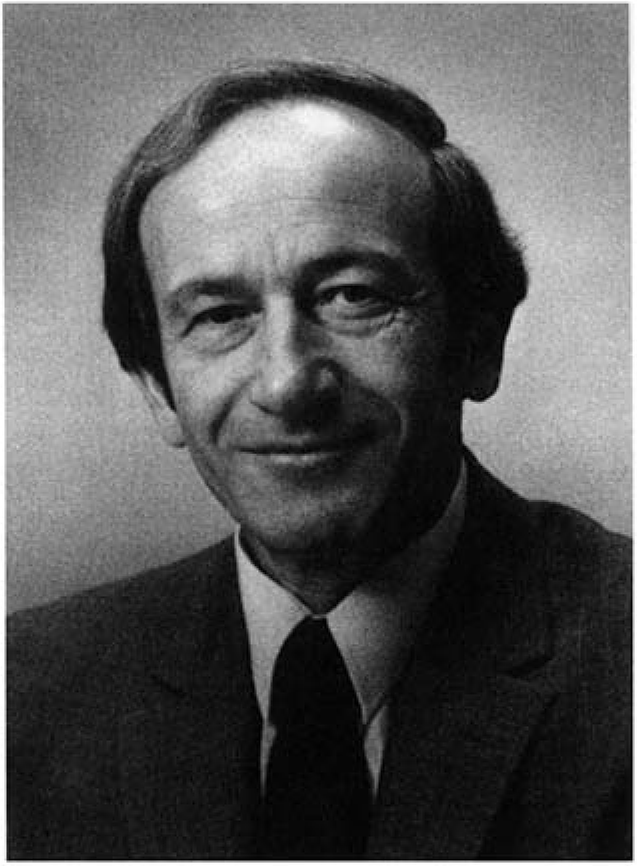
1962 *Social Justice*. Englewood Cliffs, N.J.: Prentice-Hall. *Conflict and Defense: A General Theory*. New York: Harper & Row.

1963 With E. Benoit, eds. *Disarmament and the Economy*. New York: Harper & Row.

1964 *The Meaning of the Twentieth Century: The Great Transition*. New York: Harper & Row.

1968 *Environmental Quality in a Growing Economy*. Baltimore: Johns Hopkins Press.
1970 *Beyond Economics: Essays on Society, Religion, and Ethics*. Ann Arbor: University of Michigan Press. *Economics as a Science*. New York: McGraw-Hill.
1971 *Economics of Pollution*. New York: New York University Press. *Collected Papers*. Boulder, Colo.: Associated University Press.
1973 *The Economy of Love and Fear: A Preface to Grants Economics*. Belmont, Calif.: Wadsworth. *Peace and the War Industry*. New Brunswick, N.J.: Dutton.
1976 *Adam Smith as an Institutional Economist*. Memphis: P. K. Seidman.
1977 With M. Kammen and M. Lipset. *From Abundance to Scarcity: Implications for the American Tradition*. Columbus: Ohio State University Press.
1978 With T. F. Wilson, eds. *Redistribution Through the Financial System: The Grants Economics of Money and Credit*. New York: Praeger.
1980 *Beasts, Ballads and Bouldingisms*. New Brunswick, N.J.: Transactions Publishers.
1981 *Evolutionary Economics*. Beverly Hills, Calif.: Sage Publications. *Ecodynamics: A New Theory of Societal Evolution*. Beverly Hills, Calif.: Sage Publications.

1984 *Economics of Human Betterment*. Albany, N.Y.: SUNY Press.
1985 With others. *Morality of the Market: Religious and Economic Perspectives*. Vancouver, Canada: Fraser Institute.
1986 *Mending the World: Quaker Insights on the Social Order*. Wallingford, Pa.: Pendle Hill.
1992 *Towards a New Economics: Critical Essays on Ecology, Distribution, and Other Themes*. Brookfield, Vt.: Edward Elgar.
1993 *Structure of a Modern Economy: The United States, 1929-89*. New York: Macmillan Press Ltd. (*Economists of the Twentieth Century Series*.)
1994 *Sonnets from Later Life*. Wallingford, Pa.: Pendle Hill.
1995 With E. Boulding. *Future: Images and Processes* Beverly Hills, Calif.: Sage Publications.



Solomon J. Buchsbaum

December 4, 1924–March 8, 1993

KENNETH G. MCKAY

SOLOMON J. BUCHSBAUM IS DEAD. That fact is as shocking now as it was on March 8, 1993. It rattled around academia, government, and the AT&T Bell Laboratories where he was employed for thirty-five years. He was alert and insightful, with a broad range of knowledge. His interests extended from plasma and solid-state physics and nuclear weaponry to demonstrated leadership in determining and promoting public policy in science and technology. As senior vice-president of technology systems at AT&T Bell Laboratories, he was responsible for product realization planning and engineering, government systems, the architectural framework for AT&T products, systems and services, and R&D in support of manufacturing. He was a well-rounded man.

Sol (he was always called "Sol," and this usage will be followed throughout this memoir) was essentially a forward-looking individual; he never looked back. So the facts about his extraordinary early life are largely gleaned from his wife of thirty-seven years, Phyllis Isenman Buchsbaum, to whom I am extremely grateful.

In 1941, two years after the Nazis invaded Poland, Jacob Buchsbaum, Sol's father, was "removed" along with other businessmen in the town of Stryj and never heard from again. Two and a half years later, all the other Jewish residents in the area—including thirteen-year-old Sol; his mother, Berta; and one sister, Judy—were rounded up and jailed. His mother told them that if they had the chance they should run away. His sister refused to leave their mother, and Sol never saw either of them again. But Sol fled barefoot to a factory where his other sister, Dorothy, had been taken. There he obtained some money and supplies and took the train to Warsaw. He found refuge in a Catholic orphanage—no questions were asked. There he recited Mass every week and even became an altar boy.

Without formal schooling, Sol "read and survived." He learned Latin, which proved useful when he later studied French and English. After the war, the Canadian Jewish Congress helped him emigrate to Canada just two weeks before his eighteenth birthday—the age limit for acceptance. Within a year Sol taught himself English with a Polish accent, obtained his high school equivalency, and found work in a hat factory.

Sol was always good with numbers and life in the hat factory was definitely limited, so, thinking that he might become an accountant, he applied for and received a one-year general studies scholarship to attend McGill University in Montreal. Later that year he won a full scholarship in mathematics and physics. McGill's curriculum for honors students was different. After one year of general subjects, the student received one course in physical chemistry, and all the other courses for three years were solely in mathematics and physics. Clearly, this was no hindrance to anyone with Sol's breadth of interests, and he graduated in 1952 with the Anne Molson Gold Medal for Science, Mathematics, and Physics. Professor Gar A. Woonton had established the Eaton Electronics Laboratory at McGill, so Sol continued for a master's degree while working in the lab. At that time, the research in Woonton's group concentrated on the theory and measurement of the electromagnetic fields that exist in an illuminated aperture in a metal screen. Sol did his master's thesis on the EM fields in an elliptical aperture.

With another scholarship, Sol entered MIT aiming at a Ph.D. in physics. Unlike today, the MIT student was very much on his own. I entered MIT with a M.Sc. from McGill in 1939 and found it to be mysterious, not user friendly. Today the student, upon entering, is given sheets of information, is assigned a mentor, and is given assistance in selecting a thesis professor. It is more efficient, although we question whether it builds self-confidence. But clearly it did not affect Sol. Professor Will Allis, aided by Professor Stanford Brown, was studying microwave plasmas in magnetic fields; Sol eagerly plunged in. There followed a long-time fascination with plasmas and the beginning of a substantial outpouring of publications, some experimental, some theoretical, which swelled as he took on plasma research at AT&T Bell Laboratories. Meanwhile, during his first year at MIT, Sol met Phyllis Isenman, a freshman at Simmons College. They were married in 1955 and both graduated two years later. The following year Sol was an instructor at MIT pursuing his favorite research interest.

Let us appreciate what Sol had accomplished up to this point. He had no formal education until he arrived at McGill University. However, within a relatively short time, he accumulated a B.Sc., a M.Sc., and a Ph.D., while later he had over fifty peer-reviewed technical papers to his name.

In 1958 Sol accepted a research position at Bell Laboratories. He intended to stay but a few years and then return to academic research. However, every time the opportunity to leave arose, he found that he was involved with more and more interesting things where he was. So he continued studies in plasma research, branching out into solid-state physics. Within three years he became head of the Solid-State and Plasma Physics Research Department. Research Vice-President Arno Penzias said, "The work that was done in Sol's lab in quantum electronics, which led to lasers and the whole era of lightwave communications, influenced not only Bell Labs, but pretty much the whole world."

Sol left Bell Labs in 1968 to become vice-president of research at Sandia National Laboratories in Albuquerque. Sandia is responsible for the design of the nonnuclear components of our nuclear arsenal, and its research effort lies on the cutting edge of technology. In 1971 Sol returned to Bell Labs and, after a series of promotions, became responsible in 1979 for product realization planning and engineering, government systems and the architectural framework for AT&T products, and systems and services. Again to quote Penzias, "He became what I would call 'Executive Vice-President of Everything Else.' John Mayo (ex-President Mayo) headed the part of Bell Laboratories that served the telephone companies; everything else—computers, information systems (which later became American Bell), the communication products folks who make the phone you buy at Sears these days, government work that dealt with under-water sound, the architecture area—all nestled under Sol. More recently, he tried his hand at new things, like high-definition television."

If all this action were not enough, Sol expanded his interests into the scientific and political life of the nation. He was an associate editor of *Physics of Fluids* (1963-66), *Journal of Applied Physics* (1968-70), and the prestigious *Review of Modern Physics* (1968-76). His participation in professional societies was impressive. For example, in 1973 he was elected to membership in the National Academy of Engineering and in 1975 to the National Academy of Sciences—the top scientific and engineering bodies in the country. Also in 1975 he became a fellow of the American Academy of Arts and Sciences, the American Association for the Advancement of Science, and the American Physical Society. In 1972 he became a senior fellow of the Institute of Electrical and Electronics Engineers; in 1968 he was chairman of the American Physical Society's Division of Plasma Physics and a member of its council in 1973.

As far as universities are concerned, Sol was a member of the School of Engineering Advisory Board of Stanford University, which took the board's recommendations very seriously. At MIT he was a member of the Lincoln Laboratory Advisory Board, the Corporation Development Committee, and the Physics Visiting Committee. He was also on the board of two non-profits: the Rand Corporation (1982-) and the Charles Stark Draper Laboratories (member, 1983-; director, 1984-). I was chairman of Draper Laboratories when Sol joined the board with a minimum of arm twisting. He was a most effective member—always asking pertinent and sometimes embarrassing questions and at the same time having relevant answers.

For all of these activities, Sol was given substantial public recognition. As mentioned earlier, he received the Anne Molson Gold Medal upon graduation from McGill University. In 1987 he was given the IEEE's Frederik Philips Award. In 1977 he received the Medal for Outstanding Public Service from the Secretary of Defense, and four years later the Award for Exceptional Public Service from the Secretary of Energy. In 1986 President Reagan presented him with the National Medal of Science.

Much of Sol's work never appeared in print. Five U.S. presidents and their administrations benefited from Sol's personal scientific and technological counsel during the past three decades. In addition to his rigorous service on more than a dozen advisory councils concerned with defense, energy, and the state of American science, Sol was frequently called on by congressional committees to comment on matters of technology. His expert testimony included such recent subjects as the role of American corporations in supplying high-definition television systems, research into high-critical-temperature superconductivity materials, and the health of the semiconductor industry in the United States. His promotion of better understanding of technology in the government was matched by his active coordination of communication between industrial, university, and government policymakers, who knew him best for his twenty-two years of service on the Defense Science Board, where he was chairman from 1972 to 1976.

An overview of Sol's national advisory activities follows.

- *Energy Research and Development Advisory Council's Fusion Power Coordinating Committee*. Sol served as a consultant on this committee from 1972 through 1978. The council was established to provide advice on the overall direction of the federal energy research and development effort; to suggest new energy research and development programs, including technical approaches that may contribute to the solution of energy-related problems; and to examine specific recommendations by federal agencies regarding energy-related research and development.
- *Naval Research Advisory Committee*. Sol served on this committee from 1978 through 1981. The committee is the senior scientific advisory group to the Secretary of the Navy, Chief of Naval Operations, Commandant of the Marine Corps, Chief of Naval Research, and the Chief of Naval Development. It is the task of the committee to know the problems of the Navy and the Marine Corps, to evaluate the current

solutions, and to suggest appropriate means for improvement.

- *White House Advisory Group on Technical Advances*. Sol was a member of this group from 1975 to 1976.
- *President's Science Advisory Committee*. Sol functioned on this committee from 1970 until it ceased to exist in 1973. The committee advised the president on matters relating to science and technology, and it developed a national policy on space in light of the Russian Sputnik launchings.
- *Energy Research Advisory Board*. Sol chaired this board from 1978 to 1981 and continued as a member until his death. The board consists of twenty-five members who represent a cross-section of industry, local and state governments, and utility commissions, as well as residential, commercial, and industrial users. It advises the Secretary of Energy on energy policy and on scientific and technical matters of interest to the Energy Department. Specifically, the board provides advice on overall research and development being conducted in the department and provides long-range guidance in these areas.
- *Defense Science Board*. Sol chaired this board from 1972 through 1976 and was a senior consultant since 1978. The board consists of 150 civilian members representing the industrial, academic, and scientific communities. It advises the Secretary of Defense and the director of defense acquisitions on overall scientific and technical research and engineering and provides long-range guidance in these areas.
- *White House Science Council*. Sol chaired this council since its inception in 1982. Its thirteen members advise the director of the Office of Science and Technology Policy on science and technology issues of national concern. The council studies issues assigned by the director to keep him informed of changing perspectives in the science and technology communities.

- *President's Council of Advisors on Science and Technology*. President Bush named Sol to membership in this group in 1990.

The other side of these extraordinary activities was that Sol was often late for scheduled meetings and was difficult to locate. However, a very efficient secretary eased this situation; he tried to be and, usually was, accessible.

Sol's other world was his home, family, and synagogue. In addition to his wife, Phyllis, and sister, Dorothy, he had three children—Rachel, David, and Adam, and three grand-children, Hannah, Jacob, and Joshua. He was an avid tennis player; he went west for a skiing vacation every spring; and he was competitive at the bridge table. This enthusiastic and competitive spirit permeated Sol's entire life except with his family. There he relaxed and showed a softer side of his personality, that of a loving husband and father. While he was often asked to relate his wartime experiences, he preferred not to dwell on the painful part but rather to enjoy the present and anticipate the future.

Now we come to the unusual circumstances concerning his death, an event that was anticipated yet unexpected. Several years before he died, a routine examination by the Bell Laboratories doctor disclosed that Sol had developed multiple myeloma, a cancer of the bone marrow. He underwent treatment while, in typical fashion, he performed just as usual without disclosing the situation to anyone but his immediate family. However, his condition worsened, until finally he had to make the ultimate decision concerning treatment: whether to undergo a bone marrow transplant. Sol, as usual, agreed to proceed. After the arduous treatment, he had to remain for a month in isolation until his white corpuscle count regenerated. Meanwhile, he was equipped with a telephone, a facsimile machine, and all the equipment that enabled him to carry on "business as usual." In fact, I attended a meeting in the auditorium in Holmdel, New Jersey, at which Sol was expected to participate—and so he did, via a microphone and loudspeaker system while in his Boston germ-free room. He sounded as cheerful and as smart as ever. Finally, he was released. His platelet count was still far below normal, but it was increasing. He was overjoyed and continued to work at home. However, during the post-transplant period, he died suddenly of complications. It seemed particularly ironic that, after he had survived so much, the end came just as he was celebrating another victory.

Sol left behind eight patents, over fifty technical publications, a large hole in governmental advisory committees, an unusually large range of friendships, and a loving family. He is sorely missed.

I WISH TO EXPRESS my appreciation to John S. Mayo, Ian M. Ross, A. Penzias, and Dan van Atta of AT&T Bell Labs, whose words I have woven into this memoir.

Selected Bibliography

1957 With S. C. Brown. Microwave measurements of high electron densities. *Phys. Rev.* 106:196-99.

1960 Resonance in a plasma with two ion species. *Phys. Fluids* 3:418-21. Ion resonance in a multicomponent plasma. *Phys. Rev. Lett.* 5:495-97. With L. Mower and S. C. Brown. Interaction between cold plasmas and guided electromagnetic waves. *Phys. Fluids* 3:806-20.

1961 With E. I. Gordon and S. C. Brown. Experimental study of a plasma column in a microwave cavity. *J. Nucl. Energy, Pt. C, Plasma Phys.* 2:164-68. With P. M. Platzman. Effect of collisions on the Landau damping of plasma oscillations. *Phys. Fluids* 4:1288-93. With J. K. Galt. Alfven waves in solid-state plasmas. *Phys. Fluids* 4:1514-17.

1962 With W. P. Allis. Coupling between electromagnetic and electron waves in a plasma. *Nucl. Fusion* 2:49-53. With others. Containment of plasmas by high frequency fields. *J. Appl. Phys.* 33:2429-34. With L. Mower. Interaction between cold plasmas and guided electromagnetic waves. II. *Phys. Fluids* 5. With G. E. Smith. Microwave induced a.c. voltage in bismuth. *Phys. Rev. Lett.* 9:342-43.

1963 With G. E. Smith and L. C. Hebel. Hybrid resonance and "tilted-orbit" cyclotron resonance in bismuth. *Phys. Rev.* 129:154-68. With W. B. Cottingham. Electron ionization frequency in hydrogen. *Phys. Rev.* 130:1002-6. With G. A. Baraff. Anisotropic electron distribution and the dc and microwave avalanche breakdown in hydrogen. *Phys. Rev.* 130:1007-19. With P. M. Platzman. Transmission of electromagnetic waves through plasma slabs. *Phys. Rev.* 312:2-9.

1964 With P. M. Platzman and N. Tzoar. Light-off-light scattering in a plasma. *Phys. Rev. Lett.* 12:573-75. With C. S. Roberts. Motion of a charged particle in a constant magnetic field and a transverse electromagnetic wave propagating along the field. *Phys. Rev.* 135:A381-89.

1965 With A. G. Chynoweth and W. L. Feldman. Microwave emission from indium antimonide. *Appl. Phys. Lett.* 6:67-69. With W. B. Cottingham. Diffusion in a microwave plasma in the presence of turbulent flow. *J. Appl. Phys.* 36:2075-78. With G. A. Baraff. Surface wave instability in helicon wave propagation. *Appl. Phys. Lett.* 6:219-21. With P. A. Wolff. Effect of open orbits on helicon and Alfven-wave propagation in solid-state plasmas. *Phys. Rev. Lett.* 15:406-9.

1966 With A. G. Chynoweth and W. L. Feldman. Low-field microwave emission from indium antimonide. *J. Appl. Phys.* 37:2922-24. With A. Hasegawa. Longitudinal plasma oscillations near electron cyclotron harmonics. *Phys. Rev.* 143:303-9. With G. A. Baraff. Surface-wave instability in helicon-wave propagation. *Phys. Rev.* 144:266-76.

1967 With P. M. Platzman. Nonlocal damping of helicon waves. *Phys. Rev.* 154:395-98.

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Albert Hewett Coons

June 28, 1912–September 30, 1978

HUGH O. MCDEVITT

ALBERT COONS, professor in the Department of Bacteriology and Immunology at Harvard Medical School and a member of the National Academy of Sciences since 1962, died September 30, 1978, at the age of sixty-six. He was born in Gloversville, New York, on June 28, 1912, the son of Albert S. and Marion (Hewett) Coons. He was educated in Gloversville public schools, graduated from Williams College in 1933, and received the M.D. degree from Harvard Medical School in 1937.

He initiated a major revolution in immunology and cell biology that continues to this day by developing the immunofluorescent technique for labeling specific antibodies with fluorescent dyes, thus permitting the detection of antibodies, antigens, and virtually any antigenic protein in cells and tissues. Fluorescent and immunohistochemical localization of foreign and self-proteins through the use of labeled antibodies is now an indispensable research tool in almost every field of biomedical research and continues to contribute to major new discoveries in all of these fields. The development of the fluorescent antibody technique in the early 1940s was a technical tour de force that required Dr. Coons to use the techniques of protein chemistry and organic chemistry. However, the impetus for the develop ment of this technique came from his medical training and his interest in the pathogenesis of rheumatic fever.

Following his graduation from Harvard Medical School, Dr. Coons was a house officer on the Medical Service at the Massachusetts General Hospital from 1937 to 1939. In 1939 he joined the Thorndike Memorial Laboratory at Boston City Hospital as an assistant resident in medicine. Having completed his clinical training, Dr. Coons began a research fellowship in the Department of Bacteriology and Immunology at Harvard Medical School, where he remained from 1940 to 1942. His research studies were interrupted by service as a captain, and later as a major, in the Army Medical Corps, where he served as a pathologist and director of laboratory services with the 105th General Hospital in the southwest Pacific.

He returned to the Department of Bacteriology and Immunology as a research fellow in 1946, became an instructor in 1947, and finally a visiting professor of bacteriology and immunology and a career investigator of the American Heart Association in 1953. He was appointed a professor of bacteriology and immunology in 1970 and became a professor in the Department of Pathology in 1971.

As is true of many biomedical researchers, Dr. Coons's interest in research and immunology was originally stimulated by exposure to a bright, dynamic, and articulate teacher and researcher—in this case, Hans Zinsser, who was professor of bacteriology and immunology at Harvard Medical School from 1925 to 1940. Dr. Coons took Dr. Zinsser's course in immunology, which stimulated him to work during the summer of 1935 with John Enders, who was then an assistant professor in the department. His research project was to attempt to determine the blood levels of passively administered antibody before and after the induction of anaphylactic shock in the guinea pig. The precipitin reac tion was used to measure the levels of passively administered rabbit and horse antibodies in guinea pig blood. The aim of the experiment was to find out why horse antibodies, in contrast to rabbit antibodies, were incapable of sensitizing guinea pigs for anaphylactic shock. The problem was never finished, and no clear results emerged from these studies. Nonetheless, Dr. Coons had become intimately acquainted with the use of antibodies to detect foreign proteins in host tissue fluids. As he said much later, "Somehow, though, it bent the twig." This knowledge of the use of antibodies to detect foreign proteins in host fluids and tissues lay dormant through the next two years of medical school and through two years as a house officer at the Massachusetts General Hospital.

The next, and crucial, stage in the development of the concept that led to the development of the fluorescent antibody technique is best described in Dr. Coons's own words, since they offer fascinating insights into how important concepts develop in the mind of an investigator:

> At the end of my internship I had a six months' gap before my next appointment as an Assistant Resident and I was lucky enough to spend them in Berlin. This was the summer of 1939, a period of great international excitement, just before the outbreak of the war. I did not go there as a student but as a tourist. However, I had an entrée into the pathological institute at the Charite Krankenhaus where a friend, Kurt Apitz, was the Oberartz. I spent my mornings watching autopsies, and my afternoons wandering around talking to people in cafés and trying to improve my halting German. I also had many talks with Apitz, who was an exceptional young pathologist interested in leukemia and the Schwartzman reaction.
>
> In strange cities, visitors have many hours alone. I found myself walking in the streets or sitting in my room reading or brooding. One afternoon I was thinking about rheumatic fever and about the Aschoff nodule, the microscopic lesion characteristic of it. It was at that time and I think probably in many circles still is, thought to be the result of a local hypersensitivity reaction involving components of the group A hemolytic strepto

> coccus and circulating antibodies or hypersensitive cells. It struck me that this theory had never been tested and indeed could not be tested without the demonstration of antibody or antigen, preferably both, in the local lesions. I considered that it might be easier to find the antigen than the antibody, for a start anyway, and that what was required was a visible microprecipitate. The notion of labeling an antibody molecule with a visible label was perfectly obvious in such a context. However when I tried this notion on my friend, Apitz, he was not enthusiastic. I think he thought it was not feasible and indeed, in the terms in which I initially thought of it, as a colored molecule, it wasn't.

Dr. Coons's remark that the idea of putting a visible label on an antibody molecule was perfectly obvious was perhaps too modest. Given the primitive knowledge of the structure, nature, isolation, and manipulation of antibodies in 1939, the concept of putting a visible label on an antibody molecule seems both bold and original, even if technically naive. The technical problems, which might have stymied many young researchers, were to occupy the next several years.

At this critical stage in his career Dr. Coons received both support and encouragement from Dr. George Minot, director of the Thorndike Memorial Laboratory, and from John Enders of the Department of Bacteriology and Immunology at Harvard. Both urged him to pursue the problem and to apply for a research fellowship instead of continuing with clinical studies, and argued that even if labeled antibodies did not solve the problems of rheumatic fever, they would provide a general procedure for locating proteins in tissues and cells that would obviously have wide application to countless problems.

Having received this initial support and encouragement, Dr. Coons was fortunate to receive further encouragement and expert assistance from a number of researchers at Harvard University. Louis Fieser, professor of organic chemistry at Harvard, introduced Dr. Coons to Hugh Creech and Norman Jones, who were working in his department on the conjugation of isocyanates of various carcinogens to protein molecules. Since Dr. Coons had already determined that coupling of dyes to antibody molecules resulted in immunoprecipitates that were only faintly pink even in solution, he urged Creech to help him couple a fluorescent dye, anthracene isocyanate, to some antipneumococcal antiserum. This antibody solution agglutinated specific pneumococci, and the agglutinated bacteria were brilliantly fluorescent in ultraviolet light. At this point, primarily because many tissues showed blue or red autofluorescence, Dr. Coons asked Dr. Fieser if he could obtain or synthesize fluorescein isocyanate. Fluorescein was chosen because it fluoresces with a brilliant apple green color not seen in any normal tissues. Dr. Fieser assigned a graduate student, Ernst Berliner, to this synthetic organic problem, and Berliner and Coons became fast friends. During this period Coons learned how to synthesize fluorescein isocyanate, knowledge that was invaluable in later years.

The next step was to visualize antigen in tissue sections. To do this, a fluorescent microscope was needed. Once again, by another stroke of luck, a colleague, Dr. Allan Grafflin, assistant professor of anatomy, was engaged in the assembly of an apparatus for fluorescence microscopy. Using his single fluorescent antipneumococcal antibody conjugate, Coons was able to find bacterial polysaccaride in the phagocytic cells of mice injected intravenously with large numbers of pneumococci and to carry out inhibition reactions for the establishment of specificity and to show that these reactions were reversible. Thus, by the beginning of 1942, Coons had succeeded in demonstrating the feasibility of putting fluorescent tags on antibodies and using them to localize foreign antigens in host tissues. His initial results were described in two brief papers (1941, 1942) and the research was halted while he joined the army and spent the next four years in the South Pacific.

Much work remained to be done before the fluorescent antibody method could be generally applicable to a wide variety of biological problems. When Dr. Coons returned to the Department of Bacteriology and Immunology in 1946, he found that no one in Fieser's Department of Organic Chemistry was interested in synthesizing fluorescein isocyanate. He therefore decided to synthesize his fluorescent compounds himself, a process that was slow and painful. With sufficient fluorescein isocyanate available, many of the other technical problems were tackled. With his colleagues Gene Connolly and Melvin Kaplan, Coons began studying frozen sections and discovered the problem of nonspecific staining of tissues by fluorescent-labeled antibody solutions. This led to the use of acetone-dried tissue extracts as an absorbing agent to remove nonspecific staining.

With the availability of frozen sections and antibody solutions that did not bind nonspecifically to tissues, the fluorescent antibody technique became widely applicable to many problems in immunology and cell biology. This led to a series of papers on the localization of a variety of antigens in animal tissues (1950,1-4;1951) and on the localization of viral antigens in infected tissues. These crucial papers had a major impact on immunology and related fields. The demonstration that virtually any protein could be localized in tissues by the fluorescent antibody method led to its use in many laboratories around the world by immunologists, microbiologists, pathologists, and cell biologists.

The fluorescent antibody technique permitted the study of the fate of antigens in tissues, the expression of many different cell proteins in tissues, the identification of cells producing specific antibodies, detection of immune complexes and complement in lesions of serum sickness and in glomerulonephritis, the location of viral antigens in infected cells and of tumor antigens in malignant cells, and the use of the fluorescent antibody method in a wide variety of experimental settings.

At the same time, Dr. Coons embarked on a study of mechanisms of antibody production and developed methods for detecting specific antibody in cells producing that antibody in tissue sections of spleen and lymph nodes (1955,1). This led to a series of studies on antibody production (1955,2) and on specific inhibition of antibody formation during immunological paralysis (1959). These studies, showing sharply localized clusters of plasma cells all producing antibody of the same specificity, both foreshadowed and supported the clonal selection theory of antibody formation.

Dr. Coons was among the first to perceive that a detailed understanding of the mechanisms of antibody formation would require the development of techniques for the study of in vitro antibody production. This led to a series of papers from his laboratory on the establishment and analysis of in vitro secondary antibody responses. These studies were among the first to establish reproducible secondary antibody responses in vitro, to demonstrate the critical role of early cellular proliferation in the in vitro secondary response, and to permit analysis of the effect of a wide variety of drugs, including chloramphenicol and colchicine (1963,1-3).

As the importance of the immunofluorescent technique became apparent, Dr. Coons was widely honored by his colleagues. He received the Lasker Award in 1959, the Paul Ehrlich Award in 1961, the Passano Award in 1962, the Gairdner Foundation Annual Award in 1963, and the Emil von Behringer Prize in 1966, as well as honorary Sc.D. degrees from Williams College, Yale University, and Emory

University. He is survived by his wife, Phyllis (Watts); his son, Albert H., Jr.; and four daughters, Elizabeth, Susan, Hilary, and Wendy.

A shy, bright, articulate, and gentle man with a wonderful, if private, sense of humor, Dr. Coons began his career with the idea of becoming a clinician and perhaps a teacher in a medical school. Stimulated by Zinsser and Enders to study immunology, and pondering the problems of rheumatic fever in a small hotel room in Berlin, he developed the bold and elegant idea of putting a sensitive visible label on antibody molecules. Given encouragement by mentors and help from colleagues, he carried through all of the technically difficult steps required to take an elegant but impractical idea into the realm of everyday application. Although acutely aware of the endless possibilities opened up by his technique, he nonetheless remained a little amazed and perhaps a bit embarrassed at how widely successful his method became. He left a legacy that will only grow with the passing years, as well as advice to his colleagues that is also a description of his own research methods:

> Imaginative approaches and the fruitful association of apparently unrelated phenomena are the result of indirection, brooding, indolence. In the beginning a store of facts and methods—in the end the free hand. (1961)

Selected Bibliography

1941 With H. J. Creech and R. Jones. Immunological properties of an antibody containing a fluorescent group. *Proc. Soc. Exp. Biol. Med.* 47:200.

1942 With H. J. Creech, R. N. Jones, and E. Berliner. The demonstration of pneumococcal antigen in tissues by the use of fluorescent antibody. *J. Immunol.* 45:159.

1950 With M. H. Kaplan. Localization of antigen in tissue cells. II. Improvements in a method for the detection of antigen by means of fluorescent antibody. *J. Exp. Med.* 91:1. With M. H. Kaplan and H. W. Deane. Localization of antigen in tissue cells. III. Cellular distribution of pneumococcal polysaccharides types II and III in the mouse. *J. Exp. Med.* 91:15. With J. C. Snyder, F. S. Cheever, and E. S. Murray. Localization of antigen in tissue cells. IV. Antigens of rickettsiae and mumps virus. *J. Exp. Med.* 91:31. With A. G. S. Hill and H. W. Deane. Localization of antigen in tissue cells. V. Capsular polysaccharide of Friedlander bacillus, type B in the mouse. *J. Exp. Med.* 92:35.

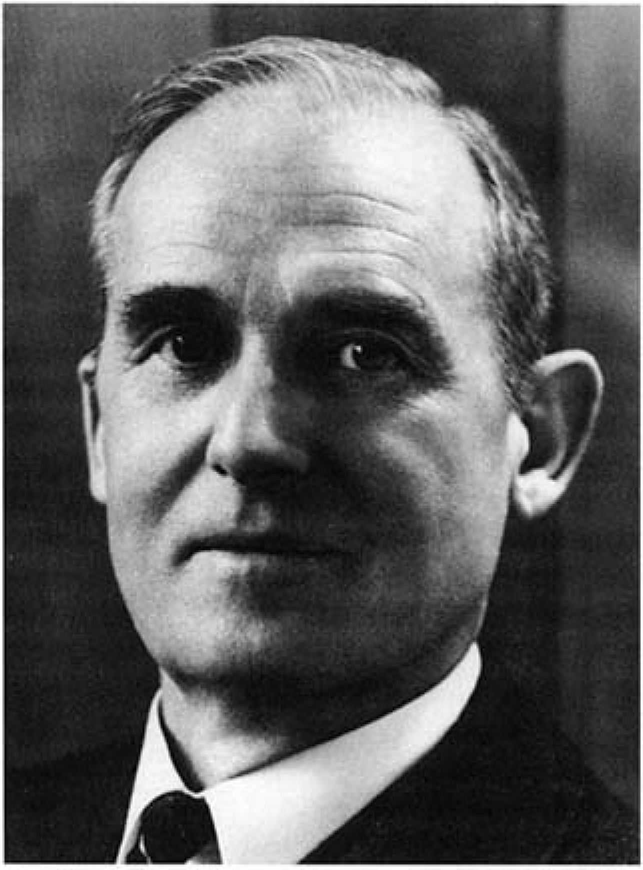
1951 With E. H. Leduc and M. H. Kaplan. Localization of antigen in tissue cells. VI. The fate of injected foreign proteins in the mouse. *J. Exp. Med.* 93:173.

1955 With E. H. Leduc and J. M. Connolly. Studies on antibody production. I. A method for the histochemical demonstration of specific antibody and its application to a study of the hyperimmune rabbit. *J. Exp. Med.* 102:49. With E. H. Leduc and J. M. Connolly. Studies on antibody production. II. The primary and secondary responses in the popliteal lymph node of the rabbit. *J. Exp. Med.* 102:61.

1959 With E. Sercarz. Specific inhibition of antibody formation during immunologic paralysis and unresponsiveness. *Nature* 184:1080.

1961 The beginnings of immunofluorescence. *J. Immunol.* 87:499.

1963 With M. C. Michaelides. Studies on antibody production. V. The secondary response *in vitro. J. Exp. Med.* 119:1035. With T. F. O'Brien. Studies on antibody production. VII. The effect of 5-bromodeoxyuridine on the *in vitro* anamnestic antibody response. *J. Exp. Med.* 119:1063. With C. T. Ambrose. Studies on antibody production. VIII. The inhibitory effect of chloramphenicol on the synthesis in antibody in tissue culture. *J. Exp. Med.* 119:1075.



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William Francis Giauque

May 12, 1895–March 28, 1982

KENNETH S. PITZER AND DAVID A. SHIRLEY

WILLIAM FRANCIS GIAUQUE IS remembered particularly for his discovery of adiabatic demagnetization as a means to reach very low temperatures as well as for his exhaustive and meticulous thermodynamic studies, over a lifetime of research, which utilized the third law of thermodynamics while also developing a large body of evidence for its validity. His "achievements in the field of chemical thermodynamics and especially his work on the behavior of matter at very low temperatures and his closely allied studies of entropy" were cited by the Nobel Committee for Chemistry in the award of the prize in 1949.

Giauque was born May 12, 1895, in Niagara Falls, Ontario, Canada, the eldest of two sons and one daughter of William Tecumseh Giauque and Isabella Jane (Duncan) Giauque. His father was an American citizen, and thus William Francis Giauque was able to adopt American citizenship although born in Canada. Neither of Giauque's parents completed a formal high school education, but both were convinced of the value of education. His father was a skilled carpenter and cabinetmaker and was adept at mechanical procedures in general. He was employed variously as a weighmaster and station agent for the Michigan Central Railroad.

Giauque's mother was skilled in sewing and tailoring and worked in those occupations on occasion.

His father died when Giauque was thirteen, leaving the family with meager financial resources that had to be supplemented from part-time and summer jobs by all members. Among these jobs was part-time seamstress work by the mother for the family of Dr. John Woods Beckman, assigned to Niagara Falls by his employer, American Cyanamid Company. This connection had a pivotal role in William Francis Giauque's later education and career.

To his mother's consternation, Giauque made a youthful, headstrong decision upon entering high school that he would prepare for gainful employment as soon as possible; he elected the two-year business course rather than the five-year college-preparatory course. Unable to change Giauque's mind and distraught that he would forego a college education because of financial pressure, Mrs. Giauque enlisted the help of Mrs. (Gertrude Wheeler) Beckman. Giauque often described to his students the long walk he took with Mrs. Beckman in the course of which she contrasted for him the experience of her brothers. One had foregone a college education; a second, Charles Stetson Wheeler, graduated from the University of California with the class of 1884, had a highly successful career as an attorney, and served as a regent of the university from 1902 to 1907 (and later from 1911 to 1923). Giauque switched to the college-preparatory curriculum, with electrical engineering as his goal. His search for employment upon graduation from high school led Giauque, by chance, to the Hooker Electrochemical Company, in Niagara Falls, New York, where his new fascination with chemistry changed his career goal from electrical to chemical engineering. His supervisor, Mr. Burr H. Ritter, assisted this change by answering his questions about chemistry whether they were related to the work or not, and he fully supported Giauque's decision to leave Hooker after two years to continue his education.

Mr. Ritter also promptly and permanently changed Giauque's nickname from Frank to Bill, at least among chemists, to avoid confusion with another employee. The thought of his having a nickname assigned by someone else would have amazed his students, who knew him in later years as "Giauque" and addressed him as "Professor Giauque." They realized that he was called Frank by his family and Bill by his peers, but his stern demeanor and his practice of always addressing *them* by their last names, unadorned by modifiers, discouraged experimentation along these lines on their part.

Again the Beckmans were to play a key role in determining Giauque's future direction. While Giauque worked at Hooker, Dr. Beckman, himself an electrochemical engineer with American Cyanamid, had been transferred to Berkeley. When Giauque's mother wrote to Mrs. Beckman of Giauque's decision to enter chemical engineering, Mrs. Beckman wrote back about her husband's admiration for the work that G. N. Lewis and his colleagues, J. H. Hildebrand, W. C. Bray, and others, were doing at the University of California. Giauque had been considering the Massachusetts Institute of Technology and Rensselaer Polytechnic Institute, but Lewis's scientific reputation, the pleasant climate, and the fact that there was no tuition, even for out-of-state students, at that time and only a total of $10 per semester in fees combined to persuade him to move to Berkeley in August 1916 and enroll at the University of California.

Giauque thus began an association with the College of Chemistry, University of California, that lasted for the remaining sixty-six years of his life, as undergraduate, graduate student, faculty member, and professor emeritus, unin terrupted by sabbatical leaves and with few trips made for any purpose except to receive major awards. He also persuaded his family to move to Berkeley in 1919 when his brother and sister were ready to enter the university.

Motivated in part by the need to support himself through part-time work throughout his student years, Giauque led a life style even as an undergraduate that was orderly in the extreme. He neither smoked nor drank alcoholic beverages, by preference. Although while in high school he rarely passed up an opportunity to play basketball, as an undergraduate time constraints limited his participation in sports to the boxing team during his freshman year. He also decided at an early age to read no more fiction, regarding time thus spent as wasted.

As an undergraduate Giauque continued his interest in engineering as well as chemistry, and he received very substantial engineering training that served him well in planning and carrying out his later scientific work. However, the faculty assembled by G. N. Lewis soon stimulated his primary interest toward fundamental research. During his senior year Giauque pursued low-temperature research on the third law of thermodynamics under the direction of G. E. Gibson —research that was to evolve into his life's work.

After graduation in 1920 with a B.S. in chemistry (with highest honors), Giauque was awarded a university fellowship to continue his education, earning a Ph.D. in chemistry in 1922, with a minor in physics. His thesis research, also supervised by G. E. Gibson, was on the heat capacity of glycerol. It showed that the third law of thermodynamics cannot be applied directly to the disordered systems known as glasses. In later life, Giauque liked to point out to his students the four-year gap between the publication of his thesis work in 1923 and his next publication, as proof by example that a large number of early publications are not essential for an academic career with tenure, if one's senior colleagues have enough patience.

Giauque was informed by G. N. Lewis early in 1922 that he would be offered a faculty appointment upon completion of his Ph.D. work, and he weighed this offer for several months before accepting. He had planned to apply the fundamental science that he had learned to engineering problems, and he also had done no teaching in his two-year tenure as a graduate student, having been able to devote all his available time to his studies for the first time since entering college. The excellent research atmosphere in the College of Chemistry prevailed and Giauque accepted the offer, although his interest in engineering persisted throughout his career and was often expressed by his tendency to do research on a pilot-plant scale. He designed and supervised the construction of the heavy equipment for the liquefaction of both hydrogen and helium, as well as for the production of the high, uniform magnetic fields needed for his research. He was registered as a professional engineer in the state of California.

During his graduate studies and in his early days as a faculty member, Giauque interacted extensively with Raymond T. Birge of the physics department. He thus acquired an understanding of the applicability of quantum statistics to the calculation of thermodynamic quantities, in particular calculation of the absolute entropy of any gas of diatomic molecules from spectroscopic data. Giauque realized that this would provide an absolute reference with which he could compare calorimetric values of entropy, thus achieving a more definitive test of the third law of thermodynamics than had previously been possible.

His study of the spectra of diatomic molecules led to the discovery of the isotopes of oxygen. While the spectra of ^{16}O–^{16}O gave a calculated entropy in agreement with the calorimetric measurements, some faint lines in the oxygen spectrum remained unexplained. With typical thoroughness, he explored several possibilities, concluding finally that an isotopic molecule $^{16}O–^{18}O$ would exhibit the unexplained lines. The world authority on isotopes, Aston, had studied oxygen with a mass spectrograph. He asserted that only ^{16}O existed and thus that oxygen was an ideal atomic weight reference. Undaunted by Aston's authority, Giauque calculated the frequencies expected for the $^{16}O–^{18}O$ molecule and found agreement with the unexplained faint lines. However, his calculations predicted a number of additional lines that were not included in the data reported by Dieke and Babcock, whose spectra he was using. These authors had not reported faint lines that did not lie close to strong ones, believing that they were not associated with oxygen. At Giauque's request, Babcock provided the unreported lines, most of which agreed with Giauque's predictions. Further study identified the ^{17}O isotope as well. The discovery of the oxygen isotopes provided the first clear proof that molecules retain zero-point vibrational energy at absolute zero temperature. It also revealed that physicists and chemists had unknowingly been using different atomic weight scales, a situation that persisted until the ^{12}C scale was adopted in 1961.

Giauque's discovery of adiabatic demagnetization was a consequence of his broad scientific interests as well as his keen and innovative mind. In the fall of 1924 another young colleague, Nelson W. Taylor, invited Giauque to join him in developing a seminar on magnetism, which Taylor was studying. Giauque agreed to present anything he could learn about the relationships of thermodynamics with magnetism. After following several lines of investigation in which the small effect of magnetism on total energy led to uninteresting results, Giauque came across a report from Leiden on the low-temperature magnetic susceptibility of $Gd_2(SO_4)_3 \cdot 8H_2O$. Because of the eightfold spin degeneracy of the Gd^{+3} ion, a large residual entropy remained in this salt, in the absence of a magnetic field, even at very low temperatures. Applying the thermodynamic equations that he had just developed, Giauque found that readily available magnetic fields could remove very substantial amounts of entropy from this or similar systems at very low but currently accessible temperatures. Given his engineering training, it was natural for him to associate entropy changes with heat engines and refrigerating machines, and adiabatic demagnetization became obvious to him as a means for achieving lower temperatures than those available by the conventional use of cryogenic liquids.

Giauque shared his idea for magnetic cooling, conceived late in 1924, freely with his colleagues and with visitors to Berkeley from European laboratories, and he published it in 1927, but over eight years passed before Giauque and MacDougall, his student, carried out the first adiabatic demagnetization experiment in March 1933. The Berkeley laboratory was ill equipped to conduct the experiment when it was conceived, lacking a helium liquefier and an air-core magnet, which would be required for meaningful measurements of the final low temperatures reached upon demagnetization. Characteristically, Giauque set out on a long-range program to develop the necessary equipment. Although he had the support of G. N. Lewis and W. M. Latimer, who favored him in the allocation of scarce resources, there was very little money available for research. Colleagues at better-funded low-temperature laboratories in Europe could have carried out the experiment earlier, but perhaps they lacked Giauque's conviction. It was in 1933 that all of the equipment was completed and Giauque's first demagnetization experiment yielded 0.25 K.

In addition to lesser honors, Giauque was elected to the National Academy of Sciences in 1936 and to the American Philosophical Society in 1940. He received the Nobel Prize in chemistry in 1949.

Muriel Francis Ashley, who earned a B.S. in chemistry at the University of California in 1922, had been a longtime friend of both Giauque's sister and mother, but it was only after she returned to Berkeley for her graduate work in physics that he displayed any interest in her. On the day that she filed her Ph.D. thesis in 1932 she and Giauque were married. The union produced two sons, William Francis Ashley Giauque and Robert David Ashley Giauque, and four grandchildren. Muriel became an accomplished botanist, specializing in fern spores collected for her by a worldwide network of friends. Although characteristically reserved in direct praise, Giauque was clearly very proud of her accomplishments. When the Giauques traveled to Stockholm for his Nobel award, she received almost comparable attention from her botanist friends. Giauque's students remember pleasant Thanksgiving dinners at the Giauque home, with Muriel as cook and Frank (as she called him) as reconteur, with a keen sense of humor. The stories he most enjoyed telling were those in which the joke was on him. She predeceased him by eight months, on July 28, 1981.

Although adiabatic demagnetization was a dramatic discovery, Giauque's primary interest was in entropy and the third law of thermodynamics, which he explored by meticulously accurate absolute measurements: in this context, magnetic cooling was a means to an end. He eschewed making approximate measurements and insisted on a target accuracy of a tenth of 1%, a tall order for thermodynamic data. He envisioned building a 10-Tesla iron-free magnet large enough to produce a uniform magnetic field over a volume of 100 cubic centimeters or more, with an iron-free envi ronment over a 10-meter diameter. This project took over two more decades, reaching completion only in 1959, with the successful operation of a multiple-layer solenoid of 3/4-inch by 1/4-inch copper conductor carrying 10,000 amperes of current and dissipating 7 megawatts of power. Giauque insisted that magnetic samples be accurately ellipsoidal, and he required that all calorimetric measurements, magnetic or not, be made by direct-current methods. Standard cells from the National Bureau of Standards were delivered first to his laboratory, where they were treated with great respect. They were released to the college only when the next shipment of standard cells arrived. After the Low-Temperature Laboratory (later the Giauque Laboratory) was completed in 1954, and the first 10-Tesla magnet was finished in 1959, Giauque and his colleagues proceeded with magnetic and thermodynamic studies of paramagnetic compounds as he had originally intended forty years earlier. Many of his later publications report careful and accurate data on these compounds, setting an enviable standard for future workers.

Giauque's research interests were not restricted to magnetic systems or to very low temperatures. Early in his career he measured the heat capacities and heats of transition of the halogen acids from very low temperatures upward. With his careful measurements the excitations of degrees of freedom "frozen in" at very low temperatures (e.g., molecular rotation) were identified as sharp anomalies in the heat capacity. In other molecular systems, accurate heat capacity measurements allowed him to identify random molecular orientations that showed up as residual entropies, such as $S = R\ln 2$ for the carbon monoxide molecule, which could be oriented as C-O or O-C. The structure of ordinary ice was of special interest in this regard. Giauque expected a molecular rotation degree of freedom, while Linus Pauling proposed a tetrahedral structure for the oxygen atoms, connected by random hydrogen bonds, leading to a residual entropy $S = R\ln 3/2$. Giauque and Stout confirmed this value experimentally, supporting Pauling's model. Giauque used this example to convince his students of the need for careful measurement as well as the superiority of fact over speculation.

In addition to his interest in magnetic salts and simple molecules that illustrated statistical thermodynamic principles, Giauque made substantial contributions to instrumentation and experimental techniques. He helped to refine low-temperature scales throughout his career. He wrote an amusing parable in *Nature* in 1939 as a plea to use a single fixed point in defining the size of the degree in the absolute temperature scale. He also studied the chemically very important and difficult systems of sulfuric acid and sodium hydroxide over a period of years from 1950, using low-temperature calorimetry and other thermodynamic measurements to establish the properties of these complicated and corrosive materials.

Giauque's conservatism was legendary. He always appeared at the university dressed in an iron-gray tweed suit. He recounted that one day in 1924 he had sought clothing appropriate for a young faculty member and had a tailor make him a suit. He bought the suit and the bolt of cloth from which it was made, and over the years he always owned two identical suits. Whenever a jacket or pair of trousers showed enough wear, he had another made from that material, which lasted for over twenty years.

He did not learn to drive an automobile and did not own one until after receiving the Nobel Prize in 1949. He lived only seven blocks from the Berkeley campus and walked each way, except in his later years. Then he suffered from arthritis, and his wife Muriel drove him to work and back.

His conservatism was expressed in many ways in the laboratory. Giauque required that permanent metal joints be hard soldered, then covered with soft solder to ensure vacuum tightness. For many years his students used "ball-vee" vacuum-tight seals to hold sample chambers in place. Both surfaces in these seals were made of clean stainless steel, and a torque wrench was used to create a vacuum-tight seal without deforming the steel. It was through these and other meticulous but very difficult techniques that Giauque and his students were able to make precise absolute measurements.

Giauque's immunity to social fads had its counterpart in his scientific work. Although well versed in statistical mechanics, he was comfortable with the more empirical perspective of thermodynamics when he felt the situation warranted it. He also enjoyed the role of iconoclast when he felt that a colleague's approach didn't deliver all that it advertised. He jokingly referred to unusual g-factors as the "activity coefficients of magnetism," and he was very unenthusiastic about the ease with which the concept of spin temperature was adopted and applied to assign negative temperatures to systems with inverted populations, without also demonstrating the requisite rapid internal "thermal" equilibrium.

Giauque taught his research students to be the most demanding critics of their own data, reasoning that once published their work would then stand the test of time. He conveyed to them many of the practical aspects of experimental science, such as the improvement in accuracy on integrating, and loss on differentiating, a data set; the unreliability of the first point in a heat-capacity run because of hysteresis; and the advantages and pitfalls of least-squares fitting procedures. A dominant personality himself, Giauque not only tolerated but respected students who dis agreed with him, and he was especially pleased when they could prove their point.

The constancy of Giauque's commitment to classroom teaching was no less remarkable than his dedication to research. Starting with his appointment as instructor in 1922, he taught a discussion laboratory section of the freshman chemistry class in every semester for thirty-four consecutive years. In 1926 G. N. Lewis assigned him the responsibility for teaching the college's course in advanced physical chemistry, taken mainly by graduate students. Giauque taught that course every spring semester thereafter until his nominal retirement in June 1962. His classroom style was to lecture while using the blackboard to solve problems and prove points. His tests were problem based, and the problems were designed to test the students' understanding in depth. Over the years old problems recurred in somewhat altered forms, and students adopted the strategy of studying collections of problems that Giauque had used in previous years. Giauque must have regarded this as a good way to learn the material. In 1943 he also assumed the responsibility for a section of chemical thermodynamics for graduate and undergraduate honors students, which he taught every fall semester through 1960. While eschewing administrative posts in the university, he unstintingly gave his time in helping students, serving as adviser for undergraduates in the College of Letters and Science who wished to major in chemistry throughout the period 1945-60.

Giauque loved his work and made it the dominant part of his life, commenting on many occasions that he didn't need vacations because he spent the whole year doing what he enjoyed most. His legacy is that of one of the later major figures in the development of chemical thermodynamics, specifically regarding the influence of atomic and molecular structure on entropy and the third law of thermody namics. His work will long endure in the textbooks. His influence on colleagues and students, though largely unrecorded, will also endure.

Selected Bibliography

1923 With G. E. Gibson. The third law of thermodynamics. Evidence from specific heats of glycerol that the entropy of a glass exceeds that of a crystal at the absolute zero. *J. Am. Chem. Soc*. 45:93-104.

1927 A thermodynamic treatment of certain magnetic effects. A proposed method of producing temperatures considerably below 1° absolute. *J. Am. Chem. Soc*. 49:1864-70.

Paramagnetism and the third law of thermodynamics. Interpretation of the low-temperature magnetic susceptibility of gadolinium sulfate. *J. Am. Chem. Soc*. 49:1870-77.

1928 With R. Wiebe. The entropy of hydrogen chloride. Heat capacity from 16°K. to boiling point. Heat of vaporization. Vapor pressures of solid and liquid. *J. Am. Chem. Soc*. 50:101-22.

With H. L. Johnston. Symmetrical and antisymmetrical hydrogen and the third law of thermodynamics. Thermal equilibrium and the triple point pressure. *J. Am. Chem. Soc*. 50:3221-28.

1929 With H. L. Johnston. An isotope of oxygen, mass 18. Interpretation of the atmospheric absorption bands. *J. Am. Chem. Soc*. 51:1436-41.

With H. L. Johnston. The heat capacity of oxygen from 12°K to its boiling point and its heat of vaporization. The entropy from spectroscopic data. *J. Am. Chem. Soc*. 51:2300-2321.

Isotope effect in spectra and precise atomic weights. *Nature* August 17.

With H. L. Johnston. An isotope of oxygen, mass 17, in the earth's atmosphere. *J. Am. Chem. Soc*. 51:3528-34.

1930 The entropy of hydrogen and the third law of thermodynamics.

The free energy and dissociation of hydrogen. *J. Am. Chem. Soc.* 52:4816-31.

The calculation of free energy from spectroscopic data. *J. Am. Chem. Soc.* 52:4808-15.

1931 Nuclear spin and the third law of thermodynamics. The entropy of iodine. *J. Am. Chem. Soc.* 53:507-14.

1932 With J. O. Clayton. The heat capacity and entropy of carbon monoxide. Heat of vaporization. Vapor pressures of solid and liquid. Free energy to 5000°K. from spectroscopic data. *J. Am. Chem. Soc.* 54:2610-26.

With C. W. Clark. The conditions for producing temperatures below 1° absolute by demagnetization of $Gd_2(SO_4)_3 \cdot 8H_2O$. Temperature-magnetic field isentropics. *J. Am. Chem. Soc.* 54:3135-42.

1933 With M. F. Ashley. Molecular rotation in ice at 10°K. Free energy of formation and entropy of water. *Phys. Rev.* 43:81-82.

With D. P. MacDougall. Attainment of temperatures below 1° absolute by demagnetization of $Gd_2(SO_4)_3 \cdot 8H_2O$. *Phys. Rev.* 43:768.

With J. O. Clayton. The heat capacity and entropy of nitrogen. Heat of vaporization. Vapor pressures of solid and liquid. The reaction $1/2N_2 + 1/2\ O_2 = NO$ from spectroscopic data. *J. Am. Chem. Soc.* 55:4875-89.

1936 With J. W. Stout. The entropy of water and the third law of thermodynamics. The heat capacity of ice from 15 to 273°K. *J. Am. Chem. Soc.* 58:1144-50.

1937 With C. J. Egan. Carbon dioxide. The heat capacity and vapor pressure of the solid. The heat of sublimation. Thermodynamic and spectroscopic values of the entropy. *J. Chem. Phys.* 5:45-54.

With C. C. Stephenson. A test of the third law of thermodynamics by means of two crystalline forms of phosphine. The heat capac ity, heat of vaporization and vapor pressure of phosphine. Entropy of the gas. *J. Chem. Phys.* 5:149-58.

1938 With J. D. Kemp. The entropies of nitrogen tetroxide and nitrogen dioxide. The heat capacity from 15°K. to the boiling point. The heat of vaporization and vapor pressure. The equilibrium $N_2O_4 = 2NO + O_2$.*J. Chem. Phys.* 6:40-52.

1939 With J. W. Stout and R. E. Barieau. Measurements of the viscosity of liquid helium II. *J. Am. Chem. Soc.* 61:654-61.

A proposal to redefine the thermodynamic temperature scale. A parable of measures to improve weights. *Nature* 143:623-32.

With T. M. Powell. Propylene. The heat capacity, vapor pressure, heats of fusion and vaporization. The third law of thermodynamics and orientation equilibrium in the solid. *J. Am. Chem. Soc.* 61:2366-70.

1941 With J. W. Stout, C. J. Egan, and C. W. Clark. The measurement of adiabatic differential magnetic susceptibility near 1° absolute. The heat capacity of gadolinium phosphomolybate tridecahydrate from 0.17 to 4.7° absolute. *J. Am. Chem. Soc.* 63:405-10.

1942 With W. R. Forsythe. The entropies of nitric acid and its mono-and tri-hydrates. Their heat capacities from 15 to 300°K. The heats of dilutions at 298.1°K. The internal rotation and free energy of nitric acid gas. The partial pressures over its aqueous solutions. *J. Am. Chem. Soc.* 64:48-61. Errata: *J. Am. Chem. Soc.* 64:3069 (1942); 65:2379 (1943).

1949 With J. J. Fritz and D. N. Lyon. The measurement of magnetic susceptibility at low temperatures. *J. Am. Chem. Soc.* 71:1657-64.

Some consequences of low temperature research in chemical thermodynamics. Nobel lecture, delivered in Stockholm, December 12, pp. 91-114.

1953 With R. H. Busey. The equilibrium reaction $NiCl_2 + H_2 = Ni + 2HCl$. Ferromagnetism and the third law of thermodynamics. *J. Am. Chem. Soc.* 75:1791.

Determination of thermodynamic temperatures near 0°K. without introducing heat below 1°K. *Phys. Rev.* 92:1339.

1959 With D. A. Shirley. The entropy of iodine. Heat capacity from 13 to 327°K. Heat of sublimation. *J. Am. Chem. Soc.* 81:4778.

1960 With E. W. Hornung, J. E. Kunzler, and T. R. Rubin. The thermodynamic properties of aqueous sulfuric acid solutions and hydrates from 15 to 300°K. *J. Am. Chem. Soc.* 82:62-70. Erratum: *J. Am. Chem. Soc.* 83:5047 (1962).

1965 With G. E. Brodale. The heat of hydration of cobalt sulfate hexahydrate to heptahydrate. Their solubilities and heats of solution. *J. Phys. Chem.* 69:1268-77.

1967 With E. W. Hornung, R. A. Fisher, and G. E. Brodale. Thermodynamic temperature and heat capacity of $NiSiF_6 \cdot 6H_2O$ without heat introduction below 0.35°K. Magnetic moment and internal energy from 0.05° to 4.2°K. Fields 0-90 kG perpendicular to the *c* axis. *J. Chem. Phys.* 47:2685-700.

1969 With R. A. Fisher, E. W. Hornung, and G. E. Brodale. Magnetothermodynamics of a-$NiSO_4 \cdot 6H_2O$. V. Proton spin polarization rate and activation enthalpy as a function of temperature and field to 90 kG along the axis. *J. Chem. Phys.* 51:1959-65.

1970 With R. A. Fisher, E. W. Hornung, and G. E. Brodale. Magnetothermodynamics of antiferromagnetic a-$MnCl_2 \cdot 4H_2O$. IV. Reversibility conditions in the a, b, and p regions with H *c* axis. *Spin Flop,* an inappropriate term. *J. Chem. Phys.* 53:1474-90.

1971 With R. A. Fisher, E. W. Hornung, and G. E. Brodale. Magnetothermodynamics of $CuK_2(SO_4)_2 \cdot 6H_2O$. V. Fields along the axis. Thermodynamic temperature without heat introduction below 0.5°K. The freezing-in of magnetic structure in the lambda region. *J. Chem. Phys.* 55:2859-67.

1972 With G. E. Brodale. The relationship of crystalline forms I, III, IV, and V of anhydrous sodium sulfate as determined by the third law of thermodynamics. *J. Phys. Chem.* 76:737-43.

1973 With R. A. Fisher, E. W. Hornung, and G. E. Brodale. Magnetothermodynamics of $Ce_2Mg_3(NO_3)_{12} \cdot 24H_2O$. II. The evaluation of absolute temperature and other thermodynamic properties of CMN to 0.6 millidegrees. *J. Chem. Phys.* 58:5584-5604. Erratum: *J. Chem. Phys.* 61:3869 (1974).

1975 With R. A. Fisher, E. W. Hornung, and G. E. Brodale. Magnetothermodynamics of $Ce_2Zn_3(NO_3)_{12} \cdot 24H_2O$. II. Determination of absolute temperature and other thermodynamic properties of CZN to 0.80 m°K. *J. Chem. Phys.* 62:555-72.

With G. E. Brodale, E. W. Hornung, and R. A. Fisher. Magnetothermodynamics of gadolinium gallium garnet. III. Heat capacity, entropy, magnetic moment from 0.5 to 4.2°K, with fields to 90 kG along the [110] axis. *J. Chem. Phys.* 62:4041-49.

With R. A. Fisher, G. E. Brodale, and E. W. Hornung. Magnetothermodynamics of single crystal $CuSO_4 \cdot 5H_2O$. VI. Properties below 0.5°K by heat introduction with constant fields to 33 kG along the axis. The initial T^3 dependence of entropy and heat capacity for dipole-dipole magnetic interactions. *J. Chem. Phys.* 63:4817-30.

1978 With R. A. Fisher, G. E. Brodale, and E. W. Hornung. Magnetothermodynamics of Nd $(C_2H_3SO_4)_3 \cdot 9H_2O$. IV. Determination of absolute temperature scales and other properties below 0.5°K with constant magnetic fields along the *a* crystal axis. *J. Chem. Phys.* 68:169-84.

With R. A. Fisher, E. W. Hornung, and G. E. Brodale. Magnetothermodynamics of antiferromagnetic, polarized ferroelectric, ferroelastic -$Gd_2(MoO_4)_3$. V. Thermodynamic temperature and other properties with heat introduction below 0.5°K. Fields to 5 kG along the *b* crystal axis. *J. Chem. Phys.* 69:2892-2900.

Susumu Hagiwara

November 6, 1922-April 1, 1989

THEODORE H. BULLOCK AND ALAN D. GRINNELL

BORN IN YUBARI, Hokkaido, Japan, on November 6, 1922, the son of a school principal, Susumu Hagiwara went through public schools, graduating from high school in Mito, Honshu. Among the boyhood hobbies that persisted throughout his life were butterfly observation and collecting, begun with his father. He also enjoyed bird watching and painting. He was one of the select few admitted to the prestigious University of Tokyo. There he completed both his medical degree in 1946 and Ph.D. in physiology under Prof. T. Wakabayashi in 1951. During this period he was diagnosed with tuberculosis and had one lung removed, but he continued to write papers during his convalescence and recovered enough to live a surprisingly normal life.

In 1948 his first paper demonstrated that cicadas begin to sing at a certain level of light in the morning, delayed correspondingly on cloudy days. His thesis topic, the fluctuation of intervals in rhythmic excitation in frog stretch receptors, with comparisons to the intervals in human motor units during voluntary movement, foretold a lifelong bent toward comparative physiology. Already prolific, he published a substantial series of papers before 1953 on topics as diverse as the myogenic rhythm in cicada muscles (with A. Watanabe), the first penetration of Mauthner's neu ron in fish (with I. Tasaki and A. Watanabe), and the statistical analysis of neuronal firing intervals. Each was a pioneering contribution; the last mentioned is often cited as a seminal study in the field now called neural computation that is rising rapidly in parallel with molecular neurobiology, the one more system oriented, the other more reductionist. Susumu Hagiwara cannot be pigeon-holed in one camp or the other.

One of the fascinations of this man's career is that a true hero of general physiology owes the essence of his fame to comparative studies, not in the usual sense that he spent his life on an unconventional favorable species but in sampling many species, far apart phylogenetically, upward of sixty different preparations in about as many species, from plants to humans, from clonal pituitary cells to leech neurons, from cicadas to barnacles, giant squid to bats, chirping birds to clam larvae, soft corals to mouse hybridoma cells, invertebrate eggs to cats. The motivation to search deliberately for unusual materials and to explore the world of species is an obvious thread throughout his work and more than a leitmotif. He learned early the lesson that even so basic an organelle as the cell membrane cannot be represented adequately by any one exemplar. He set out to broaden our view of nature's scope and range of available mechanisms.

Pretending to know nothing about zoology or botany, he asked his friends in that gentle, undemanding manner: "What kinds of eggs can we get? Where can one get amphioxus? Where is an example of a distributed synapse?" (The answer to the last is the sabellid polychaete giant, which he then took for study.) "If it's not too much trouble, is there any chance we could get some hummingbirds?" Procurement problems were a constant source of amazement and amusement. Susumu's antennae were tuned to the first hint of interesting material, as when Graham Hoyle discovered the giant muscle fibers in a North Pacific barnacle and later the simple photoreceptors in barnacles. Finding himself on the Great Barrier Reef or the Amazon River, Hagi could quickly find a remarkable species and bit of tissue in that species. To be sure, some preparations were suggested to him, but the notable feature was the instant sympathetic response and eagerness to try his iridectomy scissors and skillful fingers on it. Some preparations were not so delicate. It required 75-mm nails hammered into a hardwood plank to hold down a strong electric eel or *Gymnarchus*, both of which jerk each time they indulge in an air-breathing gulp.

If it needed another example, Hagiwara's life outstandingly illustrated how finding differences among species, preparations, or cell types can be a major source of insight into general physiology. This premier intracellular biophysicist must also be recognized as a neuroethologist. Not only is this true because of the implicit relevance of his membrane discoveries to species-characteristic behavior, but he was explicitly interested in uncovering the mechanisms of outstanding examples of natural behavior. How does a cicada sing? How does a hummingbird move its wings? What rules apply to interval distribution in a chirping bird's rhythm or the intervals between gulps in air-breathing loaches in the aquarium beside his hospital bed? These examples manifest his lifelong interest in animals as such.

At the time Hagiwara completed his Ph.D., Yasuji Katsuki (1905-94), head of the Department of Physiology of the Tokyo Medical and Dental University, was building a group that became the most fertile laboratory in sensory neurophysiology in the country. He was an international figure in auditory physiology who appreciated and himself indulged in many comparative studies, among diverse animals and various modalities. He recruited Hagiwara to his department in 1950 as his second in command and soon departed for a two-year stint abroad, including working periods in the laboratory of one of the authors (T.H.B.). During this early period at the TMDU, Hagiwara completed a series of studies on (1) the curious sonic muscle in certain cicadas that produce their summer buzzing sound by a myogenic rhythm, (2) the neuromuscular transmission in insects, (3) intracellular recording in Mauthner's neuron in catfish and insect muscle, and (4) a prescient paper on the effects of tetraethylammonium chloride on the muscle cell membrane.

When Katsuki returned, he promptly sent Hagiwara abroad for a similar two-year sequence of working visits to laboratories in Europe and the United States. Hagi spent such a period with Yngve Zotterman in Stockholm, participating in recording from the chorda tympani nerve of cats. He visited Sven Dijkgraaf in Utrecht; John Pringle, Alan Hodgkin, and Lord Adrian in Cambridge; and Ichiji Tasaki in Bethesda, Maryland. After six month at UCLA and three months at the Marine Biological Laboratory in Woods Hole with T.H.B. Hagiwara spent several months at the Johns Hopkins University working with Charles Edwards and Stephen Kuffler, several months in New York at Rockefeller University, and half a year at the National Institute of Neurological Diseases and Blindness in Bethesda with Tasaki. All this resulted in experience and papers on the physiology of taste in cats, cardiac ganglion pattern generators in lobsters, giant synapses in squid, stretch receptors in crayfish, and the electrical capacitance of muscle fiber membranes, among others.

He returned to Japan in 1957 and within two years rose to professor in the Second Department of Physiology in the Tokyo Medical and Dental University. He soon discovered that the expectations and obligation in this role were not for him and his chronic lung problem. He rejoined Bullock in Los Angeles in 1960 and by 1964 became completely independent, with his own laboratory and grants. During these years, while Hagi rapidly rose to the rank of research professor, eventually to overscale professor, we enjoyed many joint projects. Some brought distinguished Europeans such as Ladislav Tauc, Thomas Szabo, Hans Lissmann, and Per Enger, as well as accomplished and promising co-workers from Japan, including Hiromichi Morita, Koroku Negishi, Kenichi Naka, Shiko Chichibu, and Nobuo Suga. Besides continuing with previous preparations, Hagi and his co-workers began experiments on electroreceptors, polychaete giant synapses, and barnacle and hummingbird muscle. It was still possible to maintain active interest in both integrative and ion channel mechanisms, much to the benefit of both fields.

Hagi traveled during these years, visiting, for example, Hans Lissman in Cambridge, Alfred Fessard in Paris, and Angelique Arvanitaki in Monaco. From a hospital in Rome he wrote: "I was brought to this hospital unconscious from the hotel … acute pneumonia .. I lost all my weight. Since I did not have much weight, I am almost losing myself." He flew to Tokyo to recuperate, surprising his family and missing his symposium. This was not the first or the last episode of health problems during his trips, but it is significant that they did not stop him from traveling even to fairly remote places.

One notable trip was in March 1964 to the U.S.-Japan Joint Cooperative Program Symposium on Neurophysiology, for which the Japanese delegation, led by Yasuji Katsuki, included Tasunosuke Araki, Taro Furukawa, Kojiro Matsuda, Koichi Motokawa, Yutaka Omura, Masayasu Sato, Sadataka Takagi, Tsuneo Tomita, Koji Uchizono, and Akira Watanabe. The U.S. delegation, led by T. H. Bullock, included Michael

Bennett, Robert Galambos, Harry Grundfest, Susumu Hagiwara, Carlton Hunt, Stephen Kuffler, David Potter, Floyd Ratliff, and Walter Rosenblith. We mention the names because this influential meeting triggered a number of later projects and collaborations. We recall the visit Hagi, Mike Bennett, and Bullock made together, after the symposium, to the beautiful seaside resort of Shirahama. They asked the attendant in the public bath why it happened that they were the only bathers and were told that they were the only guests who were not newlyweds, bathing in private.

The very next month Hagi was off to the uncertainties of living and working in Belem, on the Amazon River, to do electric fish experiments with colleagues from UCLA and abroad. He did not do South America minimally but returned via Rio de Janeiro, Buenos Aires, and Santiago, apparently enjoying the sights and adventures, meeting scientists, and collecting butterflies.

In 1965 an invitation arrived from the Scripps Institution of Oceanography to be the first professor of neurobiology at the University of California at San Diego—quite independently of the new medical school, which was at the moment creating the first Department of Neurosciences. Hagi was recruited by the marine biology division of Scripps. Some credit is certainly due the late, great comparative physiologist, Prof. P. F. Scholander, on the faculty of that division and to the then-director of the UCLA Brain Research Institute, John D. French, who had persuaded the Scripps faculty to cosponsor with the institute a unique, and for many years jointly operated, marine neurobiology facility on the third floor of Scholander's new Physiological Research Laboratory. A popular hypothesis is that the marine biologists remembered the elegant electrophysiological demonstrations of functions of sense organs in fish by Yasuji Katsuki, Hagi's sponsor, and Yngve Zotterman from

Stockholm, Hagi's first foreign host, during their short visits years before. In the transition period, prior to moving to La Jolla, Hagi spent a period between May and August of 1965 as a visiting professor at the Collége de France and published several papers in French with Thomas Szabo.

With a group of postdoctoral associates, Hagiwara initiated the Marine Neurobiology Facility of Scripps and the Brain Research Institute. He enjoyed four years of idyllic existence and outstanding scientific creativity in La Jolla. The maiden voyage of the unique research vessel *Alpha Helix*, created by Scholander as an arm of the Physiological Research Laboratory and a national—really international—facility for comparative biochemistry and physiology in remote habitats, set off for the Great Barrier Reef in the spring of 1966. Hagi and Kunitaro Takahashi and chief scientist Bullock were among the ten scientists in the first three-month program. Many vignettes of that great experience crowd into memory. One was due to the chance that the three visited Bora Bora on the way to Australia and found that a fellow guest at the seaside hotel was the great astronomer Bart Bok, who took them to the end of the pier and gave a private lecture on the Milky Way and Southern Cross as an introduction to the South Pacific. As this expedition proved and many others subsequently confirmed, Hagi well exemplified the kind of bench scientist who could make good use of a few weeks in a laboratory at an exotic location.

Telling incidents shared by Hagi on the *Alpha Helix* Operation Billabong are recounted in a book by P. F. Scholander, *Enjoying a Life in Science* (University of Alaska Press, 1990). Susumu and "Pete" Scholander were kindred souls in their eagerness to explore the world of species and in their skill in finding specially favorable material for the study of fundamental problems, about half the time planned ahead of embarking on an expedition and half the time discovered among the species that had not been anticipated. His South American experience was enlarged in 1967 with a several-month trip to Chile, together with Alan Grinnell and Jared Diamond, to study synaptic mechanisms in giant squids at Monte Mar, the marine station near Valparaiso. A major storm aborted that project by driving the squid to other waters (for several years), but with customary ease Hagi found an ideal preparation in the muscle fibers of Chilean giant barnacles. Two years later he was off to New Guinea on an *Alpha Helix* expedition led by George Bartholomew, studying bats and collecting butterflies and carvings, again with Grinnell. A highlight of this trip was a stopover enroute at Marlon Brando's atoll off Tahiti. In 1973 Hagi was organizer and chief scientist of his own *Alpha Helix* expedition to the Great Barrier Reef, where he again worked with blue-spotted sting rays, as he had done in 1966 on the maiden expedition.

In 1969 he was enticed back to UCLA, a great loss to San Diego and gain for Los Angeles. He and his wife, Satoko, made an attractive home in West Los Angeles, decorated not only with his great butterfly collection, mounted birds, and New Guinea wood carvings but also scores of hanging plants and tanks with varieties of koi and tropical fish. In a short time he was named the Eleanor I. Leslie Professor of Neuroscience and enjoyed twenty happy and productive years at the Brain Research Institute and its Jerry Lewis Neuromuscular Research Center before succumbing in 1989 to an illness that demanded respiratory reserves he had lost nearly half a century before.

Whereas Hagiwara was best known for his contributions to membrane and channel biophysics, he made a wide variety of important contributions to systems physiology at the integrative level. His pioneering series of papers on the sound-producing muscle of cicadas started a pregnant line of research on its dynamics and neural control in England and the United States, showing that classical muscle physiology illuminates only a fraction of the properties of muscle that evolution has spun off. One of these studies was his own, in 1968, on the neuromuscular specialization in the wingbeat of hummingbirds. His analysis of sensory nerve impulse interval fluctuation is a standard reference point for a considerable later literature. He made major contributions to synaptic physiology and to the integrative mechanisms of the nine-celled cardiac ganglion of lobsters. He played a central role in the initial discovery of electroreceptors in weakly electric fish and in the further discoveries of multiple types of electroreceptors and nerve impulse codes.

