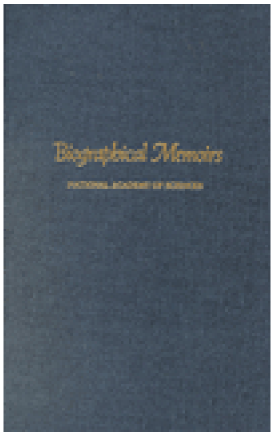
http://www.nap.edu/catalog/9977.html
We ship printed books within 1 business day; personal PDFs are available immediately.



Biographical Memoirs V.78

Office of the Home Secretary, National Academy of Sciences

ISBN: 0-309-56985-0, 382 pages, 6 x 9, (2000)

This PDF is available from the National Academies Press at: http://www.nap.edu/catalog/9977.html

Visit the National Academies Press online, the authoritative source for all books from the National Academy of Sciences, the National Academy of Engineering, the Institute of Medicine, and the National Research Council:

- Download hundreds of free books in PDF
- Read thousands of books online for free
- Explore our innovative research tools – try the “Research Dashboard” now!
- Sign up to be notified when new books are published
- Purchase printed books and selected PDF files

Thank you for downloading this PDF. If you have comments, questions or just want more information about the books published by the National Academies Press, you may contact our customer service department toll-free at 888-624-8373, visit us online, or send an email to feedback@nap.edu.

This book plus thousands more are available at http://www.nap.edu.

Copyright © National Academy of Sciences. All rights reserved.
Unless otherwise indicated, all materials in this PDF File are copyrighted by the National Academy of Sciences. Distribution, posting, or copying is strictly prohibited without written permission of the National Academies Press. Request reprint permission for this book.

THE NATIONAL ACADEMIES
Advisers to the Nation on Science, Engineering, and Medicine

Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

VOLUME 78

NATIONAL ACADEMY PRESS

WASHINGTON, D.C.2000

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

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.

INTERNATIONAL STANDARD BOOK NUMBER 0-309-7035-X
INTERNATIONAL STANDARD SERIAL NUMBER 0077-2933
LIBRARY OF CONGRESS CATALOG CARD NUMBER 5-26629
Available from
NATIONAL ACADEMY PRESS 2101 CONSTITUTION AVENUE, N.W. WASHINGTON, D.C.20418

PRINTED IN THE UNITED STATES OF AMERICA

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

CONTENTS

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

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.

R. STEPHEN BERRY
Home Secretary

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs

VOLUME 78

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ROBERT COOLEY ELDERFIELD

May 30, 1904–December 10, 1979

BY NELSON J. LEONARD

ROBERT C. ELDERFIELD WAS an organic chemist who achieved considerable success in applying his research to areas of medicine. While at the Rockefeller Institute for Medical Research, he established the fundamental relationship between the cardiac aglycones and the sterols and bile acids. He determined the stereochemical structures of the rare sugar components of the related glycosides. Together with his students at Columbia University and later at the University of Michigan, he worked on alkaloids, provided the best synthetic routes to the effective antimalarial pamaquine, examined a new category of anionic substitution reactions, and contributed to our array of anticancer agents. His breadth of research contributions was complemented by his enthusiastic and innovative teaching and by his writing. He was editor and author of the nine-volume definitive treatise *Heterocyclic Compounds.*

Robert Elderfield was born May 30, 1904, in Niagara Falls, New York, the son of Charles James Elderfield and Nellie Cooley. Charles's parents were Ellen Croal of Aberdeen, Scotland, and Charles Elderfield of the Isle of Wight, England. Robert's grandfather was a skilled cabinetmaker who, with Ellen, emigrated to Hamilton, Ontario, and from there

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

moved to Niagara Falls, New York. Robert's father dropped out of school at the age of fourteen to help support the family by employment at Oliver Brothers in Niagara Falls. There he met and married Nellie Cooley in 1901. Born in Canandaigua, New York, she attended Upham School for Girls to learn secretarial skills and found a job with the same employer. Charles J. Elderfield went on to found and become president of a prosperous mill supply business. He was, incidentally, an enthusiastic fisherman, and he transmitted this passion to his son, Robert, who from the age of four spent part of every summer fishing in Canada, except during the war years. There will be more said about the son's fishing prowess later.

Robert Elderfield's early years were spent in attendance at the public schools of Niagara Falls. Inasmuch as he was rather young when he finished high school in 1920, he was advised to attend a preparatory school prior to entering college. The Choate School in Wallingford, Connecticut, was selected. His original plan was to go directly thereafter to the Massachusetts Institute of Technology for undergraduate work, but upon the advice of his cousin Mortimer E. Cooley, Dean of the Engineering School of the University of Michigan, Robert decided to attend a small liberal arts college, Williams College, before going to MIT for graduate work. To satisfy the entrance requirements for Williams, particularly insofar as Latin was concerned, he returned to Choate for a second year, completing that requirement and accumulating considerable advanced credit. While he was at Choate, his parents adopted a girl of eight years, Esther, whose mother had died in the flu epidemic of 1918. Robert realized that his new sister was confused and unhappy, and for her first Christmas in the Elderfield family he gave her a black Labrador puppy. This was followed by gifts of a sled, ice skates, and skis as further evidence of his caring and

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

understanding, all of which is still remembered warmly by Esther (Sherwood Elderfield Green).

At Williams, Robert majored in chemistry and qualified for a second major in German, which gave him a great deal of satisfaction. During his senior year at Williams, he was a teaching assistant in the freshman chemistry laboratory. He received the A.B. degree in 1926. There was never a doubt that he would enter the chemical profession. During his formative years, he came in contact with many technically trained individuals who were connected with the chemical industry that was then concentrated in the Niagara Falls region. During the summers between his college years at Williams, he worked in chemical plants in Niagara Falls. The experience thus gained strengthened his desire to pursue a career in chemistry.

The first research paper that bore the name of Robert C. Elderfield appeared with that of MIT Professor Tenney L. Davis in 1928. The Ph.D. degree was awarded by MIT in 1930, and in 1932 and 1933 Elderfield and Davis published two more papers that were based on his thesis research. An important development of his MIT years resulted from his appointment as a laboratory assistant. One of the students in the undergraduate course to which he was assigned was Mary Elizabeth (Polly) Betts, who had been born in Philadelphia and brought up in Washington, D.C. Robert and Polly were married in the summer of 1930. She died only recently in February of 1999. Their two daughters survive: Nancy Elderfield Hall Shanahan and Margaret Helen Elderfield Ritchie.

Appointed assistant in 1930 in the Rockefeller Institute of Medical Research, Elderfield worked with Walter A. Jacobs in the Laboratory of Chemical Pharmacology. The research was concentrated on the cardiac glycosides, a category of natural products noted and used for their specific action

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

on the myocardium. Little was known initially about these compounds, which are present in plants in extremely small amounts. One of the major goals of the research was to determine any structural interrelations that might exist, and one of the strategies was to concentrate on those sources (e.g., *Strophanthus kombé*) wherein the glycosides were most abundant. It should be remembered that in the 1930s the methods of comparison of compounds were limited mainly to ultraviolet spectroscopy and to identity in physical properties. The structural achievements of the era were remarkable in the light of these restrictions, together with the lack of efficient methods for separation of the mixtures of compounds that nature provided and the small number of degradation procedures available. The twentieth article in the Rockefeller series on strophanthin was the first to include Elderfield's name as an author, and the thirty-fifth article in the series was the last. By means of conversions and degradations, strophanthidin was correlated with periplogenin and these two were correlated to digitoxigenin and gitoxigenin. The Rockefeller studies merged with research that had been going on in Germany to show that the cardiac aglycones were closely related to cholesterol and the bile acids, an important conclusion that had not been foreseen. Elderfield related in his *Biographical Memoir* of Walter A. Jacobs (vol. 51, National Academy Press, 1980) the amusing account of the successful degradation of a digitoxigenin derivative to etiocholanic acid for structural correlation. Elderfield committed their entire supply, a few hundred milligrams, to a process that took three days and nights, while Jacobs, who had been hesitant to commit the precious substance to such a series of reactions, was out admiring the fall foliage in the Adirondack Mountains. Elderfield's own work at Rockefeller was also responsible for determi

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

nation of the structure of the rare sugar cymarose and its relationship to digitalose.

In 1936 Elderfield moved to Columbia University, where he was an assistant professor for only one year, associate professor during 1937-41, and professor of chemistry (1941-52). There he taught a comprehensive lecture course in organic synthesis that for the first time stressed intermediates, reaction pathways, and relative costs of different routes to the desired products. His laboratory course in advanced organic synthesis was no less rigorous. During my graduate time at Columbia (1939-42) he showed us that he was indeed a terrific experimentalist. Once, when I was having trouble purifying reaction intermediates, he zipped through a four-step sequence on a one-mole scale during two evening sessions. Center cuts of the liquids had perfect elemental analyses, and the initial and terminal refractive indexes of each lot were uniform. I learned by watching and assisting, and I was most grateful for his guidance.

While Elderfield's individual research directions and advice to his graduate students involved a scientific jargon that combined tough vernacular, slang, and metaphor, he gave that advice freely and was always available. He showed great compassion and helped more than one student who got in trouble. He was also supportive of the graduate student society that was organized in my time at Columbia. A fair number of our classmates were “subway graduate students” who had no place to relax or study between classes. A graduate student lounge was the answer. Elderfield and the other faculty members provided funds to convert a classroom (gift of Columbia) for use as a lounge and group seminar room.

Elderfield's humor ran to teasing, an observation corroborated in communication with members of his family. He helped organize an expedition that took advantage of

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

some of the city-dwelling students by taking them on a “snipe hunt” on a moonless night. We left the “baggers” widely distributed in a park in Westchester County while we “beaters” retired to an all-night cafe nearby until we felt it was time to rescue them. I have been told that, when on a fishing expedition with a colleague or grandson, Bob Elderfield would usually throw back the first fish he caught, no matter the size. This bold act was usually accompanied by the statement “too small” or “grow up,” which never failed to impress.

Elderfield's research at Columbia University included the subject of alkaloids with which he had become familiar while at Rockefeller University. His investigation of the cardiac aglycones was extended to include final structure verification and synthesis of model unsaturated lactones related to the aglycones. He and his students devised general methods whereby representative members of the group of cardiac drugs became available synthetically by transformations of naturally occurring sterol derivatives. The methodology, which has been widely applied, is also applicable to the synthesis of isotopically labeled compounds in the series. The pharmacological activity of representative lactones and aglycones was determined in collaboration with K. K. Chen at Eli Lilly and Company in Indianapolis, Indiana. During the years of the Second World War, Elderfield had increased responsibility. He was a section member of the National Defense Research Committee and worked on explosives. He was also executive secretary and eastern regional director of the Panel on Synthesis of the Board for Coordination of Malarial Studies (1943-46), which was succeeded after the war by the Malaria Study Section of the National Institutes of Health (1946-49), and Elderfield served as a consultant during this period. In the Columbia laboratory, he and his coworkers concentrated on the synthesis of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

aminoquinoline drug candidates, discovered a previously undetected rearrangement in one of the synthetic processes, and developed several drugs that represented distinct improvements over those available for the treatment of relapsing vivax malaria. The synthetic innovation that led to the efficient production of pure primaquine (pamaquine, or plasmoquine) was the catalytic reductive condensation of 6-methoxy-8-aminoquinoline with 1-diethylaminopentan-4-one. The naphthoate salt has been used for administration to patients. During this period of antimalarial research, Elderfield enjoyed the valuable cooperation of Leon H. Schmidt of Christ Hospital, Cincinnati, Ohio, in the pharmacological work and of A. S. Alving of the University of Chicago in the clinical work of advancing the medical investigation of antimalarial therapy.

Elderfield was called to the University of Michigan as professor of chemistry in 1952, where he continued his fruitful research, writing, and teaching in a very friendly atmosphere. He did further research on antimalarials and on *Alstonia* alkaloids, structure and synthesis, and added volumes to his *Heterocyclic Compounds.* He was not only the editor of all nine volumes but he was also the author of extensive sections in each. The writing in this series reflects the breadth of his knowledge and interest, concomitant with his clarity of presentation. With his Michigan students he initiated research on novel anionic aromatic substitution reactions and engaged in an ambitious program on potential anticancer agents. The program was centered on the synthesis and testing of nitrogen mustard moieties attached to nuclei of known pharmacological compatibility. The idea was original and provocative and has certainly been stimulating to chemists endeavoring to provide multiple agents capable of attacking cancer cells. From 1952 Elderfield was a scientific con

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

sultant to the (then) Sloan-Kettering Institute for Cancer Research.

At various times he was consultant for Eli Lilly and Company and Esso Research and Engineering. His awards include the Presidential Certificate of Merit (1948), election to membership in the National Academy of Sciences (1949), an honorary doctorate from Williams College (1952), and the Distinguished Faculty Achievement Award of the University of Michigan (1969). His editorial time, notable for the *Heterocyclic Compounds* series, was also donated generously to the major publications of the American Chemical Society. The memorial resolution for Professor Elderfield from the faculty of the College of Literature, Science, and the Arts of the University of Michigan (1980) included a section on his importance to his colleagues:

As well as a superb scholar, Professor Elderfield was a tower of academic and personal strength for his colleagues, particularly the younger ones, and many of the now older members of faculty remember his support with gratitude.

Whatever he did, Bob Elderfield did well. That was simply central to his philosophy. The practice extended to building things, whether bookcases, tables, rock walls, a child's set of wooden alphabet letters, or a cabin and a cookhouse on an island in Sand Lake, Jones Falls, Ontario. He was dependable enough as a fisherman to provide breakfast or supper as needed when the family was in Canada. His pleasure in cooking ranged from making sugar cookies and doughnuts for his girls, through fish chowder and camp stew in the summer, to outdoor grilling for his students and coworkers that followed some competitive athletic endeavor in Hastings-on-Hudson while he was at Columbia. Bob's competitive enthusiasm made its appearance in Michigan when he encouraged the football or basketball team or the slow-

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

running horse of one of his colleagues who had decided to go into horse breeding. He and his wife, Polly, played cribbage regularly and kept annual score. Bob took great pride in the accomplishments of his students—graduates, undergraduates, and postdoctorates—and never forgot that students are an important product of university research.

I AM MOST GRATEFUL to Bob's family—his daughters, Nancy and Margaret Helen, and his sister, Esther—and to Bob's Michigan colleagues, especially Martin Stiles, for the information they provided. I also used the material that Bob himself placed on file in the Office of the Home Secretary of the National Academy of Sciences.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1928

With T. L. Davis. The catalytic preparation of methylamine from methyl alcohol and ammonia. *J. Am. Chem. Soc.*50: 1786-89.

1931

With W. A. Jacobs. Strophanthin. XXI. The correlation of strophanthidin and periplogenin. *J. Biol. Chem.* 91: 625-28.

With W. A. Jacobs. Strophanthin. XXII. The correlation of strophanthidin and periplogenin with digitoxigenin and gitoxigenin. *J. Biol. Chem.* 92: 313-21.

1935

With W. A. Jacobs. The structure of the cardiac aglycones. *J. Biol. Chem.* 108: 497-513.

The chemistry of the cardiac glycosides. *Chem. Rev.* 17:187-249.

The structure and configuration of cymarose. *J. Biol. Chem.* 111:527-35.

1941

With J. Fried. Studies on lactones related to the cardiac aglycones. V. Synthesis of 5-alkyl-α-pyrones. *J. Org. Chem.*6: 566-76.

1943

With E. R. Blout. Synthesis of β-substituted-$\Delta^{\alpha,\beta}$-butenolides from methyl ketones. *J. Org. Chem.* 8: 29-36.

1947

With F. C. Uhle and J. Fried. Synthesis of glucosides of digitoxigenin, digoxigenin and periplogenin . *J. Am. Chem. Soc.* 69: 2235-36.