Beginning in 1964 his concentration on ionic mechanisms in active membranes and especially on calcium channels became marked. After the great advance and wide acceptance of the Hodgkin-Huxley concept of the sodium and potassium ion basis of the nerve impulse in the squid giant axon membrane, Hagi asked himself four questions: (1) How widely can one apply the original sodium-potassium channel concept to electrical excitation among a variety of tissues in different animals? (2) What other voltage-gated membrane channels exist besides the original sodium and potassium channels? (3) What are the biological functions of those other channels? (4) How did the various ion channels evolve phylogenetically and how do they develop ontogenetically?

These questions led him to study preparations such as muscle fibers in barnacles, mussels, and amphioxus; eggs of starfish, annelids, and *Drosophila;* mudpuppy hair cells; crustacean photoreceptors; chromaffin cells in rats; lymphocytes and tumor cells in mice; seminiferous tubule cells; pituitary cells; left-handed snail cells; and human T cells. Only a vastly flexible expertise and a disciplined theoretical mindset could so successfully carry out the basic experiments, avoid dilettantism, and glean the harvest of general principles within natural diversity that Hagiwara did. He was a principal player during the years when the concept of one channel for each of two or three ions was gradually replaced by the understanding of a multitude of distinct channels for each ion, differing in proportions and distribution among cell types.

Hagiwara's name is particularly associated with calcium channels in cell membranes. Whereas the action potential had been adequately accounted for as a sodium spike for more than a decade, in 1964 Hagi and his colleagues recognized the calcium spike in an unlikely preparation—the normally nonspiking muscle fiber of a giant barnacle. For a time this spike was regarded as a curious anomaly—resistant to removal of external sodium but converted from nonspiking to a spiking cell by injecting sodium if its anion was a calcium binder like sulfate. He recounts, with characteristically self-deprecating humor, how, during this period when "the calcium channel was only found in miserable animals like crustaceans and was thought to play no important function in the human brain … I suffered tremendously from a minority [*sic*] complex," until time went by and calcium channels were found in a variety of tissues, including mammalian brains, and it became difficult to name an excitable tissue that does not possess calcium channels. He proposed the rule that sodium spikes are found where the function of the action potential is propagation of an impulse and calcium spikes are found where action potentials are coupled with effector functions such as contraction, secretion, transduction, transmission, and bioluminescence.

Bertil Hille, ten years ago, summarized: "Hagi [was] a research scientist of peerless distinction. … He is remembered as the champion who brought the calcium channel to its rightful respected place and in the process discovered blocking ions, flux saturation, inactivation dependent on internal calcium, and many other unanticipated phenomena." To a much higher degree than the sodium channel of the squid axon, the calcium channel does not obey the independence principle; the current (ion flux) is not linearly proportional to the external calcium concentration. The system becomes saturated, implying the existence of a limited number of carriers or charged membrane sites with which ions must interact in order to permeate the membranes. Hagiwara carefully distinguished between specific ionic binding sites and the general layer of charges forming the surface charge on the membrane. The permeability of the calcium channel to different cations was carefully dissected into its components, the binding constants for each ion and the relative mobility of each within the membrane. The binding sites were also characterized in terms of their affinities for different blocking cations. Hagi's discovery that intracellular calcium blocks calcium channels was the first clear demonstration of an important mechanism of calcium channel inactivation.

Hagiwara's analysis of calcium permeation in terms of binding affinities, dissociation constants, and screening potentials was probably the first application of the concept of enzyme kinetics to channel permeation mechanisms, and he pioneered what is now the generally accepted framework in which to interpret most ionic permeation and channel-blocking mechanisms. Studies of this nature make it possible ultimately to construct a physical model of a given channel and the factors governing the gating of permeability, selectivity of the channel, and mechanism of permeation. Through Hagiwara's research, our understanding of active calcium permeability mechanisms has advanced to the point that it is equivalent to our understanding of sodium permeability mechanisms and in many ways is much more important. In all cells studied to date, the voltage-dependent calcium flux properties are very similar to those shown by Hagiwara. His work thus laid a foundation for and provided invaluable guidance to everyone trying to understand the role of calcium in a wide variety of other important cell functions. In particular, these discoveries opened the way for new methods of treatment of various cardiac disorders, based on the rational use of external chemical messengers and calcium channel-blocking agents such as Verapamil, D-600, nifedipine, and others.

Hagiwara was one of the first to recognize and adopt the new technique of patch clamping when it was introduced and pioneered its application to the study of small mammalian cells whose membrane properties were previously almost completely unknown, such as pituitary cell lines and lymphocytes. His 1981 paper with L. Byerly on "Calcium channel" in the *Annual Reviews of Neuroscience* had been cited in 1,070 publications by 1992, making it the most cited paper in that journal up to that time.

Hagi never ran a large laboratory, but his co-workers were numerous. He was especially popular among young Japanese scientists experiencing their first visits abroad. His influence is conspicuous in the number of department chairpersons and prominent scientists who trained in his laboratory. He was an apparently relaxed, undemanding but inspiring leader. Particularly important to those who worked in his laboratory were the free-wheeling discussions of scientific ideas that took place at lunchtime each day. Hagi loved to talk science and to discuss the development of ideas and the role of personalities in science. Many feel that they learned more from these lunches than from their formal education in science. Hagi excelled at drawing out ideas from his students and remained deeply involved in their career development long after they left his laboratory. Those who experienced his formal lectures inform us that they were typically shorter than the allotted time and always crisp and rich in content, vivid and somewhat humorous, and delivered beautifully. This description reminds us of his writing style.

Hagi's ever-present gentle sense of humor is legendary. In the early 1960s the Brain Research Institute at UCLA was planning an outpost, the Marine Neurobiology Facility at the Scripps Institution of Oceanography of UCSD in La Jolla, which he was later recruited to head, as we recounted above. It was the top floor of a new building donated by the National Science Foundation. Hagi liked to tell how he sat in on a meeting with the site visiting committee. They asked about the obstacle of a hundred-odd miles between campuses and, according to Hagi, were told by Dr. French, the BRI director: "The San Diego Freeway starts a few blocks from the BRI and passes a few blocks from the SIO." This apparently was satisfactory and the grant was awarded, although the portions of the freeway constructed at the time ended about 10 miles south of UCLA and about as far north of San Diego. "Since that time I have been learning how neuropolitics works," said Hagi. He opened his talk at one meeting with these words: "It is a great honor to be selected as one of the speakers of this symposium. I am supposed to discuss my personal view of the brain. Unfortunately, I have not achieved enough sophistication to handle the problem philosophically. I hope I can reach such a level of sophistication while my own brain is still undeteriorated and viewable."

Shared experiences and warm memories of the authors extend well beyond Southern California. Traveling together in Japan, Hagi asked Bullock to ask for directions in the underground, since we got polite and helpful replies that way; when he asked, people were likely to say, curtly, "That way, as you should know!" Susumu brought out the best in others, including the Australian aborigine who speared sting rays on the coral reef for his studies of a special muscle. Joe, the giant aborigine, knew he needed undamaged tissue and asked Susumu, "Which side you want spear?" He was in no hurry to become a Californian and wrote in a letter quite some time after taking up residence in Los Angeles, "I am now learning how to drive but it is much more difficult than microelectrode penetration." In awe of the examination on American civics, Hagi delayed becoming a citizen for years after his wife breezed through her ceremony, but eventually (1971) screwed up his courage and did it.

Looking back over the record, it is plain that we cannot explain Hagi's unique success simply by his choice of species and interest in basic neuroethology. One major factor deserving notice is the sheer element of skill in many of Hagi's triumphs, manual and manipulative skill combined with patience. In addition, another factor can be inadequately termed ingenuity. This was nicely shown in his sandwich preparation of the barnacle muscle fiber membrane—from an opened fiber laid flat between holders that present the inside of the membrane to one chamber and the outside to another. Setups were never more complicated than they needed to be. He could develop a new preparation, do a set of experiments, and be on to something else, while others would still be thinking of where to begin. The common denominator of many clever holders for diverse preparations was simplicity. One often thought of the ultimate compliment: "Why didn't I think of that?"

A long list of firsts belong to Hagi's credit. To mention a few, he was the first to penetrate insect muscle, and the Mauthner's neuron of fish, and to apply the voltage clamp to the neuron soma. He introduced many species and preparations into physiology. With a succession of co-workers, he was the first to penetrate the squid giant synapse on both pre- and postsynaptic sides, close to the junction. Likewise, he and his associates succeeded in recording from inside the pacemaker and the follower cells of a miniature crustacean ganglion in the wall of the heart. This led to the first discovery of the subthreshold electronic influence of one neuron upon another, via very slow currents. Hagi and his co-workers found the specialized receptors that sense electric organ discharges from the same fish as well as from neighboring conspecifics. He was the first to make a barnacle muscle membrane spike, although this type of cell has probably not given a spike for hundreds of millions of years. He started the study of the statistical structure of spike interval trains in apparently stochastic series of motor units and sensory discharges—one of the early landmarks in neural computation.

Hagi had a direct impact on generations of students. He was a superior lecturer—speaking, as he wrote, in simple declarative sentences. Lucidity and a transparent organization of his exposition were characteristic. His talents in this department must have influenced many more people than were in his classrooms, since he was a successful lecturer at national meetings and at institutions he visited all over the world. Besides his formal teaching, Hagi profoundly shaped the lives of his many collaborators. His laboratory was always a hive of hard-working young investigators. We have no accurate count of them, but his bibliography shows about seventy coauthors.

Hagiwara's achievements were recognized by numerous awards. He was elected to membership in the American

Academy of Arts and Sciences in 1971. In 1976 he won the Kenneth Cole Award of the Biophysical Society. In 1978 he was elected to the National Academy of Sciences. In 1981 he was the Distinguished Lecturer of the Society of General Physiologists. This resulted in a seminal monograph titled *Ion Channels in Cell Membranes: Phylogenetic and Developmental Approaches* (1983). In 1983 a symposium volume was published in his honor (*The Physiology of Excitable Cells*, edited by A. D. Grinnell and W. J. Moody). In the same year he was awarded an honorary doctorate from the Université Pierre et Marie Curie in Paris. In 1984 he shared the Ralph Gerard Prize of the Society for Neuroscience with his longtime friend and colleague, T. H. Bullock. The National Institutes of Health indicated its confidence in Hagi's long-term productivity by giving him a seven-year Javits Neuroscience Investigator Award in 1987. He was honored posthumously by the Japanese government with the Order of the Rising Sun. Also posthumously, an international symposium was held in his memory in Okazaki, Japan, at the National Institute for Physiological Sciences and a book was published titled *Basic Neurobiology: Half a Century and Future*, edited by H. Ohmori and S. Ebashi (1991). In 1994 the Hagiwara Chair of Neurobiology was created at UCLA, with Francisco Bezanilla its first holder. Few, if any, neuroscientists have matched Hagi's record of fundamental contributions throughout the range of behavioral, integrative, cellular, and molecular neurobiology.

Hagiwara is survived by his wife and son. Among a large circle of friends across the world he was a special favorite, and an even larger circle of admirers and followers have benefited from his discoveries, insights, and elegant experimental foundations.

Selected Bibliography

1948 Sound-producing activities of cicada. I. Effects of light. *Kagaku* 18:464 (in Japanese).

On the charping rhythm of the bird, Hororunis cantans. *Kagaku* 18:468-69 (in Japanese).

1949 Sound-producing activities of cicadas. II. Interaction. *Kagaku* 19:40 (in Japanese).

On the fluctuation of the interval of rhythmic excitation. I. Analysis on the interval between impulse of a motor unit during human voluntary movement. *Rep. Physiol. Sci. Inst. Tokyo Univ.* 3:19-24.

1950 On the fluctuation of the interval of rhythmic excitation. II. Analysis on impulses from stretch receptor of a frog muscle. *Rep. Physiol. Sci. Inst. Tokyo Univ.* 4:28-35 (in Japanese, Ph.D. thesis).

1953 With T. Wakabayashi. Mechanical and electrical events in the main sound muscle of cicada. *Jpn. J. Physiol.* 3:249-53.

Neuromuscular transmission in insects. *Jpn. J. Physiol.* 3:284-96.

1954 With A. Watanabe. Action potential of insect muscle examined with intracellular electrode. *Jpn. J. Physiol.* 4:65-78.

With I. Tasaki and A. Watanabe. Action potentials recorded from inside a Mauthner cell of the catfish. *Jpn. J. Physiol.* 4:79-90.

Analysis of interval fluctuation of the sensory nerve impulses. *Jpn. J. Physiol.* 4:234-40.

With H. Uchiyama and A. Watanabe. The mechanism of sound production in certain cicada with special reference to the myogenic rhythm in insect muscle. *Bull. Tokyo Med. Dent. Univ.* 1:113-24.

With M. J. Cohen and Y. Zotterman. The response spectrum of taste fibers in the cat: a single fiber analysis. *Acta. Physiol. Scand.* 33:316-52.

1955 With A. Watanabe. The effect of tetraethylammonium chloride on the muscle membrane examined with an intracellular microelectrode . *J. Physiol.* 129:513-27.

1956 With A. Watanabe. Discharges in motoneurons of cicada. *J. Cell Comp. Physiol.* 47:415-28.

Neuromuscular mechanisms of sound production in the cicada. *Physiol. Comp. Oecologia* 4:142-53.

1957 With T. H. Bullock. Intracellular potentials in pacemaker and integrative neurons of the lobster cardiac ganglion. *J. Cell Comp. Physiol.* 50:25-47.

With T. H. Bullock. Intracellular recording from the giant synapse of the squid. *J. Gen. Physiol.* 40:565-77.

With I. Tasaki. Capacity of muscle fiber membrane. *Am. J. Physiol.* 188:423-29.

With I. Tasaki. Demonstration of two stable potential states in the squid giant axons under tetraethylammonium chloride. *J. Gen. Physiol.* 40:859-85.

With S. Saito. Mechanism of action potential production in the nerve cell of a puffer. *Proc. Jpn. Acad.* 33:682-85.

1958 Synaptic potential in the motor giant axon of the crayfish. *J. Gen. Physiol.* 41:1119-28.

With Y. Oomura. The critical depolarization for the spike in the squid giant axon. *Jpn. J. Physiol.* 8:234-45.

With I. Tasaki. A study on the mechanism of impulse transmission across the giant synapse of the squid. *J. Physiol.* 143:114-37.

1959 With S. Saito. Membrane potential change and membrane current in supramedullary nerve cell of puffer. *J. Neurophysiol.* 22:204-21.

With A. Watanabe and S. Saito. Potential changes in synctial neurons of lobster cardiac ganglion. *J. Neurophysiol.* 22:554-72.

With S. Saito. Voltage-current relations in nerve cell membrane of Onchidium verruculatum. *J. Physiol.* 148:161-77.

With C. Edwards. Potassium ions and the inhibitory process in the crayfish stretch receptor. *J. Gen. Physiol.* 43:315-21.

1960 Current-voltage relations of nerve cell membrane. In *Electrical Activity of Single Cells*, ed. Y. Katsuki, pp. 145-57. Tokyo: Igakushion.

With K. Kusano and S. Saito. Membrane changes in crayfish stretch receptor neuron during synaptic inhibition and under action of gamma-aminobutyric acid . *J. Neurophysiol.* 23:505-15.

With K. Ogura. Analysis of song of cicadas. *J. Insect Physiol.* 5:259-63.

1961 Nervous activities of the heart in crustacea. *Ergeb. Biol.* 24:287-311.

With K. Kusano. On the integrative synaptic potentials of Onchidium nerve cell. *Jpn. J. Physiol.* 11:96-101.

With K. Kusano and S. Saito. Membrane changes on Onchidium nerve cell in potassium-rich media. *J. Physiol.* 155:470-89.

With K. Kusano. Synaptic inhibition in giant nerve cell of Onchidium verruculatum. *J. Neurophysiol.* 24:167-75.

1962 With K. Kusano and K. Negishi. Physiological properties of electroreceptors of some gymnotids. *J. Neurophysiol.* 25:430-49.

With H. Morita. Electrotonic transmission between two nerve cells in leech ganglion. *J. Neurophysiol.* 25:721-31.

1963 With H. Morita. Coding mechanisms of electroreceptor fibers in some electric fish . *J. Neurophysiol.* 26:551-67.

1964 With K. Naka and S. Chichibu. Membrane properties of barnacle muscle fiber. *Science* 143:1446-48.

With K. Naka. The initiation of spike potential in barnacle muscle fibers under low intracellular Ca^{++}. *J. Gen. Physiol.* 48:141-62.

With S. Chichibu and K. Naka. The effects of various ions on rest ing and spike potentials of barnacle muscle fibers. *J. Gen. Physiol.* 48:163-79.

With H. Morita and K. Naka. Transmission through distributed synapses between two giant axons of the sabellid worm. *J. Comp. Physiol.* 13:453-60.

With G. Edwards and S. Chichibu. Relation between membrane potential changes and tension in barnacle muscle fibers. *J. Gen. Physiol.* 48:225-34.

1965 Relation of membrane properties of the giant muscle fiber of a barnacle to internal composition. *J. Gen. Physiol.* 48:55-57.

With S. Nakajima. Tetrodotoxin and manganese ions: effects on action potential of the frog heart. *Science* 149:1254-55.

With T. Szabo and P. S. Enger. Physiological properties of electroreceptors in the electrical eel, Electrophorus electricus. *J. Neurophysiol.* 28:775-83.

With T. Szabo and P. S. Enger. Electroreceptor mechanisms in a high-frequency weakly electric fish, Sternarchus albifrons. *J. Neurophysiol.* 28:784-99.

With T. Szabo. Le fonctionnement de certains electrorecepteurs. *J. Physiol* (Paris) 57(5):707-8.

1966 Membrane properties of the barnacle muscle fiber. *Ann. N.Y. Acad. Sci.* 137:1015-24.

With S. Nakajima. Effects of the intracellular Ca ion concentration upon the excitability of the muscle fiber membrane of a barnacle. *J. Gen. Physiol.* 49:807-18.

With S. Nakajima. Differences in Na and Ca spikes as examined by application of tetrodotoxin, procaine, and manganese ions. *J. Gen. Physiol.* 49:793-806.

With T. Szabo. Effects de dephasage au niveau d'organes sensoriels participants au d'electrolocation. *J. Physiol.* (Paris) 58(2):267-68.

With T. Szabo. Exploration intracellulaire de l'epithelium sensoriel de la vesicule de Savi chez Torpedo marmorata. *J. Physiol* (Paris) 58(5):621-22.

1967 With K. Takahashi. Surface density of calcium ions and calcium spikes in the barnacle fiber membrane. *J. Gen. Physiol.* 50:583-601.

With K. Takahashi. Resting and spike potentials of skeletal muscle fibers of salt-water elasmobranch and teleost fish. *J. Physiol.* 190:499-518.

With T. Szabo. A latency-change mechanism involved in sensory coding of electric fish (Mormyrids). *Physiol. Behav.* 2:331-35.

1968 With K. Takahashi and D. Junge. Excitation-contraction coupling in a barnacle muscle fiber as examined with voltage clamp technique. *J. Gen. Physiol.* 51:157-75.

With I. Cooke, J. Diamond, A. Grinnell, and H. Sakata. Suppression of the action potential nerve by nitrobenzene derivatives. *Proc. Natl. Acad. Sci. U.S.A.* 60:470-77.

With S. Chichibu and N. Simpson. Neuromuscular mechanisms of wing beat in hummingbirds. *Z. Vgl. Physiol.* 60:209-18.

With R. Gruener, H. Hayashi, H. Sakata, and A. Grinnell. A. Effect of external and internal pH changes on K and Cl conductances in the muscle fiber membrane of a giant barnacle . *J. Gen. Physiol.* 52:773-92.

1969 With H. Hayashi and K. Takahashi. Calcium and potassium currents of the membrane of a barnacle muscle fiber in relation to the Ca-spike. *J. Physiol.* 205:115-29.

With H. M. Brown, R. Meech, and H. Koike. Current-voltage regulations during illumination: photoreceptor membrane of a barnacle. *Science* 166:240-43.

1970 With H. M. Brown, H. Koike, and R. W. Meech. Membrane properties of a barnacle photoreceptor examined by the voltage clamp technique. *J. Physiol.* (London) 208:385-413.

1971 With H. M. Brown, H. Koike, and R. W. Meech. Electrical characteristics of a barnacle photoreceptor. *Fed. Proc.* 30:69-78.

With K. Toyama and H. Hayashi. Mechanisms of anion and cation permeations in the resting membrane of a barnacle muscle fiber. *J. Gen. Physiol.* 57:408-34.

With H. Koike and H. M. Brown. Hyperpolarization of a barnacle photoreceptor membrane following illumination. *J. Gen. Physiol.* 57:723-37.

With M. Henkart and Y. Kidokoro. Action potentials and excitation contraction coupling of muscle cells in Amphioxus. *J. Physiol.* 219:232-33.

With Y. Kidokoro. Na and Ca components of action potential in Amphioxus muscle cells. *J. Physiol.* 219:217-32.

1972 With A. D. Grinnell. Adaptations of the auditory nervous system for echolocation: studies of New Guinea bats. *Z. Vgl. Physiol.* 76:41-81.

With A. D. Grinnell. Studies of auditory neurophysiology in nonecholocating bats, and adaptation for echolocation in one genus, *Rousettus. Z. Vgl. Physiol.* 76:82-96.

With D. C. Eaton, A. E. Stuart, and N. P. Rosenthal. Cation selectivity of the resting membrane of squid axon. *J. Membr. Biol.* 9:373-84.

1973 Ca spike. In *Advances in Biophysics*, vol. 4, ed. M. Koitani, pp. 71-102. Tokyo: University of Tokyo Press.

1974 With K. Takahashi. Mechanism of anion permeation through the muscle fiber membrane of an elasmobranch fish, Taeniura lymma. *J. Physiol.* 238:109-27.

With Y. Kidokoro and M. Henkart. Electrical properties of obliquely striated muscle fiber membrane of Anodonta glochidium. *J. Comp. Physiol.* 90:321-38.

With J. Fukuda and D. C. Eaton. Membrane currents carried by Ca,

Sr and Ba in barnacle muscle fiber during voltage clamp. *J. Gen. Physiol.* 63:564-78.
With K. Takahashi. The anomalous rectification and cation selectivity of the membrane of a starfish egg cell. *J. Membr. Biol.* 18:61-80.
1975 Ca dependent action potential. In *Membranes, A Series of Advances*, vol. 3, ed. G. Eisenman, pp. 359-81. New York: Marcel Dekker.
With S. Ozawa and O. Sand. Voltage clamp analysis of two inward current mechanisms in the egg cell membrane of a starfish. *J. Gen. Physiol.* 65:617-44.
With S. Ozawa, K. Nicolaysen, and A. E. Stuarat. Signal transmission from photoreceptors to ganglion cells in the visual system of the giant barnacle. Cold Spring Harbor Symposium, vol. XL, pp. 563-70.
With O. Sand and S. Ozawa. Electrical and mechanical stimulation of hair cells in the mudpuppy. *J. Comp. Physiol.* A102:13-26.
1976 With K. Ikeda and S. Ozawa. Synaptic transmission reversibly conditioned by a single-gene mutation in Drosophila melanogaster. *Nature* 259 (5543):489-91.
With S. Miyazaki and N. P. Rosenthal. Potassium current and the effect of cesium on this current during anomalous rectification of the egg cell membrane of a starfish. *J. Gen. Physiol.* 67:621-38.
With M. Henkart. Localization of calcium binding sites associated with the calcium spike in barnacle muscle. *J. Membr. Biol.* 27:1-20.
With A. Miyazaki. Electrical properties of the Drosophila egg membrane. *J. Dev. Biol.* 53:91-100.
1977 With S. Miyazaki. Ca and Na spike in egg cell membrane. *Prog. Clin. Biol. Res.* 15:147-58.
With B. L. Brandt, Y. Kidokoro, and S. Miyazaki. Action potentials in the rat chromaffin cell and effects of acetylcholine. *J. Physiol.* 263:417-39.
With S. Miyazaki. Changes in excitability of the cell membrane dur

ing "differentiation without cleavage" in the egg of the annelid, Chaetopterus pergamontaceus. *J. Physiol.* 272:197-216.

With S. Ozawa and K. Nicolaysen. Neural organization of shadow reflex in a giant barnacle, Blanus nubilus. *J. Neurophysiol.* 40:982-95.

With S. Miyazaki, S. Krasne, and S. Ciani. Anomalous permeabilities of the egg cell membrane of a starfish in K^+-$T1^+$ mixtures. *J. Gen. Physiol.* 70:269-81.

1978 Differentiation of Na and Ca channels during the early development. In *Membrane Transduction Mechanisms,* vol. 33, ed. R. A. Cone and J. E. Dowling, pp. 189-97. New York: Raven Press.

With S. Miyazaki, W. Moody, and J. Patlak. Blocking effects of Ba and H ions on the K current during anomalous rectification in the starfish egg. *J. Physiol.* 279:167-85.

With L. A. Jaffe and R. T. Kado. The time course of cortical vesicle fusion in sea urchin eggs observed as membrane capacitance changes. *J. Dev. Biol.* 67:243-48.

With S. Ciani, S. Krasne, and S. Miyazaki. A model for anomalous rectification: electrochemical-potential-dependent gating of membrane channels. *J. Membr. Biol.* 44:103-34.

1979 With L. A. Jaffe. Electrical properties of egg cell membranes. *Annu. Rev. Biophys. Bioeng.* 8:385-416.

With Y. Kidokoro and A. Ritchie. Effect of tetrodotoxin on adrenaline secretion in the perfused rat adrenal medulla. *Nature* 278:63-65.

With M. Yoshii. Effects of internal K and Na on the anomalous rectification of the starfish egg as examined by internal perfusion. *J. Physiol.* 292:251-65.

1980 The Ca ion permeability of the cell membrane. *Jpn. Circ. J.* 44:239-48.

With S. Ciani and S. Krasne. A model for the effects of potential and external K^+ concentration of the Cs^+ blocking of inward rectification. *Biophys. J.* 30:199-204.

With M. Yoshii. Effect of temperature on the anomalous rectification of the membrane of the egg of the starfish, Mediaster aequalis. *J. Physiol.* 307:517-27.

1981 Membrane potential dependent Ca channels. In *Physiology of Excitable Membrane,* ed. J. Salanki, pp. 105-8.

General considerations in the study of the Ca^{++} channel. In *Molluscan Nerve Cell: Biophysics to Behavior,* vol. 1, ed. J. Koester and H. Byrne, pp. 33-40. Cold Spring Harbor Report in Neurosciences.

General properties of gated Ca transport. In *Mechanism of Gated Calcium Transport Across Biological Membranes,* ed. S. T. Ohnishi and M. Endo, pp. 3-9. New York: Academic Press.

With L. Byerly. Membrane biophysics of calcium currents. *Fed. Proc.* 40:2220-25.

With L. Byerly. Ca channel. *Annu. Rev. Neurosci.* 4:69-125.

With S. Yoshida and M. Yoshii. Transient and delayed K currents in the egg cell membrane coelenterate, Renilla koellikeri. *J. Physiol.* 318:123-41.

With H. Ohmori and S. Yoshida. Single K^+ channel currents of anomalous rectification in cultured rat myotubes. *Proc. Natl. Acad. Sci. U.S.A.* 78:4960-64.

1982 With L. Byerly. Calcium currents in internally-perfused nerve cell bodies of Limnea stagnalis. *J. Physiol.* 322:503-28.

With W. J. Moody. Block of inward rectification by intracellular H^+ in immature starfish oocytes of the starfish Mediaster aequalis. *J. Gen. Physiol.* 79:115-30.

With H. Ohmori. Studies of calcium channels in the rat clonal pituitary cells with patch electrode voltage clamp. *J. Physiol.* 331:231-52.

1983 *Ion Channels in Cell Membrane: Phylogenetic and Development Approaches*. Distinguished Lecture Series of General Physiologists Society. New York: Raven Press.

With L. Byerly. Ca channel. *Trends Neurosci*. 6:189-93.

With H. Ohmori. Studies of single calcium channel currents in rat clonal pituitary cells. *J. Physiol.* 336:649-61.

With Y. Fukushima. Voltage-gated Ca channel in mouse myeloma cells. *Proc. Natl. Acad. Sci. U.S.A.* 80:2240-42.

1984 With Y. Fukushima and M. Henkart. Potassium current in clonal cytotoxic T lymphocytes from the mouse. *J. Physiol.* 351:645-56.

With K. Kawa. Calcium and potassium currents in spermatogenic cells dissociated from rat seminiferous tubules. *J. Physiol.* 356:135-49.

With Y. Fukushima and R. E. Saxon. Variations of calcium current during the cell growth cycle in mouse hybridoma lines secreting immunoglobulins. *J. Physiol.* 355:313-21.

1985 With Y. Fukushima. Currents carried by monovalent cations through calcium channels in mouse neoplastic B lymphocytes. *J. Physiol.* 358:225-84.

1986 With Y. Fukushima. Ion channels of lymphocytes. In *Comparative Neurobiology: Modes of Communication in Nervous Systems,* ed. F. Strumwasser and M. Cohen, pp. 119-31. New York: Wiley and Sons.

With L. Schlichter and N. Sidell. Potassium channels mediate killing by human natural killer cell. *Proc. Natl. Acad. Sci. U.S.A.* 83:451-55.

With T. Iijima and S. Ciani. Effects of the external pH on Ca channels. Experimental studies and theoretical considerations using a two-site, two-ion model. *Proc. Natl. Acad. Sci. U.S.A.* 83:654-58.

With L. Schlichter and N. Sidell. K channels are expressed early in human T cell development *Proc. Natl. Acad. Sci. U.S.A.* 83:5625-29.

With N. Sidell, L. C. Schlichter, S. Wright, and S. H. Golub. Potassium channels in human NK cells are involved in discrete stages of the killing process. *J. Immunol.* 137:1650-58.

1987 With T. Iijima. Voltage dependent K channels in protoplasts of trap-lobe cells in *Dionae muscipula. J. Memb. Biol.* 100:73-81.

1988 With L. Byerly. Calcium channel diversity. In *Ion Channel Modulation* , ed. A. D. Grinnell, D. L. Armstrong, and M. B. Jackson, pp. 3-18. New York: Plenum Press.

With T. Hirano. Synaptic transmission between rat cerebellar granule and Purkinje cells in dissociated cell culture: effects of excitatory-amino acid transmitter antagonists. *Proc. Natl. Acad. Sci. U.S.A.* 85:934-38.

With E. E. Serrano and E. Zeiger. Red light stimulated an electrogenic proton pump in Vicia guard cell protoplast. *Proc. Natl. Acad. Sci. U.S.A.* 85:436-40.

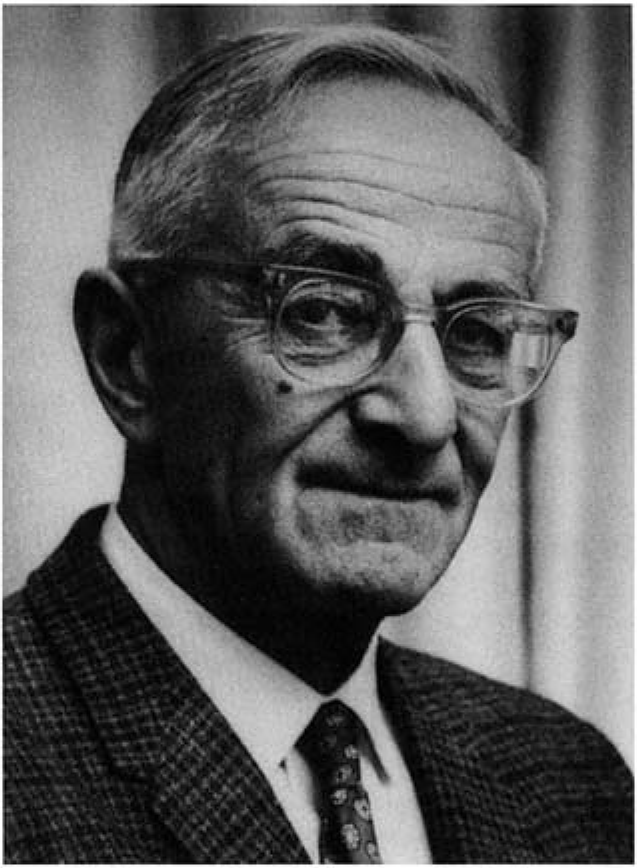
1989 With T. Hirano. Kinetics and distribution of voltage-gated Ca, Na and K channels on the somata of rat cerebellar Purkinje cells. Flugers Archiv. *Eur. J. Physiol.* 413:463-69.

1990 With V. Corvalan, R. Cole, and J. de Vellis. Neuronal modulation of calcium channel activity in cultured rat astrocytes. *Proc. Natl. Acad. Sci. U.S.A.* 87:4345-48.

With J. I. Schroeder. Repetitive increases in cytosolic Ca^{2+} of guard cells by abscisic acid activation of non-selective Ca^{2+} permeable channels . *Proc. Natl. Acad. Sci. U.S.A.* 87:9305-9.

With N. Yamashita. Membrane depolarization and intracellular Ca^{2+} increase caused by high external Ca^{2+} in a rat calcitonin-secreting cell line. *J. Physiol.* 431:243-67.

With N. Yamashita and S. Ciani. Effects of internal Na^{+} on the Ca channel outward current in mouse neoplastic B lymphocytes. *J. Gen. Physiol.* 96:559-79.



Bernhard Haurwitz

Bernhard Haurwitz

August 14, 1905–February 22, 1986

JULIUS LONDON

BERNHARD HAURWITZ WAS AN unusually productive physical scientist and educator throughout his adult life. His principal scientific interests and accomplishments were in the are of dynamic meteorology, that is, the application of mathematics and fluid dynamics to all scales of atmospheric motions. In addition to his many basic contributions to the study of short-period atmospheric wave motions, planetary waves, including atmospheric tides, and vortex motions in tropical cyclones, he wrote important papers on such subjects as atmospheric radiation, wave structure of noctilucent clouds, and attempts to document internal tides in the oceans. The main directions of his work were in both analytic and diagnostic investigations of the structure and motions of the atmosphere.

Although primarily a theoretician, Haurwitz enjoyed working with observed atmospheric and oceanic data. His analyses were always directed toward gaining insight into the physical structure and important physical processes in the atmosphere. This was already evident in his Ph.D. thesis on the relations between changes of atmospheric pressure and temperature and continued throughout his research career. In general, he preferred writing short papers, with the idea that they would be more apt to be read than long ones. At the memorial for Haurwitz at the National Center for Atmospheric Research (NCAR), Philip Thompson, who had an almost continuous association with him for about forty years, commented, "I gradually came to realize that the range of Bernhard's interests and contributions spanned virtually the whole range of atmospheric science."

His role as an educator went beyond the more than fifty years he spent in active involvement at different academic institutions. Two of his textbooks were still listed in the *Science Citation Index* covering the five-year period of 1988-92.

EARLY YEARS

Bernhard Haurwitz was born in Glogau, Germany, in 1905, to upper-middle-class parents. His father, Paul Haurwitz, was a reasonably successful merchant in the city. He had a younger sister, Ilse, who was born in 1907. While still a teenager, he developed an interest in astronomy and, together with his friend Wolfgang Gleissberg, became a cooperative solar observer, sending sunspot information to the central solar observatory in Zürich, Switzerland. This interest in solar phenomena stayed with him through his entire professional life.

Haurwitz completed his Gymnasium (high school) studies, specializing in classical languages (Latin and Greek) and in mathematics and physics. In 1923, at the age of eighteen, he enrolled at the University of Breslau, where he spent one and a half years before going on to the University of Göttingen where he studied mathematics, physics, and geophysics. In Göttingen he took courses from Richard Courant, Richard Frank, Emil Wiechert, and others. It was during that time that he developed an interest in meteorology as a result of preparing to present a seminar in his course in geophysics. The paper he reviewed was on the subject of atmospheric waves written by Ludwig Weickmann, then professor of geophysics at the University of Leipzig. He decided to apply to Professor Weickmann to write his Ph.D. thesis at the Geophysical Institute in Leipzig and was soon accepted.

Haurwitz arrived at the University of Leipzig in 1925 and began work on his thesis under Weickmann's direction. The thesis made use of then-available data from self-registering atmospheric sounding balloons. (Radiosonde observations became available only after their development in the late 1920s.) His thesis was motivated by observations that weather systems tended to move along with patterns of large surface-pressure changes. Haurwitz's results indicated that an atmospheric pressure decrease at the surface, accompanied by a surface-temperature increase, is associated with a pressure increase at levels near the tropopause, a relation to be anticipated if hydrostatic conditions obtain. This effect would suggest the existence of a layer in midtroposphere where the wind field was geostrophic and thus nondivergent, an important assumption made in the late 1940s at the time of early numerical weather prediction efforts as applied to two-dimensional flow at 500 mb.

After completing his dissertation (1927) and his second (habilitation) thesis (1931), Haurwitz became a lecturer at the University of Leipzig. It was there that he first heard guest lectures from the early British and Scandinavian pioneers in atmospheric and ocean dynamics—namely, Lewis F. Richardson, Vilhelm Bjerknes, and Harald U. Sverdrup. Haurwitz was impressed with the lectures and subject matter presented and arranged for a three-month visit to Oslo and Bergen in early 1929. Thus, he could interact with meteorologists who were in the forefront of research in geophysical fluid dynamics (Oslo) and synoptic meteorology

(Bergen). This research was of particular interest to him since his own studies at that time involved methods of solving the highly nonlinear problems of wave motions in the atmosphere and oceans by a simplification process based on perturbation approximations to the nonlinear equations. This technique continued to be used extensively before numerical methods of solution were practical as a result of the development of high-speed computers. Indeed, up to that time, applied mathematicians would quip that there were two types of differential equations—linear and nonsolvable! In an article written for the *Compendium of Meteorology* in 1951, Haurwitz reviewed the rationale for the use of the perturbation equations as a method of getting closed-form solutions to problems in atmospheric dynamics.

During his stay in Norway, Haurwitz spent most of his time working on problems of fluid dynamics with Scandinavian colleagues Halvor Solberg and H. U. Sverdrup and with a young student at that time, Jörgen Holmboe, with whom he frequently went skiing. As a matter of fact, one of the attractions for his Norwegian visit was the increased opportunity for cross-country skiing and mountain hiking, which were his favorite sports. These interests certainly played a nontrivial role some thirty years later in his decision to move to Boulder, Colorado.

While in Oslo he also occasionally visited with Carl Störmer, who was involved in a program of observations of the height of occurrence and main features of the polar aurora. This experience clearly contributed to his later interests and research applied to upper-atmosphere phenomena.

Upon his return from Norway, Haurwitz continued to work on the problem of wave motions in a compressible fluid, the general area of his main interest when he came to the Geophysical Institute in Leipzig. He used this subject for his "Habilitationsschrift." His research quickly became focused on the problem of short-wave "billow" clouds that appear at the interface of a two-layer model in the atmosphere (or oceans). By introducing assumptions of inhomogeneity, stratification, compressibility, and wind shear across the interface, Haurwitz was able to get good agreement between theory and observations of the billow cloud wave-lengths and periods. He returned to this problem off and on over the next forty years and extended his model applications in an attempt to explain waves associated with noctilucent clouds.

After completion of his thesis and professional examination, Haurwitz became a lecturer at the University of Leipzig. During the next two years he gave a set of three courses in atmospheric physics: atmospheric acoustics, meteorological optics, and atmospheric radiation. Haurwitz then felt that it would be interesting to spend some time abroad, and at the invitation of Carl-Gustaf Rossby, who was then associate professor at the Massachusetts Institute of Technology, he came to the United States in 1932 for what was supposed to be a relatively brief seven-month visit.

PROFESSIONAL ACTIVITIES IN THE UNITED STATES AND CANADA (1932-41)

Bernhard Haurwitz arrived in the United States in October 1932 to share a temporary appointment at MIT and the Blue Hill Observatory of Harvard. He divided his time between giving a series of lectures at MIT on problems related to the integration of the atmospheric perturbation equations and a research program at the Blue Hill Observatory involving, among other things, analysis of solar radiation data and their use in determination of atmospheric turbidity. Among the graduate students at MIT who attended his lectures and who later made significant contributions in meteorology and oceanography were Harry Wexler, Jerome Namias, Athelstan Spilhaus, and Raymond Montgomery.

During his stay in the Boston-Cambridge area, he also worked on a problem that had intrigued him for some time. In the absence of high-flying aircraft or other practical methods of measuring the midtropospheric pressure in and around hurricanes, it had commonly been assumed that tropical cyclones extended only to heights of about 2 to 3 km above the ocean surface. No one up to that time had attempted to determine the vertical extent of these storms, that is, the height at which the pressure would be horizontally uniform. Haurwitz assumed that atmospheric columns near the center and the outer part of the storm are each in approximate hydrostatic equilibrium and that the vertically averaged mean temperature near the center of the storm is warmer than that at the outer part. He was then able to show that the level of the pressure equalization—the height of uniform pressure around the storm—was approximately 10 km. This height range was, of course, substantially verified as both direct and indirect measurements became available. Moreover, he was also able to show that the shape of the eye of the storm approximated that of a funnel, as verified by later observations.

In early 1933 Haurwitz accepted an invitation from the seismologist Beno Gutenberg, a former colleague in Germany, to visit the California Institute of Technology in Pasadena where he gave lectures on atmospheric dynamics. Among the attendees at these lectures was Albert Einstein, who had just come from Berlin to spend the winter at Caltech. A short time after Haurwitz's arrival at Pasadena, Adolf Hitler was appointed chancellor of the German Reich. Both Haurwitz and Einstein independently chose not to return to Germany. Haurwitz decided that when his visitor's visa expired he would apply for a visa extension and investi gate the possibility of a position in Canada. He was able to get a research appointment in the physics department at the University of Toronto through a Carnegie Institution grant. However, it took two years, until the summer of 1935, before the clerical red tape was straightened out. Meanwhile, he spent those two years continuing his lectures and research at MIT and the Blue Hill Observatory.

In 1934 he married Eva Schick, who had done her academic studies in Germany in physics before immigrating to the United States. They went to Toronto in 1935 where he worked at the University of Toronto and the Canadian Meteorological Service for the next six years before they returned to the United States in 1941.

Haurwitz came to Toronto as a Carnegie Institution fellow (1935-37) in the physics department at the University of Toronto and continued as a visiting lecturer in the department until 1941. When the Carnegie fellowship ended, he took a position as meteorologist with the Canadian Meteorological Service (1937-41). The Canadian Service had set up a cooperative meteorological training program with the physics department at the University of Toronto, and each year he gave a regular graduate course in dynamic meteorology. In addition, he presented a series of eight lectures at the university on the subject of "The Physical State of the Upper Atmosphere." The lectures were based, in part, on the course he gave while he was at the University of Leipzig. They were published as a series in the *Journal of the Royal Astronomical Society* (Canada) and as a special short book in 1937. Although the material in that book is now almost completely out of date, it was the first book of its kind and summarized what was then known about the "upper atmosphere." A second edition was published in 1941, when a large-scale meteorological training program was started during the early period of World War II.

By 1940 there was also increased need for a new English-language textbook on dynamic meteorology. This gave Haurwitz the opportunity to refine and edit his lecture notes. His book, *Dynamic Meteorology*, was also published in 1941, at the time of the rapid increase in the training of meteorologists in the United States during World War II. The book was widely used as a standard textbook on dynamic meteorology for the next twenty years.

The meteorology program at the University of Toronto was also used to train people newly hired by the Canadian Meteorological Service. As a result, Haurwitz spent considerable time in Toronto preparing educational programs for weather forecasters and instructional booklets for the British Commonwealth Air Training Plan. In 1938 Eva gave birth to their son, Frank. (At the time Bernhard was giving a lecture at the university.)

When World War II started in 1939, Haurwitz, who still held a German passport, was classified as an "enemy alien" and had to report to the Royal Canadian Mounted Police once a month. But after a brief background check, that requirement was lifted. Being an enemy alien, however, did not interfere with his having access to the "secret" weather codes developed for use at that time or his being involved with coordinating the joint use of these codes by the meteorological services of the United States and Canada.

Despite all of these academic and semiadministrative duties, he still made time to work on a number of research problems, including fundamental studies of the motions of large-scale atmospheric disturbances. The latter resulted in three publications during the period 1937-40 that are still considered classic in the field of planetary waves in the atmosphere.

Haurwitz's study of planetary waves stemmed from his early interest as a graduate student in the theory of solar-induced atmospheric tides. The original motive for the 1937 paper, "The Oscillations of the Atmosphere," was to find an explanation for resonance of the solar semidiurnal pressure tide. However, the emphasis on that paper was on the class of planetary waves whose periods are long compared to a sidereal day and move westward relative to the mean zonal flow in which they are embedded. It was in that paper that Haurwitz derived the speed of low-frequency nondivergent planetary waves on a spherical earth that are typical of large-scale meteorological systems. An analogous result was derived by Rossby and collaborators in 1939 for the speed of long waves in midlatitudes based on the assumptions that the air motion was horizontal and nondivergent on a plane earth with no lateral shear in the basic westerly current. Only the latitudinal variation of the Coriolis parameter was considered, and the wave motion was assumed to be purely zonal. These waves are known as Rossby waves. In the two papers Haurwitz published in 1940, "The Motion of Atmospheric Disturbances" and "The Motion of Atmospheric Disturbances on a Spherical Earth," he extended the work of Rossby et al. and also rederived the formal results of the paper he published in 1937 to show direct application of the results to the observed "centers of action" of the northern hemisphere mean circulation system.

Haurwitz modified Rossby's assumptions to include the meridional extent of the wave, the effects of friction and of baroclinic forcing as, for instance, with zonal flow across a north-south coastline. His results indicated that the importance of the latitude variation of the Coriolis force (effect) on the wave speed decreased as the lateral extent of the disturbance became smaller. He also found that, when the effect of friction is applied to the perturbed flow, the amplitude of the disturbance decreases exponentially with time.

In addition, he showed that the effect of imposing a longitudinally fixed external force on the flow pattern, such as a land-ocean interface, generates stationary waves of approximately the same wavelength as the free oscillation of the system. This latter result was subsequently shown in the literature to apply as well to imposed fixed external forcing on planetary waves associated with north-south orographic surface features. The treatment of these horizontal planetary waves on a rotating spherical earth as developed in the two papers by Haurwitz in 1940 has given rise to the identification of this general class of waves as Rossby-Haurwitz waves, and they are so referred to in the literature.

In 1940 Sverre Pettersson, then chair of the Department of Meteorology at MIT, visited the Meteorological Service in Canada. He had known Haurwitz from the time when they were both in Norway. He invited Haurwitz to come back to the department at MIT, and in July 1941 Bernhard returned to Cambridge, this time as associate professor of meteorology. At the same time, Bernhard received an appointment as Abbott Lawrence Rotch Research Fellow at Harvard's Blue Hill Observatory.

When Haurwitz arrived at MIT in mid-1941, the department was already involved in the Army Air Corps/Navy advanced training program in meteorology. (MIT was then one of five universities participating in the national program that eventually trained over 10,000 weather officers.) While at MIT, Bernhard's principal academic responsibilities were to teach a course on dynamic meteorology and a course dealing with the physical principles of climate. The latter course led to the publication of a textbook, *Climatology*, coauthored with his colleague James Austin.

At the time of his return to Cambridge, the United States was not yet at war and Bernhard's official immigration status was as a "neutral alien." However, when the United States entered the war on December 7, 1941, he again became an "enemy alien." Early in 1942 a representative of the Army Air Force (formerly Army Air Corps) asked him to conduct a research program on long-range weather forecasting that was based on statistical techniques tried a decade earlier in Germany. The project was labeled as secret, and since Bernard was classified as an enemy alien, he was only able to supervise the program as the unofficial director. As he had anticipated, the results of the suggested technique showed no particular forecasting skill, but it did give him the opportunity to work with two very bright, young weather officers who had just completed the meteorology course—Richard Craig and Edward Lorenz—who remained colleagues and friends of his for a long time afterward.

During this time his research dealt with problems of atmospheric fluid dynamics, atmospheric radiation, and possible solar-weather relations. One of the more notable of the fluid dynamic studies involved a continuation of some of the problems dealt with in his habilitation thesis on the theory of wave motion in a stratified fluid. Waves in the atmosphere that give rise to cloud bands or billow clouds may occur as a result of convective patterns where the instability due to atmospheric stratification is an important factor in their development, or they may be a manifestation of internal waves that result from vertical wind shear across a surface of density discontinuity or within a shallow transition region. In a paper he published in 1947, he concluded that convection patterns and internal wave patterns are "largely one and the same phenomenon."

Haurwitz's return to the Cambridge-Boston area also provided him with the opportunity to resume his past association with Hurd Willett and other colleagues and friends at MIT and the Blue Hill Observatory. At Blue Hill he extended some of his earlier studies of observed solar irradi ance to develop empirical relations between solar irradiance measurements at the earth's surface and synoptic reports of total cloudiness and cloud type. He reasoned that, if such relations were found to be statistically reliable, they could be used to derive information on the solar irradiance at the surface in the absence of such measurements because total cloudiness and cloud type information was normally available from routine weather reports. The results of these studies have been used as historic references in recent years as more direct information on the dependence of cloud transmittance as a function of cloud type has been derived from satellite and surface observations.

Haurwitz's renewed association with the Blue Hill Observatory and its director, Charles F. Brooks, also revived an earlier interest of his on solar variability and its possible effect on the lower atmosphere. Most published studies on this subject were confined to statistical analyses of such possible solar relations. He felt that this approach was inadequate and stated that "when looking for empirical proofs of the connection between solar activity and weather, it is imperative to have a clear picture of the physical cause of the relation to be established." Although it was well known from both observations and theory that solar perturbations resulted in disturbances in the high atmosphere, he thought it important to provide a plausible physical mechanism by which anomalous solar behavior could either directly or indirectly affect the lower atmosphere in an observable fashion.

In 1948 Haurwitz qualitatively outlined such a proposed mechanism based on a physical-dynamic model of how a solar eruption could influence the pressure distribution in the troposphere. He postulated that the initial disturbance could come from increased ultraviolet radiation associated with a short-lived solar flare. This energy would be absorbed by ozone in midstratosphere over subsolar (equatorial) latitudes. Then in a simplified model he showed that the resultant heating would produce a net poleward air flow in the stratosphere that would result in a temporary reduction in the surface pressure at low latitudes and thus affect the low-tropospheric winds in the tropics. He later abandoned this model when observations indicated that his initial assumed solar energy perturbation was much too high, by orders of magnitude, and it was not possible to detect any of the predicted changes. Nevertheless, the concept proposed by Haurwitz of latitudinal differential heating of the ozone layer during times of high solar activity, as has been postulated over solar-rotation or solar-cycle periods, continues to be one of the main directions of study in the search for solar-weather relations.

Difficulties had been developing in his marriage, and in early 1946 Bernhard and Eva were divorced. Shortly afterward he accepted an invitation from Herbert Riehl to visit the Institute of Tropical Meteorology in Puerto Rico, which at that time was administered by the University of Chicago. The visit was planned for midsummer and early fall but was somewhat delayed until shortly before he received his naturalization papers. When he finally arrived in October, Haurwitz was able to take advantage of the results of a special program of three hourly radiosonde observations over the Eastern Caribbean to carry through a preliminary analysis of the diurnal and semidiurnal pressure and temperature oscillations at various levels in the troposphere. Determination of the characteristics of solar and lunar tidal oscillations in the oceans, at the earth's surface, and in the free air up to heights of 100 km continued to occupy him through the rest of his research career.

The following summer (1947), while he was a research associate at the Woods Hole Oceanographic Institution

(WHOI), Haurwitz was asked by Athelstan Spilhaus, then head of the Meteorology Department at New York University, to be the new chair of the department. Haurwitz accepted and in September 1947 moved to New York as professor and chair of the Department of Meteorology at NYU, where he built a strong and interactive department. He broadened its academic scope and soon changed its name to the Department of Meteorology and Oceanography. He also arranged to increase the size of the faculty to accommodate the growing number of graduate students in the department. He brought to the department an informal and collegial mode, particularly among graduate students and academic staff, that was characteristic of his own interpersonal and professional style.

While at NYU, Bernhard actively worked with other professional groups on problems of mutual interest. For instance, he developed a program of occasional joint seminars with the graduate mathematics department at NYU, which was then directed by Richard Courant, from whom he had taken a course when he was a student at the University of Göttingen. Participants in those seminars included faculty and graduate students of both the Department of Meteorology and Oceanography and the Courant Institute. The seminars gave both groups a chance to interact on applied mathematical problems of atmospheric interest, such as atmospheric tides and the stability of atmospheric waves.

Bernhard spent at least part of each summer (1947-55) as a research associate at WHOI, where he worked closely with many institution colleagues, namely, Andrew Bunker and Henry Stommel, and visiting associates, namely, Richard Craig, Hans Panofsky, and others. Although he couldn't swim, he did enjoy spending time on the beach near Falmouth relaxing with his son, Frank. They both enjoyed New England seafood and frequently walked on the beach hunting crabs. They both enjoyed, and often attended, the summer Gilbert and Sullivan operetta program in the area. Being at Woods Hole also gave Haurwitz the chance to be away from New York City during the summer months.

During his time at Woods Hole, Bernhard's research efforts were largely devoted to investigations of internal waves in the oceans and analysis of the observations and theoretical basis for the existence of tidal oscillations, particularly of the semidiurnal lunar period, associated with these waves. In 1950 he was able to show that, if the earth's rotation was included in the theoretical analysis, the periods of long internal waves would be reduced and their speeds increased so that internal waves in the oceans could contain motions that were characteristic of tidal oscillations. After careful statistical analysis of temperature and density data taken from ship observations at different depths and from remote recording thermometers, it was found that such periodic oscillations may indeed exist. But the data were rather noisy. In discussions about twenty-five years later of tidal influences within the oceans, Bernhard agreed that there was still a lack of substantial observational evidence of a lunar period at levels below the ocean surface.

Bernhard returned to the study of tidal oscillations in the atmosphere in the early 1950s. At that time he was interested in further development of resonance theory as an adequate explanation of the solar semidiurnal pressure oscillation and to document the global distribution of the amplitude and phase of these tides. These studies continued as a major part of his research activities during the remaining part of his active professional career at NYU and subsequently when he moved to Colorado. During these times he worked closely with a number of colleagues, namely, Sydney Chapman, Walter Kertz, Fritz Möller, Manfred Siebert, Gloria Sepulveda, and Ann Cowley (one-third of his papers on atmospheric tides were coauthored with Ann Cowley). The areas covered in his tidal studies involved analyses of (a) the diurnal and semidiurnal surface-pressure oscillation, (b) the lunar semidiurnal surface-pressure oscillation, and (c) the diurnal and semidiurnal wind oscillation in the mesosphere.

In 1956 Haurwitz published an analysis of the mean annual global distribution of the solar semidiurnal surface-pressure oscillation, $S2(p0)$. This was an extension and systematic improvement of the representation presented by George Simpson about forty years earlier based on a limited data set. His principal motivation for the study was to provide improved empirically derived descriptions of the two components of the $S2(p0)$ oscillation: (a) the east-to-west-traveling wave and (b) the stationary zonal wave.

He analyzed the geographic distribution of the mean annual observed amplitude and phase of $S2(p0)$ and computed spherical harmonic representations of the improved set of observed values. The computed maximum amplitude at the equator (1.2 mb) decreased to near zero at polar latitudes. The computed local phase was approximately 9.7 hours up to about ±50° but varied locally at higher latitudes, where the stationary wave was dominant. The results of this important paper were documented the following year in two studies with Gloria Sepulveda in which they verified that in the Northern Hemisphere poleward of about 70° the amplitude and phase of the semidiurnal pressure oscillations are mainly controlled by the standing wave.

In the winter semester of 1955-56 Bernhard spent a sabbatical leave visiting with Fritz Möller in Mainz, Germany, motivated, in part, by the wish to continue an earlier study done with Möller at NYU on the analysis of the global distributions of the semidiurnal temperature variation [$S2(T0)$] and its effect on the semidiurnal pressure variation [$S2(p0)$].

During that time, he had the chance to revisit Göttingen and meet with two of Bartels's students, Manfred Siebert and Walter Kertz, who were then working on the problem of direct thermal input as an alternative to resonance theory for the main forcing of the semidiurnal atmospheric tidal oscillation.

MIGRATION TO THE WEST

Bernhard had his first onsite experiences with the Rocky Mountain region in 1954 when he began his summer visits to the western states. He spent part of that summer at the Sacramento Peak Observatory (Sac Peak) at Sun Spot, New Mexico, at the invitation of Jack Evans, then director of the observatory. There he interacted with solar physicists who were involved in, among other things, studies of the effect of solar disturbances on radio propagation in the upper atmosphere. Discussions with colleagues at Sac Peak brought to mind his earlier attempts at finding a possible physical mechanism for solar influences on atmospheric variability. The gradual shift of his summer workplace locale from WHOI on the east coast to Sac Peak in New Mexico and later to the High Altitude Observatory (HAO) in Boulder, Colorado, represented a transition toward increased research application to problems of upper-atmosphere dynamics.

When Walter Orr Roberts, knowing of Bernhard's desire to spend some time away from New York, asked him to participate in the HAO summer program dealing with solar-terrestrial relations, Bernhard agreed and consequently spent the summers of 1957 and 1958 as a visiting research associate with the High Altitude Observatory. In 1959 he accepted Walt's offer of a joint, full-time appointment as professor of geophysics at the University of Colorado and research associate at HAO.

The attractions in Boulder, both intellectual and envi ronmental, were many. Bernhard was able to work in more relaxed surroundings than before, especially with a minimum of administrative responsibilities. After 1959, when he became a permanent resident in Boulder, he would go to the mountains almost every weekend—hiking during summer and fall and ski touring or snowshoeing during the winter and spring. Frequently he would go hiking with his son when Frank visited Boulder during the summer or with local or visiting colleagues. One of those colleagues was Sydney Chapman, who was a member of the research staff of HAO and with whom Bernhard maintained a close association. They had strong overlapping scientific interests and a shared appreciation of Boulder because of, among other things, its proximity to the many nearby mountain trails.

One of Bernhard's hiking companions was Marion Wood, a scientist working at the National Bureau of Standards in Boulder and a native of Colorado who shared his appreciation of the mountains and associated outdoor activities. Bernhard and Marion were married in January 1961 and were together until his death twenty-five years later.

Bernhard and Marion went to Europe during the summer of 1961 for an extended visit to Switzerland, Austria, and Germany. This was their first trip abroad together, and it represented a somewhat delayed honeymoon. They participated in scientific symposia in Arosa and Vienna and went to Munich for three months at the invitation of Fritz Möller, who was then professor of meteorology at the Meteorological Institute in Munich. Bernhard held a professorial chair at the university for the summer and gave a course on atmospheric dynamics. During his stay in Munich, he worked principally on a representation of the global distribution of the daily variations of surface temperature through the use of Legendre functions. Some of the results of that study were used in his later discussion of the possible ther mal excitation of the observed diurnal surface pressure oscillation.

After five years at the University of Colorado, Haurwitz decided in 1964 to move to a full-time position at NCAR as a senior scientist with the Advanced Study Program, which he directed for a three-year period (1967-69). He continued his appointment at NCAR until his retirement in 1976, when he became a senior research associate. In 1964 he also started his affiliation with the Geophysical Institute of the University of Alaska, first as a research associate and then as a visiting professor. He and Marion went to Fairbanks for three months in what was to become an almost annual visit until the winter of 1985.

Soon after he arrived at the Geophysical Institute in Fairbanks, Bernhard had the opportunity to continue to work on a problem that had intrigued him since his visit with Carl Störmer in Oslo some thirty-five years before. Störmer had been an early and diligent observer of noctilucent clouds (NLC). In 1930 Haurwitz was involved in a theoretical analysis of the dynamics of billow clouds in the lower troposphere, and Störmer thought that he, Haurwitz, might find applications of the theory to the observed waveforms in noctilucent clouds. In 1961 Bernhard published a paper that attempted to draw an analogy between billow clouds that form at an interface between two layers in the troposphere and billow clouds observed at the top of the mesosphere. As a result of preliminary analysis, however, he concluded that "it appears likely that the billow clouds observed in noctilucent clouds are manifestations of internal waves" rather than windshear.

At the Geophysical Institute, Bernhard met Benson Fogle, who was then a graduate student working with Sydney Chapman. Fogle had been collecting data on NLC observations made in polar regions, and in 1966 they wrote a re view paper describing what was then known of the observed characteristics of these clouds. Then in 1969, after Fogle joined NCAR, they published a theoretical analysis of the origin of the wave forms of noctilucent clouds. Observations indicated that the clouds generally took on two different forms: high-frequency, short-wavelength billow clouds and lower-frequency, longer-wavelength bands. They proposed that the shorter lifetimes for billow clouds were probably due to viscous damping, which is more effective for shorter than longer wavelengths. On the basis of their analysis they concluded that the wind shear in the layer of the NLC bands was certainly smaller than that required if these bands appeared as an interface wave near the mesopause, and they suggested that both bands and billow clouds are caused by internal gravity waves. Bernhard became convinced that the problem of the origin and energy source, particularly for the high-frequency component of the NLC, could not be definitively resolved without a carefully designed observational program to measure NLC heights, wavelengths, and amplitudes of the different waveforms.

During this time, Bernhard continued with his studies of atmospheric tides. It had long been known that the solar atmospheric surface pressure tide is thermally rather than gravitationally produced. However, the observed amplitude of the diurnal tide is smaller than that of the semidiurnal tide, which is apparently inconsistent with the relative amplitudes of the diurnal and semidiurnal temperature oscillations. In a paper published in 1965 Bernhard pointed out that "one of the main problems of atmospheric tidal theory is to explain the small size of $S1(p0)$ as compared to $S2(p0)$." It was by then generally agreed that resonance could not be the major cause for the large amplifications of $S2(p0)$. Resonance theory, normally accepted up to ten years earlier to explain the dominance of $S2(p0)$, required that the atmo sphere have a free period very close to twelve hours. This would call for an upper-stratospheric temperature of about 350 K, 75 K higher than observed. By the early 1960s, however, it had been shown by Siebert, Butler, Small, and others, that direct heating by absorption of solar radiation by water vapor and ozone in the troposphere and stratosphere could largely account for the observed amplitude of $S2(p0)$.

In the 1965 paper Haurwitz presented for the first time a spherical harmonic analysis of the worldwide geographic distribution of $S1(p0)$, similar to that done earlier for $S2(p0)$, to document the observed relative amplitudes of the two principal components of the surface pressure solar tide and to explain the apparent suppression of $S1(p0)$. He showed that the main part of $S1(p0)$ was a westward-traveling wave with an equatorial amplitude of ~0.6 mb, one-half that of $S2(p0)$. Also, the average amplitude of the diurnal oscillation decreased strongly with latitude, and the diurnal wave, unlike the semidiurnal wave, was strongly modified by properties of the lower boundary such as orography and the distribution of land and water surfaces. Bernhard, however, erroneously attributed the excitation of $S1(p0)$ to the daily surface temperature oscillation, $S1(T0)$. At the time of the analysis, he did not realize that the representation of $S1(p0)$ by Hough functions should contain negative equivalent depths, as later pointed out by Richard Lindzen and others. The excitation of such Hough modes would result from absorption of solar radiation principally from water vapor and ozone in the lower and upper stratosphere, respectively. For a number of reasons, the propagation of this energy from the source regions to the surface is not very effective, thus producing a diminished $S1(p0)$.

In his last major study of atmospheric tides (completed in 1973), Bernhard, together with Ann Cowley, presented an analysis of the quasi-global distribution and seasonal varia tion of the diurnal and semidiurnal pressure oscillations. Again, they performed spherical harmonic analyses of the station data, and the wave characteristics were expressed by associated Legendre functions and Hough functions. They extended their earlier studies to higher-order wave numbers and confirmed that the dominant component of the diurnal wave was zonal wave number 1 and that for the semidiurnal wave was zonal wave number 2. The more complete study again showed that at the equator the ratio of the relative amplitudes of $S1(p0)$ to $S2(p0)$ was approximately 1:2. $S2(p0)$ was found to be much more regular than $S1(p0)$, a result that is consistent with the nature of the forcing of the two waves. The results of this study are cited in the literature as one of the standard references on atmospheric tides.

While at NCAR, Bernhard would frequently give courses at Colorado State University and in 1973 he and Marion moved to Fort Collins. For the next three years he divided his time among CSU, NCAR, and the Geophysical Institute at Fairbanks. In 1976 he resigned his formal NCAR position but kept his ties to NCAR as a senior research associate.

Bernhard was elected to the National Academy of Sciences in 1960, and in 1964 he was elected to the Deutsche Akadamie der Naturforscher Leopoldina (the German Academy of Sciences, founded in 1562). He was awarded the Order of Merit First Class by the Federal Republic of Germany in 1976 for his efforts in helping German meteorologists return to the mainstream of the international scientific community in the years following World War II. Bernhard received the prestigious Carl Gustaf-Rossby Award for Extraordinary Scientific Achievement from the American Meteorological Society in 1962, and in 1972 he received the Bowie Medal of the American Geophysical Union.

In December 1985 while he was in Fairbanks, Bernhard developed a chest infection that was diagnosed as pneumonia, and he returned with Marion to Fort Collins. He was hospitalized in January, and on February 27, 1986, he died of renal failure.

Bernhard represented a prime example of a person who successfully combined superior teaching with excellence in research by removing the unnatural barrier that often separates the two. He was unpretentious, and although he did not suffer fools, his interchanges with students and colleagues were never marked with derogation. It is clear that he left a strong imprint on his colleagues and students through his writings and lectures. Both were outstanding examples of tidiness and clarity with a studied avoidance of jargon, particularly when dealing with complex and difficult subjects.

IN PREPARING THIS MEMOIR, considerable use was made of the material contained in a series of papers, "Meteorology in the 20th Century—A Participant's View," by Bernhard Haurwitz, published in 1985 in the *Bulletin of the American Meteorological Society* (vol. 66, pp. 281-91, 424-31, 498-504, 629-33), and *Conversations with Bernhard Haurwitz*, by George W. Platzman (NCAR/TN-257, 1985). I am indebted to George Platzman for many discussions about Bernhard and for his comments on an early draft of this memoir.

Selected Bibliography

A full bibliography is contained in *Conversations with Bernhard Haurwitz*, by George W. Platzman, NCAR/TN-257, June 1985, National Center for Atmospheric Research, Boulder, Colo.

1927 Beziehungen zwischen Luftdruck- und Temperaturanderungen. Ein Beitrag zur Frage des Sitzes dur Luftdruckschwankungen. (Doctor's thesis.) *Veroeff. Geophys. Inst. Univ. Leipzig* 3:266-335.

1930 Zur Berechnung von oscillatorischen Luft- und Wasserstromungen. *Gerlands Beitr. Geophys*. 27:26-35.

1931 Zur Theorie der Wellenbewegungen in Luft und Wasser. (Habilitationsschrift.) *Veroeff. Geophys. Inst. Univ. Leipzig* 5(1).

Über die Wellenlange von Luftwogen. *Gerlands Beitr. Geophys*. 34:213-32.

1932 Über die Wellenlange von Luftwogen (2. Mitteilung). *Gerlands Beitr. Geophys*. 37:16-24.

1935 On the change of the wind with elevation under the influence of viscosity in curved air currents. *Gerlands Beitr. Geophys*. 45:243-67.

The height of tropical cyclones and of the "eye" of the storm. *Mon. Weather. Rev*. 63:45-49.

1937 *The Physical State of the Upper Atmosphere*. Toronto: Royal Astronomical Society of Canada.

The oscillations of the atmosphere. *Gerlands Beitr. Geophys*. 51:195-233.

1940 The motion of atmospheric disturbances. *J. Marine Res.* 3:35-50.

The motion of atmospheric disturbances on a spherical earth. *J. Marine Res.* 3:254-67.

1941 *Dynamic Meteorology*. New York: McGraw-Hill.

1944 With J. M. Austin. *Climatology*. New York: McGraw-Hill.

1947 Internal waves in the atmosphere and convection patterns. *Ann. N.Y. Acad. Sci.* 48:727-48.

1948 Solar activity, the ozone layer, and the lower atmosphere. In *Centennial Symposia*, Harvard Observatory Monograph, vol. 7, pp. 353-69.

1951 The motion of binary tropical cyclones. *Arch. Meteorol. Geophys. Bioklimatol*. A4:73-86.

1952 With R. A. Craig. Atmospheric Flow Patterns and Their Representation by Spherical-Surface Harmonics. Geophysics Research Paper, No. 14.

1956 The geographical distribution of the solar semidiurnal pressure oscillation. Meteorology Paper 2, No. 5.

1959 With H. Stommel and W. H. Munk. On the thermal unrest in the ocean. In *The Atmosphere and the Sea in Motion*, Rossby Memorial Volume, ed. B. Bolin, pp. 74-94. New York: Rockefeller Institute Press.

1961 Wave formations in noctilucent clouds. *Planet. Space Sci.* 5:92-98.

Frictional effects and the meridional circulation in the mesosphere. *J. Geophys. Res.* 66:2381-91.

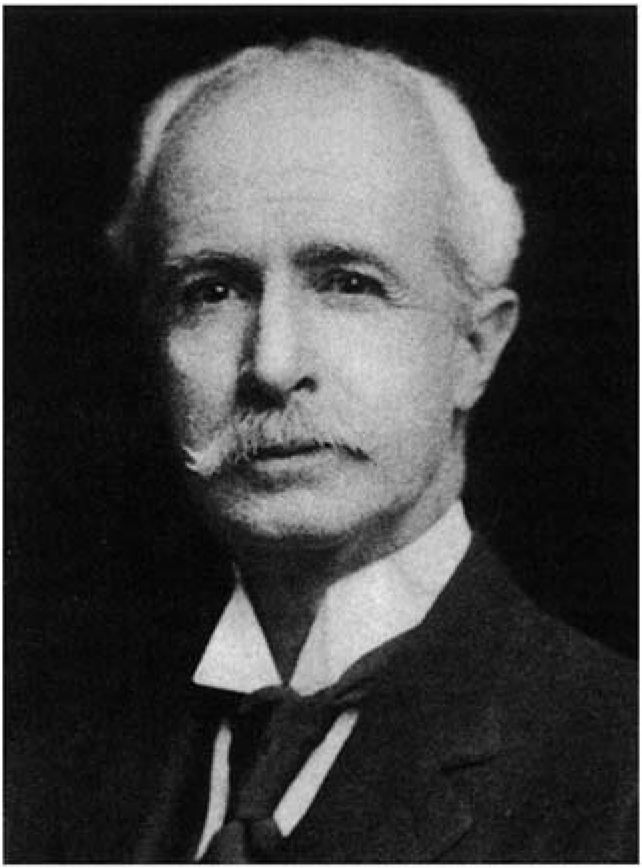
1964 Tidal Phenomena in the Upper Atmosphere. Technical Note No. 58. Geneva: World Meteorological Organization.

1965 The diurnal surface-pressure oscillation. *Arch. Meteorol. Geophys. Bioklimatol*. A14:361-79.

1969 With B. Fogle. Wave forms in noctilucent clouds. *Deep-Sea Res.* 16 (Suppl.):85-95.

With A. D. Cowley. The lunar barometric tide, its global distribution and annual variation. *Pure Appl. Geophys*. 77:122-50.

1973 With A. D. Cowley. The diurnal and semidiurnal barometric oscillations, global distribution and annual variation. *Pure Appl. Geophys*. 102:193-222.



Jos. P. Iddings

Joseph Paxson Iddings

January 21, 1857–September 8, 1920

H. S. YODER, JR.

JOSEPH PAXSON IDDINGS WAS an outstanding leader of petrology widely cited at the turn of the twentieth century, although little known to the present generation of petrologists. He was one of a small group to introduce, about 1880, the microscopic investigation of rocks to the United States and apply the petrographic observations to the then-new inquiry of the origins of rocks called petrology. His fading into the history of science can be attributed no doubt to his gentlemanly, retiring nature and his early withdrawal from the academic and societal scene. Nevertheless, Iddings's record of discovery, both observational and theoretical, initiated many of the ideas that served the more heralded petrologists who followed him. Those ideas, for which he was reluctant to claim originality, were "established or improved by subsequent research."

The writing of Iddings's biography was originally assigned to his lifelong friend, C. Whitman Cross, to whom he had given his autobiographical manuscript, "Recollections of a Petrologist," for editing and publication. Unfortunately, Cross died (in 1949) before a biography could be prepared or the autobiography published. Iddings's manuscript, dated

March 19, 1918, was not among the papers retained by Cross's namesake grandson but was discovered by Carol A. Edwards in the Field Records Library of the U.S. Geological Survey in Denver. The present memoir was undertaken before the writer was alerted by Dr. E. L. Yochelson of the availability of the autobiography. The principal incentive resulted from a renewed appreciation while investigating the history of petrology[1] of the vital role Iddings played in developing the quantitative aspects of petrology.

Joseph Paxson Iddings was born in Baltimore, Maryland, the second son of William Penn Iddings (1822-1906) of Philadelphia and Almira Gillet (1826-96) of Baltimore. His father was a wholesale dry-goods merchant (1900 census). His grandfather was Caleb Pierce Iddings (1778-1863), who built the family estate in 1855 in Brinklow, Maryland, where Joseph later lived. The genealogy of the Iddings family has been established through five generations and is available in open file at the Montgomery County (Maryland) Historical Society in Rockville. Caleb Pierce Iddings was a Quaker but was "disowned" for marrying "out of the unity." For this reason there are no Quaker records in Philadelphia of the family after 1812, the date of Caleb's marriage. Joseph was named after the husband, Joseph S. Paxson (1814-89), of William's older sister, Deborah J. Iddings (1815-77).

EDUCATION

After a brief stay in New York City, Joseph Iddings's father established a home in Orange, New Jersey (100 High Street) when Joseph was about ten years old. With the preparation at the private school of Rev. F. A. Adams, Iddings registered for the civil engineering course at the Sheffield Scientific School of Yale University. His father had recommended that he become a mining engineer in light of

Joseph's early interests in collecting rocks and butterflies. According to the records of his class of 1877, he was treasurer of the Yale Football Club, recording secretary of the Yale Society of Natural History, and class treasurer. In his freshman year, Iddings divided a prize for German, a skill that was to prove especially useful to him. In his junior and senior years, respectively, he received prizes in mathematics and civil engineering. He participated in the Alpha Chi, Phi Gamma Delta, and Berzelius societies.