1948

With F. J. Kreysa, J. H. Dunn, and D. D. Humphreys. A study of the synthesis of plasmochin by the reductive amination method with Raney nickel. *J. Am. Chem. Soc.* 70: 40-44.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1950

Three-, four-, five-, and six-membered monocyclic compounds containing one O, N, and S atom. In *Heterocyclic Compounds,* vol. 1. New York: John Wiley & Sons.

1952

Certain anionic aromatic substitution reactions. *Rec. Chem. Prog.* 13: 119-128.

With E. Werble. 6-Methoxy-8-(4-amino-1-methylbutylamino) quinoline, U.S. Patent 2,604,474, July 22.

1954

With S. L. White. *Alstonia* alkaloids (IV). The structure of alstoniline . *J. Org. Chem.* 19: 683-92.

1957

With H. E. Boaz and E. Schenker. *Alstonia* alkaloids (VII). The structure of alstonidine . *J. Am. Pharm. Soc.* 46: 510-11.

1958

With B. A. Fischer. Total synthesis of alstonilinol. *J. Org. Chem.* 23: 332.

With I.S. Covey et al. Potential anticancer agents (I) Nitrogen mustards derived from p-[N,N-bis (2-chloroethyl)amino] benzaldehyde. *J. Org. Chem.* 23: 1749-53.

1960

With T. H. Bembry and G. L. Krueger. Amino derivatives of strophanthidin (I) Reaction of primary and secondary amines with the butenolide side chain of strophanthidin . *J. Org. Chem.* 25: 1175-79.

With E. LeVon. Potential anticancer agents (III) Nitrogen mustards derived from 8-aminoquinolines. *J. Org. Chem.* 25: 1576-83.

1961

With T.-K. Liao. Potential anticancer agents (XII) Nitrogen mustards from p- aminobenzoic acid derivatives. *J. Org. Chem.* 26: 4996-97.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1967

Pteridines, alloxazines and compounds with 7- membered or larger rings. In *Heterocyclic Compounds,* vol. 9.New York: John Wiley & Sons.

With J. Roy. Synthesis of potential anticancer agents. XVIII. Nitrogen mustards from 6-substituted coumarins. *J. Med. Chem.* 10: 918-21.

With A. C. Mehta. Synthesis of potential anticancer agents. XIX. Nitrogen mustards from 7-hydroxycoumarin derivatives. *J. Med. Chem.* 10: 921-23.

1969

With J. M. Cook and P. W. Le Quesne. Alstonerine, a new alkaloid from *Alstonia muelleriana.J. Chem. Soc. D.* 1306-1307.

1972

With R. E. Gilman. *Alstonia* alkaloids. XI. Alkaloids of *Alstonia muelleriana. Phytochemistry.*11: 339-43.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

GERTRUDE B. ELION

January 23, 1918–February 21, 1999

BY MARY ELLEN AVERY

IN THE SPRING OF 1933 Gertrude Elion graduated from high school and that summer she had to select a major subject before she could begin her freshman year at Hunter College. This posed a quandary for the future Nobel Prize recipient, as well as holder of 45 patents, 23 honorary degrees, and a long list of other honors: She had liked all her school subjects, making it difficult to select just one. “I loved to learn everything, everything in sight and I was never satisfied that I knew everything there was to know in each of my courses.” Fatefully, that summer her grandfather, whom she loved dearly, died of cancer. “I watched him go over a period of months in a very painful way, and it suddenly occurred to me that what I really needed to do was to become a scientist, and particularly a chemist, so that I would go out there and make a cure for cancer.” (All quotations in this memoir are from the author's taped 1997 interview with G. B. Elion).

Become a scientist she did, and along the way she synthesized and co-developed two of the first successful drugs for the treatment of leukemia (thioguanine and mercaptopurine), as well as azathioprine (Imuran), an agent to prevent the rejection of kidney transplants and to treat rheumatoid arthritis. Trudy (as she was called by her many friends) also

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

played a major role in the development of allopurinol for the treatment of gout and of acyclovir, the first selective antiviral agent that was effective against herpes virus infections.

From her first publication in 1939 to her last in 1998, Trudy was involved in the investigation of purines and purine analogs as chemotherapeutic agents. Working in collaboration with George H. Hitchings, she synthesized a large number of purines, including 6-mercaptopurine and thioguanine, and investigated their loci of action in microbiological systems. The 6-mercaptopurine (6MP) became the first purine antagonist to be useful in the treatment of acute lymphoblastic leukemia in children. She elucidated some alterations in metabolic pathways that led to resistance to the purine antagonists. Her 1954 paper that quantified the synergistic effects of purine antagonists with pyrimidine and folic acid antagonists has become a classic in the field. To make the successful 6MP, Trudy replaced an oxygen atom on the purine ring with a sulfur atom. This chemical not only had anti-tumor activity in mice but it also produced remissions, without undue toxicity, in children who had acute leukemia. The excitement about 6MP was so great that the U.S. Food and Drug Administration approved its use late in 1953—only 10 months after clinical trials began and seven months before all the data supporting its effectiveness were made public (NAS, undated).

Gertrude Belle Elion was born in New York City on January 23, 1918. Her father emigrated from Lithuania when he was twelve years old and went on to become a dentist in the United States. Her homemaker mother arrived in the United States from Poland at the age of fourteen. When Trudy was seven, the family moved from a Manhattan apartment-cum-dental office to the Bronx, where she continued her public school education. By the time she was twelve, she had been

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

promoted two years ahead of her class. After receiving her bachelor's degree in chemistry from Hunter College in 1937, Trudy realized that neither she nor her family had enough money for her to attend graduate school. She began to look for a job, and immediately ran into the proverbial brick wall. “Nobody . . . took me seriously. They wondered why in the world I wanted to be a chemist when no women were doing that. The world was not waiting for me.”

Secretarial school followed, and then teaching at a hospital and a high school. She finally landed a position, albeit non-paying, with a chemist, just to keep busy in her field; during this period she decided to pursue her master-of-science degree, which she received in 1941 from New York University. During her graduate studies, she started teaching high school chemistry and physics as a “permanent substitute” for $7.50 a day. Her big break came when the United States entered World War II. Since there were few men around, women came to be seen as potential employees, and Trudy was hired as an analytical chemist; her job included the measurement of the acidity of pickles and the color of mayonnaise. After a while she tired of those functions and a spell of testing the tensile strength of sutures and sought more meaningful work. The most interesting opening was at Burroughs Wellcome, where biochemist George Hitchings was trying to make antagonists to nucleic acid derivatives. Hitchings, who would later become a member of the National Academy of Sciences, “talked about purines and pyrimidines, which I must confess I'd never even heard of up to that point, and it was really to attack a whole variety of diseases by interfering with DNA synthesis. This sounded very exciting.” She accepted the position of biochemist in 1944 and spent the next 39 years at Burroughs Wellcome, becoming head of the Department of Experimental Therapy in 1967.

Let Trudy explain how she started out making compounds

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

and ended up eventually with the first effective drug that induced remission in childhood leukemia.

"At the beginning . . . it was my job to find out how to make (compounds). So I'd go to the library, look up the old literature to see if I could figure out how to do it . . . I would just go ahead and make the compounds, and then the question was, well what do we do with these compounds? How do we find out if they really do anything? [Working with a microorganism like *Lactobacillus casei*] you could throw it in a defined medium and you could tell when you added something that was a real growth antagonist, then analyze why it was an antagonist. We knew that this organism would grow and from that it could make DNA and folic acid . . . You could make everything just from the amino acids, medium, and folic acid, and so on. We knew folic acid was essential, or if you could replace folic acid with a purine, it would grow . . . It would make lactic acid. If the organisms didn't grow, we knew we had something and we might be antagonizing folic acid or it might be antagonizing the purine. So you could with that one organism really make an analysis of three different kinds. You could add purine or folic acid and reverse the antagonism . . . [We] didn't know the structure of DNA, because nobody did at the time, but [we] knew what the building blocks were, and so we were starting really at the very basic portion of the DNA and saying we don't know how it gets to be DNA . . . but let's find out how we can deal with it . . . One of the things we had in mind was to inhibit what kills cancer cells."

By 1949 Trudy had synthesized a purine that inhibited growth in mouse leukemia, which Joseph Burchenal at Sloan Kettering Institute in New York used to treat four patients with chronic granulocytic leukemia, two of whom went into remission. This was the forerunner of 6-mercaptopurine, which continues to be effective in some cancers.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Trudy never completed the requirements for a Ph.D. degree. Shortly after starting her new job at Burroughs Wellcome, she began night courses at Brooklyn Polytechnical Institute in pursuit of a doctor's degree. After two years the institute requested that she convert to full-time to prove she was serious about the degree, but she did not wish to relinquish her exciting work. “It was exactly the kind of job I wanted, and Dr. Hitchings was kind enough to say you won't need your Ph.D. to do the work we're doing.”

In 1983 Trudy retired and assumed the status of scientist emeritus. For the next 16 years she remained active in her field as an advisor to many organizations, including the World Health Organization and the American Association for Cancer Research. As a consultant she was also able to maintain her association with her former employer, now Glaxo Wellcome, Inc., in Research Triangle Park, North Carolina. She attracted many associates who became known as a research dream team, some of whom invented azidothymidine (AZT), a mainstay drug for treatment of HIV (human immunodeficiency virus) infection.

A most rewarding activity was her mentoring each year of a third-year Duke University medical student, who would take a year off from courses and do research under her aegis. “I think it's a very valuable thing for a doctor to learn how to do research, to learn how to approach research, something there isn't time to teach them in medical school. They don't really learn how to approach a problem, and yet diagnosis is a problem; and I think that year spent in research is extremely valuable to them.”

Nephew Jonathan Elion, M.D., recollects the wonder of his aunt's relationships. “She made herself available to students. While people tell me now she was an advocate to the advancement of women in science, this actually comes as news to me, as I always thought of her as advocating the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

advancement of ALL persons in science. She was active in the North Carolina School of Science and Math, did lots with Duke medical students, loved to have young students visit Burroughs Wellcome (and kept a stack of books about herself directed at kids to give away). When she was a visiting professor at Brown, she didn't want to meet with the VIPs and department heads, she asked to arrange for time with the students" (J. Elion, e-mail message, September 15, 1999).

Trudy was awarded the Nobel Prize in physiology or medicine in 1988 for her discovery of important principles for drug treatment. "People ask me often [whether] the Nobel Prize [was] the thing you were aiming for all your life, and I say that would be crazy. Nobody would aim for a Nobel Prize because, if you didn't get it, your whole life would be wasted. What we were aiming at was getting people well, and the satisfaction of that is much greater than any prize you can get."

The prize was shared with her long-time associate George H. Hitchings and English scientist James Black. Others who contributed to the evaluation of her drugs included Joseph H. Burchenal of Sloan-Kettering Institute in New York and Roy Calne, a Cambridge, England, surgeon who came to Boston to join Joseph Murray of Harvard in the hope of finding the answer to rejection of transplanted kidneys in dogs. Dr. Robert Schwartz and William Damashek of Tufts University, also in Boston, pioneered the use of 6-mercaptopurine in patients. In 1998 Joseph Murray and E. Donnall Thomas of Seattle received a Nobel Prize for furthering studies of immunosuppression with azathioprine and later cyclosporin in the 1960s and 1970s.

Trudy remained active in research and professional organizations and held adjunct professorships at Duke University, the University of North Carolina, and Ohio State University.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Among her awards, in addition to the 1988 Nobel Prize, were the National Medal of Science, presented by President George Bush in 1991; the Garvan Medal from the American Chemical Society in 1968; the President's Medal from Hunter College in 1970; the Judd Award from Memorial-Sloan Kettering Institute in 1983; the Cain Award from the American Association for Cancer Research in 1984; the Ernst W. Bertner Memorial Award from the M. D. Anderson Cancer Center and the Medal of Honor from the American Cancer Society in 1990; and 23 honorary degrees.

She was elected to membership in the National Academy of Sciences in 1990 (and served on the Council) and to the Institute of Medicine in 1991. She was a fellow of the American Academy of Pharmaceutical Scientists and the American Academy of Arts and Sciences; a foreign member of the Royal Society; and an honorary member of the Spanish Academy of Dermatology and Venereology, among many others.

Trudy's favorite pastimes were photography, music (especially Puccini, Verdi, and Mozart operas), travel (“I'll climb a mountain to get a picture,” she said in 1993 during an interview with the *Tampa Tribune*), and a passion for raspberries. “Over the years, my work became both my vocation and avocation. Since I enjoyed it so much, I never felt a great need to go outside for relaxation. Nevertheless, I became an avid photographer and traveler. Possibly my love for travel stems from the early years when my family seldom went away on vacation. Thus, my curiosity about the rest of the world did not begin to be satisfied until I began to travel. I have traveled fairly widely over the world, but there still remain many places for me to explore. Another major interest is music, not because I am musically talented, but because I love to listen to it. I am an opera lover and have been a

Copyright © National Academy of Sciences. All rights reserved.

subscriber to the Metropolitan Opera for over 40 years. I also enjoy concerts, ballet, and theater" (*Les Prix Nobel,* 1999).

Jon Elion remembers vividly one particular scene from the Nobel festivities in Stockholm. "The ceremonies are held in an ornate and regal hall. The laureates and other officials are seated on the stage, all men except for Trudy. They are all dressed in black and white, all very staid, stiff, and proper. Trudy stands out . . . in her "Trudy blue" chiffon dress. She is relaxed and is enjoying every minute. She was a lifelong opera fan . . . The chamber orchestra strikes up one of the arias from Mozart's *Don Giovanni.* Trudy smiles, as if they were playing this just for her. She taps her foot and nods her head in time to the music, no doubt thinking of the words to the aria as the music is played. This was in such stark contrast, as the men continue to sit black-and-white and stiff, while "Trudy Blue" Dr. Elion smiles, nods, taps, and mentally sings."

In Jon Elion's words again, "The day after she died, I was sorting through her mail. There were two letters that struck me as representative. One was from a university president thanking her for being a visiting professor there. The other was from a young girl . . . The girl talked excitedly about a school project in which they were doing a wax museum, and the students would play the wax figures. She had researched scientists on the Internet and had selected Trudy as her heroine. Her mother subsequently sent me photos of the girl dressed in a lab coat, holding a beaker, and with an imitation Nobel medal around her neck. I know that Trudy would have nodded briefly while reading the letter from the university president, but would have read and reread the letter from the young girl, and would have taken the time to respond. I did that for her, and sent the girl Trudy's last copy of the book she liked to give out" (J. Elion, op. cit.).

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

The following is from her hand-written notes for a lecture to students.

It seems like only yesterday that I was sitting where you are now. Time passes rapidly when you are having fun. Have a goal that you really care about (cancer research). Don't be afraid of hard work. Nothing worthwhile comes easily. Don't let others discourage you or tell you that you can't do it. In my day I was told women didn't go into chemistry. I saw no reason why we couldn't. It's true it took seven years of various jobs, including a year in graduate school and two years of high school teaching before the shortage of men in civilian jobs gave me the opportunity to prove myself. But after that, I never looked back.

It is important to go into work you would like to do. Then it doesn't seem like work. You sometimes feel it's almost too good to be true that someone will pay you for enjoying yourself. I've been very fortunate that my work led to useful drugs for a variety of serious illnesses. The thrill of seeing people get well who might otherwise have died of diseases like leukemia, kidney failure, and herpes virus encephalitis cannot be described in words. The Nobel Prize was only the icing on the cake. There may be those who try to deter you and discourage you along the way. But keep your eye on the goal. And in the words of Admiral Farragut, "Damn the torpedoes, full speed ahead!" (J. Elion, op. cit.)

I recall Trudy saying, "I don't really want to die until I'm used up." On Sunday, February 21, 1999, she went for her daily walk, but never made it home. Trudy collapsed and was taken to the University of North Carolina Hospital in Chapel Hill, where she died at midnight at age eighty-one. She was never used up.

She is survived by four nephews and two nieces, to whom

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

she was devoted. She is greatly admired by a host of students and colleagues who remember her brilliance, devotion to science, and drive to find a cure for cancer. Little did she anticipate the myriad drugs that she would synthesize for so many diseases, including some forms of malignancies. This great humanitarian rejoiced when she could be helpful to others, and her enthusiasm for her work was contagious.

I THANK THE FOLLOWING persons for their assistance in producing this memoir: Trudy's nephew Dr. Jonathan Elion for his reminiscences, Martha Peck of the Burroughs Wellcome Fund for her cooperation and recollections of Trudy, Dr. Edward D. Harris, editor of *The Pharos,* who had requested the oral interview on behalf of Alpha Omega Alpha, and especially Jim Lawson, who organized and wrote much of the text of this memoir.

REFERENCES

Avery, M. E.1997. Interview with Gertrude Elion, Chapel Hill, N, Car., for the Alpha Omega Alpha Oral History Project, May 1997.

*Les Prix Nobel.*1999. Gertrude B. Elion. At www.nobel.se/laureates/medicine-1988-2-autobio.html, accessed September 9, 1999.

NAS (National Academy of Sciences). Undated. Beyond Discovery: A Leap of Faith. Available at www4.nas.edu/beyonddiscovery.nsf/web/leukemia7, accessed October 18, 1999.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

SELECTED BIBLIOGRAPHY

(from a total of 303)

1939

With A. Galat. Preparation of primary amines. *J. Am. Chem. Soc.*61: 3585-86.

1946

With W. S. Ide and G. H. Hitchings. The ultraviolet absorption spectra of thiouracils. *J. Am. Chem. Soc.*68: 2137-40.

1949

With J. A. Burchenal, A. Bendich, G. B. Brown, G. H. Hitchings, C. P. Rhoads, and C. C. Stak. Preliminary studies on the effect of 2,6-diaminopurine on transplanted mouse leukemia. *Cancer*2: 119-20.

1951

With G. H. Hitchings and H. VanderWerff. Antagonists of nucleic acid derivatives. VI. Purines. *J. Biol. Chem.*192: 505-18.

1952

With E. Burgi and G. H. Hitchings. Studies on condensed pyrimidine systems. IX. The synthesis of some 6-substituted purines. *J. Am. Chem. Soc.*74: 411-14.

1953

With S. Singer and G. H. Hitchings. The purine metabolism of a 6-mercaptopurine-resistant *Lactobacillus casei.J. Biol. Chem.*204: 35-41.

1954

With S. Singer and G. H. Hitchings. Antagonists of nucleic acid derivatives. VIII. Synergism in combinations of biochemically related antimetabolites. *J. Biol. Chem.*208: 477-88.

1961

With S. Callahan, S. Bieber, G. H. Hitchings, and R. W. Rundles. A summary of investigations with 6-[(1-methyl-4-nitro-5-imidazolyl) thio] purine (BW 57-322). *Cancer Chemother. Rep.*14: 93-98.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1963

With S. Callahan, H. Nathan, S. Bieber, R. W. Rundles, and G. H. Hitchings. Potentiation by inhibition of drug degradation: 6-substituted purines and xanthine oxidase. *Biochem. Pharmacol.*12: 85-93.

1966

With S. Callahan, R. W. Rundles, and G. H. Hitchings. Relationship between metabolic fates and antitumor activities of thiopurines. *Cancer Res.*23: 1207-17.

With A. Kovensky, G. H. Hitchings, E. Metz, and R. W. Rundles. Metabolic studies of allopurinol, an inhibitor of xanthine oxidase. *Biochem. Pharmacol.*15: 863-80.

1969

Actions of purine analogs: Enzyme specificity studies as a basis for interpretation and design. *Cancer Res.*29: 2448-53.

1977

With P. A. Furman, J. A. Fyfe, P. de Miranda, L. Beauchamp, and H. J. Schaeffer. Selectivity of action of an antiherpetic agent, 9-(2-hydroxyethoxymethyl) guanine. *Proc. Natl. Acad. Sci. U.S.A.*74(12): 5716-20.

1982

Mechanism of action and selectivity of acyclovir. *Am. J. Med.*73: 7-13.

With K. Biron, J. A. Fyfe, and J. E. Noblin. Selection and preliminary characterization of cyclovir-resistant mutants of varicella zoster virus. *Am. J. Med.*73: 383-86.

1983

With L. S. Kucera and P. A. Furman. Inhibition of acyclovir of herpes simplex virus type 2 morphologically transformed cell growth in tissue culture and tumor-bearing animals. *J. Med. Virol.*12: 119-27.

1985

With G. H. Hitchings. Layer on layer. *Cancer Res.*45: 2415-20.

Selectivity—Key to chemotherapy. Presidential address. *Cancer Res.*45: 2943-50.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1993

Acyclovir discovery, mechanism of action and selectivity. *J. Med. Virol.* Supp. 1: 2-6.

1998

With others. Therapeutic efficacy of vinorelbine against pediatric and adult central nervous system tumors. *Cancer Chemother. Pharmacol.* 42(6): 479-82.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

EDWARD VAUGHAN EVARTS

March 28, 1926–July 2, 1985

BY WILLIAM THOMAS THACH, JR.

ON JULY 2, 1985, Edward Vaughan Evarts, Chief of the Laboratory of Neurophysiology at the National Institute of Mental Health (NIMH), died suddenly in his office from a myocardial infarction. He was fifty-nine years old and at the peak of his career.

Born in New York City, Evarts received his undergraduate education at Harvard College and was granted his M.D. by Harvard Medical School in 1948. After an internship at Boston's Peter Bent Brigham Hospital, Evarts worked for one year with the psychoneurologist Karl Lashley at Yerkes Laboratories of Primate Biology in Orange Park, Florida, and for another year at the National Hospital for Nervous Diseases in London. He completed his postdoctoral training with a two-year residency in psychiatry at the Payne Whitney Institute in New York. Evarts then began his lifelong association with the NIMH in Bethesda, upon his appointment as head of the section on physiology in its new Laboratory of Clinical Science directed by Seymour Kety. He remained in that position until he became chief of the Laboratory of Neurophysiology in 1970.

Evarts's neurobiological research spanned three and a half decades, starting with his work on the behavioral effects

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

of ablating various areas of the visual and auditory cerebral cortex in monkeys that he carried out in Lashley's laboratory. On moving to the NIMH, he investigated the neurophysiological effects of LSD, which at that time promised to provide a fruitful approach to understanding schizophrenia. He also studied post-tetanic potentiation in the cat's visual pathway, as a possible model for behavioral adaptation.

At the age of thirty-six, Evarts made his first major discoveries, when he began to take electrophysiological recordings from single cortical neurons in cats and monkeys in their waking and sleeping states (1962, 1964). He observed that such single unit activity is higher during the rapid eye movement (REM) phase of sleep than during visual experience in the waking state. This was a crucial finding, and it showed that sleep is not attributable to a passive state of the cerebral cortex. In further support of this inference he showed, in collaboration with Kety, that cerebral blood flow during REM sleep is also higher than during the wakeful state. To pursue the study of brain function during waking behavior Evarts developed the methods for recording single-neuron activity during operantly conditioned movements in monkeys (1966), for which he is well known. The principle of correlation of brain cell activity with behavior, well established by that time, was rooted in the pioneering studies of E. D. Adrian, Vernon Mountcastle, David Hubel, and Torsten Wiesel in anesthetized paralyzed animals. There had been some prior studies in awake animals by Ricci, Doane, and Jasper, who first recorded brain function in trained monkeys, and by Hubel who recorded brain function in freely moving cats; but it was Evarts's own brilliant perfection of the method of single unit recording from the cortex of awake animals that would lead to its later widespread use.

Evarts's first studies of movement control proved the utility of this method and supported previous inferences about

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the role of motor cortex in voluntary movement based on ablation and electrical stimulation studies. He trained monkeys to release a telegraph key promptly at a visual or acoustic signal. In this way he found that a motor cortex neuron projecting toward the spinal cord (identified by electrophysiological back-excitation from the medullary pyramid) usually alters its firing pattern just prior to the onset of movement (1966). While any one neuron fires (or pauses) at some fixed time before (or after) the movement, the average time of change across all neurons precedes the electromyographic (EMG) activity of the target muscles by as much as 60 ms (1973, 1974). Some of these neurons increase their firing rate with the force generated in the movement (1968), while others do so with the force needed to hold still (1969). With Fromm, he found (1981) that neurons in the motor cortex appear to be organized according to a "size principle" similar to the one Henneman applied to the activities of spinal motor neurons. This principle states that, for the command of smaller movements involving lesser force, only the smaller cortical neurons are recruited, while for the command of larger movements involving greater force the activity of the smaller neurons is supplemented by an additional recruitment of larger cortical neurons. Thus, motor cortex neurons would appear to behave similarly to spinal motor neurons in controlling muscle force by a dual mechanism of modulation of neuronal firing rate and neuronal recruitment.

Evarts presented these early results at a meeting jointly sponsored by the Parkinson's Disease Foundation and the National Institutes of Health (1967), where they aroused considerable interest. He obtained even more exciting results when he addressed questions about the roles of other parts of the brain in motor and mental activities that had not been answerable hitherto by less direct approaches. As for

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

his aspirations regarding the ultimate goal of understanding the physical basis of mental activity, he agreed with Roger Sperry, who held that one must understand movement before one can understand the mind behind it. According to Sperry, (as quoted by Evarts):

An analysis of our current thinking will show that it tends to suffer generally from a failure to view mental activities on their proper relation, or even any relation, to motor behavior . . . We conclude that the unknown cerebral events in psychic experience must necessarily involve excitation patterns so designed that they intermesh in intimate fashion with the motor and premotor patterns. Once this relationship is recognized as a necessary feature of the neural correlates of psychic experience, we can automatically exclude numerous forms of brain code which otherwise might seem reasonable but which fail to meet this criterion.

As to a method for pursuing this goal, Evarts credited C. S. Sherrington for having recognized the need for recording brain cell activity during the actual performance of behavioral tasks that critically identify dissociate and control the pertinent motor and mental variables. Evarts (1967) wrote:

Sherrington had written that the problem of whether the discharges of motor cortex neurons represents a step toward psychical integration or, on the other hand, expresses the motor result of psychical integration or are participant in both is a question of the highest interest, but one which does not seem as yet to admit of satisfactory answer . . . [but] by combining methods of comparative psychology . . . with the methods of experimental physiology, investigation may be expected ere long to furnish new data of importance toward the knowledge of movement as an outcome of the working of the brain.

Here then was a plan for future experiments laid out for all to see, in that open manner so very characteristic of Evarts. In his own laboratory his students began to study

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

the neural timing and coding of eye movements and of cerebellar, basal ganglia, red nucleus, and premotor cortical neural control of limb movements. Evarts took little or no direct credit for his students' achievements. He did not put his name on papers reporting the results of projects carried out by his junior colleagues, although Evarts's contributions to them were obvious. Their studies followed questions and strategies that he had pursued himself in his work on the motor cortex and had prescribed for other areas in his seminal 1967 article. Inspired by the sense of independence that Evarts had nurtured and the skills that he had taught, students left him to set up their own laboratories. Evarts unstintingly helped this process with ideas for projects, plans for building equipment, computer programs for data analysis, and hardware for making their startups possible and successful. Soon his students' laboratories began to make significant contributions, and their productivity testified to the generosity and genius of their mentor. Other laboratories in the NIH and its vicinity quickly adopted Evarts's methodologies, if only to adapt and modify them further to meet their individual needs, all with his enthusiastic help. Eventually, more distant laboratories in the United States, Europe, Asia, and Australia began to study sensation, movement, motivation, attention, and their integration and adaptation by means of the methods Evarts had pioneered. Their common goal was the direct observation of neural signals as they are correlated with and, by inference, generate overt behavior. The fruits of this endeavor reflected Evarts's energy and generosity that had made it possible to create a new field of scientific endeavor within a decade

Perhaps Evarts's most important contribution was the elucidation of the phenomena termed “psychomotor set” and “transcortical reflex” (1973, 1976). In the former the word “set” refers to the state of psychological preparedness for a

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

motor action in response to an anticipated stimulus. As established by Evarts, the psychomotor set is manifested in the temporal correlation of single unit activity in the motor cortex with the preparation of an animal (and therefore with its intent) to carry out a trained movement and its subsequent implementation. In the latter term the word "transcortical" implies a motor reflex arc in which the stimulus is provided by neurons in cortical areas outside the motor cortex and the responding motor neurons are located in the motor cortex.

Evarts's experimental paradigm was to train a monkey to make a particular movement by contracting a particular muscle in response to two successive signals. The first, or set, signal is an instruction to implement the movement by contracting the particular muscle as quickly as possible after a second, or "go," signal. (The go signal was a brief passive stretching of the same muscle). Evarts observed a sustained increase in firing rate in motor cortex neurons that began soon after the occurrence of the set signal and lasted for many seconds while the animal expectantly awaited the go signal (i.e., had the intent to implement the movement for which it had been trained). When the muscle to be contracted was passively stretched by the go signal, a burst of EMG activity occurred with a 12- to 25-ms delay, followed by a brief twitch contraction that was insufficient to accomplish the instructed move. Evarts reasoned that it was the spinal stretch reflex arc leading from the muscle spindle stretch receptor to the spinal cord motoneuron that evoked this twitch. However, 20 ms or so after the go signal's passive muscle stretch, motor cortex neurons underwent a burst of activity. After another 10-20 ms, the motor cortex activity burst was followed by a second burst of EMG activity in the muscle that, in turn, was followed by a second contraction. That second contraction differed from the first in that it

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

was sufficiently prolonged to implement the instructed movement.

Evarts inferred that the instruction (i.e., the set-related activity burst of motor cortex neurons) must have been set off by cerebral neurons outside the motor cortex that are involved in the expectancy of and planning for implementation of the movement. That set-related activity burst must be used, in turn, to gate the motor cortex for its response to the go signal to implement the trained movement. Without the set signal and its gating of the motor cortex the go signal could not have excited the motor cortex neurons enough to reach the threshold for command of a muscle contraction. This phenomenon thus constituted what Evarts referred to as a transcortical reflex.

Evarts acknowledged the predictions of other workers regarding the presence of neuronal activity representing transcortical gating signals in psychomotor set. But it was his direct observation of these phenomena at a single unit level that connected them and provided important clues about psychomotor mechanisms. Because of the novelty and the importance of Evarts's discovery, his findings and claims gave rise to controversy; however, they were soon confirmed by the independent work of others and a decade later were crucially extended by Cheney and Fetz (1984). Evarts lived to see the controversy definitively resolved in his favor.

In his book *Neurophysiological Approaches of Higher Brain Functions* written jointly with Shinoda and Wise (1984), Evarts summarized the advances in understanding of set and attention made possible through single unit recording in awake behaving trained animals. In his characteristically open manner he laid out plans for future experiments that he (and others) would use to study the mechanisms of synthesis and use of set and attention signals in brain cell activity. Some experimenters applied Evarts's method of single unit

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

recording in trained monkeys to locate the cortical areas giving rise to set signals and to characterize the nature of their timing and coding. But many more investigators still used the older methods of controlled electrical stimulation, combined with extra- and intracellular recording to reveal the nature of the interaction between the set and go signals that result in the triggering of a motor response. Evarts supposed that gating for the go signal most probably occurred in a vertical column of cells of the motor cortex. According to him, set signals from supplementary motor, premotor, and ultimately from the prefrontal cortex impinge on these cells, while the go signal reaches them from the dentate nucleus of the cerebellum. Before long, both of these suppositions would be shown to be correct by many other workers. Another of Evarts's suppositions was that the cerebellar go signals would set off activity of motor cortex neurons if, and only if, the motor cortex was primed by set signals. He predicted, moreover, that other cortical areas would be found where convergent inputs to the cortex would perform similar gating and trigger functions. Indeed, he concluded that such a gating process is likely to provide a general basis for linking an appropriate motor response to an adequate stimulus. Evarts's book was his last will and testament. His death prematurely stopped his own skilled hand, but he left a legacy of ideas, plans, methods, students, and competitors that guaranteed the continued pursuit of his long-range goals.