Following graduation at which he was a commencement speaker, Iddings spent the next year at Yale in graduate studies in chemistry and mineralogy. He also assisted in courses in mechanical drawing and surveying, but it was the ongoing study of George Wesson Hawes (1848-82) on thin sections of New Hampshire granites that attracted his attention. The academic year of 1878-79 was spent at the Columbia School of Mines in New York City under the tutelage of John S. Newberry (1822-92). In late spring Iddings abruptly changed directions toward geological research as a result of the influence of the enthusiastic Clarence King, who had lectured at Yale; the fascinating microscopic work of Hawes; and a general loss of interest in mining as a profession. In the fall of 1879, on the recommendation of G. W. Hawes, who was then studying in Heidelberg, Iddings became a student of Karl Harry Ferdinand Rosenbusch (1836-1914), the most outstanding petrographer of the day. This opportunity arose while Iddings was awaiting a response to his application to the newly formed U.S. Geological Survey under the directorship of Clarence King. During July 1879 he learned that his young pastor, Joseph A. Ely of the Orange Valley Congregational Church, was to tour the Swiss Alps, and it seemed a golden opportunity to see spectacular geology in his company and then spend the winter with Rosenbusch. His experiences under the enthusiastic

Rosenbusch resulted in Iddings setting a course for a career in petrography.

CAREER COURSE DETERMINED

His three-week tour in the Alps, two months of private language study, and five months with Prof. Rosenbusch were recorded in great detail in his diary and letters to his family. These were summarized with considerable literary style in his autobiography. Iddings's reception of the first lecture in German from Prof. Rosenbusch is especially descriptive:

> It is a positive pleasure now to hear him lecture, to listen to him roll off those long, and to us, complicated sentences; here and there throwing in a phrase in parentheses, which is rendered like lightning; and then the whole wound up with a string of participles and infinitives that have a most pleasing effect, when someone else has to get them off. It's like watching the development of some great piece of fireworks. It is certainly a complicated language. You can see how he has to figure out his cases and endings, and have everything in his mind's eye before he begins his sentence. Sometimes he may want to change the number or case of his noun, after he has gone on for some time qualifying it with innumerable adjective phrases.

The lectures and almost private laboratory sessions with Rosenbusch had great impact on Iddings and significantly influenced the course of his future career in petrography. Although King had recommended Prof. Zirkel in Leipzig over Rosenbusch as a tutor, Iddings stayed in Heidelberg. Had he gone to Leipzig he would have met C. Whitman Cross, who became his lifelong friend several years later. Iddings's friendship with Rosenbusch continued for many years until their "views diverged seriously and correspondence ceased." In Rosenbusch's instruction, emphasis had been placed on mineral composition and rock texture with little reference to chemical composition, a factor Iddings eventually believed was dominant. This view no doubt arose from his close association with Samuel Lewis Penfield, a classmate at Yale, who was then doing graduate work on chemical mineralogy under Prof. George Jarvis Brush. There was "little or no discussion of the origin and mode of eruption of igneous rocks" and nothing on the physical chemistry of magmas. Nevertheless, Iddings was captivated by the study of rock thin sections, presented with great enthusiasm by Rosenbusch. He was indeed impressed by the beauty of the colored minerals and especially the brilliancy of their interference colors, which he related to the colorful stained glass in the windows of his church.

"HIGHEST EXPECTATIONS"

Iddings had the good fortune to arrange for his return to the United States on the same ship as Arnold Hague, who was returning from studies in China. Hague was to be his first supervisor at the U.S. Geological Survey, his appointment having been secured by mail through Clarence King. By obtaining a position in the USGS[2] Iddings hoped to realize his "highest expectations." During May and June of 1880, he worked as a temporary assistant to Hague at the American Museum of Natural History in New York, where King had "temporarily" stored the rock collections from the 40th Parallel Survey.

Iddings's next assignment with Hague took him to the mining district around Eureka, Nevada, where he mapped igneous rocks. There he shared a tent with Charles D. Walcott, who was later to become director of the U.S. Geological Survey, also assisting him in collecting fossils. As a result of his first field efforts, Iddings developed a very cautious attitude toward naming a rock, especially one where crystals could not be identified by eye. An ideal outcrop of granite with off-shooting dikes led him to think that rock texture was governed by the physical conditions attending solidification. It was twelve years after the fieldwork was completed before his microscopical petrography of the rocks from the Eureka District was published by Hague (1892) as Appendix B. A printed note dated November 1893 that was glued to the first page of the monograph expressed Iddings's dismay over the delayed publication and also the fact that this was "a production of the first year of the writer's work in this field of research, and as such needs no apology." In this appendix the term "phenocryst" was introduced[3] to describe the megascopically visible crystals in a fine-grained groundmass of a porphyritic rock, but the term appeared in print earlier (Iddings, 1889). Iddings's part of the monograph is also noteworthy for the method by which he determined the composition of feldspars, a method that was attributed to A. Michel-Lévy[4] more than ten years later. He also provided strong evidence for the gradational change in composition of the plagioclases—first proposed by T. Sterry Hunt[5] and later attributed to G. Tschermak[6]—a major concept to which he eventually contributed to its experimental demonstration (Iddings with Day and Allen, 1905). In addition, Iddings described a "red laminated mineral," a common alteration of olivine that was later described as "iddingsite" by Lawson.[7] The alteration process became known as "iddingsization."[8]

Before returning to the U.S. Geological Survey offices at the American Museum of Natural History in New York, Iddings spent a week with George F. Becker examining the volcanic rocks of the Washoe District, Nevada, previously examined microscopically by Zirkel. While in Virginia City, Nevada, he met Carl Barus, with whom he reviewed the mathematics of certain physical phenomena being studied by Becker. In New York, Hague, Iddings, Walcott, and Becker cooperated on the study of the Eureka and Washoe rocks and fossils as well as those from the earlier 40th Parallel Survey. It was this experience that persuaded Iddings that the most satisfactory way of studying rocks is to examine a large collection of closely related rocks—a philosophy he was to embellish in later years.

Iddings's first paper in print was a description with Hague (1883) of the principal volcanoes of the Sierra and Cascade ranges. They were impressed with the "gradations in the microstructure in the groundmass of rocks of the same mineral composition from a purely glassy form to one wholly crystalline. …" The second paper, also with Hague (1884), contained notes on the volcanic rocks of the Great Basin. In it they recognized the chemical relationship between olivine and hypersthene; as the rocks became higher in silica, hypersthene took the place of olivine. Their first attempt at chemico-mineralogical generalization was of exceptional importance and became a major factor in petrologic theory.

In their discussion of the Washoe District, Nevada, igneous rocks, Iddings and Hague (1885) attacked the widely held view shared by Becker that there was a distinction between Tertiary and pre-Tertiary igneous rocks. After examining Becker's large collection and material from the extensive mining network in the celebrated Comstock lode around Virginia City, they concluded that all the rocks were of Tertiary age. In their view the Comstock lode occupied a fissure along a fault line in rocks of Tertiary age and "could not be considered as a contact vein between two different rock masses." They held that the structural character of eruptive masses was not a function of their age but of the physical condition controlling crystallization.[9]. The paper did not "promote good fellowship" with Becker, but they eventually became friends despite continuing opposing views. On the other hand, the paper was widely acclaimed in Europe by the leading petrographers of the day.

MAPPING YELLOWSTONE NATIONAL PARK

Iddings's major field experience under the leadership of Hague was in Yellowstone National Park, established in 1872. Seven consecutive field seasons (1883-90) were spent in the region, where the focused on Obsidian Cliff, Electric and Sepulchre mountains, Crandall Basin, and Haystack Mountain. From an examination of the now-famous Obsidian Cliff, Iddings described the lithophysae (hollow spheres due to expanding gas bubbles), spherulites (spherical bodies with radiating crystals), columnar partings, and variations in the degree of crystallization, and he emphasized the role of water in magmas. Within the lithophysae he discovered the first natural occurrence of fayalite, the iron end member of the olivines, previously identified in lumps of slag carried as ship's ballast and dumped on a beach in the island of Fayal in the Azores. He realized that the inflation of pumiceous glass was due to escaped gases and appreciated the nature of layers described as welded tuff, outlining the process itself.

The intrusive rocks of Electric Mountain and the extrusive rocks of Sepulchre Mountain provided an exceptional opportunity for comparison after it was established that the two groups of rocks had essentially identical chemical compositions. The glassy extrusive andesites, with pyroxene and brown or red hornblende phenocrysts, contrasted with the coarsely crystalline diorites containing biotite and green hornblende. The different assemblages from the same bulk composition were attributed by Iddings to different conditions of crystallization. Recent experimental studies on the oxidation of hornblende and the breakdown of biotite verified this important relationship also emphasized by Washington.[10] In addition, Iddings viewed the magma as a homogeneous fluid in which the constituents could combine in different mineralogical associations depending on the conditions of crystallization. He also recognized that volatiles contained in magma were more effective as mineralizers when in the magma conduit, in contrast to a magma that reached the surface.

Crandall Basin and Haystack Mountain were also centers of old volcanoes, and the data collected by Iddings reinforced his views on the role of the physical conditions attending consolidation in defining the mineral assemblages. In February 1890 he took a two-month trip to England to meet J. J. H. Teall, A. Harker, and J. W. Judd; pay his respects to Rosenbusch in Heidelberg; visit Vesuvius and the Sicilian region; and stop in Paris to see A. A. Lapparent. Michel-Lévy was ill, and F. Fouqué and A. Lacroix were on Easter vacation. The summer of 1890 was spent studying the eastern and central portions of the quadrangle immediately north of Yellowstone Park, with Louis V. Pirsson as his assistant. The western and northern parts were explored by W. H. Weed. The publication of that work (in 1894 by Iddings and Weed) on the Livingston, Montana, quadrangle constituted the first folio of the geological atlas of the United States.

Sandwiched between the work on the Yellowstone rocks, Iddings managed after office hours to translate the second edition of the first volume of Rosenbusch's book, *Mikroscopische Physiographie der petrographisch wichtigen Mineralen*.[11] He abridged the book to serve the needs of the average student, eliminating most of the historical portions and inserting notes on American occurrences. After a review by George H. Williams of the Johns Hopkins University, it was published in 1888, with revised editions in 1889, 1892, and 1898; publication was terminated because of copyright problems, and Iddings was beginning to think about preparing a textbook on rock minerals himself. He collected and sum marized prevailing views on the crystallization of igneous rocks in 1889; however, his own philosophy on the origin of igneous rocks was put forth in 1892. That paper had the same effect as N. L. Bowen's classic of 1928 and established Iddings as a leader in petrologic thought.[12]

The year 1892 may have been an intellectual triumph for Iddings personally, but it was a disaster for the U.S. Geological Survey. On July 14, 1892, the appropriations for the Geologic Branch were severely cut and all fieldwork was stopped.[13] Iddings's position as geologist was eliminated! (Major Powell's friend, Joseph S. Diller, head of the petrographic laboratory in Washington, was retained by shifting him to a temporary position, in preference to Iddings.) Iddings had been considered a possible successor to James D. Dana, who had relinquished his duties at Yale in October 1890 due to ill health, but Dana felt his "experience in general geology too slight," an objection he later withdrew. Nevertheless, Iddings turned to a university position. After turning down an offer from Leland Stanford, the University of Chicago offered him an appointment in August 1892 as associate professor of petrology, the first chair in petrology in the world.

RELUCTANT TEACHER

The new Department of Geology at the University of Chicago was staffed with a spectacular group: R. D. Salisbury, R. A. F. Penrose, Jr., and J. P. Iddings, with T. C. Chamberlin as chairman. In addition, there were three nonresident professors: C. R. van Hise, W. H. Holmes, and C. D. Walcott, who was never able to attend and resigned after the second year. Iddings disliked teaching and objected to teaching mineralogy and crystallography in addition to petrology. Although only two Ph.D. Thesis (Charles H. Gordon, 1895, and William H. Emmons, 1904) were completed under

Iddings, advanced degrees were not yet a necessity in the academic world. He started teaching in January 1893 and was immediately confronted with the problem of the kind of rock classification to be presented to the students. The classification of Rosenbusch was based at first on textural features and then on a geological occurrence basis, neither scheme appealing to Iddings. He believed that the chemical composition of a rock was fundamental, whereas the mineral assemblage, texture, and structure were dependent on the conditions of formation.

Iddings "approached the problem of petrological instruction as a student among students, knowing how many things were as yet undetermined, how many were matters of opinion, and to what extent definitions of rock kinds (types) were arbitrary and illusory." Early in 1983 he wrote to his friends C. Whitman Cross, Louis V. Pirsson, and George H. Williams for their opinions on classification. The response was extensive, and Cross in particular considered the query a challenge to revise the entire rock classification system. Williams suggested a conference during the Easter vacation. Iddings could not attend but sent a list of ideas. As a result it was proposed that each write a proposal and exchange them among the group. Williams provided an outline of the field and listed the difficulties in classifying rocks. Cross described the weaknesses of existing classifications and suggested that the first criterion should be "chemical composition, as expressed in mineral composition, perhaps by molecular ratios. …" Pirsson reviewed the French system and urged "making the magma as the initial idea and running it out through various grades of structure, with subvarieties according to mineralogical variations." Iddings pushed for differentiation of rocks as petrographical entities and rock bodies as geological units and magmas "as solutions of chemical compounds capable of crystallization and differentiation," and he thought that differentiation led to natural rock families (= consanguineous groups). Further letter exchanges took place, but the project received a severe blow with the sudden death of Williams[14] from typhoid fever in 1894.

ROCK CLASSIFICATION

Iddings found his own classification difficult to defend before his students and shifted his views after a visit with W. C. Brøgger in Norway and discussions with other petrographers at the International Geological Congress in St. Petersburg in 1897. The shift was from the genetic relationships, advocated by Brøgger, to one in which chemical and mineral compositions were interdependent and fundamental. His ideas were recorded in 1898 in two papers in the *Journal of Geology*,[15] titled "On Rock Classification" and "Chemical and Mineral Relationships in Igneous Rocks." In the spring of 1899 a circular letter was received from the International Committee on Rock Nomenclature asking for opinions. Iddings responded, but Cross did not believe that widely divergent international discussion would influence the result. The circular letter, however, did rekindle the classification project initiated in 1893, and Cross, Iddings, Prison, and H. S. Washington,[16] who took Williams's place in the group, met in Washington, DC., during the meeting of the Geological Society of America in December 1899. They declared that a new system of classification was needed, one based on the quantitative proportions of minerals and chemical components. They recognized the need for a method to express the chemical composition of a rock in terms of minerals in a quantitative way.

Washington[17] had been assembling chemical analyses of igneous rocks and testing various schemes of classification. It was in December 1900 that Washington made the gener ous offer to publish his collection of chemical analyses classified in the proposed system, but the others "thought it too much of a sacrifice on Harry's [H.S.W.] part to share the authorship of his great work. …" After numerous conferences, much correspondence, and openly expressed mental stress, Iddings was commissioned to handle the quantitative form of the classification and Washington was to attend to the nomenclature because of his familiarity with the taxonomic methods in botany. They all appreciated that the new classification scheme was indeed different, logical, and of considerable importance. The authorship of the final manuscript was alphabetical—Cross, Iddings, Pirsson, Washington—despite a sincere protest from Cross. The quantitative system published in 1902 became known as the CIPW system, from the first letter of each of the authors' last names. The quantitative method of reducing a chemical analysis of a rock to a set of ideal end-member minerals (the norm), close to those observed in the rocks (the mode), has had a profound influence on both field and experimental petrology. On the other hand, the drastic, complex, and foreign nature of the nomenclature was never accepted by practicing petrographers. All of the authors of the system had contributed in a major way to the successful conclusion of the ingenious project, but Iddings's colleagues acknowledged his leadership and persistence.[18]

PROGRAM FOR EXPERIMENTAL PETROLOGY

The CIPW system displays a remarkable understanding of the physico-chemical relationships of most of the major igneous rock types in the early stages of the systematic investigation of silicate melts.[19], [20], [21], [22] The calculation of normative minerals, especially from analyses of fine-grained rocks and natural glasses, has been a cornerstone for choosing critical components in the phase equilibria studies of ex perimental petrologists. Its formulation appears to have had a major role in fostering and expediting the experimental approach to petrology. As a result of discussions among Charles D. Walcott, George F. Becker, and Charles R. Van Hise, Iddings was asked to "draw up a list of possible problems which might be studied in a chemico-physical laboratory." He presented a preliminary list in June 1903 to his CIPW colleagues as well as to F. D. Adams, James F. Kemp, John E. Wolff, and Alfred C. Lane. After obtaining their additions and suggestions, the revised list from the Committee of Eight was submitted to the trustees of the newly formed Carnegie Institution of Washington (*Year Book 2*, 1903, pp. 195-201). It served as guidance for selecting experimental programs for which funding was provided to Becker and Arthur L. Day at the U.S. Geological Survey and F. D. Adams at McGill University. As a result of their successes in dealing with geological problems experimentally, the trustees voted in December 1905 to establish a geophysical laboratory, and Day became its first director. The first paper published in 1905 by the laboratory was "The Isomorphism and Thermal Properties of the Feldspars." The thermal study was by A. L. Day and E. T. Allen, and the optical study was by J. P. Iddings. The thermal study was actually carried out at the U.S. Geological Survey and examination of the thin sections of the feldspar preparations was carried out by Iddings at Chicago. In recognition of the support of the Carnegie Institution, the USGS consented to have the work listed as paper no. 1 of the new laboratory. The program for experimental petrology outlined by the Committee of Eight became the initial scientific program of the Geophysical Laboratory[23] and continues "to play so important a role in the advancement of petrology." It appears that Iddings again was the leader of a productive group, not only providing direction to a highly successful enterprise but also contributing personally to its first scientific results.

TEXTBOOK PREPARATIONS

For an extended period, Iddings's colleagues encouraged him to produce a textbook on igneous rocks. Even his mentor, Rosenbusch, had suggested to him, after consenting to a translation of *Mikroskopische Physiographie*, to write "a more general book on the petrography of igneous rocks." With the problems of classification and nomenclature of igneous rocks in hand, despite criticisms from abroad, Iddings believed a student of petrology should first have a firm foundation in the minerals that compose the rocks. The result was a 617-page treatise called *Rock Minerals: Their Chemical and Physical Characters and Their Determination in Thin Sections* (1906).

In the same year his book was published his father and older brother, Charles Fry Iddings, died. To his grief was added the loss in the same year of Sam Penfield, his classmate and very close friend. Despite recent trips to Yellowstone Park, field trips through Skye, Scotland, with Alfred Harker, a scenic tour of France with C. W. Cross and Frank Adams, participation in the centenary celebration of the Geological Society of London, election to the National Academy of Sciences (1907), and an honorary Sc.D. from Yale (1907), the variety of events apparently did not cure Iddings's need for "rejuvenation." The long-contemplated book on the petrology of igneous rocks—proposed some twenty years before—was still foremost in his mind, and he began work while still absorbed in university teaching.

THE SUDDEN COLLAPSE

In the spring of 1908 Iddings suddenly departed from the University of Chicago. The nature of his departure was recorded by two students in his class at that time. In a letter from Albert D. Brokaw to D. Jerome Fisher,[24] Iddings was alleged to have received word that an aunt had died in Maryland and an inheritance was involved. The present descendants in the Iddings's family do not have record of an aunt dying at that time. Arthur C. Trowbridge gave a lecture to the Geology Club of the State University of Iowa on February 28, 1968, that was recorded and transcribed.[25] After describing Iddings as a gentleman and scholar, Trowbridge recalled the day Iddings failed to meet his class. After waiting the prescribed ten minutes for a professor, a student committee went to his apartment near the university to inquire. Salisbury lived in the same building and said he had breakfast with Iddings and that "he seemed to be all right then." The students were later told that he had inherited a fortune in England and left to settle the estate. The present Iddings family knows of no relatives in England at the time and has no knowledge of an inheritance.[26] His departure has also been described as merely retirement.

The events are perhaps best revealed by Iddings himself in a letter dated May 19, 1908, to his close friend, Whitman Cross, written at the home of his younger sister, Lola LaMotte Iddings, in Winchester, Massachusetts:

> I am here as the result of a rather sudden collapse and am taking rest and fresh air treatment. Owing to contributing causes which you will please keep strictly to yourself for the present, is a determination to cut loose from my colleagues at the University. Whether this is strictly a cause or a result, may be a psychological question. They are pretty well mixed up. The situation is not clearly understood out there, and you can see how it is better to keep strictly mum on the subject until I can find out whether I can get a foothold somewhere else.

There is no letter of resignation or request for leave on record at the University of Chicago, but there is reference by T. C. Chamberlin in a letter dated June 23, 1908, to a note Iddings wrote on June 8, 1908, requesting a leave of absence, which was granted. Iddings never returned to the University of Chicago.

"FREE TO WANDER"

Iddings spent the summer of 1908 visiting Frank Adams in Montreal[27] and camping with Whitman Cross in the mountains north of Durango, Colorado. The camp life and hunt for butterflies brought about the desired renewal. He set to work again on the book on igneous rocks at the family estate "Riverside"[28] in Brinklow, Maryland, just fifteen miles from Washington, D.C. The book was completed in the spring of 1909 and published that year. It applied the "modern conception of physical chemistry to the elucidation of the phenomena of crystallization, and of genetic relationships among igneous rocks." It was indeed a new treatment of the subject, emphasizing differentiation, chemical reactions leading to hybrid rocks, assimilation, sequencing of magmas, and eruption processes. These aspects dominated over his favorite topics of crystallization and texture, modes of occurrence, classification and nomenclature, and diagrams for plotting the analyses of igneous rock types. A congratulatory letter was received from A. Harker, who dispatched him a copy of his own *Natural History of Igneous Rocks* , also published in 1909. Other flattering letters were later received from Judd, Geikie, Zirkel, Barrois, Lawson, and others.

With completion of volume 1 of the book on igneous rocks, Iddings felt "free to wander." With funds obtained from serving on a legal case related to calcium carbide, a subsidy from the Smithsonian Institution arranged by Walcott (then its secretary) to collect Cambrian fossils in Manchuria, and a sum from his friend Charles M. Pratt for rock collections to be made for two colleges, plus his own re sources, Iddings set out for Japan, the Philippines, China, and other countries on a round-the-world tour, passing through Suez and the European continent.

Following his worldwide observations on volcanoes, extensive collections, conversations with other petrographers, and enforced periods of contemplation, which accompanies long-distance travel, Iddings was prepared to put his views on paper. He wanted to collate the interrelationships of the chemical, mineralogical, textural, and occurrences of rocks. Fortunately, he did not use the new nomenclature of 1902 but the names previously employed for igneous rocks "both in order to be understood by petrographers already familiar with them and also to make it possible for students to understand the literature of the subject." He recognized the continuous-series aspect of rocks but proceeded to partition the series into quantitatively definite parts. His divisions were fivefold: rocks were characterized by (1) quartz and feldspar, (2) feldspars with little or no quartz, (3) feldspars and feldspathoids, (4) feldspathoids, and (5) chiefly mafic minerals. For the most part he depended on the chemical composition, the mode or the norm when aphanitic, and "occult minerals" dependent on lesser constituents or in glass but nevertheless represented in the norm. Almost half the text pages of the monumental work are devoted to occurrences of igneous rocks in the world. During the writing of the book, Iddings attended from 1910 to 1914 the Petrologist's Club, which initially held its meetings in the home of Whitman Cross. Papers were presented on four different occasions, and his discussions at ten meetings were recorded by the secretary. He also enjoyed conversations with various scholars at the Cosmos Club, to which he had been elected in 1885. The quiet country atmosphere of the Riverside estate was greatly conductive to writing, but consultations with his friends and use of the library at the

U.S. Geological Survey were essential for such an encompassing compilation. The book was completed in April 1913 and published that year.

Early in 1911 Iddings's good friend, Louis Pirsson, invited him to give the Silliman Lecture Series at Yale University on the problems of volcanism. Eight one-hour lectures were to be prepared, one of which Pirsson hoped would be on Reginald A. Daly's stoping hypothesis, which was not acceptable to either of them. Preparation of the lectures was undertaken as soon as the second volume of *Igneous Rocks* was completed. The date was set for the spring of 1914. Iddings took the opportunity to range widely from discussions of T. C. Chamberlin's nebular hypothesis to the physical characteristics and dynamic status of the earth and ended up with three lectures on the mechanics of the intrusion and eruption processes. The subject of overhead stoping received only two paragraphs of discussion, but Iddings saved his ammunition for a severe criticism of Daly's book, *Igneous Rocks and Their Origin*, in a separate salvo later in 1914. The lectures were given as scheduled, and the manuscript for book publication, as "The Problem of Volcanism," was turned over to Yale University Press the day after the last lecture!

SECOND CIRCLING OF THE GLOBE

The wanderlust hit again and Iddings circled the globe for a second time, this time from east to west, financed in part again by his good friend, Charles M. Pratt. He was booked in June 1914 for a short course of lectures at University College, London, repeating some of his Silliman lectures but focusing on the normative calculations of the CIPW system, petrographical provinces in North America, and the philosophy of physico-chemical petrology. From England he visited his old friends in Norway, France, and Italy. He stopped in Java and took time to write a piercing analysis of Daly's new book, already mentioned, criticizing his "remarkable distortion of petrographic relationships," grotesque conclusions, and "indifference to rational geodynamics," but admitting to his "tireless energy, vigorous methods of attack, and honesty of his convictions."

Month-long collecting tours of both rocks and butterflies were undertaken in Borneo,[29] the ancient volcanic island of Bawéan with its rocks rich in leucite and nepheline, and the potassic lavas of western Celebes. In the Celebes Iddings learned of the onset of World War I, to his great distress in light of his Quaker ancestry, descendence from English[30] and French ancestors, and many friends throughout Europe. Following stops in Java and Australia, he settled down for three months in New Zealand before journeying on to his prime target, Tahiti, where he arrived on April 8, 1915. His rock collecting was confined to the fresh-stream boulders in the many deeply eroded gulches, the interior of Tahiti being mostly wild and inaccessible. After six months in Tahiti and the Leeward Islands, Iddings visited the Marquesas with a full month on Hiva-Oa, which he managed to explore on horseback. Bouts of influenza reduced his energy, and he wrote, "I have reached an age [58] where the comforts of civilization are a desideratum." He landed in San Francisco in the fall of 1915, bringing eleven substantial boxes of rocks.

On return from his second world tour, Iddings and his sister Lola leased the Grove Hill Farm (established ca. 1796) near the family estate in Brinklow, Maryland, in October 1915. In the peaceful surroundings of the farm, Iddings was able to write, withdrawing almost completely from societal affairs. He is listed, however, as being vice-president of the Geological Society of America in 1916. From a petrographic study of his collections, seven papers were produced from 1915 to 1918 describing the rock types of those relatively little known regions of the world that he had explored. The papers contain only brief descriptions of the rocks but provide ninety new chemical analyses by E. W. Morley, H. W. Foote, and H. S. Washington, which were funded by the National Academy of Sciences. With the exception of his revision of the first volume of *Igneous Rocks* (1920), Iddings became withdrawn upon the death of his sister Lola, a poet, on April 3, 1918, from pneumonia. Iddings, himself an occasional poet (some of his poems are recorded in his autobiography), assembled Lola's poems, added an introduction, and had them published as a book[31] after her death. He never married.

Iddings died at the Montgomery County, Maryland, Hospital in Olney on September 8, 1920, from chronic intersilial nephritis according to the official death certificate. His youngest sister, Estelle Iddings Cleveland, was the sole beneficiary of his estate (see section on Honors). His rock collections were given to the Smithsonian Institution; most are identified as the Iddings East Indian Collection, and some are under the Petrographic Reference Collection. His extensive butterfly collection and library also were turned over to the Smithsonian. Burial was in the Woodside Cemetery adjoining the family estate in Brinklow, Maryland, alongside his parents and sister. As is fitting for a petrologist, his tombstone is a large boulder. The plaque is enscribed with "Blessed are the dead which die in the Lord" (Rev. xiv-xiii), but his middle name is misspelled even though his namesake is buried nearby.

HONORS

Among the honors already mentioned, Iddings was elected a foreign member of the Scientific Society of Christiania in 1902 and the Geological Society of London in 1904, an honorary member of the Societé francaise de Mineralogie in 1914, and the American Philosophical Society in 1911, and was a fellow of the Geological Society of America. In 1914 he was made an honorary curator of petrology in the U.S. National Museum. In addition to the mineral "iddingsite" named by Lawson, he was honored by the naming of an early Cambrian trilobite, "*Olenellus iddingsi* Walcott" (1884), [32] which was later recognized as a new genus and called "*Peachella iddingsi* Walcott" (1910).[33] Walcott also named a brachiopod *Orthis (Plectorthis) iddingsi*. The trilobite genus *Iddingsia* was established by Walcott in 1924 in memory of his field associate.[34] The Iddings Scholarship for Graduate Studies was set up at the Sheffield Scientific School at Yale by his sister, Estelle Iddings Cleveland, with the residua of his estate and supplementary funds. One of its famous recipients was Aaron C. Waters. The fund was later transferred to general departmental use and continues to support students and research in petrology.

SCHOLAR AND GENTLEMAN

Iddings is described as a reserved gentleman of broad culture who made lasting friendships wherever he went in the world. He visited and corresponded with most of the leaders in petrology. He held to his views with tenacity and was not reluctant to promote them. He dealt with the initial severe criticism of the CIPW system by presenting the arguments in greater detail but devoid of humor.[35]

Despite his extensive worldly travels Iddings was not an outgoing conversationalist like his friend Harry Washington. Nevertheless, there was a personal charm that attracted friends. Even his close friend Pirsson recommended that Iddings read his Silliman lectures rather than attempt to give them extemporaneously. His love of the rugged western U.S. scenery and camplife was in sharp contrast to his poetic and romantic view of his surroundings and attention especially to the dramatic display of colors of a sunset, a rock, or a butterfly. The allure of the South Sea Islands, a long coveted dream did not result in the customary abandonment of civilization suffered by so many visitors. In the end it only strengthened his appreciation of the comforts and intellectual stimulation of his own culture. Iddings remained a conscientious and devoted worker to petrology throughout his travels.

After seventy-five years it is difficult to understand why his contributions have not received the attention they deserve. Iddings himself did not believe he was endowed with originality but did recognize his ability to analyze and synthesize observational facts. As one of the pioneers in introducing petrography to the United States,[36] he must be given a large measure of credit for developing that field into petrology. He was a promoter of King's[37] one-magma hypothesis, an early advocate of magma differentiation, and a supporter of the basic-to-acid sequencing of magmas. He recognized the significance of Judd's[38] concept of petrographic provinces and was the first to recognize that igneous rocks of the same bulk composition produced different assemblages under different conditions of crystallization. He was quick to adapt Reyer's[39] use of diagrams representing rock composition to explain rock relations. As a result of his strong support of Bunsen's concept of magma as a solution, Iddings helped bring about the transition from descriptive petrography to a physico-chemical view of igneous rock interrelationships. In his quiet way he exercised leadership in the construction of the CIPW system and in formulating the experimental program of the Geophysical Laboratory. He was among the first to recognize the role of volatiles in volcanic eruption and to show concern for the physics of the eruption process. All in all, one can easily agree with his peer group of 1903 that Iddings was one of the giants in petrology at the turn of the twentieth century.

THE PRIMARY SOURCES OF information for this memoir were the published works of Iddings, all of which have been assembled at the Geophysical Laboratory. A draft of recollections compiled by Iddings from letters written to his parents and family from Switzerland and Heidelberg in 1879-80 and his autobiography, edited and amended by C. Whitman Cross, are available in the Field Records Library of the U.S. Geological Survey in Denver. An inventory and finding guide of other items in Denver has been prepared by Carol A. Edwards. Letters written during Iddings's travels to the South Pacific during 1914-15 and correspondence with Charles D. Walcott are in the archives of the Smithsonian Institution. Correspondence with Arthur L. Day, director of the Geophysical Laboratory from 1907 to 1920, is in the archives of the laboratory. Letters to T. C. Chamberlin are in the archives of the Department of Geophysical Sciences, University of Chicago. Iddings's activities during his college days are described in the *Class of 1877 Sheffield Scientific School 1877-1921* and the *Obituary Record*, available in the Manuscript and Archive Division of the Yale University Library. His days at the University of Chicago have been described by Fisher.[23]

Through the kindness of Mrs. Sylvia Nash of the Sandy Spring Museum (Olney, Md.), copies of the pages from Thomas and Kirk's *Annals of Sandy Spring: History of a Rural Community in Maryland* (vol. 4, 1929, Times Printing Co., Westminster, Md.) relevant to the Iddings family from 1914 to 1920 were made available. Mrs. Elizabeth Iddings Small Hartge, current owner and resident of "Riverside" and member of the Woodside Cemetery Association, provided information from the records available and introductions to living Iddings family relatives.

A detailed biography and an almost complete bibliography of J. P. Iddings were written by E. B. Mathews ("Memorial of Joseph Iddings," *Geol. Soc. Am. Bull.* 44(1933):352-74). Brief biographies are also given by G. P. Merrill, "Obituary," *Am. J. Sci.* 50(1920):316; L. J. Spencer, "Biographical Notices," *Min. Mag.* 29(1921):247-48; J. J. H. Teall, "Joseph Paxson Iddings," in R. D. Oldham, "The Anniversary Address of the President," *Proc. Geol. Soc. London* 77(1921):lxi-lxiii; and W. C. Brøgger, "Mindetale over Prof. Dr. Joseph Paxon (sic) Iddings," *Furhandl. Videns-selsk. Kristiania*, 1921:45-50 (in Norwegian).

Portions of the Brøgger memorial were translated by Bjørn Mysen.

The diaries of his paternal grandfather and grandmother are at Duke University, and a finding aid is available. A family photo album and Iddings's photographs of Yellowstone National Park are at the University of Wyoming, Laramie. Iddings's family notes from the Steinmetz and Gearhart collections were consulted at the Genealogical Society of Pennsylvania in Philadelphia.

Finally, it is a pleasure to thank the Geophysical Laboratory's librarian, Shaun Hardy, and his assistant, Merri Wolf, for their energetic and enthusiastic help in the investigation of a very cold trail. The reviews of R. M. Hazen, C. M. Nelson, E. L. Yochelson, and S. Hardy were greatly appreciated.

NOTES

1. H. S. Yoder, Jr. Timetable of petrology. *J. Geol. Ed.* 41(1993):447-89.
2. The USGS Appointments Ledger records the fact that Iddings joined the USGS on July 1, 1880, from New Jersey's 6th Congressional District as an assistant geologist (temporary) for work in New York and the field. He was promoted after several salary increases to Geologist on August 10, 1888, and transferred by J. W. Powell to the permanent rolls on January 21, 1890. As a result of the general reduction in force, Iddings resigned on December 31, 1892. He was reappointed by J. D. Walcott on a per diem basis on January 17, 1895.
3. Iddings is also credited with the introduction to the petrological literature of the terms bysmalith, chadacryst, consanguinity, laminated texture, lithophysae, occult mineral, oikocryst, soda-orthoclase, and spherulite. Attributed to him are the following rock names: banakite, hawaiite, kanaiite, kohalaite, langenite, llanite, marosite, shoshonite, and tautirite (A. Johannsen, *A Descriptive Petrography of the Igneous Rocks*, vol. I, Chicago: University of Chicago Press, 1939).
4. A. Michel-Lévy. *Étude sur la détermination des Feldspaths dans les plaques minces*. Paris: Librarie Polytechnique, 1904, 16 pp.
5. T. S. Hunt. Illustrations of chemical homology. *Am. Assoc. Adv. Sci. Proc.* (1854):237-47.

6. G. Tschermak. Chemisch-mineralogische Studie-I: Die Fedlspatgruppe. *Sitzberichte Akad. Wissenschafter Wien* 50(1864):566-613.
7. A. C. Lawson. The geology of Carmelo Bay. *Bull. Dept. Geol. Univ. California* 1 (1893):31-36.
8. The alteration was first thought to be a single mineral but is now considered an intergrowth of two or more phases resulting from a continuous transformation of an original olivine crystal, presumably during the deuteric stage of consolidation of a magma. See, for example, P. Gay and R. W. LeMaitre, Some observations on iddingsite, *Am. Miner* 46(1961):92-111.
9. Iddings specifically stated that the chemical composition of a rock was not indicative of its age in "With notes on the petrographic character of the lavas" in C. D. Walcott, *Pre-Cambrian Igneous Rocks of the Unker Terrane, Grand Canyon of the Colorado, Arizona, U.S. Geological Survey Annual Report* 14, Part II(1894):520-24.
10. H. S. Washington. The magmatic alteration of hornblende and biotite. *J. Geol.* 4 (1896):257-82.
11. Iddings's Survey Division was moved from New York to Washington in 1885. In the Washington directories Iddings is listed as living at the following addresses: 1886-87, 1528 I St., N.W.; 1888-89, 1330 F St., N.W; 1890-91, 1028 Vermont Ave., N.W.; and 1892-93, 730 17th St., N.W.
12. Bowen, N. L. *The Evolution of the Igneous Rocks.* Princeton: Princeton University Press, 1928. 334 pp.
13. M. C. Rabbitt. *Minerals, Lands, and Geology for the Common Defence and General Welfare, 1879-1904*. Washington, D.C.: U. S. Government Printing Office, 1980.
14. According to F. J. Pettijohn (pp. 30-31, *A Century of Geology, 1885-1985, at the Johns Hopkins University*, Baltimore: Gateway Press, 1988), Iddings was considered as a replacement for Williams by W. B. Clark, head of the Geology Department at The Johns Hopkins University. The offer was made in 1894 but declined by Iddings. In 1913 Iddings did present five lectures at Hopkins as part of the guest lecture program.
15. The *Journal of Geology* was established at the University of Chicago in 1893 by T. C. Chamberlin. Iddings served on the editorial board from 1893 to 1909.
16. Washington had first introduced himself by letter to Iddings

in 1894. He was the cousin of Iddings's cousin, Elmsie Gillet. Washington had studied petrography under Zirkel at Leipzig in 1891-92 and made chemical analyses of rocks under Pirsson at Yale in 1895. He was independently wealthy at the time and had a complete laboratory for the analysis of rocks in his boyhood home. In 1898 Washington published a paper on the alkaline rocks of Essex, Co., Mass., in which he urged a systematization of nomenclature and classification (H. S. Washington, Sölvsbergite and tinguaite from Essex Co., Mass., *Am. J. Sci. Ser.* 4,6(1898):176-87). His training and interests were eminently compatible with the other members of the group.

17. H. S. Washington. Chemical analyses of igneous rocks published from 1884 to 1900, with a critical discussion of the characters and use of analyses. *U.S. Geological Survey Professional Paper 14*, 1903.

18. In 1903 a group of peers listed the 100 leading men of science in the United States in geology and arranged them in order of distinction. The CIPW group were included: no. 14, Joseph Paxson Iddings (1857-1920); no. 32, (Charles) Whitman Cross (1854-1949); no. 49, Henry S. Washington (1867-1934); and no. 55, Louis Valentine Pirsson (1860-1919). The results were not published in *American Men of Science* until 1933 (pp. 1274-75). Only Cross and Washington lived to learn the results.

19. C. Doelter. Synthetische Studien. *Neues. Jahrb. Min.* 1(1886):119-35.

20. F. Fouqué and A. Michel-Lévy. *Synthese des minéraux et des roches* . Paris: Masson, 1882.

21. J. Morozewicz. Experimental Untersuchungen über die Bildung der Minerale in Magma. *Tschermak's Min. petr. Mitth.* 18(1899):1-90.

22. J. H. L. Vogt. Die Silikatschmelzlösungen: I. Über die Mineralbildung in Silikatschmelzlösungen. *Norsk Videnskaps-Akad. Mat. Natur. Klasse* 8(1903):1-236.

23. H. S. Yoder, Jr. Development and promotion of the initial scientific program for the Geophysical Laboratory. In *The Earth, the Heavens and the Carnegie Institution of Washington*, vol. 5, pp. 21-28. Washington, D.C.: American Geophysical Union, 1994.

24. D. J. Fisher. *The Seventy Years of the Department of Geology, University of Chicago, 1892-1961*. Chicago: University of Chicago Press, 1963.

25. The tape was originally provided through the courtesy of Richard A. Davis, transcribed by J. V. Cole, and edited by B. F. Glenister

in March 1976. A copy of the transcription is on file at the University of Chicago.

26. The possibility was investigated that an inheritance may have been forthcoming from his father's estate that presumably would have been settled by that time. He died in Orange, N.J., on June 20, 1906, according to the official death certificate. Unfortunately, there is no record of William Penn Iddings's will or letters of administration in Essex County, N.J. He is buried, however, in Woodside Cemetery adjoining the Riverside Estate in Brinklow, Md., but there were no details recorded by the cemetery association of his death.

27. It was presumed by others that Iddings had taken a position at McGill University, but a search by the university's archivist revealed no record of his being on the staff or cited in the newsletter, newspaper clippings, or calendars for that period.

28. The estate was along the Patuxent River in the eastern portion of Montgomery County. It is described by R. B. Farquahar (*Old Homes and History of Montgomery County, Maryland*, pp. 257-59, Silver Spring, Md., 1962) along with other historic homes in the county. The estate is shown on the 1865 homeowner's map of the county by Martenet and Bond under the name of Charles A. Iddings (1831-98), the youngest son of Caleb Pierce Iddings (1778-1863).

29. At this point, Iddings appears to have abandoned his customary daily record of events. His friend Whitman Cross reconstructed the remainder of his tour from Iddings's detailed letters to his family and friends.

30. According to J. J. H. Teall, Iddings hoisted "The Union Jack alongside the Stars and Stripes at his country house on 'British Day' during World War I, when he returned to the United States.

31. L. L. Iddings. *Poems*. New Haven: Yale University Press, 1920.

32. C. D. Walcott. *Olenellus iddingsi* Walcott. *U.S. Geol. Surv. Mongr.* 8(1884):28.

33. C. D. Walcott. *Peachella iddingsi* Walcott. *Smithson. Miscl. Coll.* 53(1910):343-45.

34. C. D. Walcott. Cambrian geology and paleontology V. No. 2. Cambrian and lower Ozarkian trilobites. *Smithson. Miscl. Coll.* 75(1924):1-60.

35. One brief, subtle, humorous comment on the CIPW system is given by A. Johannsen (*A Descriptive Petrography of the Igneous Rocks*, vol. I. Chicago: University of Chicago Press, 1939) who gave in the

chapter heading two bars of music from an old (about 1828) German folksong, "Du, du liegst mir in Herzen" ("You, you lie in my heart"). The fourth line of the stanza was omitted, which in one version runs, "Weiss nicht wie gut ich dir bin" ("You know not how good I am to you"). It reflects Johannsen's disappointment with the reviews of his own monumental work on petrography. It was Johannsen who replaced Iddings as professor of petrology at the University of Chicago.

36. C-H. Geschwind. The beginnings of microscope petrography in the United States, 1870-1885. *Earth Sci. Hist.* 13 (1994):35-46.

37. C. King. *Systematic Geology*. Washington, D.C.: U.S. Government Printing Office, 1878.

38. J. W. Judd. On the gabbros, dolerites and basalt of Tertiary age in Scotland and Ireland. *Q. J. Geol. Soc. Lond.* 42(1886):49-97.

39. E. Reyer. *Beiträge zur Fysik der Eruptionen und der Eruptiv-gesteine.* Wien: A. Hölder, 1877.

Selected Bibliography

1883 With A. Hague. Notes on the volcanoes of northern California, Oregon and Washington Territory. *Am. J. Sci.* Ser. 3, 26:222-35.

1884 With A. Hague. Notes on the volcanic rocks of the Great Basin. *Am. J. Sci.* Ser. 3, 27:453-63.

1885 With A. Hague. On the development of crystallization in the igneous rocks of Washoe, Nevada, with notes on the geology of the district. *U.S. Geol. Surv. Bull.* 17:1-44.

1887 The nature and origin of lithophysae and the lamination of acid lavas. *Am. J. Sci.* Ser. 3, 33:36-45.

1888 Obsidian Cliff, Yellowstone National Park. *U.S. Geol. Surv. Ann. Rep.* 7:249-95.

With H. Rosenbusch. *Microscopical Physiography of the Rock-Making Minerals: An Aid to the Microscopical Study of Rocks*. Translated and abridged by J. P. Iddings. New York: Wiley & Sons.

1889 On crystallization of igneous rocks. *Philos. Soc. Washington Bull*. 11:65-113.

1891 The eruptive rocks of Electric Peak and Sepulchre Mountain, Yellowstone National Park. *U.S. Geol. Surv. Ann. Rep.* 12:569-664.

Spherulitic crystallization. *Philos. Soc. Washington Bull*. 11:445-64.

1892 With A. Hague. Appendix B: Microscopical petrography of the eruptive rocks of the Eureka District, Nevada, pp. 335-96. In *Geology of the Eureka District, Nevada*. U.S. Geological Survey Monograph 20.

The origin of igneous rocks. *Philos. Soc. Washington Bull*. 12:89-216.

1898 Chemical and mineral relationships in igneous rocks. *J. Geol.* 6:219-37.

1899 With A. Hague et al. *The Geology of the Yellowstone National Park*, part II. U.S. Geological Survey Monograph 32, 849 pp.

1902 With C. W. Cross et al. A quantitative chemico-mineralogical classification and nomenclature of igneous rocks. *J. Geol.* 10:555-690.

1903 Chemical composition of igneous rocks expressed by means of diagrams, with reference to rock classification on a quantitative chemico-mineralogical basis. *U.S. Geol. Surv. Prof. Pap.* 18:1-98.

1905 With A. L. Day and E. T. Allen. The isomorphism and thermal properties of the feldspars. Part II. Optical study. *Carnegie Inst. Washington Publ.* 31:77-95.

1906 *Rock Minerals, Their Chemical and Physical Characters and Their Determination in Thin Sections* . New York: Wiley & Sons.

With C. W. Cross. The texture of igneous rocks. *J. Geol.* 14:692-707.

1909 *Igneous Rocks: Composition, Texture and Classification, Description, and Occurrence*, vol. I. New York: Wiley & Sons, 464 pp.

1911 Problems in petrology. *Am. Philos. Soc. Proc.* 50:286-300.

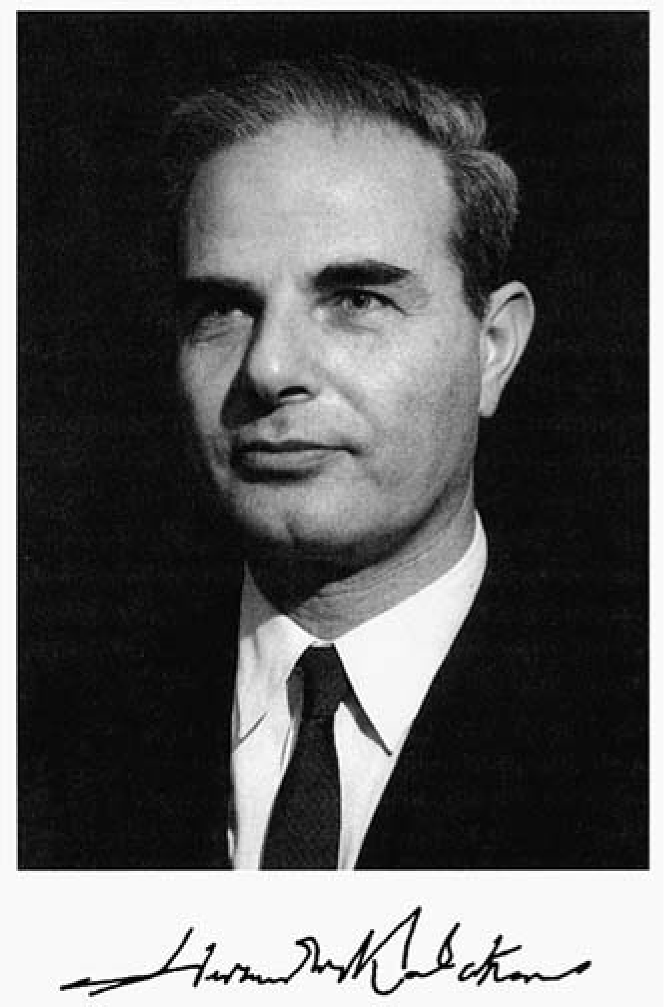
1913 *Igneous Rocks: Composition, Texture and Classification, Description, and Occurrence*, vol. II. New York: Wiley & Sons, 685 pp.

1914 *The Problem of Volcanism*. New Haven: Yale University Press, 273 pp.

1915 With E. W. Morley. Contributions to the petrography of Java and Celebes. *J. Geol.* 23:231-45.

1916 With E. W. Morley. The petrology of some South Pacific Islands and its significance. *Proc. Natl. Acad. Sci. U.S.A.* 2:413-19.

1918 With E. W. Morley. A contribution to the petrography of the South Sea Islands. *Proc. Natl. Acad. Sci. U.S.A.* 4:110-17.



Herman Moritz Kalckar

March 26, 1908–May 17, 1991

EUGENE P. KENNEDY

HERMAN MORITZ KALCKAR died in Cambridge, Massachusetts, on May 17, 1991, at the age of eighty-three. His scientific career spanned much of the period of development of modern biochemistry, to which he made contributions of central importance.

Kalckar was born in Copenhagen on March 26, 1908, into a family he described in an autobiographical essay as having been middle-class Jewish-Danish for many generations. Kalckar's broad interests in literature and the arts had their origins in his early family life. His mother, Bertha Rosalie Melchior Kalckar, read widely in French and German as well as Danish literature. His father, Ludvig Kalckar, a businessman, was devoted to music and the theater. Ludvig Kalckar attended the world premiere of Ibsen's "A Doll's House" at the royal Theater in Copenhagen in November 1879 and later wrote an enthusiastic review of it. Herman Kalckar traced some of his own enthusiasm for music, and in particular for Mozart, to his father's example.

Herman's younger brother, Fritz Kalckar, was a gifted physicist and a colleague and protégé of Niels Bohr. The death of Fritz in 1938 at twenty-eight years of age was a devastating blow for the entire family.

Kalckar received his early schooling in the Ostre Borgerdyd Skole, located within an easy walk of his home in Copenhagen. The headmaster, J. L. Heiberg, was a Greek scholar of international repute, and Kalckar paid tribute to the "Athenian flavor" of the school. He felt a special gratitude to the physics teacher, H. C. Christiansen, whom he recalled many years later as a formidable and passionately devoted teacher.

Kalckar completed his studies for a degree in medicine at the University of Copenhagen in 1933 and then began his scientific career in 1934 as a candidate for the Ph.D. degree in the Department of Physiology under the direction of Ejnar Lundsgaard. Lundsgaard had earlier made the important finding that frog muscles poisoned with iodoacetate and therefore unable to carry out glycolysis (the splitting of glucose to lactic acid) are nevertheless capable of carrying out a limited number of contractions. Lundsgaard later showed that these "nonlactic" contractions were at the expense of the dephosphorylation of creatine phosphate, which had been discovered and characterized only a few years earlier by Cyrus Fiske at the Harvard Medical School.

In 1932 Fritz Lipmann, unable to work in Nazi Germany, moved to Copenhagen, where he was closely associated with Lundsgaard. Lipmann became one of Kalckar's mentors and a close friend, a relationship that was to be lifelong. Lipmann was already deeply interested not only in Lundsgaard's work on the role of phosphate esters in muscle contraction but also in the biological functions of phosphorylation reactions more generally. In his masterly and highly influential review in 1941 Lipmann was to emphasize the central role of adenosine triphosphate (ATP) as an "energy-rich" phosphate ester, the breakdown of which to adenosine diphosphate (ADP) and inorganic phosphate (Pi) drives not only muscle contraction but also a host of other energy-requiring processes. Because the cell has a limited supply of ATP, the ADP formed by its breakdown must be continuously rephosphorylated to ATP. In muscle this may be done by use of creatine phosphate, a cellular reserve of "energy-rich phosphate" that is present, however, only in limited amounts. At that time the only primary source of energy known to biochemists for the rephosphorylation of ADP to ATP was the splitting of glucose to lactic acid in muscle or in yeast to alcohol and carbon dioxide. The brilliant achievements of Otto Warburg (which both Kalckar and Lipmann greatly admired) had revealed the reactions by which ADP is phosphorylated to ATP during glycolysis.

Glycolysis, however, is a process that can occur anaerobically in the absence of molecular oxygen. Classic investigations by Pasteur had made it clear that aerobic metabolism of glucose by yeast is vastly more energy efficient than the anaerobic process. How is energy captured by the oxidation of sugars and other foodstuffs linked to the reduction of molecular oxygen? This was the question confronted by Kalckar as he began the investigations in the period 1937-39 that led him to the demonstration that cell-free extracts of kidney cortex catalyze oxidative phosphorylation—that is, the formation of ATP in reactions strictly dependent on the reduction of oxygen and independent of glycolysis. An important technical point in these experiments was the use of sodium fluoride to inhibit interfering phosphatases that otherwise would break down ATP and other phosphate esters almost as soon as they were formed.

As is now well known, aerobic nonphotosynthetic organisms, including ourselves, derive vastly more metabolic energy from oxidative phosphorylation than from any other source. It is estimated that the complete utilization of glucose in muscle leads to the production of about seventeen times more ATP via oxidative phosphorylation than is produced in anaerobic glycolysis. Oxidative phosphorylation is therefore a process of central bioenergetic importance. It became (and still is) the object of intensive studies in many laboratories throughout the world. Localized in the inner membrane of the mitochondrion, oxidative phosphorylation proved extraordinarily resistant to biochemical dissection. Two decades were to pass before real insight was gained into the mechanism of oxidative phosphorylation with the development by Peter Mitchell of the chemiosmotic theory.

To this date many important features of oxidative phosphorylation remain imperfectly understood, but Kalckar's work opened the way to its systematic exploration.

At about the same time as Kalckar's experiments V. A. Belitzer, working in virtual isolation in the Soviet Union, made similar observations on oxidative phosphorylation in experiments on preparations derived from pigeon breast muscle. Although Belitzer's work did not become known in western Europe until a considerable time after Kalckar's first publications, it was characteristic of Kalckar that he was always generous in acknowledging Belitzer's contribution. During his trip to the Soviet Union in 1960 Kalckar looked up Belitzer in Kiev and took some pains to make arrangements to be photographed with him.

Kalckar's early experiments on oxidative phosphorylation also provided evidence for the production of phosphoenolpyruvate from fumaric or malic acids, observations that later provided an important clue to the mechanisms involved in the formation of glucose from noncarbohydrate sources in animal tissues.

Kalckar later wrote an interesting historical account of the origins of the concept of oxidative phosphorylation and his early experimental work on this theme (1974).

In 1939, having completed his work for the Ph.D. degree, Kalckar was appointed a Rockefeller research fellow for a year of postdoctoral study at the California Institute of Tech nology. On his trip across the United States he stopped at the famous laboratory of Gerty and Carl Cori at Washington University in St. Louis, then one of the few centers of the "new biochemistry" in the United States. There he found Sidney Colowick, then a graduate student, attempting to duplicate Kalckar's experiments on oxidative phosphorylation, without success. Colowick, untrained in the methods introduced by Warburg and followed by Kalckar, had been simply incubating tissue extracts in test tubes without providing for the efficient diffusion of oxygen into them. Thirty-five years later Colowick summarized Kalckar's helpful advice: "Shake it!' said Dr. K., and everything was OK!"

Kalckar's stay in California allowed him to take the famous microbiology course in Pacific Grove taught by C. B. van Niel, whose insight into the underlying unity of the biochemistry of living organisms and charismatic personality made a deep impression on Kalckar, as on so many others. This experience may have planted a seed that led later to Kalckar's interest in microbial molecular biology.

During his stay in Pasadena, with the encouragement of Linus Pauling, Kalckar undertook the preparation of a comprehensive review of bioenergetics with emphasis on the role of phosphate esters in energy transduction. Its publication (1941) did much to advance the new ideas that he and Lipmann were pursuing.

In 1940 Kalckar accepted an appointment as research fellow in Cori's Department of Pharmacology at Washington University. The invasion of Denmark by the Nazis in the spring of 1940 had made it impossible for Kalckar and his wife, Vibeke, to return to Copenhagen, and Kalckar was fortunate to have the opportunity to spend the next three years in a stimulating and productive environment with congenial colleagues. He joined forces with Sidney Colowick. Their work led them to the discovery in muscle extracts of a remarkable enzyme, named myokinase by them, but now more precisely called adenylate kinase, that catalyzes the following readily reversible reaction:

$$\text{ATP} + \text{AMP} \longleftrightarrow 2\ \text{ADP} \qquad (1)$$

Many biological processes lead to the production of adenosine monophosphate (AMP), which, however, cannot be phosphorylated to ATP during oxidative phosphorylation or glycolysis, processes that are specific for ADP as phosphate acceptor. In the absence of reaction (1), which "rescues" AMP by converting it to ADP, all of the adenine nucleotides of the cell would be irreversibly converted to AMP. Later experiments by other workers showed that mutations that block the activity of adenylate kinase are lethal to cells of *Escherichia coli*.

In 1943 Kalckar was appointed research associate at the Public Health Institute of the city of New York. One of the attractions of the post was a laboratory equipped with a new ultraviolet spectrophotometer, then still a rather rare instrument. Kalckar developed spectrophotometric methods for the study of the metabolism of nucleosides and nucleotides, the building blocks of RNA and DNA. It had earlier been reported by Klein that the enzymic hydrolysis of nucleosides is stimulated by the addition of phosphate or arsenate. Coming from the Cori laboratory, the center of work on glycogen phosphorylase, Kalckar realized the possible significance of the role of phosphate and soon made the important discovery that the phosphorolytic cleavage of nucleosides is similar to that of glycogen.

$$\begin{aligned}&\text{ribose-hypoxanthine} + \text{Pi} \longleftrightarrow \\ &\quad \text{ribose-1-P} + \text{hypoxanthine} \qquad (2)\end{aligned}$$

The reaction is readily reversible. Indeed, the equilibrium position lies to the left of Equation (2) as written, favoring the synthesis of the nucleoside rather than its breakdown. As the first demonstration of the enzymic synthesis of a nucleoside, this work attracted considerable attention. It must be recalled that in 1945, when this work was done, very little was known of the metabolism of nucleic acids or indeed of their functions in living cells. Later work was to show that nucleoside phosphorylases function as "salvage enzymes" in the degradation rather than the synthesis of building blocks of nucleic acid.

In 1946 Kalckar returned to Copenhagen, where a new laboratory was set up for him with the support of Ejnar Lundsgaard and with financial backing from American as well as Danish sources. The principal theme of research at the new "Cytofysiologisk Institute" was the metabolism of nucleosides and nucleotides. Kalckar attracted gifted young collaborators, such as Hans Klenow, Morris Friedkin, and Walter McNutt, and the laboratory became a leading center for work in this field.

In 1952 Kalckar began his studies on the metabolism of galactose in microbial and animal tissues. This became a principal pursuit after his move to the National Institutes of Health in 1952, first as a visiting scientist and later with a permanent appointment at the National Institute of Arthritis and Metabolic Diseases.

In mammals the utilization of galactose, a component of milk sugar and therefore a major constituent in the diet of infants, begins with its phosphorylation to galactose-1-P, which then must be converted to glucose-1-P, the further metabolism of which occurs by well-known reactions. The pioneering work of Luis Leloir led to the discovery of the central role of uridine diphosphate derivatives in the interconversion of galactose and glucose, sugars that are epimers, that is, differing in configuration only at a single carbon atom (C-4). Leloir showed that the two sugars are interconverted in the form of their uridine diphosphate derivatives as in Equation (4), catalyzed by an epimerase, and suggested that the synthesis of uridine diphosphate galactose (galactose-1-P-P-uridine) might take place by reaction (3):

$$\text{galactose-1-P} + \text{glucose-1-P-P-uridine} \longleftrightarrow \text{galactose-1-P-P-uridine} + \text{glucose-1-P} \qquad (3)$$

$$\text{galactose-1-P-P-uridine} \xleftrightarrow{\text{NAD}} \text{glucose-1-P-P-uridine} \qquad (4)$$

In the sum of (3) and (4) the uridine-linked forms of the sugars cancel out, and the overall reaction is:

$$\text{galactose-1-P} \longleftrightarrow \text{glucose-1-P} \qquad (3 + 4)$$

In 1953 Kalckar and his collaborators reported direct evidence that the synthesis of uridine diphosphate galactose does in fact occur in extracts of the yeast *Saccharomyces fragilis* by reaction (3), catalyzed by the enzyme galactose-1-P uridylyl transferase. They also found an alternative reaction for the synthesis of uridine diphosphate galactose in yeast:

$$\text{galactose-1-P} + \text{P-P-P-uridine} \longleftrightarrow \text{galactose-1-P-P-uridine} + \text{P-P} \qquad (5)$$

It is important to note that reaction (5) does not occur in human tissues, in which reaction (3), catalyzed by the uridylyl transferase, is an essential step in the utilization of galactose as an energy source.

Kalckar devised a method to determine the levels of the uridylyl transferase catalyzing reaction (3) in lysates of red blood cells, employing his favorite spectrophotometric approach. At just this time Kurt Isselbacher was also at NIH, carrying out research in collaboration with Gordon Tomkins and Julius Axelrod. As part of his clinical duties, Isselbacher was treating a child whom he diagnosed as suffering from galactosemia, a severe inherited disorder characterized by the inability to break down galactose, which leads to accumulation of high levels of galactose in blood and tissues. Isselbacher sought out Kalckar, and a collaboration was begun that soon led to the finding that the enzyme defect in the most serious form of human galactosemia is in the uridylyl transferase that catalyzes reaction (3). This in turn led to the development of a simple test for the presence or absence of this enzyme in red blood cells that is now widely used to screen newborn infants for galactosemia, a disease that can be effectively treated by removal of milk and other sources of galactose from the diet. The development of this test was of great practical consequence since early diagnosis is vital to prevent severe mental retardation and other developmental defects.

In 1958 Kalckar accepted a professorship in the Department of Biology of Johns Hopkins University. This year also marked the publication of his highly original proposal that the contamination of foodstuffs from the fallout following atmospheric tests of nuclear weapons could be measured by the analysis of the content of strontium-90 in the milk-teeth of young children. As he pointed out, measurement of radiation from deciduous incisor teeth would reveal the levels of isotope ingested about seven years previous to shedding of the teeth, when the calcified structure of the teeth had been deposited. By this proposal Kalckar hoped to focus attention in a dramatic way on the pollution of the environment by tests of nuclear weapons. The idea attracted considerable attention, and extensive collections of milk-teeth were in fact made, particularly by a group of researchers at Washington University in St. Louis. It was learned that milk-teeth from children born in 1956 contained about ten times more strontium-90 than teeth from children born in 1950. Fortunately, after the ban on atmospheric testing the levels fell once again to lower levels.

In 1961 Kalckar moved to the Harvard Medical School as professor of biological chemistry and head of the Biochemical Research Laboratory of Massachusetts General Hospital (MGH). He succeeded Fritz Lipmann in that position. Here he continued his studies on the metabolism of galactose in animal tissues with special attention to the epimerase that catalyzes reaction (4) above.

Kalckar now also became deeply interested in the role of the cell surface in sensory processes and in cell signaling and often referred to this field, then newly emerging, as "ektobiology." Winfried Boos, his young colleague at MGH, carried out an intensive study of the transport of galactose into cells of *E. coli*, which culminated in the isolation and detailed characterization of a specific galactose-binding protein that was shown to be an essential part of the transport system. Julius Adler and his colleagues at the University of Wisconsin discovered at about the same time that cells of *E. coli* can detect the presence of galactose in the medium and "chase" this sugar by a positive chemotactic response. Kalckar immediately suggested that the galactose-binding protein that could "recognize" galactose for transport might also be needed for the chemotactic response. The first tests of this notion, however, were disappointingly negative. Kalckar persisted in his idea, however, and further experiments revealed that an unexpected complexity of the transport system had rendered the first tests invalid. The final definitive experiments revealed that the binding protein plays a vital role not only in the transport of galactose but also in chemotaxis, an outcome that gave Kalckar considerable satisfaction.

Kalckar was also greatly interested in the conversion of galactose to cell-surface lipopolysaccharides in bacteria, work vigorously pursued in his MGH laboratories by Hiroshi Nikaido. Nikaido's pioneering studies on the biosynthesis of lipopolysaccharide in *Salmonella* employed both genetic and biochemical approaches and did much to clarify the complex reaction sequences, particularly the role of lipid-linked intermediates.

Kalckar turned next to the problem of the regulation of the transport of sugars into mammalian cells and the importance of this process in tumor cells. This work was in part stimulated by a collaborative study in 1973 with Senitiroh Hakomori at the University of Washington in Seattle on carbohydrate utilization and the uptake of galactose in hamster cells transformed by polyoma virus.

In 1974 Kalckar retired as head of the Biochemical Research Laboratory but continued his research as visiting professor in the Huntington Laboratories at MGH until 1979. At that time he moved to the Department of Chemistry at Boston University as distinguished research professor, an appointment he greatly valued because it permitted him to continue his research interests in a new and stimulating environment to the very end of his life. The work on hexose transport and metabolism in normal and malignant cells continued to be the theme of much of the work in the laboratory at Boston University and the subject of many papers with his longtime collaborator, Donna Ullrey, the last of which was published only shortly before his death.

Kalckar's achievements in science brought him wide recognition, including election to the National Academy of Sciences, the Royal Danish Academy, and the American Acad emy of Arts and Sciences, as well as honorary degrees from Washington University, the University of Chicago, and the University of Copenhagen.

To his many friends, Kalckar's character and personality were as impressive as his scientific accomplishments. Throughout his entire career Kalckar won the affection and admiration of a large number of students and junior associates who were trained in his laboratory. The sweep of his intellect was very broad, his spirit was open and generous, and he had a wonderful sense of humor. Upon first acquaintance many found it difficult to follow the thread of his discourse, partly because he was apt to begin a new topic *in medias res* without explanatory preamble and partly because he often paid the listener the compliment of omitting from a chain of reasoning the links that seemed obvious. After one became accustomed to this style it enhanced the effect of his gentle, understated wit.

The same enlightened humanism that shaped Kalckar's tastes in music and the arts was evident in his view of world problems, as evidenced by his concern for the mounting threat of nuclear warfare and the dangers of continued testing of nuclear weapons, which he had dramatized by the milk-teeth collection project.

Kalckar's first marriage, to the musician Vibeke Meyer, ended in divorce in 1950. Three children, Sonja, Nina, and Niels, to whom he was deeply devoted, were born of his second marriage to the developmental biologist Barbara Wright. After dissolution of that marriage, Kalckar in 1968 married the interior designer Agnete Fridericia Laursen, who survives him. For the last twenty-three years of his life, Agnete's love and support were an essential part of Herman's life, and their home in Cambridge was a focus of warmth and hospitality for friends and colleagues.

Selected Bibliography

1937 Phosphorylation in kidney tissues. *Enzymologia* 2:47.
1938 Formation of a new phosphate ester in kidney extracts. *Nature* 142:871.
1939 Coupling between phosphorylations and oxidations in kidney extracts. *Biochem. J.* 6:209.
1941 The nature of energetic coupling in biological synthesis. *Chem. Rev.* 28:71.
With S. P. Colowick. An activator of the hexokinase system. *J. Biol. Chem.* 137:789.
1942 The enzymatic action of myokinase. *J. Biol. Chem.* 143:299.
1943 With S. P. Colowick. The role of myokinase in transphosphorylations. I. The enzymatic phosphorylation of hexoses by adenylpyrophosphate. *J. Biol. Chem.* 148:117.
1944 Spectroscopic microdetermination of muscle adenylic acid. *Science* 99:131.
1945 Enzymatic synthesis of a nucleoside. *J. Biol. Chem.* 158:723.
1947 The enzymatic synthesis of purine ribosides. *J. Biol. Chem.* 167:477.

1948 With H. Klenow. Enzymatic transformation of pteroylglutamic acid. *J. Biol. Chem.* 172:351.

1949 With M. Friedkin and E. Hoff-Jorgensen. Enzymatic synthesis of desoxyribose nucleoside with desoxyribose phosphate ester. *J. Biol. Chem.* 178:527.

1952 Enzymatic reactions in purine metabolism. *Harvey Lect. Ser.* 45, pp. 11-39.

1953 With B. Braganea and A. Munch-Petersen. Uridyl transferases and the formation of UDP-galactose. *Nature* 172:1038.

1954 Biosynthesis and metabolism of phosphorus compounds. *Annu. Rev. Biochem*. 23:527.

With J. L. Strominger, J. Axelrod, and E. Maxwell. Enzymatic oxidation of uridine diphosphate glucose to uridine diphosphate glucuronic acid. *J. Am. Chem. Soc.* 76:6411.

1955 With E. Maxwell and R. M. Burton. Galacto-waldenase and the enzymatic incorporation of galactose-1-phosphate in mammalian tissues. *Biochem. Biophys. Acta* 18:389.

1956 With E. P. Anderson and K. J. Isselbacher. Galactosemia, a congenital defect in a nucleotide transferase. A preliminary report. *Proc. Natl. Acad. Sci. U.S.A.* 42:49.

With K. J. Isselbacher, E. P. Anderson, and K. Kurahashi. Congenital galactosemia, a single enzymatic block in galactose metabolism. *Science* 123:635.

1957 Biochemical mutations in man and microorganisms. *Science* 125:105.

With E. P. Anderson, K. Kurahashi, and K. J. Isselbacher. A specific enzymatic assay for the diagnosis of congenital galactosemia. I. The consumption test. *J. Lab. Clin. Med.* 50:469.

1958 An international milk teeth radiation census. *Nature* 182:283.

With E. Maxwell and H. de Robichon-Szulmajster. Yeast uridine diphosphate galactose-4-epimerase: correlation between activity and fluorescence. *Arch. Biochem. Biophys.* 78:407.

1962 With T. A. Sundararajan and A. M. C. Rapin. Biochemical observations on *E. coli* mutants defective in uridine diphospho-glucose. *Proc. Natl. Acad. Sci. U.S.A.* 48:2187.

1965 Galactose metabolism and cell sociology. *Science* 150:305.

1968 With A. M. C. Rapin and L. Alberico. The metabolic basis for making of receptor sites on *E. coli* K12 for C21, a lipopolysaccharide core-specific phage. *Arch. Biochem.* 128:95.

1969 *Biological Phosphorylations: Development of Concepts.* Englewood Cliffs, N.J.: Prentice-Hall.

With H. C. P. Wu and W. Boos. Role of the galactose transport system in the retention of intracellular galactose in *Escherichia coli. J. Mol. Biol.* 41:109.

1971 The periplasmic galactose binding protein of *Escherichia coli. Science* 174:557.

1973 With D. Ullrey, S. Kijomoto, and S. Hakomori. Carbohydrate catabolism and the enhancement of the uptake of galactose in hamster cells transformed by polyoma virus. *Proc. Natl. Acad. Sci. U.S.A.* 70:839.

1974 Origins of the concept of oxidative phosphorylation. *Mol. Cell. Biochem.* 5:55.

1975 With T. J. Silhavy and W. Boos. The role of the *Escherichia coli* galactose-binding protein in galactose transport and chemotaxis. In *Twenty-fifth Mosbacher Colloquium*, ed. L. Jaenicke, pp. 1-30. Berlin: Springer-Verlag.

1976 Cellular regulation of transport and uptake of nutrients: an overview. *J. Cell. Physiol.* 89:503

1985 With P. Plesner and D. B. Ullrey. Mutations in the phosphoglucose isomerase gene can lead to marked alterations in cellular ATP levels in cultured fibroblasts exposed to simple nutrient shifts. *Proc. Natl. Acad. Sci. U.S.A.* 82:2761.

1986 Autobiographical notes from a nomadic biochemist. In *Selected Topics in the History of Biochemistry*, ed. G. Semenza and R. Jaenicke, pp. 101-76. Amsterdam: Elsevier Science Publishers.

1991 Fifty years of biological research—from oxidative phosphorylation to energy-requiring transport regulation. *Annu. Rev. Biochem.* 60:1.

With D. B. Ullrey. Search for cellular phosphorylation products of D-allose. *Proc. Natl. Acad. Sci. U.S.A.* 88:1504.

Israel Michael Lerner

May 14, 1910–June 12, 1977

R. W. ALLARD

I. MICHAEL LERNER MADE sophisticated contributions to population, quantitative, and evolutionary genetics, and animal breeding. He excelled in teaching at all levels, from providing nonscientists with realistic concepts of science and its importance in making policy decisions regarding the future of society, to teaching advanced courses in genetics. He also had exceptional talent for management and served with distinction in many assignments dealing with intramural affairs at the University of California and with scientific policy at the national and international levels. Despite the predominantly scientific cast of his professional career, Lerner's primary interests throughout his life were in the humanities. It is thus remarkable that he accomplished so much in science. How this came to be Lerner explained eloquently in the brief autobiographical statement he submitted to the National Academy of Sciences upon his election in 1959. The following quotation, with minor editing, is taken from that statement:

> I have been a scientist, not through any overwhelming curiosity about nature, not because of a drive to contribute to the welfare of humanity, nor because of the promise of any aesthetic satisfaction from experimentation and generalization. Indeed my inclinations have always been in the direction of the humanities (I still regard myself as an historian manqué), to

ward such arts as the theater or toward politics or the law. I drifted into a scientific career by following a line of least, or at best, little resistance. I was lucky in my associates, I have been fortunate in the circumstances of my personal life, and the genes I inherited interacted favorably with the environments I found myself in. That is as much as introspection can yield regarding how I came to be me. The outward facts follow.

There was nothing in my family's tradition or in my home environment that would have predisposed me to an academic career in science. My father, at the time of my birth, was a successful importer and exporter living in Harbin, Manchuria (then a Chinese territory under a long-term Russian government lease). The life we led was reasonably typical of middle-class prosperous Russian families with some cultural pretensions, the theater, lectures, and concerts occupying a fairly prominent place in our daily existence. Certain departures from the Russian norm were occasioned by Harbin's geographical position (Chinese house servants, English regarded as a more important language than French). My sister (two years older than I) and I were first taken care of by two Russian nurses and then by German governesses.

By the time the Russian revolution broke out in 1917 my sister and I were being tutored at home in the regular school subjects appropriate to our ages, with English and piano lessons on the side. The revolution had a tremendous impact on Harbin and on the personal circumstances of our family. The wave of émigrés passing through Harbin, among whom a high proportion belong to the intelligensia, increased cultural activities for a short period far beyond the town's proportions in size or its provincial geographical position. Former University professors (true bearded Geiheimrats rather than the American variety) were so numerous that even many secondary schools were able to obtain their services for teaching. Thus, when at age 12, my home education gave way to school attendance, much of my education was by specialists in University subjects, rather than by secondary school pedagogues—thus I was exposed to political economy, philosophy, literary criticism and history at a much younger age than most of my contemporaries from elsewhere. However, although letters, humanities and social sciences were taught by former University professors in the Harbin schools, this was not the case in the natural sciences. I suppose the reason was that there were fewer scientists in Russia to start with, that fewer scientists migrated, and that an even smaller proportion took up teaching as an occupation. Another result of the revolution was that, during my childhood, Harbin became a center of music and theater. We had a full nine month opera season, a symphony, at least one dramatic theater operating throughout the year, a ballet troupe, a light opera troupe, and concerts by instrumentalists and singers. I acquired from these influences a deep and lasting interest and love for the performing arts, particularly opera.