Throughout his life, Evarts found the energy and made time for many activities outside the laboratory to serve the cause of the neural sciences. In addition to his membership in the National Academy of Sciences, he belonged to the American Physiological Society, the International Brain Organization, the Neurosciences Research Program, and the Society for Neuroscience. He served on the editorial boards

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

of nine journals and was the editor in chief of the *Journal of Neurophysiology.* His many honors included the Karl S. Lashley Award of the American Philosophical Society.

Persons who knew Edward Evarts remember him as a tall, spare, and fastidious man with blue eyes that never wavered, with a deep, rich voice that was quiet, reasonable, and gently persuasive, and with an open manner that was usually friendly and smiling (but could also be quite otherwise). For Evarts, science was deeply personal. He relied on experience and introspection to put him in touch with the abstract. He related his work on the neurophysiology of sleep to his own sleep, wondering about the relation between dreams, rapid eye movement sleep, and somatic exercise and repair functions. His own mental and physical exercise became a setting for his work on movement control. He organized a study group that met three or four times a year and included his own students, as well as senior neurobiologists from other institutions, such as Eric Kandel and Alden Spencer. The group was dedicated to trying to solve such problems as the linkage between stimulus and a response; the temporal programming of multi-jointed movements; how the spatial guidance of movements is guided in space; and the mechanisms that underlie the learning of motor routines.

Evarts's life was thoroughly integrated; every component seemed grist for his work. Perhaps the most ironic parallel was his study of mental set as a preface to action—a paradigm and parable of his own determination. He tried hard and worked hard, and he encouraged in others the perfection that he demanded of himself. He was always the first one to arrive in the laboratory in the morning, and he insisted that others arrive on time, a practice that he once tried to enforce by having his younger colleagues sign in with a time clock. He would not tolerate idleness, dishonesty, or second-rate work in his associates. He would dismiss a

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

person he thought was not suited to a job that he controlled, even if that person was a friend. He was as hard on himself as he was on others.

Edward's joy of life was unforgettable. His laugh was always the loudest in the room. It was infectious. If he laughed everyone laughed with him. He was a thinking man, and he found amusement in human foibles and even in genuine villains. He usually saw the humorous side of things, even when he was the victim, which was often enough. He lost some of his battles, but accepted defeat with stoic humor, making the best of it and living gracefully with his adversaries. He could make his point with tact, without giving offense.

To the many for whom Edward Evarts was a friend, he gave insightful and wise advice and unstinting help. Ever generous, he was a loyal friend.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1952

Effect of ablation of prestriate cortex on auditory-visual association in monkey. *J. Neurophysiol.* 15: 191-200.

Effect of auditory cortex ablation on auditory-visual association in monkey. *J. Neurophysiol.*15: 435-41.

1954

Psychopathological effects of drugs. In *Proceedings of the Fourth National Medicinal Chemistry Symposium,*pp. 145-50. Syracuse, N.Y.: American Chemical Society.

1955

With W. Landau, W. H. Freygang, Jr., and W. H. Marshall. Some effects of lysergic acid diethylamide and bufotenine on electrical activity in the cat's visual system. *Am. J. Physiol.*182: 594-98.

1956

With J. R. Hughes and W. H. Marshall. Post-tetanic potentiation in the visual system of cats. *Am. J. Physiol.*186: 483-87.

1957

With J. R. Hughes. Relation of post-tetanic potentiation to subnormality of lateral geniculate potentials. *Am. J. Physiol.*188: 238-44.

1958

Neurophysiological correlates of pharmacologically induced behavioral disturbances. In *The Brain and Human Behavior,* vol. XXXVI, pp. 347-80. Baltimore: Williams and Wilkins.

1960

With T. C. Fleming and P. R. Huttenlocher. Recovery cycle of visual cortex of the awake and sleeping cat. *Am. J. Physiol.*19: 373-76.

1961

Effects of sleep and waking on activity of single units in the unrestrained cat. In *The Nature of Sleep,*pp. 171-82. London: J. & A. Churchill.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1962

A neurophysiologic theory of hallucinations. In *Hallucinations,* ed. L. J. West, pp. 1-14. New York: Grune and Stratton.

Activity of neurons in visual cortex of cat during sleep with low voltage fast EEG activity. *J. Neurophysiol.*25: 812-16.

1964

Temporal patterns of discharge of pyramidal tract neurons during sleep and waking in the monkey. *J. Neurophysiol.*27: 152-71.

1966

Methods for recording activity of individual neurons in moving animals. In *Methods in Medical Research,* ed. R. F. Rushmer, pp. 241-50. Chicago: Year Book Medical Publishers.

Pyramidal tract activity associated with a conditioned hand movement in the monkey. *J. Neurophysiol.*29: 1011-27.

1967

Representation of movements and muscles by pyramidal tract neurons of the precentral motor cortex. In *Neurophysiological Basis of Normal and Abnormal Motor Activities,* ed. M. D. Yahr and D. P. Purpura, pp. 215-51. Hewlett, N.Y.: Raven Press.

1968

Relation of pyramidal tract activity to force exerted during a voluntary movement. *J. Neurophysiol.*31: 14-27.

1969

Activity of pyramidal tract neurons during postural fixation. *J. Neurophysiol.* 37: 375-85.

1971

Activity of thalamic and cortical neurons in relation to learned movement in the monkey. *Int. J. Neurol.*8: 321-26.

1973

Motor cortex reflexes associated with learned movement. *Science*179: 501-503.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1974

With J. Tanji. Gating of motor cortex reflexes by prior instruction. *Brain Res.*71: 479-94.

Precentral and postcentral cortical activity in association with visually triggered movement. *J. Neurophysiol.*37: 373-81.

1976

With J. Tanji. Reflex and intended responses in motor cortex pyramidal tract neurons. *J. Neurophysiol.*39: 1069-80.

With J. Tanji. Anticipatory activity of motor cortex in relation to direction of an intended movement. *J. Neurophysiol.*39: 1062-68.

1981

With C. Fromm. Relation of size and activity of motor cortex pyramidal tract neurons during skilled movements in the monkey. *J. Neurosci.*1: 453-60.

1984

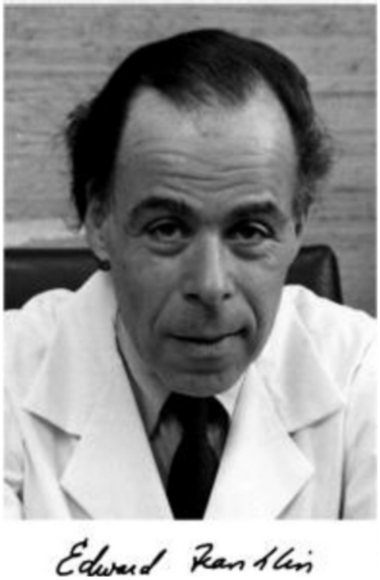
With Y. Shinoda and S. P. Wise. *Neurophysiological Approaches to Higher Brain Functions.*New York: Neurosciences Research Foundation and John Wiley & Sons.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.



Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

EDWARD C. FRANKLIN

April 14, 1928–February 20, 1982

BY HENRY METZGER

EDWARD C. FRANKLIN, was an outstanding example of a physician-scientist. By applying the new tools for analyzing protein structure he made significant contributions both to clarifying the fundamental structure of antibodies and to our understanding of particular clinical syndromes. Although his specialty training was in rheumatology, his career would today be characterized as encompassing clinical immunology. In addition to his achievements in research he was a dedicated clinical teacher and contributed actively to professional societies in his discipline. He died of a brain tumor at the height of his career just prior to his fifty-fourth birthday.

Franklin, the only child of a prosperous attorney and his wife, was born in Berlin, Germany, on April 14, 1928. The family did not flee Germany until late 1938, likely reflecting the ambivalence well-assimilated German Jews felt about leaving their homeland. After an enforced fifteen-month sojourn in Cuba, they were finally able to emigrate to New York City in 1940.

Franklin's native intelligence, his excellent scholarly preparation in Germany, and hard work allowed him to graduate from Townsend Harris High School at the age of fifteen. He went on a full scholarship to Harvard University, from

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

which, despite working part time, he graduated magna cum laude as a biochemistry major at the age of eighteen.

In the late 1940s most medical schools in the United States still enrolled few members of minorities and women, and despite his outstanding credentials, he was admitted only to New York University, from which he graduated in 1950. A year each of internship at New York's Beth Israel Hospital and residency in internal medicine at Montefiore Hospital were followed by two unremarkable years of military duty and then the completion of his residency at the Bronx Veterans Administration Hospital.

Biomedical research and the expanding support for physician-scientists in the United States got their jump-start in the decade of the 1950s, and ultimately, because of Henry G. Kunkel's investigations of liver disease, Franklin was drawn to Kunkel's laboratory at the Rockefeller Institute for Medical Research. Kunkel's enthusiasm for newly initiated studies on antibodies and multiple myeloma persuaded Franklin to work in those areas. He later reminisced that the laboratory was a “cauldron of excitement” with “endless stimulating discussions at all hours of the day or night.” It is hard to think of a single laboratory whose influence was as profound in the training particularly of those leaders in immunology who would so fruitfully shuttle between the laboratory and the bedside. Hans Müller-Eberhard, who would become one of the world leaders in the field of complement, was already there; Gerald Edelman (Nobel prize, 1972), one year junior to Franklin at Harvard, became a graduate student with Kunkel during Franklin's tenure at the Rockefeller, and remained a longtime friend of the Franklins.

Franklin embarked on the field of immunology at one of its most exciting phases, namely, the development of the subdiscipline of molecular immunology. Specifically, his scientific career of about twenty-five years spanned the pivotal

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

period during which the structures of antibodies and the unusual genetic organization that codes for them were elucidated by the group of molecularly oriented immunologists of which he became an active member.

The advent of powerful new tools for separating proteins such as the ultracentrifuge and free electrophoresis made it possible to determine some of the physical characteristics of antibodies in the late 1930s. It was not until two decades later that the techniques of cellulose-based ion exchange chromatography, molecular sieve chromatography, and zone electrophoresis on starch blocks and in polyacrylamide gels spectacularly extended the preparative and analytic options. Likewise, the transfer of the classical precipitin reaction between antibodies and antigens from solution in test tubes to two-dimensional gels—later coupled with electrophoresis—added incisive tools. Finally, just as Franklin was beginning his career, the initial productive use of proteolytic enzymes to dissect the structure of antigens and antibodies validated the belief that the bewildering phenomenology of the immune response could yield to the reductionist approach. Ed Franklin was among the earliest of those who saw the opportunities these methods provided.

Franklin's special contribution was his perceptiveness in recognizing those “accidents of nature” occurring in the clinic that could provide insight into normal structure and function and in pursuing these with thoroughness and rigor. Some of his most important contributions relate to the abnormal proteins that piqued his curiosity. While some of the most influential immunochemists of his day looked askance at the so-called paraproteins as freaks, the investigation of which was more likely to mislead than to inform, others (among them Kunkel and Frank W. Putnam) recognized that the homogeneity of these proteins offered a unique opportunity for revealing canonical aspects of antibody struc

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ture that were shrouded by the confusing heterogeneity typical of “normal” antibody preparations. Franklin's contributions testify to the validity of the more optimistic assessment.

Franklin's bibliography reflects his constant interest in exploring the structure of the γ-globulins, or as they would ultimately be dubbed the immunoglobulins, and almost a third of his publications dealt with various aspects of their structure and relationship to each other. His early work in Kunkel's laboratory involved the relationship between the high molecular weight (19S) and low molecular weight (7S) antibodies and the nature of the autoantibody-like factor seen in patients with rheumatoid arthritis (rheumatoid factor). Other notable investigations after he became independent dealt with the relationship of various myeloma proteins to normal antibodies, characterizations of the disulfide linkages in various antibody classes, structural differences between the closely related human IgA1 and IgA2, and the unusual hinge region of IgG3. He became especially prominent because of his achievements in three particular areas: heavy chain disease, essential mixed cryoglobulinemia, and amyloid.

Franklin described the discovery of the heavy chain diseases as his major scientific contribution in a short autobiography he prepared in September 1980 in connection with his election to the National Academy of Sciences; it was also this subject that he chose for his lecture to the Harvey Society. (Franklin had been diagnosed as having a glioblastoma at the end of 1980, and on the day scheduled for the lecture, November 19, 1981, he was already so incapacitated that his talk had to be read by his wife, Dorothea Zucker-Franklin,[1] with Franklin in attendance. He died three months later.)

His discovery, described in that lecture, began character

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

istically with the observation of a grossly abnormal electrophoretic pattern of the serum of a patient. Mr. Cra, a Bellevue employee, had been followed for some months because of unexplained fever and lymphadenopathy. Compared to an earlier sample of his serum, the recent one showed a virtual disappearance of the normal globulin fraction. It was replaced by a newly prominent peak of intermediate mobility that was likewise observed in the urine. Its plentiful supply from this source (1g / L!) aided its initial characterization, and within days that December of 1962, Franklin submitted an abstract describing his studies for the meeting of the American Association of Immunologists scheduled for Atlantic City four months later.

Even before his presentation, Franklin generously allowed the patient and his serum and urine to be studied by Elliot F. Osserman at the Francis Delafield Hospital. Just three months later a patient with similar clinical and laboratory findings was referred to Osserman prompting him and his colleague K. Takasuki (still an active investigator at Kumamoto University in Japan) to review the 400 cases of monoclonal gammopathies Osserman had collected. One of these, examined four years earlier, proved to be the third case of what was clearly a plasma cell dyscrasia with clinical features distinct from those seen in multiple myeloma and with a unique γ-globulin-like serum component consisting of an incomplete heavy chain. It was they who designated the syndrome as heavy ($H^{\gamma 2}$) chain (Franklin's) disease.[2]

Franklin's first full description of his original patient appeared in 1964, and is his third most cited paper. The molecular defect in one instance of heavy chain disease was first fully elucidated in 1969 in the laboratory of Caesar Milstein (Nobel prize, 1984) by Franklin's future longtime colleague, Blas Frangione, and in 1971 Frangione and Franklin uncovered the abnormality in the original pro

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

tein, CRA. By the time of his Harvey Lecture, Franklin had authored twenty additional research reports on this subject and seven reviews. Franklin recognized that these immunoglobulin sports gave insight into the genetic organization of immunoglobulin structure, a subject that excited not only some of the foremost immunologists of the day but geneticists more broadly.

The mixture of invariant and variable domains (a term popularized by Gerald Edelman) challenged the dogma of “one gene/one polypeptide chain.” I remember well how in 1964 at a workshop in Warner Springs, California, Norbert Hilschman first showed (briefly!) his still unpublished sequences of the two Bence-Jones proteins he had analyzed in Lyman C. Craig's laboratory (also at the Rockefeller). The complete partitioning of the constant and variable regions of the two kappa light chains electrified the participants and provoked animated discussion and speculation. J. Claude Bennett and William J. Dryer, who had attended that meeting, were the first to clearly articulate the heretical hypothesis that eventually proved if anything an understatement: that in the case of immunoglobulins a single polypeptide was encoded by two discrete genes.[3] In analyzing the ever-increasing number of heavy chain disease proteins, Franklin extended this idea and was led to the notion that in heavy chains, the hinge and each domain might be coded for by separate gene segments. He took pride in having anticipated by many years the molecular genetic studies, for example those by Tasuku Honjo, which directly demonstrated the genetic discontinuities.

Franklin's discovery of γ heavy chain disease proved to be only the first example of such discordant synthesis of heavy chains. Maxime Seligman and his colleagues at the Hôpital St. Louis in Paris discovered α chain disease, a syndrome previously known as Mediterranean lymphoma character

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ized by intestinal infiltrates of plasma cells. Two years later, Franklin was a co-author of the publication describing the first patient with recognized μ chain disease.

In the mid-1960s Franklin spearheaded a systematic study of cryoglobulinemia, and the back-to-back articles in the prestigious *American Journal of Medicine* describing the results are the most cited works in Franklin's bibliography. Proteins that reversibly precipitate on cooling of blood had been described in cases of multiple myeloma and macroglobulinemia for some thirty years and had been implicated in the symptoms of peripheral vascular insufficiency that were induced or aggravated by cold in some of these patients. Franklin, Martin Meltzer, and their colleagues in the New York University Rheumatic Diseases Study Group exhaustively studied some twenty-nine consecutive patients they encountered in their clinic, and the first paper describes the clinical picture and the common and variable features of the abnormal proteins in those patients. The investigators were only partially successful in uncovering the molecular mechanisms by which the abnormal serum proteins induced the clinical consequences. They were unable to define any distinctive physical chemical characteristics of those proteins exhibiting cryoprecipitability, and they remained unclear about how the cryogammaglobulins produced the complex of symptoms and why these symptoms occurred at particular concentrations. They did note that the temperature at which precipitation of the proteins began, rather than the concentration of the cryoglobulin, appeared to be one of the more important factors that correlated positively with clinical severity.

In part because they allowed the serum to incubate over many hours in the cold, they discovered a relatively high incidence of mixed cryoglobulins (in most, a complex of an IgM rheumatoid factor and IgG) compared to prior

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

studies, and it is the twelve patients with this phenomenon on which the second paper focuses. A careful clinical description of each patient in turn documents the characteristic clinical features in this group with essential mixed cryoglobulinemia: a female presenting with purpura involving principally the lower extremities, arthralgias generally without arthritis, moderate anemia, and hypergammaglobulinemia. Where data could be obtained, there was evidence for a diffuse glomerulonephritis as well as more widespread arteritis. The group concluded that they were likely dealing with a previously unrecognized type of connective tissue disease. Its similarities to experimental serum sickness suggested an aberrant response to some antigenic insult.

Their repeated observation of clinical or laboratory evidence of hepatic involvement in these patients suggested a hepatitis virus as a plausible culprit. They were also aware of the reports from the Rheumatology Service at the nearby Hospital for Special Surgery and by a group at Baylor University of extensive extra-hepatic manifestations in the absence of severe hepatic signs and symptoms in cases of hepatitis B viral infections. Assays for the hepatitis B surface antigen (or for antibodies to it) and electron microscopy of the cryoprecipitates strongly supported their suspicion. They proposed that the hepatitis B virus "plays a part in the pathogenesis of the syndrome of essential cryoglobulinemia in the majority of cases," and a subsequent more complete clinical analysis supported their hypothesis.

Franklin's last assessment of the pathogenesis of the syndrome he described appeared in 1980 in a report on the long-term follow-up of forty of their patients. He and his colleagues reiterated that the clinical features, the characteristics of the cryoglobulins, the usually depressed levels of complement during the active phase of the disease, and the deposition of immune complexes and complement in the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

lesions together supported the notion that the cryoglobulins are immune complexes and that the disorder is an immune complex-type of vasculitis. They reviewed the circumstances leading to the proposal that the mixed cryoglobulins represented immune complexes, and then summarized the variety of antigen-antibody complexes likely responsible for the elicitation of the rheumatoid factors that formed the basis of the cryoglobulinemia. That cryoglobulins interacted with complement and the localization of IgM, IgG, and complement in the cutaneous and renal lesions was felt to be compelling evidence for the direct pathogenetic role of the cryocomplexes. Nevertheless, the underlying etiologic agents "remain[ed] ill defined," and they concluded that a variety of infectious agents and perhaps other stimuli might play a role. They proposed further that the different sex ratios in particular subsets of patients suggested a predisposition to an aberrant immune response in those with particular HLA genotypes or hormonal status. Over the succeeding twenty years only two features need to be added to their assessment: clinically, the greater appreciation of peripheral neuropathy as part of the symptom complex and pathogenetically the association of hepatitis C infection in $\geq$90% of the patients.

In 1968 Mordechai Pras, a newly arrived Fulbright fellow with an interest in amyloidosis, brought with him from Israel a frozen spleen from a patient with the idiopathic form of the disease. Franklin had never personally worked on amyloid, and had planned for Pras to work on one or another aspect of immunoglobulins. However, Pras had shown some of the sections to Dorothea Zucker-Franklin who was fascinated with amyloid's birefringent properties, and she convinced Ed that it would be fun to learn more about this substance.

Infiltrates of the material had been observed in a variety of tissues in an assortment of diseases. Four main types had been distinguished: primary, secondary (e.g., to chronic

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

inflammation), multiple myeloma-associated, and a variety of familial types. (Thirty years later, during his training at the National Institutes of Health under Daniel Kastner, Pras's son Elon was to be a principal in uncovering the gene for familial Mediterranean fever, one of the most common causes of such familial amyloid.) Despite the common structural features of the amyloid fibrils, differences in their binding of Congo red and certain metachromatic dyes had suggested that there might be differences among amyloids, but biochemical investigations had been hampered by the lack of a suitable solvent that could quantitatively extract the native material.

Persuaded by his wife, Franklin asked Pras to solubilize some of the protein. Pras's inexperience with protein physical chemistry came to the rescue! Instead of using buffered isotonic saline to extract the homogenate as a more sophisticated protein chemist might have, he used distilled water. Remarkably this worked and he was able to obtain in high yield a protein that had all the anticipated characteristics of amyloid. In that first paper, which is still cited more than thirty years later, Pras and his colleagues reported on the amyloid's solubility characteristics, physical properties, amino acid and carbohydrate composition, and stoichiometry of binding of Congo red (a useful quantitative assay). The electron microscopic characteristics of both the water-soluble and saline-precipitated material, chemically cross-linked with glutaraldehyde, were also detailed. Despite this considerable progress, they noted that many fundamental questions remained: the relationship of amyloids from different disease states and even its homogeneity in a single disease; the nature of amyloid's interaction with other tissue components; and the possibility of associated γ-globulins.

Earlier studies from Osserman's laboratory had noted the association of immunoglobulin proteins with amyloid deposits,

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

but it was not until 1971 that George G. Glenner and his colleagues at the National Institutes of Health showed that certain amyloids are themselves composed of the variable region of light chains. It soon became apparent that other amyloids were not, and the amino acid sequence determined by the New York University group on amyloid fibrils from a patient with familial Mediterranean fever revealed a protein that is still referred to as serum amyloid precursor or serum amyloid A (SAA).

Franklin and Zucker-Franklin were particularly intrigued with the mechanisms responsible for the cleavages leading to generation of amyloid from a variety of proteins, particularly SAA. They uncovered evidence that the proteases might be on the plasma membrane of mononuclear leukocytes rather than inside the cell, an idea for which there was virtually no precedence. It is a testimony to their prescience that not only has the general subject of surface proteases become an important one, but that the specific question of how amyloid is generated by proteases is now of intense interest, especially with respect to the amyloid associated with Alzheimer's disease and various spongiform encephalitides. It would also gratify Franklin that the whole subject of pathogenic fibrillization is now becoming intimately related to the most fundamental investigations of protein folding.

During the fifteen years after his first publication on amyloid, Franklin authored or co-authored some forty papers on amyloid—more than he wrote on any other single subject except for his papers on various aspects of immunoglobulin structures per se. Seven of his last ten papers, some published posthumously, were on amyloid, including the characterization of a prealbumin mutant as the lesion in a heredofamilial amyloidosis syndrome, the last paper in his bibliography of almost 250 publications.

Before reviewing some of Ed Franklin's other professional

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

activities and his approach to research, it is appropriate to consider Franklin as an individual. This aspect of Franklin is not easily apprehended. He was generally a quiet and private man lacking the more overt exuberance of his wife.

In his younger years, the Zucker and Franklin (then Freundlich) families lived across from each other in Berlin, but their paths parted upon emigration from Germany, with the Zuckers secreted away in the Netherlands throughout World War II. By extraordinary coincidence the parents met again while on vacation in Lake Placid in 1952 and discovered that they again lived virtually across from each other in Forest Hills, New York. Ed and Dotty's reacquaintance in New York was in the context of a blind date, which she remembers as “a bore.” Nevertheless, a relationship developed, although Dorothea's friends were puzzled at her attraction to this taciturn individual, who was such a wall-flower at parties. The mutual attachment blossomed and matured into a very close and lively marriage. Indeed they had to arrange separate offices for themselves in their home because when together their constant conversation prevented them from getting their work accomplished.

Comments of his former colleagues refer to Franklin's “extreme conscientiousness and hard work” and Dennis Stanworth, whose family became close to the Franklins, writes that coming from England he was bemused by Franklin's dynamism in his pursuit of both laboratory and clinical investigations. It wasn't only research that occupied Ed. He and Dorothea shared broad cultural interests, and he was a devoted father to his daughter, Deborah. In 1957 the Franklins purchased a farm in the Berkshires and his friends remember with fondness weekends spent there. The Franklins had an extensive apple orchard, and a gift of some of the fifty gallons of the cider they would produce annually was a cherished memory of the fortunate recipients.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

The photograph that accompanies this memoir shows Franklin looking full-face into the camera. Perhaps having now familiarized myself somewhat more with this man, I read into that picture more than others might, but I think it admirably captures his high intelligence, sophistication, skepticism, and puckishness.

Returning to Franklin's professional achievements, after his apprenticeship in the Kunkel laboratory he was awarded a coveted senior investigatorship by the Arthritis Foundation in 1958, and he moved from Rockefeller to New York University, one of the world centers for biomedical research in general and probably at that time the premier center of immunological research. Under Currier McEwen, the director of the interdepartmental Rheumatic Diseases Study Group and the erudite Lewis Thomas, newly appointed chief of medicine, the young faculty were protected from excessive routine duties and assured ample time for research. Five years later, Franklin was appointed career scientist of the New York City Health Research Council and after another five years was a full professor of medicine, an attending physician at University Hospital, and had succeeded McEwen. In 1973 he was appointed director of Irvington House Institute, a privately endowed research enterprise originally focused on research and treatment of rheumatic fever. Franklin had an informal and non-interfering approach to management, and likely this contributed to his effectiveness as an administrator.

He noted in his autobiography that his participation in numerous editorial boards, study sections, and councils of the National Institutes of Health and advisory boards of several private research foundations “managed to occupy some of [his] few leisure hours.” He also served with professional societies, and was elected to the Council of the

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

American Society for Clinical Investigation, and served as its president in 1972-73.

It might be supposed that Franklin's style of research would reflect his virtually Prussian early upbringing, tending towards the rigid and excessively formal. (His wife remembers Ed's mother as somewhat pedantic.) Perhaps in reaction, Franklin harmonized his approach more with the expressive, innovative aspect of German culture characteristic of that period of the twentieth century, particularly in the arts. His wife described it this way: “Our attitude vis-à-vis scientific research? Nothing was ever planned! Neither by Ed, nor by me. If data looked intriguing, they were pursued . . . Repetition of experiments and appropriate controls were kept to a minimum, their number being entirely dictated by the need for publication, if possible in prestigious journals.”

In his presidential address at the sixty-sixth annual meeting of the American Society for Clinical Investigation in 1974, Franklin approvingly quoted Jaques Monod's comment that “rational intelligence is an instrument of knowledge especially designed for mastering inert matter but utterly incapable of apprehending life's phenomena.” He added in his own words that “instinct and intuition serve as additional tools in our quest to answer questions in the realm of living matter. . . .”

These remarks were made in the context of his fears about the trends he saw towards centralized direction of federally funded biomedical research. He noted that those who had not had an opportunity to participate in research “do not always appreciate the crucial importance of the intangible factors. Straight-jacketed centrally directed programs would leave no room for these essential ingredients.” He closed with a plea that we continue to support the individual as an

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

essential component in the researcher enterprise, "giving everyone the opportunity to evolve his own style."

In his autobiographical essay, referred to above, he reiterated similar concerns. "It is my hope that the pressures of fiscal restraints and the tendency to emphasize directed research will not inhibit investigator-initiated research in years to come. Freedom to choose a problem and follow up exciting leads is the surest way to success. No committee or administrator, no matter how wise, can anticipate important leads and approaches in biology."

I AM GRATEFUL TO Dorothea Zucker-Franklin for sharing some of her reminiscences with me, as well as for providing me with copies of several eulogies and obituaries, and for Lalezari Parviz's concise, detailed "In Memoriam for Edward C. Franklin" *(Montefiore Medicine* 7[1982]:78-81). Letters from several of Franklin's colleagues were also helpful. Candace Canto of the NIH library kindly performed a citation analysis of some of Franklin's most influential papers. Paul Plotz made helpful suggestions on a first draft.

NOTES

1. Dorothea Zucker-Franklin pioneered the application of electron microscopy in hematology and frequently collaborated with Franklin. Her early career (through the mid-1980s) is described in M. Wintrobe, *Hematology, the Blossoming of a Science,* pp. 468-69, Philadelphia: Lea & Febiger, 1985.

2. E. F. Osserman and K. Takatsuki. *Medicine* 42(1963):357-84.

3. W.J. Dryer and J. C. Bennett. *Proc. Natl. Acad. Sci. U. S. A.* 54(1965):864-69.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

SELECTED BIBLIOGRAPHY

1956

With H.J. Müller-Eberhard and H. G. Kunkel. Two types of γ-globulin differing in carbohydrate content. *Proc. Soc. Exp. Biol. Med.*93: 146-50.

1957

With H. G. Kunkel. Immunologic differences between the 19S and 7S components of normal γ-globulin. *J. Immunol.*78: 11-18.

With H. R. Holman, H. J. Müller-Eberhard, and H. G. Kunkel. An unusual protein component of high molecular weight in the serum of certain patients with rheumatoid arthritis. *J. Exp. Med.*105: 425-38.

1959

With H. G. Kunkel and H.J. Müller-Eberhard. Studies on the isolation and characterization of rheumatoid factor. *J. Clin. Invest.*38: 424-34.

1961

With D. Stanworth. Antigenic relationships between immune globulins and certain related paraproteins in man. *J. Exp. Med.*114: 521-33.

1963

With B. Benacerraf, Z. Ovary, and K. Bloch. Physicochemical properties of guinea pig antibodies. I. Electrophoretic separations of two types of antibodies. *J. Exp. Med.*117: 937-47.

1964

With J. Lowenstein, B. Bigelow, and M. Meltzer. Heavy chain (7S γ-globulin) disease. A new clinical entity. *Am J. Med.*37: 332-50.

1966

With M. Meltzer. Cryoglobulinemia—a study of twenty-nine patients. I. IgG and IgM cryoglobulins and factors affecting cryoprecipitability. *Am. J. Med.*40: 828-36.

With M. Meltzer, K. Elias, R. T. McCluskey, and N. Cooper. Cryoglobulinemia—a clinical and laboratory study. II. Cryoglobulins with rheumatoid factor activity. *Am. J. Med.*40: 837-56.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1968

With M. Pras, M. Schubert, D. Zucker-Franklin, and A. Rimon. The characterization of soluble amyloid prepared in water. *J. Clin. Invest.*47: 924-33.