My father's finances suffered such severe reverses as a result of the revolution that, instead of several tutors, my sister and I were sent to schools, private because public schools were only at the primary level. Piano lessons, and for a very short time drawing lessons (for which I exhibited absolutely no talent), were the only extra lessons continued. During this period my parents made attempts (unsuccessful) to emigrate to Switzerland. In the fall of 1922, I was sent to the Harbin Public Commercial School, where I spent five years, graduating in the spring of 1927. The Russian Commercial School in Harbin was a compromise between a classically oriented (Gymnasium) and a technically oriented (Realschule) secondary school. The direction I was to take after school was not at all clear. It was understood that I should go on to University, but whether it would be to one of the institutions in Harbin, or whether I would follow my sister to Russia (where she became a physician), or emigrate to Europe, or to America, was unclear.

Of the various prospects, going to America appealed most. I knew the language and I understood that working one's way through college there was much more common than in Europe. The prospect of doing military service in Russia, the difficulties for a scion of a bourgeois entering a Russian University at that time (my sister had difficulties), the uncertainties as to how my further education would be financed, were factors militating against going to Russia. Harbin itself, even if I were successful in completing a university course in some subject, provided only dismal vistas. So, America was the choice. But, by 1927, U.S. Immigration laws had tightened and a wait of many years for a visa was likely. However, a rumor spread through our school that Canada was an equally good place to go, provided that one announced intention to engage in agriculture. Suffice it to say that I left Harbin without passport, visa, or funds and found myself in September of 1927 in Vancouver, B.C. engaged in a farm job, digging ditches and caring for chickens at $2.00 per day on the Poultry Farm of the University of British Columbia. Thus I drifted by accident into a field which interested me only casually. A factor of major importance in staying in this field was the encouragement of Vigfus F. Asmundson, then an Assistant Professor in the Department, who was engaged in Poultry Genetics research. I soon became his assistant and continued to work with him until I

> obtained my B.S. and M.S. degrees. He lent me money to pay tuition and often, when the Department budget was strained, paid me out of his own pocket for the work I was doing. It was to him that I owed my determination not only to enter into an academic career, but to do so specifically in the field of Genetics (I had no inkling that he would move to Davis and I would one day become his fellow staff member in the Poultry Department at the University of California). In 1931 Theodosius Dobzhansky spent a month in Vancouver and I had nearly daily contact with him. Dobzhansky's enthusiasm for research in genetics provided very strong reinforcement for my wishes to continue graduate work but it was not until 1933 that an offer of an assistantship that I could afford to accept presented itself. It was in the Poultry Department at Berkeley with L. W. Taylor, a fact that committed me to work with the chicken for the next 25 years.

When Lerner received his Ph.D. in genetics at Berkeley in 1936, he was appointed instructor in poultry husbandry, from which level he received accelerated promotions to professor. Thus, revolution, financial problems, periods of going hungry, and other dire difficulties, interspersed with some comic relief episodes, together with much heart-warming help and encouragement, launched what would prove to be a remarkable career but one that took a very different direction from the course that might have been predicted from his early knowledge and deep attraction to the humanities.

The researches conducted by Lerner in his twenty-five years in the Department of Poultry Husbandry at Berkeley (many in collaboration with Everett R. Dempster of the Department of Genetics and Dorothy C. Lowry, his technical assistant) were reported in more than 175 published papers. As a young faculty member he dealt with the inheritance of a number of components underlying egg production, the effects of practicing selection in conjunction with inbreeding, and with empirical tests of theoretically predicted gains from simultaneous selection for several different inherited characteristics. These studies led to construc tion of selection indices, credited by commercial poultry producers as responsible for substantial increases in egg production. Two of Lerner's books (*Population Genetics and Animal Improvement*, 1950, and *Genetic Basis of Selection*, 1958) were highly influential in transforming animal breeding from an art to a science based on multifactorial Mendelian inheritance. In another book (*Genetic Homeostasis*, 1954) Lerner formulated a brilliant hypothesis relating natural selection and evolution that stimulated much thought, discussion, and controversy (in the words of one generally unfriendly critic, it was speculative, imaginative, controversial, and influential).

During the late 1950s, Lerner's interests turned increasingly to the ways that studies of domestic and laboratory animals might throw light on the genetic basis of selection and evolution. In 1958 he joined the Department of Genetics at Berkeley, adopting the common flour beetle as an experimental organism more suitable for his new purposes and carried out many exquisitely designed competition experiments. He showed that the outcomes of his experiments were almost entirely deterministic when the experimental conditions as well as the genetic compositions of the competing entities were carefully controlled. He also showed that some of the characteristics involved in competitive ability were behavioral. This led him to in-depth studies of the technical literature in psychology, and he was invited to join, on a part-time basis, the Institute of Personality Assessment on the Berkeley campus.

During the late 1960s and until his death in 1977, Lerner's research activities were increasingly replaced by administrative and editorial work and the summarizing of various aspects of evolutionary genetics in numerous invited addresses and articles. He served as chairman of his department and the graduate council at Berkeley and on various boards of the statewide University of California system and the U.S. Department of Agriculture. He also served as editor of the journal *Evolution* and as secretary of the International Commission on Genetic Congresses.

Lerner considered teaching to be of primary importance. Among his major contributions to teaching were a book titled *Heredity, Evolution and Society* and an associated course designed to provide nonscientists with understanding of the role of science in formulating sound public policy. Both the course and the book were highly popular at Berkeley, and both have been widely imitated.

Lerner received many honors and recognitions. Those he valued most, in addition to membership in the National Academy of Sciences, were election to the American Academy of Arts and Sciences, to the vice-presidency of the American Society of Naturalists, as a foreign member of the Florentine Academia dei Georgofili, and as editor of *Evolution*. He also valued receiving the Borden Award and Gold Medal, the Belling Prize in Genetics, the Poultry Science Research Award, and honorary degrees conferred by the University of British Columbia and the University of Edinburgh.

Throughout his life Lerner followed a demanding ethical imperative. He was meticulously honest and straightforward in expressing his opinions while at the same time managing to avoid offense. During the last years of his life his health was poor. His death on June 12, 1977, at age sixty-seven, followed a series of major abdominal operations as well as operations for cataracts and a detached retina, with complications from emphysema. His courage during these tribulations was remarkable. Lerner greatly enjoyed many aspects of life and conveyed his pleasure to others. He is greatly missed by his many friends throughout the world. It is appropriate to close this memoir with an appreciation of

Ruth Steward Lerner, his classmate at the University of British Columbia. She provided him with advice, encouragement, and support, all greatly appreciated, throughout the forty years of their marriage.

I first met Michael Lerner in Berkeley, probably in 1939 or 1940, when I was an undergraduate student on the Davis campus of the University of California. Starting in 1946, when I joined the faculty at Davis, many opportunities arose to talk to Lerner at Davis, Berkeley, and at scientific meetings at various places in North America and Europe. I admired his breadth of knowledge in biology and the humanities and treasured his friendship and counsel over the more than three decades I was privileged to continue my association with him.

Selected Bibliography

1932 With V. S. Asmundson. Inheritance of growth rate in the domestic fowl. *Sci. Agric.* 2:652-64.

1933 With J. V. Bierly and V. E. Palmer. Fowl paralysis (Neurolymphomatosis gallinarum) in chicks under three months of age. *Can. J. Res.* 8:30.

1936 Heterogony in the axial skeleton of the creeper fowl. *Am. Nat.* 70:595-98.

1937 With L. W. Taylor. The spurious nature of linkage between the length of laying year and sexual maturity in the fowl. *Am. Nat.* 71:617-22.

1939 The shape of the chick embryo growth curve. *Science* 89:16-17.

Allometric studies in poultry. In *Proc. 7th World Poultry Congress.* Cleveland, pp. 85-88.

1940 With L. W. Taylor. The effect of controlled culling on the efficiency of progeny tests. *J. Agric. Res.* 61:755-64.

With J. Needham. The terminology of relative growth rates. *Nature* 146:618.

1941 With J. S. Huxley and J. Needham. Terminology of relative growth rates. *Nature* 148:225.

1943 The failure of selection to modify shank-growth ratios of the domestic fowl. *Genetics* 28:119-32.

1944 Lethal and sublethal characters in farm animals. *J. Hered*. 35:219-24.
1947 With L. N. Hazel. Population genetics of a poultry flock under selection. *Genetics* 32:325-39.
With E. R. Dempster. The optimum structure of breeding flocks. 1. Rate of genetic improvement under different plans. *Genetics* 32:555-66.
With E. R. Dempster. Heritability of threshold characters. *Genetics* 35:212-34.
1948 With E. R. Dempster. Some aspects of evolutionary theory in the light of recent work in animal breeding. *Evolution* 2:19-28.
With D. Lowry. The heritability of accumulative monthly and annual egg production. *Poult. Sci.* 27:67-78.
1949 With A. Robertson. The heritability of all-or-none traits: viability in poultry. *Genetics* 34:395-411.
1950 *Population Genetics and Animal Improvement*. Cambridge: Cambridge University Press.
Genetics in the U.S.S.R.: An Obituary. University of British Columbia Publ. Lecture Series 8.
1954 *Genetic Homeostasis*. Edinburgh: Oliver and Boyd.
1955 Concluding survey. *Cold Spring Harbor Symp. Quant. Biol.* 20:334-40.
1958 *Genetic Basis of Selection*. New York: John Wiley.
The concept of natural selection: a centennial view. *Proc. Am. Philos. Soc.* 103:173-82.
1960 Marxist biology viewed dimly. *Am. Nat.* 91:45-55.

Dr. Thomas S. Lovering.

Thomas Seward Lovering

May 12, 1896–April 9, 1991

HAL T. MORRIS

INNOVATIVE FIELD AND laboratory studies of the relations of hydrothermally altered wall-rocks to minable ore deposits were the principal scientific contributions of T. S. Lovering during an illustrious career that also included theoretical analysis, laboratory research, teaching, and administrative duties in the fields of economic geology and geochemistry. For more than forty years he was affiliated with the U.S. Geological Survey in detailed investigations of mining districts and mineralized terranes chiefly in Colorado and Utah. During the latter half or so of his USGS career, he often served as a U.S. delegate to mineral conferences throughout the world and as a minerals consultant to other federal civilian and military agencies. To many who knew him only casually from his penetrating questions and discussions at scientific meetings and symposia, he sometimes appeared brusque, argumentative, and perhaps egocentric. To his close associates and co-workers, however, he was invariably courteous, generous, and steadfast in his support and friendship. To him the search for scientific excellence was paramount and all else was secondary.

Tom, as he was known by his colleagues and a wide circle of both older and younger acquaintances, was born in St. Paul, Minnesota, on May 12, 1896. During World War I he trained as a Navy aviation cadet, but the armistice was signed before he was transferred to combat duty, and upon his discharge in 1919 he entered the Minnesota School of Mines. In 1922 he graduated with an E.M. degree and later in the same year enrolled in the graduate school of the University of Minnesota, where he received an M.S. degree in geology in 1923 and a Ph.D. in economic geology in 1924. While at Minnesota he was strongly influenced by Professors Frank F. Grout and John W. Gruner with whom he maintained an infrequent correspondence for many years.

Tom's first position after completing his doctorate was an instructorship in the Department of Geology at the University of Arizona. He remained at Arizona for only one academic year, accepting a position in 1925 with the U.S. Geological Survey to conduct studies of selected mining districts in the Colorado Front Range under the general supervision of B. S. Butler. In 1934 he gave up this full-time position with the USGS and became an associate professor of geology at the University of Michigan. During the following eight academic years, he also undertook many laboratory investigations and worked during the summer months for the USGS in Colorado, where he continued his studies of tungsten and base- and precious-metal mining districts and participated in regional mapping projects.

Upon the entry of the United States into World War II, Tom took a leave of absence from Michigan and rejoined the USGS on a full-time war service appointment to assist in the Strategic Minerals Program. His wartime activities included the completion of several detailed reports on mining districts in Colorado and the early phases of what became a long-range study of deeply concealed ore bodies and associated surficial alteration zones in the East Tintic mining district of central Utah.

At the end of World War II, Tom returned to the Univer sity of Michigan, where he resumed his professorship in the Department of Geology and Mineralogy for the 1946-47 academic year. By this time, however, his field research at East Tintic had reached a critical phase, and in 1947 he resigned from the faculty at Michigan and accepted a permanent assignment with the Mineral Deposits Branch of the USGS. He remained in this position until his retirement in 1966 at age seventy. During his retirement years he continued to pursue both academic and research activities for nearly two decades, including the authorship of a number of scientific papers, teaching, mineral deposits consulting activities, and worldwide travel. Within about a month of reaching his ninety-fifth birthday he succumbed to leukemia on April 9, 1991, at his residence in Santa Barbara, California.

Tom Lovering made significant contributions in several disciplines of geological science, including geologic mapping, ore deposits studies, geochemistry, and the thermodynamics and cooling rates of igneous intrusions. He is probably most widely remembered for his studies of the geochemistry of magmatic hydrothermal wall-rock alteration in the Boulder County tungsten and gold district in Colorado and the East Tintic mining district in Utah. These studies have helped clarify the general processes of ore deposition and in a number of instances have provided direct guides to the occurrence of concealed ore deposits.

When Lovering undertook his alteration studies in Boulder, Colorado, in the 1930s, it was generally believed that the altered selvages of the tungsten- and gold-bearing ore shoots were created by wall-rock reactions with a single hydrothermal solution that concurrently deposited the ore and gangue minerals. Tom was able to show, however, that the wide outer zone of strongly argillized (clay-mineral rich) wall rock gives way abruptly near the ore shoots to a zone of sericitic alteration, which indicated a change from early strong acid solutions to near neutral solutions. These neutral solutions were then followed in turn by weakly alkaline fluids from which the gold, tungsten, and associated gangue minerals were deposited.

To further refine his hypothesis of the widely differing compositions of wall-rock altering solutions as compared to ore depositing solutions, Lovering undertook a detailed study of Utah's East Tintic mining district in the early 1940s. In this district hydrothermal wall-rock alteration zones adjacent to and above large replacement ore bodies are greatly more extensive than the relatively narrow selvages bordering the Boulder County veins. In addition, some well-defined geologic events, including minor faulting and igneous intrusions and eruptions, could be used to establish the relative timing of surges of the various altering solutions. The early results of his studies in East Tintic were published in 1949 as *Monograph I* by the Society of Economic Geologists. In this report he describes five distinct and separable periods of movement of magmatic hydrothermal fluids:

> First, an early district-wide flooding by neutral, magnesium-rich reducing solutions that dolomitized limestone wall-rocks and locally propylitized the basal parts of the earliest erupted lavas.
>
> Second, after a period of intrusion of minor bodies of monzonite and quartz-monzonite and the eruption of lavas, localized invasions of hot acidic solutions that severely leached and sanded the underlying limestones and hydrothermal dolomites, and strongly argillized the border areas of the minor intrusive plugs and the adjacent and nearby lavas. These argillized zones were comparable in many respects to the argillized envelopes of the Boulder County tungsten- and gold-ore shoots.
>
> Third, a multiple late-barren stage following closely in time on the mid-barren argillizing stage that is characterized in part by extensive silicification of carbonate rocks at depth and minor silicification of areas of lava and porphyry. Shortly following the silicifying solutions there was extensive

flooding of near-neutral, sulfur-rich solutions that converted iron oxide minerals in the intrusive rocks, lavas, and some of the sedimentary rocks to cubic pyrite.

Fourth, closely following the silicifying and pyritizing solutions, potassium-rich fluids moved along many of the channel-ways and conduits used by the preceding jasperoidizing solutions, converting some of the early-formed clay minerals to adularia, sericite, and illite. In most places minor clear quartz, pyritohedral pyrite, and sparse barite were deposited at this time.

Fifth, a change in composition of the potassium-rich solutions at the source with the abrupt and increasing appearance of ore ions in the hydrothermal solutions, eventually leading to the abundant precipitation of sulfide, sulfantimonide, sulfarsenide, and other ore minerals that replace part of the early-formed jasperoid, sanded dolomite, and other fresh and altered rocks. Fluid inclusions entrapped within these ore and gangue minerals indicate that they were deposited from near neutral, saline solutions at temperatures ranging from 150°-300°C.

For Lovering the selection of the East Tintic mining district for his most definitive studies of hydrothermal wall-rock alteration was highly fortuitous, inasmuch as relatively few major mining districts elsewhere in the world show such a distinct sequential series of hydrothermal events leading to ore deposition. Many other large districts are greatly complicated, in fact, by overprints of repetitive stages of solution activity, continued igneous emplacement, and similar geologic events.

Tom Lovering also was a lifelong advocate of detailed geologic mapping, and he often expressed his personal observation that theoretical and experimental studies were valid only when closely linked with meticulous field examinations and demonstrable physical relations. His geologic and alteration maps of the East Tintic district, published in 1960 as U.S. Geological Survey Mineral Investigations Field Studies Map MF-230, for example, were widely used by private mining and exploration groups in the district, leading to the discovery and development of two major new mines and the delineation of several other prospective zones that may be worthy of development.

Tom was particularly proud also of his contributions to the geologic map of Colorado, which was published in 1935 in collaboration with W. S. Burbank, E. N. Goddard, and E. B. Eckel, and his geologic maps of the Colorado Front Range and the Front Range mineral belt that accompany U.S. Geological Survey Professional Paper 223, published in 1950 with Eddie Goddard.

As an exception to his long-standing rule to avoid administrative and supervisory positions if at all possible, Tom agreed in 1954 to become chief of the USGS Section of Geochemical Exploration. In large part this reflected his deeply held interest in the refinement and continued development of new mineral exploration techniques. On stepping down from this position in 1958, Tom served until his retirement as a senior research scientist within the Geologic Division, continuing his studies of the geochemistry of hydrothermal wall-rock alteration, innovative techniques of geochemical exploration, and worldwide mineral resource evaluation.

During his lifetime Tom received many honors, including election to the National Academy of Sciences, the Distinguished Service Medal of the U.S. Department of the Interior, the Penrose Medal of the Society of Economic Geologists, the Jackling Medal of the American Institute of Mining and Metallurgical Engineers, and the Achievement Award Gold Medal of the University of Minnesota. He was an active member and supporter of numerous scientific and engineering societies, some of which include the American Association for the Advancement of Science; the American Geophysical Union; the American Association of Mining, Metallurgical, and Petroleum Engineers; the American Association of Petroleum Geologists; the Clay Minerals Society; the Geochemical Society; the Geological Society of

America; the Society of Economic Geologists; and, a particular favorite of his, the Colorado Scientific Society.

For approximately ten years after his retirement from the USGS Tom maintained a residence near the USGS regional headquarters in Lakewood, Colorado, but spent many winters in Tucson, Arizona, where he later accepted a research professorship in economic geology at the University of Arizona. During this time he also taught special courses in economic geology at the University of Texas, University of Utah, and other academic institutions. In 1976 he moved from Lakewood to Santa Barbara, California, where he became a research associate at the University of California, Santa Barbara. As with his other postretirement academic activities, this affiliation allowed Lovering to interact with bright students and an outstanding faculty in the academic and research environment that he so greatly enjoyed.

Throughout the greater part of his years as a student and as a professional geologist Tom enjoyed the love, support, and companionship of his wife, Corinne. He married Alexina Corrine Gray on October 11, 1919, shortly after his discharge from the Naval Aviation Corps. She was no stranger to the rigors of geologic fieldwork and cheerfully accepted the discomforts of wilderness camping in the Colorado Rockies, spartan lodgings in declining mining camps, and less than palatial accommodations in a wide variety of motels and hotels in small towns throughout the west. Corrine died on August 27, 1969. Finding his life lonely and incomplete in many ways without a close companion, Tom later married Mildred Stewart, with whom he shared many common interests, especially extensive land and sea travel throughout the world. Millie also preceded him in death on March 13, 1983.

Tom is survived by one son, Tom G. Lovering; a daughter-in-law, Dorothy; and two grandchildren, David and Karen.

Selected Bibliography

1923 The leaching of iron protores; solution and precipitation of silica in cold water. *Econ. Geol*. 18:523-40.

1927 Organic precipitation of metallic copper. *U.S. Geol. Surv. Bull*. 795:45-52.

1928 Geology of the Moffat Tunnel, Colorado. *Am. Inst. Min. Metall. (Engl. Trans.)* 18:337-46.

1929 The New World or Cooke City mining district, Montana. *U.S. Geol. Surv. Bull*. 811:1-87.

The Rawlins, Shirley, and Seminoe iron ore deposits, Carbon County, Wyoming. *U.S. Geol. Surv. Bull*. 811:203-35.

1932 Field evidence to distinguish overthrusting from underthrusting. *J. Geol*. 40:651-63.

1933 With J. H. Johnson. Meaning of unconformities in the stratigraphy of central Colorado. *Am. Assoc. Pet. Geol. Bull*. 17:353-74.

1934 Geology and ore deposits of the Breckinridge mining district, Colorado. *U.S. Geol. Surv. Prof. Pap*. 176.

1935 Geology and ore deposits of the Montezuma quadrangle, Colorado. *U.S. Geol. Surv. Prof. Pap*. 178.

Theory of heat conduction as applied to geological problems. *Geol. Soc. Am. Bull*. 46:87-100.

With W. S. Burbank, E. N. Goddard, and E. B. Eckel. Geologic map of Colorado. *U.S. Geol. Surv.* Scale 1:500,000.

1936 Heat conduction in dissimilar rocks and the use of thermal models. *Geol. Soc. Am. Bull.* 47:87-100.

1938 With E. N. Goddard. Laramide igneous sequence and differentiation in the Front Range, Colorado. *Geol. Soc. Am. Bull.* 49:35-68.

1941 The origin of the tungsten ores of Boulder County, Colorado. *Econ. Geol.* 36:229-79.

1943 *Minerals in World Affairs*. New York: Prentice-Hall.

1947 Sericite-kaolin alteration as a guide to ore. In Report of the Committee on Research on Ore Deposits of the Society of Economic Geologists. *Econ. Geol.* 42:534-35.

1948 With V. P. Sokoloff and H. T. Morris. Heavy metals in altered rocks over blind ore bodies, East Tintic district, Utah. *Econ. Geol.* 43:384-99.

1949 With others. Rock alteration as a guide to ore—East Tintic district, Utah. *Econ. Geol. Monograph I*.

1950 With E. N. Goddard. Geology and ore deposits of the Front Range, Colorado. *U.S. Geol. Surv. Prof. Pap.* 223.

The geochemistry of argillic and related types of alteration. *Colo. Sch. Mines Q.* 45:231-60.

1953 With O. L. Tweto. Geology and ore deposits of the Boulder county tungsten district, Colorado. *U.S. Geol. Surv. Prof. Pap.* 245.

1954 Safeguarding our mineral-dependent economy. *Geol. Soc. Am. Bull.* 64:101-25.

1955 Temperatures in and near intrusions. In *Economic Geology, 50*th *Annual Volume, Part 1*, ed. A. M. Bateman, pp. 249-81.

1958 Current developments in geochemical exploration. *Pakistan J. Sci.* 10:28-33.

1959 Significance of accumulator plants in rock weathering. *Geol. Soc. Am. Bull.* 70:781-800.

1960 With A. O. Shepard. Hydrothermal alteration zones caused by halogen acid solutions, East Tintic district, Utah. *Am. J. Sci. Bradley* Vol. 258-A:215-29.

With others. Geologic and alteration maps of the East Tintic district, Utah. *U.S. Geol. Surv. Min. Invest. Field Studies Map* MF-230, two sheets, Scale 1:9600.

1961 Sulfide ores formed from sulfide-deficient solutions. *Econ. Geol.* 56:68-99.

1963 Epigenetic, diplogenetic, syngenetic, and lithogenic deposits. *Econ. Geol.* 58:315-31.

1965 With H. T. Morris. Underground temperatures and heat flow in the East Tintic district, Utah. *U.S. Geol. Surv. Prof. Pap.* 504 F:F1-F28.

1967 With C. Engel. Translocation of silica and other elements into *Equisetum* and other grasses. *U.S. Geol. Surv. Prof. Pap.* 594 B:B1-B16.

1969 The origin of hydrothermal and low-temperature dolomite. *Econ. Geol.* 64:743-54.

1978 With O. L. Tweto and T. G. Lovering. Ore deposits of the Gilman district, Eagle County, Colorado. *U.S. Geol. Surv. Prof. Pap.* 1017.

1979 With H. T. Morris. General geology and mines of the East Tintic mining district, Utah and Juab Counties, Utah. *U.S. Geol. Surv. Prof. Pap.* 1024.

Courtesy of the Mary Lea Shane Archives of the Lick Observatory

N. U. Mayall

Nicholas Ulrich Mayall

May 9, 1906–January 5, 1993

DONALD E. OSTERBROCK

NICHOLAS U. MAYALL WAS born in the Midwest near the beginning of the twentieth century and died in Arizona near its end. He was an outstanding observational astronomer of gaseous nebulae, globular clusters, and galaxies. A product of the University of California, he joined the staff of its Lick Observatory even before he completed his Ph.D. and remained a member for more than a quarter of a century, obtaining with its small reflecting telescope excellent data on objects too faint for most astronomers to see. During World War II, he did important weapons development work at Cambridge, Massachusetts, and then at Pasadena, California. He helped launch the Lick Observatory's large, new, postwar telescope and then left the University of California to become the director of the still very young Kitt Peak National Observatory. He built it up into an important research institution, with large telescopes in both hemispheres, the National Optical Astronomy Observatories of today.

LIFE HISTORY

Mayall was born in Moline, Illinois, on May 9, 1906, the first of two sons. His father, Edwin L. Mayall, Sr., was an engineer who worked for a manufacturing firm in Illinois. His mother, Olive Ulrich Mayall, although she did not go to college, had a visionary understanding of higher education and set high standards for her two sons, Nicholas and Edwin, Jr. The family moved to the central valley of California sometime between 1907, when Edwin, Jr., was born, and 1913, when Nick began first grade at a small rural school near Modesto. By 1917 they had moved to Stockton. Except for one year back in Peoria, Illinois (1918-19), they stayed there through 1924, when Nick graduated from high school. Sometime along the line, probably while Nick was in high school, his parents were divorced.

In his senior year at Stockton High School, Nick arranged for the science club, of which he was secretary, to visit Lick Observatory, atop 4,200-foot-high Mount Hamilton, near San Jose. He was allowed to drive his father's auto and take a car full of boys up the winding mountain road, at that time unsurfaced dirt and gravel. It was his first sight of the observatory where he was to be a student and spend so much of his professional career. The visit inspired him to read all the astronomy books in the high school and local public libraries, but he had never thought of making astronomy his profession.

EDUCATION

In the fall of 1924 Mayall entered the University of California in Berkeley as a freshman in the College of Mining. He lived with his mother in an apartment on Durant Avenue and worked in the stacks at the university library to earn the money they needed to survive. Mayall was a good student, who ultimately was elected to Phi Beta Kappa and Sigma Xi, but by the midterm examinations in the first semester of his sophomore year he was heading for poor marks in mineralogy and chemistry laboratory. The dean called him in and found that Mayall was color-blind. He could not see the subtle color changes in flame and bead tests, nor in titrations and precipitations. His adviser told Mayall that there was nothing to do but change his major; a mining engineer had to be able to do those tests to graduate.

At this point Mayall decided that maybe astronomy was for him after all. He consulted his mother, who urged him to do what interested him most but, whatever it was, to do it well. First, he investigated carefully, asking several professors in the Berkeley astronomy department if they were happy in their work and making a decent living. Receiving affirmative answers, he transferred to the College of Letters and Science and majored in astronomy. This did not delay his progress, for nearly all his freshman work had been in mathematics and basic physical sciences. He found he liked astronomy very much and decided to go on to graduate work and a career as a research scientist.

In 1928, when Mayall finished his undergraduate work and received his A.B., the University of California had the most outstanding graduate astronomy program in the country. All the courses were taught by the professors in the Berkeley department on the campus, but many of the students did their thesis work on Mount Hamilton, under the tutelage of a Lick astronomer who became a member of their thesis committee. The founder and long-time chairman of the Berkeley department, Armin O. Leuschner, was an expert on celestial mechanics. Most of the other faculty members in the department were his former students, selected much more for their teaching ability than their research qualifications. What research they did do was also in celestial mechanics, except for C. Donald Shane, another Berkeley product who had done his thesis on carbon stars at Lick and who taught all the astrophysics courses in the depart ment, undergraduate or graduate. The graduate students were very well trained in the "theoretical astronomy" of that day (celestial mechanics), especially "Leuschner's method" for determining the orbit of a newly discovered comet or asteroid from three observations of its position.

Mayall received a teaching fellowship (making him equivalent to a teaching assistant of today, worth $600 a year then) for his first year as a graduate student, 1928-29. He enjoyed the course work, especially in astrophysics, and learned to calculate orbits rapidly and accurately. In the summer of 1928, before beginning as a graduate student, Mayall worked as the grader for an astronomy course taught in the Berkeley summer session by Seth B. Nicholson of the Mount Wilson Observatory staff. He was a Berkeley product himself, who in 1914 at Lick discovered Jupiter IX, a small faint moon of the giant planet. Its orbit became the subject for his 1915 Ph.D. thesis. Nicholson told Mayall that there would be an opening for a computer (a job held by a human being at that time) at the Mount Wilson Observatory offices in Pasadena the following year. By then Mayall was tiring of course work; he applied for the Mount Wilson position and got it. He worked there two years (1929-31), learning research by doing it. His job was to assist Nicholson and several other staff members, including Edwin Hubble, Alfred H. Joy, and Director Walter S. Adams, by measuring and reducing their observational data. When Clyde Tombaugh discovered Pluto at Lowell Observatory in 1930, Mayall, working with Nicholson, demonstrated that Leuschner had taught them well. They used the first preliminary orbit for the new planet, calculated by Ernest Clare Bower and Fred L. Whipple at Berkeley, to search along its early path for a direct photograph in the Mount Wilson collection that would provide an early position of it. They found it, a very faint object in a crowded field, on a plate taken in 1919, mea sured it, and quickly calculated and published the first definitive orbit with an eccentricity. It showed that Pluto was certainly a planet whose orbit crossed that of Neptune. But Mayall managed to get to Mount Wilson often himself, working with the staff astronomers on all the telescopes and using most of them on his own as well. He became especially close to Milton L. Humason and was inspired by Edwin Hubble. Mayall decided he wanted to make his career in nebular spectroscopy and research.

When he returned to Berkeley in the fall of 1931 to complete his graduate course work, Mayall had a Martin Kellogg Fellowship (worth $1,000 per year) and a prospective thesis topic, suggested by Hubble. It was to count the number of galaxies per unit area on the sky, as a function of position, on direct plates taken with the Crossley (36-inch) reflector, on Mount Hamilton, to supplement the counts Hubble himself was making with the 60-inch and 100-inch telescopes at Mount Wilson. Mayall did very well in his courses and went to Lick Observatory in the summer of 1932 to begin the main part of the observational work. He had learned well from his Mount Wilson mentors and was an expert at obtaining first-class direct photographs with the ancient and tricky Crossley telescope. Mayall made the counts on his own plates, after closely inspecting earlier ones taken by his predecessors at the instrument, Heber D. Curtis, Charles D. Perrine, and James E. Keeler, the latter the second director of Lick Observatory, who had put the Crossley into operating condition and with it first discovered (at the turn of the century) the very large number of spiral "nebulae" in the universe.

Mayall finished his thesis and received his Ph.D. degree at the 1934 Berkeley commencement. Hubble praised his work, which was in fact excellent technically. However, the whole program, on which Hubble himself spent years, never achieved very significant results. It was flawed by the lack of accurate magnitude standards for the faint galaxies at which it was aimed and by the then-unrecognized very strong clustering tendency of galaxies.

LICK OBSERVATORY

Mayall's thesis adviser, William H. Wright, a University of California graduate and a Lick Observatory staff member since 1897, was a nebular researcher and spectroscopist himself and a great friend and admirer of Hubble. Mayall wanted to design and build a small fast spectrograph, optimized for nebulae and galaxies, to use at the Crossley to make it competitive for at least some of the spectacular work that Humason and Hubble were then doing with the larger telescopes at Mount Wilson. Wright and Joseph H. Moore, the head of the Lick stellar spectroscopy program, encouraged Mayall to go ahead with the design and then had the spectrograph built in the observatory shop. Though there was no opening on the staff for even the most junior astronomer, they kept Mayall at Lick as an observing assistant after he got his Ph.D. It was the same position he held for the final year of his thesis work and in reality allowed him to devote most of his time to his own research. The job paid very poorly, but the Great Depression was at its height (or in its depth), and there were few available alternatives, none for Mayall, who was committed to a research career with the spectrograph he had designed. He had hoped for a position at Mount Wilson, but there were no openings at all there because of the Depression.

As he began his postdoctoral career at Lick, Mayall married Kathleen (Kay) Boxall of Los Angeles on June 30, 1934. They met during his two years in Pasadena, according to family legend, at a field hockey game, probably at Tournament Park, very near Caltech. Whether it was a mixed game or Mayall was a spectator was not reported. They moved into a small apartment in the little astronomy village on the summit of Mount Hamilton, where all the astronomers lived.

One year later, on July 1, 1935, Robert G. Aitken, the elderly director of Lick Observatory, retired, and Wright succeeded him in the post. The two of them, and Moore, had managed to keep Mayall and his good friend Arthur B. Wyse, who had also received his Lick Ph.D. in 1934, on the staff, initially as observing assistants. Now as assistant astronomers they replaced Aitken and Robert J. Trumpler, who moved to Berkeley in 1935.

Mayall began using his new nebular spectrograph at the Crossley. Although it was not competitive with Humason's instrument on the much larger 100-inch telescope for stars or elliptical galaxies, with their condensed, relatively bright nuclei, the Lick spectrograph was actually faster for extended, low-surface-brightness gaseous nebulae and irregular galaxies. This was particularly the case in the ultraviolet, for Mayall, with Wright's strong encouragement, used quartz and ultraviolet transmitting optics, in contrast to the Mount Wilson spectrographs with their heavy glass lenses and prisms. With it Mayall got the first really good spectrum of the Crab nebula. From it and the previously published angular rate of expansion of the nebula, he was able to estimate its distance. With this data, Mayall became the first to recognize and prove the Crab nebula to be the remnant of a supernova observed and recorded by Chinese astronomer-astrologers in 1054, rather than an ordinary nova. He also found important new results on emission nebulae in the nearby spiral galaxy M 33 and various irregular galaxies and on the unexpected occurrence of forbidden emission lines of ionized oxygen in the spectra of the nuclei of many galaxies, a sign of the frequent presence of ionized interstellar gas even in the centers of these objects. With Hubble's encour agement Mayall measured spectroscopically the rotational velocities of several spiral galaxies. Wyse collaborated with Mayall on several papers on the interpretation of the measured radial velocities of the H II regions in M 31 and M 33 in terms of the rotation of these two especially nearby spiral galaxies, the gravitational field that it implied, and hence the distribution of mass within them. They made a good team, Wyse more theoretically inclined, Mayall an exceptionally skilled observer with the Crossley reflector he knew so well. His color blindness stood him in good stead here, for along with it he apparently had much more acute sensitivity to very low light levels than most mortals. Certainly he could see on the slit and in the periscope eyepiece of his spectrograph objects that were too faint for most other astronomers who observed with him, including myself.

Among the California graduate students who worked with him in those early years at Lick, the closest to Mayall were Daniel M. Popper and Lawrence H. Aller (who finished his Ph.D. at Harvard but came back to Mount Hamilton to observe several times). They both admired him greatly for his observational skills, his dedication to astronomy, and his warm, friendly personality. He was a good adviser to them and a realistic one.

Jan H. Oort, the outstanding Dutch astronomer, collaborated with J. J. L. Duyvendak, an Oriental scholar, in establishing the identity of the Crab nebula with the "new star" recorded by the Chinese a millennium ago. After Holland was overrun by the Nazis early in World War II, Oort, by correspondence, suggested to Mayall further ancient sources, including Semitic records, which might contain information. Mayall helped track them down at Berkeley and published the results in a joint paper with Oort.

The Mayalls had two children, Pamela and Bruce, who grew up on the mountain and attended its one-room school.

When they graduated from eighth grade, they had to go away to boarding schools. Mayall took the hour-a-day job as postmaster on Mount Hamilton to help pay the associated costs.

WORLD WAR II

World War II abruptly put Mayall's research career on hold, just as he was getting established. Well before America entered the war both he and Wyse applied for Naval Reserve commissions, to be called to duty as navigation instructors if needed. Mayall was rejected, because his color blindness prevented him from passing the required physical examination. In the fall of 1941 Wyse joined a wartime Navy antisubmarine technical project at San Diego as a civilian scientist. He was killed in sea trials of a proposed new submarine detection system in June 1942 in a collision of two dirigibles over the Atlantic Ocean. He had been Mayall's close friend. Mayall, along with nearly everyone else who knew Wyse, expected that he would someday become director of Lick Observatory.

Even before Wyse's death, Mayall accepted a position at the Radiation Laboratory in Cambridge, Massachusetts, to work on radar development. Gerald E. Kron (who recruited him) and Hamilton M. Jeffers, of the Lick staff, both single men, had begun working there before Pearl Harbor. Mayall worked on testing and calibrating the accuracy of the positions of airplanes provided by the early radar systems, comparing them with optical, visual, and photographic positions. However, the Boston climate, changeable and extreme compared with the California weather to which Mayall was accustomed, caused him and his family many colds and illnesses and aggravated his arthritis and sciatica. He felt he could not make real contributions at the Radiation Laboratory, dominated by electronic and antenna experts.

In mid-1943 Mayall arranged to transfer to the Mount Wilson Observatory offices in Pasadena, where several wartime Office of Scientific Research and Development projects concerned with optics, aerial gunnery, bombing tactics, and aerial photography were under way. There, in the atmosphere of California and astronomy, Mayall's health was restored. In one of the ironies of the war, he ate lunch daily with the two German-born Mount Wilson Observatory staff members—Rudolph Minkowski, a naturalized American citizen who was working with him on the OSRD projects, and Walter Baade, still a German national, who could not participate in or even know of the war work. Presumably, Mayall and Minkowski "buttoned up their lips" (as the wartime posters urged them to do) and did not pass any military secrets to Baade. His enemy alien status meant he was restricted to Los Angeles County, and, as practically the only member of the Mount Wilson staff not doing any war work, Baade had nearly unlimited use of the 100-inch telescope. Under the wartime brownout in Los Angeles, the night skies were unusually dark, and Baade was able to take direct photographs of the nearby Andromeda galaxy, M 31, and its companions, which showed stars to a fainter level than he had been able to reach before. This observation was the final evidence in his great discovery of the "two stellar populations," young stars and old. Mayall was on the scene in the late summer and fall of 1943 as Baade did this work and discussed it daily with him. Astronomical observations were not military secrets, and the enthusiastic Mayall kept the few elderly Lick astronomers still on the job at Mount Hamilton informed of Baade's epochal results.

Mayall enjoyed the Mount Wilson Observatory atmosphere, especially when he was even allowed to work with the 60-inch telescope for two nights during his Christmas vacation! But he believed that, owing to mismanagement in the higher levels of the OSRD optical instruments division, he and the Mount Wilson group were not being given a chance to make a really effective contribution to the war effort. Hence, he welcomed an opportunity to transfer in February 1944 to the California Institute of Technology, whose big rocket development project was clearly providing immediately effective weapons. Several other astronomers were working there, including Kron, who had moved there earlier; Horace W. Babcock, who did his Lick Ph.D. thesis under Mayall's supervision; and John B. Irwin, a former Berkeley and Lick graduate student. Mayall, who worked on the Caltech campus and sometimes at the more open testing areas at Inyokern, in the Mojave Desert, soon made himself a highly productive member of the project. He became an expert at high-speed photography, necessary to study and understand rocket trajectories and impacts. After the German surrender in the spring of 1945, Mayall was transferred to an ultrasecret group working on very-high-speed photography for the atomic bomb project, in connection with the plutonium implosion weapon. He made at least two trips to Los Alamos, one at about the time of the Trinity test, but he was not there for the firing. Probably he had briefed the Los Alamos high-speed photography group or discussed some of their results with them. Mayall, a confirmed believer in security to the end of his life, steadfastly declined to discuss this aspect of his wartime career.

Soon after the war ended, Mayall was released from his post, and by October 1, 1945, he was back in astronomical research at his beloved Mount Hamilton. During his three years with the OSRD, Mayall had made important contributions to the war effort, particularly at Caltech and Inyokern. In addition, he had gotten out of the comfortable little world of astronomy and had greatly broadened his outlook on science, research, and leadership. At the Radiation Labo ratory and at Caltech, Mayall saw big science in action, in its wartime version, and he made many contacts with physicists with whom he would interact frequently in his later years as director of Kitt Peak National Observatory.

THE 120-INCH TELESCOPE

During World War II, Mayall played an important role in determining the future of Lick Observatory. From the time he returned to Berkeley and Lick in 1931, after his two years as an assistant at Mount Wilson, he felt acutely the need for a larger telescope at Mount Hamilton. The Lick astronomers prided themselves on getting important results with their small, 36-inch Crossley reflector, which had been dwarfed by the Mount Wilson 60-inch since 1908, the Dominion Astrophysical Observatory 72-inch since 1917, and the Mount Wilson 100-inch since 1919. Mayall became an expert observer with the Crossley, but he realized that it could never really compete with a telescope with three times its diameter, nine times its collecting area. It would be even worse when the Palomar 200-inch came into use. Mayall and the other younger Lick faculty members believed that the older astronomers, Wright and Moore, were too committed to the small telescopes and should have worked harder to get a larger reflector for Mount Hamilton. Wright was proud of what he had accomplished with the Crossley and tended to scoff at the big-telescope mystique, but actually behind the scenes he and Aitken, his predecessor, had tried hard to raise the money for a larger reflector from private sources and also to persuade University of California President Robert G. Sproul to put it in the budget. They failed in each attempt, largely because of the Great Depression. However, unknown to Mayall, Sproul changed his mind in 1942, after his first choice for a director to succeed Wright, Paul W. Merrill of Mount Wilson Observatory, declined to leave the big telescopes even for the directorship of the University of California's famous research institution. Sproul, shaken by Merrill's refusal, told the regents that they must raise the money somehow after the war ended. At the same time, he either secretly appointed Shane, by then chairman of the Berkeley Astronomical Department, as postwar director of Lick, or promised him the post.

In September 1944 news of the planned big postwar telescope but not of the new director surfaced in the University of California's budget proposals. Moore, by then the interim wartime director, and Wright, seventy-two years old but still very much on the scene as a retired astronomer recalled to service, thought of it as an 85-inch or 90-inch telescope, the largest instrument that they believed could be built for the funds specified in Sproul's budget proposal. Mayall and the other young Lick astronomers and former graduate students now in Pasadena, all of whom longed to return to Mount Hamilton and who discussed frequently everything that happened there, believed that Moore and Wright were out of touch with the real needs of astronomy. Emboldened by the wartime emphasis on youth and on cutting through red tape to get results, Mayall resolved to go straight to the president of the university himself. He wrote Sproul, asking for an appointment to see him on one of his regular monthly visits to the University of California at Los Angeles campus. Kron also signed the letter, in which they said that, "as younger members of the [Lick] staff, who hope to use the instrument," they wished to discuss "what kind of telescope" the University of California should build when the war ended. Sproul welcomed them to his Los Angeles office in December 1944. Mayall did most of the talking. He emphasized the need for a telescope bigger than a 90-inch. In Pasadena he had seen the 120-inch glass disk originally intended for testing the

200-inch Palomar mirror, then nearly finished in the Caltech optical shop. He urged Sproul to make the Lick reflector that large. To Mayall and Kron's surprise, Sproul quickly assented and in turn urged them to keep up the pressure on "the old men."

In early 1945 Sproul appointed a committee, chaired by Shane, then on leave at Los Alamos as assistant director for scientific personnel, to plan the postwar Lick telescope. He also appointed Moore and Mayall to the committee, as well as Mount Wilson Observatory Director Walter S. Adams and Caltech physicist Ira S. Bowen, who would succeed Adams and become the first director of Mount Wilson and Palomar Observatories at the war's end. The committee worked mostly by correspondence, and Mayall's first letter helped to persuade Shane that it was reasonable to hope for a 120-inch rather than settle for a 90-inch, as he had earlier thought. Mayall, on the scene in Pasadena, was invaluable in providing liaison between the strong telescope group there and Shane, whose expertise was much more in teaching and university administration than in instrument design. Adams and John A. Anderson, executive officer of the 200-inch project, made their drawings, plans, and experience freely available to the California astronomers. Mayall was present at the one actual meeting of the committee, in Pasadena on March 6, 1945, when Shane could get away from Los Alamos briefly. On that day the committee made all the basic decisions for what eventually became the Lick 120-inch reflector. Shane and Mayall went on to Mount Hamilton the next day and there, with Moore (who had not been able to get to Pasadena) and Wright, picked out the spot where the telescope would be erected.

After the war Shane guided the 120-inch project through the university and helped Sproul sell it to the legislators and the governor. Caltech made its disk available at cost, which bypassed the delay that ordering a new one would have caused. Mayall, as the most experienced big telescope user on the Lick faculty, who often went to Mount Wilson and later even to Palomar to observe with Baade, Minkowski, and Humason, made many suggestions that were incorporated into the Lick reflector. It was safe, sound, conservative, and productive—his style exactly.

POSTWAR LICK RESEARCH

The 120-inch telescope was years in the building. Meanwhile, Mayall worked actively on research with the Crossley reflector. He began obtaining the integrated spectra of globular clusters with his fast spectrograph well before the war. Now he finished this work and published the results—the radial velocities of the first fifty clusters. The result was important in proving that the system of globular clusters shares only slightly in the galactic rotation exhibited by the flattened system of young stars and interstellar matter in the Milky Way. This work, like all of Mayall's, was very well suited to his small telescope and fast spectrograph, optimized for extended, low-surface-brightness nebulae, galaxies, and clusters.

Much of Mayall's best work was done in collaboration with or at the suggestion of his friends and mentors at Mount Wilson Observatory. Although he still idolized Hubble, who had been on leave as director of the Army's Ballistics Research Laboratory at Aberdeen, Maryland, for the duration of the war, by the time the 200-inch telescope went into operation in 1948 the great observational cosmologist was tired and ill. He suffered a heart attack in 1949, never fully recovered, and died in 1953. Baade had become Mayall's chief source of inspiration during World War II. He wrote frequently to his younger friend at Mount Hamilton, and they had long discussions at the informal Lick-Mount Wil son and Palomar nebular research conferences that Shane and Bowen arranged. With Baade's continued encouragement and advice, Mayall carried out a long, important program of spectroscopy of the H II regions in the spiral arms of M 31, to better define its rotation curve.

One very important paper Mayall published in observational cosmology, with Humason and the young Allan Sandage as coauthors, was a catalog of the Lick, Mount Wilson, and Palomar redshifts of galaxies. It contained the redshifts of more than 800 galaxies observed over the years from 1935 to 1955. Humason provided the data on the ellipticals and distant spirals for which the 100-inch and 200-inch telescopes and their spectrographs were so well suited, Mayall for the irregulars and nearer spirals, and Sandage provided most of the magnitudes. He was also chiefly responsible for the discussion in terms of the velocity-distance relationship. This paper enormously strengthened the observational evidence for a linear velocity-distance relationship. The value they determined for the Hubble constant, 180 km/sec/Mpc was a step along the way from the outstanding observational cosmologist's early value of 530 km/sec/Mpc to the currently accepted values of 50 to 100 km/sec/Mpc.

The great expert on spectral classification of stars, William W. Morgan, worked with Mayall and his collection of galaxy spectra during a visit to Lick. They published a joint paper on the results, a spectral classification of galaxies that showed many of the population and heavy-element abundance differences between spiral and elliptical galaxies, much later made quantitative by detailed CCD spectrophotometry. Mayall and Kron, again with the very active encouragement of Baade, collaborated on measuring the colors of globular clusters in our own Galaxy and in M 31 and its companions, for information on interstellar extinction and the stellar populations of these clusters.

From the end of World War II, Mayall was the editor of all the Lick Observatory scientific publications and hence a member of the editorial board of the *Astrophysical Journal*. He devoted considerable effort to this task and greatly improved the clarity and accuracy of presentation of several of his colleagues' papers.

As the 120-inch telescope approached completion on Mount Hamilton, Mayall was responsible for taking the test exposures that showed how close the primary mirror was to the correct form and what additional figuring was necessary to bring it to the final ideal paraboloid. On this project he worked with Stanislavs Vasilevskis, who measured the plates and reduced the numerical results to quantify the form of the mirror.

In 1958 with the telescope still not completed, Shane stepped down as director. Albert E. Whitford, his successor, was brought from the University of Wisconsin to finish the task and put the 120-inch into operation. The opticians finally figured the mirror correctly, as the test plate Mayall took on June 17, 1959, confirmed. Then the mirror could be aluminized and the auxiliary instruments installed. By early 1960 the 120-inch was in regular operation. Mayall began taking direct exposures of nebulae and galaxies at the prime focus, but only a few months later, in September 1960, he left Lick Observatory and Mount Hamilton.

KITT PEAK NATIONAL OBSERVATORY

Mayall left the University of California, where over a span of more than a quarter of a century he had been undergraduate, graduate student, and assistant and held every rank from assistant astronomer to astronomer, to become the second director of Kitt Peak National Observatory. The national observatory concept had only become a reality a few years before. Under the financial sponsorship of the

National Science Foundation, a group of universities organized a consortium, Associated Universities for Research in Astronomy, to build and operate a research observatory for all American astronomers to use. The first director, Aden B. Meinel, located the site, Kitt Peak, a 7,000-foot mountain near Tucson, Arizona. He selected and recruited the first staff members and built the first telescopes. But in the spring of 1960, as the Kitt Peak 84-inch reflector was completed and dedicated, the AURA Board of Directors decided that Meinel was not the person to manage it. He resigned, and the board named Mayall to succeed him. Shane, who represented the University of California on the AURA board and was its president at that time, played the major role in persuading him to take the job.

Mayall had never had any previous administrative experience, but he was an excellent choice for the post. Then fifty-four years old, he was ready for a change, and, after only a brief hesitation, he accepted the preferred appointment. He gave Kitt Peak instant credibility, in a way that Meinel, a postwar Ph.D., and the few young staff members he had assembled could not do. Several of the Lick, Mount Wilson, and Palomar astronomers, comfortable with the idea of an elite few having the largest telescopes in the world at their disposal, had scoffed at the concept of an observatory for everyone (although Bowen, Shane, and Whitford all supported the project strongly). But no one could scoff at Mayall, one of the most respected research astronomers in America. A member of the National Academy of Sciences since 1949 and chairman of its astronomy section, former president of the Astronomical Society of the Pacific, and president of the International Astronomical Union Commission on Extragalactic Nebulae, he clearly belonged. He could recruit new staff members who would come firm in the knowledge that he would be there for years and that the national observatory was there to stay.

In his early years Mayall received frequent advice from Shane, but he quickly picked up the skills needed to direct Kitt Peak National Observatory. He was particularly effective in handling its external relations, with NSF administrators, university vice-presidents and business managers, and Arizona and government officials. The 1960s were the post-sputnik era in American science. The country was prosperous and eager to support research. Mayall saw to it that Kitt Peak got its share of the available funding. He knew well all the astronomers who represented their various universities on the AURA board and could work effectively with them.

Mayall delegated almost all the responsibility for designing new instruments and operating the telescopes to the younger staff astronomers. Some met the challenge; others did not. As the staff grew, Mayall brought in some first-rate scientists who were willing to give their time and effort to make it possible for short-term visitors from all over the country to get important scientific results. Everyone who worked under Mayall at Kitt Peak considered him kind and gentle, and some thought that he was a little too gentle—with others. One administrator, not a scientist, was a continual source of problems, but he was useful, too, and Mayall never got rid of him.

As director, Mayall presided over the building of the 4-meter reflector, Kitt Peak's largest telescope. It was a huge team-engineering project that had been planned even before he came on board. Some of his former Lick colleagues and students were surprised and somewhat disappointed that he never began a research program of his own with the big Kitt Peak reflector, but he felt that he had too many other responsibilities that had to come first.

Mayall was much more personally involved in the expan sion of the national observatory to the southern hemisphere, in the Chile project that eventually became Cerro Tololo Interamerican Observatory. He and Shane went to Chile two months after he accepted the directorship and scouted the prospective sites. Mayall reported that he favored the one that was subsequently chosen, on Cerro Tololo. He strongly believed in the southern hemisphere observatory, as he demonstrated by his frequent trips to Chile and almost daily radio and telex contacts with the CTIO director, Victor Blanco. Mayall helped it grow, and its 4-meter reflector was well under way when he retired in 1971, at the age of sixty-five.

Mayall's retirement was marked by a scientific symposium, held in Tucson, at which Morgan, Minkowski, Sandage, and Margaret Burbidge were the invited speakers. He and his wife remained in Tucson, and, when the Kitt Peak 4-meter reflector was completed and dedicated in 1973, it was named the Mayall telescope for him. He was present for its "first light" on February 27 of that year. The telescope was in full operation by 1974, as was the Cerro Tololo 4-meter telescope the following year. Mayall had lived to see his work bear fruit. In retirement he corresponded frequently with Shane and Frank K. Edmondson, the long-time AURA representative of Indiana University who was one of the strongest early proponents of the cooperative or national observatory concept. Mayall kept in touch with the observatory and his many friends on its staff. He had suffered from diabetes for thirty years and died at his home in Tucson on January 5, 1993.

In summary, Mayall was an outstanding observational astronomer. At Lick Observatory he made many contributions to our knowledge of gaseous nebulae, supernovae, the motions within spiral galaxies, and the redshifts of the galaxies in the universe. In eleven years as director of Kitt

Peak National Observatory, he built it and Cerro Tololo Interamerican Observatory into first-rate research observatories, with world-class telescopes. Throughout his career he remained a kind, considerate person who was respected and admired by all who worked for him.

THIS BIOGRAPHICAL MEMOIR IS based largely on the written record of Mayall's research, given in his published scientific work, and of his other accomplishments and his life, drawn from hundreds of letters to, from, and about him in the Mary Lea Shane Archives of the Lick Observatory, going back to the 1924 letter that he wrote as secretary of the Stockton High School science club. These include a great many letters from his own personal scientific correspondence, which he presented to the archives. Some of the material on his student days is from his autobiographical chapter in a University of California centennial volume.[1] I knew Mayall personally since 1957, had many conversations with him in his later years about his scientific life, and interviewed him extensively in 1987, just before the Lick Observatory centennial. In addition, I received letters and messages from many of Mayall's former colleagues at Lick, Kitt Peak, and the wartime Caltech project, giving their reminiscences of him. I am greatly indebted to all of them, as I am to Kay and Bruce Mayall, who kindly provided additional information, particularly on this great astronomer's early life.

NOTE

1. Nicholas U. Mayall. In *There Was Light, Autobiography of a University: Berkeley: 1868-1968*, ed. I. Stone, pp. 107-19. Garden City: Doubleday & Co., 1970.

Selected Bibliography

1928 With H. G. Miles and F. L. Whipple. Elements and ephemeris of comet *k* 1927 (Skjellerup). *Lick Obs. Bull.* 13:120-22.

1930 With S. B. Nicholson. The probable value of the mass of Pluto. *Publ. Astron. Soc. Pac.* 42:350-51.

1931 With S. B. Nicholson. Positions, orbit, and mass of Pluto. *Astrophys. J.* 73:1-12.

Recent novae in the great spiral nebula in Andromeda (M 31). *Publ. Astron. Soc. Pac.* 43:217-20.

1934 A study of the distribution of extra-galactic nebulae based on plates taken with the Crossley reflector. *Lick Obs. Bull.* 16:177-98. The spectrum of the spiral nebula NGC 4151. *Publ. Astron. Soc. Pac.* 46:134-38.

1935 An extra-galactic object three degrees from the plane of the galaxy. *Publ. Astron. Soc. Pac.* 47:317-18.

1936 A low dispersion UV glass spectrograph for the Crossley reflector. *Publ. Astron. Soc. Pac.* 48:14-18.

1937 The spectrum of the Crab nebula in Taurus. *Publ. Astron. Soc. Pac.* 49:101-5.

1939 The Crab nebula, a probable supernova. *Astron. Soc. Pac. Leaflet* 3:145-54.

With L. H. Aller. Emission nebulosities in the spiral nebula Messier 33. *Publ. Astron. Soc. Pac.* 51:112-14.

The occurrence of 3727 [O II] in the spectra of extragalactic nebulae. *Lick Obs. Bull.* 19:33-39.

1940 With L. H. Aller. The rotation of the spiral nebula Messier 33. *Publ. Astron. Soc. Pac.* 52:278.

With J. H. Moore and J. F. Chappell. *Astronomical Photographs Taken at the Lick Observatory*. Mount Hamilton: Lick Observatory.

1941 With A. B. Wyse. Increased speed of two Lick Observatory spectrographs treated with non-reflecting films. *Publ. Astron. Soc. Pac.* 53:120-22.

The radial velocity of IC 10. *Publ. Astron. Soc. Pac.* 53:122-24.

With E. Hubble. Direction of rotation of spiral nebulae. *Science* 93:434.

1942 With L. H. Aller. The rotation of the spiral nebula Messier 33. *Astrophys. J.* 95:5-23.

With A. B. Wyse. Distribution of mass in the spiral nebulae Messier 31 and Messier 33. *Astrophys. J.* 95:24-43.

With J. H. Oort. Further data bearing on the identification of the Crab nebula with the supernova of 1054 A.D. Part II. The astronomical aspects. *Publ. Astron. Soc. Pac.* 54:95-104.

1946 The radial velocities of fifty globular star clusters. *Astrophys. J.* 104:290-323.

1951 With W. Baade. Distribution and motions of gaseous masses in spirals. In *Problems of Cosmical Aerodynamics: Proceedings of the Symposium on the Motion of Gaseous Masses of Cosmical Dimensions Held at Paris, August 16-19, 1949*, pp. 165-84. Dayton: Central Air Documents Office.

Comparison of rotational motions observed in the spirals M 31 and M 33 and in the Galaxy. *Publ. Obs. Univ. Michigan* 10:19-24.

1956 With M. L. Humason and A. R. Sandage. Redshifts and magnitudes of extragalactic nebulae. *Astron. J.* 61:97-162.

1957 With W. W. Morgan. A spectral classification of galaxies. *Publ. Astron. Soc. Pac.* 69:291-303.

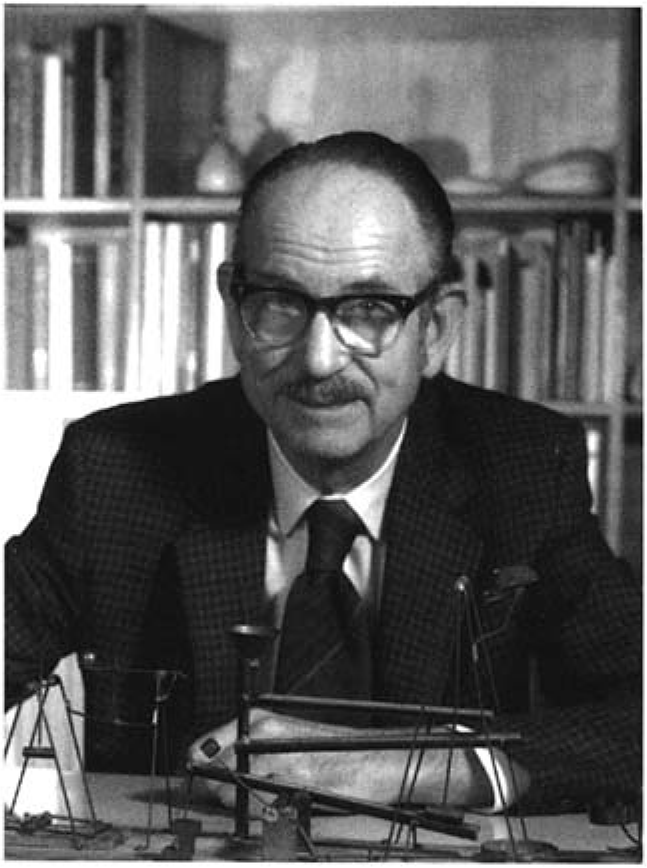
1960 With S. Vasilevskis. Quantitative tests of the Lick Observatory 120-inch mirror. *Astron. J.* 65:304-17.

With G. E. Kron. Photoelectric photometry of galactic and extragalactic star clusters. *Astron. J.* 65:581-620.

1962 The story of the Crab nebula. *Science* 137:91-102.

With A. de Vaucouleurs. Redshifts of 92 galaxies. *Astron. J.* 67:363-69.

1970 With P.-O. Lindblad. Mean rotational velocities of 56 galaxies. *Astron. Astrophys.* 8:364-74.



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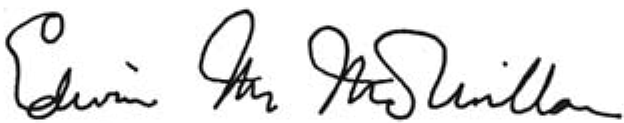


Edwin Mattison McMillan

September 18, 1907–September 8, 1991

J. DAVID JACKSON AND W. K. H. PANOFSKY

WITH THE DEATH OF Edwin Mattison McMillan on September 8, 1991, the world lost one of its great natural scientists. We advisedly use the term "natural scientist" since McMillan's interests transcended greatly that of his profession of physicist. They encompassed everything natural from rocks through elementary particles to pure mathematics and included an insatiable appetite for understanding everything from fundamental principles.

Edwin McMillan spent a large part of his professional life in close association with Ernest O. Lawrence[1] and succeeded Lawrence as director of what is now the Lawrence Berkeley Laboratory in 1958. Yet the two men could hardly be more different. Lawrence was a man of great intuition, outgoing, and a highly capable organizer of the work of many people. Edwin McMillan was thoroughly analytical in whatever he did and usually worked alone or with few associates. He disliked specialization and the division of physics divided into theory and experiment. He remarked at an international high-energy physics meeting, "Any experimentalist, unless proven a damn fool, should be given one half year to interpret his own experiment."

McMillan's first and last publications illustrate the unusual breadth of his interests. While still an undergraduate student in 1927, he published a paper[2] on the x-ray study of alloys of lead and thallium, clearly a topic in chemistry. At the time, he took many more courses in chemistry than was customary for a physics major, and this publication was undertaken at the suggestion of Linus Pauling. His last paper,[3] written together with the mathematician Richard P. Brent, was on an improved algorithm for computing Euler's constant: the limit of the difference between the sum of the inverse integers from 1 to *n* and the natural logarithm of *n*, as *n* ` ` .

One of us (J.D.J.) recalls an incident that illustrates Ed McMillan's range in science. When Jackson corresponded at the beginning of 1957 with Luis Alvarez and his colleagues about muon-catalyzed fusion, he was startled to receive facsimile copies of handwritten notes by McMillan on a calculation of the mu-mesic molecular formation process! At that time, he knew McMillan's name as the discoverer of neptunium, the codiscoverer of plutonium, and the inventor of phase stability in accelerators but never dreamt that he was a molecular theorist! At the time, Ed was busy as associate director under Lawrence. His molecular physics Ph.D. thesis research with Condon could be the origin of such expertise, but with McMillan it could just as easily be knowledge acquired for the fun of it.

The son of Edwin H. McMillan and Anna Maria Mattison, Edwin M. McMillan was born on September 18, 1907, in Redondo Beach, California; both parents were Scots. He was brought up in Pasadena, California, beyond age one and a half. His father was a physician, as were the parents of his wife Elsie McMillan (born Blumer), who incidentally is the sister of E. O. Lawrence's wife, Molly. McMillan is survived by his wife and their three children (Ann Bradford Chaikin, David Mattison McMillan, and Stephen Walker McMillan). They were a wonderful and harmonious family.

As a child, McMillan built gadgets and made use of the proximity of the California Institute of Technology in attending lectures and seminars and getting acquainted with physicists there. After high school McMillan entered Caltech, where he had a first-rate academic record leading to both the B.S. and M.S. degrees. He completed his work leading to the Ph.D. at Princeton University in 1932.

McMillan's work can be separated into five phases that exhibit a great deal of overlap—not surprising considering the universality of McMillan's interests: (1) the early prewar period; (2) studies of the transuranic elements; (3) military work during World War II; (4) accelerator physics; and (5) laboratory director. These phases were paralleled by work on advisory committees and other roles as a statesman of science.

THE EARLY PREWAR PERIOD

McMillan's Ph.D. thesis, under Professor E. U. Condon, examined the generation of a molecular beam of hydrogen-chloride nuclei in a nonhomogeneous electric field.[4] In parallel, McMillan received a thorough education in experimental nuclear physics at Princeton. He published a paper [5] on the isotopic composition of lithium in the sun from spectroscopic observations immediately after receiving his Ph.D. He then won a highly prized National Research Council (NRC) fellowship, supporting him at any university of his choice.

He accepted the invitation of E. O. Lawrence to come to Berkeley, where Lawrence was at the time engaged in exploring the experimental potential of the cyclotron. After McMillan accepted Lawrence's invitation, he dedicated his first two years to activities somewhat separate from the mainstream activities of Lawrence's new Radiation Laboratory. He intended to measure the magnetic moment of the pro ton, but that plan came to naught when Otto Stern and collaborators in Germany did the measurement. He continued to work on hyperfine structure as revealed in optical spectroscopy and published papers on the nuclear magnetic moment of tantalum[6] as well as on the hyperfine structure of the solar spectrum.[7] But McMillan became progressively more involved with the work on Lawrence's cyclotron, which by early 1934 could produce a deflected beam of 2.3-MeV deuterons. His experimental skill was recognized by Lawrence and his collaborators and was put to increasing use on both the cyclotron and its instrumentation and physical experiments with the beam.

McMillan used the extracted deuteron beam in collaboration with M. Stanley Livingston to irradiate nitrogen to produce the positron emitting ^{15}O. Again, McMillan's skill as a chemist was put to work. He used a tracer technique in which first nitrogen gas was bombarded and then mixed with oxygen and an excess of hydrogen. This mixture was catalyzed to water over heated platinized asbestos, and the water was collected on anhydrous calcium chloride. The radioactivity was shown to be localized in the calcium chloride and absent elsewhere, proving that oxygen carried the activity.[8]

This work was followed by fundamental studies on the absorption of gamma rays,[9] which revealed the (at that time new) process of electromagnetic pair production in the Coulomb field of a nucleus. The 5.4-MeV gamma ray produced by bombardment of fluorine with protons and also the gamma rays of other isotopes were absorbed by foils of aluminum, copper, tin, and lead, enabling McMillan to isolate the components of the absorption process. At 5.4 MeV, electron-positron pair production is about one-half the total absorption cross-section in lead.

In 1935, with Lawrence and R. L. Thornton, McMillan studied the radioactivity produced when a variety of targets are exposed to a deuteron beam.[10] At deuteron energies below 2 MeV, the activity increases rapidly with energy, as expected from the quantum mechanical penetration of the Coulomb barrier, first used to explain alpha radioactivity lifetimes by George Gamow. The experiments of McMillan and co-workers on (d,p) reactions with energies up to 3.4 MeV showed that the yield curves flattened above 2 MeV, even though the Coulomb barrier effects were expected to be considerably steeper from conventional estimates of the effective nuclear radii. A deuteron seemed to be able to have its neutron captured by the target nucleus while its proton remained relatively far away. These data intrigued J. Robert Oppenheimer and his student, Melba Phillips, who then developed the theoretical explanation of the phenomenon: the small binding energy, and therefore large size, of the deuteron permits it to be polarized in the nuclear Coulomb field; this polarization places the neutron within the deuteron close to the nucleus, accessible for capture, while the proton is away from it. In essence, the proton becomes a "spectator" of the process. The Oppenheimer-Phillips process gives a quantitative explanation of the energy independence of the yield curves and the predominance of the (d,p) reactions in deuteron bombardments.

Following this work McMillan investigated the properties of ^{10}Be, with its extraordinarily long half-life for a light element (approximately 2.5 million years). He pursued further details of the properties of ^{10}Be in later publications.[11] During that period McMillan did several additional experiments in what today has become nuclear chemistry, some of them successful and some unsuccessful. At the same period, he wrote a seminal paper[12] on the production of X rays by the acceleration of very fast electrons, a subject in which he maintained a lifelong interest.

McMillan made numerous experimental contributions to the cyclotron, in particular to its beam-focusing properties, to beam extraction, and to vacuum gauges. His deep understanding of the factors that limit the energy attainable by conventional cyclotrons is illustrated by his correspondence in late 1937 and early 1938 with Hans Bethe. Bethe had worked with M. E. Rose at Cornell on the energy limit problem, and McMillan was carrying out calculations at Berkeley with Robert R. Wilson developing orbit-tracing methods. In 1937 Bethe sent an advance copy of the Bethe-Rose paper to McMillan. McMillan found some errors in the paper and showed that the electrostatic defocusing effect of the cyclotron dee's could be counteracted by the insertion of grids. McMillan also understood clearly the focusing effect of the radial fall-off of the magnetic field and the magnitude of the deviation from the synchronicity condition in the cyclotron produced by that radial fall-off, added to the relativistic mass increase. Bethe suggested that McMillan publish his findings, but characteristically McMillan felt that an additional paper would be redundant. The correspondence demonstrates McMillan's deep quantitative mastery of the subject while at the same time exhibiting his basic humility. He preferred making an input to the Bethe-Rose paper over cluttering up the literature with controversy.

STUDIES ON TRANSURANIC ELEMENTS

The discovery of fission of uranium by Hahn and Strassmann in 1939 initiated intense activity worldwide. At Berkeley McMillan first performed a simple experiment to measure the ranges of the energetic fission fragments by exposing a thin layer of uranium oxide on paper sandwiched between several thin aluminum foils on either side to the neutrons from 8-MeV deuterons striking a beryllium target in the 37-inch cyclotron. The amounts of radioactivity in successive foils established the maximum range of the fragments as equivalent to approximately 2.2 centimeters in air. He also used cigarette papers instead of the aluminum foils in another sandwich and followed the radioactivity in different papers after bombardment, finding the same time dependence in all. In contrast, the activity associated with the layer of paper on which the uranium oxide had been placed had different components. In addition to the fission fragment activity, there was one component with a twenty-five-minute half-life and another of roughly two days. McMillan speculated that the twenty-five-minute activity was ^{239}U, identified earlier by Hahn and co-workers as a product of resonant neutron capture in uranium.[13]

The two-day nonrecoiling activity intrigued McMillan. Accordingly, he bombarded thin ammonium uranate layers deposited on a bakelite substrate and covered with cellophane (to catch the energetic fission fragments). After exposure to the neutrons, the ammonium uranate was scraped off the bakelite and its activity followed. At long times the 2.3-day activity was dominant; at short times, the twenty-three-minute half-life of ^{239}U predominated. In contrast, the cellophane showed the characteristic power law decay associated with a mixture of fission fragments of different lifetimes. With the new activity physically separated, it was possible to begin study of its chemical properties. As a putative new element next to uranium, the activity seemed likely to have chemical properties akin to rhenium. McMillan therefore enlisted Emilio Segrè, who was familiar with the chemistry of rhenium from his discovery of a homolog, technetium, in 1937. Segrè found that the 2.3-day activity behaved like a rare earth, not like rhenium. Since rare earths are prominent among the fission fragments, it appeared that the 2.3-day activity was one of those. After a gap in his pursuit, McMillan had become persuaded by early 1940 that the nonrecoiling 2.3-day activity just could not be the decay of a fission fragment. He began a set of experiments with the new 60-inch cyclotron and its 16-MeV deuterons. Two observations confirmed his belief as a certainty. One, using cadmium absorbers to reduce the thermal neutrons, showed greatly reduced fission activity but left the two nonrecoiling activities in the same relative proportion. The other, a fission product experiment with extremely thin collodion catcher foils, showed that the range of the 2.3-day "fragments" was less than 0.1 millimeter of air equivalent. The 2.3-day activity could not be from fission; the twenty-three-minute and 2.3-day activities almost certainly were genetically related. The beta decay of ^{239}U was producing atoms of a new element with Z = 93! McMillan found chemically that the 2.3-day activity had some, but not all, the characteristics of a rare earth.

Philip H. Abelson was a student at Berkeley in 1939, working on the chemistry of fission products and was familiar with McMillan's first observations of the 2.3-day activity. In 1939-40 at the Carnegie Institution in Washington, D.C., Abelson attempted (unknown to McMillan) to separate the 2.3-day activity, initially with rare-earth chemistry, but found his procedures inadequate. In May 1940, as McMillan was doing his chemistry, Abelson came to Berkeley and they began a collaboration. The key to successful chemistry, as Abelson found, was control of the state of oxidation of the material. In the reduced state the activity coprecipitates with rare-earth fluorides; when in an oxidized state it does not. In fact, the oxidized state behaves similarly to uranium, coprecipitating with sodium uranyl acetate. On the other hand, uranium does not precipitate in an HF solution with SO_2, while the 2.3-day activity coprecipitates with rare-earth carriers. Abelson and McMillan were thus able to use an "oxidation-reduction cycle" to make a series of precipita tions of the 2.3-day activity from a uranyl solution and establish its growth from the twenty-three-minute ^{239}U, thus proving it to be an isotope of element 93. They searched for alpha activity associated with the decay product of the 2.3-day isotope (an isotope of element 94) and noted that it must be long-lived. The work was submitted to the *Physical Review* on May 27, 1940.[14] The technique of an oxidation-reduction cycle formed the basis of all the transuranic chemistry to follow.

After Abelson's return to Washington, McMillan turned to the search for the alpha activity of the daughter of ^{239}Np (as we now denote it). Strong samples of the 2.3-day activity did show some alpha particle emission, distinguished from possible natural uranium activity by greater range. With the hope of producing a different isotope of neptunium and so its decay product, McMillan bombarded a uranium target directly with 16-MeV deuterons. A two-day beta activity, with more energetic beta particles than the earlier 2.3-day decay, was observed, along with a considerably more intense 5-MeV alpha activity (now known to be from ^{238}Pu; ninety-two-year half-life). He tried to separate the alpha activity chemically, eliminating protactinium, uranium, and neptunium as species, while showing that it behaved similarly to thorium and 4-valent uranium.

In November 1940 McMillan left Berkeley for military work at MIT. Glenn T. Seaborg, who, with colleague J. W. Kennedy and graduate student A. C. Wahl, had perfected the oxidation-reduction technique for isolating neptunium, wrote to McMillan to say that they would "be very glad to carry on in his absence as his collaborators" in the search for element 94.[15] McMillan replied (in Seaborg's words), "informing me that he will not be back soon in Berkeley and it would please him very much if I continue to work on elements 93 and 94."[16] McMillan's letter explicated his own findings on the physical and chemical characteristics of the various activities.