1969

With M. Pras, D. Zucker-Franklin, and A. Rimon. Physical, chemical, and ultrastructural studies of water-soluble human amyloid fibrils. Comparative analyses of nine amyloid preparations. *J. Exp. Med.*130: 777-96.

1970

With F. A. Forte, F. Prelli, W. J. Yount, L. M. Jerry, S. Kochwa, and H. G. Kunkel. Heavy chain disease of the gamma (gamma M) type: Report of the first case. *Blood*36(2): 137-44.

Heavy-chain diseases. *N. Engl. J. Med.*282: 1098-99.

1971

With B. Frangione. The molecular defect in a protein (CRA) found in gamma-1 heavy chain disease, and its genetic implications. *Proc. Natl. Acad. Sci. U. S. A.*68: 187-91.

1972

With C. Wolfenstein-Todel and B. Frangione. Structure of immunoglobulin A. II. Sequence around the hinge region and labile disulfide bonds of an immunoglobulin A2 myeloma protein. *Biochemistry*11(21): 3971-75.

1973

With M. Levin and M. Pras. Immunologic studies of the major nonimmunoglobulin protein of amyloid. I. Identification and partial characterization of a related serum component. *J. Exp. Med.*138: 373-80.

1974

The individual, science and society (presidential address)*J. Clin. Invest.* 53: 1755-60.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1975

With J. B. Adlersberg and B. Frangione. Repetitive hinge region sequences in human IgG3: Isolation of an 11,000-dalton fragment. *Proc. Natl. Acad. Sci. U. S. A.*72: 723-27.

1977

With Y. Levo, P. D. Gorevic, H. J. Kassab, and D. Zucker-Franklin. Association between hepatitis B virus and essential mixed cryoglobulinemia. *N. Engl. J. Med.*296: 1501-1504.

With T. E. Michaelsen and B. Frangione. Primary structure of the “hinge” region of human IgG3. Probable quadruplication of a 15-amino acid residue basic unit. *J. Biol.Chem.*252: 883-89.

1978

With G. Lavie and D. Zucker-Franklin. Degradation of serum amyloid A protein by surface-associated enzymes of human blood monocytes. *J. Exp. Med.*148: 1020-31.

1979

With B. Frangione. Correlation between fragmented immunoglobulin genes and heavy chain deletion mutants. *Nature*281: 600-602.

1980

With P. D. Gorevic, H. J. Kassab, Y. Levo, R. Kohn, M. Meltzer, and P. Prose. Mixed cryoglobulinemia: Clinical aspects and long-term follow-up of 40 patients. *Am. J. Med.*69: 287-308.

With G. Lavie and D. Zucker-Franklin. Elastase-type proteases on the surface of human blood monocytes: Possible role in amyloid formation. *J. Immunol.*125: 175-80.

1982

The heavy chain diseases. *Harvey Lect.*78: 1-22.

1983

With M. Pras, F. Prelli, and B. Frangione. Primary structure of an amyloid prealbumin variant in familial polyneuropathy of Jewish origin. *Proc. Natl. Acad. Sci. U. S. A.*80: 539-42.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

CLIFFORD GROBSTEIN

July 20, 1916–September 6, 1998

BY NORMAN K. WESSELLS

DISCS OF TRANSPARENT, exquisitely thin filters, branching embryonic salivary glands and kidneys, collagen fibers and extracellular glue—the artifacts of Clifford Grobstein's science. New science buildings for research and teaching, new ways to organize biological knowledge for teaching, reorganization of biological and medical institutions, and the recruitment of the first faculty to a new medical school—products of Clifford Grobstein as an academic leader and administrator. Development of public policy on assisted human reproduction, on recombinant DNA usage, and on other controversial topics where science and society meet—contributions of Clifford Grobstein as biomedical ethicist.

These diverse landmarks trace the career of Clifford Grobstein, regarded by many as the preeminent bridge between classical embryology and late twentieth-century developmental biology. Grobstein as scientist made the key discoveries that implicated extracellular materials as essential elements during embryonic induction processes. He made the startling observation that different developing cell populations from embryos could interact across membranous filters that prevented direct cell-to-cell contact. And, he defined the specificity rules for inductive interactions: which

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

combinations of epithelium and mesenchyme (the two kinds of interacting embryonic tissues) would result in morphogenesis or cellular differentiation. Those results, amplified over 40 years by new techniques and molecular biology, have established the importance of the extracellular materials and matrix, cell adhesion molecules, and extracellular enzymes that modify those materials in a variety of normal developmental processes, as well as in cancer metastasis, wound healing, and related biological processes.

Grobstein was a pioneering advocate in reorganizing the way contemporary biology is taught and how university life science departments are organized. His enormous intellectual capacity to think beyond his scientific discipline, coupled to a palpable integrity and trustworthiness, made him a successful builder and recruiter of faculty and programs in his universities and in a new medical school at the University of California, San Diego. Grobstein had a truly deep social conscience and awareness and brought his analytical and problem-solving skills to bear as a biomedical ethicist on pressing issues generated by scientific advances of the past 30 years, as he contributed wisdom and insight, no matter how complex or controversial the topic might be.[1]

Clifford Grobstein's career, interrupted near its start by the Second World War, is an odyssey of success in science and service to students and society that stretched from Bethesda (the National Cancer Institute) to Palo Alto (Stanford University) to La Jolla (the University of California, San Diego). Grobstein personifies the group of brilliant, creative American scientists who emerged from the Depression and war years, lived and worked in such marvelous communities and universities, and who transformed the sciences, our country's universities, and society itself.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

GROWING UP—BRILLIANCE SEEN EARLY

Clifford Grobstein was born in New York City on July 20, 1916, the son of Aaron "Harry" Grobstein and Birdie Grobstein. Fern, a sister, and Richard, a brother, shared the family adventures that included two years in Colorado Springs as father Harry recuperated from tuberculosis. Cliff attended what would later become Bronx High School of Science, where at one point he tested above the "genius" level, prompting the principal to call in Birdie to inquire why Cliff was not doing better at school. Graduation was at the age of sixteen and enrollment at City College of New York followed. By his junior year Cliff had decided on biology and graduate school; it was the practice of many undergraduate biology majors, most of whom were destined for medical schools, to walk home with a particularly friendly CCNY professor to talk and obtain a letter of recommendation to medical school. When asked if that was what Grobstein wanted, Cliff responded, no, he wanted a letter for graduate school so that he could become a professor. The response was, "Well, that's fine, but you know there are only six Jewish biologists in the country and I'm one." The letter and Cliff's credentials worked. He headed west for Berkeley, and on the way visited the University of California, Los Angeles (by that time his parents had moved to Los Angeles), where wind of his remarkable record had surfaced. He was induced to go there to work in endocrinology on the pituitary and hormones.

This early personal history reflects a process that transformed American science in the mid-century: highly intelligent, creative Jewish children in New York and other major cities became educated in science, and then in the postwar years joined the faculties of the major universities and research institutes. This changed the cultures of those places

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

even as the excellence of the new science brought Nobel Prizes, elections to learned societies, and other forms of recognition.

THE EARLY RESEARCH YEARS—BACKGROUND TO SUCCESS

The late 1930s witnessed many searches for experimental systems in which defined chemicals exerted clearly interpretable actions on whole embryos or other developing tissues. Clifford Grobstein's earliest published experiments, stemming from the UCLA graduate student days, focused on endocrine organs and, in particular, how the thyroid gland hormone, thyroxine, and androgens such as testosterone affected anal fin regeneration and morphogenesis in fishes. Here at the very start of his career were two of the ingredients central to his later seminal studies in mammals: diffusible causative agents and the process of morphogenesis, the phenomenon by which populations of cells form complex structures.

The hormone studies continued at Oregon State University in the zoology department and then were resumed after World War II at the National Cancer Institute in Bethesda. Aviation physiology was Cliff's wartime job and focus while he served in the U. S. Army Air Force between 1943 and 1946. He was in a small group of scientists identified by Detlev W. Bronk (then coordinator of research for the Office of the Air Surgeon and later president of Johns Hopkins University and Rockefeller University) that was in a special category of the military doing war-related research. During the late 1930s, early 1940s, and the first war years, Charles A. Lindbergh and others were undertaking the first flights above 40,000 and 50,000 feet, so it became important to the Allied war effort to discover how the human body reacted to high altitudes, oxygen deprivation, and the high G forces being experienced in the new fighter planes. Cliff

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

retained a lifelong passion for flying from those war days. During the 1970s and 1980s, he and his colleague and friend Harold J. Simon (professor and chief, Division of International Health and Cross-cultural Medicine, University of California, San Diego, Medical School) often flew in one of Mr. Piper's monoplanes along the La Jolla coast looking for whales or over the rugged hills around San Diego, enjoying the art of nature.

Beginning in 1948 Grobstein entered what I view as the real preparative years for his major research focus. He realized the importance of using simplified experimental systems rather than intact organisms or even embryos, and so employed intra-ocular grafts (a procedure in which the anterior chamber of the adult mouse eye is used as a culture chamber). Even that in vivo procedure presented problems in interpretation of results, so he explored various culture techniques. He also began to think hard about determination of embryonic cells, the process in which developing cells become committed or stable toward a subsequent developmental fate. He and his friend and collaborator Edgar Zwilling of Brandeis University observed different patterns of cell maturation when variously sized pieces of early chick embryo blastoderms were cultured. They carefully distinguished the difference between the determined state of a tissue and that of its component cells. What may appear to be a determination to form, say, neural tissue may lie more in the pattern of cell interactions than in the cells themselves. It is no surprise, therefore, to see in the same year as that work (1953) the four papers that established Grobstein's eminent position in American biology.

THE CORE YEARS—ACTION AT A DISTANCE

Grobstein switched his research focus to mammalian embryos and to developing internal organs that had obvious,

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

easily identifiable forms of morphogenesis, namely, characteristic branching of sheets of cells called epithelia into hollow, tree-like structures. He recognized that in vitro culture methods were essential to study experimentally such organs. Next, he chose several organs to investigate because of earlier observations of E. Borghese in Italy, employed a number of the culture techniques of Honor B. Fell at the Strangeways Laboratory in Cambridge, England, and used enzyme solutions perfected by Aaron A. Moscona at the University of Chicago to separate the epithelial and mesenchymal components of the tiny organs. Cliff was always generous in recognizing and thanking these and other scientists for their discoveries and techniques that he used in his own research program.

Some major conclusions of the 1953 quartet are that epithelium of the embryonic mouse submandibular salivary gland will only carry out morphogenesis (branching) if it is in proximity to its own normal mesenchyme. Similarly, epithelium of the metanephric kidney requires its enveloping mesenchyme to branch. Furthermore, he found that there is specificity in the interaction between epithelial and mesenchymal cell populations, so that salivary mesenchyme will not support kidney morphogenesis or kidney mesenchyme salivary morphogenesis. Grobstein also discovered that not all systems are so specific in their requirements; for instance, kidney mesenchyme will respond to salivary epithelium by forming proper kidney tubules that, in an intact embryo, would become the tubular portion of nephrons (the sites where urine initially forms). Elegant, simple experimental design, employment of combinations of techniques in new ways, and parsimony of interpretation mark these early papers that brought new visual and analytical clarity to the process of organogenesis. These papers, more than any others,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

began to establish the phenomenon of epithelio-mesenchymal interaction as a principle of development.[2]

Even more followed in that same year. Grobstein placed salivary epithelium on one side of a newly available kind of porous filter (special types of very thin Millipore filter) and mesenchyme cells on the other side, and behold, the epithelium branched! A few years later we knew that Moscona's enzyme procedures used to separate epithelium from mesenchyme really did remove 100% of the mesenchyme cells, as well as the collagen and basal lamina materials to which epithelial cells adhere. In 1953, before electron microscopy was used to view such developing systems, the induction of morphogenesis across a filter in the apparent absence of direct cell-to-cell contact surely implied the existence of diffusible causal agents, that is, "action at a distance." The transfilter results in combination with the other 1953 papers were exciting indeed. A new door appeared to be opening for the investigation of embryonic induction. In the words of William Telfer,[10] Grobstein's experiments "seemed to be getting mechanisms of induction down to an experimentally practical form."

Experiments published by Grobstein over the following 17 years built on the 1953 foundation. A search for the kinds of extracellular materials involved in embryonic tissue interactions focused on collagen and glucosamine-containing polysaccharides, as well as investigation of effects of enzymes (as, collagenase) that degrade such materials. A collaboration at Stanford with electron microscopist Frances L. Kallman was particularly important to Cliff, and defined the ultrastructural features of cell interaction across Millipore filters, as well as the distribution of isotopically labeled materials as morphogenesis took place. Undergraduate research students, graduate students, postdoctoral fellows (the author was one of the first), and a stream of more senior

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

scientists worked with Grobstein at Stanford and later at the University of California, San Diego, to extend the Grobstein-type studies to a variety of embryonic systems. Similar such studies ensued in laboratories around the world. Skin, hair, teeth, mammary glands, pancreas, thyroid, cartilage—only a partial list of cases in which distinct cell populations interact to stimulate the morphogenesis of cell populations or the differentiation of component cells. Of course, advances in electron microscopy, molecular biology, and biochemistry occurred during those years, so that the sophistication of analysis and kind of experimental questions evolved dramatically. Grobstein's initial sets of questions and answers were the foundation, and a number continue to be cited prominently in literature as the millennium turns.

Two examples from the Grobstein laboratory give perspective on Cliff. In 1962 Cliff worked with Stanford undergraduate Nicholas Golosow and showed that the differentiation of mammalian pancreatic epithelial cells (ones that synthesize and secrete such digestive enzymes as amylase and trypsin) was dependent on the nearby mesenchyme cells. The following year William J. Rutter, a future member of the National Academy of Sciences, worked in Grobstein's laboratory along with me, an assistant professor in the department. Thus began a series of experiments that defined biochemically and ultrastructurally the earliest stages of cell differentiation of exocrine and endocrine pancreas. I still recall the long exchanges with Rutter, Grobstein, and our colleagues and how Grobstein was open to the importance and impact of developing and using the supersensitive assay procedures that Rutter, as biochemist, knew were essential if we were to understand the earliest stages of differentiation. Parenthetically, the student dishwasher and lab assistant for Rutter that year was undergraduate Edward E. Penhoet, later a graduate student with Rutter, professor of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

biochemistry at the University of California, Berkeley, and co-founder with Rutter of the Chiron Corporation. It was no accident that such bright people gathered about Grobstein, and their subsequent successes remain as testimonies to the Grobstein impact.

A second thread of scientific history involves Merton R. Bernfield, a research fellow with Grobstein at the University of California, San Diego. After participating at the National Institutes of Health in early studies of the genetic code, Bernfield learned about tissue interactions from Grobstein and began to carry out detailed analysis of the biochemistry of the interface between interacting mesenchyme and epithelium. Later at Stanford Medical School and Harvard Medical School, Bernfield and his collaborators studied in unprecedented exactness the deposition and turnover of extracellular materials in the developing salivary glands pioneered by Grobstein. Included were the very first observations of localized effects in morphogenetic systems of what we call today matrix metalloproteases—enzymes that can degrade such substances as collagens, laminin, fibronectin, nidogen, and other stabilizing agents to which the integrin cell surface adhesion molecules of epithelial cells are linked. Others have extended these studies to developing mammary glands and other systems as a general explanation has emerged of the processes that Grobstein observed through a much more primitive lens in 1953.

SCIENTIST AS TEACHER

Cliff was, in the words of Michael Flower, “a superb teacher in both the classroom and laboratory. I arrived at Stanford as an undergraduate headed for a career in biochemistry. However, after the first meeting of Cliff's embryology class (“developmental biology” was not yet the name for this field) in which he introduced development by an accounting of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the cellular slime mold, I was hooked."[3] Superb organization, logical presentation, both the forest and the trees in useful measure, and the perspectives of a deep mind came across in those lectures as I recall them. In his laboratory, Cliff taught his postdocs, graduate students, and undergraduate research students one on one. Everyone was expected to learn and share in every phase of the day-to-day labor (for instance, by working in the mouse colony and identifying newly impregnated female mice early on lonely Sunday mornings) and the material infrastructure underpinning the laboratory and the experiments. Each of Cliff's students regularly met alone with him in his office to review research data, progress since the last meeting, and ideas about the next experiments. Getting ready for those meetings was serious and sometimes daunting business, for one could be quite sure that every stone would be turned and that all alternative explanations of experimental results would be chewed over before the next experiments were planned. Cliff let every student see that good science is hard intellectual work that must be pursued with utmost objectivity and integrity.

The weekly lab meetings were enlivened by the presence of so many fine visiting scientists who came to Bethesda, Palo Alto, or La Jolla to be with Cliff. E. Zwilling, L. Saxen, W. J. Rutter, F. H. Wilt, K. Kratochwill, W. H. Telfer, M. R. Bernfield, B. Unsworth, and many others came. Some of those visits spawned lifelong friendships; the Lauri Saxens from Helsinki and the Grobsteins from La Jolla were especially close. Some visitors worked closely with Cliff on tissue interactions and cell differentiation and morphogenesis. Others were free to pursue lines of experimentation they brought to the Stanford basement laboratory or ones that emerged in the conversations with Cliff. They all had experiences like those of Fred Wilt, professor at the University

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of California, Berkeley: “My clearest and dearest memories of Cliff are those daily meetings, you (the author), he, and I had . . . in the basement in Palo Alto. I don't know who made the coffee, but I think we three spent a lot of time in front of the blackboard in the hallway in very stimulating discussion about tissue interactions and other such matters. His personal generosity to me was just incredible, and I shall never forget it . . . as he welcomed me to his lab, let me go my own wayward way, and . . . supported me to the hilt in my attempt to learn something about how tissues do interact”[4] (Wilt worked that year on interactions in blood islands of chick embryos). Every visitor participated in the personal meetings with Cliff and in the weekly lab discussions, where results, progress, and plans sank or swam after intense questioning and debate.

Grobstein's fundamental generosity and concern for the well-being of his students and scientific collaborators was reflected in the authorship of publications from his laboratory. Dozens of publications stemming from work in the Grobstein laboratory bear only the name of a graduate student, postdoctoral fellow, or senior visitor. Cliff added his name to a paper only when he knew that he had been a major contributor of ideas, hands-on experimentation, writing, and editing. He got real pleasure from seeing his younger associates establish their independence and careers, and knew that independent publication without the added name of a heavyweight in developmental biology would help that process.

AN IMPORTANT SIDE PATH—REORGANIZING BIOLOGY

Even as Grobstein and his associates were engaged in fruitful research, Cliff took time to help reshape the American biological community and its teaching. The early 1960s were, of course, a time of ferment and challenge in biology,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

as new techniques and results from biochemistry, electron microscopy, and biochemical genetics began to force biologists to take new ideas and discoveries into account. Most university and college departments were organized around kinds of organisms—zoology, botany, microbiology—and the undergraduate curriculum each department offered focused on those animals or plants or microbes. Embryology, Grobstein's field, was just beginning to confront new views of cells and organelles and how genes might play roles in developing embryos. As Cliff recognized, the very character of the biological community would be changed by advances at the cell and molecular levels; the new cadre of scientists trained in physics, chemistry, and mathematics who were studying biological problems; and the vast increase in federal funding of biomedical research.

Grobstein took a leadership role in stimulating life scientists to think differently about their science. He was one of the key people espousing the new “levels of organization” approach to teaching undergraduates and, more importantly, to thinking about then contemporary biology. He articulated the need for a multilevel research approach and simultaneous study at molecular, cellular, and supra-cellular levels. He and David Goddard brought together a diverse group of talented researchers to write three volumes published by John Wiley & Sons: David Nanney and Herbert Stern on cell biology, Donald Kennedy and William Telfer on organismal biology, and Robert MacArthur and Joseph Connell on population biology. He proselytized by writing in *The American Biology Teacher* and *American Scientist* about the levels of organization approach and defined the concept of a core curriculum as the essential knowledge common to all subdivisions of a science. Cliff recognized that the old-time religion would be hard to overcome and that most college and university teachers would find it hard not

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

to teach what they learned when they were young. As a result, new material, to the extent it was covered at all, was being pushed into the upper-division advanced courses and was not incorporated as basic foundational material. In 1966, in a major presidential address before the American Society of Zoology, he said forthrightly that the biology of animals was no longer an optimal common interest for a scientific group (or professional society); the organism would be the better focus for that group, and the equivalent for the botanists et al. Unsettling words to the old-guard zoologists present! Even as he called for changes in the old, Cliff was helping to spawn the new: he was one of the early advocates and participants in the formation of the new Society for Cell Biology.

Cliff's experience in academic administration by that time (department chairman at both Stanford and at the University of California, San Diego, as will be described below) let him see firsthand how department structures were a key to adapting to the new kind of biology. It was only later, of course, that cell biology and structural biology departments would emerge from anatomy in medical schools, and that the terms “molecular biology” and “developmental biology” would assume special connotations and corresponding legitimacy in university organization and curricular content. Grobstein argued strongly that it was the responsibility of faculty in research-intensive universities to take the lead during the early and mid-1960s in defining these new levels of organization and patterns for teaching, and he argued that “joint performance of faculty in universities of research and teaching is nowhere more important than in defining the (new) core curriculum.” He called such activity a primary creative function of faculty.

Cliff's perceptiveness is nowhere better illustrated than in his arguments that combined approaches of research at

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

several levels would be required to crack some of the knottiest biological problems. To do this, members of these interacting sciences ought to share a common multilevel training, and he recognized that a person at any one level is no better equipped to formulate powerful, trans-level concepts, or to appreciate the need for them, than persons at other levels. These thoughts were prescient harbingers of what was to come in the following 30 years as molecular biologists, cell biologists, geneticists, physicists, and scientists from other disciplinary backgrounds teamed together for multifaceted investigations of the embryos and cells that were Cliff's main love in science.

A NATURAL CULMINATION—SCIENCE AND SOCIAL VALUES

Grobstein's administrative positions as department chairman and medical school dean, plus his involvement in science policy and advisory committees, necessarily diverted attention from laboratory science. Beginning in 1976 and continuing through the rest of his career, Cliff published a series of books, articles, and public commentaries in areas where science, ethics, and the public welfare interweave. Recombinant DNA policy and guidelines and the whole complex issue of in vitro fertilization, human embryos, and assisted reproduction were two topics he studied at length and which he could interpret cogently for the public.

The so-called self-policing by scientists of recombinant DNA procedures began with the Asilomar conference in 1973. Guidelines governing experiments with different levels of possible risk were promulgated by the National Institutes of Health in 1976; but the public debate continued to rage as some local governments entered the science policy area and some legitimate scientists expressed grave worries about untoward consequences of escaped engineered organisms. Grobstein's lengthy, careful analysis published in

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Scientific American (1976) provided a clear view of the way Cliff attacked complex controversial subjects. Scientific thoroughness and accuracy, fair presentation of different sides of issues, and balanced argumentation were his hallmarks. Most importantly, he took positions and recommended rational procedures that were practical, did not go beyond what was needed or could be delivered, and that safeguarded the public where safeguarding was warranted. Cliff's sensitivity to and understanding of the public's uneasiness with science, which it does not understand, was extraordinary. Instead of expressing impatience, his effort was to educate and guide in a responsible way, for he saw in this case that genetic engineering was truly a momentous advance, one that marked a beginning of the “age of intervention” in biomedicine.

Typical of Cliff's service in Washington, D.C., was his chairmanship of the Committee on Diet, Nutrition, and Cancer, which in 1982 issued the first clear summary of the linkage between diet and cancer. The evidence that dietary fat intake increases the risk of breast, prostate, and colorectal cancers came from searching studies of worldwide data and provided compelling arguments that lifestyle, specifically diet, correlates with cancer incidence. Cliff was a voice of reason and authority, as such controversial conclusions contradicted the 1980 National Academy of Sciences' Food and Nutrition Board report and made the American Meat Institute and similar vested interests very unhappy. Cliff had faith that many people would respond to such information by changing their own behavior; his message was strong and simple: “What we eat does affect our chances of getting cancer, especially particular types of cancer. This is . . . good news because it means that by controlling what we eat we may prevent such diet-sensitive cancers.”[5]

Grobstein's depth and breadth of understanding of mam

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

malian development provided ideal perspective to the issues of in vitro human fertilization, defining "personhood" qualities of human embryos, assisted human reproduction, and abortion. Just a year after Louise Joy Brown, the first human born after fertilization of the human egg outside the mother's body, was born in 1978, Grobstein summarized the field and the practical, legal, and ethical issues it posed for society in another *Scientific American* article. He provided one of the deepest explorations of human personhood: just when does the developing human embryo go beyond being a developing group of cells, tissues, and organs, and attain a state that physicians, scientists, parents, the lay public, or our legal system call a human being? In 1985 Grobstein worked hard to stimulate the National Science Foundation to support a formal study of these aspects of human reproduction. The result was a series of articles and books addressed to the lay public, the medical research and practice communities, and the government and foundations involved in regulating or supporting such new science and medicine. Important ones were published with co-authors M. Flower (who, as an undergraduate research student with Grobstein at Stanford, had published on tissue induction problems) and J. Mendeloff and were addressed to the medical community in several papers, among which is one in the *New England Journal of Medicine,* which treats the vexing issues raised by the storage of frozen human embryos. What rights do such embryos attain, if any, since they could apparently be stored indefinitely, perhaps well beyond the reproductive capacity or even lifetime of the original parental donors of egg and sperm? Grobstein focused his scientific understanding and argumentative abilities on the new powers and processes that late twentieth-century science and medicine was giving to society, and which posed complex philosophical, ethical or practical

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

questions. All the papers and books are marked by specific recommendations—to scientists, to physicians, to the public and its representatives—for Cliff wanted to help solve these controversial questions, not just critique them. His attitude was stated succinctly in response to a charge that to ask science to define human life is a travesty: "Not only is it not a travesty, it is precisely what science should do to assist any public decision making that involves substantive scientific content." The American Publisher's Association recognized the excellence of Cliff's writing in these areas with its award for best publication of the year in 1989 for *From Chance to Purpose, an Appraisal of External Human Fertilization.*

ACADEMIC LEADER AND MEDIATOR

Grobstein's personal qualities as a large, room-filling presence and person marked him for leadership roles, but they would come only after several years as professor at Stanford University. During the late 1950s and early 1960s, it was the newly arrived easterners Charles Yanofsky from Western Reserve University and Cliff from the NIH who were shocked by how little concern there was among older department faculty about adding new faculty who could become outstanding researchers. Applicants for graduate study were selected on the basis of their teaching assistantship credentials, not on their interests in research or research careers. Yanofsky recalls that Cliff's was the strongest voice for change, as the two argued vigorously for different criteria for hiring and a refocusing of the Department of Biological Sciences toward strong research appointments to the faculty. This was the department, after all, where George Beadle and Edward Tatum had done their Nobel Prize experiments on genes and enzymes and where C. B. van Neil had elucidated the key chemical principles of photosynthesis. The

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

new kinds of biology made possible by advances in biochemistry, biophysics, and neurobiology were absent until Yanofsky and Grobstein worked hard to establish the concept that topflight young investigators using the new techniques could also be outstanding undergraduate and graduate teachers. Grobstein "deserves credit for pointing us in the right direction," says Yanofsky about those times.[6]

Grobstein played an analogous role in the broader Stanford setting, where fractures were beginning to appear in the new center of excellence created by the recruitment of Arthur Kornberg, Paul Berg, Joshua Lederberg, David Hogness, Dale Kaiser, Yanofsky, Grobstein, and others. In the words of participant Melvin Cohn, who spent the rest of his career at the Salk Institute, Stanford's Garden of Eden was becoming a battlefield about who owned the apple tree. For instance, medical and premedical students revolted against the subject matter the new kinds of faculty were teaching. Two intransigent cultures were clashing on classic grounds, utilitarian-driven versus curiosity-driven research and teaching.

Cohn's description provides a perfect image of Grobstein: "It was at a faculty meeting where the collective creativity was failing to cope with the problem that I first met Cliff. He was a handsome figure as he reflectively chewed on his empty pipe. He dominated the meeting when he good-naturedly admonished us to stop defending self-serving values. One would think that a committee of remarkably 'creative' people, a number of whom would be Nobel Prize winners, would have been able to cope, in a meaningful way, with this complex issue." Cohn continues, "Cliff immediately stood out as being special. He showed us by example that there is a difference between 'creativity' and 'intelligence.' The ability to manipulate objective knowledge in novel and unexpected ways (my definition of creativity) is

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

not equivalent to the ability to deal with moral and esthetic values in a Socratic way (my definition of intelligence). Everyone at the meeting was creative; Cliff, in addition, was intelligent. He was unique in that he brought a fresh understanding of human nature, as an evolved part of the biological world, to bear on his thinking about values."[7] Cohn goes on to remark that Cliff "had a quiet way of making you feel guilty about your irrationalities. He distinguished strongly between being erroneous (with which he could deal) and being irrational (with which he could not deal). At one faculty meeting he brusquely said to a colleague, "there is no way to refute an absurdity."

Grobstein, as I knew him, was surely a curiosity-driven scientist and it was not easy for him to gradually shift focus and time from laboratory bench to desk and meeting room, as time after time he was asked to chair committees and negotiate crises. Cliff was truly excited by the new discoveries in developmental biology, and he recognized that this was an area of science whose time had come. Cliff also had a special sense of social responsibility as well as a gift for dealing with people, policy, and controversy. The administrative path began in a formal way when Cliff assumed the chairmanship of the biological sciences department at Stanford in 1963, where he soon played a pivotal role in convincing the university administration that expansion and modernization of the department on the main campus was critical to the future of the whole university. He recognized the importance of having a new laboratory home for the department and exploited the availability of federal funds by winning funding for construction of two new biology buildings for research and teaching. Just two years later, in 1965, Cliff moved to the University of California, San Diego, in La Jolla, where he became chairman of biology. The move from private to public higher education fit, I believe,

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

with Cliff's sympathy and support for widespread educational opportunity; and the move allowed him to be with Jonathan Singer, who had recently moved from Yale University to the new UCSD campus, and with Donald Helinski and others who gathered at that new university with so much potential before it. Those were times of faculty hiring and expansion and even more opportunity for Cliff, for in 1967 he became the dean of the School of Medicine and vice-chancellor of health sciences at UCSD just before the first medical school class matriculated. The appointment of a non-M.D. as medical school dean anywhere is controversial, and astonishment and no doubt some chagrin greeted the appointment that proved to be just right for the mid-1960s, as a self-consciously innovative institution emphasizing the sciences in medicine in both teaching and research was just getting going. Grobstein brought key vision and persuasive powers to bear as the new medical school took form and recruited its first faculty.