Following McMillan's lead, by late February 1941 Seaborg, Kennedy, and Wahl had made definite the discovery of the ninety-two-year isotope of element 94 (^{238}Pu). A short paper on the joint work with McMillan was submitted to the *Physical Review* on January 28, 1941 (before the final proof of separation from thorium had been made) but was voluntarily withheld from publication until 1946.[17]

For his discovery of neptunium with Abelson and of plutonium with Kennedy, Seaborg, and Wahl, McMillan shared with Seaborg the Nobel Prize in chemistry in 1951.

MILITARY WORK DURING WORLD WAR II

McMillan's first assignment at MIT in November 1940 was work on airborne microwave radar at the newly established MIT Radiation Laboratory. The work initially capitalized on his technical and physical ingenuity, but when emphasis shifted from individual invention to collaborative engineering, McMillan moved to the U.S. Navy Radar and Sound Laboratory in San Diego in 1941. There he invented and developed a repeater for underwater echoes that greatly extended the detection range of undersea warfare devices. He was then recruited by J. Robert Oppenheimer, who had been appointed director of the Los Alamos weapons laboratory to be and served as his principal adviser on practical technical issues, starting in the fall of 1942.

McMillan's nuclear weapons work started with the site selection of Los Alamos. He then led the development of the gun-type weapon, a device in which ^{235}U bodies are fired at one another with a gun to constitute a critical assembly. A requirement for such a device to work meant the development at a separate site near Los Alamos of gun barrels of lower weight to propel objects at higher speed than was previously considered feasible. The work then continued with McMillan serving as deputy to William S. (Deak) Parsons, the naval officer who was then in charge of all conventional explosive work at Los Alamos. McMillan's work proceeded until it was established that the gun device would work; he did not participate in the actual "weaponization." The Hiroshima weapon was based on these developments without a nuclear test. The rest is history.

Oppenheimer asked McMillan to undertake a large number of additional responsibilities. One was to serve as the liaison officer between Los Alamos and the California Institute of Technology project known as CAMEL, which among other activities tested the aerodynamic properties of air-dropped bombs, with McMillan in charge. Another experimental responsibility was development of diagnostics of the implosion assembly for the plutonium bomb, using a magnetic detector. McMillan was an observer during the Trinity Test when the first implosion device was detonated.

He and his wife Elsie were mainstays in the evolution of social life in Los Alamos with all its joys and heartbreaks.[18]

ACCELERATOR PHYSICS

By the middle of 1945 many scientists at Los Alamos, including McMillan, were making plans to return home. For the Berkeley physicists, this included planning new accelerator facilities. Before the beginning of the war Lawrence had started to construct a huge conventional cyclotron. It had a pole-face diameter of 184 inches and a magnet gap of 5 feet. McMillan had designed some power supplies for that machine. That large magnet gap was needed because the conventional cyclotron required dee voltages in excess of 1 million volts to reach energies close to 100 MeV. This voltage required very large clearances between the dee and the vacuum chamber walls. Acceleration had to be accom plished during very few turns in order to keep the particles in step with the accelerating rf voltage even with their relativistic increase in mass.

McMillan was fully acquainted with this situation, but he disliked pursuing the plan for completing the 184-inch cyclotron. In mulling over this problem, McMillan, in June 1945, envisioned the idea of phase stability, which in a single stroke of invention made this brute force approach obsolete. McMillan recognized that when particles are accelerated in a radiofrequency field not at the crest of the radiofrequency amplitude but on the side of the waveform, the particles would be locked stably at a certain phase. The idea had great generality and applied to many types of accelerators, including circular heavy particle and electron machines and heavy particle linear accelerators. For circular accelerators using magnetic fields uniform in azimuth, the phase stability region is during the *decreasing* part of the radiofrequency amplitude. If a particle has less than the normal energy, it is bent into a tighter circle in a circular accelerator and thus takes less time to complete its orbit. Such a particle thus arrives earlier at the next period and therefore is exposed to a higher accelerating field during the decreasing part of the rf amplitude. It therefore receives a larger energy increase. Conversely, a particle above average energy receives less acceleration. In consequence, the particles execute "phase oscillations" about a stable phase angle determined by the ratio of the peak acceleration made possible by the rf amplitude and the actual, lesser, acceleration required by the specific accelerator design.[19] McMillan expressed these facts in differential equations describing a stable "bucket" with particles oscillating about a synchronous phase within the bucket at a frequency defined by the accelerator parameters.

McMillan, in his discussions at Los Alamos, fully recog nized the generality of this new principle and its wide range of application. He published[20] his discovery in the *Physical Review* in September 1945. After publication McMillan learned[21] that the Russian physicist Vladimir I. Veksler had conceived the same idea and had published it previously in a Russian journal that had not reached the United States during wartime. There followed an exchange of letters between Veksler and McMillan that will remain an example of gracious interaction between scientists. McMillan acknowledged [22] the priority in time of Veksler's invention. Both parties agreed that their respective inspirations were indeed independent and that the idea of phase stability would inevitably have surfaced.

In McMillan's words, "It seems to me that this is another case of a phenomenon that has occurred before in science—the nearly simultaneous appearance of an idea in several parts of the world, when the development of the science concerned has reached such a point that the idea is needed for its further progress." And in Veksler's words, "You are quite justified in saying that the history of science affords many examples of the simultaneous appearance of similar ideas in several parts of the world, as in our own case." The two physicists became friends and mutual admirers. They shared the Atoms for Peace Prize for the invention of phase stability in 1963.

The concept of phase stability revolutionized accelerator design and construction throughout the world. It led to proposals for new accelerators in France and at the new European laboratory at CERN, in the United Kingdom, and in Australia, and it led to vigorous initiatives in Russia and the United States.

The original plans for the "classical" 184-inch cyclotron were scrapped. The magnet was modified to produce a larger magnetic field over a smaller gap. This conversion made it into a "synchro-cyclotron." Here the principle of phase stability was used together with frequency modulation (provided by a rotating capacitor) of the rf accelerating field, needed to compensate for the relativistic change in orbital frequency. The ions, injected at the center of the synchro-cyclotron magnet, are locked at stable phases in many orbits of increasing radius as they gain energy. Since synchronization is guaranteed by phase stability, acceleration can occur stably over many turns. Lower dee voltages are therefore sufficient, and a smaller gap and a higher magnetic field can be utilized.

A model was constructed in record time in the small 37-inch cyclotron on the Berkeley campus. The success of this model led to full-speed conversion of the 184-inch machine by 1948. That machine supported an impressive series of discoveries, including many important experiments on the first man-made pi-mesons. McMillan himself participated in the mapping of the neutron beam produced by high-energy deuterons on internal targets and was an advisory participant in innumerable experiments. However, his primary interest shifted to another application of phase stability, a 300-MeV electron synchrotron that became his responsibility for both construction and research supported by Lawrence and the Atomic Energy Commission.

Prior to the invention of phase stability, the highest energy reached by an electron accelerator was achieved with the betatron with its energy limit—about 100 MeV—set by that emission of electromagnetic radiation by the electrons.

In McMillan's machine the electrons were confined to an annular chamber and accelerated in the traditional betatron manner to about 2 MeV. Subsequent phase-stable gain in energy was produced by the electric field of an electromagnetic cavity as the guiding magnetic field is raised. McMillan's machine had a radius of 1 meter and attained an energy of 300 MeV. McMillan personally directed the building of all phases of this pioneering machine and contributed engineering ideas. There were technical problems with the vacuum chamber exposed to electromagnetic radiation; the magnets had to be designed for proper focusing and for control of eddy current effects; special power supplies involving high-current switching had to be built to control the time sequence of the magnets; the rf system had to be engineered.

Nevertheless, the job was done and the machine, like the 184-inch cyclotron, yielded important new discoveries. McMillan personally participated in the first experiments of production of pions by photons.[23] Many other experiments were done, including demonstration of the existence of the neutral pion and detailed studies of the high-energy electromagnetic cascades. The 300-MeV electron synchrotron gave McMillan, for the first time, the opportunity to direct all phases of an accelerator laboratory; he was designer and builder of the synchrotron and also manager of the scientific program associated with that novel tool, which could not have been built prior to the invention of phase stability.

The success of the 184-inch synchro-cyclotron and 300-MeV electron synchrotron provided the impetus for the next stage of accelerator building at Berkeley—the Bevatron. McMillan contributed to the initial concepts of the design of that machine, including the calculations that showed the machine should reach 6 GeV comfortably to produce proton-antiproton pairs. Construction was in the hands of William Brobeck, a highly capable engineer long associated with Lawrence and McMillan.

Today, essentially all high-energy accelerators, be they for electrons, protons, or heavy ions, could not operate unless they were "phase stable." The explosive development of high-energy accelerators, which led to an increase in obtainable energy by roughly a factor of ten per decade, is largely a consequence of the invention of McMillan and Veksler.

McMillan made other significant contributions to accelerator physics. He published[24] the "McMillan theorem," a mathematical proof that in a linear accelerator radial focusing and phase stability are mutually incompatible unless external focusing devices (magnet lenses or grids) are applied to the beam. He also carried out calculations on the spin motion in electron linear accelerators, and during a sabbatical visit to CERN in 1975 he traced the puzzling loss of muons in a storage ring to minute machining irregularities in the magnet pole faces. He contributed extensively to the analysis of orbit dynamics at the Berkeley laboratory.

LABORATORY DIRECTOR

While in the years after 1945 McMillan's research focused on the design and construction of accelerators at the Radiation Laboratory, his interest in other sciences remained acute. He was a faculty member in the Department of Physics, University of California at Berkeley, engaged in regular undergraduate and graduate teaching in the period 1946-54 and supervision of fifteen graduate students to the Ph.D. His classroom teaching ended with his appointment as associate director of the Radiation Laboratory (1954-58), becoming deputy director and, later that year after Lawrence's death in August 1958, director of the renamed Lawrence Radiation Laboratory.

McMillan served for fifteen years (1958-73) as director of the Lawrence Radiation Laboratory and, after separation of the Berkeley and Livermore components in 1970, the Lawrence Berkeley Laboratory (LBL). In 1958 the laboratory already had 2,000 employees in Berkeley and about

3,300 at Livermore. The Berkeley part was multidisciplinary, with the major focus on physics, with numerous accelerators, but also had divisions of nuclear chemistry, biology and medicine, and bioorganic chemistry. The vigorous particle physics research program at the Bevatron, with the 72-inch bubble chamber and a variety of electronic particle detectors, drew physicists from around the world and made the Berkeley laboratory the center of high-energy physics from the late 1950s to the mid-1960s. Work with the 184-inch cyclotron and McMillan's 300-MeV synchrotron remained active.

The first half of McMillan's tenure as director was perhaps the high point of LBL, at least in high-energy physics. The latter part of his term saw changes, both in the scientific effort at the laboratory and in its funding from Washington. By the early 1960s accelerators elsewhere achieved higher energies, and so the particle energy frontier began to move away from Berkeley. To McMillan higher-energy facilities were desirable and inevitable. In fact, he played an important role in the creation of Fermilab, serving on the board of the Universities Research Association in its formative years.

McMillan provided scientific and administrative leadership to the laboratory in increasingly complex times, with particle physics funding leveling off and Livermore beginning to dwarf Berkeley.[25] Maintaining a strong and diverse research program in physics and the other fields with limited resources was difficult. His tendency was to let the heads of the scientific divisions have free rein, but he did not hesitate to arbitrate conflicting views and set the laboratory's course when necessary. He was successful in maintaining a strong multidisciplinary laboratory, with growth in new fields such as energy conservation and the environment as older programs leveled off.

In the later years, the "Rad Lab" suffered internal and external stresses: internal, as some researchers disagreed on priorities among existing activities and clamored for scarce research dollars for alternative projects less firmly connected to the laboratory's mission; external, as the partnership between the laboratory and the Atomic Energy Commission (ERDA after 1974) and the U.S. Congress began to erode. Moreover, the Vietnam war raised tensions, particularly on university campuses.

Lawrence had run the Radiation Laboratory from the beginning as a personal empire, and this benevolent stewardship from the top continued under McMillan, although he did not enjoy the exercise of power.

By the late 1960s, protesters against the Vietnam war and the military-industrial complex had tarred the Radiation Laboratory as a "bomb factory" and worse. The distinction between Livermore and Berkeley, while fully understood within the scientific community, was lost on the average person. The proximity of the Berkeley part of the Radiation Laboratory to the Berkeley campus made it an easy target for abuse. Within the laboratory tensions were rising, fueled by some faculty and graduate students who thought that the war was a legitimate topic for noontime discussion within the laboratory and members of the lab staff who did not. The issue hinged largely on conflicting views of the laboratory: a part of the academic campus, where free speech should prevail, or a governmental research enterprise, where politics was inappropriate.

Attempts to hold open meetings to discuss the Vietnam war were initially met with heavy-handed prohibition and discipline. Soon, however, McMillan saw that the protesters were sincere and responsible opponents of the war but not of the Berkeley Laboratory. In his quiet, cautious way, he addressed the perceived lack of academic freedoms at the laboratory. In the spring of 1971 he appointed an ad hoc committee of staff and faculty to draw up rules for independent open meetings at the laboratory. He promulgated these rules in September 1971, but the general counsel of the regents of the University of California promptly demanded that the rules be withdrawn. McMillan dug in his heels because he knew that the committee had transmitted all earlier drafts of its proposed rules to the general counsel for review. McMillan and the committee rejected most of the criticisms as trivial, made a few cosmetic changes, and left the rules in place to see them serve a useful purpose, without adverse consequences. McMillan did not like conflict, but he held strong principles. When he saw that something was fair and reasonable, he stuck to it over all objections.

Another example of McMillan's clear vision was the decision to separate Livermore from Berkeley. The turmoil in the country at large over the Vietnam war, the antimilitary sentiments, and the perceived security issues argued for separation. Voices at Livermore urged separation; voices at Berkeley urged the status quo—both for the same reason, money. The Livermore voices believed that the Berkeley side was riding the Livermore juggernaut; the Berkeley voices feared loss of support with separation. McMillan recommended separation and so became director of the smaller Lawrence Berkeley Laboratory. Funding did change, but not because of the separation and not for the worse. The subsequent profound changes in the Lawrence Berkeley Laboratory, with particle physics playing an ever-decreasing role, occurred under subsequent directors. McMillan stepped down as laboratory director at the end of 1973 and retired from the Berkeley faculty in June 1974. He continued to participate in the laboratory's work until he suffered a series of disabling strokes in 1984.

CONCLUSION

The above account of the five major phases of McMillan's contributions falls far short of describing his total contributions as a scholar, teacher, and human being. McMillan was an excellent teacher both inside and outside the classroom. His formal courses were extremely well received, with their clarity and total absence of preaching from on high. He instilled in his students an appreciation of physics in its fundamental aspects. He loved to explain scientific facts as well as gadgets to younger audiences, with his effectiveness resting entirely on deep knowledge combined with an absence of showmanship.

McMillan served on the then General Advisory Committee to the Atomic Energy Commission from 1954 to 1958 and participated as a member of scientific policy committees and program advisory committees to several laboratories. In committees McMillan tended to be relatively taciturn, but when he spoke up his remarks were decisive and to the point. When President Eisenhower in 1959 announced his decision to build the Stanford Linear Accelerator Center, he said, "I am told by the scientists that this is the most extraordinary thing that has been attempted …"; the spokesman referred to by the President was Ed McMillan.

McMillan's contributions to the progress of science did not go unappreciated. As mentioned above, he shared the Nobel Prize with Glenn Seaborg for his discoveries of transuranic elements, and he shared the Atoms for Peace Prize with Vladimir I. Veksler for the discovery of phase stability. He was elected to the National Academy of Sciences in 1947. He was awarded the National Medal of Science in 1990. Since by then he was confined to a wheelchair, the award was presented to his son, Stephen, by the President. McMillan received numerous other awards and honorary degrees, but none of this recognition affected his general humility. He did his work quietly, spoke concisely, and seemed to enjoy everything he was doing. He kept up with evolving knowledge in a surprisingly large number of fields.

In his private life McMillan was a good family man and was greatly supported in all he did by his wife Elsie. He liked hiking and exploring. His particular love was the Anza Borrego desert region, where he collected rocks and concretions that were spread around his office, house, and garden. He was interested in plants and grew orchids as well as insect-eating Venus Fly Traps.

In many of the obituaries Ed McMillan was flagged as an atomic bomb pioneer. Yet while the very discovery of plutonium and his subsequent work at Los Alamos were major contributions to the nuclear weapons program, his own views on nuclear weapons became increasingly critical after the war. He shunned all Cold War rhetoric and remained detached during the Korean War from efforts at Berkeley aimed at replenishing the plutonium supply when it appeared that the United States might be cut off from overseas supplies of uranium. The buildup of nuclear weapons during the Cold War led him to state publicly, "This country has in its hands some incredibly powerful weapons. The way our government deals with the question of nuclear disarmament is shameful—a disgrace to our nation."

Ed McMillan was a humble unassuming person. He enjoyed his science, all of nature, his friends, and his family. His great contributions seemed to flow naturally from him without apparent effort but as a simple product of his mind. The world is richer through Ed McMillan's contributions and poorer through his death.

NOTES

WE THANK EDWARD J. LOFGREN for opening his files on McMillan to us and Philip H. Abelson for a thoughtful perspective on McMillan's research in the prewar years.

1. National Academy of Sciences, *Biographical Memoirs*, vol. 41, p. 251. Washington, D.C.: National Academy Press, 1970.
2. E. McMillan and L. Pauling. *J. Am. Chem. Soc.* 49(1927):666-69.
3. R. P. Brent and E. M. McMillan. *Math. Comput*. 34(1980):305.
4. E. M. McMillan. *Phys. Rev.* 42(1932):905.
5. E. M. McMillan. *Phys. Rev.* 44(1932):240.
6. N. S. Grace and E. M. McMillan. *Physics Rev.* 44(1933):325.
7. E. M. McMillan. *Phys. Rev.* 45(1934):134.
8. M. S. Livingston and E. M. McMillan. *Physics Rev.* 46(1934):437-38.
9. E. M. McMillan. *Phys. Rev.* 46(1934):325.
10. E. O. Lawrence, E. M. McMillan, and R. L. Thornton. *Physics Rev.* 48(1935):493-99.
11. E. M. McMillan and S. Ruben. *Phys. Rev.* 70(1946):123-26.
12. E. M. McMillan. *Phys. Rev.* 47(1935):801.
13. E. M. McMillan. *Phys. Rev.* 55(1939):510.
14. E. M. McMillan and P. H. Abelson. *Phys. Rev.* 57(1940):1185.
15. Quotation from *The Plutonium Story, The Journals of Professor Glenn T. Seaborg, 1939-46*, p. 13. Battelle Press, 1994.
16. *Ibid.* p. 14.
17. G. T. Seaborg, E. M. McMillan, J. W. Kennedy, and A. C. Wahl. *Phys. Rev.* 69(1946):366.
18. *Reminiscences of Los Alamos*, ed. L. Badash, J. O. Hirschfelder, and H. P. Broida, pp. 13-20, 41-48. Holland: Reidel Publishing Company, 1980.
19. In a linear accelerator the situation is reversed. Here the stable phase angle exists at the *rising* part of the rf amplitude; a particle whose energy and therefore velocity are below the norm arrives late and therefore experiences a larger radiofrequency amplitude, with the converse being true for a particle whose energy and velocity are above the norm.
20. E. M. McMillan. *Phys. Rev.* 68(1945):143-44.
21. V. Veksler. *Phys. Rev.* 68(1945):143.
22. E. M. McMillan. *Phys. Rev.* 69(1946):534.

23. E. M. McMillan and J. M. Peterson. *Science* 109(1949):438-39 and with R. S. White, 110 (1949):579-83.
24. E. M. McMillan. *Phys. Rev.* 80(1950):493.
25. By 1965 Livermore and its ancillary sites had 5,300 employees, and Berkeley had 3,200; the Livermore budget was two and a half times Berkeley's. McMillan was nominally director of the whole laboratory. When the Lawrence Berkeley Laboratory and the Lawrence Livermore Laboratory came into separate existences in 1970, Livermore was more than twice as large, with a budget more than three times that of Berkeley.

Selected Bibliography

1927 With L. Pauling. An X-ray study of the alloys of lead and thallium. *J. Am. Chem. Soc.* 49:666-69.

1932 Deflection of a beam of HCL molecules in a non-homogeneous electric field. *Phys. Rev.* 42:905.

1933 The isotopic constitution of lithium in the sun. *Phys. Rev.* 44:240.

With N. S. Grace. Hyperfine structure in the tantalum arc spectrum. *Phys. Rev.* 44:949-50.

1934 Absorption measurements of hard gamma-rays from fluorine bombarded by protons. *Phys. Rev.* 46:325.

With M. S. Livingston. The production of radioactive oxygen. *Phys. Rev.* 46:437-38.

Some gamma-rays accompanying artificial nuclear disintegrations. *Phys. Rev.* 46:868-73.

1935 With M. S. Livingston. Artificial radioactivity produced by the deuteron bombardment of nitrogen. *Phys. Rev.* 47:452-57.

The production of X-radiation by very fast electrons. *Phys. Rev.* 47:801.

With E. O. Lawrence and R. L. Thornton. The transmutation functions for some cases of deuteron-induced radioactivity. *Phys. Rev.* 48:493-99.

1939 Radioactive recoils from uranium activated with neutrons. *Phys. Rev.* 55:510.

1940 With P. H. Abelson. Radioactive element 93. *Phys. Rev.* 57:1185-86.

1945 The synchrotron—a proposed high energy particle accelerator. *Phys. Rev.* 68:143-44.

1946 With G. T. Seaborg, J. W. Kennedy, and A. C. Wahl. Radioactive element 94 from deuterons on uranium. *Phys. Rev.* 69:366-67.

1947 Further remarks on reciprocity. *J. Acous. Soc. Am.* 19:922.

With A. C. Helmholz and D. C. Sewell. Angular distribution of neutrons from targets bombarded by 190 Mev deuterons. *Phys. Rev.* 72:1003-7.

1949 With J. M. Peterson and R. S. White. Production of mesons by X-rays. *Science* 110:579-83.

1950 The origin of cosmic rays. *Phys. Rev.* 79:498-501.

The relation between phase stability and first-order focusing in linear accelerators. *Phys. Rev.* 80:493.

1952 The transuranium elements; early history. In *Les Prix Nobel en 1951*, pp. 165-73. Stockholm: The Nobel Foundation.

1959 History of the cyclotron, part 2. *Phys. Today* 12(10):24-34.

1966 Vladimir Iosifovich Veksler (Obituary). *Phys. Today* 19(Nov.):104-5. Correction, *ibid* 19 (Dec.):14.

1979 Early history of particle accelerators. In *Nuclear Physics in Retrospect, Proceedings of a Symposium on the 1930's*, ed. R. H. Stuewer, pp. 111-55. Minneapolis: University of Minnesota Press.

1980 With R. P. Brent. Some new algorithms for high-precision computation of Euler's constant. *Math. Comput.* 34:305-12.
1984 History of the synchrotron. *Phys. Today* 37(2):31-37.

Hermann Rahn

July 5, 1912–June 23, 1990

JOHN PAPPENHEIMER

> I do not wish to be a professor and just sit and count the veins of a butterfly (wing) and then try to determine their names. I could not stand that for long. I am more interested in the whole animal world. … My ideal is not a professor but a man like Ernest Thomson Seton… by reading his books I try to learn how to live in the woods, how one can sleep warm there in the winter at below freezing temperatures, what wood burns best, how to make a fire in the rain, what provisions one takes on a long trip when you are all alone.

SO WROTE THE SIXTEEN-YEAR-OLD Hermann Rahn to his closest friend and schoolmate, Wolf Tischler, in Germany. Despite these youthful sentiments, Hermann did become a professor and one of the foremost physiologists of his generation. Only a few years later, while still a student, he again wrote to his friend, Wolf, this time saying:

> Natural History is not the problem of today, it is merely a good and interesting basis. I believe that experimental zoology with its "cause and effects" has gotten hold of me. … I was torn between two worlds. Both afford interesting problems, but I never found a connection between them. You work either systemically and anatomically or on the other hand as physiologist and experimental zoologist. According to my mind the latter offers the greater problems. … After being torn between these two worlds it is a good feeling at last to know my way.

These were serious thoughts for a schoolboy, and they reveal Hermann's commitment to research in biology at an early age. In the end he found the connection between the two worlds, and indeed his abiding love of natural history and the call of the wild determined the course of much of his experimental work on respiratory and comparative physiology.

Hermann was brought up in an academic milieu. His father, Otto Rahn, was professor of bacteriology and dairy chemistry, first at Kiel in Germany and later at Cornell University in Ithaca, New York. His mother (neé Bell Farrand, 1883) was a fourth-generation native of Lansing, Michigan, and she was Otto's research assistant at the Agricultural College in Lansing prior to their marriage. Hermann was the eldest of their four children, and most of his formative years were spent in a happy, stable, and intellectually stimulating family environment in Kiel, Germany, and Ithaca, New York. Nevertheless, the first few years of his life were unusually turbulent; no account of his life would be complete without providing brief biographies of his parents and the history of their early married life.

Otto Rahn was one of eleven children born to uneducated Mennonite parents in the little town of Tiegenshof in East Prussia. He was a star student at the town school, and, with the sympathetic support of his father and his mathematics teacher, he was able to attend the University of Göttingen, where he studied physical chemistry with W. Nernst and organic chemistry with Professor Wallach (later to become Nobel laureate in chemistry).

Otto obtained his Ph.D. in 1902 at the age of twenty-one, but academic positions in chemistry were scarce, and he accepted a position in the Department of Dairy Science. His advanced training in organic chemistry and mathematics was unusual in this field, and he soon found applications to bacterial metabolism that brought him international renown. In 1906 he was offered a faculty position in bacteriology at Michigan State and 1907 found him struggling with the English language while teaching chemistry and bacteriology to students in the agricultural college at Lansing.

In the same year, Bell Farrand (Hermann's mother-to-be) graduated from Michigan State and accepted a job as assistant to Professor Clinton Dewitt Smith, who was about to become president of a small agricultural college in Perchicaba, Brazil, six-hours travel west of San Paulo. That was an extraordinary adventure for a well-brought-up young lady from Michigan in 1907, but in her own words, "My nature has always been a bit on the romantic side and having a young and adventurous heart I just could not turn down such an offer to see the world, even though I did not like Mr. Smith." Her romantic nature was to be transmitted and amplified in her son Hermann, whose interests in environmental and comparative physiology led him to roam the world. Bell returned to Lansing in 1908 to join the Department of Bacteriology under Professor Marshall, but she was soon assigned to Otto Rahn as a research and teaching assistant. They were married in 1911, and Hermann was born on July 5, 1912.

Two years later the Rahns boarded a ship for Germany so that Bell could meet Otto's family and show off their two-year-old Hermann to relatives. The Archduke Ferdinand was assassinated while they were on the high seas, and World War I began shortly after they disembarked in Hamburg.

Although Otto was in the process of becoming a U.S. citizen, he and his American bride were classified in Germany as German citizens, and they were not permitted to return to the United States even though Bell was six months pregnant and anxious to return home. Hermann's sister, Marie, was born in November 1914, and soon thereafter

Otto was drafted into the army and assigned menial administrative jobs at a small airfield in Latvia.

One of Otto's sisters owned a bookstore in Danzig, and Bell, Hermann, and the baby moved into a room over the store. For the next four years Bell was cut off from communication with her own family in Michigan, and it was not until November 1918 that she learned through the Norwegian Red Cross of her mother's death. Hermann, by then six years old, had no shoes and froze his toes while waiting in line for the daily family ration of 1 pint of skimmed milk. Bell's footwear was reduced to her satin wedding slippers until Otto managed to provide her with a pair of second-hand army boots he had traded for his tobacco rations.

After the war anti-German feelings remained high in the United States, and since Otto had served in the German army he was not invited to return to Michigan. At the same time, German universities were in disarray, and it was not until 1920 that he obtained a suitable post as professor of dairy physics at Kiel. Hermann was then eight years old, and so his early schooling began in Germany. At this time, also, he and his friend Tischler (later to become professor of ecology at Kiel) began collecting and identifying butterflies, insects, birds, and fauna from the beaches of the Baltic Sea. In 1923 his parents were able to transfer Hermann's savings account of $19.00 from America to Kiel, where inflation was such that they were able to buy him a microscope and a camera, "hoping he would someday become a scientist."

In 1925 W. A. Noyes, professor of chemistry at Illinois (and a member of the National Academy of Sciences), made a goodwill tour of German universities, and he became interested in Otto Rahn's work on the physical properties of milk products, noting that no comparable department of dairy physics existed in the United States. Unbeknownst to her husband, Mrs. (Bell) Rahn confided to Noyes that she yearned to return home to visit her family and asked whether it might be possible for him to arrange a lecture tour for her husband. This idea bore fruit, and in 1926 the entire Rahn family visited America, the children learning English from their American cousins while Professor Rahn went coast to coast on his lecture tour. Shortly after returning to Kiel he received an offer to come to Cornell University as tenured professor of bacteriology and dairy physics. Hermann was enrolled in the local high school in Ithaca and started his transition to the American educational system and way of life. The wilderness of the Finger Lake district of upper New York state was in contrast to the manicured country around Kiel, and Hermann was fascinated by it. Throughout his school and undergraduate years at Cornell he spent many days and sometimes weeks camping out in the wilds, collecting or identifying the flora and fauna. During the summers he took jobs as nature counselor at a Boy Scout camp or as assistant in government fisheries or wildlife departments. At college he grounded himself in the chemical and physical sciences needed for his planned career in experimental zoology. After graduating from Cornell in 1933, Hermann returned to Kiel for one year before enrolling as a graduate student and teaching assistant in zoology at the University of Rochester. His roots in Germany were deep, and he was torn between Germany and the United States. It was not until 1936, after a second visit to Kiel, that he was able to write to his friend Wolf, "… America has at last become my real home."

CONTRIBUTIONS TO REPRODUCTIVE PHYSIOLOGY OF SNAKES AND BIRDS, 1937-43

Hermann's first publications, based on his independent work as a graduate student, were concerned with the repro ductive physiology of viviparous snakes and on the development of the pituitary gland in birds. His discovery in 1937 that viviparous snakes develop a primitive placenta analogous to the mammalian organ won him a National Research Council postdoctoral fellowship to work in reproductive physiology with Frederick Hisaw at the Harvard Biological Laboratories. His year at Harvard (1938-39) was evidently a productive one for it led to a series of eight papers on the structure and function of the pituitary in birds and snakes. With Louis Kleinholz he developed a biological assay for the melanophore-stimulating hormone ("intermedin") of the pars intermedia and determined its activity in a variety of mammalian species. At the same time, he completed a detailed histological study of cell types in the pars anterior of eighteen species of birds and showed that all these species lacked an intermediate lobe. In the same year he found that female garter snakes, collected from Penekese Island off Cape Cod, could store viable sperm in utero for at least one month following insemination, so that the exact time of fertilization of ova and the gestation period were indeterminate. Finally, he found time to court and marry Katherine (Kay) Wilson, a student at the Graduate School of Landscape Architecture.

In September 1939 Hermann moved to his first academic post as instructor in zoology at the University of Wyoming at Laramie. There he made good use of the mountains and prairies to combine his love of nature and natural history with his interests in the reproductive behavior of reptiles. He found that rattlesnakes living at an altitude of 6,000 feet, where the summers are short, have a two-year reproductive cycle, a phenomenon made possible by storage of viable sperm over the winter in a special pocket of the uterus. During the winter hibernation period, also, mature ova were retained in the ovaries and not discharged to meet the sperm until spring. This bizarre schedule meant that at any one time during the summer approximately 50 percent of the adult females were gravid. At lower altitudes, where the summers are longer and warmer, the same species of rattler has a one-year reproductive cycle, and almost all the adult females are gravid. Rahn's principal paper describing this work (published in *Copeia*, 1942) is as convincing as it is interesting, and the simple but original style of this early publication foreshadowed the elegant simplicity of exposition for which he later became noted. His sojourns on the prairies also led him to study reproductive behavior and sexual dimorphism of the sage grouse; when Hermann spoke of the elaborate courtship dances of these birds, you listened to sheer poetry.

PULMONARY MECHANICS AND BLOOD-GAS EXCHANGE; YEARS WITH WALLACE FENN, 1941-56

World War II and chance events abruptly altered the course of Rahn's career and the direction of his research. Of the chance events, undoubtedly the most important was his meeting with Wallace Fenn in Rochester in the summer of 1941. This meeting occurred when Hermann, coming east from Wyoming for a visit to his parents, stopped briefly in Rochester to visit friends before proceeding to Ithaca. He called on Fenn, whom he greatly admired, and before their conversation was over Fenn offered him a job as instructor in physiology, an offer that was accepted on the spot. Fenn was already one of the most distinguished general physiologists in the country, having made pioneer contributions to the mechanism of phagocytosis, the heat production of contracting muscle, the metabolism of active nerve, and the exchange of electrolytes in excitable tissues.[1] Hermann was to become Fenn's closest colleague, confidant, and scientific protégé. Shortly after the United States entered World

War II, the National Research Council asked Fenn to investigate the possibility that the operational altitude of Air Force personnel might be increased by breathing oxygen under pressure (positive pressure breathing). Neither Fenn nor his junior colleagues had ever worked in the field of human respiration, but they accepted the challenge and in the period 1941-45 developed fundamental new approaches to pulmonary mechanics and respiratory gas exchange—concepts that helped to introduce a golden age of theoretical and applied respiratory physiology in the decade following World War II. In this development, Hermann Rahn played a central role, although his association with Fenn and other colleagues was so close that it is difficult for a biographer to separate the relative contributions made by each individual. It is reasonable to suppose, however, that Fenn's quantitative biophysical approach awakened latent talents in Hermann Rahn that made him a full partner in the enterprise and shaped his own approach to biological problems during the next forty years.

Two major contributions to respiratory physiology emerged from the 1941-45 work on positive pressure breathing by the Rochester team, and both were published in 1946 in the *American Journal of Physiology* . The first was titled "The Pressure-Volume Diagram of the Thorax and Lung" with Rahn as senior author; the second was "A Theoretical Study of the Composition of Alveolar Air at Altitude" with Fenn as senior author. The first paper became the starting point for research on pulmonary mechanics in many physiological and clinical laboratories. The second paper provided a graphical solution to equations describing the partial pressures of oxygen and carbon dioxide in alveolar gas as a function of barometric pressure (altitude), inspired gas composition, and respiratory exchange ratio. While most of the equations underlying this analysis had been derived inde pendently by others, their representation in a graphical form that could easily be understood and applied to a variety of problems was a major contribution comparable to the classic nomographic analysis of blood chemistry by L. J. Henderson.[2] Indeed, the next important step was to extend the analysis to the blood-gas exchange, and in 1949 Rahn published his now classic paper titled "A Concept of Mean Alveolar Air and the Ventilation-Blood Flow Relationships During Pulmonary Gas Exchange." In this paper Rahn showed how regional differences in the ratio of alveolar ventilation to alveolar blood perfusion (VA/Q) give rise to oxygen pressure differences between mean alveolar gas and blood leaving the lungs. His analysis was presented in a clear graphical form that has been used by many subsequent investigators. At the time of this work, there were no experimental methods for determining regional pulmonary blood flow or ventilation, and Rahn had to assume normal Gaussian distributions in order to provide numerical solutions in graphical form. More than ten years later, when methods for determining regional ventilation and perfusion using radioactive gases had been developed by J. B. West[3] and others, it was found that the distribution of VA/Q was far from Gaussian. Nevertheless, the new experimental data were easily incorporated into Rahn's theoretical analysis, which continues to be the preferred means of presenting the data. Abnormalities of VA/Q, rather than diffusion capacity, proved to be the most common cause of poor oxygenation of arterial blood in a variety of pulmonary diseases, and Rahn's analysis provides the theoretical basis for clinical tests of impaired gas exchange.

The techniques and concepts developed to investigate respiratory gas exchange during acute exposure to low barometric pressures (altitude) were well suited to studies of other perturbations of the respiratory environment, includ ing the inhalation of CO_2, hyperventilation, breath-holding, diving, and acclimatization to altitude. All of these perturbations were studied by the Rochester team, but for Rahn the lure of the mountains was not to be denied, and during the immediate postwar years he organized three expeditions to high altitudes in Wyoming, Colorado, and the Peruvian Andes. He and his colleagues were first to show that respiratory acclimatization and deacclimatization to altitude, measured in terms of alveolar gas composition, occurs exponentially with a half-time of about twelve hours. The results were clearly delineated as a hysteresis loop on the Fenn-Rahn O_2-CO_2 diagram, and they provided the starting point for subsequent studies by Severinghaus[4] and others showing that the time course of acclimatization is determined by changes in composition of cerebral fluids bathing medullary chemoreceptors.

BLOOD-GAS EXCHANGE AT HIGH AND LOW PRESSURES; PHYSIOLOGY OF DIVING IN THE AMA (DIVING WOMEN) OF KOREA AND JAPAN, 1956-68

In 1956 Rahn moved to the University of Buffalo Medical School as chairman of the Department of Physiology and with him moved the center of gravity of the Rochester school of respiratory physiology. In the years to come he was to attract more than 100 collaborators from some twelve countries to work on such diverse topics as respiratory gas exchange in diving insects, the regulation of pH in poikilotherms, the role of nitrogen in the absorption of gas pockets in animals and humans, distribution of ventilation and perfusion in health and disease, respiratory gas equations as applied to gill breathing, the physiology of diving in the Ama sea-women of Korea and Japan, and allometric studies of gas exchange through the eggshells of developing bird embryos ranging from hummingbirds to ostriches. Although each of these topics was interesting in itself, Rahn always sought for generalities and for ways to present both the problem and its solution so clearly that even a nonspecialist could appreciate its interest and significance. For this reason he was invited to give many public lectures, and the following examples taken from the titles of some of his lectures or essays will illustrate the point:

- The Unique Behavior of Nitrogen Gas
- Breath-Holding in the Mountains and Underwater
- Lung Collapse and Our Space Missions
- Hydrogen Ion Regulation, Temperature, and Evolution
- The Diving Women of Korea and Japan
- How We Store Oxygen
- Why Fish Have Very Low Arterial CO_2 Tensions
- How Eggs Breathe

Rahn's interest in the physiology of diving arose from his analysis of gas exchange during breath-holding, and eventually this led him to investigate the remarkable diving ability of the diving women of Korea and Japan. This work was catalyzed by S. K. Hong, a Korean physiologist who came to Buffalo to study respiratory physiology with Rahn. Together they organized expeditions to the coasts and islands of southern Korea and Japan, where for centuries women divers have harvested the sea floor for food, using only face masks for equipment and enduring high pressures and extreme cold. Using a simple but ingenious device for collecting alveolar samples underwater, Rahn and Hong were able to chart the changes of alveolar gas composition as a function of time and pressure during dives by specially trained native divers. Compression of the lungs during dives to 7 to 10 meters produced correspondingly increased gas pressures, but, of course, oxygen was consumed, so expansion of gases during ascent to the surface caused rapid decrease of oxygen pressure to astonishingly low values—so low in fact that blood entering the lungs lost oxygen to the gas phase and imperilled consciousness. Detailed quantitative explanation of this reversal and the critical conditions for surviving free dives were subsequently worked out in the home laboratories in Buffalo and Seoul, and in subsequent years, also, the physiological adaptations of these hardy women to extreme cold were investigated. A popular account of this work was presented by Rahn and Hong in a *Scientific American* article (1967). Several of the young Korean medical doctors enlisted to help with this project were stimulated to choose physiology as a career, and indeed this collaborative enterprise introduced modern respiratory physiology to both Korea and Japan. This was a special satisfaction to Rahn, who was awarded an honorary LL.D. degree from Yonsei University in 1965. At this stage of his scientific career, Rahn's important contributions to respiratory physiology were also recognized by his election to the American Academy of Arts and Sciences, the presidency of the American Physiological Society, and in 1968 to the National Academy of Sciences.

GAS EXCHANGE IN AVIAN EGGS AND ITS ROLE IN EMBRYONIC DEVELOPMENT, 1968-90

In 1968 Rahn started a completely new venture, namely the respiratory physiology of avian eggs and embryos. This project was to be the principal focus of his research until his death in 1990 and to it he brought an unprecedented knowledge of respiratory gas exchange combined with his lifelong enthusiasm for field studies in classical zoology. In the preface to his two-volume collection of papers in this field he remarks, "The beginning of our interest in gas exchange of avian eggs can be clearly documented. It oc curred in 1968 after the arrival of Douglas Wangensteen as a Postdoctoral Fellow. … One day he asked us how eggs breathe. Since none of us had even thought about this problem, we suggested that he might find out." Wangensteen and Rahn soon determined that exchange of oxygen, CO_2, and water vapor occurs principally by simple passive diffusion through pores in the shell, and this raised a host of basic questions. If gas exchange is limited by diffusion through the shell, how do gas pressures in the tissues change as the embryo increases in size and metabolism during incubation? What determines the number, diameter, length, and total area of the pores in the shell? How are pore dimensions adjusted to provide sufficient conductance for respiratory gases but without fatal loss of water vapor? What are the relations between pore area, thickness of shell, and gas conductance as a function of egg sizes from 1 gram (wrens) to 1,500 grams (ostriches)? How does porosity of eggs at high altitude compare with those of the same size at sea level? Can birds from sea level adjust the porosity of their eggs to compensate for changed diffusivity and oxygen pressures at altitude? The answers to almost all these questions and many more were described in some seventy publications with more than fifty collaborators from around the world. The answers involved measurements of gas exchange on fresh fertile eggs from some 100 species of birds nesting in locations from Spitsbergen to remote islands of the South Pacific, from the deserts of Israel to the Himalayas, from Alaska to the nesting mounds of wild turkeys in Australia. Two illustrations (Figures 1 and 2) from a *Scientific American* article titled "How Birds Breathe" by Rahn, Ar, and Paganelli (1979) are reproduced in this memoir because they exemplify the generality of Rahn's thinking and the elegant simplicity of his expository skill. Rahn's last paper, published posthumously, was on a noninvasive recording of

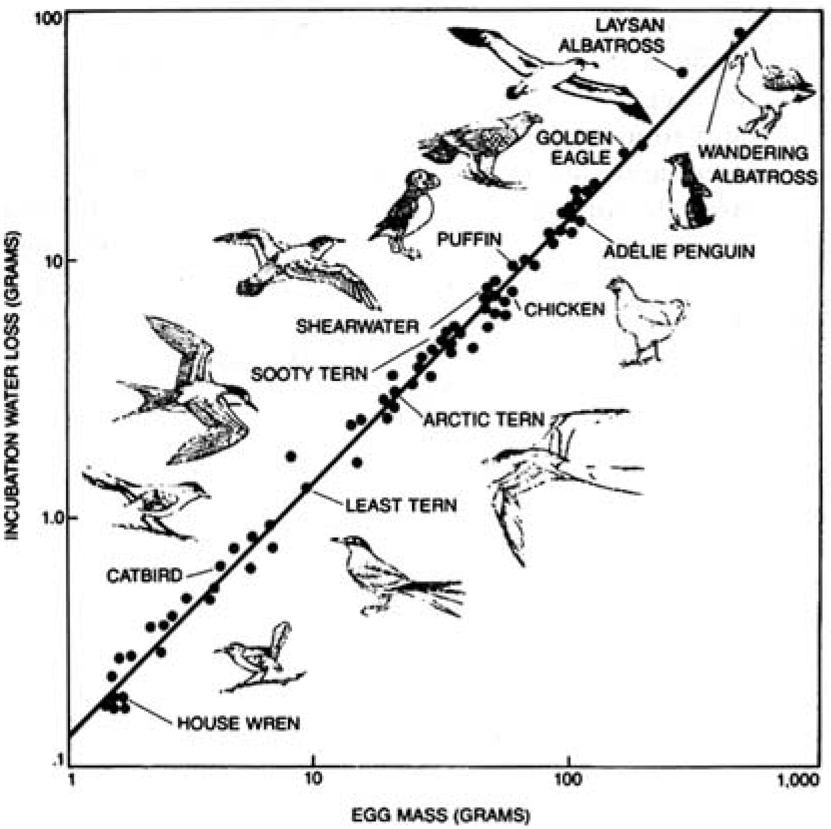


Figure 1 Loss of water during incubation is independent of the metabolic rate of the embryo, yet it appears to be essential for successful hatching. Here the total amount of water lost during incubation has been plotted against the initial mass of the egg. The graph includes data obtained from sixty-five species of eggs ranging in size from 1 gram to 500 grams, with incubation times ranging from eleven to seventy days. There is a remarkably consistent trend: regardless of egg mass or incubation time, the typical egg will lose 15 percent of initial mass during natural incubation.

the heartbeat of developing bird embryos by means of a microphone placed in a sealed chamber containing the egg.

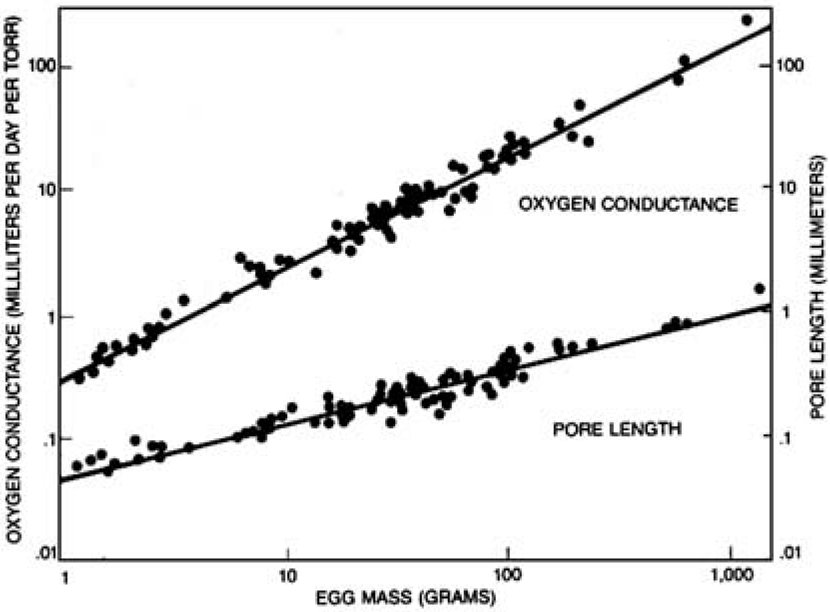


Figure 2 Pore length and oxygen conductance increase at different rates with increasing egg mass, as is shown in this graph encompassing data from the eggs of some ninety species from different parts of the world. For every tenfold increase in mass, the oxygen conductance of the eggshell increases 6.5 times, but the pore length increases only 2.7 times. Pore length probably increases slower because the eggshell must be thin enough for the embryo to hatch.

SERVICE TO SCIENCE: NATIONAL AND INTERNATIONAL

Rahn was a member of numerous scientific societies, and he received distinguished service awards from several of them. However, his primary allegiance was to the American Physiological Society (APS) and to the International Union of Physiological Sciences (IUPS). He was president of APS

(1963-64) and served for many years as scientific editor and board member of its publications, including the comprehensive and scholarly two-volume *Handbook of Respiratory Physiology*, which crowned more than a decade of major advances in this field. He also gave generously of his time to advisory panels of the National Research Council, the National Institutes of Health, the National Aeronautics and Space Administration, and the American Institute of Biological Sciences.

Travel and international aspects of physiology played a major role in Rahn's life. His roots in Europe, his collaborative research with physiologists from many countries, and his sensitivity to different cultures gave him a strong voice in the International Union of Physiological Sciences. He served on its council from 1965 to 1971 and subsequently as its vice-president. From Wallace Fenn he absorbed a strong tradition of loyalty to the triennial international congresses of physiology, and he served on the executive committee of the large and successful XXIVth Congress held in Washington, D.C., in 1968. On several occasions he served as resident visiting professor at foreign universities.

TEACHER, SCHOLAR, AND GENTLEMAN

Rahn grew up in a prewar academic environment in which research was regarded as a joyous, spare-time privilege of a university teacher rather than a driving professional career. This point of view changed rapidly after the war, when large-scale government support for research made it possible for young scientists to create individual research empires without regard for teaching or other traditional academic responsibilities. Rahn was especially vulnerable to this development because his research on gas exchange at high and low barometric pressures had important applications to both clinical and military problems. He was well supported by contracts from the Air Force and the Office of Naval Research as well as from the National Science Foundation and the National Institutes of Health; it would have been easy for him to neglect his teaching and university responsibilities, but to this temptation he never succumbed. Instead, he remained true to his principles, namely, that the primary responsibility of university professors is to their students and departments. At the time he moved to Buffalo in 1956, he organized a comprehensive course in human physiology for medical students, and he continued to play a major role in teaching it throughout his tenure as head of the department. At the same time, he created a stimulating research environment for all members of his staff as well as for the continual stream of postdoctoral fellows, many from abroad, who came to work with him. He was a magnetic source of ideas, drawing in all those around him, and over the years he collaborated and published with all fourteen of his permanent staff members. As one staff member put it, "[Rahn] had a way of sharing his excitement over a new idea and before you know it both of you were in the lab trying it out."

Hermann Rahn was equally at home in the wilderness and in the most formal settings. In civilized society he usually dressed impeccably, and his innate courtesy, modesty, and sensitivity to others (perhaps best described as "courtliness") allowed him to fit in with all social situations, however foreign or sophisticated. His concern for others and his willingness to take on responsibility endeared him to all those who had the privilege of working with him. I have a vivid memory of an exhausted Hermann after he hosted a three-day meeting of 800 members of the American Physiological Society in Buffalo; he was walking back to the lab with drooping shoulders, laden with shopping bags full of presents for the secretaries and others who had helped him organize the meeting.

In 1973 Rahn retired as chairman of the department and became distinguished service professor of physiology. The international renown he brought to this university was further recognized by various awards, including the Stockton Kimball Award in 1969 and the Chancellor Norton Medal in 1981. In 1985 the experimental diving laboratory that had been constructed by the Office of Naval Research for Rahn's studies of underwater physiology was renamed "The Hermann Rahn Laboratory of Environmental Physiology."

In the spring of 1990 Rahn learned that he had incurable pancreatic cancer, but he continued to work in the lab as long as physically possible, and he was working on a manuscript in bed at home a few days before the end. In one of his last letters to his lifelong friend Wolf Tischler he commented on his life:

> … the general maturing of a happy child with his insect collecting, his love with all nature, his wonders and aspirations … to the mature student, the young investigator and finally the reflecting scientist … I am happy to have stayed a *romantic* in science. Today my colleagues have become *business* scientists and I am sure your colleagues have to do the same in order to survive as researchers. So we have both been most fortunate because we are both in a sense still children, with our youthful enthusiasm to explore and search for answers.

ACKNOWLEDGMENTS ARE DUE PROFESSOR Wolfgang Tischler for reading the manuscript of this memoir and for giving permission to quote from his correspondence with Hermann; Hermann's sister, Marie Wohlmann, and his son, Robert, for allowing me to read and quote from the typewritten autobiographies left to them by Otto and Bell Rahn; and members of Rahn's staff at Buffalo, especially Charles Paganelli, R. Blake Reeves, and Augusta Dustan for their comments.

NOTES

1. See "Wallace O. Fenn," in *Biographical Memoirs*, vol. 50, pp. 141-73. Washington, D.C.: National Academy of Sciences, 1979.
2. L. J. Henderson. *Blood: A Study in General Physiology*. New Haven, Conn.: Yale University Press, 1928.
3. J. B. West. *British Medical Bulletin*, 19(1963):53-60.
4. J. W. Severinghaus, et al. *Journal of Applied Physiology*, 18(1963):1155-56.

SELECTED AWARDS AND DISTINCTIONS

1938-39	National Research Council fellow
1960	Harvey Society lecturer
1963-64	President, American Physiological Society
1964	Doctor of medicine, Honoris Causa, University of Paris
1965	Honorary LL.D., Yonsei University, Seoul
1966	American Academy of Arts and Sciences
1968	National Academy of Sciences
1971	Institute of Medicine
1971-74	Vice-president, International Union of Physiological Sciences
1973	Albert Behnke Award, Undersea Medical Society Honorary D.Sc., University of Rochester Distinguished professor, State University of New York at Buffalo
1976-77	Alexander von Humboldt Award and visiting professor, University of Göttingen
1977	Painton Award, Cooper Ornithological Society
1980	Professor honorario, Universidad Peruana, Lima
1981	Doctor of medicine, Honoris Causa, University of Berne
1981	Elliott Coues Award, American Ornithological Union Chancellor Norton Medal, State University of New York at Buffalo
1985	Dedication of the Hermann Rahn Laboratory for Environmental Physiology, State University of New York at Buffalo

Selected Bibliography

1939 Structure and function of placenta and corpus luteum in viviparous snakes. *Proc. Soc. Exp. Biol. Med.* 40:381-82.

With L. H. Kleinholz. The distribution of intermedin in the pars anterior of the chicken pituitary. *Proc. Natl. Acad. Sci. U.S.A.* 25:145-47.

1940 Sperm viability in the uterus of the garter snake, *Thamnophis. Copeia* (3):109-15.

1941 With G. A. Drager. Quantitative assay of the melanophore-dispersing hormone during development of the chicken pituitary. *Endocrinology* 29:725-30.

1942 With F. L. Clarke and M. D. Martin. Seasonal and sexual dimorphic variations in the so-called "air sacs" region of the Sage Grouse. *Wyoming Game and Fish Dept. Bull.* (2):13-27.

The reproductive cycle of the Prairie Rattler. *Copeia* (4):233-40.

1946 With J. Mohney, A. B. Otis, and W. O. Fenn. A method for the continuous analysis of alveolar air. *J. Aviation Med.* 17:173-79.

With A. B. Otis, L. E. Chadwick, and W. O. Fenn. The pressure-volume diagram of the thorax and lung. *Am. J. Physiol.* 146:207-21.

With W. O. Fenn and A. B. Otis. A theoretical study of the composition of alveolar air at altitude. *Am. J. Physiol.* 146:637-53.

1947 With A. B. Otis. Alveolar air during simulated flights to altitude. *Am. J. Physiol.* 150:202-21.

1948 With A. B. Otis and W. O. Fenn. Alveolar gas changes during breath-holding. *Am. J. Physiol.* 152:674-86.

1949 With A. B. Otis. Man's respiratory response during and after acclimatization to high altitude. *Am. J. Physiol.* 157:445-62.

A concept of mean alveolar air and the ventilation-blood flow relationships during pulmonary gas exchange. *Am. J. Physiol.* 158:21-30.

1950 With A. B. Otis and W. O. Fenn. Mechanics of breathing in man. *J. Appl. Physiol.* 2:592-607.

1953 With R. C. Stroud, S. M. Tenney, and J. C. Mithoefer. Adaptation to high altitude: respiratory response to CO_2 and O_2. *J. Appl. Physiol.* 6:158-62.

1955 Respiration. *Ann. Rev. Physiol.* 17:107-28.

With L. E. Farhi. A theoretical analysis of the alveolar-arterial O_2 difference with special reference to the distribution effect. *J. Appl. Physiol.* 7:699-703.

With W. O. Fenn. *A Graphical Analysis of the Respiratory Gas Exchange*. Washington, D.C.: The American Physiological Society.

1957 Gasometric method for measurement of tissue oxygen tension. *Fed. Proc.* 16:685-702.

1960 With E. Agostoni. Abdominal and thoracic pressures at different lung volumes. *J. Appl. Physiol.* 15:1087-92.

With S. K. Hong and E. Y. Ting. Lung volumes at different depths of submersion. *J. Appl. Physiol.* 15:550-53.

1961 With F. J. Klocke. The arterial-alveolar inert gas (N_2) difference in normal and emphysematous subjects, as indicated by analysis of urine. *J. Clin. Invest.* 40:286-94.

The Role of N_2 Gas in Various Biological Processes with Particular

Reference to the Lung. Harvey Lecture Series 55. New York: Academic Press, pp. 173-99.

1962 With J. Piiper and R. E. Canfield. Absorption of various inert gases from subcutaneous gas pockets in rats. *J. Appl. Physiol.* 17:268-74.

1963 With S. K. Hong, D. H. Kang, S. H. Song, and B. S. Kang. Diving pattern, lung volumes and alveolar gas of the Korean diving women (Ama). *J. Appl. Physiol.* 18:457-65.

With E. H. Lanphier. Alveolar gas exchange during breath-hold diving. *J. Appl. Physiol.* 18:471-77.

1964 With L. E. Farhi. Ventilation, perfusion and gas exchange—the VA/Q concept. In *Handbook of Physiology: Respiration*, vol. 1, ed. W. O. Fenn and H. Rahn. American Physiological Society.

1966 Aquatic gas exchange: theory. *Respir. Physiol.* 1:1-12.

1967 With S. K. Hong. The diving women of Korea and Japan. *Sci. Am.* 216:34-43.

1968 With C. V. Paganelli. Gas exchange in gas gills of diving insects. *Respir. Physiol.* 5:1455-64.

1970 With J. Farber. Gas exchange between air and water and the ventilation pattern in the electric eel. *Respir. Physiol.* 9:151-61.

1971 With K. B. Rahn, B. J. Howell, C. Gans, and S. M. Tenney. Airbreathing of the gar fish (Lepisosteus Ossues). *Respir. Physiol.* 11:46-53.

With O. D. Wangensteen and D. Wilson. Diffusion of gases across the shell of the hen's egg. *Respir. Physiol.* 11:16-30.

With O. D. Wangensteen. Respiratory gas exchange by the avian embryo. *Respir. Physiol.* 11:31-45.

1972 With F. W. Baumgartner. Temperature and acid-base regulation in fish. *Respir. Physiol.* 14:171-82.

1973 With W. F. Garey. Arterial CO_2, O_2, pH and HCO_3 values of ectotherms living in the Amazon. *Am. J. Physiol.* 225:735-38.

1974 With A. Ar, C. V. Paganelli, R. B. Reeves, and D. G. Greene. The avian egg: water vapor conductance, shell thickness and functional pore area. *Condor* 76:153-58.

With O. D. Wangensteen, R. R. Burton, and A. H. Smith. Respiratory gas exchange of high altitude adapted chick embryos. *Respir. Physiol.* 21:61-70.

1975 With R. B. Reeves and B. J. Howell. Hydrogen ion regulation, temperature and evolution. *Am. Rev. Respir. Dis.* 112:165-72.

1976 With B. deW. Erasmus. Effects of ambient pressures, He and SF6 on O_2 and CO_2 transport in the avian egg. *Respir. Physiol.* 27:53-64.

1977 With C. Carey, K. Balmas, B. Bhatia, and C. V. Paganelli. Reduction of pore area of the avian eggshell as an adaptation to altitude. *Proc. Natl. Acad. Sci. U.S.A.* 74:3095-98.

1978 With B. J. Howell. The OH^-/H^+ concept of acid-base balance: historical development. *Respir. Physiol.* 33:91-97.

1979 With A. Ar and C. V. Paganelli. How bird eggs breathe. *Sci. Am.* 240:46-55.

1980 With C. Carey and P. Parisi. Calories, water, lipid and yolk in avian eggs. *Condor* 82:335-43.

1982 With G. S. Grant, T. N. Pettit, G. C. Whittow, and C. V. Paganelli. Regulation of water loss from Bonin Petrel (*Pterodroma hypoleuca*) eggs. *Auk* 99:236-42.

With H. T. Hammel. Incubation water loss, shell conductance and pore dimensions in Adele Penguin eggs. *Polar Biol.* 1:91-97.

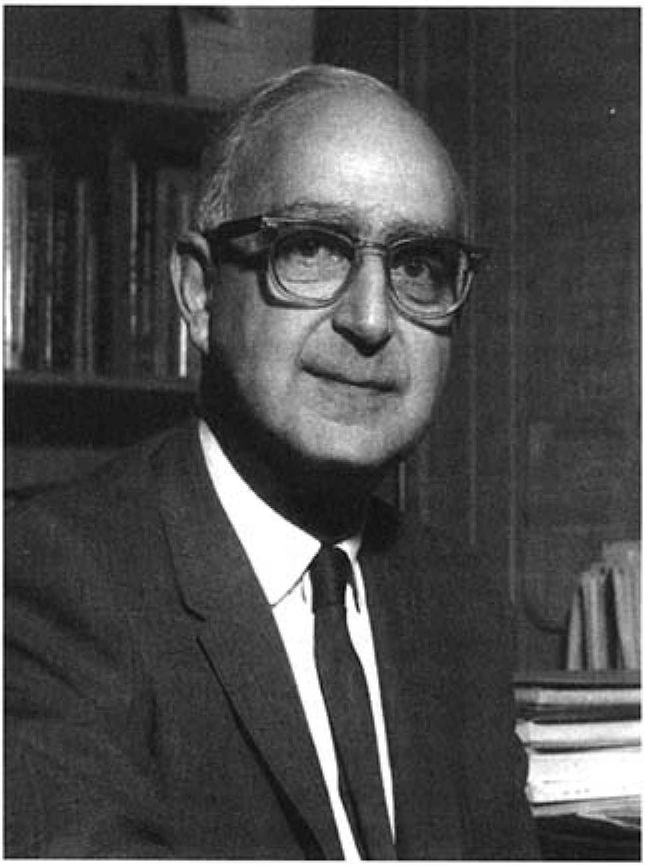
1983 With J. Krog and F. Mehlum. Microclimate of the nest and egg water loss of the Eidar (*Somateria mollissima*) and other waterfowl in Spitsbergen. *Polar Biol.* 1:171-83.

1987 With F. Mehlum, C. Bech, and S. Haftorn. Interrelationships between egg dimensions, pore numbers, incubation tie and adult body mass in Procellariformes with special reference to the antarctic petrel (*Thalassoica antarctica*). *Polar Res.* 5:53-58.

1988 With A. J. Olazowka and H. Tazawa. A blood-gas nomogram of the chick fetus: blood flow distribution between the chorioallantois and fetus . *Respir. Physiol.* 71:315-30.

With D. Swart. Microclimate of ostrich nests: measurements of egg temperature and nest humidity using egg hygrometers. *J. Comp. Physiol. B* 157:845-53.

1990 With S. A. Poturalski and C. V. Paganelli. The acoustocardiogram: a noninvasive method for measuring heart rate of avian embryos in ova. *J. Appl. Physiol.* 69:1546-48.



Photograph by William Dellenback, Bloomington, Indiana

Tracy Morton Sonneborn

October 19, 1905–January 26, 1981

JOHN R. PREER, JR.

WITH PIPETTES, CULTURE VESSELS, a low- and a high-power microscope, and a few collections of water from nearby ponds and streams, Tracy Sonneborn worked with species of the *Paramecium aurelia* group and learned more about the basic biology of protozoa than anyone else ever has. He discovered mating types in *Paramecium*, thereby advancing biological studies on the protozoa by a quantum leap. He demonstrated simple Mendelism and established the behavior of genes, nuclei, and cytoplasm in the complex processes of the life cycle. He showed that the uniparental nuclear reorganization that occurs periodically in many paramecia is the sexual process of autogamy, not the asexual process of endomixis as originally thought.

He discovered macronuclear regeneration and cytoplasmic exchange, both invaluable for genetic analysis. He demonstrated caryonidal inheritance, showing that individual ciliate macronuclei, although descended asexually from identical micronuclei, can acquire different genetic properties during their development. He showed that the phenotype of *Paramecium* is determined by the macronucleus, not the micronucleus. He advanced our understanding of the states of immaturity, maturity, and senescence in the life cycle of the ciliated protozoa, showing that aging can be reversed by autogamy as well as conjugation. His analysis of species in lower organisms produced novel evolutionary concepts.

Primarily he will be remembered for his studies on non-Mendelian inheritance. When he began his work, the role of the cytoplasm in heredity was entirely unknown. He showed that the various cases of non-Mendelian inheritance could be classified into distinct groups, most involving interactions between nuclear genes and the cytoplasm. His early studies on the cytoplasmic factor "kappa" established the first case of cytoplasmic inheritance in animals, and subsequent work by him and his students showed that intracellular symbiotes and cell organelles have become inextricably combined during evolution. His studies on surface proteins showed that complex systems of interacting elements in protein synthesis can create stable states of gene expression dependent on factors present in the cytoplasm. In a most elegant series of experiments on the ciliate cortex, he and his collaborators showed that the form and arrangement of preexisting structures determine the form and arrangement of new structures.

Finally, studying mating type and an unusual trichocyst mutant, he uncovered the first examples of a strange non-Mendelian phenomenon in which the macronucleus of ciliated protozoa determines the cytoplasm, and the cytoplasm in turn determines newly forming macronuclei, thereby passing genetic information from the old disintegrating macronucleus to the newly forming macronuclei.

PERSONAL HISTORY

Tracy Morton Sonneborn was born on October 19, 1905, in Baltimore, Maryland. His mother was Daisy Bamberger, and his father, Lee, was a businessman. Both encouraged him in his education. Others having an important influ ence in his early life were his uncle, Jacob Bamberger, and his cousin, Louis Bamberger. It was Louis Bamberger who established the Institute for Advanced Study at Princeton. As a teenager, Tracy became interested in the humanities and religion and at one point seriously considered becoming a rabbi. However, his beliefs soon changed, and, after attending Baltimore Polytechnic High School for two years and Baltimore City College High School for two more years, he entered Johns Hopkins University with the intention of studying literature. His interests changed to science when he took an introductory course in biology taught by E. A. Andrews. He received the B.A. degree from Hopkins in 1925.

He then began graduate work on the flatworm, *Stenostomum*, under the supervision of Herbert S. Jennings, director of the Zoological Laboratories at Johns Hopkins. Jennings was a remarkable scholar, one of the pioneers of biology. He published extensively and was renowned as a scientist, philosopher, and educator. Jennings had a broad view of biology. He worked on lower organisms and was concerned with the most fundamental aspects of behavior, inheritance, development, population biology, and evolution. Jennings had a profound influence on Tracy's development as a scientist. Tracy's passion for thoroughness and detail and his broad view of biology were like that of his teacher. He received the Ph.D. at Johns Hopkins in 1928.

At that time, he received a National Research Council fellowship and spent 1928 and 1929 with Jennings at Hopkins working on the ciliate, *Colpidium*. In 1929 he married Ruth Meyers; it was a happy union that lasted until his death fifty-two years later. At the end of Tracy's fellowship in 1930, his attempts to obtain a faculty position failed, but he was offered a position as a research assistant at Hopkins with Jennings, who had just obtained a research grant from the

Rockefeller Foundation for work on *Paramecium*. During the period 1930 to 1939, Tracy held the positions of research associate and then associate at Johns Hopkins. After seven years of basic studies on the life cycle of *Paramecium*, he made his discovery of mating types in 1937, which immediately won him fame as an investigator. In 1939 Fernandus Payne persuaded him to accept a position at Indiana University as an associate professor. There he stayed for the rest of his life, becoming professor in 1943, distinguished service professor in 1953, and distinguished professor emeritus in 1976.

His first son, Lee, was born in 1929 in Baltimore and became a mathematician. His second son, David, was born in 1934, also in Baltimore and, like his father, became a biologist. Tracy's family life was remarkable. His wife, Ruth, was educated as a social worker and might have had a distinguished career of her own. Instead, she devoted her life to family and to his career. He was deeply grateful to Ruth, for she made it possible for him to devote himself virtually full-time to his scholarly activities. She was clearly the mother and personal confidant of all the many students and postdoctorals who passed through the Sonneborn Laboratory at Indiana University. When Tracy arrived home from work, his role of eminent scientist whose every word was carefully considered by his students changed completely. He was just one more member, albeit a greatly beloved member, of a very close, well-adjusted, happy family. At one point during Thanksgiving dinner at my first visit to his home, amid all the gay conversation, Tracy was finally able to get in an opinion on the topic at hand. There followed a sudden silence around the table followed by a pronouncement from his youngest son, age five: "Old dummy Daddy." As a new graduate student I was indeed shocked, for at the laboratory his every pronouncement was worthy of the utmost respect and consideration, but here everyone thought it was a splendid joke. Throughout his life these close relations within his family never changed.

Tracy was vitally interested in the activities and accomplishments of those about him. In conversation he spoke quickly and thought even more quickly. His incisive and often blunt comments were a bit intimidating at first for some of his new students, but his kindness and humor made him easy to engage in conversation. After a full day in his office and laboratory, he spent almost every evening thinking, writing, and making notes. For most of his life he met for a long session once a week in the evening with his students and others in his research group. Music and birds were his primary hobbies, but they took only a small portion of his time.

Every task that claimed his attention—an experiment, a new course, a research report, a manuscript to review, a student's class paper—somehow became the most important thing in the world to him. It had to be done with thoroughness and perfection. Nothing was too much trouble. An undergraduate lecture was as important as a keynote address at a major scientific meeting. He regularly took his place at undergraduate registration, interviewing each student (often 200 or 300), making sure all had the appropriate background and interests for his class. He once commented that teaching and research in no way interfered with each other, for all one needs to do is devote forty hours per week to each. For him that was clearly an understatement.

His lectures, whether for large classes, small classes, undergraduates, graduates, or scientific papers presented to his peers, were presented in a clear and exciting fashion. His enormous enthusiasm spread to all his audience. After a lecture at Goucher College in 1937 describing his first finding of mating types, his audience was ready to follow him out the door and back to his laboratory to find what the experiments in progress would show. His first course at Indiana University, which was supposed to cover all the invertebrates, got no farther than the flatworms. He used to joke that when his department chairman, Fernandus Payne, learned that he had only covered protozoa, co-elenterates, and flatworms in his course on invertebrates he almost got fired. It is noteworthy that two members of the class went on to careers studying protozoa, one even shifting from a commitment in another field. The excitement he generated was genuine and long lasting. For example, when he lectured on algae in a course with no formal laboratory, it was routine to see algae appear spontaneously in the various laboratories in which the graduate students worked, as they attempted to repeat and carry some of the experiments a step further.

In the late 1940s his laboratory enlarged. He brought in Wilhelm van Wagtendonk, a biochemist from Oregon, who Sonneborn hoped would work out the biochemical basis for the many genetic traits that he was investigating. However, it turned out that these traits were not readily accessible to biochemical investigation. Van Wagtendonk decided that it was necessary first to develop a defined medium for culturing *Paramecium*. This endeavor proved to be very difficult and time consuming. In the end he was successful, but it required the remaining portion of Van Wagtendonk's research career to achieve success. Early on, Ruth Dippell became his research technician. Ruth eventually received the doctorate degree and became a faculty member, but she always worked closely with him in her research. As the laboratory enlarged and his Ph.D. students increased in number, numerous postdoctoral workers also came, many from Europe and some from Japan and China. Bloomington became the Mecca for all who would work on *Paramecium*. These investigators went on to important positions in universities and research institutes throughout the world. Soon most of the work on *Paramecium* was being done by those who had passed through his laboratory.

He continued to do research until his death in Bloomington in 1981, following a short illness with cancer.

Tracy received many honors during his career. He was elected a member of the National Academy of Sciences in 1946, a foreign member of the Royal Society of London in 1964, a member of the American Academy of Arts and Sciences in 1946, and a member of the American Philosophical Society in 1952. He received the Kimber Genetics Award of the National Academy of Sciences in 1939, the Mendel Centennial Medal of the Czechoslovakian Academy of Sciences in 1965, and the Newcomb-Cleveland Medal and Prize of the American Association for the Advancement of Science in 1946. He was an honorary member of the French Society of Protozoology, the Genetics Society of Japan, and the American Society of Protozoologists. He received honorary doctor's degrees from Johns Hopkins University, Northwestern University, Indiana University, the University of Geneva (Switzerland), and the University of Westphalia (Germany). He served as president or board member of many scientific organizations and gave numerous prestigious lectures in this country and abroad.

PROFESSIONAL HISTORY

Stenostomum

When Tracy began his work for the Ph.D. in 1926, his mentor, Jennings, believed that, although Mendelian genes were responsible for most of the traits in higher organisms, other genetic mechanisms might also exist. These factors were thought to be especially important in lower organisms, and it was also thought that they might be localized in the cytoplasm and susceptible to environmental modification. The way to test these speculations was simply to study the effects of environment and heredity on the development of various traits in selected lower forms of life. Such studies were to form the basis of Tracy's whole research career. His Ph.D. problem was on inheritance in *Stenostomum*, which reproduces asexually by dividing transversely into an anterior and a posterior half. He was able to identify and follow these halves in isolation cultures and found that progressive lines of anterior division products were more likely to age and die than lines of posterior products. He also exposed *Stenostomum* to lead acetate and found that abnormalities appeared. After such treatments he was able to isolate two-headed "monsters" that reproduced true to type. Since these traits were maintained for many generations, they were judged to have a hereditary basis. However, these variants arose and were lost at a much higher frequency than one would expect if they were due to mutations in simple Mendelian genes.

Colpidium

After his Ph.D. work, Tracy stayed for eleven more years in Jennings's laboratory at Johns Hopkins. His first work was on the small ciliate, *Colpidium*, an organism he had used to feed his *Stenostomum*. He cultured *Colpidium* on a strain of bacteria on which they flourished. When he changed the bacterium to another less favorable kind, abnormalities appeared in the body form. From these abnormal animals he was able to isolate double animals, and these doubles reproduced true to type indefinitely, even when they were returned to culture on the more favorable bacterium. Again, the effect of the environment in inducing abnormal ani mals of a particular kind in high frequency was not what one would expect on the basis of mutation in Mendelian genes.

The Life Cycle Of Paramecium

Sonneborn began his work on *Paramecium* when the problems of genetics, development, cell biology, and evolution were being attacked energetically by such workers as Morgan, Sturtevant, Bridges, Darlington, Haldane, Wright, Demerec, Jennings, Ephrussi, Beadle, Tatum, Emerson, McClintock, and Stadler. Sonneborn assumed his position as one of that group. His plan for research was simple: learn all he could about a single organism and apply his knowledge generally where applicable. By choosing a single organism, *Paramecium*, he thought he could attain a mastery of that organism that would enable him to carry out sophisticated experiments impossible for scientists who pick a single problem and move from organism to organism. He stuck to his plan faithfully, studying *Paramecium* almost exclusively during his whole research career. Sonneborn noted that, while protozoa are whole organisms, they are also single cells, and he recognized a rare chance to study inheritance independently of the complex multicellular life cycle that precluded investigations of cellular genetics in most organisms. While procaryotes are also unicellular, he felt that most studies on bacteria were concerned with populations of cells, not individual cells.

A test for Mendelism by breeding analysis could not be made in the case of either *Stenostomum* or *Colpidium* because both lacked sexual reproduction. By turning to *Paramecium*, which is able to conjugate and exchange germinal nuclei, he thought definitive tests of Mendelism would be possible. The only problem was that mating reactions, while common in both nature and the laboratory, could not be controlled and often occurred even in clones (i.e., cultures derived by binary fission from single cells). So Sonneborn set about learning to understand and control mating and the life cycle.

Members of the *Paramecium aurelia* complex of species have a vegetative polyploid macronucleus that directly controls the characters of the cell and also two germinal micronuclei that periodically give rise to new macronuclei. *Paramecium* reproduces vegetatively by binary fission. The macronucleus divides amitotically, and the micronuclei divide by mitosis. At conjugation and autogamy, the old macronucleus breaks into fragments and normally is lost during subsequent fissions, while the two micronuclei undergo meiosis. A single haploid meiotic nucleus then divides to give a migratory and a stationary haploid nucleus. The migratory nucleus from each conjugant fuses with the stationary nucleus of its mate, or in the uniparental process of autogamy the two products simply fuse with each other. In each cell the diploid zygote micronucleus gives rise by mitosis to four micronuclei. Two remain as micronuclei and two develop independently into macronuclei. At the next fission the two new macronuclei are segregated one to each daughter cell, while the micronuclei divide mitotically and are distributed two to each daughter cell, restoring the normal vegetative state. Sonneborn was able to control autogamy when he found that a rapid fission rate in an excess of fresh culture medium inhibited autogamy while starvation induced it, provided the animals had undergone a sufficient number of fissions since the last conjugation or autogamy. Note that following the first fission after autogamy and conjugation each of the two cells has a macronucleus derived independently from different micronuclei—he called the two lines "caryonides." He discovered that mating within a caryonide is seen only rarely, while for the strains of *Para* *mecium* that he was studying, mixing cells of different caryonides in the proper physiological condition often resulted in immediate and massive mating reactions leading to pair formation and conjugation. In this way he not only discovered mating types in protozoa, but acquired the ability to make crosses between different lines. Many different mating types, characteristic of different strains of *P. aurelia* were described. The discovery of mating types was an exciting discovery and won Sonneborn immediate recognition by the academic community and even in the press.

Later he showed that sometimes fragments of the old macronucleus are not lost but persist and in subsequent asexual generations regenerate into macronuclei. Moreover, he learned how to induce this process of macronuclear regeneration at will. Although cytoplasm is not normally exchanged at conjugation, he also learned how to induce cytoplasmic exchange. Furthermore, he proved that the nuclei behaved as described above by showing that, after autogamy, lines are homozygous in all their genes, and after conjugation typical Mendelian ratios could be produced. These techniques gave him exquisite control over his organism and made it possible for him to carry out highly sophisticated genetic experiments.

Mating-Type Inheritance

As he continued his investigations on the genetics of *Paramecium*, Sonneborn studied all the character differences he could find. Unlike students of genetics in organisms like *Drosophila*, maize, and, later, *Neurospora* and yeast, he found that virtually every character he looked at in those early days proved to involve a combination of Mendelian and non-Mendelian elements. In some strains of *Paramecium* two mating types were found. Determination occurred at the formation of the new macronuclei at conjugation or auto gamy. Other strains, expressed only one mating type. It was shown that the difference in strains was accounted for by a single genic difference, the first gene demonstrated in ciliates. Today we know that massive reorganization of the DNA occurs at macronuclear formation in the ciliates, involving chromosome breakage, deletions, and reordering of sequences. In the case of mating type determination, reorganization can proceed in such a way that one mating type is expressed in one caryonide, while another mating type is expressed in a sister caryonide. Only today are we coming to appreciate the many cases of nuclear differentiation that occur during development in the metazoa.

In simple caryonidal inheritance, mating type is determined independently of the parental type and independently of each of the two sister caryonides after conjugation or autogamy. However, it was found that, in some strains of *Paramecium*, mating type inheritance proved to be caryonidal but also showed a marked tendency for the new caryonides to be like each other and like the mating type of the original cell in which they were formed. The results appeared to indicate cytoplasmic inheritance, and this conclusion was reinforced by crosses involving cytoplasmic transfer from one mate to the other. In a brilliant experiment involving conjugation, cytoplasmic exchange, and macronuclear regeneration, Sonneborn was able to produce individual cells that contained fragments of the old macronucleus destined to regenerate, micronuclei that were developing into macronuclei, and cytoplasm of the opposite mating type derived from the mate. At subsequent fissions, macronuclei of the two kinds segregated, and by means of genetic markers he was able to distinguish those derived from the old macronuclear fragments from those arising from new macronuclei developing in the normal way from micronuclei. The results demonstrated that newly forming macro nuclei derived from micronuclei respond to the cytoplasm in which they are found and become determined like the cytoplasm that surrounds them. However, the type of fragments is always like that of the original macronucleus from which they were derived. In short, in the formation of new macronuclei the cytoplasm determines the macronucleus for mating type, and once the macronucleus is determined it never changes. The cytoplasm, on the other hand, always reflects the type of macronucleus with which it is found. Thus, it appears that genetic information is passed from the old macronucleus to the cytoplasm to the newly forming macronuclei. This mode of inheritance has since been called "macronuclear inheritance" by Meyer. Macronuclear inheritance has been shown to occur for a number of other traits in *Paramecium*. Its molecular mechanism is still not understood.

Killers

Sonneborn also discovered and studied killers, paramecia that produced a toxin that could kill other strains of paramecia yet that are resistant to their own toxin. Crosses showed the presence of nuclear genes necessary for the perpetuation of the killer trait and also showed the presence of an essential cytoplasmic element that he called "kappa." Strains that lost kappa became sensitive to the toxin. Kappa proved puzzling to Sonneborn for many years, but it was finally shown in other laboratories that kappa is an example of a symbiotic bacterium able to live only in *Paramecium*. Many such forms have been described with various degrees of benefit and harm to their hosts. They emphasize to all geneticists the difficulties of distinguishing between infection and cytoplasmic heredity. In fact, it is now considered that all cases of cytoplasmic heredity based on the presence of self-replicating cytoplasmic nucleic acids are probably derived evolutionarily from viruses or bacteria. Even such "normal" organelles as mitochondria and chloroplasts are thought to have such an origin.

Serotypes

Early workers showed that antiserum prepared by injecting paramecia into rabbits initially mobilized all cells of the injected clone. It was also found that resistant cells often appeared and that after isolation they produced resistant clones. Sonneborn began a study of this phenomenon and quickly confirmed the general features of the early studies. By injecting the resistant paramecia into rabbits, he obtained new sera and eventually showed that from a single clone of *Paramecium* he could obtain many subclones pure for up to a dozen different antigenic types, called serotypes, each reacting only with its own homologous antiserum. Moreover, he showed that exposure to antiserum actually induced the shift from one serotype to another. Genetic analysis revealed a series of independent genetic loci, each specific for a given serotype, with one gene active at a time. The shift from one serotype to another was due to switching from the activity of one gene (all the others inactive) to the activity of another. Serotype specificity and the ability of a serotype to be expressed at all were shown to be due to alleles at the different serotype loci. In one set of environmental conditions, Sonneborn found that most of the serotypes would reproduce stably for many generations. Crosses between serotypes of a single pure genotype always revealed cytoplasmic inheritance. Early in the investigation of serotypes it was pointed out that serotype inheritance could be explained in terms of stable states of gene expression that rely on feedback mechanisms for their perpetuation. This interpretation was eventually accepted by Sonneborn and has recently received support from molecular studies.

Plasmagenese

Virtually *all* the traits Sonneborn encountered in his early studies were non-Mendelian, with strong genic and strong cytoplasmic components. At this point the evidence seemed to lead to the conclusion that cytoplasmic inheritance was an important component in all cases of inheritance in *Paramecium*. Perhaps in higher organisms that same was true, but it was being masked by the processes occurring in embryological development. So at this time the plasmagene theory was born: It was postulated that all genes in all organisms produce a self-reproducing entity that persists through somatic cell divisions but that is lost during sexual reproduction. The theory was given support by a number of studies done by others, especially the studies of Spiegelman on adaptive enzymes in yeast.

As work progressed, however, it became clear that the interpretation of the data as evidence for plasmagenes was not valid. Kappa and its relatives turned out to be symbiotic bacteria, dependent upon special genes for their maintenance. Further work on the expression of genes for surface proteins seemed to be best interpreted as a special interplay of competing inhibitors and activators of protein synthesis. Mating type inheritance was more difficult to evaluate, but Sonneborn was able to show that mating type inheritance was ultimately under nuclear control, the cytoplasm acting only to transmit information from the old macronucleus to the newly developing macronucleus. There was, in fact, no evidence for self-reproducing cytoplasmic genes.

The Cortex

Faced with these new findings, the notion of plasmagenes was discarded, and Sonneborn embarked on his investigations of the structure of the cellular cortex in *Paramecium*. Sonneborn always viewed his early work on *Stenostomum* and *Colpidium* as incomplete, for, although his two-headed monsters in *Stenostomum* and doublets in *Colpidium* arose in high frequency and in response to environmental stimuli in a decidedly non-Mendelian fashion, the organisms were asexual, and decisive breeding tests were not possible. He found, however, that doublets could easily be induced in *Paramecium* by exposing conjugating cells to antiserum. He now set about crossing singles with doubles. The results ruled out Mendelian genes. He also ruled out both the presence of determinants in the fluid cytoplasm and macronuclear inheritance like that observed for certain mating types. He was left with the cortical structure itself as the basis for the inheritance. Moreover, he and his collaborators were able to show that rearrangements in the pattern of the cilia, trichocysts, parosomal sacs, and fibrillar structures that make up the cortex also can be inherited in the same fashion. Sonneborn said that these instances were based on a new principle of inheritance that he called "cytotaxis," the ability of preexisting structures to control the formation and placement of new structures. Cytotaxis has since been studied extensively in the ciliate cortex by many workers.

Again, Sonneborn produced a brilliant series of experiments. They showed without doubt that preexisting structure controls the way new structures are formed in the cortex of ciliated protozoans. This work was held to be a major new phenomenon in genetics and development, applicable to all organisms. Currently, it appears that these principles are indeed applicable to other organisms and organelles, but its true general significance is yet to be determined.

Senescence And The Life Cycle

It has been known for many years that after conjugation many ciliates undergo an immature period of many vegetative generations in which they are unable to mate. Then after a period of maturity, if mating does not occur, there is a period of senescence and finally death. Jennings pointed out that each stage lasts for such a long period that one must consider the stages heritable. The basis for the changes has remained unknown, although recent experimental evidence involving microinjection reinforces the view that its basis lies within the macronucleus. In any case, it is clear that the mechanism does not rely on simple Mendelian genetics. Sonneborn investigated the matter in relation to the unisexual process of autogamy in *Paramecium*. He found that autogamy could substitute for conjugation in rejuvenating senescent lines of paramecia. Another life cycle change he noted was that after autogamy paramecia must undergo a certain number of fissions, in some cases a large number, before cells can undergo another autogamy. The basis for these life cycle changes is unknown.

The Species Problem

When Sonneborn discovered mating types, he found twenty-eight types among different strains. He was able to show that only mating type I could mate with mating type II, only III with IV, and so on, for a total of fourteen different mating pairs. He noted that each pair constituted a single interbreeding group. Since each group shared a common gene pool, it was clear that they constituted a series of sibling species. From the beginning he realized the taxonomic problem presented by the situation, for mating types can not be readily ascertained in the field. Even in the laboratory the process is time consuming, requiring the isolation and mixing of clones with standard mating types. He realized the problem that would be presented to taxonomists if the groups were given binomial names. His initial solution was to call the groups varieties. Later, in recognition of the genetic isolation of the varieties, he changed the designation to a newly invented term, "syngen." Finally, as more became known about the syngens, particularly their isozymes, responses to different strains of killers, fission rate, and other traits, Sonneborn recognized the syngens of *Paramecium aurelia* as separate species and designated them *P. primaurelia, P. biaurelia*, and so on. In other less-well-characterized ciliates, the mating groups are still called syngens.

Sonneborn also pointed out that the sibling species of the *P. aurelia* group, as well as the sibling species of other protozoans that are delimited primarily by their mating types, presented a set of interesting ecological and evolutionary problems. He noted that some species in the *P. aurelia* group have a long immature period after conjugation, while others have a very short or no immature period. Since those with a long immature period are less likely to mate with each other in nature, he classified them as "outbreeders," while those with a short immature period he classified as "inbreeders." Outbreeding, which favors genetic diversity, was held to be the ancestral type. He made detailed studies of the properties of the various species and also studied the viability of progeny obtained from crosses. He related this information to the ecology and evolution of the groups.

Genes

Different Mendelian genes were not readily found in *Paramecium*, but the behavior of the first ones that Sonneborn found were sufficient to establish the validity of the com plex cytological events of the life cycle. Eventually, it was found that numerous mutants could be isolated after chemical mutagenesis. Sonneborn then engaged in a large study, isolating more than 100 different visible morphological and behavioral mutants. These were then tested for linkage. Because of the large number of chromosomes in *Paramecium* and perhaps because of a high rate of recombination, few cases of linkage and no maps resulted.

Trichocysts

Sonneborn's last project was the investigation of an aberrant mutation that reduced the ability of trichocysts to discharge. Although several simple gene mutations had the same effect, this mutant seemed to follow the cytoplasm in crosses. A more detailed analysis revealed that it was inherited just as mating type was inherited in many strains—that is, it was macronuclear inheritance as described above. Since Sonneborn's death, additional cases of macronuclear inheritance affecting other traits have been found and are now being actively investigated.

CONCLUSION

While Sonneborn was learning whatever *Paramecium* could teach him about biology, a new generation of microbial geneticists, working with fungi, bacteria, and bacteriophages, was establishing the foundations for the new science of molecular biology. Unlike *Paramecium*, these organisms had properties that proved to be invaluable in the new science. They had simple nutritional requirements, synthesizing most of the complex substances they needed and thereby enabling the investigator to study the genetic control of many of the enzymes of metabolism. They could be plated onto agar, making possible the quick and easy examination of innumerable clones. This technique was absolutely es sential for the study of mutations or rare recombinants. In these organisms one could carry out studies on the role of genes in controlling metabolic pathways, enzyme synthesis, and enzyme structure. Investigations of mutations and genetic fine structure also were possible. These were the studies that finally led to our knowledge of the roles of DNA and RNA and produced the modern revolution in molecular biology. *Paramecium* was eminently unsuited for any of these studies.

Hence, Sonneborn did not participate in this revolution that was sweeping biology and biochemistry, although it was clear that, like everyone else, he greatly appreciated and admired the work that was going on. He would have loved to be at its forefront. But *Paramecium* did not lead him there and could not have led him there, for it was simply not useful for such studies. The Nobel prizes that were awarded so generously to the disciples of the new biology eluded Sonneborn. That is not to say that his work was unnoticed. He was, indeed, widely recognized as an outstanding investigator. Nevertheless, a glance at any current textbook of general biology or genetics leads one to the conclusion that he was not the originator of concepts that are basic to the thinking of most biologists and geneticists today.

It has been suggested that Sonneborn avoided more conventional genetics and focused on the role of the cytoplasm in heredity. In my view, that notion is not correct. Sonneborn concentrated on the inheritance of whatever traits he could find in *Paramecium* without prejudice. It simply turns out that most of the easily observable traits in *Paramecium* are inherited in a non-Mendelian fashion. Would he have pursued his research differently had he known that *Paramecium* could not take him to the forefront of the great revolution in biology that was just developing? In those early days no one knew what *Paramecium* had to offer. It was a member of a group of organisms that was simply too big and too different to be left unexplored. We had to know what protozoa were like, just as we had to know about bacteria and viruses and insects and mice and corn and worms and zebra fish. We had to know about the role of the cytoplasm in genetics. And, indeed, *Paramecium* turned out to be ideal for the study of inheritance at the cellular level and for the study of nuclear differentiation. Although there are no plasmagenes, there are cytoplasmic entities that contain DNA. There are stable metabolic states that are passed from one generation of cells to the next. Preexisting structures and patterns of structures are important in determining new structures at cell division. And, finally, differentiation of new nuclei in ciliates can produce new stable configurations and can be influenced by factors emanating from preexisting nuclei and passed through the cytoplasm. The role of preexisting structure in developmental biology is not yet understood, and the strange nuclear and cytoplasmic effects that Sonneborn uncovered are still unexplained at the molecular level. Whatever the final outcome of studies of these phenomena, he must take his place among the most brilliant and devoted experimentalists in the history of biology and a true giant, like no other, in the field of protozoan research.

I HAVE DRAWN ON unpublished material in my files, much received from Tracy himself over the years, as well as unpublished material from Ruth Dippell and Ruth Sonneborn, his wife. The reader is also referred to an account of Tracy's life by G. H. Beale in *Biographical Memoirs of Fellows of the Royal Society*, vol. 28, pp. 537-74 (London: Royal Society, 1982).

Selected Bibliography

1930 Genetic studies on *Stenostomum incaudatum* (nov. spec.), II. The effects of lead acetate on the hereditary constitution. *J. Exp. Zool.* 57:409-39.

1932 Experimental production of chains and its genetic consequences in the ciliate protozoan *Colpidium campylum. Biol. Bull.* 63:187-211.

1937 Sex, sex inheritance and sex determination in *Paramecium aurelia. Proc. Natl. Acad. Sci. U.S.A.* 23:378-85.

1939 *Paramecium aurelia*: mating types and groups; lethal interactions; determination and inheritance. *Am. Nat.* 73:390-412.

1941 Relation of macronuclear regeneration in *Paramecium aurelia* to macronuclear structure, amitosis and genetic determination. *The Collecting Net* 16:3-4.

Sexuality in unicellular organisms. In *Protozoa in Biological Research*, ed. G. N. Calkins and F. M. Summers, pp. 666-709. New York: Columbia University Press.

1943 Gene and cytoplasm. I. The determination and inheritance of the killer character in variety 4 of *P. aurelia. Proc. Natl. Acad. Sci. U.S.A.* 29:329-38.

Gene and cytoplasm. II. The bearing of the determination and inheritance of characters in *P. aurelia* on the problems of cytoplasmic inheritance, *Pneumococcus* transformations, mutations and development. *Proc. Natl. Acad. Sci. U.S.A.* 29:338-43.

1945 Gene action in *Paramecium. Ann. Mo. Bot. Garden* 32:213-21.

1946 Experimental control of the concentration of cytoplasmic genetic factors in *Paramecium. Cold Spring Harbor Symp. Quant. Biol.* 11:236-55.

1947 Recent advances in the genetics of *Paramecium* and *Euplotes. Adv. Genet.* 1:263-358.

1948 The determination of hereditary antigenic differences in genically identical *Paramecium* cells. *Proc. Natl. Acad. Sci. U.S.A.* 34:413-18.

With A. LeSeur. Antigenic characters in *Paramecium aurelia* (variety 4): determination, inheritance and induced mutations. *Am. Nat.* 82:69-78.

1950 Methods in the general biology and genetics of *Paramecium aurelia. J. Exp. Zool.* 113:87-148.

Beyond the gene—two years later. In *Science in Progress*, ed. G. A. Baitsell, pp. 167-203. New Haven: Yale University Press.

1954 The relation of autogamy to senescence and rejuvenescence in *P. aurelia. J. Protozool*. 1:36-53.

1957 Breeding systems, reproductive methods, and species problems in protozoa. In *The Species Problem*, ed. E. Mayr, pp. 155-324. Washington, D.C. : American Association for the Advancement of Science.

1959 Kappa and related particles in *Paramecium. Adv. Virus Res.* 6:229-356.

1962 Does preformed cell structure play an essential role in cell heredity?

In *The Nature of Biological Diversity*, ed. J. M. Allen, pp. 165-221. New York: McGraw-Hill.

1965 With J. Beisson. Cytoplasmic inheritance of the organization of the cell cortex in *Paramecium aurelia. Proc. Natl. Acad. Sci. U.S.A.* 53:275-82.

1970 Methods in *Paramecium* research. In *Methods in Cell Physiology* , vol. 4, ed. D. Prescott, pp. 241-339. New York: Academic Press.

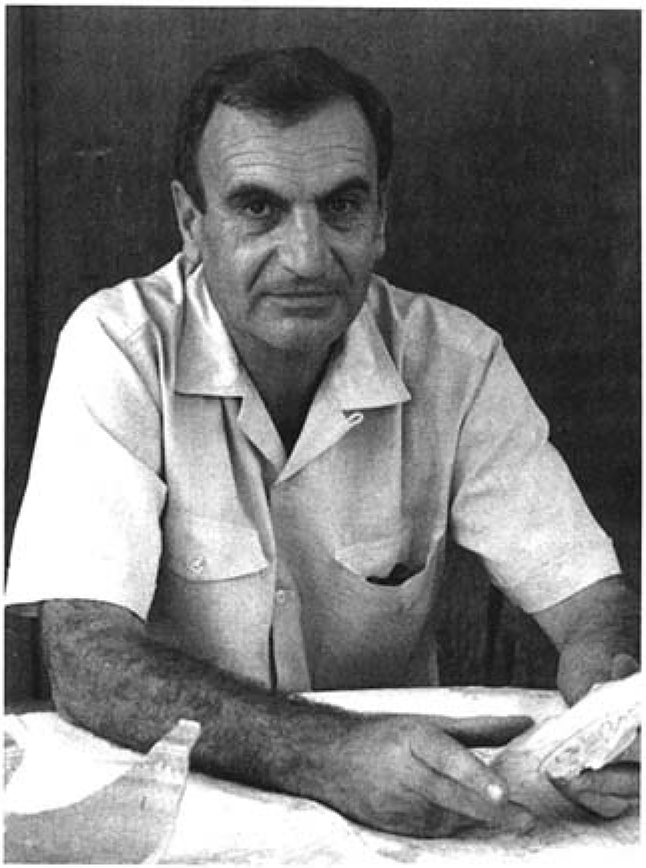
1974 *Paramecium aurelia.* In *Handbook of Genetics,* vol. II, ed. R. King, pp. 469-594. New York: Plenum Press.

1975 The *Paramecium aurelia* complex of fourteen sibling species. *Trans. Am. Micros. Soc.* 94:155-78.

1977 Local differentiations of the cell surface of ciliates: their determination, effects and genetics. In *The Synthesis, Assembly and Turnover of Cell Surface Components,* ed. G. Poste and G. L. Nicholson, *Cell Surface Reviews*, vol. 4, pp. 829-56. New York: Elsevier/North Holland.

1979 With M. V. Schneller. A genetic system for alternative stable characteristics in genomically identical homozygous clones. *Dev. Genet.* 1:21-46.

1980 With Y. Brygoo, A. M. Keller, R. V. Dippell, and M. V. Schneller. Genetic analysis of mating type differentiation in *Paramecium tetraurelia*. II. Role of the micronuclei in mating type determination. *Genetics* 94:951-59.



Alexander Sporh

Alexander Spoehr

August 23, 1913–June 11, 1992

DOUGLAS OLIVER

AMONG PERSONS TRAINED TO become scientists, there are some who excel in carrying out, and publishing, original and significant research, some who educate and inspire as teachers, some who provide and supervise opportunities for fellow scientists to conduct and publish their research, some who donate much of their time and energy to the benefit of their whole profession, and some who devote much of their time and talents to serving the wider community, local or national. There are, however, only a few anthropologists who have succeeded in two or three such roles and only two or three, in my memory, who have succeeded in all five; one of those was Alexander Spoehr.

First, a chronology of Alex's seventy-eight years of life.

He was born on August 23, 1913, in Tucson, Arizona. His father, Herman Augustus, was a biochemist and plant physiologist and a staff member of the Carnegie Institute. His mother, Florence (nee Mann), was a writer and a translator of Danish and German. Herman's forebears were Danish and German; Florence's were Austrian.

In 1920 the Spoehrs moved to Palo Alto, California, where Alex attended public schools and then Stanford, but after two and a half years at Stanford he transferred to the Uni versity of Chicago. There he earned an A.B. in economics but transferred to anthropology for graduate work, persuaded by lecture courses with Fay-Cooper Cole and A. R. Radcliffe-Brown. These latter, along with Manuel Andrade, Robert Redfield, and Fred Eggan, were singled out by him as his most influential mentors. Although his principal interest was, and remained, social anthropology, he gained experience in archeology during three summers of fieldwork—one at the Kincaid (Illinois) mounds and two in southwest Colorado. Under the supervision of Fred Eggan, he carried out his dissertation research among southeastern U.S. Indians, focusing on social change. In Oklahoma this involved salvage ethnography among some dispersed rural families; in Florida it was a functioning community of Seminoles.

In January 1940 Alex joined the staff of Chicago's Field Museum as assistant curator of American ethnology and archeology. In this position he had much to do with the design and installation of a new exhibition hall labeled "Indians Before Columbus," which was a radical departure from the previous practice of most U.S. museums of anthropology of stuffing their cases with artifacts, of storing them mainly for study by scholars. The new purpose, based on Rene d'Harnoncourt's exhibition, "Indian Art of North America," at New York's Museum of Modern Art, was more widely and specifically educational—the presentation of objects in their visual cultural contexts. For Alex a most fortunate bonus from work on the Field Museum project was its employment of Anne Harding, a talented exhibit designer who had worked with d'Harnoncourt on the New York exhibit; Alex and Anne were married in 1941. From this marriage were born two children: Alexander Harding and Helene (Dinsdale)—the former was to become administratively associated with native Hawaiian support organizations; the latter, an artist, now resides in Vermont.

During World War II, and after short tours of duty in the U.S. Marine Corps Reserve and the U.S. Army Corps of Engineers, Alex was commissioned in 1942 as a lieutenant in the Naval Reserve, where he served in air combat intelligence and mainly in air-sea rescue operations in the western sea frontier and central Pacific areas. His experiences in the Marshall, Gilbert, and Caroline islands led him to shift his anthropological interests from North America to the Pacific—a change that led to his appointment as curator of oceanic ethnology (including Southeast Asia) when he returned to the Field Museum in 1946. During the next eight years, he supervised reorganization of the museum's huge collection of Oceania objects, both for exhibiting and studying, and undertook two sessions of fieldwork: a sociological study of Majuro (Marshall Islands) and archeological and ethnological researches in the Marianas and Palau. Also, during this chapter of his professional life he did much teaching—one term at Harvard and regularly at the University of Chicago.

In January 1953 he moved to Honolulu to become director of the Bernice Pauhi Bishop Museum, the position having become vacant through the death of its part-Maori director, Sir Peter Buck (Te Rangi Hiroa). During the next nine years, Alex succeeded not only in rehabilitating that famous institution—financially, organizationally, and scientifically—and in improving its public educational function and community support, but he also served as member and sometime chairman of the Pacific Science Board (NRC); provided office space and other facilities for headquarters of the Pacific Science Association; served as one of two U.S. commissioners of the South Pacific Commission; and taught for one semester at Yale (like his Bishop Museum predecessors he held an ex officio professorship there).

In 1962, responding to the challenge of heading and pioneering a new, larger, and potentially more widely influential organization, Alex resigned his museum job and accepted the chancellorship of the Honolulu-based Center for Cultural and Technical Interchange Between East and West (subsequently abbreviated as the East-West Center). Two years later, however, he resigned because of power rivalries among the center's sponsors, but only after he had planned its initial structure and programs and had recruited some 1,500 persons to participate in those programs.

As to be expected for a person of his qualifications, Alex was soon offered high administrative positions in several mainland institutions, but he chose instead to accept a professorship in anthropology at the University of Pittsburgh, a position he held until retirement. While thus engaged he was coeditor (with G. P. Murdock) of the journal *Ethnology*, served a term as president of the American Anthropological Association as well as on several national scientific committees (e.g., NRC, NSF, Smithsonian), and was an outside member of the Harvard Overseers' Visiting Committee to the Peabody Museum and Department of Anthropology. In addition, he indulged his wish to return to research by undertaking archeological and ethnological surveys and intensive studies in the Philippines, which resulted in two book-length monographs and eight journal articles. Meanwhile, he was elected to the National Academy of Sciences in 1972.

In 1978, at age sixty-five, Alex retired from Pittsburgh and returned to the family's home in Honolulu, reportedly to rest. "Rest" consisted of observational study of the tool-using techniques of Japanese-American carpenters and archival research on the history of the Hudson's Bay Company in nineteenth-century Hawaii; the writing and publication of several journal articles; and service on several committees and trustee boards (e.g., of the Bishop Mu seum, the Western Pacific Regional Fisheries Council, the Hawaiian Historical Society). He was in the process of undertaking more archival research in Hawaii's fine libraries when, in 1990, his wife, Anne, suffered a crippling stroke, which required his daily full-time efforts and which undoubtedly contributed to the heart attack from which he died on June 11, 1992—just two weeks before the death of Anne.

So much for the chronology of Alex's extraordinarily full and multifaceted career. It remains now to describe how valuable it was scientifically and societally.

I begin with his associations with museums. During his employment at Chicago's Field Museum, Alex undertook, as mentioned earlier, to remodel and install some of the museum's vast, and largely storaged, American Indian collection into a public-oriented exhibition. That undertaking was, however, interrupted by his lengthy war service, mainly in Micronesia, which served to shift his anthropological interests to their peoples and, at war's end, to have his Field Museum position changed to curatorship of Oceanic Ethnology with responsibility over one of the very largest and most important collections of native Pacific objects in the world.

During the following seven years, in addition to carrying out that job (with extraordinary success in preserving, documenting, and exhibiting the collection), Alex engaged in field research in the Marshall and Mariana islands and joined forces with Fred Eggan to develop a program of ethnological research in the Philippines. However, just as that program was getting under way, he received a job offer that he described as "too exciting to resist"—namely, to become director of Honolulu's Bishop Museum.

The Bishop Museum was founded in 1889 by Charles Bishop in memory of his wife, Bernice Pauhi, last of the Kamehameha line of Hawaiian rulers. The museum had achieved an international reputation for its researches and publications on Pacific biology and anthropology but had made only token attempts to exhibit its rich collections, and had permitted access to its unique archival resources to only a few scholars. During his nine-year directorship, Alex changed all that and did much else besides.

He ended the museum's isolation by inviting the general public to become members of an association that was to participate influentially in the planning and operation of the museum's activities. Additionally, he originated an extensive and informative exhibition program, including periodically new displays in the museum itself, portable "Museums in Miniature" for traveling display among the several Hawaiian Islands, and support of a liaison teacher with the island government's education department to serve the public schools in matters respecting the museum's collections and activities. Other public educational endeavors initiated were the establishment of a planetarium and a bookshop that offered for sale not only the museum's own publications but also a very large inventory of books and pamphlets on Pacific science.

Another of Alex's firsts consisted of fund-raising. Previously, the museum and its meagerly paid staff survived mainly on the small proceeds of its original grant, having received only small grants from local foundations and occasional ones from wealthy philanthropists, including some who gave for personally accompanied expeditions. In contrast, Alex went out actively in search of funds. He began by seeking, and receiving, the museum's first grant from the Hawaiian Territorial Government—a sum of $25,000 to improve facilities for the care of the museum's collections. Even more important than that money itself was the precedent set, it having been the beginning of a continuing and growing source of government support for the museum.

Those and other monies that Alex succeeded in obtaining for the museum served not only to preserve and increase its collections, to enlarge and better compensate its staff, and to widen greatly its educational reach but also most importantly in the eyes of many scientists to provide sponsorship for more research throughout the Pacific. The largest-scale example of the latter was the Tri-Institutional Pacific Program (TRIPP), which was initiated in 1953 with a Carnegie Corporation grant of $100,000 for anthropological and linguistic research. Under general oversight of a steering committee consisting of Spoehr, Murdock (Yale), Leonard Mason (and the president of the University of Hawaii), and Harold J. Coolidge (NRC), more than a score of experienced scholars—anthropologists, linguists, historians, and political scientists—carried out field studies in places extending from Palau and New Britain to the Society and Marshall islands.

Other research programs initiated or sponsored by Spoehr were the Yale-Bishop Museum fellowships, a survey of the insects of Micronesia, the zoogeography of Pacific insects, several Hawaiian archeological digs, the natural and cultural history of the Honaunau (Hawaii Island) City of Refuge, and the Sulu Sea Expedition (in collaboration with the Philippines National Museum) for studies in zoology, history, and anthropology.

In addition to the above, Alex managed the day-to-day operations of the continually growing museum organization with great success. In the words of one long-time staff member who worked at the museum before, during, and after Alex's directorship:

> Dr. Spoehr was not only a scientist and scholar, he was a gentle person who was a most unabrasive leader, with the ability to delegate authority and at the same time the intelligence to stay in the background, provide support when asked for and await results. He was seldom disappointed. His ability

to empower his staff in this way was the key to transforming the Museum's program in a very short time.

Also, he found—or made—time to befriend and assist the increasing number of scientists passing through Honolulu en route to researches elsewhere. And with the gracious help of his wife, Anne, he established friendly and beneficial relations with many of Honolulu's most influential community leaders.

In view of Alex's unique combination of regional knowledge, administrative ability, professional expertise, and social-relational skills, it is not surprising that he was invited to become the first head—that is, chancellor—of the newly created Honolulu-based East-West Center. The idea for such a center was inspired by a few internationally minded faculty members of the University of Hawaii and was made possible, in 1960, by means of a grant-in-aid agreement, to be funded by Congress, between the U.S. Department of State and the University of Hawaii. Its stated objective was "the increase and improvement of mutual understanding among the countries of the Asian-Pacific area and the United States," with emphasis on the interchange of "persons, knowledge, and ideas"—a daunting challenge even to someone as hitherto successful as Alex Spoehr, who nevertheless accepted, later explaining: "After nine years at the Bishop Museum I felt I was growing stale at the job; and succumbing to a sense of adventure and with the blessing of my wife, I accepted the University of Hawaii Regents' offer and assumed my duties in January 1962."

Unfortunately for the fledgling center, but fortunately for anthropology, this chapter of Alex's life did not last long. He resigned the chancellorship, to become effective at the end of 1963. Some of his reasons for resigning may never be known; the most obvious ones included increasing tensions among the Center's three controlling bodies (i.e., the University of Hawaii, U.S. Department of State, and U.S. Congress) concerning goals, priorities, and, derivatively, the budgeting of funds—plus the perceived wish of some University of Hawaii administrators and faculty to exercise stronger and more direct control of the center's programs. Concerning the latter, Alex himself was outspokenly in favor of expanding the center's academic ties to include other Asian-Pacific-oriented universities on the U.S. mainland and to some leading universities in the Pacific and Asia as well—a predictably unwelcome proposal to some members of the University of Hawaii. Adding to those complications were the circumstances that the university itself was subject to political pressures from Hawaii's governor and legislature and that congressional control of the center was split among four separate committees. In other words, this stew had so many cooks, each with his own recipe, that its chancellor, the one responsible for preparing it, was permitted only to heat and stir.

Nevertheless, under Alex's brief chancellorship, the center became fully operational and structured in a way that might have become highly productive had not each succeeding set of leaders changed its course.

After this "challenging" but frustrating interlude, Alex and his wife needed, and took, a lengthy vacation through the South Pacific, and then returned to Honolulu to plan what he labeled the next "chapter" in their lives, which turned out to be a teaching position at the University of Pittsburgh.

Throughout his career, Alex had taught often at the University of Chicago and occasionally at Harvard and Yale and, upon leaving the East-West Center, he received other offers to teach. In the end he chose Pittsburgh, attracted partly by its promising innovations and partly by the presence on its faculty of two close friends, G. P. Murdock and

John Gillin. During his stay there, he taught a full schedule of courses and engaged in several other activities listed earlier. In 1978, at age sixty-five, he retired from Pittsburgh. Because his principal duty there was teaching, it is pertinent to assess his performance through the eyes of one of his most successful students, Richard Scaglion:

> Spoehr offered a wide variety of seminar courses, all of which were both comprehensive and extraordinarily well-organized. His area course on the Pacific, for example, included the geology, ecology, prehistory, history, and contemporary politics, as well as the ethnography and ethnology of the region, thus reflecting [his] own wide-ranging interests and expertise. Ever responsible to both student needs and contemporary directions, I remember how, on at least two occasions, he offered new seminar courses in direct response to student requests (the courses were "Maritime Adaptations" and "History of Anthropology").
>
> That he was greatly respected by students is evidenced by the fact that even [up to 1995] he had supervised more Ph.D. thesis than any other faculty member in the history of the Department.

Alex's fifteen years of retirement may have been "restful" in comparison with the thirty-eight "working" years of his professional life, but they were anything but leisured. (From his house on the green hills above Honolulu, he enjoyed a wide view of the Pacific, but I doubt that he ever sat on its beaches.) Among the many public services he performed were a term as a trustee of the Bishop Museum, membership on the Scientific and Statistical Committee of the Western Pacific Regional Fisheries Council, a consultancy to the newly founded Hawaii Maritime Center, plus very active membership in the Hawaiian Historical Society, including membership on its Board of Trustees and a term as its president.

"Retirement" also provided Alex with more time for research and writing, including an observational study of the tool-using techniques of local carpenters of Japanese descent and archival research on the nineteenth-century ac tivities of the Hudson's Bay Company in Hawaii. Other research projects were being planned when his wife's paralyzing illness required him to lay them aside.

Mention has already been made of some of the previous outside organizations and causes in which this otherwise fully employed man played voluntary, often leadership, roles. Perhaps most notable of these were the South Pacific Commission, the Pacific Science Association, and the American Anthropological Association (AAA). Limitations of space prevent a fuller description of that side of his life, but his service to the AAA deserves special mention.

The period of Alex's presidency of the AAA, in 1965, has been correctly characterized as the most crucial one of its history. The crisis arose when it became publicly known that a young anthropologist had been employed in a clandestine CIA operation, known as Project Camelot, in politically riven Chile. Social scientists throughout the United States became concerned by the disclosure, many of them holding that members of their profession ought not to engage in politically motivated activities that contradicted what they considered to be their moral responsibilities toward the peoples they studied. The AAA was especially concerned, and angrily divided, over the issue until Spoehr, then the association's president, commissioned a respected senior member, Ralph Beals, to investigate the matter and submit a report. Spoehr's initiative and the findings of that report resulted eventually in a code of ethics being adopted by the association—a document that doubtless contributed, nationwide, to a more ethically principled policy concerning the clandestine use of academics in government work in peacetime.

There remains to add some comments about Alex's principal research publications. In all he individually authored, coauthored, edited, or prefaced some 114 items, including short ethnographic or archeological reports, theoretical ethnological pieces, substantial ethnographic monographs, bibliographic surveys, and book reviews. Of these the most noteworthy are the monographs based on his own wide-ranging field researches. For an evaluation of these I have consulted anthropologists more familiar than myself with their subject matters, beginning with that of his dissertation research (1941, 1942, 1944, 1947). The following is a paraphrase of remarks by William Sturtevant of the Smithsonian Institution, who made a more recent study of those Indians, the Florida Seminole, observed by Alex in 1938-39:

> Spoehr was the first real anthropologist to study the Florida Seminole, his work among the *Oklahoma* Seminole was pioneering too, but others had been there before him. His field work in Florida was not lengthy, but he did manage to collect a good deal of very valuable data under very difficult circumstances. [The Seminole did not like to be "studied"; Sturtevant had some problems even in the 1950s.] The fact that Spoehr followed his Florida work with the study in Oklahoma was innovative, and may have been suggested by his mentor Eggan, who would do comparative kinship studies too. Spoehr's kinship data from Florida [were] valuable compared with [those] from Oklahoma, and since there was almost 100 years of fairly complete isolation between the two groups, it made an interesting study to see changes in terminology.

The major publication to result from Alex's study of Majuro (Marshall Islands) is listed in the Selected Bibliography (1949,1). About this I quote from Robert Kiste, director of the Center for Pacific Island Studies of the University of Hawaii, who has carried out intensive ethnographic fieldwork in the Marshalls:

> Spoehr provided excellent description and analysis of Marshallese social organization. He outlined the ideal system as Marshallese themselves describe it. They couch things in terms of a system of matrilineal clans and lineages with the latter being the landholding corporations. As things work

out in reality, however, residential extended families tend to be bilateral which reflects the system of land use rights. The children of males and affinal relatives have use rights [of] the land of their fathers and spouses, and thus matrilineality is not readily evident in the social units on the ground. Spoehr clearly understood all of this and his description is very clear. His description is of such quality that the reader can do an independent evaluation of the generalizations and conclusions that Spoehr offers. I don't know what else one could ask of the ethnographer. Spoehr did all of this before David Schneider and Kathleen Gough produced their monumental work on matrilineal kinship. The reader also gets a good feel for what daily life on Majuro was like—as with most atolls, boring.

About Alex's ethnographic studies in the Marianas, Kiste adds:

[It is] "top drawer." Because of the long period of colonial rule in the Marianas, Spoehr devoted about a fourth of his book on history. That was necessary to account for the nature of Chamorro culture as he found it in the late 1940s. I don't think anyone has subsequently written a better historical account. He also provides a good description and understanding of the Carolinian community on Saipan, and his outline of the ethnic relations between the Chamorros and Carolinians is also quite good. As with the Majuro book, we have good clear description, and one comes away from the work with the feeling [of having] a solid understanding of the place. I think [it] is a crucial work in that it would be very difficult to understand Saipan if we did not have this piece of work as a point of reference. Both works [of Majuro and Saipan] represent solid well-rounded ethnography, the holistic approach at its best.

The specialist consulted about Alex's archeological researches in the Marianas is Ross Cordy, who has conducted numerous archeological studies throughout Micronesia (and Hawaii):

Alexander Spoehr's 1949-50 work in the Marianas—primarily on Saipan and Tinian—included identification of a number of village sites, and important excavation work. These were the first modern archaeological excavations in Micronesia. His excavations contain careful description of soil layers in which artifacts were found and document features (post-holes, firepits, burial pits, etc.) within the layers. His findings were revolutionary

> for Micronesian research at the time. His excavations uncovered deep deposits, and he hypothesized two chronological phases of culture based on pottery types and on the presence/absence of stone house pillars (latte). More surprising, his radiocarbon dates—the first samples processed for Micronesia—placed initial occupation ca. 1500 B.C., a time depth far greater than any researcher had anticipated for the islands of Micronesia. These initial findings were roughly concurrent with the spread of. … linguists' findings, which also postulated a long time depth for Micronesia. Indeed, Spoehr's findings [together with] the linguists' molded many of our present ideas on the origins of Micronesian cultures. Today, the details of Marianas prehistory differ somewhat from those proposed by Spoehr, but few would disagree that the basic underpinnings of today's models owe much to Spoehr's initial work.

The most important publications to come from Alex's researches in the Philippines are *Protein from the Sea* (1980) and *Zamboanga and Sulu* (1973). An evaluation of the former was provided for this memoir by social anthropologist Richard Lieban (emeritus professor at the University of Hawaii, Manoa), who has carried out much fieldwork in and has written prolifically about the Philippines:

> As Spoehr observed when he wrote this monograph, anthropological interest in fishing and fishing communities in the Philippines and other parts of S.E. Asia had been slow to develop, and with regard to these areas there was a major disparity between anthropological knowledge of the use of land as opposed to the use of the sea. Spoehr's monograph helped to redress the balance.
>
> The monograph is a description and analysis of the technology and economic organization of the capture fishing industry in the Central Philippines. The fundamental problem addressed is technological change and its economic impact. A historical perspective is maintained throughout the monograph. Documentation of continuity and change in fishing equipment and procedures is a basic concern of the author, and his diligent and perceptive search for evidence in this regard is one of the strengths of the work.
>
> Five of the eight chapters of the monograph are devoted to fishing technology, which is described lucidly and comprehensively. In these chapters the author discusses small, middle and large scale enterprises. He finds

> that small scale fishermen have shown receptivity and ingenuity in adapting to change. In examining the dynamics of technological change in middle and large scale fishing endeavors, Spoehr is attentive to the relative importance of technical specialists (boat builders and master fishermen) and operators of fishing enterprises in the process. … Although the main emphasis of this monograph is on fishing techniques and its economic ramifications, a substantive chapter is devoted to fish markets in the urban centers. Sociocultural as well as economic dimensions of the exchange system receive attention in an informative description of how the markets work. … He originally planned a study of technological change in a simple Filipino fishing community. However, he soon realized that knowledge of a larger network of production and marketing was necessary to place a community study in appropriate perspective. The monograph … is a work of considerable scope that contributes significantly to knowledge of both local and broader aspects of a set of marine activities that are of fundamental importance in an archipelagic society.

For an assessment of Spoehr's archeological researches in the Philippines, I turned to another Philippines specialist in the anthropology department of the University of Hawaii, Manoa—Bion Griffin:

> *Zamboanga and Sulu* has had a bigger impact [than *Protein from the Sea*], as has the related archaeological excavations Spoehr undertook. He influenced a generation (no huge crowd, to be sure) of Filipino archaeologists at the National Museum of the Philippines. He encouraged the young scholars to take their studies seriously, to get into the field and dig, and to undertake serious research topics. He also was decidedly influential in his choice of Mindanao and Sulu as excavation locations. These places were considered real backwaters by Manila people; the awareness of archeological materials there led to further work in the south by the National Museum. In addition, [his] inquiry into historic/Muslim archeology was unique. Spoehr really complimented the influence of Robert Fox, who was the teacher and leader of all the Filipino archeologists, … [who was] largely untrained in archaeology … [and] who never wrote up anything. Spoehr provided a different model. I really see this as his Philippines legacy.

For a sampling of Alex's shorter but nevertheless significant writings, the reader is referred to the Selected Bibliog raphy, which will also provide an impression of the wide interests and talents of this remarkable man.

I WISH TO ACKNOWLEDGE, gratefully, information from the following individuals used in compiling this memoir: Steve Boggs, Ross Cordy, Barbara Dunn, Roland Force, Bion Griffin, Alan Howard, Marion Kelly, Yosihiko Sinoto, Robert Kiste, Richard Lieban, Roger Rose, Richard Scaglion, Alexander Harding Spoehr, William Sturtevant, and Stephen Williams.

Selected Bibliography

1941 *Camp, Clan, and Kin Among the Cow Creek Seminole of Florida*. Field Museum of Natural History, Anthropological Series, vol. 33, no. 1, pp. 1-28.

1942 *Kinship System of the Seminole*. Field Museum of Natural History, Anthropological Series, vol. 33, no. 2, pp. 29-114.

1944 *Florida Seminole Camp*. Field Museum of Natural History, Anthropological Series, vol. 33, no. 3, pp. 115-50.

1947 *Changing Kinship Systems: A Study in the Acculturation of the Creeks, Cherokee, and Choctaw*. Field Museum of Natural History, Anthropological Series, vol. 33, no. 4, pp. 151-235.

1949 *Majuro: A Village in the Marshall Islands*. Fieldiana: Anthropology, vol. 39.

The generation type kinship system in the Marshall and Gilbert islands. *Southwest. J. Anthropology* 5:107-16.

1950 Observations on the study of kinship. *Am. Anthropol*. 52:1-15.

1951 With G. I. Quimby. *Acculturation and Material Culture—I*. Fieldiana: Anthropology, vol. 36, no. 6, pp. 107-47. Time perspective in Micronesia and Polynesia. *Southwest. J. Anthropology* 8:457-65.

1952 With T. D. Stewart. Evidence on the paleopathology of yaws. *Bull. Hist. Med*. 26:538-53.

1954 *Saipan: The Ethnology of a War-Devastated Island*. Fieldiana: Anthropology, vol. 41.

1956 Cultural differences in the interpretation of natural resources. In *Man's Role in Changing the Face of the Earth*, ed. W. Thomas, pp. 93-102. Chicago: University of Chicago Press.

1957 *Marianas Prehistory: Survey and Excavations on Saipan, Tinian, and Rota*. Fieldiana: Anthropology, vol. 48.

1960 Port town and hinterland in the Pacific Islands. *Am. Anthropol*. 62:586-92.

1966 The part and the whole: reflections on the study of a region. *Am. Anthropol*. 68:629-40.

1967 A commentary on the study of contemporary Polynesia. In *Polynesian Culture History: Essays in Honor of Kenneth P. Emory*, ed. G. E. Highland et al., pp. 241-53. Honolulu: Bishop Museum Press.

1968 Technical innovation and economic development: Basnig fishing boats of Zamboanga. *Philipp. J. Sci*. 97:77-92.

1973 *Zamboanga and Sulu: An Archaeological Approach to Ethnic Diversity* . Ethnology Monographs No. 1. Department of Anthropology, University of Pittsburgh, Pittsburgh.

1976 With N. A. Cuyos. The fish supply of Cebu City: a study of two wholesale markets. *Philipp. Q. Culture Soc*. 4:160-98.

1978 Conquest culture and colonial culture in the Marianas during the Spanish period. In *The Changing Pacific: Essays in Honour of Harry E. Maude*, ed. W. N. Gunson, pp. 247-60. Melbourne: Oxford University Press.

1980 *Protein from the Sea: Technological Change in Philippine Capture Fisheries*. Ethnology Monographs No. 3, Department of Anthropology, University of Pittsburgh, Pittsburgh.

1981 Lewis Henry Morgan and his Pacific collaborators: a nineteenth century chapter in the history of anthropological research. *Proc. Am. Philos. Soc*. 125:449-59.

1983 With H. Goto and K. Sinoto. Craft history and the merging of tool traditions: carpenters of Japanese ancestry in Hawaii. *Hawaii. J. Hist*. 17:156-84.

1984 Change in Philippine capture fisheries: an historical overview. *Philipp. J. Culture Soc*. 12:25-56.

1986 Fur traders in Hawaii: the Hudson's Bay Company in Honolulu, 1829-1861. *Hawaii. J. Hist*. 20:27-66.

Eliot Stellar

November 1, 1919–October 12, 1993

JAY SCHULKIN

ONE OF THE FOUNDERS of what we now call behavioral neuroscience died recently. Eliot Stellar was seventy-three years old at the time of his death and was university professor of physiological psychology at the University of Pennsylvania. He was a former editor of *The Journal of Comparative and Physiological Psychology*, the precursor of behavioral neuroscience. During that time the journal grew in stature as the field rapidly expanded. He led the journal as he did most things, fairly and with catholic sensibilities. He championed individual initiative and expanded the possibilities for others to be included in physiological psychology. As editor and as an individual, he demonstrated the art of inclusion: namely, he brought diverse individuals to participate in the inquiry of the role of the brain and behavior.

In our lifetimes most of us meet few truly great people. Eliot Stellar was for me, and many others, one of them. His particular genius was to nurture both scientific excellence and humane expression. In fact, Eliot Stellar is the paradigmatic example of the statesman-scientist. His example inspired others as they tried to pursue science. The greatness of Eliot Stellar is that he nurtured the science that one was pursuing, and, perhaps more importantly, he bolstered the life that one ought to be living.

Who was Eliot Stellar? Eliot was born and raised in Boston. He attended the Boston Latin School and Harvard College. At Harvard he heard lectures by Karl Lashley and the philosopher Alfred North Whitehead. At Harvard he began his inquiry into relationships between the brain and behavior. Clifford Morgan was at Harvard at the time, and Eliot began to work with him. This culminated in a paper on symbolic representation in the rat and the role of the neocortex (1942).

Eliot Stellar then attended Brown University and received his advanced degrees in psychology, under the tutelage of Professor Hunt. From Hunt, Eliot's interest in motivation was engendered. This interest in motivation was lifelong for him.

After a stint in the Army during the war, Eliot took a position at Johns Hopkins as an assistant professor of psychology. Clifford Morgan was chairman of the department and was instrumental in hiring Eliot. During this period the two of them worked on the second edition of *Physiological Psychology* (1950). It radically extended and improved on Morgan's first edition and became the main text in physiological psychology for the next twenty-five years.

Of Eliot's many students during his Johns Hopkins years, three stand out. One is Robert MacCleary, the second is Philip Teitelbaum, and the third is Alan Epstein. It was a great period for Eliot and for physiological psychology. MacCleary's thesis was on the role of specific hungers and the differential contribution of taste and postingestive mechanisms in determining ingestion. Stellar and Phil Teitelbaum's work was on the lateral hypothalamic syndrome and recovery of function from this brain damage (1954). Alan Epstein, an undergraduate in Eliot's laboratory, worked on the prob lem of sodium appetite. He and Stellar demonstrated (1955) that the appetite for sodium was innate, a finding that Curt Richter also had postulated.

Richter was also at Hopkins, having founded the first laboratory in psychobiology in this country there. Richter's influence on Eliot Stellar was enormous. Richter never really had any students and worked largely alone. But Eliot quickly saw that Richter's concerns were on a continuum with his own—namely, the way in which behavior served in the regulation of the internal milieu. The appetite for sodium was an example of how behavior served to regulate the needs of the body. Both Richter and Stellar wanted to know how the brain served to initiate and integrate behavioral responses that served the body. For Eliot Stellar the biological basis of motivated behavior was pervasive and amenable to study; basic drives for minerals, water, or the sexier one—namely, the motivation for sex—served as model systems in which to study how the brain produced motivated behavior to serve bodily needs.

Eliot Stellar's classic paper was titled "The Physiology of Motivation" (1954). It was a seminal work that dominated the field for over thirty years, integrating what was known about hypothalamic function in regulating basic drives like hunger and sex into a model of brain function. It oriented basic research to a tremendous degree and is now noted as one of the most cited papers in psychology.

But Eliot did not align himself with the tradition of elegant and rigorous experimental design that was emerging from psychology. The tradition of Richter and Stellar is less about design and more about biology. While the experiments were perhaps less elegant, they were tied to real-world events. Statistics were never the determining factor; large phenomena serving biological ends were.

Eliot Stellar was also an inventor, having made an impor tant contribution to stereotaxic surgery with the introduction of his stereotaxic apparatus. It was a simple innovation of a technique that made a major difference in the field. His early work with Hill on the lick rates of rats was about how the hardware of systems worked (1952): How many licks could the rat generate? When did it decline? How does motivation for thirst interact with it? These were Eliot's questions.

Eliot moved to the University of Pennsylvania (1954) under unfortunate circumstances. he was told that the Department of Psychology at Hopkins could not house both him and Clifford Morgan, so he began to look around for another job. At that time Penn was in the midst of recruiting faculty for something brand new—the Institute of Neurological Sciences. Lewis Flexner was the chairman of Anatomy at Penn and the director of the institute. After a few minutes of conversation, he hired Eliot as the behavioral person in the group. At that time one could still do that.

Thus began a wonderful period for Eliot and for the University of Pennsylvania. Within a short period, the Institute of Neurological Sciences and the Department of Anatomy came to house very special scientists who worked well with one another in the new field (e.g., Bill Chambers, John Liu, Jim Sprague) that we now call neuroscience. Interesting work on memory and attention appeared within a short time (1961,2; 1963). The inquiry was oriented to what we now call behavioral neuroscience. Each was reductionistic but without reducing behavior from the purview of what was to be explained. Behavior was one level of analysis among others, such as anatomy and physiology. Eliot's role was as the "behaviorist." Of course, he was no behaviorist, either in Hull's or Skinner's sense. What they meant was that his focus was on behavior, on how the brain regulated it, and how behavior influenced the brain.

Eliot had a number of students in behavioral neuroscience at Penn (e.g., Douglas Mook, who went on to the University of Virginia, John Corbitt to Brown University). It is not surprising that an award in behavioral neuroscience named for Eliot Stellar was established at Penn for the best thesis in behavioral neuroscience.

Eliot's role at the University of Pennsylvania was a large one. At one point he was head of the Institute of Neurological Sciences, provost of the university, and then at the end of his life chairman of the Department of Anatomy. He helped cultivate the Department of Psychology into one of the best departments in America and with a strong biopsychology group. He also initiated a number of educational programs at the university. They included the University Scholars Program, Biological Basis of Behavior Major, and scholarship programs that reached out to universities in Europe, Asia, and the Middle East. Students and scholars were both coming to Penn under Eliot's encouragement or going to some place. His sense of scholarship and science was one that knew no borders. The programs of scholarship that he established at Penn reflected this fact. And they always had one important property; they reached out to people

Eliot in his elegant manner ran a number of seminars. One that he helped run for almost forty years at the university was something he called the "feeding seminar." It was founded by Eliot and Mickey Stunkard in the mid-1950s and is still going on. It brings together a broad base of scholars to discuss over lunch the mechanisms of ingestive behavior.

Eliot had great colleagues that championed behavioral neuroscience at Penn. They included Vincent Dethier, who also taught at Penn, Princeton, and the University of Massachusetts at Amherst. He died several weeks before Eliot at age seventy-eight. They wrote a book (1961) together that represented a comparative approach to behavioral neuroscience. I can remember the debates between Eliot Stellar and Vince Dethier about whether the concept of motivation was necessary in the explanation of behavior. If there was a concept that Eliot thought necessary in the explanation of behavior it was that of motivated behavior. Motivation was a central state for Eliot in the sense in which Karl Lashley, his teacher, had envisioned it.

Eliot Stellar did not publish many papers, but what he did publish made a profound difference. With his son, Jim Stellar, now a dean at Northeastern University, he wrote a book titled *The Neurobiology of Reward and Punishment* (1985).

As I have indicated, beyond his academic and administrative roles, Eliot had a wonderful way with people and a capacity to nurture inquiry and scientific cooperation. Someone working with me on a program project grant from the National Institute of Mental Health asked what Eliot Stellar would contribute. The answer I think is that he civilized us. Eliot was always the impetus for the team spirit in inquiry. That was one of his gifts. He loved to see inquiry thrive.

At the end, he was busy on two major fronts—one as head of the Committee on Human Rights at the National Academy of Sciences. Earlier he had worked for the Committee on the Ethics of Medical Research in Washington, and his interest in human rights was a long-standing one. He prized his work on this committee. They labored to free other scientists abused and in prison around the world. This work and the bonds of the community of scientists were major themes in Eliot's life. Scientists form a community, and this community needs to bond together. After all, both rights and inquiry were formed during the scientific enlightenment period in culture.

The other activity was as president of the American Philosophical Society, the oldest intellectual society in America.

It was founded by Benjamin Franklin and is devoted to what Franklin called "practical philosophy." Eliot loved the work at the society. It was, after all, what his life was devoted to—the expression and cultivation of inquiry, the bringing together of people to pursue that noble end.

Eliot Stellar served the community of inquirers in so many ways. And he was a political man, not just because of the work he did at the University of Pennsylvania but also that of the boards he was on. He cultivated science at the National Institutes of Health, and he was on the board of foundations that he helped orient to behavioral neuroscience—the MacArthur and Whitehall foundations.

Let me end with several personal notes. As a graduate student and still in the philosophy department, I came to see Eliot Stellar on the advice of Paul Rozin, who then was chairman of the psychology department. I was not sure where I fit in the intellectual arena at that time. Eliot had the gift to lift the spirits of those around him. I walked out of his office feeling that, despite the fact that I did not dovetail nicely under the rubric of any department, it was legitimate to pursue inquiry, and he backed me then and right up until he died.

I was Eliot Stellar's last student. I worked with him, published one paper with him (1985), and we were faculty members in the same department over a number of years. I went to Penn because my science teacher (George Wolf) told me as a undergraduate to go there because Eliot Stellar, he thought, would appreciate me. He did. How lucky I was.

Eliot's large imprint is on the people he cultivated and his work for the community. A world without Eliot Stellar is a world with one less smiling face.

Selected Bibliography

1942 With C. T. Morgan and M. Yarosh. Cortical localization of symbolic processes in the rat. *J. Comp. Physiol. Psychol*. 34:107-26.

1950 With C. Morgan. *Physiological Psychology*. New York: McGraw-Hill.

1952 With J. H. Hill. The rat's licking rate of drinking as a function of water deprivation. *J. Comp. Physiol. Psychol*. 45:96-102.

1954 The physiology of motivation. *Psychol. Rev*. 61:5-22.

With P. Teitelbaum. Recovery from the failure to eat produced by hypothalamic lesions. *Science* 10:894-95.

1955 With A. N. Epstein. The control of salt preference in the adrenalectomized rat. *J. Comp. Physiol. Psychol*. 48:167-72.

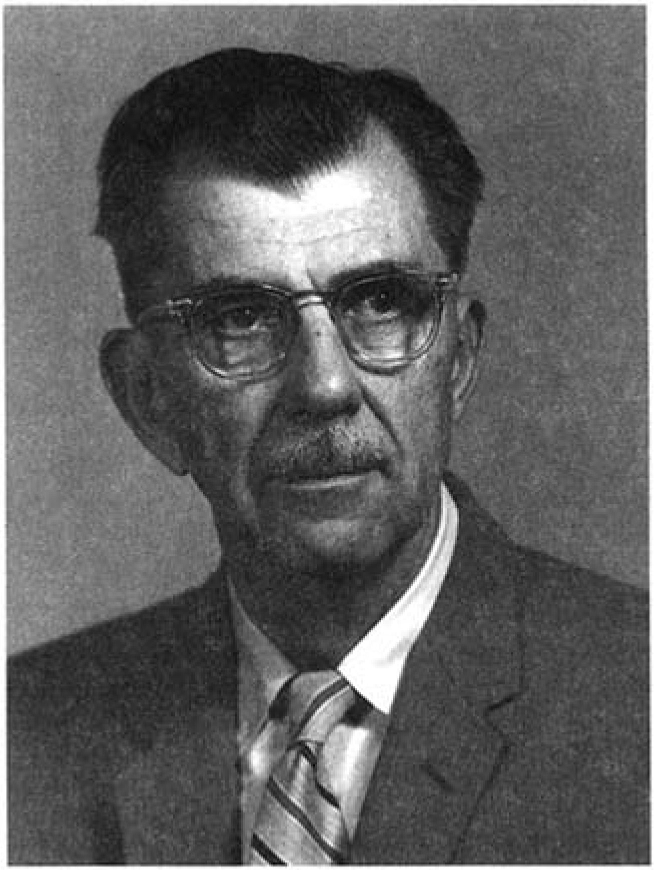
1961 With V. G. Dethier. *Animal Behavior*. Englewood Cliffs, N.J.: Prentice-Hall.

With J. M. Sprague and W. W. Chamber. Attentive, affective and adaptive behavior in the cat. *Science* 133:165-73.

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

1985 With J. Schulkin and P. Arnell. Running to the taste of salt in mineralocorticoid treated rates. *Horm. Behav*. 19:413-25.

With J. R. Stellar. *The Neurobiology of Motivation and Reward*. New York: Springer-Verlag.



Courtesy of the University of Illinois

Julian H. Steward

Julian Haynes Steward

January 31, 1902–February 6, 1972

ROBERT A. MANNERS

JULIAN HAYNES STEWARD, ANTHROPOLOGIST, was born in Washington, D.C., the son of Thomas G., chief of the Board of Examiners of the U.S. Patent Office, and Grace Garriott, whose brother, Edward Garriott, was chief forecaster of the U.S. Weather Bureau.

In an autobiographical sketch prepared for the National Academy of Sciences, Steward remarked that nothing in his family background or in his early education accounted for his later interest in anthropology. On the other hand, his school and neighborhood in the suburbs of Washington involved him in close association with the children of writers, senators, representatives, doctors, and "generally persons of some distinction" who apparently did contribute to a developing interest in intellectual matters.

When he was sixteen, Steward was admitted to the newly established Deep Springs Preparatory School (now Deep Springs College), a school located near Death Valley and devoted to the development of practical skills and to the promotion "of the highest well-being." At this time, he said

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somewhat laconically, "I took this purpose seriously but did not know what to do about it." His time at Deep Springs exposed him to the lifeways of the local Paiute and Shoshoni Indians, an experience that lay partly dormant until his freshman year at the University of California, Berkeley, where he discovered academic anthropology in a course given jointly by Alfred Kroeber, Robert Lowie, and Edward Gifford. The following year he transferred to Cornell where, in the absence of an anthropology faculty, he completed his undergraduate training in zoology and geology. Livingston Farrand, then president of Cornell and himself an anthropologist, nurtured Steward's continuing interest in anthropology—temporarily sidetracked by circumstances—and urged him to return to Berkeley and its reigning triumvirate for his doctorate in anthropology.

In 1928 Steward joined the faculty at the University of Michigan, where he gave the first course in anthropology ever given there. In 1930 he went to the University of Utah, where he taught and conducted considerable archeological research in Puebloid cultures until 1933. Accompanied by his wife, the former Jane Cannon, he spent the next year (1934) conducting research in Owens Valley, Death Valley, and northward through Nevada to Idaho and Oregon. In 1935 he left university teaching to take a position as associate anthropologist in the Bureau of American Ethnology of the Smithsonian Institution, remaining there until 1946. During one year of his tenure at the BAE, he was loaned to the Bureau of Indian Affairs at the request of its director, John Collier, and assisted in the creation of programs for the reform of the BIA. The product, a radical transformation in the organization and functioning of the BIA, is usually referred to as a New Deal for American Indians. The experience was valuable, for it was there that Steward had a chance to examine the effectiveness of a fairly well-financed program in applied anthropology and to observe at firsthand the practical as well as the theoretical significance of the relation between subcultures and the larger society of which they were a part—an issue that occupied a dominant place in his teaching, research, and writing for the remainder of his life and is the central theme of such major works as *Area Research: Theory and Practice, The People of Puerto Rico*, and the three-volume *Contemporary Change in Traditional Societies*, as well as a number of shorter pieces dealing with the study of nonisolated, nonself-sufficient cultures or part-cultures.

While at the BAE he set up and was the first director of the Institute of Social Anthropology, a branch of the Smithsonian Institution. During his last years at the BAE, Steward chaired a committee that reorganized the governance of the American Anthropological Association. He was also involved in the planning and establishment of the National Science Foundation and was instrumental in persuading Congress to appropriate funds for the creation of the Committee for the Recovery of Archeological Remains, subsequently the nation's River Basin Archeological Surveys Program, often referred to as the model and stimulus for salvage archeology in the United States.

In partnership with Wendell Bennett, Steward planned and helped to establish the Viru Valley Project in Peru, a research program whose contributions to theory in archeology and especially to the archeology of South America have been of major significance.

On the whole, and despite the wealth of Steward's contributions recorded during his years at the BAE, it is generally agreed that his most concretely impressive achievement during his tenure was the organization, staffing (over 100 scientists were involved), and editorship of the six-volume *Handbook of South American Indians*. Despite its imperfections, it remains a monument to Steward's sustained efforts to identify links between what he saw as culture types and the evolutionary schema toward which his research had clearly inclined him—an evolutionary design that eschewed the form of unilinear stages emphasized in the earlier work of Lewis Henry Morgan as well as the updated and amended evolutionary design proposed by Leslie White. Steward called his schema multilinear evolution, an approach that paid special attention to the varieties of ecological, technological, and historical circumstances exposed by expanding global research. It is "essentially a methodology based on the assumption that significant regularities in culture change occur, and it is concerned with the determination of cultural laws."

Although Steward was always identified as a cultural anthropologist, his publications in archeology constituted about half of his output in the period from the 1920s to about 1940. This may help explain, in part, his persistent fascination with evolutionary formulations extending over long periods of time. He maintained that the line between the subdisciplines of archeology and cultural anthropology was largely artificial, referring to the data of archeology as ethnohistory on (or in) the ground. Since he believed that archeology was more than potsherds and monuments, test pits and stratigraphy, he urged Gordon Willey, against Willey's wishes, to deal intensively with settlement patterns in the Viru Valley Project, which Steward helped launch.

He prevailed on Willey by insisting that intensive study of settlement patterns in the valley would show "when and how these patterns changed through time and what the changes implied" (Willey, p. 216). Willey's work set a pattern for archeological research that grew virtually to dominate the field in later years. Steward had himself signaled the importance of such analysis in "Ecological Aspects of

Southwestern Society" (1937), a research thesis that, until publication of Willey's Viru Valley work, largely had been unnoticed.

Throughout his professional life Steward carried on his search for cross-culturally valid regularities. In effect, he saw his anthropological mission as a search for causes. And while he was appropriately cautious about spelling out laws or ineluctable causes, he was not immune from the criticism of those who dismissed the search for cultural regularities, citing diffusion as an argument against Steward's evolutionary propositions. He responded to these objections by drawing attention to the force of cultural ecological factors in determining when, where, how, and if diffusion of cultural items or artifacts could take place, thus making diffusion an aspect of cultural evolution, a dependent rather than an independent variable. Steward pressed on with his search for what may be referred to as middle-range generalizations or, more daringly, analysis and inference with predictive potential.

In short, Steward "minimally hoped that anthropologists would accept the position that culture is an orderly domain in which causality operates, and [its] operation is accessible through scientific method. Given the complexity of our subject matter, this may have been a naive expectation, but to Steward these were the unstated premises which underlay the rest of his theories" (Murphy, p. 10).

In 1946 Steward accepted a professorship at Columbia University, entering at a time when the influence of Boas still dominated the program and when the small department (six full-time and several adjunct staff members) was deluged by an influx of 120 graduate students, overwhelmingly G.I. Bill recipients. Steward remained at Columbia until 1952, when he left to take a position as University Professor at the University of Illinois.

During his years at Columbia, Steward supervised some thirty-five doctoral dissertations and served on the committees of several dozen others. In all, he influenced a large number of students, many of whom later came to occupy senior professorships in universities around the country.

While at Columbia Steward planned and supervised the preparation, fieldwork, and write-up activities of five graduate students in the execution of the discipline's first attempt to study the culture of an entire area. He chose the island of Puerto Rico. The student team prepared for the enterprise with a semester seminar on the history, economy, polity, social structure and dependency constraints, and opportunities in Puerto Rico from just before initial contact with the Spanish conquerors in 1492 through the period of American control and into the late 1940s. The fieldwork was conducted from the end of 1947 to August 1949. It was during this period that Steward completed work on *Area Research: Theory and Practice* (1950). Publication of the team enterprise, *The People of Puerto Rico*, was delayed until 1956. It is still reckoned one of the several significant contributions that mark Steward's eminence among anthropologists in the middle years of the twentieth century.

At the University of Illinois, Steward conceived and executed an even more ambitious research effort to document "the processes of change in peasant agricultural systems that have been exposed to outside markets and wage labor" (Murphy, p. 12). To this end he established a program called "Studies in Cultural Regularities." With a grant from the Ford Foundation, Steward selected eleven field workers who were assigned to test the theories developed in the Cultural Regularities program; one field worker was assigned to Nigeria, one to Mexico, two to Peru, one to Kenya, two to Tanganyika, one each to Burma and Malaya, and two to Japan. The fieldwork was carried out between

1957 and 1959, and the results, *Contemporary Change in Traditional Societies*, were published in three volumes in 1967.

The Puerto Rican project and the work that grew out of the program in Studies in Cultural Regularities were guided by research principles that marked all but the very earliest of Steward's research activities. He combined induction with deduction, moving from hunches stimulated by reading and observation and advanced by certain "logical inferences" to create a hypothesis. He did not see the field as a place where one went to record as carefully as possible a general description of a culture. Rather he was among the earliest anthropologists to go into the field guided by a firm set of problems, a set of deductive hypothesis to be tested by examination of documentary and archival resources and by induction, by the careful collection of data in the field. The cross-cultural comparisons on which his work placed such great emphasis were a calculated test and attachment of the hypothesis/problem-oriented fieldwork for which he and his students had prepared.

Steward's significance in the history of anthropology derives from a number of innovative ideas and practices, many of which helped to determine major developments in research methodology. He will be remembered for his "theory of cultural ecology," a theory that Murphy called his "greatest contribution to anthropology." Other anthropologists had dealt with the shaping force of environmental factors (Kroeber, Wissler, etc.). But it remained for Steward to emphasize the importance of culture and its effects on the environment, in a sense to relegate the natural habitat to the role of dependent variable in determining the lifeways of the group, society, or region. Consequently, Steward was most impatient with those anthropologists who used the terms "environment" and "ecology" interchangeably. The theory and method of cultural ecology goes beyond the influence of the natural habitat, or it postulates a relationship among the resources of a particular environment, the technology (tools and knowledge) available at a particular time to exploit these resources and the patterns of work designed to bring the technology to bear upon the resources. "The organization of work, in turn, is hypothesized as having a determinative effect upon other social institutions and practices. The key element in the equation is not the environment" (Murphy, p. 22).

Despite his devotion to the search for causes and regularities in processes of culture change, Steward remained generally indifferent to the premises and promises of applied anthropology because he was keenly aware of the differing values, theoretical positions, and conflicting prescriptions for social action that criss-crossed the discipline. And he was acutely sensitive to the gaps in our understanding of process in the genesis and decline of specific sociocultural phenomena. Finally, he was appropriately cynical about the uses of admonition divorced from the exercise of power.

Steward is generally credited with introducing a few conceptual terms *de novo* into the anthropological lexicon—for example, "multilinear evolution" and "levels of sociocultural integration." His name is also associated with the refinement and popularization of other concepts now widely employed in anthropology, such as the "search for regularities," "cultural causality," and the significance of "the larger context," i.e., forces and influences from outside the locus of research that must be reckoned with as significant determinants of local change and/or persistence.

He persuaded most of his colleagues to replace the stultifying "culture area" concept with the concept of "culture type." And he participated in a generally successful revolt against the restrictions of historical particularism and the perversion of cultural relativism from methodological tool to an immutable principle of identification. He also fought to keep anthropology within the "sciences," for he saw its mission as the search for explanation rather than the hopeless pursuit of immutable truths.

In 1952 Steward was awarded the Viking Fund Medal in General Anthropology, a distinction Alfred Kroeber had predicted a couple of years earlier when he referred to Steward's outstanding contributions to anthropology and added that he believed Steward to be the "finest teacher in our field in the past 20 years." In 1954 he became one of the earliest scholars outside the hard sciences to be elected to the National Academy of Sciences. In 1956-57 Steward went to Japan as director of the Kyoto American Studies Seminar. In 1960-61 he was appointed a fellow of the Center for Advanced Study in Behavioral Sciences at Palo Alto. And when the University of Illinois established its own Center for Advanced Study, Steward was one of the four initial appointees (and the only social scientist) out of a faculty numbering about 4,000.

By way of commemorating Steward's sixtieth birthday, twenty-six of his colleagues and former students honored him with a Festschrift: *Process and Pattern in Culture* (1964). And in 1969 a group of graduate students from the University of Illinois anthropology department launched a twice-yearly publication, *The Steward Anthropological Society Journal*.

Because Steward was diligent in the use of empirical data in his theoretical formulations, a few critics have labeled his results inductive or empirical generalizations. Although he was uncommonly sensitive at times, he considered these charges vacuous, remarking that it was self-evident that no theory springs fullblown out of a dataless vacuum. Steward used the empirical data derived from his own research and that of others as a catalyst for the imaginative leap that would offer an explanation that went "beyond the facts." In short, he said he could construct theory in the only way possible—by affirming the inescapable value of facts but not binding the scope of explanation exclusively to those facts.

NOTES

1. During the war the Office of Naval Research funded a number of projects designated "Studies of Culture at a Distance." These were generally defined as culture and personality studies and did indeed attempt to characterize national cultures by exposing the dominant patterns or the ethos of each country as revealed in the course of interviews with expatriate citizens of these nations then living in the United States. Ruth Benedict, Sula Bennett, Margaret Mead, and others associated with Columbia were participants in these activities, which, unlike Steward's program, did not involve fieldwork.
2. The British anthropologist Max Gluckman used the term "social field" to describe the same phenomenon, notably in a couple of essays, "Malinowski's Sociological Theories," first published in the late 1940s.

FURTHER READINGS

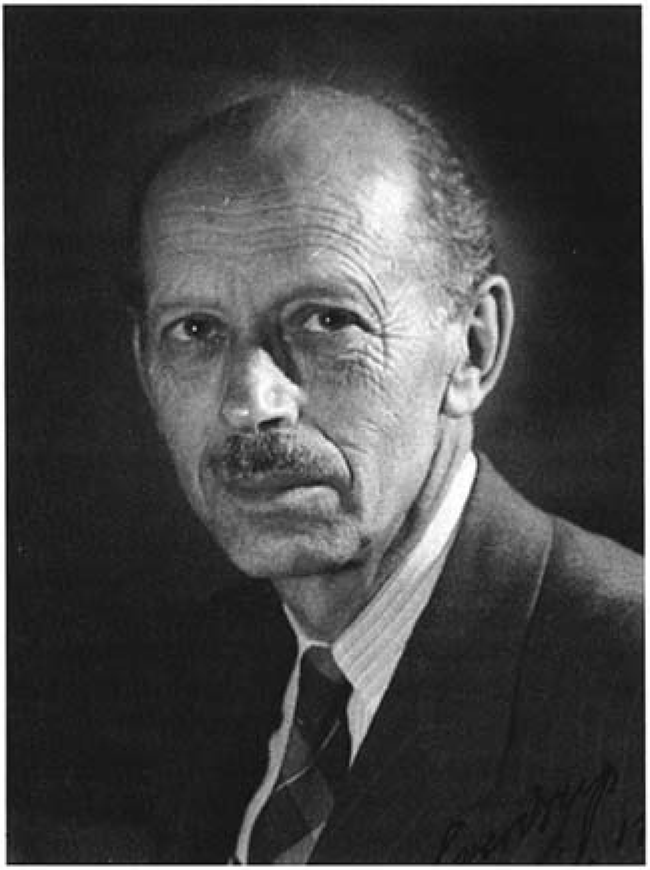
Steward's papers, including copies of an extensive correspondence (1926-73), are in the University of Illinois Archives. Brief biographical entries may be found in *Who Was Who* (vol. 5), the *International Dictionary of Anthropology* (1991), and the *International Encyclopedia of the Social Sciences* (vol. 18). A complete bibliography of Steward's work appears as an appendix to his Obituary, Manners, Robert A. and Jane C. Steward, *American Anthropologist* (75:886-903). A substantial but slightly less exhaustive bibliography, since it appeared in 1964, is in *Process and Pattern in Culture: Essays in Honor Of Julian Steward*, ed. Robert A. Manners, pp. 418-24.

Of a number of bibliographical essays honoring Steward, four, in particular, are noteworthy for the personal information they provide along with striking analytical and criti cal insights dealing with his work: "Julian Haynes Steward" in *Portraits in American Archeology: Remembrance of Some Distinguished Americans* , Gordon R. Willey, 1988, pp. 218-41; "Julian H. Steward: A Contributor to Fact and Theory in Cultural Anthropology" in *Process and Pattern in Culture: Essays in Honor of Julian H. Steward*, Demitri B. Shimkin, pp. 1-17; "Julian Steward's Writings and the Essays: A *post hoc* Articulation," ibid., pp. 18-25; "Introduction: The Anthropological Theories of Julian H. Steward," by Robert Murphy in *Evolution and Ecology: Essays on Social Transformation; Julian Steward*, ed. Jane C. Steward and Robert T. Murphy, 1977, pp. 1-39. This posthumous publication and Steward's *A Theory of Culture Change* (1955) together contain some of Steward's more noteworthy essays, culled with care by Steward himself in the 1955 publication and with respect and insight in the 1977 book of selections arranged by his wife and one of his most distinguished students.

Selected Bibliography

In addition to the several titles referred to in the body of this piece, at least a few more of Steward's many papers demand inclusion in order to document the significance of his contributions to anthropological thought and method. These have been chosen with studied arbitrariness (from a body of more than 200 publications) and are presented in chronological order.

1937 The economic and social basis of primitive bands. In *Essays on Anthropology in Honor of Alfred Louis Kroeber*, ed. R. H. Lowie, pp. 311-50.
1938 Ecological aspects of southwestern society. *Anthropos* 32:87-104.
1941 *Basin Plateau Aboriginal Sociopolitical Groups*. Bureau of American Ethnology, Bulletin 120.
1943 Determinism in primitive society? *Scientific Monthly* 53:491-501.
1947 Acculturation and the Indian problem. *America Indigena* 3:323-28.
1951 American culture history in the light of South America. *Southwestern Journal of Anthropology* 3:85-107.
1956 Level of sociocultural integration: an operational concept. *Southwestern Journal of Anthropology* 7:374-90.
1969 Cultural evolution. *Scientific American* 194:69-80.
Limitations of applied anthropology: the case of the American Indian New Deal. *Journal of the Steward Anthropological Society* 1:1-17.



Harald Ulrik Sverdrup

November 15, 1888–August 21, 1957

WILLIAM A. NIERENBERG

A BIOGRAPHY OF Harald Ulrik Sverdrup written for consumption in his native Norway has a different perspective than one written for the National Academy of Sciences. In the United States, Sverdrup is recognized as the founder of the modern school of physical oceanography. A great scientist and "father" of a school is recognized by the number and fame of his students and colleagues. In Sverdrup's case the list includes Robert S. Arthur, John Crowell, Dale Leipper, Richard Fleming, Walter Munk, and Roger Revelle. This distinguished group formed the nucleus of the development of the science of physical oceanography in the United States, which, before Sverdrup, had been simply a punctuated, part-time effort by a few individuals. This lifetime achievement received most unusual recognition by the naming of an oceanographic term after this great scientist, the Sverdrup, "a unit of volume transport equal to one million cubic meters per second."[1] The American Meteorological Society honored him with the Sverdrup Gold Medal, which recognizes researchers for outstanding contributions to the scientific knowledge of interactions between the oceans and the atmosphere. A building bearing Sverdrup's name is on the campus of the Scripps Institution of Oceanography.

This memoir is being written during the fiftieth anniversary of the publication of *The Oceans*, when ceremonies marking this event are being prepared. Sverdrup was the principal author along with colleagues Martin Johnson and Richard Fleming, and his chapter XV on the oceanic currents is still the most recent publication that treats all the world's oceans in one work. This remarkable text not only marked the onset of modern oceanography but also survives as a leading source today. In the field of science this longevity is almost unique.

In Norway, Sverdrup is not only recognized as a great scientist but also as an arctic explorer and a member of an old and distinguished family. The following is a personal history taken from Sverdrup's unpublished autobiography written for the National Academy of Sciences when he left the United States to return to Norway in 1948.[2] In view of Sverdrup's achievements in establishing a new science in the university, it is of historical value to portray his family and academic background in more detail than is customary in these Academy records in order to appraise their influence on his development.

Harald Sverdrup was born on November 15, 1888, in Sogndal, Sogn, Norway. At the time, his father, Johan Edvard Sverdrup (1866-1923) was teaching at the adult school there. Sverdrup's father, as were his four uncles, was a minister of the State Church of Norway (Lutheran), and in 1894 his father became minister in the island district of Solund, about 40 miles north of Bergen. Then his father moved to Rennsö near Stavanger. In 1908 he became professor of church history in Oslo, where he died in 1923.

The first record of a Sverdrup appeared in Norway in 1620, but Sverdrup can only trace his ancestry on his father's side to his great-great-grandfather, a large land owner in northern Norway. In 1813 one of his three sons, Georg

Sverdrup, was one of the first professors of the University in Oslo in classical philosophy and languages. He participated in the Norwegian constitutional convention and was one of the three authors of the final document. The youngest of the three, Sverdrup's great-grandfather, Jacob Liv Borch Sverdrup (1775-1841), became an expert in land management and established the first agricultural school in Norway. Two of his sons reached considerable prominence. Johan Sverdrup (1816-92), a lawyer, was a member of the Storting and became the leader of the liberal party and succeeded in introducing the parliamentary system.

The older brother, Harald Ulrik Sverdrup (1813-91), Sverdrup's grandfather, was also a churchman who served as a Lutheran minister in Sogndal, Sverdrup's birthplace. He also served a long time in the Storting and was involved in many enterprises, from fruit growing to banking to shipping.

His mother, Maria Vollan, died when her son was still a child. Her family was related to the Grieg family. His maternal grandmother was of Scotch descent, and his maternal grandfather had a religious education also but served as the editor of a large newspaper and was the author of an important arithmetic textbook.

As a result of his father's varied career, Sverdrup spent much of his boyhood in various sites in western Norway and was taught by governesses until he was fourteen years old, when he went to school in Stavanger. During Sverdrup's adolescence, he experienced conflicts between his interest in natural science and his family's profession of theology. By his own account he was an avid reader of a Danish publication, *Frem*, meaning "forward," that spanned the entire gamut of the sciences. He had difficulty reconciling the concept of evolution with his religious upbringing.

It did not occur to Sverdrup at the time that one could study science in the university whose subject matter to him was synonymous with theology. Thus, when he entered gymnasium in 1903, he chose the classical curriculum instead of the science curriculum. But that gave him the opportunity to read everything he could find on astronomy, his major interest at the time, and when he learned that he could pursue the natural sciences in the university, his career path was determined.

Much of Sverdrup's scientific development was associated with the military. After leaving the gymnasium with honors, he spent a year in Oslo preparing for and passing university preliminary examinations. He decided to combine his compulsory military service at the Norwegian Academy of War with an end toward becoming a reserve officer and having the security of an income. He joined this training with the study of physics and mathematics and thus was able to return to the university. He makes a special point in his memoirs that his year at the academy was not wasted because he needed physical training. He was proud of the fact that he finished the period served (1907-8) as the top man in athletics. It probably was essential to his survival and positive performance later during his long arctic ordeals.

When he entered the university in 1908, Sverdrup's intended major was astronomy. The precise title of the subject at the university was "Physical Geography and Astronomy." He defines the content in more modern terms as including geophysics, meteorology, oceanography, and terrestrial magnetism. His ultimate research interests were fixed in 1911 when he was offered an assistantship with Professor Vilhelm Bjerknes, the preeminent Norwegian meteorologist and founder of the Bergen School.[3] Theirs was not the usual relationship of mentor to student. Bjerknes's assistants were the brightest young scientists of their generation. They included Jacob Bjerknes, Tor Bergeron, Olaf Devik, Theodore

Hesselberg, Carl-Gustaf Rossby, and Halvor Solberg. Bjerknes expected them all to make substantial intellectual contributions to the work. Sverdrup later recalled that Bjerknes's[4]

> … own work centered completely around the further development of the theoretical tools. He has to my knowledge never attempted to draw a weather map, nor has he ever discussed actual meteorological observations. In the course of the years many of his assistants worked with the data, they tried to interpret the observed conditions, and, step by step, to gain better understanding of the physical processes in the atmosphere or the ocean. Bjerknes gave them complete freedom in their work. He was no hard taskmaster, but he laid the course.

The Bergen School was supported by an annual grant that Bjerknes received from the Carnegie Institution of Washington almost from the first days of its establishment after his visit to Washington in 1905. Bjerknes received this grant continuously to the end of his career. Sverdrup notes the vital role that the Carnegie Institution played in developing the earth sciences in those early years, and Sverdrup himself received support from the institution throughout his career.

Sverdrup initially expected to continue research in astronomy, but he became more and more interested in meteorology and oceanography and so changed his major. His first published paper (in 1914) was for his candidacy and was in meteorology. When, in 1912, Bjerknes went to the University of Leipzig as professor and director of the new Geophysical Institute, Sverdrup accompanied him and spent January 1913 to August 1917 in Germany. These were war years, and Sverdrup suffered from wartime shortages. He did his thesis for the University of Oslo while there and received his doctorate in June 1917 on a published paper on the North Atlantic tradewinds.

It seems that Sverdrup could not avoid his destiny in the Arctic. In 1913 Roald Amundsen resumed his plan for a north polar expedition to the Arctic on R/V *Maud*. Sverdrup turned down an invitation as an assistant to the chief scientist because he wished to complete his university work. In 1917 the opportunity resurfaced with the position elevated to that of chief scientist and he accepted. Sverdrup's experiences in the Arctic formed his character both as a man and a scientist.[5] He regaled his students throughout his later life with tales of the hardships of the Arctic and once remarked, "These years were really very valuable because they brought me in the closest possible contact with nature, a circumstance which to one who works in geophysics cannot be overestimated."[6]

The expedition left Norway on July 18, 1918, with a projected duration of three to four years. Instead, it lasted seven and one-half years, including an interruption of ten months in 1921-22 spent in the United States. Sverdrup did not return to Norway until December 22, 1925. In his own words, he would not have missed the experience of any one of those years. He called the most interesting period the eight months of 1919-20 that he spent in Siberia living with nomadic reindeer herders, the Chukchi. This was at the suggestion of Amundsen during a period when the vessel was ice bound. It is not easy to understand his statement, for although Sverdrup gave several lectures and talks on the Chukchi, he never published these. Sverdrup left a handwritten monograph on the Chukchi that was translated and published some thirty years after his death by his colleagues at Scripps as a tribute to Sverdrup,[7] although by then a larger and more authoritative ethnographic study of the people had already been published.

Sverdrup felt that the long years in the Arctic and the heavy responsibilities of his work as chief scientist were justified by the firsthand experience he gained in field research and data taking, but seven years away from home would seem to have overdone it. His arctic work also allowed him to visit the Carnegie Institution of Washington for the first time in the winter of 1921-22, his first trip to the United States and a very valuable scientific contact.

A more specific justification that Sverdrup gave in support of his concentration on the Arctic was the easier insight it affords in our understanding of the basic physical oceanography of currents. He argued that the effect of the earth's rotation, a fundamental aspect of the dynamics of the oceans, is best and most simply observed in the polar regions, where it is greatest. He recounted how Nansen empirically recognized the possibility of the rotation of the current vector as a function of depth and suggested to Bjerknes that it should be examined more formally. Bjerknes assigned the problem to a young mathematical physicist, V. Walfrid Ekman, who solved it and thereby his name was given to the phenomenon known as the Ekman Spiral. It has never been clear why so much fuss is made over this formula. It is the direct analogy to the Coq effect (skin effect) in the electromagnetics of a resistive conducting medium. This murkiness may be related to the curious fact that the effect of the earth's rotation is called the geostrophic force by the earth scientists where all the rest of physics call it after its elucidator, Coriolis.

Sverdrup had well established his scientific reputation and in 1926 was offered the chair of meteorology at Bergen, which had been vacated by Bjerknes, who had returned to Oslo. There he worked on the data collected on the *Maud* expedition and edited the scientific report of the expedition. He later estimated that he personally contributed two-thirds of the report. Before assuming this post, however he spent ten months at the Carnegie Institution in Washington working on the electric and magnetic records of the same expedition. There he met and made a favorable im pression on a number of American scientists and was twice offered and refused permanent positions. He visited several American laboratories, including a brief visit to the Scripps Institution. It is interesting to note parenthetically that if Sverdrup had accepted a Carnegie position he would almost certainly have become the first director of the Woods Hole Oceanographic Institution, organized in 1930.[8] If this had occurred, the institutional history of U.S. oceanography would have been vastly different.

Shortly after his return to Norway, Sverdrup married Gudrun Bronn Vaumund. They had no children, but he adopted Anna Margrethe, the daughter of Gudrun's first marriage.

In 1930 Sverdrup spent another six months at the Department of Terrestrial Magnetism, working on oceanographic data collected by the R/V *Carnegie* for whose cruise he had earlier served as a consultant. Not long after his return from the United States in 1931 he accepted a research professorship in the newly established Christian Michelsens Institute carrying on pretty much the same work on the *Maud* data. In 1931 he was the leader of the scientific group in the Wilkins-Ellsworth North Polar Submarine Expedition, where valuable information was gathered despite the failure to achieve the chief goal of the expedition, the submarine exploration of the Arctic in the *Nautilus.* In 1934 he spent two months studying boundary layer processes over high-lying snow fields in Spitsbergen with glaciologist H. W. Ahlman.

But it was in December 1935 that the great change occurred in Sverdrup's life when the director of his institute, Bjorn Helland-Hansen, just returned from the United States, informed Sverdrup that his name had come up as a possible replacement for Thomas Wayland Vaughan, retiring director of the Scripps Institution of Oceanography in La

Jolla and part of the University of California system.[9] Sverdrup agreed, if he could be given a three-year leave of absence, and he accepted the invitation that was soon tendered. Scripps was positioned to be an important center of oceanographic research, but it badly needed increased resources and rigorous scientific leadership. Sverdrup knew, perhaps more than anyone else in the world, about the emerging sciences of oceanography, meteorology, and geophysics, but he needed a position with the scope to expand the sciences.

As an aside, it is interesting to note that Sverdrup was preceded in his immigration to the United States by his brother, Leif, who had been sent abroad for career purposes. Lief also made a great success of his life. He became an American citizen, a civil engineer, and cofounder of the firm of Sverdrup and Parcel in St. Louis, one of the largest and most important American engineering companies. During the second world war, he served as chief engineer to Douglas MacArthur and rose to the rank of general in the U.S. Armed Forces.

The choice of Sverdrup as director of the Scripps Institution of Oceanography in 1936 was one of the most felicitous decisions made by the University of California and was an action that enhanced the university, the Scripps Institution, and the science and teaching of oceanography. No one could possibly have foreseen the immense consequences except, perhaps, Robert Gordon Sproul, the long-time president of the University of California. Sproul worked steadily with Sverdrup to improve the institution, which had been neglected by former university presidents. In fact, the Scripps Institution had almost lost the support of the Scripps family by neglecting them. When Scripps's major benefactor, Ellen Browning Scripps, was on her deathbed, she expressed her unhappiness with the fact the university regents had never visited the institution. When Sproul learned of her unhap piness, he came to La Jolla and called on her at her home to reaffirm his strong interest.

Sproul was a great university president. When he identified exceptional talent, he went all out to support it and made every effort to personally see that the individual was supplied with whatever he or she needed to do the job and to create an environment that would keep the individual at the university. This was certainly how he helped Ernest Lawrence, among many others, achieve his goals by bypassing the university bureaucracy and communicating directly with Lawrence in his early years at the university.

Sproul's sponsorship was all the more remarkable in the case of Sverdrup, who headed what then was an irregular outpost of the university. Scripps was, in 1936, a small rather remote and dusty marine station with one research vessel capable of only coastal cruises and a staff of about thirty people, including eight faculty members. It had an annual operating budget of about $89,000 derived largely from contributions made by the Scripps family and matched by the state of California. It had potential in the form of an endowment provided by Ellen Browning Scripps, a spectacular campus, and a promising staff, including some interesting students. One of these was Roger Revelle, about whom more later, who received his training at the Scripps Institution and his doctorate from the University of California in Berkeley, which was the degree-granting campus for Scripps. Despite the remoteness and the relative insignificance of the Scripps Institution in the university system, Sproul was in constant communication with Sverdrup and particularly helpful in what was Sverdrup's perpetual headache, the operation and maintenance of a research vessel.

Sverdrup remained as head of the institution from September 1936 for almost twelve years before returning to Norway to head the Norwegian Polar Institute. Sverdrup must have been surprised at the small size and rather poor facilities he found when he arrived in La Jolla in August 1936. He never commented about it, although Gudrun Sverdrup is said to have wept when she first saw the director's little house where the family was to live throughout their years at Scripps. Sverdrup went to work with several immediate goals in mind. He needed a new ocean-going vessel, and he wanted to increase the institution's income, improve staff morale, and establish closer ties among researchers. His longer-term goals were to develop an institution-wide research program and improve teaching at the institution.

Sverdrup got the vessel he needed in 1937 from Robert Paine Scripps, who headed the family in La Jolla at the time. Mr. Scripps was greatly impressed by Sverdrup and agreed not only to provide the institution with an oceangoing schooner, R/V *E. W. Scripps*, but also to increase the annual family contribution to the institution. Sverdrup's hope for long-term support from the Scripps family was dashed by Mr. Scripps's untimely death the following year.

Despite financial worries, Sverdrup pressed forward to improve both the research program and the curriculum of the institution. He evaluated faculty research and was concerned to find that Scripps did not have an institution-wide research program. Each scientist worked independently and set his own research agenda. Sverdrup focused the staff's attention on the California Current, and he organized the several expeditions to the Gulf of California, the first comprehensive hydrographic survey of that area. Sverdrup was alarmed to find that the only physical oceanographer at Scripps was not up to the highest European standards and focused on the preparation of long-range weather forecasts that were "close to witchcraft."[10] The forecasts were terminated, and Sverdrup moved on to improve instruction in physical oceanography and to attract students to the field.

Sverdrup developed close working contacts between Scripps and UCLA, which became the degree-granting campus for Scripps in 1936. He was active in the Academic Senate, taught courses at UCLA, and developed friendships with UCLA administrators and faculty. He found himself among friends there. Two of Vilhelm Bjerknes's other assistants, Jacob Bjerknes and Jorgen Holmboe, were building a department of meteorology at UCLA. Sverdrup also developed close contacts with UCLA physicists such as Vernon Knudsen, dean of the Graduate Division, to improve graduate instruction both at Scripps and UCLA.

Close contact with UCLA was essential as Sverdrup reformed the curriculum in oceanography. Scripps was the first and for a long time the only American institution that offered graduate instruction in oceanography. [11] Sverdrup was fortunate to have several gifted young instructors at SIO when he arrived, including Martin Johnson, Richard Fleming, and Roger Revelle. The faculty included biologists Denis Fox and marine microbiologist Clause ZoBell, and submarine geologist Francis Shepard, who became affiliated with Scripps beginning in 1937. A young physicist, Walter Munk, came to Scripps to study with Sverdrup in 1939. Sverdrup basically formalized a curriculum that included and fully integrated physical oceanography with marine biology, marine geology, and geophysics.

Many historians and scientists have commented on Sverdrup's publication with coauthors Martin Johnson and Richard Fleming of the first comprehensive text in oceanography, *The Oceans: Their Physics, Chemistry and General Biology*.[12] This was the crowning achievement of his teaching career. The book was considered to be of such military value to the Allies when it was published in 1942 that Washington forbade its distribution abroad during the war. After the war, it was distributed broadly and earned the Scripps

Institution an international reputation as a center of oceanographic research.

Sverdrup's contributions to physical oceanography were many and varied, as his publications list indicates, but his major breakthrough in the interpretation of ocean currents was his demonstration that the systematic use of the curl of the vector current greatly simplified the understanding of the movements. In other words, transforming the geodynamic equations of motion from the usual starting point of momentum balance to that of vorticity balance gives a more tractable mathematic approach to the physics of the problem. This is now the practical, accepted starting point in investigations of the oceanic currents. Sverdrup recognized that the curl of the surface wind stress was the primary agent in transferring mechanical energy to the ocean, usually labeled as the wind-induced curl.

Harald Sverdrup's three-year term as director of the Scripps Institution ended just as Norway was invaded and occupied. In his brief autobiography he writes that, when he realized his stay in La Jolla was to be indefinite, he decided to become an American citizen. Other accounts indicate that problems with his security clearances during the war also were an impelling factor. His wartime work required high levels of security, and, because so much of his family remained in occupied Norway, his clearance was intermittently canceled and renewed. This occurred despite the efforts of Roger Revelle, then in the Navy, to regularize Sverdrup's security status. Sverdrup's clearance difficulties were all the more painful as his younger brother, Einar, a captain in the Norwegian Free Forces, had been killed by Nazi forces in action in Spitsbergen in 1942. Sverdrup's student, Walter Munk, had similar problems for similar reasons, but, despite this handicap, they both made extremely important contributions to the Allied war effort.

The American military placed urgent demands on oceanographers and meteorologists as soon as the war began. Antisubmarine warfare became high priority for the military, and the University of California Division of War Research was formed at the Navy Radio and Sound Laboratory at Point Loma to undertake research on this subject. The training of military weather forecasters was also a high priority for several branches of the service. There were many other demands for help in solving urgent problems, such as the recovery of men lost at sea, the control of fouling organisms, and maintaining a steady harvest from the sea. Scripps quickly converted itself to a center of wartime oceanographic research and integrated its activities with those of other academic and navy laboratories working on similar problems. This was a notable feat for, as H. R. Seiwell noted, in 1941 there was

> … not a single military or naval organization trained to evaluate information on the oceans and coast lines of the world and to transform it into the type of strategical and tactical intelligence required for military operations.[13]

Probably because of his clearance difficulties, Sverdrup did not have a direct association with the primarily underwater sound work at the University of California Division of War Research. UCDWR was directed by physicist Gaylord P. Harnwell, who later became the distinguished president of the University of Pennsylvania and author of an excellent text on electricity and magnetism. Sverdrup did work on the very important set of problems related to forecasting surf conditions for military beachhead assaults. His current and wave forecasting methods were applied by military weathermen—many of them trained by Sverdrup at Scripps—to predict landing conditions for allied invasions of North Africa, Sicily, Normandy, and many Pacific islands.[14] This work was credited by naval officers with saving many lives. Sverdrup's student, Walter Munk, was a close associate and valuable contributor in this research. Walter Munk was probably Sverdrup's most important and famous student/associate. Of course, other places and institutions were involved in the same work, among them the group at Berkeley under the leadership of Morrough (Mike) P. O'Brien of engineering who became the famous dean of engineering there immediately after the war. Both the UCDWR and the Berkeley group were to become important sources of manpower and resources for SIO at war's end. John Dove Isaacs, for example, came from the Berkeley group and Carl Eckart from USDWR.

Sverdrup showed much more restraint and perspective in his postwar discussions of the value of his war work than is common among scientists of his caliber. While the achievements were of great practical importance, he would point out that they were based on very old theorems on waves and fetch from classical hydrodynamics. One is struck when reading through his available correspondence by the evenness of his tone whatever the situations, some of which must have been very vexing, unless the academic world was very different from what it is today. That evenness, one could say steadiness, probably accounts for his accomplishing so much in a relatively short time, despite adversity. Sverdrup patriotically considered endurance to be a trait innate to Norwegians, who "having grown up in a sparsely populated rugged country … have become self-reliant and resourceful, and have the temperament to endure monotony and loneliness and the daily toil of the trail."[15] If that trait was not innate, it certainly had to have been developed in Sverdrup during his many expeditionary years where he had to adjust and survive so many varied and difficult physical and human situations.

The war years and the immediate postwar period were very important times for oceanography, and Sverdrup knew it. The Navy played a vital role in advancing the field both by providing direct support from the various bureaus and through the formation of the Office of Naval Research, which set the administrative pattern for the immensely successful future of science in the United States. The unwritten story goes so far as to assert that specific navy actions actually saved the Scripps Institution from disappearance when the war was over. It starts with the statement that the regents of the University of California felt that, the war being over, there no longer was a reason for the continuation of the institution. The Navy saw otherwise and approached the regents directly to make a commitment to support the institution financially for "as long as it was budgetarily feasible." The documentary record shows that Admiral Edward Cochrane, with the urging of Roger Revelle, wrote UC President Robert Gordon Sproul offering to continue support for postwar research in oceanography at the Scripps Institution. Sproul, with the urging of Harald Sverdrup, accepted the support and created an administrative structure to oversee large-scale sponsored research at the University of California. Behind the scenes, Sverdrup, Revelle, and Lyman Spitzer consulted with Lieutenant Commander John T. Burwell and other naval officers who fostered the Office of Naval Research.[16]

Continued federal research support was an essential element in Sverdrup's postwar plan for Scripps. He had begun thinking as early as 1943 about the end of the war, and he had a written postwar plan prepared by 1944. Continued navy funding was critical and made all the difference. The university and the Navy entered into a partnership that built the world's leading institution in the field. It was a fruitful partnership that strengthened both the Navy and the aca demic spheres. For instance, the accelerated development of magnetic anomaly detection, both ship and airborne, so important in antisubmarine warfare, was one of the two key technologies that brought about the plate tectonics revolution. The other was the advanced acoustic gear for bathymetry that revealed the midoceanic ridges and the thickness of ocean sediments. These were long-term consequences, but the partnership also had immediate advantages. The Navy made a specific commitment to support several professorships at Scripps indefinitely, as well as offering continued research funding. This was important because it allowed Scripps to build academic strength. One professorship kept Carl Eckart in La Jolla. Eckart had spent the war at UCDWR and was prepared to return to his professorship at the University of Chicago. Quick decisions had to be made because of the dissolution of UCDWR at the end of the war, and Sverdrup was well prepared with a plan. In those years Eckart was probably one of the most important members of SIO, second only to Sverdrup. In addition to Eckart, the early navy chairs were used to attract Russell Raitt, among others, who also had been in the UCDWR and was destined to become a principal in the development of plate tectonics.

Sverdrup was the central figure on the West Coast during the postwar development of oceanography and allied sciences. He became increasingly active on scientific committees after the war, and his contributions to science were recognized by his peers. He was elected to the National Academy of Sciences in 1945. He joined the Executive Committee of the American Geophysical Union in 1945 and presided over the AGU Oceanography Section. In 1946 he became president of the International Association of Physical Oceanography. He chaired the Division of Oceanography and Meteorology at the 1946 Pacific Science Confer ence. During these years he worked tirelessly to promote and expand research in geophysics.

Sverdrup worked closely with Robert Gordon Sproul, Vern Knudson, Roger Revelle, and many others. However credit is shared, the postwar program in oceanography was an immense success. Sverdrup's postwar plan for Scripps had a number of elements. He wanted to train more oceanographers, expand the Scripps research fleet, and focus institutional manpower and ships on a united scientific research program that would take the institution to sea and address fundamental oceanographic problems.[17] During the immediate postwar period, manpower issues were discussed intensely by Sverdrup, Rossby, Revelle, Iselin, and others concerned with the postwar development of meteorology, oceanography, and geophysics. The newness of oceanographic teaching was underscored by a letter written by Columbus O'Donnell Iselin to John Fleming in 1944.[18] Iselin pointed out that Scripps was the only institution offering graduate degrees in the subject and wondered if another center of training was really needed. Even if it seemed reasonable for Woods Hole to start a teaching program, Iselin, in his characteristically laid-back temperament, said he was not likely to generate the drive to start one.

Sverdrup was a compelling force in two other notable postwar initiatives on the West Coast. He was one of a number of University of California faculty members who convinced Robert Gordon Sproul to establish an institute of geophysics at UCLA. This was the nucleus for the Institute of Geophysics and Planetary Physics with centers both at UCLA and SIO. Sverdrup had established cooperative research projects before the war, and as early as 1945 he discussed the possibility of resuming cooperative research in fisheries with Oscar O. Sette of the U.S. Fish and Wildlife Service. Harry Scheiber characterized Sverdrup as one of the masterminds of the California Cooperative Fisheries Investigation (CalCOFI), a long-term, broad, coordinated marine fisheries and oceanographic study that made great methodological advances in fisheries research. CalCOFI brought some $400,000 for research to the Scripps Institution. It also allowed the institution to acquire three vessels from the government, with a little help from Roger Revelle, and convert two of them into research vessels, *Crest* and *Horizon*.[19]

Sverdrup left SIO in 1948 to return to Norway. The reason he gave was that he thought he could be more influential in international affairs operating out of a smaller country, something that is certainly true. But there was probably a mixture of other factors as well. For one, the University of California salary scale at the time was notoriously low. Faculty at Berkeley would grumble that the difference was supposed to be made up by the splendid California climate. Undoubtedly, Harald Sverdrup and certainly his wife, Gudrun, were homesick. Sverdrup was away from La Jolla for considerable periods of time, and his wife must have felt isolated in a country in which she was not totally comfortable. She was also concerned that their daughter, Anna, so long away from Norway, was becoming Americanized.

Sverdrup returned to organize and head the Norwegian Polar Institute in Oslo. He resumed his early work on polar exploration by arranging the 1949-52 Norwegian-British-Swedish expedition to Antarctica. He served as professor of geophysics at the University of Oslo from 1949 until his death, serving also as dean of the faculty of science and vice-director of the university. Sverdrup's work at the Norwegian Polar Institute was not of the intensity or depth of what had occupied him at Scripps, but he did achieve his goal of being useful in the international sphere. Specifically, he was successful as chairman of the Norwegian relief program in India. He also accomplished much in strengthening the Indian fisheries.

One noble goal he set for himself was the easing of the Cold War tensions between the Soviet Union and the United States. He counted on his experiences and associations in both countries.

His greatest achievement, however, was as chairman of the commission that overhauled the Norwegian educational system. The system is still identified with the Sverdrup name.

There are several different accounts of Harald Sverdrup's death. One of his students reported that he suffered a fatal heart attack exactly while his physician was reporting to him that he was in fine shape. Nels Spjeldnaes says he died while attending a meeting. Another report says he knew he had a heart problem but decided not to pamper it. He died suddenly on August 21, 1957.

Harald Sverdrup's correspondence and other papers documenting his career in Norway after 1948 are at the Norwegian Polar Institute in Oslo. His records as director of the Scripps Institution of Oceanography are in the archives at Scripps.

NOTES

1. B. B. Baker et al., ed. *Glossary of Oceanographic Terms*, 2nd ed., p. 160. Washington, D.C.: U.S. Naval Oceanographic Office, 1966.
2. "Informal Autobiography of Harald Ulrik Sverdrup," unpublished manuscript submitted to the National Academy of Sciences c. 1948 in Records of the SIO Office of the Director (Sverdrup), Accession 82-56, Scripps Archives, UCSD, La Jolla, Calif.
3. R. M. Friedman. *Appropriating the Weather: Vilhelm Bjerknes and the Construction of a Modern Meteorology*. Ithaca, N.Y.: Cornell University Press, 1989. This book is a history not only of V. Bjerknes but of the entire Bergen School and gives full background on the training of Harald Sverdrup and Bjerknes's other talented assistants.

4. H. U. Sverdrup. Vilhelm Bjerknes in memoriam. *Tellus* 3(1951):218.
5. R. M. Friedman provided the best description of Sverdrup's contribution as an arctic scientist and explorer in "The Expeditions of Harald U. Sverdrup: Contexts for Shaping an Ocean Science," William E. and Mary B. Ritter Memorial Fellowship Lecture, Scripps Institution of Oceanography, October 29, 1992, and in a lecture presented at the Fifth International Congress on the History of Oceanography, held at the Scripps Institution of Oceanography in July 1993. Publication of these lectures is forthcoming.
6. Autobiographical precise by H. Sverdrup, Bergen, April 11, 1936, in Records of the SIO Office of the Director (Sverdrup), Scripps Archives, UCSD.
7. H. U. Sverdrup. *Among the Tundra People*. Translated by M. Sverdrup. La Jolla: Scripps Institution of Oceanography, UCSD, 1978.
8. Sverdrup to Vernon Knudson, February 1, 1938, in Records of the Office of the Director (Sverdrup), SIO Archives, UCSD.
9. Sverdrup and Vaughan apparently met for the first time in 1926. Sverdrup visited SIO for several days that year. They met again in 1932 when Vaughan visited Bergen. Vaughan to Sproul, August 17, 1936, in Records of the SIO Office of the Director (Vaughan), Archival Collection 11, Box 3, folder 121, SIO Archives, UCSD.
10. E. L. Mills. Useful in many capacities. An early career in American physical oceanography. *Historical Studies in the Physical Sciences* 20(1990):298.
11. V. O. Knudsen et al. Education and training for oceanographers. *Science* 111 (June 23, 1950):701.
12. Five scientists, D. James Baker, Walter Munk, Bruck Warren, Sharon Smith, and Mean McManus, discussed the importance of this book in articles published on the fiftieth anniversary of the publication of the book in *Oceanography* 5 (1992).
13. H. R. Seiwell. Military oceanography in World War II. *Military Engineer* 39(1947):202.
14. Many books and papers have been written about this work, including R. Revelle, "The Age of Innocence and War in Oceanography," *Oceans* 1 (1969); J. C. Crowell, "Sea, Swell and Surf Forecasting Methods Employed for the Allied Invasion of Normandy, June 1944," UCLA thesis, February 1946; and C. C. Bates and J. F. Fuller, *America's Weather Warriors 1814-1985*, College Station: Texas A&M, 1986. A summary of the meteorology courses for military officers offered by Sverdrup at SIO and at UCLA are included in "Report on the Activity of the Scripps Institution of Oceanography, Biennium 1944-1945," July 1, 1946, pp. 1-2, SIO Archives, UCSD.

15. H. U. Sverdrup. Roald Amundsen. In *One Hundred Norwegians: An Introduction to Norwegian Culture and Achievement*, ed. S. Mortensen and P. Vogt, p. 155. Oslo: Johan Grundt Tanum Forlag, 1955.

16. Lyman Spitzer to Columbus O. Oselin, January 22, 1945, in Records of the SIO Office of the Director (Sverdrup), SIO Archives, UCSD.

17. There are a number of documents in Sverdrup's papers at Scripps that describe his postwar plans, including his undated "Memorandum on Post-war Research of Interest to the U.S. Navy with Special Emphasis on the Participation of the Scripps Institution of Oceanography, University of California," and "Memorandum on Postwar Studies of Oceanography of the Surface Layers, May 31, 1945."

18. Columbus O. Iselin to John A. Fleming, December 1, 1944, in Records of the SIO Office of the Director (Sverdrup), SIO Archives, UCSD.

19. H. N. Scheiber. California marine research and the founding of modern fisheries oceanography: CalCOFI's early years, 1947-1964. *California Cooperative Oceanic Fisheries Investigations Reports* 31 (1990):63-83.

Selected Bibliography

1914 Ausgedehnte Inversionsschichten in der freien Atmosphäre. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 3:75-100.

With T. Hesselberg. Über den Einfluß de Gebirge und die Luftbewegung längs der Erdoberfläche und auf die Druckverteilung. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 4:101-16.

With T. Hesselberg. Das Beschleungigungsfeld bei einfachen Luftbewegungen. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 5:117-46.

1915 With T. Hesselberg. Die Reibung in der Atmosphäre. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 10:241-309.

With T. Hesselberg. Beitrag zur Berechnung der Druck und Massenverteilung im Meere. *Bergens Museums Aarbok* 1914-1915, no. 14.

With T. Hesselberg. Die Stabilitätsverhältnisse des Seewassers bei vertikalen Verschiebungen. *Bergens Museums Arbok* 1914-1915, no. 15.

With T. Hesselberg. Die Windänderung mit der Höhe vom Erdboden bis etwa 3000 m Höhe . *Beitr. Phys. Atmos.* 7:156-66.

1916 Druckgradient, Wind und Reibung an der Erdoberfläche. *Ann. Hydrogr. Marit. Meteorol.* 413-27.

Der feucht-adiabatische Temperaturgradient. *Meteorol. Z.* 6:265-72.

Stationäre Bewegungsfelder. *Meteorol. Z.* 5:208-10.

Über Mittelwerte von Vektorpaaren mit Anwendungen auf meteorologische Aufgaben. *Meteorol. Z.* 9:411-20.

1917 Der Nordatlantische Passat. *Veroeff. Geophys. Inst. Univ. Leipzig* , series 2, 2.

With J. Holtsmark. Über die Beziehung zwischen Beschleunigungen und Gradientenänderungen und ihre prognostische Verwendung. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 2:143-71.

Über die Korrelation zwischen Vektoren mit Anwendungen auf meteorologische Aufgaben. *Meteorol. Z.* 8/9:285-91.

With J. Holtsmark. Über die Reibung an der Erodoberfläche und die direkte Vorausberechnung des Windes mit Hilfe der hydrodynamischen Bewegungsgleichungen. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 2:97-141.

Zur Bedeutung der Isallobarenkarten. *Ann. Hydrogr. Marit. Meteorol.* 325-29.

1918 Die Beziehung der elfjährigen Flimaschwankungen zur Sonnentätigkeit. *Ann. Hydrogr. Marit. Meteorol.* 191-93.

Einige Untersuchungen über die Radioaktivität des Seewassers in den Fjorden in der Nähe von Bergen (Norwegen). *Bergens Museums Aarbok* 12:1-5.

Über den Energieverbrauch der Atmosphäre. *Veroeff. Geophys. Inst. Univ. Leipzig*, series 2, 2:173-96.

1921 Blandt rentsjuktsjere og lamuter, in Amundsen. *Nordost-passasjen*, pp. 257-391. Kristiania: Gyldendalske.

1922 Customs of the Chukchi natives of northeastern Siberia. *J. Wash. Acad. Sci.* 12:208-12.

Maud-ekspeditionens videnskabelige arbeide 1918-1919 og nogen av dets Resultater. *Naturen* 1/2:5-32.

Maud-ekspeditionens videnskabelige arbeide 1918-1919 og nogen av dets Resultater. *Naturen* 3/4:65-88.

Meteorology on Captain Amundsen's present arctic expedition. *Mon. Weather Rev.* 50:74-75.

With C. R. Duvall. Results of magnetic observations on the "Maud Expedition," 1918-1921. *Terrestr. Magnetism Atmos. Electr.* 27:35-56.

1925 The north-polar cover of cold air. *Mon. Weather Rev.* 53:471-75.

1926 Maud-ekspeditionen 1918-1925. *Ymer* (Svenska Sällskapet för Anthropologi och Geografi) 1:1-18.

"Maud"-ekspeditionen videnskabelige arbeide 1922-1925. *Naturen* 161-80.

Scientific work of the Maud expedition, 1922-1925. *Sci. Mon.* 22:400-10.

With O. Dahl. Two oceanographic current-recorders designed and used on the "Maud" expedition. *J. Opt. Soc. Am.* 12:537-45.

The tides on the North Siberian shelf: their bearing on the existence of land in the Arctic Sea, and their dynamics. *J. Wash. Acad. Sci.* 16:529-40.

Tre år i isen med "Maud." Oslo: Gyldendal.

1927 Ergebnisse der Messuen des Potentialgefälles auf der "Maud"-Expedition. *Z. Geophys.* 3:93-102.

Dynamic of tides on the North Siberian Shelf: results from the Maud expedition. *Geofys. Publ. Nor. Meteorol. Inst.* 4.

Magnetic, atmospheric-electric, and auroral results, Maud expedition, 1918-1925. *Publ. Carnegie Inst. Wash.* 6(175):309-524.

With G. R. Wait. Preliminary note on electromotive forces possibly produced by the earth's rotating magnetic field and on observed diurnal variation of the atmospheric potential gradient. *Terrestr. Magnetism Atmos. Electr.* 32:73-83.

Nordenskiölds hav og det Øst-Sibiriske Hav. *Nor. Geograf. Tidsskr.* 6/7:321-35.

Scientific work of the "Maud" expedition, 1922-1925. *Smithsonian Report for 1926*, pp. 219-33.

1928 Finn Malmgrens videnskabelige virke. *Ymer* 3:246-52.

Aufgaben, Bemannung and Ausrüstung einer Wissenschaftlichen Beobachtungsstation auf dem Treibeis bei 1-2-jähriger Überwinterung in der Inneren Arktis. *Arktis* 1/2:29-36.

Die Eistrift im Weddelmeer. *Ann. Hydrogr. Marit. Meteorol.* 56:265-74.

On the importance of auroral photographs taken from one station. *Terrestr. Magnetism Atmos. Electr.* 33:195-202.

Die Renntier-Tschuktschen. *Mitt. Geogr. Ges. Hamburg* 39:87-135.
Minnetale over Roald Amundsen. *Norske videnskaps-akademi i Oslo, Arbok*, pp. 125-29.
Results of astronomical observations. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 1. Bergen: Statens Forskningsfond.
The wind-drift of the ice on the North-Siberian Shelf. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 4. Bergen: Statens Forskningfond.
Appendix. From the diary of Harald Ulrik Sverdrup, in "Birds from the north-eastern Siberian Atlantic Ocean." *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 5, pp. 13-16.
1929 Currents on the North Siberian Shelf. *Skand. Naturforskermøde* 18.
Polferden med "Graf Zeppelin." *Naturen* 54:353-67.
With T. Hesselberg. Über die Genauigkeit der Berechnung der Druckund Massenverteilung und der Stabilitätsverhältnisse im Meere. *Ann. Hydrogr. Marit. Meteorol.* 57:73-75.
The waters on the North-Siberian Shelf. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 4. Bergen: Statens Forskningfond.
1930 The bottom water on the North-Siberian Shelf. *Congreso Internacional de Oceanografía, Hidrografía Marina e Hidrologia Continental*, Sevilla (May 1-7):331-36. Madrid: Graficas Reunidas.
Dyrelivet i drivisen. Efter erfaringene på "Maud"-ferden. *Naturen* 54:133-45.
Fridtjof Nansen. *Arktis* 1/2:1-4.
Meteorology, Part 2, Tables. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 3. Bergen: Geofysisk Institutt.
Some aspects of oceanography. *Sci. Mon.* 31:19-34.
Some oceanographic results of the *CARNEGIE*'s Work in the Pacific—The Peruvian Current. *Trans. Am. Geophys. Union* 257-64.
1931 Audibility of the Aurora Polaris. *Nature* (Sept. 12).

Fridtjof Nansen som videnskapsmann. *Nor. Geogr. Tidsskr*. 3:306-13.
Dirunal variation of temperature at polar stations in the spring. *Gerlands Beitr. Geophys*. 32:1-14.
Die Meteorologischen Untersuchungen und Ergebnisse der "Maud"-Expedition. Petermanns Mitteilungen, Ergänzungsheft 191, Internationale Studiengesellschaft zur Erforschung der Arkis mit dem Luftschiff (Aeroarctic).
The Deep-Water of the Pacific According to the Observations of the Carnegie. Carnegie Institution of Washington, Department of Terrestrial Magnetism, Report to Section of Oceanography, International Geodetic and Geophysical Union, Stockholm, 87-93.
The origin of the deep-water of the Pacific Ocean as indicated by the oceanographic work of the Carnegie. *Gerlands Beitr. Geophys*. 29:95-105.
Resultater av Maudferdens Oseanografiske undersøkelser. *Naturen*.
Scientific results of the Andrée expedition. I. Drift-ice and ice-drift. *Geogr. Ann.* 2/3:121-40.
Snedekkets termiske egenskaper. *Chr. Michelsens Institutt for Videnskaps og Åndsfrihet*, vol. 1.
Das Tier- und Vogelleben im Treibeis. *Petermanns Geographische Mitteilungen* 1/2:3-20.
Die Wissenschaftlichen Arbeiten auf der Wilkins-Ellsworth-Expedition 1931. *Arktis* 3/4:49-50.
Hvorledes og Hvorfor Med "Nautilus." Oslo: Gyldendal.
1932 Als Meeresforscher mit dem Unterseeboot "Nautilus" im Nordopolargebiet. *Das Meer Polarbuch* 1:1-22.
Arbeider i luft- og havforskning. *Chr. Michelsens Institutt for Videnskaps og Åndsfrihet*, vol. 2.
Wärmehaushalt und Austauschgrö e auf Grund der Beobachtungen der "Maud"-Expedition. *Beitr. Phys. Atmos*. (Bjerknes-Festschrift) 19:276-90.
1933 General Report of the Expedition. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 1. Bergen: Geofysisk Institutt.

Pendulum observations near Cape Chelyuskin. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 2.

Meteorology, Part I, Discussion. *Norwegian North Polar Expedition with the "Maud" 1918-1925, Scientific Results*, vol. 2. Bergen: Geofysisk Institutt.

Geofysiske undersøkelser, saerlig over vindens betydning for Havstrømmene. *Chr. Michelsens Institutt for Videnskaps og Åndsfrihet*, vol. 3.

Narrative and oceanography of the Nautilus expedition, 1931. *Papers in Physical Oceanography and Meteorology*, vol. 2. Massachusetts Institute of Technology and Woods Hole Oceanographic Institution.

Naturvidenskap og Religion. *Fritt Ord* 3:107-11.

On vertical circulation in the ocean due to the action of the wind with application to conditions within the Antarctic circumpolar current. *Discovery Reports* 7:139-70.

Vereinfachtes Verfahren zur Berechnung der Druck- und Massenverteilung im Meere. *Chr. Michelsens Institutt. Geofys. Publ.* 10:3-9.

1934 The circulation of the Pacific. In *Proceedings of the Fifth Pacific Science Congress*, Canada, pp. 2141-45.

Oversikt over "Maud"-ekspedisjonens videnskapelige Resultater. *Chr. Michelsens Institutt for Videnskap og Åndsfrihet*, vol. 4.

Air circulation over the Polar Sea. *Arctica* 2:47-63.

Bjørn Helland-Hansen, 1877. *Nor. Biogr. Leksikon* 6:10-13.

Videnskapens Bakgrunn. Studentersamfunded i Trondhjem. Smäskrifter, vol 2.

Wie entsteht die Antarktische Konvergenz? *Ann. Hydrogr. Marit. Met.* 315-17.

1935 The temperature of the firn on Isachsen's plateau, and general conclusions regarding the temperature of the glaciers on West-Spitsbergen. In *Scientific Results of the Norwegian-Swedish Spitsbergen Expedition in 1934*, Part III. Geografiska Annaler 1/2:53-88.

The ablation of Isachsen's plateau, and on the fourteenth of July glacier in relation to radiation and meteorological conditions. In

Scientific Results of the Norwegian-Swedish Spitsbergen Expedition in 1934, Part IV. Geografiska Annaler 3/4:145-66.

Temperaturen i Vest-Spitsbergens breer. *Naturen* 7/8:239-48.

Übersicht über das Klima des Polarmeeres und des Kanadischen Archipels. *Handbuch der Klimatologie herausgegeben von Köppen und Geiger*, vol. 2.

Varmeutvekslingen mellem en snaflate og luften. *Chr. Michelsens Institutt for Videnskap og Åndsfrihet*, vol. 5.

Zum Wärmehaushalt der Gletscher auf West-Spitzbergen. *Meteorol. Z.* 12:495.

Polar-humor. *Polar-Årboken* 5-14.

1936 Austausch und Stabilität in der untersten Luftschicht. *Meteorol. Z.* 1:10-15.

The eddy conductivity of the air over a smooth snow field. Results of the Norwegian-Swedish Spitsbergen expedition in 1934. *Geofys. Publ.* 11.

Das Maritime Verdunstungs problem. *Ann Hydrogr. Marit. Meteorol.* 41-47.

Results of the Meteorological Observations on Isachsen's Plateau. Scientific Results of the Norwegian-Swedish Spitsbergen expedition in 1934. *Geograf. Ann.* 1/2:34-47.

Turbulensforskning i Laboratoriet og i Naturen. *Chr. Michelsens Institutt for Videnskap og Åndsfrihet*, vol. 6.

Beziehungen Zwischen den Ånderungen der Gletscher auf Spitzbergen und Kleineren Klimatischen Ånderungen. *Publ. Chr. Michelsens Inst.* 6:31.

1937 Oceanographic research at the Scripps Institution of Oceanography during April 1936 to April 1937. *Trans. Am. Geophys. Union* 210-16.

On the evaporation from the oceans. *J. Mar. Res.* 1:3-14.

With D. L. Fax and J. Cunningham. Rate of water propulsion by the California mussel. *Biol. Bull.* 72:417-38.

The work at the Scripps Institution of Oceanography. *Collecting Net* 12:57-61. Page 368

1938 Notes on erosion by drifting snow and transport of solid material by sea ice. *Am. J. Sci.* 35:370-73.

On the explanation of the oxygen minima and maxima in the oceans. *Journal du Conseil International pour l'Exploration de la Mer* 13:163-72.

On the process of upwelling. *J. Mar. Res.* 1:155-64.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1937 to April 1938. *Trans. Am. Geophys. Union* 238-42.

Oceanographic problems off the coast of California. *Trans Am. Geophys. Union*, Papers, Joint Meeting, Meteorology and Oceanography, pp. 173-74.

Hos Tundra-Folket. Oslo: Gyldendal.

1939 Ocean circulation. In *Proceedings of the Fifth International Congress of Applied Mechanics*, pp. 279-93.

On the influence of stability and instability on the wind profile and the eddy conductivity near the ground. In *Proceedings of the Fifth International Congress of Applied Mechanics*, pp. 369-72.

Physics and Geophysics: With Special Reference to Problems in Physical Oceanography. Berkeley: University of California Press.

Second note on the logarithmic law of wind structure near the ground. *R. Meteorol. Soc. Q. J.* 65:57-60.

Response of the medalist. *Science* 90:24-27.

Cruises of the E. W. Scripps in 1939. *Sci. Mon.* 49:389-91.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1938 to April 1939. *Trans. Am. Geophys. Union* 422-27.

With W. E. Allen. Distribution of diatoms in relation to the character of water masses and currents off southern California in 1938. *J. Mar. Res.* 2:131-44.

Lateral mixing in the deep water of the South Atlantic Ocean. *J. Mar. Res.* 2:195-207.

1940 Part 2, Hydrology, Discussion. In *British Australian New Zealand Ant* *arctic Research Expedition, 1929-1931*, vol. 3 (Oceanography), pp. 88-126. Adelaide: B.A.N.Z.A.R. Expedition Committee.

The currents of the Pacific Ocean and their bearing on the climates of the coasts. *Science* 91:273-82.

General remarks on turbulence in the atmosphere and the ocean. *Association Océanographique Physique, Procés-Verbaux* 3.

The Gulf of California. *Association Océanographique Physique, Procés-Verbaux* 3.

Do permanent deep-sea currents exist? *Association Océanographique Physique, Procés-Verbaux* 3:182-83.

The Arctic regions. *Association Océanographique Physique, Publ. Scientifique* 8:50-53.

On the annual and diurnal variation of the evaporation from the oceans. *J. Mar. Res.* 3:93-104.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1939 to April 1940. *Trans. Am. Geophys. Union* 343-46.

The unity of the sciences at sea. *Sigma Xi Q.* 28:105-15.

Trial blazing in the Pacific. *Calif. Mon.* 45:10.

The Gulf of California: preliminary discussion of the cruise of the "E. W. SCRIPPS" in February and March 1939. In *Proceedings of the Sixth Pacific Science Congress* , vol. 3, pp. 161-66.

Activities of the Scripps Institution of Oceanography, La Jolla, California. In *Proceedings of the Sixth Pacific Science Congress*, vol. 3, pp. 114-23.

1941 Water masses and currents of the North Pacific Ocean. *Science* 93:436.

The influence of bottom topography on ocean currents. In *Applied Mechanics*, Theodore von Kármán Anniversary Volume, pp. 66-75. Pasadena: California Institute of Technology.

The Pacific Ocean. *Science* 94:287-93.

An analysis of the ocean currents off the American west coast between 40°N and 40°S. In *Proceedings of the Dedicatory Exercises of Hancock Hall, University of Southern California Chronicle*, pp. 17-20.

With R. Fleming. The waters off the coast of southern California, March to July, 1937. *Bull. Scripps Inst. Oceanogr*. 4(4):261-378.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1940 to April 1941. *Trans. Am. Geophys. Union* 490-94.

1942 Oceanographic observations on the E. W. SCRIPPS cruises of 1938. In *Scripps Institution of Oceanography, Records of Observations*, vol. 1.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1941 to April 1942. *Trans. Am. Geophys. Union* 2:323-25.

Oceanography for Meteorologists. New York: Prentice-Hall.

With M. W. Johnson and R. H. Fleming. *The Oceans: Their Physics, Chemistry and General Biology*. New York: Prentice-Hall.

1943 Oceanographic observations of the Scripps Institution in 1939. *Scripps Institution of Oceanography, Records of Observations* 1:64-159.

On the ratio between heat conduction from the sea surface and heat used for evaporation. *Ann. N.Y. Acad. Sci.* 44:81-88.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1942 to April 1943. *Trans. Am. Geophys. Union* 244-46.

1944 Oceanographic observations on the "E. W. SCRIPPS" cruises of 1940. *Scripps Institution of Oceanography, Records of Observations* 1:161-248.

The California Current. In *Science in the University*, pp. 97-111. Berkeley: University of California Press.

With F. M. Soule et al. *Observations and Results in Physical Oceanography. Scientific Results of Cruise VII of the CARNEGIE During 1928-1929, Oceanography*. Washington, D.C.: Carnegie Institution of Washington.

1945 Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1943 to April 1944. *Trans. Am. Geophys. Union* 605.

Oceanography. In *Handbook of Meteorology*, pp. 1032-56. New York: McGraw-Hill.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1944 to April 1945. *Trans. Am. Geophys. Union* 26:127-28.

1946 The humidity gradient over the sea surface. *J. Meteorol*. 3:1-8.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1945 to April 1946. *Trans. Am. Geophys. Union* 27:571-73.

With W. Munk. Empirical and theoretical relations between wind, sea and swell. *Trans. Am. Geophys. Union* 27:823-27.

With W. Munk. Theoretical and empirical relations in forecasting breakers and surf. *Trans Am. Geophys. Union* 27:828-36.

1947 With W. Munk. *Wind, Sea and Swell: Theory of Relations for Forecasting*. Washington, D.C.: U.S. Hydrographic Office.

New international aspects of oceanography. *Trans Am. Philos. Soc.* 91:75-78.

Note of the correction of reversing thermometers. *J. Mar. Res.* 6:136-38.

Period increase of ocean swell. *Trans Am. Geophys. Union* 28:407-17.

Wind-driven currents in a baroclinic ocean; with application to the equatorial currents of the eastern Pacific. *Proc. Natl. Acad. Sci. U.S.A.* 33:318-36.

Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1946 to April 1947. *Trans. Am. Geophys. Union* 28:801-2.

With R. H. Fleming. Atlantic Ocean. *Encyclopaedia Britannica*.

With R. H. Fleming. Indian Ocean. *Encyclopaedia Britannica*.

Southern Ocean. *Encyclopaedia Britannica*.

Oceanographic observations on the E. W. Scripps cruises of 1941. *Scripps Institution of Oceanography, Records of Observations* 1:249-408.

Wind, sea and swell. In *Proceedings of the Royal Canadian Institute*, ser. 3A, vol. 12, session 1946-1947.

1948 With R. H. Fleming. Oceano Atlantico. *Bol. Geogr*. 6:1066.

Om Vekslingene i det Californiske Sardinfiske. *Naturen* 72:264-67.

Den Norsk-Britisk-Svenske Ekspedisjon til Antarkis 1949-1952. *Nor. Hvalfangst-Tidende* 37:39-41.

1949 With M. W. Johnson and R. H. Fleming. Ocean and oceanography. *Encyclopaedia Britannica*.

With R. H. Fleming. Pacific Ocean. *Encyclopaedia Britannica*.

Polarforskning. Forges Stilling Idag. *Årsskrift. Det Grønlandske Selskab* 53-56.

The wind and the sea: presidential address. *Association d'océanographie Physique, Procés-Verbaux* 4:37-55.

Theoretical tools in geophysics. *Geogr. Ann.* 31:365-68.

Vind, Sjø og Dønning. *Norsk Nautisk Almanakk og Sjøfartskalender 1950*, 217-20.

Krigs- og Forsvarsforskning i De Forente Stater. *Nor. Militaert Tidsskr.* 108:321-37.

1950 Physical oceanography of the North Polar Sea. *J. Arctic Inst. N. Am.* 3:178-86.

Golfstrømmen. *Norsk Nautisk Almanakk og Sjøfartskalender 1951*, 235-39.

Norsk Arktisk Forskning. *Svalbardposten* 23.

Oseanografiske Observasjoner som Antyder en Klimaendring. Beretning fra Utvalget for Vaer- og Klimavariasjoner 1948 og 1949. *Utgitt av Det Norske Videnskaps-Akademi i Oslo*, pp. 35-36.

The Norwegian-British-Swedish Scientific Expedition to Antarctica, 1949-1952. *Nor. Polar-Tidende* 36-44.

1951 Die Norwegisch-Britisch-Schwedische Expedition in die Ankarktis. *Polarforschung* 3:70-71.

Evaporation from the oceans. In *American Meteorological Society Compendium of Meteorology*, pp. 1071-81.

With M. W. Johnson and R. H. Fleming. Ocean and oceanography. *Encyclopaedia Britannica*.

Vilhelm Bjerknes in memoriam. *Tellus* 3:217-21.

Vilhelm Bjerknes. 14 Mars 1862-8 April 1951. *Nor. Geogr. Tidsskr*. 13:1-7.

Lincoln Ellsworth. *Nor. Geogr. Tidsskr*. 13:8-9.

With M. S. "Norsel" to Dronning Maud Land. *Nor. Polartidende*.

1952 Forslag til ny Studieordning ved Det Matematisk-Naturvitenskapelige Fakultet. *Den Hogre Skolen* 2:35-39.

Havets Beitemarker. Skrifter Utgjevne av Vest-landske Bondestemna No. 36, *Kystvakt* 7:7-9.

Meteorologiske Observasjoner på Norske Hvalkokerier. *Nor. Hvalfangsttidende* 41:70-73.

StrØmsystemet i det Nordlige Stillehav. *Norsk Nautisk Almanakk og Sjøfartskalender 1953*, 337-39.

Havlaere (Fagbøker for Fiskere).

Naturvidenskapens verdier. *Teknisk Ukeblad* 29.

Circulation and tidal currents underneath the shelf-ice, Queen Maud Land. *Association d'océanographie Physique, Procés-Verbaux* 5:157.

Some remarks on the place of hydrography in fisheries research. Rapports et procés-verbaux des réunions. *Cons Perm Intern l'explor mer* 131:7.

The three-nation arctic expedition of 1949-1952. *Am. Scand. Rev*. 40:205-12.

1953 On conditions for the vernal blooming of phytoplankton. *J. Cons.* 18:287-95.

The currents off the coast of Queen Maud Land. *Nor. Geogr. Tidsskr*. 14:239-49.

Some problems in arctic meteorology. *Proceedings of the Toronto Meteorological Conference*, pp. 69-73.

1954 Oceanography: the earth as a planet. In *The Solar System*, vol. 2, pp. 215-57.

Tidal currents off the antarctic ice barrier, Queen Maud Land. *Arch. Meteorol. Geophys. Bioklimatol*. 7:385-90.

Polhavet. *Norsk Nautisk Almanakk og Sjøfartskalender* 337-42.

1955 The existence of a submarine ridge crossing the Polar Sea, predicted by J. E. Fjeldstad in 1936. *Nor. Geogr. Tidsskr*. 15:76-77.

The place of physical oceanography in oceanographic research. *J. Mar. Res.* 14:287-94.

With G. E. Deacon, H. Stommel, and C. W. Thorntwaite. Discussion on the relationship between meteorology and oceanography. *J. Mar. Res.* 14:499-515.

Roald Amundsen. In *One Hundred Norwegians*, pp. 156-60. Oslo: Johan Grundt Tanum Forlag.

1956 Roald Amundsen. In *They Were from Norway: Portraits of Ten Men Who Made History*, pp. 86-100. Oslo.

Transport of Heat by the Currents of the North Atlantic and North Pacific Ocean. Frstskrift til professor Bjorn Helland-Hansen. Benger, pp. 226-36.

Norway's aid to India. *Am. Scand. Rev.* 44:19-26.

Arctic sea ice. In *The Dynamic North*, Book 1.

Oceanography of the Arctic. In *The Dynamic North*, Book 1.

Rekkevidden av de eksakte naturvitenskaper. Fra Universitetets Talerstol.

Report on the Photogrammetrical Work Carried Out by Norsk Polarinstitutt 1938-1955. *Photogrammetry in Norway*, pp. 7-10.

1957 Oceanography. *Handb. Phys.* 48:608-70.

Fridtjof Nansen. *Encyclopaedia Britannica*.

Tätigkeit des Norwegischen Polarinstituts. *Petersmanns Geographische Mitteilungen* 101:114.

Dictionary of Physics. London: Pergamon Press.

Finn Malmgren. *Polarboken* 85-90.

The stress of the wind on the ice of the Polar Sea. *Skriften Norsk Polarinstitutt* 111.

Introductory speech. Polar Atmosphere Symposium, Part II, Ionospheric Section, pp. xi-xiii.

Verden er full av Muligheter. *Arbeiderbladet*.

Champ B. Tanner

November 16, 1920—September 22, 1990

WILFORD R. GARDNER

CHAMP BEAN TANNER the scientist cannot be separated from Champ Tanner the individual. There was a transcendent integrity to his personality that permeated everything he did. He could be blunt, candid, and forthright, but he was never lacking in compassion and concern for students, colleagues, friends, and family. To know Champ was to know his inimitable wife, Kay, and to become adopted into their far-flung extended family as a full-fledged member.

Champ was born in Idaho Falls, Idaho, on November 16, 1920, of Mormon pioneer stock. His life exemplified the goal-oriented determination, regardless of physical or financial impediment, that was characteristic of his forbearers. His father, a construction engineer, died as a result of saving a co-worker from drowning in an accident early in Champ's life, leaving his widowed mother to provide for Champ and his two younger brothers. His mother proved to be a remarkable woman, and there was little doubt that both nature and nurture were strong determinants in Champ's life course. She eventually obtained a position as professor of English at Brigham Young University when women professors at any institution were rare and when their work was never so highly valued as that of male colleagues. She became a legendary and beloved mentor and, much too late, was honored as one of the most outstanding teachers ever to serve on that faculty. Her high standards and appreciation for the English language were not lost on Champ, whose impatience with verbosity in convoluted writing made thesis writing a feared task among his students.

Champ was one of a large and talented group of young men who were identified early in their student careers by Tommy Martin, a second beloved and legendary figure at Brigham Young University. Tommy not only encouraged his students to go on to graduate school, preferably in soil science, but also managed to find graduate assistantships for most of them at outstanding eastern universities. Champ proved to be one of the best of this distinguished group. Professor Martin was very concerned that the military draft would require that Champ enter the service and thus interrupt Champ's education at a critical point. Following a fatherly interview, he suggested that Champ and his sweetheart, Catherine Cox (she is never called Catherine, always Katie or Kay), get married immediately. He suggested they could do so secretly and continue to live in separate domiciles until an appropriate time came to reveal the marriage to their parents. Champ's marital status would, Martin hoped, help to keep him out of the clutches of the military. As tempting as this suggestion was, it was not immediately accepted. Champ and Katie were married in the fall of 1941, prior to completion of his studies at Brigham Young University in the spring of 1942, without regard for any intentions of the local draft board.

It was under Professor Martin's influence that the promising freshman chemistry student traveled immediately to North Carolina State University to carry out graduate studies in soil science. Champ's description of his days at North Carolina is difficult to reduce to the page. His adviser was of the school of thought that did not want students to make any move prior to full consultation and permission. Champ, who had worked all of his young life to help support the family, was unaccustomed to sitting around awaiting instructions, especially when such instructions often seemed trivial and pointless. It took less than a year for him to decide that military service was not only inevitable but much preferred to his present status. By this time he had already enlisted in the Army Reserve, and in the fall of 1942 he was called to active duty with the Signal Corps. The training Champ received in the Corps in the rapidly developing field of radio and electronics was to serve him well in his later research career. In 1944 he was commissioned an officer and assigned duties as an automotive officer. He was discharged in August 1946.

Champ was disinclined to return to North Carolina, but, to the good fortune of soil science, he already had in hand an open offer of an assistantship at the University of Wisconsin. One of Champ's classmates at Brigham Young had gone to Wisconsin for graduate school. This friend was so effusive in his praise of Champ that Emil Truog, the friend's major professor at Wisconsin, wrote Champ while he was still in the service and offered him an assistantship, to be taken up at such time as he was free.

THE FORMATIVE YEARS

Emil Truog was one of that fabled class of professors of the era known best as benign tyrants. Truog was then chairman of the Department of Soils. Though his own training at Wisconsin culminated with the degree of master's of science in chemistry, by the time of his retirement he had mentored over 175 graduate students, most of whom took Ph.D. degrees. Truog was an acknowledged giant in the budding field of soil science. He was insightful, demanding, creative, opinionated, and compassionate. After his ex perience at North Carolina and in the military, the thought of a martinet as a major professor held no fears for Champ. Though Truog showed tremendous concern for all his students, Champ probably most nearly exemplified Truog's own philosophy about science. One of Champ's favorite Truogisms was: "Here you are swatting at mosquitoes while an elephant is trampling all over you!" Truog also preached that if you wanted to think clearly you should "get up with the birds." Champ went Truog one better, something not easy to do. He was almost always up before the birds and into the office well before daybreak, summer or winter. This habit was initially essential since Champ and his now growing family lived at Badger Village, a student housing complex near Baraboo, Wisconsin, some 35 miles from campus. One had to be up early to catch the bus to campus, and Champ never forsook this early habit.

During his graduate student days at Wisconsin, Champ and Kay demonstrated the determination with which they faced life and its adversities. An epidemic of poliomyelitis swept the country in 1950-51 and hit many college communities especially hard. Champ contracted the virus, and he and Kay battled it with both tremendous determination and optimism. Champ never fully recovered the use of his stomach muscles, but he never allowed the consequences to deter him from whatever physical task was at hand. Despite his disability, the Army insisted that his reserve classification status should remain "Erosion Control Specialist." The military underestimated Champ's determination and eventually capitulated to reality and a superior force and reclassified him.

Champ completed his Ph.D. degree in soil physics with M. L. Jackson (NAS, 1986) as his scientific mentor and E. E. Miller of the physics department as his adviser in physics and lifelong friend. Champ was certain that his illness would prevent completion of his graduate studies. Truog and the department saw it otherwise and assured Champ that his only responsibility was to overcome to the extent possible the illness that had beset him. As a result, Champ developed a loyalty to the soils department and to the University of Wisconsin that was unwavering. He felt a debt that he could never adequately repay. Nonetheless, over his career, repay it he did, many times over.

Champ found as he approached graduation that many institutions were leery of hiring as a faculty member someone with a possible physical disability. Job offers were few, despite the rapid growth of the field of soil physics. Again, Truog's unerring judgment came to the rescue. Truog had a policy of keeping the best graduate students at Wisconsin as the department built up following World War II. Since Wisconsin at that time was turning out many of the best soils students in the country, this was more than chauvinism: it was hard-headed pragmatism. It was almost inevitable that Truog offer Champ a position at Madison. He was clearly an exceptional individual, and his sense of obligation to the university in view of the support given him during his illness made his acceptance of an offer inevitable. Over the years Champ was to receive many feelers about moving elsewhere, but he never gave any encouragement. His loyalty to Wisconsin was unwavering.

THE SOIL PHYSICS YEARS

Champ's entry into academic life as a faculty member emulated that of his mentor, Truog. When his first graduate student, R. J. Hanks, could not find housing in Madison, Champ put the necessary plumbing in an upstairs bedroom and invited John and his wife to stay with them. While later students were able to find their own housing, the pattern was set. It was a rare visiting scientist in Madison who was not invited to stay with Champ and Kay. Once familiar with their hospitality and Kay's cuisine, they rarely declined subsequent invitations. They did this with the full knowledge that, unless they had their own transportation from Middleton to campus, they would have to rise no later than 5:00 a.m. Even then, they would find Champ waiting for them so he could start his day.

Champ's first work in soil physics was along rather classical lines. His first dozen or so papers were devoted mainly to improved methods for the characterization of the physical properties of soils and soil materials. He demonstrated early his flair for improving experimental equipment and techniques to which he turned his attention. He developed improved methods for measuring water retention by soil and for measuring particle size distribution, air-filled porosity, and permeability. This was a time during which the field of soil physics was exploding rapidly, with many universities developing teaching and research programs in this area. While the fundamental physical concepts were in place, experimental techniques for both laboratory and field were generally crude and imprecise. Champ made significant improvements in every technique he addressed but, more importantly, laid the foundation for his keen understanding of the physics of soil systems.

THE MICROMETEOROLOGY PHASE

It was not until he turned his attention to the energy budget of soils, however, that Champ truly showed his talent for originality in experimentation while focusing on the most basic problems at hand. The work by Penman at the Rothamsted Experiment Station in England had laid the theoretical basis for the understanding of evaporation from crops and soils. Champ was among the leaders of an ever-growing number of researchers attracted to this area of work. A rich collaboration was begun with his colleague, Verner Suomi, and a progression of outstanding students as they began to explore the rapidly expanding area of evaporation and transpiration from plants and soil as part of the larger effort on the earth's energy budget.

In characteristic fashion, Champ first examined critically the methodology and instrumentation used in the field. This pattern was to be repeated several times in his career. He would almost invariably find ways to improve the precision and reliability of a measurement. He emphasized to his students in the strongest possible terms that instruments had to be "kept honest" or they would give the researcher misleading or incorrect results. Manufacturers' calibration curves were never to be trusted and were always to be verified or corrected. The amount of water used by crops had become a very controversial issue by this time. It had become well recognized that glass-house measurements did not duplicate external conditions adequately and that only field measurements were meaningful. Most data available were inferred from soil water content measurements. For many reasons such measurements lack precision and, even worse, do not account for drainage from the soil profile. Direct measurements offered the best hope of resolving the issues. Over the next decade almost every known or proposed experimental technique was investigated. The ratio of vapor flux to heat flux above a plant canopy is a critical quantity in many theories, and much effort around the world was focused on these flux measurements. Champ was one of the leaders in this effort. Stomatal conductance measurements were improved. Net radiation measurements above crops and bare soils were addressed and improved. In a highly active area of research, Champ's efforts often went beyond those of most colleagues. He designed and built two very precise weighing lysimeters. One was a cylindrical metal tank about 5 meters in diameter, and the other was a rectangular tank, about 2 by 3 meters. Both were over about a meter deep and were filled with soil packed to simulate a soil profile. A suction drainage system combined with a very precise weighing mechanism permitted measurements of evapotranspiration over periods of time as brief as 15 minutes. All the more remarkable was the fact that Champ kept these installations functioning for over fifteen years, despite the problems of winterizing the equipment to ensure survival through the bitter Wisconsin winters. As a legacy of his polio, pleurisy was a constant threat as he worked underground beneath the lysimeters. Nevertheless, Champ always gave every detail his personal attention.

On the other end of the measurement scale was the eddy correlation method, in which the heat content of individual wind eddies is correlated with the movement of individual eddies. This requires high-speed wind velocity and thermal measurements. Virtually every aspect of evaporation and transpiration received the Champ Tanner touch, and a large cadre of well-trained students began to be graduated. Champ was a leader in setting up a joint program with a number of midwestern universities to provide field instrumentation and experiments for biologists.

By 1965, workers in the field had worked out the general physics of water loss from cropped surfaces and were beginning to explore some of the more esoteric issues. Champ felt that he had pushed the problems of transport in the lower atmosphere about as far as he could. There were many unsolved problems, but the complexity of the plant canopy convinced him that something more than straightforward transport equations would be required to deal with this situation. Simply coupling the stomatal resistance with a canopy resistance term worked remarkably well in many cases, but he found it a very unsatisfactory approach.

During the 1960s and early 1970s Champ was one of the major driving forces behind an exciting experiment in cooperative research between the University of Wisconsin and the Agricultural Research Service (ARS) of the U.S. Department of Agriculture. This joint effort resulted in the establishment of the Hydrology Research Group, staffed by Champ, E. E. Miller, G. W. Thurtell, and W. R. Gardner from the university and P. A. C. Raats, C. Dirksen, and R. Amerman from the USDA. This assembly of scholars attracted an outstanding group of visiting scholars, postdocs, and graduate students. Seminars were not to be missed, as almost every facet of any subject of interest to any participant could lead to stimulating and enlightening debates. The entire group was singularly productive. Though it was an unquestionable scientific and educational success, it was too fragile to survive the mindless random motion characteristic of the Washington bureaucracy, and during one of many reorganizations of the ARS it was simply dissolved.

THE PLANT PHYSIOLOGY PHASE

Partly because it was not clear how to push the transport problems forward and partly as a result of an extremely stimulating sabbatical spent with John Passioura in Australia, Champ turned his attention from the plant environment to the response of the plant to its environment. Once again, he started with the literature, reading critically virtually every paper published in English on plant-water relations, making notes as though he were reviewing them for publication. Within a few months it would be hard to argue that any plant scientist had as thorough a knowledge of the literature of plant response to water stress and of the weaknesses in the experiments as did Champ. No physical measurement was ever too difficult for Champ to attempt, and he was soon into the business of building thermocouple psychrometer and plant pressure chambers.

While Champ loved understanding physical systems for understanding's sake, he always had a pragmatic end in view. His work with the lysimeters resulted in data designed to improve irrigation efficiency in order to reduce the leaching of fertilizer to ground water. Not content to work with easy plants, Champ chose to work with potatoes, an important crop in the central Wisconsin sand plains. He did not stop at measuring the water status of the plant leaves but set himself the task of observing directly the turgor of the potato tubers. Loss of turgor at a critical period could result in misshapen and less valuable potatoes. Measuring tuber elongation in situ did not daunt Champ, despite the need for minimal disturbance. The task of observing with precision minute droplets of exudate forced out of the tubers in laboratory pressure chambers in order to measure their turgor was approached with confidence. With the encouragement and advice of Arthur Kelman, Champ attacked the question of the relation between the plant water status or the water status of the tuber and certain tuber diseases.

Champ also studied the water relations of alfalfa, another difficult plant structure with which to deal. One of his favorite experiments dealt with the effect of direct solar radiation on onion umbels during flowering. This problem appealed to him very much because, geometrically, it was a sphere sitting atop a cylinder. Where else in the plant kingdom could one find an experimental arrangement so conducive to simulation? One of those simulations consisted of a styrofoam sphere covered with different densities of sequins, in order to achieve variable roughness. He and his students found the actual heat transfer from the onion umbel to the atmosphere to be greater than the theoretical, but, more importantly, they showed that the "sun scald" observed in the onion seed fields in Idaho resulted when solar radiation and wind were from the same direction. This project brought Champ almost as much fun as his favorite project on heat transfer. Always open to challenging problems whatever their context, he collaborated with colleagues from animal science to work out the temperature distribution and evaporative heat exchange in the scrotal system of the boar. Under pressure, he would confess that his approach was less "hands-on" than was normal for him.

Champ was also a keen student of science. F. H. King had held the first chair of agricultural physics in the United States, and Champ was fully conversant with all his work and felt a strong kinship with King. The two careers spanned a century of soil physics in the United States, and between them there were few important problems in the field that they did not address and did not bring more physical science to.

Champ had a unique ability to synthesize information from an extremely diverse set of experiences, theories, speculations, and observations. His career contributions are probably best summed up in the 1983 review paper with Sinclair. In simplistic terms they showed that the production of total dry matter by a plant was directly proportional to the water transpired and inversely proportional to a mean saturation deficit of the atmosphere. While C3, C4, and CAM plants all differed, their transpiration efficiency, countless generations of plant breeding, advertent or inadvertent, had served to change these efficiencies hardly at all. While this oversimplifies the actual situation, the conclusions pointed out clearly the directions that future research must take, if the relation between crop water use and crop growth was to be altered in desired directions. The heated controversy that had characterized soil physics and crop physiology for decades was now resolved.

THE FINAL YEARS

Champ was honored with a named professorship at Wisconsin, appropriately named after Emil Truog. He also received the Soil Science Research Award from the Soil Science Society of America and the Outstanding Achievement in Biometeorology Award from the American Meteorology Society. At one time he was an associate editor of journals in three distinct disciplines. He often expressed the complaint that editors seemed to send him only the most difficult papers to review. He was quite correct in this. If an editor had a paper that was certain to rouse the ire of an important and contentious scientist, it was a sure bet that Champ would get it for review.

His work hours were legendary. His standards of science and personal integrity were almost unrealistically high. His willingness to debate politics with even the most ardent partisan, coupled with the unfailing generosity and hospitality of the Tanner home, meant that an evening at the Tanner home was a never-to-be-forgotten experience. The stories his students now pass on to their students may sound apocryphal to those who did not know Champ. But it was impossible to exaggerate where Champ was concerned. He was entirely without guile and what you saw was what you got. The Tanners' youngest son, Clarke, a gifted pianist with a promising career ahead of him, died of leukemia just before he was to accept a music scholarship at Milton College. Despite such heartaches and his own physical limitations, Champ never lost his zest for life and learning that buoyed up all those who knew him.

At a time in his life when he might well have followed the tradition of many of his colleagues and started slowing down and enjoying the fruits of his labors, Champ remained entirely true to his character. He was elected to the National

Academy of Sciences in 1981, the first such soil scientist thus recognized. He took this not so much as a well-deserved honor but as a call to duty. He worked conscientiously to seek out and nominate others deserving of recognition. He accepted appointment to the Board on Agriculture of the National Research Council and played a very active role on the board. Finally, although he detested paperwork with great fervor, his loyalty to his campus and his department compelled him to accept the chairmanship of the soils department. He undertook this assignment in the only way he knew how, with thoroughness, candor, and selflessness. A series of key retirements threatened to tarnish the luster of what had been one of the top such departments in the world. Champ set about a vigorous effort to obtain positions and fill them with the best scientists available. At the same time he continued working with his students.

Champ found great satisfaction in working with his oldest son, Bert, who, trained in geophysics, eventually entered the private sector with a small, creative company producing data logging and processing systems. His middle son, Myron, trained in hydrology, also directed his talents to the private sector. The Tanners had two daughters, Taffy and Terri, whose own careers have demonstrated that they inherited both the capabilities and the standards of their parents. Both of Champ's brothers, now deceased, were talented engineers.

On the occasion of his retirement, Champ's colleagues honored him with a Symposium on Biophysical Measurements and Instrumentation at the annual meeting of the American Society of Agronomy in November 1988. Selected papers from the symposium were printed in the journal *Theoretical and Applied Climatology* (vol. 42, 1990). Despite the knowledge that his pancreatic cancer was almost certain to prove fatal, Champ maintained his work schedule to the limit of his physical ability and did as much as he could to put his personal and professional affairs in order. His life's work included some 150 technical articles, book chapters, and reports, as well as more than three dozen theses supervised.

Selected Bibliography

1956 With V. E. Suomi. Lithium chloride dewcel properties and use for dewpoint and vapor-pressure gradient measurements. *Trans. Am. Geophys. Union* 37:413-20.

1958 With V. E. Suomi. A max-min dewpoint hygrometer. *Trans. Am. Geophys. Union* 39:63-66.

With V. E. Suomi. Evaporation estimates from heat budget measurements over a field crop. *Trans. Am. Geophys. Union* 39:298-304.

1960 With W. L. Pelton and K. M. King. An evaluation of Thornthwaite and mean temperature methods for determining potential evapotranspiration. *Agron. J.* 52:387-95.

With W. L. Pelton. Potential evapotranspiration estimates by the approximate energy balance method of Penman. *J. Geophys. Res.* 65:3391-3413.

With J. A. Businger and P. M. Kuhn. The economical net radiometer. *J. Geophys. Res.* 65:3657-67.

1961 A simple aero-heat budget method for determining daily evapotranspiration. *Transactions of the 7th International Congress on Soil Science*, vol. 1, pp. 203-9.

1962 With E. R. Lemon. Radiant energy utilized in evapotranspiration. *Agron. J.* 54:207-12.

1963 Plant temperatures. *Agron. J.* 55:210-11.

1966 With C. A. Federer. The spectral distribution of light in the forest. *Ecology* 47:555-60.

With C. A. Federer. Sensors for measuring light available for photosynthesis. *Ecology* 47:654-57.

With M. Fuchs. Infrared thermometry of vegetation. *Agron. J.* 58:597-601.

1967 With D. H. Sargeant. A simple psychrometric apparatus for Bowen ratio measurements. *J. Appl. Meteorol.* 6:414-18.

1968 With M. Fuchs. Evaporation from unsaturated surfaces: a generalized combination method. *J. Geophys. Res.* 73:1299-1304.

With T. A. Black and G. W. Thurtell. Hydraulic load cell lysimeter construction, calibration, and tests. *Soil Sci. Soc. Am. Proc.* 32:623-29.

1969 With M. Fuchs, G. W. Thurtell, and T. A. Black. Evaporation from drying surfaces by the combination method. *Agron. J.* 61:22-26.

With E. T. Kanemasu and G. W. Thurtell. The design, calibration, and field use of a stomatal diffusion porometer. *Plant Physiol.* 44:881-85.

With J. M. Norman and G. W. Thurtell. Photosynthetic light sensor for measurement in plant canopies. *Agron. J.* 61:840-43.

With E. T. Kanemasu. Stomatal diffusion resistance of snap beans. 1. Influence of leaf-water potential. *Plant Physiol.* 44:1542-46.

With S. M. Goltz, G. W. Thurtell, and F. E. Jones. Evaporation measurements by an eddy correlation method. *J. Water Resour. Res.* 6:440-46.

1970 With G. W. Thurtell and M. L. Wesely. Three-dimensional pressure-sphere anemometer system. *J. Appl. Meteorol.* 9:379-85.

1972 With M. L. Wesely and G. W. Thurtell. An improved pressure-sphere anemometer . *Boundary-Layer Meteorol.* 2:275-83.

1975 With P. W. Gandar. Comparison of methods for measuring leaf and tuber water potentials in potato. *Am. Potato J.* 52:387-97.

1976 With P. W. Gandar. Leaf growth, tuber growth, and water potential in potatoes. *Crop Sci.* 16:534-38.

1983 With T. R. Sinclair. Efficient water use in crop production: research or re-search? In *Limitations to Efficient Water Use in Crop Production*, ed. H. M. Taylor, H. R. Jordan, and T. R. Sinclair, pp. 1-28. Madison, Wisc.: American Society of Agronomy.

Harland Goff Wood

September 2, 1907–September 12, 1991

DAVID A. GOLDTHWAIT AND RICHARD W. HANSON

HARLAND GOFF WOOD, WHO was descended from William Goffe (b. 1619), one of the appointed judges responsible for the beheading of King Charles I, was born on September 2, 1907, in the small town of Delavan, Minnesota. His parents, both of whom had only a high school education, taught their four sons and one daughter to work hard and to be self-reliant—the result for the sons: two Ph.D.s, one Ph.D.-M.D., one M.D., and one LL.B; and for the daughter: an honorary LL.D. It is hard to picture Harland Wood as a frail child who spent two years in kindergarten and two years in the first grade. He and his brothers helped on the family's farm in Mankato, Minnesota, walking the mile home from school at noon to water the stock and then running back after lunch. At Macalester College in Minnesota, he majored in chemistry and there met Mildred Davis, whom he married in 1929. In 1931 he was accepted as a graduate student in bacteriology at Iowa State University at Ames by C. H. Werkman, who was starting to investigate the chemistry of bacterial fermentations. It was there that Harland made his stunning discovery of CO_2 fixation, which up to that time was known to occur only in chemosynthetic and photosynthetic autotrophs. This idea was so controversial that for some time Professor Werkman doubted the validity of Harland's findings.

From 1935 to 1936 Harland worked as a fellow with W. H. Petersen at the University of Wisconsin, and it was here that he joined Ed Tatum in studying the growth factor requirements for propionibacteria. Harland returned to Werkman's department in 1936 to focus on CO_2 fixation, as will be discussed. Although Harland was tremendously productive at Ames, building a thermal diffusion column for the isolation of ^{13}C as well as a mass spectrometer to measure the isotope, Werkman would not initially allow him to work on animals and would not arrange for Harland's future independence at Ames. And so in 1943 he moved to the Department of Physiological Chemistry at the University of Minnesota, and it was there that he used ^{13}C-$NaHCO_3$ labeling of the different carbon atoms of the glucose of rat liver glycogen to study the pathways of glucose synthesis.

In 1946 Harland accepted the position of chairman of the Department of Biochemistry at the School of Medicine of what was then Western Reserve University in Cleveland, Ohio, on the condition, as he told Dean Joseph Wearn, that he be allowed to go deer hunting with his father and four brothers each autumn. He loved duck and deer hunting and even at seventy-nine years of age was seen 35 feet up a tree waiting for a deer. As chairman he brought in an entirely new faculty that was oriented to the use of isotopic tracers to study a variety of metabolic pathways. Under Harland's direction, this young and energetic group, which included future members of the National Academy of Sciences, Merton Utter and Lester Krampitz, created an outstanding national reputation for the department. At the local level, he was also unique. Harland instituted a policy that all honoraria, even for participating in study sections, should go into a student travel fund, since he felt that out side activities should have an intrinsic value based on science and not on money —echoes of William Goffe. Departmental seminars were at noon on Saturday and monthly staff meetings were held after that, often until 5:00 p.m., when they were terminated by telephone calls from irate wives. There was a pooling of resources, a sharing of all equipment, and a camaraderie that would be difficult to equal in these times.

Harland Wood spent the last forty-five years of his career at Case Western Reserve University (Western Reserve University merged with Case Institute of Technology in 1968). He retired as chairman in 1965 so that he could have more time for research, and for Harland this meant research at the bench, not just at the desk. He continued "pounding the bench," as he called it, right up until a few days before his death on September 12, 1991. Lymphoma was diagnosed four years before his death; he died of a fall that resulted in a ruptured spleen. Harland had undergone chemotherapeutic cycles several times, but they never significantly halted his scientific activities. At the time of his death, he held three grants from the National Institutes of Health, had a working group of fifteen associates, and was writing nine manuscripts. At the last meeting of the ASBMB that he attended, he had twelve posters on display and was present to discuss results related to each of them. Between his seventieth birthday and his death, he published ninety-six papers, all in well-respected journals—surely a record for an "elderly" biochemist. He is survived by his wife Mildred and two daughters.

Harland Wood left a long and distinguished record in the life sciences, beginning with his pioneering work with C. H. Werkman at Iowa State College, which demonstrated for the first time that CO_2 is utilized in heterotrophic organisms. In 1935 he demonstrated that the prevailing dogma that CO_2 was utilized only by bacterial autotrophs was incorrect. In a series of studies he determined the products formed from the fermentation of glycerol by propionic acid bacteria in a bicarbonate buffer system and calculated the carbon and oxidation-reduction balances to account for the carbon of the fermented substrate and to ensure that there was a balance of the oxidation-reduction state of substrates and products. Surprisingly, more carbon was found in the products than was supplied by the fermented glycerol. He subsequently discovered that the extra carbon was derived from CO_2 in the buffer and that oxidation balanced reduction when the reduction of CO_2 was taken into account. He proposed that CO_2 and pyruvate combined to form oxalacetate, which subsequently was reduced to succinate. This pyruvate-CO_2 reaction became known as the Wood-Werkman reaction.

When isotopic tracers of carbon became available in the late 1930s, Harland was among the first to exploit isotopes in biological studies. He was a true pioneer in developing procedures for the use of these isotopes for metabolic tracer studies. As previously noted, he built a water-cooled thermal diffusion column in a five-story elevator shaft for the separation of ^{13}C isotopic carbon. Harland was always fond of describing the day that he found the column warped and distorted due to a temporary drop in the water pressure. This drop, he finally discovered, occurred when the home economics class let out and three toilets were flushed simultaneously! To measure ^{13}C, he also built a mass spectrometer. His innovative work initially provided evidence that citrate was not part of the citric acid cycle because he had assumed that citrate was a symmetrical molecule. In his characteristic manner, he later said in a Lynen Lecture that even though he was wrong it was one of his "most important contributions" to biochemistry. The studies by Wood and his colleagues in 1945 clearly demonstrated the pathway of CO_2 incorporation into specific carbon atoms of glucose derived from hepatic glycogen. Harland graduated briefly from bacteria to cows, where his farm background helped in the injection of 14C glucose into the artery going to the right udder. Subsequently, by personally milking each side, he determined that lactose was synthesized from free glucose rather than glucose-1-phosphate and that it was glucose that reacted with UDP-galactose to form lactose. In collaboration with Joseph Katz and Bernard R. Landau, Harland also developed methods to estimate the proportion of carbohydrate metabolized in the pentose pathway and glycolysis by studying 14C distributions in glucose and glycogen. These latter studies were instrumental in establishing the stoichiometry of the pentose pathway.

The overall direction of Harland's research over sixty years continued to be on CO_2 fixation. During the last thirty years of his life, he focused on establishing the reaction mechanism of transcarboxylase (TC) from propionibacteria. This is a key enzyme in the propionic acid cycle, and it transfers a carboxyl group in the conversion of methylmalonyl CoA + pyruvate to propionyl CoA + oxalacetate. The enzyme is also extremely complex, with six identical central subunits, each with two CoA-binding sites, six dimeric outside subunits each of the six with two keto acid sites, and twelve small biotinyl subunits that carry the carboxyl groups between the CoA and keto sites. The kinetics of the reaction did not fit the accepted mechanisms, so Dexter Northrup, then a student with Harland, proposed a new kinetic mechanism for TC that was later verified by Northrup and Wood. Extensive work was done on the separation of the three subunits of TC and on the reconstitution of enzyme activity. Together with a number of associates, Wood studied the quaternary structure of TC by electron microscopy, and this revealed the "Mickey Mouse" enzyme. Using thin crystals of the enzyme, resolution of the structure at 10 Å was possible by microscopy. The primary amino acid sequence of the biotinyl subunit was determined, and, in collaboration with David Samols, the genes for all three subunits were cloned and sequenced. At the end of his life, Harland was studying the enzymatic properties of a large number of mutants that were generated in the three different subunits and was doing many of the enzyme assays himself. These studies were of great interest because of the complexity of the subunit structure of the enzyme and the ability to examine different aspects of function.

Harland Wood also discovered a novel pathway for carbon monoxide (CO) fixation in acetogens, a group of anaerobic bacteria that synthesize acetate from CO or CO_2/H_2. This new pathway of autotrophic growth, demonstrated in *Clostridium thermoaceticum* and *Acetobacterium woodii*, differs from all previously described pathways for autotrophic growth, such as the Calvin reductive pentose cycle or the tricarboxylic acid cycle. Much of Harland's work in the area was done in collaboration with Lars Ljundahl, both at Case Western Reserve University and the University of Georgia. The mechanism of this pathway involves reduction of CO_2 to methyltetrahydrofolate and transfer of the methyl group to a corrinoid protein. The methyl group is then transferred to carbon monoxide dehydrogenase (CODH); CO and CoASH/moieties combine with CODH, which catalyzes the formation of acetyl-CoA from the above three groups. Thus, CODH plays a central role in this pathway. Most of the enzymes involved in the various steps of the pathway were purified to homogeneity. The availability of purified enzymes permitted Harland and his collaborators to dissect the pathway and define the role of each enzyme. Detailed studies toward elucidating the mechanism of action of CODH were initiated. CODH contains six nickel, three zinc, thirty-two iron atoms, forty-two labile sulfides and has three acceptor sites: one for the methyl group transferred from the methyl corrinoid enzyme, a CO site, and a CoASH site. From ESR studies it was shown that the Ni-Fe center is involved in the interaction of the CO group with CODH. Also, the methyl group is bound to a cysteine residue of CODH. The CoASH substrate site has been characterized using fluorescence spectroscopy, circular dichroism, and chemical modification. From these studies it was proposed that both tryptophan(s) and arginine(s) are involved in the binding of CoASH to CODH. Even from this brief review it is clear that Harland Wood, over the sixty years that he was involved in research, "followed the trail of CO_2."

Harland Wood was also a pioneer in studying the role of pyrophosphate and polyphosphate as energy sources. It has long been accepted that the energy contained in the anhydride bond of pyrophosphate is not utilized efficiently by cells. However, Harland, together with Nelson Phillips, showed this not to be true by the isolation and characterization of bacterial enzymes that utilize pyrophosphate in reaction with oxaloacetate, with phosphoenolpyruvate, and with fructose-6-phosphate. Inorganic polyphosphates have been considered by others as primitive sources of energy. Harland extensively studied the enzymatic synthesis of polyphosphate from ATP and showed that a bacterial glucokinase utilizes polyphosphate much more effectively than ATP in the reaction with glucose. Two separate sites exist on the enzyme for these two sources of high-energy phosphate. This enzyme may represent an intermediate stage of evolution from a polyphosphate-dependent metabolism to an ATP-dependent metabolism.

Harland Wood's outstanding career was marked by many innovations. However, what most characterized Harland was his scientific style. He was remarkable for several reasons. First, one could always feel the sense of excitement and drive that he brought to the experimental aspect of science. The focus of the excitement was always on discovery. Second, he continually developed and applied the latest technology to his experimental problem. There were many jumps from fermentation balances all the way to gene sequencing. Finally, he was able to collaborate with others very productively, particularly those with expertise in specific areas where the scientific results could not have been achieved by either group alone. The flavor of the man and his approach to science are best captured by Harland himself in his autobiography in the *Annual Review of Biochemistry* in 1985.

Harland Wood's outstanding career was marked by many innovations in other areas. As chairman of the biochemistry department at Western Reserve University, he led the curriculum reform that resulted in an integrated organsystem-based method for teaching the first two years of medical school; this curriculum has had a great impact on medical education nationally. He swayed the faculty to vote for the new curriculum with the challenge, "How do you guys know it's not going to work unless you run the experiment?" He served as chairman of the biochemistry department for twenty years, as dean of sciences at Case Western Reserve University from 1967 to 1969, and finally as university professor and university professor emeritus from 1970 to 1991.

Harland Wood was president of the American Society of Biological Chemistry from 1959 to 1960. First as secretary-general and then as president of the International Union of Biochemistry in 1982–83, he did a great deal for that organization's revitalization. He served on many study sections, and his strong support for younger biochemists during his tenure on one of those study sections became known as "The Wood Factor." He was a member of many advisory boards and served as an editorial board member of a number of important journals. As a young member of the Editorial Board of the *Journal of Biological Chemistry*, he was instrumental in eliminating self-perpetuating appointments when he resigned after five years and argued, "Listen, if all you guys died tomorrow, a good board could be picked the next day to replace you." He received a number of prestigious awards, including the Eli Lilly Award, the Carl Neuberg Medal, the Lynen Lecture Medal, the Waksman Award, the Rosenstiel Award, the Michaelson-Morly Award, and the National Medal of Science. He held honorary degrees from Macalester College, Northwestern University, the University of Cincinnati, and Case Western Reserve University. He was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the Biochemical Society of Japan and served on the President's Science Advisory Committee under Presidents Johnson and Nixon.

In a 1985 *Annual Review of Biochemistry* article, Harland Wood wrote that "scientists are the fortunate few who earn a livelihood by pursuit of a hobby. This hobby sometimes consumes their every thought, but usually it provides a deeply satisfying life." He continued, "Many highly successful scientists desert the laboratory bench early in their careers and thereafter direct the research of their co-workers. My goal has been to remain personally active in the laboratory as long as I am involved in science." And so he did.

Over the sixty years that Harland Wood spent in science, he made countless friends in many countries who revered him not just for his accomplishments but for his intellectual honesty. Here was a man without pretensions, whose opinions and decisions were always based on principles and not on personal factors, a man whose mind was open to new ideas and concepts, a man who by his example and encouragement got the best out of his associates, and a man who, once he made up his mind, would drive straight toward his goal. In him one felt the warmth, strength, and integrity that made him unique and irreplaceable.

Selected Bibliography

1933 With O. L. Osburn and C. H. Werkman. Determination of formic, acetic and propionic acids in a mixture. *Ind. Eng. Chem. Analyt. Ed.* 5:247-50.

1934 With C. H. Werkman. The propionic acid bacteria: On the mechanism of glucose dissimilation. *J. Biol. Chem.* 105:63-72.

With C. H. Werkman. Pyruvic acid in the dissimilation of glucose by the propionic acid bacteria. *Biochem. J.* 28:745-47.

With C. H. Werkman. Intermediate products of the propionic acid fermentation. *Proc. Soc. Exp. Biol. Med.* 31:938-40.

With C. H. Werkman. The utilization of agricultural by-products in the production of propionic acid by fermentation. *J. Agric. Res.* 49:1017-20.

1935 With C. H. Werkman. The utilization of CO_2 by the propionic acid bacterial in the dissimilation of glycerol. *J. Bacteriol.* 30:332 (Abstract).

With C. H. Werkman. The isolation and possible intermediary role of formaldehyde in the propionic acid fermentation. *J. Bacteriol.* 30:652-53.

1936 With C. H. Werkman. The utilization of CO_2 in the dissimilation of glycerol by the propionic acid bacteria. *Biochem. J.* 30:48-53.

With C. H. Werkman. Mechanism of glucose dissimilation by the propionic acid bacteria. *Biochem. J.* 30:618-23.

With R. W. Stone and C. H. Werkman. Activation of the lower fatty acids by propionic acid bacteria. *Biochem. J.* 30:624-28.

With C. Erb and C. H. Werkman. A macro-respirometer for the study of aerobic bacterial dissimilation. *Iowa State Coll. J. Sci.* 10:295-302.

With C. Erb and C. H. Werkman. The aerobic dissimilation of lactic acid by the propionic acid bacteria. *J. Bacteriol.* 31:595-602.

With O. L. Osburn and C. H. Werkman. Determination of volatile fatty acids by the partition method. *Ind. Eng. Chem. Analyt. Ed.* 8:270-75.

With E. L. Tatum and W. H. Peterson. Growth factors for bacteria. V. Vitamin B12: a growth stimulant for propionic acid bacteria. *Biochem. J.* 30:1898-1904.

With E. L. Tatum and W. H. Peterson. Essential growth factors for the propionic acid bacteria. II. Nature of the Neuberg precipitate fraction of potato. *J. Bacteriol.* 32:167-74.

1937 With A. A. Anderson and C. H. Werkman. Growth factors for the propionic and lactic acid bacteria. *Proc. Soc. Exp. Biol. Med.* 36:217-19.

With C. Erb and C. H. Werkman. Dissimilation of pyruvic acid by the propionic acid bacteria. *Iowa State Coll. J. Sci.* 11:287-92.

With R. W. Stone and C. H. Werkman. The intermediate metabolism of the propionic acid bacteria. *Biochem. J.* 31:349-59.

With E. L. Tatum and W. Peterson. Growth factors for bacteria. IV. An acidic ether soluble factor essential for growth of propionic acid bacteria. *J. Bacteriol.* 33:227-42.

With C. H. Werkman and R. W. Stone. The dissimilation of phosphate esters by the propionic acid bacteria. *Enzymologia* 4:24-30.

1938 With A. A. Anderson and C. H. Werkman. Nutrition of the propionic acid bacteria. *J. Bacteriol.* 36:201-13.

With C. H. Werkman. The utilization of CO_2 by the propionic acid bacteria. *Biochem. J.* 32:1262-71.

With W. P. Wiggert and C. H. Werkman. The fermentation of phosphate esters by the propionic acid bacteria. *Enzymologia* 2:373-76.

1939 With R. W. Brown and C. H. Werkman. Nutrient requirements of butyric acid butyl alcohol bacteria. *J. Bacteriol.* 38:631-40.

1940 With C. R. Brewer, M. N. Mickelson, and C. H. Werkman. A macrorespirometer for the study of the aerobic metabolism of microorganism. *Enzymologia* 8:314-17.

With C. Geiger and C. H. Werkman. Nutritive requirements of the heterofermentative lactic acid bacteria. *Iowa State Coll. J. Sci.* 14:367-78.

With C. H. Werkman. The fixation of CO_2 by cell suspensions of Propionibacterium pentesaceum. *Biochem. J.* 34:7-14.

With C. H. Werkman. The relationship of bacterial utilization of CO_2 to succinic acid formation. *Biochem. J.* 34:129-38.

With C. H. Werkman. *Gewinnung-Freigelester Enzyme Specialmethoden fur Bakterien Die Methoden der Fermentforschung*, ed. Oppenheimer. Leipzig: George Thiem.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Heavy carbon as a tracer in bacterial fixation of CO_2. *J. Biol. Chem.* 135:789-90.

1941 With H. D. Slade, A. O. Nier, A. Hemingway, and C. H. Werkman. Note on the utilization of carbon dioxide by heterotrophic bacteria. *Iowa State Coll. J. Sci.* 15:339-41.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Position of carbon dioxide-carbon in propionic acid synthesized by *Propionibacterium. Proc. Soc. Exp. Biol. Med.* 46:313-16.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Note on the degradation of propionic acid synthesized by *Propionibacterium. Iowa State Coll. J. Sci.* 15:213-14.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Mechanism of fixation of carbon dioxide in the Krebs cycle. *J. Biol. Chem.* 139:483-84.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Heavy carbon as a tracer in heterotrophic carbon dioxide assimilation. *J. Biol. Chem.* 139:365-76.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Heavy carbon dioxide in succinic acid synthesized by heterotrophic bacteria. *J. Biol. Chem.* 139:377-81.

With C. H. Werkman, A. Hemingway, A. O. Nier, and C. G. Stuckwisch. Reliability of reactions used to locate assimilated carbon in propionic acid. *J. Am. Chem. Soc.* 2140-42.

1942 Criteria for experiments with isotopes. In *Symposium on the Respiratory Enzymes and the Biological Action of Vitamins* , ed. E. A. Evans, Jr., pp. 252-56. Chicago: University of Chicago Press.

With H. D. Slade, A. O. Nier, A. Hemingway, and C. H. Werkman. Assimilation of heavy carbon dioxide by heterotrophic bacteria. *J. Biol. Chem.* 143:133-45.

With C. H. Werkman. On the metabolism of bacteria. *Bot. Rev.* 8:1-68.

With C. H. Werkman. Heterotrophic assimilation of carbon dioxide. *Adv. Enzymol.* 2:135-82.

With C. H. Werkman, A. Hemingway, and A. O. Nier. Fixation of carbon dioxide by pigeon liver in the dissimilation of pyruvic acid. *J. Biol. Chem.* 142:31-45.

1943 With G. Kalnitsky and C. H. Werkman. CO_2-fixation and succinic acid formation by a cell-free enzyme preparation of Escherichia coli. *Arch. Biochem.* 2:269-81.

With L. O. Krampitz and C. H. Werkman. Enzymatic fixation of carbon dioxide in oxalacetate. *J. Biol. Chem.* 147:243-53.

1944 Metabolism of nervous tissue in poliomyelitis. *Lancet* 64:240-42.

With R. W. Brown and C. H. Werkman. Fixation of carbon dioxide in lactic acid by *Clostridium butylicum. Arch. Biochem.* 5:423-33.

With R. W. Brown, C. H. Werkman, and C. G. Stuckwisch. The degradation of heavy-carbon butyric acid from butyl alcohol fermentation. *J. Am. Chem. Soc.* 66:1812-18.

With I. I. Rusoff and J. M. Reiner. Anaerobic glycolysis of the brain in experimental poliomyelitis. *J. Exp. Med.* 81:151-59.

1945 With R. W. Brown and C. H. Werkman. Mechanism of the butyl alcohol fermentation with heavy carbon acetic and butyric acids and acetone. *Arch. Biochem.* 6:243-60.

With N. Lifson and V. Lorber. The position of fixed carbon in glucose from rat liver. *J. Biol. Chem.* 159:475-89.

With V. Lorber and N. Lifson. Incorporation of acetate carbon into rat liver glycogen by pathways other than carbon dioxide fixation. *J. Biol. Chem.* 161:411-12.

With I. I. Rusoff. The protective action of trypan red against infection by a neurotropic virus . *J. Exp. Med.* 82:297-309.

With M. F. Utter. Fixation of carbon dioxide in oxalacetate by pigeon liver. *J. Biol. Chem.* 160:375-76.

With M. F. Utter and J. M. Reiner. Measurement of anaerobic glycolysis of brain as related to poliomyelitis. *J. Exp. Med.* 82:217-26.

With M. F. Utter and J. M. Reiner. Anaerobic glycolysis in nervous tissue. *J. Biol. Chem.* 161:197-217.

With B. Vennesland and E. A. Evans. The mechanism of carbon dioxide fixation by cell-free extracts of pigeon liver: distribution of labeled carbon dioxide in the products. *J. Biol. Chem.* 159:153-58.

1946 The fixation of carbon dioxide and the interrelationships of the tricarboxylic acid cycle. *Physiol. Rev.* 26:198-246.

With V. Lorber, N. Lifson, and J. Barcroft. The metabolism of acetate by the completely isolated mammalian heart investigated with carboxyl labeled acetate. *Am. J. Physiol.* 145:557-60.

With M. F. Utter. The fixation of carbon dioxide in oxalacetate by pigeon liver. *J. Biol. Chem.* 164:455-76.

1948 Tracer studies on the intermediary metabolism of carbohydrates. In *Symposium on the Use of Isotopes in Biology and Medicine*, pp. 209-42. Madison: University of Wisconsin Press.

The synthesis of liver glycogen in the rat as an indicator of intermediary metabolism. *Cold Spring Harbor Symp. Quant. Biol.* 13:201-10.

With N. Lifson, V. Lorber, and W. Sakami. The incorporation of acetate and butyrate carbon into rat liver glycogen by pathways other than carbon dioxide fixation. *J. Biol. Chem.* 176:1263-84.

1949 Tracer studies on the intermediary metabolism of carbohydrates. In *Isotopes in Biology and Medicine*. Madison: University of Wisconsin Press.

With V. Lorber. Carbohydrate metabolism. *Ann. Rev. Biochem.* 18:299-334.

With W. Shreeve, V. Fell, and V. Lorber. The distribution of fixed radioactive carbon in glucose from rat liver glycogen. *J. Biol. Chem.* 177:679-82.

1950 Symposium on chemical transformation of carbons in photosynthesis. *Fed. Proc.* 9:553-55.

A consideration of some reactions involving CO_2 fixation. *Symp. Soc. Exp. Biol.* 5:9-28.

With V. Lorber, N. Lifson, and W. Sakami. Conversion of propionate to liver glycogen in the intact rat, studied with isotopic propionate. *J. Biol. Chem.* 183:531-38.

With V. Lorber, N. Lifson, W. Sakami, and W. W. Shreeve. Conversion of lactate to liver glycogen in the intact rat studied with isotopic lactate. *J. Biol. Chem.* 183:517-29.

With H. J. Strecker and L. O. Krampitz. Fixation of formic acid in pyruvate by a reaction not utilizing acetyl phosphate. *J. Biol. Chem.* 182:525-40.

1951 A study of acetone metabolism using glycogen and serine as indicators and the roles of Cl-compounds in metabolism. In *Ciba Foundation Conference on Isotopes in Biochemistry*, ed. G. E. W. Wolstenholme, pp. 227-45. London: Churchill.

With M. F. Utter. Mechanisms of fixation of CO_2 by heterotrophs and autotrophs. *Adv. Enzymol.* 12:41-151.

1952 The metabolism of formate by animals. *Harvey Lect. Ser.* 14:127-48. A study of CO_2-fixation by mass determination of the types of ^{13}C-acetate. *J. Biol. Chem.* 194:905-31.

Fermentation of 3,4-C^{14} and 1-C^{14}-labeled glucose by *Clostridium thermoaceticum. J. Biol. Chem.* 199:579-83.

1953 With F. W. Leaver. CO_2 turnover in the fermentation of 3,4,5 and 6 carbon components by the propionic acid bacteria. *Biochem. Biophys. Acta* 12:207-22.

With F. W. Leaver. Evidence from fermentation of labeled substrates which is inconsistent with present concepts of the propionic acid fermentation. *J. Cell. Comp. Physiol.* 41:225-40.

1954 With G. Popjak. Biological asymmetry of glycerol and formation of asymmetrically labeled glucose. *J. Biol. Chem.* 206:875-82.

With P. Schambye. The *in vivo* conversion of ^{14}C-glycerol into rat liver glycogen. In *Radioisotope Conference*, vol. 1, pp. 346-50.

1955 Significance of alternate pathways in the metabolism of glucose. *Physiol. Rev.* 35:841-59.

With I. A. Bernstein, K. Lentz, M. Malm, and P. Schambye. Degradation of glucose C^{14} with *Leuconostoc mesenteroides*: alternate pathways and tracer patterns. *J. Biol. Chem.* 215:137-52.

With R. G. Kulka and N. L. Edson. Fermentation of glucose-1-C^{14} in cell-free extracts of *Propionibacteria* . *Proc. Univ. Otago* 33:24-25.

With F. W. Leaver and R. Stjernholm. The fermentation of three carbon substrates by *Clostridium propionicum* and *Propionibacterium. J. Bacteriol.* 70:521-30.

With K. Lentz. Synthesis of acetate from formate and carbon dioxide by *Clostridium thermoaceticum. J. Biol. Chem.* 215:645-54.

With R. Stjernholm and F. W. Leaver. The metabolism of labeled glucose by the propionic acid bacteria. *J. Bacteriol.* 70:510-20.

1956 The teaching of biochemistry in an integrated medical curriculum. *Fed. Proc.* 15:865-70.

With R. G. Kulka and N. L. Edson. The metabolism of ^{14}C-glucose in an enzyme system from *Propionibacterium. Biochem. J.* 63:177-82.

With R. Stjernholm and F. Leaver. The role of succinate as a precursor of propionate in the propionic acid fermentation. *J. Bacteriol.* 72:142-52.

1957 *Transactions of the Third Conference of Polysaccharides in Biology*. New York: Josiah Macy, Jr. Foundation.

With I. A. Bernstein. Determination of isotopic carbon patterns in carbohydrate by bacterial fermentation . *Methods Enzymol.* 4:561-83.

With H. Gest. Determination of formate. In *Methods in Enzymology*, vol. 3, ed. S. Colowick and N. Kaplan, pp. 285-92. New York: Academic Press.

With P. Schambye and M. Kleiber. Lactose synthesis. I. The distribution of C^{14} in lactose of milk after intravenous injection of C^{14} compounds. *J. Biol. Chem.* 226:1011-21.

With P. Schambye and G. J. Peeters. Lactose synthesis. II. The distribution of C^{14} in lactose of milk from perfused isolate cow udder. *J. Biol. Chem.* 226:1023-34.

With P. M. L. Siu and P. Schambye. Lactose synthesis. III. The distribution of C^{14} in lactose of milk after intra-arterial injection of acetate-1-C^{14}. *Arch. Biochem. Biophys.* 69:390-404.

1958 Tracer studies on the synthesis of milk constituents. In *Proceedings 2nd International Conference on Peaceful Uses of Atomic Energy*, pp. 50-57. New York: Pergamon Press.

With R. Gillespie, S. Joffee, R. G. Hansen, and H. Hardenbrook. Lactose synthesis. V . C^{14} in lactose, glycerol and serine as indicators of the triose phosphate isomerase reaction and pentose cycle. *J. Biol. Chem.* 233:1271-78.

With J. Katz. The distribution of C^{14} in the hexose phosphates and the effect of recycling in the pentose cycle. *J. Biol. Chem.* 233:1279-82.

With S. H. Pomerantz. A mass analysis study of formaldehyde fixation and cleavage of lactate by *Propionibacterium arabinosum. J. Biol. Chem.* 231:519-31.

With S. Joffe, R. Gillespie, R. G. Hansen, and H. Hardenbrook. Lactose synthesis. IV. The synthesis of milk constituents after unilateral injection of glycerol-1,3-C^{14}. *J. Biol. Chem.* 233:1264-70.

With R. L. Stjernholm. Differential degradation of D- and L- glycerol-1-C^{14} by *A. aerogenes. Arch. Biochem. Biophys.* 78:28-32.

1959 With R. Gillespie, R. G. Hansen, W. A. Wood, and H. Hardenbrook. Arteriovenous $^{14}CO_2$ differences and the pentose cycle in the cow's udder. *Biochem. J.* 73:694-701.

With P. M. L. Siu. Conversion of galactose and glucose to liver glycogen *in vivo. J. Biol. Chem.* 234:2223-26.

1960 With J. Katz. The use of glucose-C^{14} for the evaluation of the pathways of glucose metabolism. *J. Biol. Chem.* 235:2165-77.

With R. L. Stjernholm. Trehalose and fructose as indicators of metabolism of labeled glucose by the propionic acid bacteria. *J. Biol. Chem.* 235:2753-61.

With R. L. Stjernholm. Glycerol dissimilation and the occurrence of symmetrical three-carbon intermediate in the propionic acid fermentation. *J. Biol. Chem.* 235:2757-61.

With R. W. Swick. The role of transcarboxylase in propionic acid fermentation. *Proc. Natl. Acad. Sci. U.S.A.* 46:28-41.

1961 Tracer studies on the mechanism of carbohydrate metabolism. In *Symposium on the Use of Radioisotopes in Animal Biology and the Medical Sciences*, pp 193-203. New York: Academic Press.

With L. G. Ljungdahl, E. Racker, and D. Couri. Formation of unequally labeled fructose-6-phosphate by an exchange reaction catalyzed by transaldolase. *J. Biol. Chem.* 236:1622-25.

With P. M. L. Siu and R. L. Stjernholm. Fixation of CO_2 by phosphoenolpyruvic carboxytrans-phosphorylase. *J. Biol. Chem.* 236:PC21-22.

With R. L. Stjernholm. Transcarboxylase. II. Purification and properties of methylmalonyl-oxalacetic transcarboxylase. *Proc. Natl. Acad. Sci. U.S.A.* 47:289-303.

With R. L. Stjernholm. Methylmalonyl isomerase. II. Purification and properties of the enzymes from *Propionibacteria. Proc. Natl. Acad. Sci. U.S.A.* 47:303-13.

1962 With R. G. Hansen, G. J. Peeters, B. Jacobson, and J. Wilken. Lac tose synthesis. VI. Labeling of lactose precursors by glycerol-1,3-C^{14} and glucose-2-C^{14}. *J. Biol. Chem.* 237:1034-39.

With R. W. Kellermeyer. Methylmalonyl isomerase: a study of the mechanism of isomerization. *Biochemistry* 1:1124-31.

With I. A. Rose, R. W. Kellermeyer, and R. L. Stjernholm. The distribution of C^{14} in glycogen from deuterated glycerol-C^{14} as a measure of the effectiveness of triosephosphate isomerase in vivo. *J. Biol. Chem.* 237:3325-31.

With P. M. L. Siu. Phosphoenolpyruvic carboxytransphosphorylase, a CO_2 fixation enzyme from propionic acid bacteria. *J. Biol. Chem.* 237:3044-51.

With R. L. Stjernholm. Assimilation of carbon dioxide by heterotrophic organisms. In *The Bacteria: A Treatise on Structure and Function*, ed. I. Gunsalus and R. Stanier. New York: Academic Press.

1963 With S. H. Allen, R. Kellermeyer, R. L. Stjernholm, and B. Jacobson. The isolation, purification and properties of methylmalonyl racemase. *J. Biol. Chem.* 238:1637-42.

With S. H. Allen and R. L. Stjernholm. The noninvolvement of the ureido carbon of biotin in transcarboxylation. *J. Biol. Chem.* 238:PC2889-91.

With S. H. Allen, R. L. Stjernholm, and B. Jacobson. Transcarboxylase purification and properties of methyl-malonyl-oxaloacetic transcarboxylase containing tritiated biotin. *J. Biol. Chem.* 238:547-56.

With J. Katz. The use of $C^{14}O_2$ yields from glucose-1- and 6-C^{14} for the evaluation of the pathways of glucose metabolism. *J. Biol. Chem.* 238:517-23.

With J. Katz and B. R. Landau. Estimation of pathways of carbohydrate metabolism. *Biochem. Z.* 338:809-47.

With H. Lochmuller, C. Riepertinger, and F. Lynen. Transcarboxylase. IV. Function of biotin and the structure and properties of the carboxylated enzyme. *Biochem. Z.* 337:247-66.

With R. L. Stjernholm. The symmetrical C3 in the propionic acid fermentation and the effect of avidin on propionate formation. *Iowa State Coll. J. Sci.* 38:123-40

1964 With S. H. Allen, R. W. Kellermeyer, and R. L. Stjernholm. Purifica tion and properties of enzymes involved in the propionic acid formation. *J. Bacteriol.* 87:171-87.

With J. Katz and B. Landau. Evaluation of metabolic pathways of glucose. *Abstracts, Sixth International Congress of Biochemistry*, vol. 4, pp. 495-96.

With R. W. Kellermeyer, S. H. Allen, and R. L. Stjernholm. Methylmalonyl isomerase. IV. Purification and properties of the enzyme from *Propionibacteria. J. Biol. Chem.* 239:2562-69.

With R. W. Kellermeyer, R. L. Stjernholm, and S. H. Allen. Metabolism of methylmalonyl-CoA and the role of biotin and B12 coenzymes. *Ann. N.Y. Acad. Sci.* 112:661-79.

With B. Landau, G. Bartsch, and J. Katz. Estimation of pathway contributions to glucose metabolism and the rate of isomerization of hexose-6-phosphate. *J. Biol. Chem.* 239:686-96.

1965 Incorporation of C^{14} from carbon dioxide into sugar phosphates, carboxylic acids and amino acids by *Clostridium thermoaceticum. J. Bacteriol.* 89:1055-64.

With L. G. Ljungdahl and E. Irion. Total synthesis of acetate from CO_2. I. Co-methylcobyric acid and Co-(methyl)-5-methoxy-benzimidazolyl-cobamide as intermediates with Clostridium thermoaceticum. *Biochemistry* 4:2771-80.

With G. J. Peeters, R. Verbeke, M. Lauryssens, and B. Jacobson. Estimation of the pentose cycle in the perfused cow's udder. *Biochem. J.* 96:607-15.

With M. F. Utter. The role of CO_{12} fixation in metabolism. *Essays Biochem.* 1:1-27.

1966 With J. J. Davis and H. Lochmuller. The equilibria reactions catalyzed by carboxytransphosphorylase, carboxykinase and pyruvate carboxylase and the synthesis of p-enolpyruvate. *J. Biol. Chem.* 241:5692-5704.

With L. Li and L. G. Ljungdahl. Properties of nicotinamide adenine dinucleotide phosphate-dependent formate dehydrogenase from *C. thermoaceticum. J. Bacteriol.* 92:405-12.

With H. Lochmuller and J. J. Davis. Phosphoenolpyruvate carboxy transphosphorylase. II. Crystallization and properties. *J. Biol. Chem.* 241:5578-91.

1967 With L. G. Ljungdahl. The role of corrinoids in the total synthesis of acetate from CO_2. In *Seventh International Congress of Biochemistry*, Tokyo, Colloqium XII, pp. 549-50.

1968 Mechanism of formation of oxalacetate and phosphoenolpyruvate from pyruvate. *J. Vitamins* 14:59-67.

With S. H. Allen and R. W. Kellermeyer. Methylmalonyl-CoA racemase from *propionibacterium shermanii. Methods Enzymol.* 13:194-98.

With T. G. Cooper, T. Tchen, and C. Benedict. The carboxylation of phosphoenolpyruvate and pyruvate. I. The active species of "CO_2" utilized by phosphoenolpyruvate carboxykinase, carboxy-transphosphorylase and pyruvate carboxylase. *J. Biol. Chem.* 243:3857-63.

With J. J. Davis and J. M. Willard. Phosphoenolpyruvate carboxy-phosphorylase from *Propionibacterium shermanii. Methods Enzymol.* 13:297-309.

With H. Evans. The mechanism of the pyruvate phosphate dikinase reaction. *Proc. Natl. Acad. Sci. U.S.A.* 61:1448-53.

With B. Jacobson, B. Gerwin, and D. Northrop. Oxalacetate transcarboxylase from *Propionibacterium . Methods Enzymol.* 13:215-31.

With R. W. Kellermeyer. 2-methylmalonyl-CoA mutase from *Propionibacterium shermanii. Methods Enzymol.* 13:207-15.

1969 With T. G. Cooper, T. Tchen, C. Benedict, and D. Filmer. The species of "CO_2" utilized in the carboxylation of P-enolpyruvate and pyruvate. In *Chemistry, Biochemistry, and Physiological Aspects*, pp. 183-92. Washington: National Aeronautics and Space Administration.

With J. J. Davis and J. M. Willard. Phosphoenolpyruvate carboxy-transphosphorylase. III. Comparison of the fixation of CO_2 and the conversion of phosphoenolpyruvate and phosphate to pyruvate and pyrophosphate. *Biochemistry* 8:3127-36.

With J. J. Davis and J. M. Willard. Phosphoenolpyruvate carboxytransphosphorylase. V. Mechanism of the reaction and role of metal ions. *Biochemistry* 8:3145-55.

With B. Gerwin and B. Jacobson. Transcarboxylase. VIII. Isolation and properties of a biotin-carboxyl carrier protein. *Proc. Natl. Acad. Sci. U.S.A.* 64:1315-22.

With L. G. Ljungdahl. Total synthesis of acetate from CO_2 by heterotrophic bacteria. *Ann. Rev. Microbiol.* 23:515-38.

With D. Northrop. Transcarboxylase. V. The presence of bound zinc and cobalt. *J. Biol. Chem.* 244:5801-7.

With D. Northrop. Transcarboxylase. VII. Exchange reactions and kinetics of oxalate inhibition. *J. Biol. Chem.* 244:5820-27.

With I. A. Rose, E. L. O'Connell, P. Noce, M. F. Utter, J. M. Willard, T. G. Cooper, and M. Benziman. Stereochemistry of the enzymatic carboxylation of phosphoenolpyruvate. *J. Biol. Chem.* 244:6130-33.

With A. Y. Sun and L. G. Ljungdahl. Total synthesis of acetate from CO_2. II. Purification and properties of formyltetrahydrofolate synthetase from Clostridium thermoaceticum. *J. Bacteriol.* 98:842-44.

With J. M. Willard and J. J. Davis. Phosphoenolpyruvate carboxytransphosphorylase. IV. Requirement of metal cations. *Biochemistry* 8:3137-44.

1970 With F. Ahmad and B. Jacobson. Transcarboxylase. X. Assembly of active transcarboxylase from its inactive subunits and incorporation of the biotin-carboxyl carrier protein . *J. Biol. Chem.* 245:6486-88.

With B. Jacobson, B. Gerwin, F. Ahmad, and P. Waegell. Transcarboxylase. IX. Parameters effecting dissociation and reassociation of the enzyme. *J. Biol. Chem.* 245:6471-83.

1971 Biochemistry. *Fed. Proc.* 30:1715-18.

With T. G. Cooper. The carboxylation of phosphoenolpyruvate and pyruvate. II. The active species of "CO_2" utilized by phosphoenolpyruvate carboxylase and pyruvate carboxylase. *J. Biol. Chem.* 246:5488-90.

With H. Evans. Purification and properties of pyruvate phosphate dikinase from propionic acid bacteria. *Biochemistry* 10:721.

With R. Ghambeer, M. Schulman, and L. G. Ljungdahl. Total synthesis of acetate from CO_2. III. Inhibition by alkylhalides of the synthesis from CO_2, methyltetrahydrofolate and methyl-B12 by Clostridium thermoaceticum. *Arch. Biochem. Biophys.* 143:471-84.

With R. A. Harte. International structures in science. *Fed. Proc.* 30:1713-14.

With D. Parker and T. Wu. Total synthesis of acetate from CO_2: methyltetrahydrofolate, an intermediate and a procedure for separation of the folates. *J. Bacteriol.* 108:770-76.

With M. Schulman. Determination and degradation of microquantities of acetate. *Anal. Biochem.* 39:505-20.

1972 Some comments about teaching biochemistry. *Biochem. Ed.* 1:2-3.

Transcarboxylase. In *The Enzymes*, 3rd ed., ed. P. boyer. pp. 83-113. New York: Academic Press.

My life and carbon dioxide fixation. In *The Molecular Basis of Biological Transport*, Miami Winter Symposium, vol. 3, pp. 1-54.

With F. Ahmad, D. H. Lygre, and B. Jacobson. Transcarboxylase. XII. Identification of the metal-containing subunits of transcarboxylase and stability of the binding. *J. Biol. Chem.* 247:6299-6305.

With N. M. Green, R. C. Valentine, N. H. Wrigley, F. Ahmad, B. Jacobson. Transcarboxylase. XI. Electron microscopy and subunit structure. *J. Biol. Chem.* 247:6284-98.

With M. E. Haberland and J. M. Willard. Phosphoenolpyruvate carboxytransphosphorylase: Study of the catalytic and physical structures . *Biochemistry* 11:712-22.

With Y. Milner. Isolation of pyrophosphoryl form of pyruvate, phosphate dikinase from *Propionibacteria. Proc. Natl. Acad. Sci. U.S.A.* 69:2463-68.

With D. J. Parker, R. K. Ghambeer, and L. G. Ljungdahl. Total synthesis of acetate from carbon dioxide. Retention of deuterium during carboxylation of trideuteriomethyltetrahydrofolate or trideuteriomethylcobalamin. *Biochemistry* 11:3074-80.

With M. Schulman, D. J. Parker, and L. G. Ljungdahl. Total synthesis of acetate from CO_2. V. Determination by mass analysis of the different types of acetate formed from $^{14}CO_2$ by heterotrophic bacteria. *J. Bacteriol.* 109:633-44.

1973 The Activities of the International Union of Biochemistry. Information Bulletin for 9th International Congress of Biochemistry, Stockholm, pp. 13-17.

With F. Ahmad, B. Jacobson, N. M. Green, and N. Wrigley. Transcarboxylase: A biotinyl-metallo-enzyme with a unique structure. In *Proceedings of 8th Meeting of Federation of European Biochemistry Society, Enzymes: Structure and Function*, vol. 29, pp. 201-16.

With R. K. Ghambeer and L. G. Ljungdahl. Total synthesis of acetate from CO_2. VII. Evidence with Clostridium thermoaceticum that the carboxyl of acetate is derived from the carboxyl of pyruvate by transcarboxylation and not by fixation of CO_2. *J. Biol. Chem.* 248:6255-61.

With W. E. O'Brien and R. Singleton, Jr. Phosphoenolpyruvate carboxytransphosphorylase. An investigation of the mechanism with ^{18}O. *Biochemistry* 12:5247-52.

1974 With W. E. O'Brien. Carboxytransphosphorylase. VIII. Ligandmediated interaction of subunits as a possible control mechanism in *Propionibacteria. J. Biol. Chem.* 249:4917-25.

1975 Appendix VIII to the discovery of carbon dioxide fixation in mammalian tissues (by Krebs). *Mol. Cell. Biochem.* 5:91-94.

With F. Ahmad, B. Jacobson, M. Chuang, and W. Brattin. Isolation of the subunits of transcarboxylase and reconstitution of the active enzyme from the subunits. *J. Biol. Chem.* 250:918-26.

With F. Ahmad, B. Jacobson, M. Chuang, and W. Brattin. Isolation of peptides from the carboxyl carrier subunit of transcarboxylase. Role of the non-biotinyl peptide in assembly. *Biochemistry* 14:1606-11.

With M. Berger. Purification of the subunits of transcarboxylase by affinity chromatography on avidin-sepharose. *J. Biol. Chem.* 250:927-33.

With M. Chuang, F. Ahmad, and B. Jacobson. Evidence that the two partial reactions of transcarboxylation are catalyzed by two dissimilar subunits of transcarboxylase. *Biochemistry* 14:1611-19.

With Y. Milner and G. Michaels. Pyruvate, orthophosphate dikinase of *Bacteroides* OSUS and *Propionibacterium shermanii. Methods Enzymol.* 42:199-212.

With W. E. O'Brien and S. Bowien. Isolation and characterization of a pyrophosphate-dependent phosphofructokinase from *Propionibacterium shermanii. J. Biol. Chem.* 250:8690-95.

With M. Schulman. Enzymatic determination of microquantities of acetate. *Methods Enzymol.* 35:298-301.

With M. Schulman. Succinyl-CoA: propionate CoA transferase from *Propionibacterium shermanii. Methods Enzymol.* 35:235-42.

1976 The reactive group of biotin in catalysis by biotin enzymes. *Trends Biochem. Sci.* 1:4-6.

Subunit-subunit interactions of transcarboxylase. *Fed. Proc.* 35:1899-1907.

Reflections on Lynen's laboratory in *Die Aktivierte Essigsaure und ihre Folgen. Autobiographische Beitrage von Schulem und Freunded Feodor Lynens*, ed. G. Hartmann. Berlin: Walter de Gruyter.

International responsibilities. *Trends Biochem. Sci.* 1:49-50.

Trailing the propionic acid bacteria. In *Reflections on Biochemistry: A Symposia in Honor of Severo Ochoa*, ed. A. Kornberg, B. L. Horecker, L. Cornudella, and J. Oro. New York: Pergamon Press.

With M. Berger. Immunochemistry of the subunits of transcarboxylase. *J. Biol. Chem.* 251:7021-33.

With Y. Milner. Steady state exchange kinetics. *J. Biol. Chem.* 251:7920-28.

With A. M. Spronk and H. Yoshida. Isolation of 3-phosphohistidine from phosphoryl pyruvate, phosphate dikinase. *Proc. Natl. Acad. Sci. U.S.A.* 73:4415-19.

With G. K. Zwolinski. Transcarboxylase: role of biotin, metals, and subunits in the reaction and its quaternary structure. *Crit. Rev. Biochem.* 4:47-122.

1977 Some reactions in which inorganic pyrophosphate replaces ATP and serves as a source of energy. *Fed. Proc.* 36:2197-2206.

With R. E. Barden. Biotin enzymes. *Ann. Rev. Biochem.* 46:385-413.

With J. Chiao and E. M. Poto. A new large form of transcarboxylase with six peripheral subunits and twelve biotinyl carboxyl carrier subunits. *J. Biol. Chem.* 252:1490-99.

With E. M. Poto. The association-dissociation of transcarboxylase. *Biochemistry* 16:1949-55.

With W. E. O'Brien and G. Michaels. Properties of carboxy-transphosphorylase; pyruvate, phosphate dikinase; PPi-phosphofructokinase and PPi-acetate kinase and their roles in the metabolism of inorganic pyrophosphate. *Adv. Enzymol.* 45:85-155.

With N. H. Wrigley and J. Chiao. Electron microscopy of the large form of transcarboxylase with six peripheral subunits. *J. Biol. Chem.* 252:1500-04.

With G. K. Zwolinski, B. Bowien, and F. Harmon. The structure of the subunits of transcarboxylase and their relationship to the quaternary structure of transcarboxylase. *Biochemistry* 16:4627-37.

1978 With G. A. Cook, W. E. O'Brien, M. T. King, and R. Veech. A rapid, enzymatic assay for the measurement of inorganic pyrophosphate in animal tissue. *Anal. Biochem.* 91:557-65.

With G. Michaels, Y. Milner, and B. R. Moskovitz. Pyruvate phosphate dikinase. Metal requirements and inactivation of the enzyme by sulfhydryl agents. *J. Biol. Chem.* 253:7656-61.

With Y. Milner and G. Michaels. Pyruvate, phosphate dikinase of *Bacteroides symbiosus*. Catalysis of partial reactions and formation of phosphoryl and pyrophosphoryl forms of the enzyme. *J. Biol. Chem.* 253:878-83.

With B. R. Moskovitz. Requirement of monovalent cations for enolization of pyruvate by pyruvate, phosphate dikinase. *J. Biol. Chem.* 253:884-88.

With E. M. Poto, R. E. Barden, and E. P. Lau. Photoaffinity labeling and stoichiometry of the coenzyme A ester sites of transcarboxylase. *J. Biol. Chem.* 253:2979-83.

With F. K. Welty. Purification of the "corrinoid" enzyme involved in the synthesis of acetate by *Clostridium thermoaceticum. J. Biol. Chem*. 253:5832-38.

With H. Yoshida. Crystalline pyruvate, phosphate dikinase from *Bacteroides*

symbiosus. Modification of essential histidyl residues and bromopyruvate inactivation. *J. Biol. Chem.* 253:7650-55.
1979 Obituary—Feodor (Fitzi) Lynen. *Trends Biochem. Sci.* 4:300-2.
The anatomy of transcarboxylase and the role of its subunits. *Crit. Rev. Biochem.* 7:143-60.
The role of corrinoids in the total synthesis of acetate from CO_2. In Vitamin B12, ed. B. Zagalak and W. Friedrich. Berlin: Walter de Gruyter.
With H. L. Drake and N. H. Goss. A new, convenient method for the rapid analysis of inorganic pyrophosphate. *Anal. Biochem.* 94:117-20.
With W. L. Maloy, B. U. Bowien, G. K. Zwolinski, K. G. Kumar, L. H. Ericsson, and K. A. Walsh. Amino acid sequence of the biotinyl subunit from transcarboxylase. *J. Biol. Chem.* 254:11615-22.
With L. J. Waber. Mechanism of acetate synthesis from CO_2 by *Clostridium acidiurici*. *J. Bacteriol.* 140:468-78.
1980 IUB and the person. *Trends Biochem. Sci.* 4:I-II.
With H. L. Drake and S. Hu. Purification of carbon monoxide dehydrogenase, a nickel enzyme from *Clostridium thermoaceticum. J. Biol. Chem.* 255:7174-80.
With C. T. Evans and N. H. Goss. Pyruvate, phosphate dikinase: affinity labeling of the adenosine 5'-triphosphate-adenosine 5'-monophosphate site. *Biochemistry* 19:5809-14.
With N. H. Goss and C. T. Evans. Pyruvate, phosphate dikinase: sequence of the histidyl peptide, the pyrophosphoryl and phosphoryl carrier. *Biochemistry* 19:5805-9.
With F. R. Harmon, M. Berger, H. Beegen, and N. Wrigley. Transcarboxylase: an electron microscopic study of the enzyme with antibodies to biotin. *J. Biol. Chem.* 255:9458-64.
With F. R. Harmon, B. Wuhr, K. Hubner, and F. Lynen. Comparison of the biotination of apotranscarboxylase and its aposubunits. Is assembly essential for biotination? *J. Biol. Chem.* 255:7397-7409.
1981 Obituary, Merton F. Utter. *Trends Biochem. Sci.* 6:V-VI.

Metabolic cycles in the fermentation of propionic acid bacteria. In *Current Topics in Cellular Regulation*, vol. 18, ed. R. Estabrook and P. Srere. New York: Academic Press.

With C. Bahler, N. Goss, and E. Poto. Transcarboxylase: dissociation and reassociation of the central hexameric subunit. *Biochem. Intl.* 3:349-58.

With H. L. Drake and S. Hu. Purification of five components from *Clostridium thermoaceticum* which catalyzes synthesis of acetate from pyruvate and methyltetrahydrofolate. Properties of phospho-transacetylase. *J. Biol. Chem.* 256:11137-44.

1982 From CO_2 to acetate. In *From Cyclotrons to Cytochromes: Essays in Molecular Biology and Chemistry*, ed. N. O. Kaplan and A. Robinson. New York: Academic Press.

The discovery of the fixation of CO_2 by heterotrophic organisms and metabolism of the propionic acid bacteria. In *Of Oxygen, Fuels, and Living Matter; Part 2*, ed. G. Semenza. New York: John Wiley & Sons.

With H. L. Drake and S. Hu. Studies with *Clostridium thermoaceticum* and the resolution of the pathway used by acetogenic bacteria that grow on carbon monoxide or carbon dioxide and hydrogen. In *Proceedings Biochemistry Symposium*, ed. E. S. Snell. Annual Reviews.

With N. H. Goss. Covalent chemistry of pyruvate, orthophosphate dikinase. *Methods Enzymol.* 87:51-66.

With F. R. Harmon and N. H. Goss. Stabilization of the quaternary structure of transcarboxylase by cobalt (II) ions. *Biochemistry* 21:2847-52.

With J. P. Hennessey, W. C. Johnson, and C. Bahler. Subunit interactions of transcarboxylase as studied by circular dichroism. *Biochemistry* 21:642-46.

With S. Hu and H. L. Drake. Synthesis of acetyl coenzyme A from carbon monoxide, methyltetrahydrofolate, and coenzyme A by enzymes from *Clostridium thermoaceticum. J. Bacteriol.* 149:440-48.

With G. K. Kumar. Intrinsic fluorescence of transcarboxylase during subunit-subunit interactions. *Biochem. Intl.* 4:605-16.

With G. K. Kumar, C. R. Bahler, and R. B. Merrifield. The amino acid sequences of the biotinyl subunit essential for the association of transcarboxylase. *J. Biol. Chem.* 257:13828-34.

With L. G. Ljungdahl. *Acetate Biosynthesis Vitamin B*12, vol. 2, ed. D. Dolphin. New York: John Wiley & Sons.

With W. J. Whelan. Freedom to meet. *Trends Biochem. Sci.* 7:351.

1983 With B. R. Landau. The pentose cycle in animal tissues: evidence for the classical and against the 'L-type' pathway. *Trends Biochem. Sci.* 8:292-96.

With N. F. Phillips and N. H. Goss. Modification of pyruvate, phosphate dikinase with pyridoxal 5'-phosphate: evidence for a catalytically critical lysine residue. *Biochemistry* 22:2518-23.

1984 With N. H. Goss. Formation of N-(biotinyl) lysine in biotin enzymes. *Methods Enzymol.* 107:261-78.

With S. Hu and E. Pezacka. Acetate synthesis from carbon monoxide by *Clostridium thermoaceticum*. Purification of the corrinoid protein. *J. Biol. Chem.* 259:8892-97.

With E. Pezacka. Role of carbon monoxide dehydrogenase in the autotrophic pathway used by acetogenic bacteria. *Proc. Natl. Acad. Sci. U.S.A.* 81:6261-65.

With E. Pezacka. The synthesis of acetyl-CoA by *Clostridium thermoaceticum* from carbon dioxide, hydrogen, coenzyme A and methyltetrahydrofolate. *Arch. Microbiol.* 137:63-69.

With N. A. Robinson and N. H. Goss. Polyphosphate kinase from *Propionibacterium shermanii*: formation of an enzymatically active insoluble complex with basic proteins and characterization of synthesized polyphosphate. *Biochem. Intl.* 8:757-69.

1985 The role of the International Union of Biochemistry (IUB). *BioEssays* 3:42-44.

Then and now. *Ann. Rev. Biochem.* 54:1-41.

Inorganic pyrophosphate and polyphosphates as sources of energy. *Curr. Top. Cell. Regul.* 26:355-69.

With D. V. Craft, N. H. Goss, and N. Chandramouli. Purification of biotinidase from human plasma and its activity on biotinyl peptides. *Biochemistry* 24:2471-76.

With N. H. Goss. Phosphorylation enzyme of the propionic acid bacteria and the roles of ATP, inorganic pyrophosphate, and polyphosphates. *Proc. Natl. Acad. Sci. U.S.A.* 82:312-15.

With G. K. Kumar. Transcarboxylase: its quaternary structure and the role of the biotinyl subunit in the assembly of the enzyme and in catalysis. *Ann. N.Y. Acad. Sci.* 447:1-22.

With G. K. Kumar and H. Beegen. Assembly of subunits and structure of transcarboxylase: sequence requirement and electron microscopy of crystals. *Ann. N.Y. Acad. Sci.* 447:403-5.

With S. W. Ragsdale. Acetate biosynthesis by acetogenic bacteria. Evidence that carbon monoxide dehydrogenase is the condensing enzyme that catalyzes the final steps of the synthesis. *J. Biol. Chem.* 260:3970-77.

With S. W. Ragsdale and W. E. Antholine. Evidence that an ironnickel-carbon complex is formed by reaction of CO with the CO dehydrogenase from *Clostridium thermoaceticum. Proc. Natl. Acad. Sci. U.S.A.* 82:6811-14.

1986 The synthesis of lactose and related investigations. *Vlaams Diergeneeskd. Tijdschr.* 55:274-85.

With J. E. Clark and H. Beegen. Isolation of intact chains of polyphosphate from *Propionibacterium shermanii* grown on glucose or lactate. *J. Bacteriol.* 168:1212-19.

With C. Pepin. Polyphosphate glucokinase from *Propionibacterium shermanii*. Kinetics and demonstration that the mechanism involves both processive and nonprocessive type reactions. *J. Biol. Chem.* 261:4476-80.

With C. A. Pepin and N. A. Robinson. Determination of the size of polyphosphates with polyphosphate glucokinase. *Biochem. Intl.* 12:111-23.

With E. Pezacka. The autotrophic pathway of acetogenic bacteria. Role of CO dehydrogenase disulfide reductase. *J. Biol. Chem.* 261:1609-15.

With N. F. B. Phillips. Isolation of pyrophosphohistidine from pyrophosphorylated pyruvate, phosphate dikinase. *Biochemistry* 25:1644-49.

With S. W. Ragsdale and E. Pezacka. The acetyl-CoA pathway: a newly discovered pathway of autotrophic growth. *Trends Biochem. Sci.* 11:14-18.

With S. W. Ragsdale and E. Pezacka. A new pathway of autotrophic growth utilizing carbon monoxide or carbon dioxide and hydrogen. *Biochem. Intl.* 12:421-40.

With S. W. Ragsdale and E. Pezacka. The acetyl-CoA pathway of autotrophic growth. *FEMS Microbiol. Rev.* 39:345-62.

With N. A. Robinson. Polyphosphate kinase from *Propionibacterium shermanii*. Demonstration that the synthesis and utilization of polyphosphate is by a processive mechanism. *J. Biol. Chem.* 261:4481-85.

With E. Skrzpczak-Jankum, A. Tulinsky, J. P. Fillers, and K. G. Kumar. Preliminary crystallographic data and quaternary structural implications of the central subunit of the multi-subunit complex transcarboxylase. *J. Mol. Biol.* 188:495-98.

1987 With J. E. Clark. Preparation of standards and determination of sizes of long-chain polyphosphates by gel electrophoresis. *Anal. Biochem.* 161:280-90.

With C. A. Pepin. The mechanism of utilization of polyphosphate by polyphosphate glucokinase from *Propionibacterium shermanni. J. Biol. Chem.* 262:5223-26.

With N. A. Robinson and J. E. Clark. Polyphosphate kinase from *Propionibacterium shermanii*. Demonstration that polyphosphates are primers and determination of the size of the synthesized polyphosphate. *J. Biol. Chem.* 262:5216-22.

With N. A. Robinson, C. Pepin, and J. E. Clark. Polyphosphate kinase and polyphosphate glucokinase of *Propionibacterium shermanii*. In *Phosphate Metabolism and Cellular Regulation in Microorganisms*, ed. A. Torriani-Gorini and F. C. Rothman. Washington, D.C.: American Society for Microbiology.

1988 Squiggle phosphate of inorganic pyrophosphate and polyphosphates. In *The Roots of Modern Biochemistry*, ed. H. Kleinkauf, H. von Döhren, and L. Jaenicke. Berlin: Walter de Gruyter.

With J. E. Clark. Biological aspects of inorganic polyphosphates. *Ann. Rev. Biochem.* 57:235-60.
With G. K. Kumar and H. Beegen. Involvement of tryptophans at the catalytic and subunit-binding domains of transcarboxylase. *Biochemistry* 27:5972-78.
With G. K. Kumar, F. C. Haase, and N. F. Phillips. Involvement and identification of a tryptophanyl residue at the pyruvate binding site of transcarboxylase. *Biochemistry* 27:5978-83.
With E. Pezacka. Acetyl-CoA pathway of autotrophic growth. Identification of the methyl-binding site of the CO dehydrogenase. *J. Biol. Chem.* 263:16000-06.
With S. W. Ragsdale, T. A. Morton, L. G. Ljungdahl, and D. V. DerVartanian. Nickel in CO dehydrogenase. In *The Bioinorganic Chemistry of Nickel* , ed. J. R. Lancaster, Jr. New York: VCH Publishers.
With D. Samols, C. G. Thornton, V. L. Murtif, G. K. Kumar, and F. C. Haase. Evolutionary conservation among biotin enzymes. *J. Biol. Chem.* 263:6461-64.
With T. Shanmugasundaram and G. K. Kumar. Involvement of tryptophan residues at the coenzyme A binding site of carbon monoxide dehydrogenase from *Clostridium thermoaceticum. Biochemistry* 27:6499-6503.
With T. Shanmugasundaram and S. W. Ragsdale. Role of carbon monoxide dehydrogenase in acetate synthesis by the acetogenic bacterium, *Acetobacterium woodii. BioFactors* 1:147-52.
With B. Shenoy. Purification and properties of the synthetase catalyzing the biotination of the aposubunit of transcarboxylase from *Propionibacterium shermanii. FASEB J.* 2:2396-2401.
1989 Past and present of CO_2 utilization. In *Autotrophic Bacteria*, ed. H. G. Schlegel and B. Bowien. New York: Springer-Verlag.
With T. Shanmugasundaram, G. K. Kumar, and B. C. Shenoy. Chemical modification of the functional arginine residues of carbon monoxide dehydrogenase from *Clostridium thermoaceticum. Biochemistry* 28:7112-16.

1991 Life with CO or CO_2 and H_2 as a source of carbon and energy. *FASEB J.* 5:156-63.

With L. G. Ljungdahl. Autotrophic character of the acetogenic bacteria. In *Variations in Autotrophic Life*, ed. J. M. Shively and L. L. Barton. New York: Academic Press.

With T. A. Morton, J. A. Runquist, S. W. Ragsdale, T. Shanmugasundaram, and L. G. Ljungdahl. The primary structure of the subunits of carbon monoxide dehydrogenase/acetyl-CoA synthase from *Clostridium thermoaceticum. J. Biol. Chem.* 266:23824-28.

With T. Shanmugasundaram. Interaction of ferredoxin with carbon monoxide dehydrogenase from *Clostridium thermoaceticum. J. Biol. Chem.* 267:897-900.

1992 With B. C. Shenoy, Y. Xie, V. L. Park, G. K. Kumar, H. Beegen, and D. Samols. The importance of methionine residues for the catalysis of the biotin enzyme, transcarboxylase. Analysis by site-directed mutagenesis. *J. Biol. Chem.* 267:18407-12.

1993 With S. B. Woo, B. C. Shenoy, W. J. Magner, G. K. Kumar, H. Beegen and D. Samols. Effect of deletion from the carboxyl terminus of the 12S subunit of activity of transcarboxylase. *J. Biol. Chem.* 268:16413-19.

With C. G. Thornton, G. K. Kumar, F. C. Haase, N. F. B. Phillips, S. B. Woo, V. M. Park, W. J. Magner, S. B. Shenoy and D. Samols. Primary structure of the monomer of the 12S subunit of transcarboxylase as deduced from DNA and characterization of the product expressed in *Escherichia coli. J. Bacteriol.* 175:5301-8.

With C. G. Thornton, G. K. Kumar, B. C. Shenoy, F. C. Haase, N. F. B. Phillips, V. M. Park, W. J. Magner, D. P. Hejlik and D. Samols. Primary structure of the 5S subunit of transcarboxylase as deduced from the genomic DNA sequence. *FEBS Lett.* 330:191-96.

With N. F. B. Phillips and P. J. Horn. The polyphosphate and ATP-dependent glucokinase from *Propionibacterium shermanii:* both activities are catalyzed by the same protein. *Arch. Biochem. Biophys.* 300:309-19.