John Alksne, who was recruited to the medical school by Grobstein and is currently vice-chancellor for health sciences and dean sums up key issues: “Those were exhilarating times as the school's intellectual as well as structural foundations were being laid. He (Grobstein) was well suited to leading recruitment efforts that successfully attracted many eminent physicians and scientists from around the country to La Jolla, creating a medical school that remains committed to excellence in biomedical science as well as academics and clinical medicine.”[8] Indeed, Cliff was at the center of the debates and planning that brought the strongest possible faculty in clinical medicine, in academic medicine, and in the basic medical sciences to the new campus. Here again was the possibility of the two-cultures problem, but one that could be muted or avoided as the new school was built. An institution strong in both medical and science

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

teaching and research was ideal for the place and time: the Salk Institute was emerging with great strengths, the biological components of the Scripps Institution of Oceanography just down the hill were getting stronger, and the new basic sciences on the UCSD campus were attracting fine young faculty in many disciplines. The training of physicians, physician-scientists, and Ph.D.'s in the basic biomedical sciences went on in an atmosphere that also created one of this country's premier centers for biomedical research and development. That was not done at the expense of medical student well-being. The still skeptical component of the medical academic community looked on with awe and wonder, Harold Simon recalls, when UCSD's charter medical school class placed first in the nation on the basic science section of the National Board Examinations!

Just as Grobstein in the early 1960s had helped formulate and advance the levels-of-organization debate in the life sciences, he used his decanal pulpit to stimulate thinking about medical education. Beginning in 1970, a series of five papers published in such places as *The Journal of Medical Education* and *The British Journal of Medical Education* focused on the two-cultures issue, and more specifically on research, teaching, and curriculum in clinical and basic science departments of medical schools. Those were days when new medical schools were being started in the United States and when both new and old ones were being impacted by the early stages of the revolution in biomedical knowledge that continues ever faster today. Cliff used the UCSD Medical School as example, but really tried to help medical school faculty to think about what kinds of training could best help graduate physicians remain current during their careers as biomedical knowledge expands at unprecedented rates.

The practice of being a dean was Cliff's cup of tea. He

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

was at his best in recruiting senior faculty. His impeccable scientific credentials were an immediate source of respect. His integrity communicated itself to people, especially the ones immersed in the traumatic process of making career decisions and moves. Daniel Steinberg was chief of the Laboratory of Metabolism at the National Heart, Lung, and Blood Institute, where over several years on Saturday afternoons in the 1950s he and Grobstein had edited the newsletter of the Federation of American Scientists. When Steinberg walked into Grobstein's dean's office in La Jolla in 1968, he recalls: “He was puffing on his pipe and, as usual looked very calm and reassuring. We discussed my ambivalence about basic medical science versus medical science and my desire to participate in the governance of this new venture if I were to come. Right then and there Cliff created a new position—program director for basic sciences in medicine—that would entitle me to a seat on the Council of Chairs. I was not actually a chair, but I could participate in the planning and growth of the place where I was going to be for the next 30 years.”[9] That kind of decisiveness and ability to act was Cliff at his best as administrator. Complementing it was Grobstein's insistence on exploring all sides of issues and policies, giving all the players opportunity to chime in before decisions were taken. In Melvin Cohn's words, Cliff had a native ability to be fair even when it was not in his own interest, and that became the driving force that shaped his whole later career as leader and mediator.

Cliff's leadership and social conscience met several challenges during the deanship years. Just after addressing the entering charter class of medical students, Cliff asked the “affirmative action” question, then a new one on most campuses: Had any underrepresented minority group members enrolled or even been recruited? The negative response to both queries by Harold Simon led to an immediate deci

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

sion by Cliff that necessary and appropriate efforts would be undertaken at once.[10] Much hard work, failure, and success ensued, some involving intense efforts by Ruth Grobstein; the result was that the UCSD Medical School, along with those at the University of California, San Francisco, and Stanford became national leaders in attracting minority and women candidates. In this case, and in leading toward useful dialogue rather than confrontation and the thwarting of concerns of students and faculty about Viet Nam, Grobstein demonstrated the marriage of values, knowledge of human behavior, and how to lead that so marked him as special.

THE PRIVATE MAN AND PUBLIC RECOGNITION

Grobstein's children, Paul (subsequently chairman of biology at Bryn Mayr College) and Joan (subsequently a practicing physician in Philadelphia), were born during the NIH years and grew up with Cliff and his wife, Rose Grobstein, in the Stanford campus home. Rose was a handsome, warm, and gracious person who had a successful career as a social worker. Neighbors of the Grobsteins were Joshua and Esther Lederberg and Victor C. and Florence Twitty, he a member of the National Academy of Sciences, leading amphibian embryologist, and chairman of biological sciences who had recruited Cliff to Stanford. The Grobstein home was a welcoming place for students and lab visitors. Many a weekend trip to Bean Hollow or Pescadero, nearby ocean beaches, for mussel collecting on the low tide ended with Gibsons and wine and steaming mussels and intense, noisy conversations for hours in the jammed Grobstein living room. Every senior lab visitor had experiences like the Wilts, newly arrived from the Midwest: “Almost the first day we headed up to San Francisco for a meal. He drove like a bat out of Hell, wind whipping us as we careened in his oversized convertible to the city. We (Grobsteins, Wessells, and Wilts)

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ended up at La Pantera on North Grant, where Cliff held forth in fine fettle." Indeed, "fine fettle" describes perfectly Cliff in so many of his social situations and actions. Palo Alto Sunday mornings for Cliff were spent on the doubles tennis court with Yanofsky, Donald Helinski, and the author—there Cliff's competitiveness was fierce but always in bounds, as he ran and sweated and reveled in the California sun.

Grobstein's new life in La Jolla beginning in the mid-1960s was shared with Ruth Grobstein, M.D. and Ph.D., and stepdaughters, Sandy Wilbur, Beth Beloff, and Robin Beloff-Wachsberg, all of whom were exceptionally close to him. Ruth Grobstein was the first Ph.D. student of J. P. Trinkaus at Yale University. In New Haven, Ruth and Jon Singer had done the first experiment using an electron-dense agent, ferritin, to trace the localization of a molecule inside cells with the electron microscope. She was to become the founding head of radiation oncology and a founder and interim director of the Ida M. and Cecil Green Cancer Center at the Scripps Clinic in La Jolla. Those accomplishments were a huge source of pride to Cliff, and the two professionals approaching the apices of their careers were perfect helpmates. Embracing warmth, intensity of involvement in social and medical and scientific issues, and savoring enjoyment of life at its fullest—those phrases describe the Grobsteins during their 32 years in La Jolla.

Scientific and professional recognitions for Grobstein marked the La Jolla years. When elected to the National Academy of Sciences in 1966 at the age of forty-nine, Cliff was its youngest member. Election to the National Academy of Medicine and the American Academy of Arts and Sciences followed, as did scientific honors with the award of the Brachet Medal by the Belgium Royal Society (named for Jean Brachet, the distinguished chemical embryologist)

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

and the Anniversary Medal from his undergraduate institution, City College of New York. He served as president of the Society for Developmental Biology and the American Society of Zoologists, those elected offices reflecting the high esteem of his peers in science. Membership on editorial boards, on literally dozens of committees of the National Academy of Sciences, NIH, National Science Foundation, and the Institute of Medicine, and service to various foundations filled many hours, involved innumerable flights across the country, and were generous uses of Grobstein's special insights and wisdom. In the years after the medical deanship, Grobstein served as professor of biological science and public policy at UCSD, and it was, of course, during those years that Cliff's engagement with science, policy, and public welfare produced the stream of papers and books that culminated his career.

Clifford Grobstein died following a long illness in La Jolla on Sunday, September 6, 1998, at the age of eighty-two.

A FULL LIFE SUMMED UP

Clifford Grobstein was a leading American developmental biologist of the last half of the twentieth century who defined the basic rules of the tissue interactions that support development—cell differentiation and morphogenesis —in embryos of mammals (and we know today all vertebrates). The roles of extracellular materials and matrix during such development and the ability of different cell populations (epithelial and mesenchymal) to interact at a distance are landmark findings that have stimulated and guided experimentation worldwide as deeper understanding of development in embryos and developmental phenomena in adults has been gained.

Grobstein's intelligence and creativity were coupled to generosity toward students and scientific colleagues and af

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

fected deeply many people and their careers. Capacities to lead effectively and to bring wisdom and judgment to bear on complex, often controversial problems marked Grobstein's years as successful medical school dean, department chairman, and public servant. Warmth and humor, penetrating insights into human behavior, and fundamental concern for the well-being of others and of our society marked Clifford Grobstein as a very special human being, remembered with affection by so many who knew him.

I THANK DONALD KENNEDY, Charles Yanofsky, Melvin Cohn, Daniel Steinberg, Harold J. Simon, Fred H. Wilt, William H. Telfer, Mary Telfer, and Michael Flower for their aid in preparing this biography. They serve history well and honor Clifford Grobstein by sharing their memories. Ruth Grobstein more than anyone has provided insights, details of early life, and unique perspectives on her beloved husband.

NOTES

1. R. C. Dynes. Scientist and policy expert Clifford Grobstein dies at age 82.
2. W. H. Telfer and M. Telfer. Personal communication.
3. M. Flower. Personal communication.
4. F. H. Wilt. Personal communication.
5. C. Grobstein quoted in “Research News,” *Science* 217(1982):36-37
6. C. Yanofsky. Personal communication.
7. M. Cohn, 1998. Clifford Grobstein: In memoriam, the Stanford years
8. J. Alksne: Scientist and policy expert Clifford Grobstein dies at age 82.
9. D. Steinberg. Personal communication.
10. H.J. Simon. To Clifford Grobstein, a tribute of memories.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1953

Analysis in vitro of the early organization of the rudiment of the mouse sub-mandibular gland. *J. Morphology* 93: 19-44.

Inductive epithelio-mesenchymal interaction in cultured organ rudiments of the mouse. *Science* 118: 52-55.

Epithelio-mesenchymal specificity in the morphogenesis of mouse sub-mandibular rudiments in vitro. *J. Exp. Zool.* 124: 383-414.

Morphogenetic interaction between embryonic mouse tissues separated by a membrane filter. *Nature* 172: 869.

1955

Tissue interaction in the morphogenesis of mouse embryonic rudiments in vitro. In *Aspects of Synthesis and Order in Growth,* ed. D. Rudnick, pp. 233-56. Princeton, N.J.: Princeton University Press.

Inductive interaction in the development of the mouse metanephros. *J. Exp. Zool.*130: 319-40.

1957

With A. J. Dalton. Kidney tubule induction in mouse metanephric mesenchyme without cytoplasmic contact. *J. Exp. Zool.*135: 57-74.

1959

Differentiation of vertebrate cells. In *The Cell,* eds. J. Brachet and A. Mirsky, pp. 437-96. New York: Academic Press.

1961

Levels and ontogeny. *Am. Sci.*50: 46-58.

1964

Cytodifferentiation and its control. *Science*143: 643-50.

With W. J. Rutter and N. K. Wessells. Control of specific synthesis in the developing pancreas. National Cancer Institute Monograph 13, pp. 51-65.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1965

With F. Kallman. Source of collagen at epithelio-mesenchymal interfaces during inductive interaction. *Dev. Biol.*11: 169-83.
With J. Cohen. Effect of collagenase on the morphogenesis of embryonic salivary epithelium in vitro. *Science*150: 626-78.

1966

New patterns in the organization of biology. *Am. Zool.*6: 621-26.
Defining the core of a science. *Am. Biol. Teach.*28: 804-808.

1970

Toward fully university-based health professional schools. *J. Med. Educ.*45: 684-88 and *Int. Med. Dig.*86: 261-63.

1972

University of California, San Diego, School of Medicine. In *Case Histories of Ten New Medical Schools,* eds. V. W. Lippard and E. Purcell, pp. 85-110. New York: Josiah Macy Foundation.

1974

*The Strategy of Life.*2nd ed. San Francisco: W. H. Freeman & Co.

1975

Developmental role of intercellular matrix: Retrospective and prospective. In *Extracellular Matrix Influences on Gene Expression.*New York: Academic Press.

1976

Recombinant DNA research: Beyond the NIH guidelines. *Science*194: 1133-35. Also *Sci. Am.*237: 22-33.

1979

*A Double Image of the Double Helix.*San Francisco: W. H. Freeman & Co.
External human fertilization. *Sci. Am.*240(6): 57-67.

1983

With M. Flower and J. Mendeloff. External human fertilization: An evaluation of policy. *Science*222: 127-33.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1985

With M. Flower and J. Mendeloff. Frozen embryos: Policy issues. *N. Engl. J. Med.*312(24): 1584-88.

1988

Biological characteristics of the preembryo. *Ann. N. Y. Acad. Sci.*541: 346-48.

*Science and the Unborn.*New York: Basic Books.

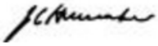
*From Chance to Purpose, an Appraisal of External Human Fertilization.*Menlo Park, Calif.: Addison Wesley.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.



Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

JEROME CLARKE HUNSAKER

August 26, 1886–September 10, 1984

BY JACK L. KERREBROCK

WHEN JEROME C. HUNSAKER died in September 1984 at the age of ninety-eight, his illustrious career had spanned the entire existence of the aerospace industry, from the very beginnings of aeronautics to exploration of the solar system. His colleagues had extended from the Wright Brothers to Charles Stark Draper, and included virtually all of the founders and leaders of aeronautics and astronautics. Beginning with important technical contributions, he soon turned his attention to creating and managing the new institutions needed to deal with the growth of the aeronautics industry. By the early 1930s he was at the pinnacle of the aeronautics industry with leadership roles in academia, government, and industry. In recognition of these achievements, in 1933 he was awarded the prestigious Guggenheim Medal, the fifth such recipient after Orville Wright, Ludwig Prandtl, Fredrick Lanchester, and Juan de la Cuerva. His career continued at this level for nearly three decades.

Even after his retirement in 1951 as head of the Department of Aeronautics at the Massachusetts Institute of Technology, a department he founded in 1939, Hunsaker continued as chairman of the National Advisory Committee for Aeronautics (NACA) until 1956, a position he had held

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

since 1941. Recognized the world over for his contributions to aeronautics, Hunsaker was also well regarded in the larger society. He was awarded the Julius Adams Stratton Prize for Cultural Achievement in 1969. In 1972 he was installed as honorary president of the American Institute of Aeronautics and Astronautics (AIAA) in commemoration of his installation 40 years earlier as president of the Institute of the Aeronautical Sciences, a parent of the AIAA. He consulted and maintained a regular presence at MIT into his nineties. The career of Jerome C. Hunsaker might be considered the epitome of success in engineering.

Hunsaker was born in Creston, Iowa, on August 26,1886, of parents Walter J. and Alma Clarke Hunsaker. His father was a newspaper publisher. Educated in the public schools of Detroit and Saginaw Michigan, he then enrolled in the U.S. Naval Academy, where he graduated at the head of his class in 1908, the same year Orville Wright successfully demonstrated the Wright Flyer to the army. Some who knew him at the time characterized him as the Einstein of the navy, awed by his brilliance.

Assigned by the navy to MIT to study ship construction, he received his master's degree in naval architecture in 1912. At about that time an aviation meet was held at Squantum, with a Bleriot flight around Boston harbor. Hunsaker did not find the design of naval super-dreadnoughts particularly stimulating, the weight of tradition oppressing him. The Bleriot flight attracted him to the fledgling field of aeronautics, and with his wife, the former Alice Porter Avery, whom he had married in 1911, he spent the summer translating for publication the classic treatise *Resistance of the Air and Aviation* by Gustave Eiffel. He found several mistakes in this seminal work, and so impressed Eiffel that he was invited to study in Eiffel's laboratories outside Paris. The navy had assigned Hunsaker to the Boston navy yard,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

but in 1913 President Richard C. Maclaurin of MIT asked that he be detailed to MIT to develop courses in aerodynamics. He was appointed an instructor in the Department of Naval Architecture. He spent some time in Europe studying aeronautical research. In Germany he attempted to study the Zeppelin but was impeded by military restrictions, so he purchased a ride as a tourist. He met the young Dutch designer Fokker, who was building an experimental monoplane that he later sold to the German army. Moving on to England he spent some time at the National Physical Laboratory at Teddington in Middlesex. Here he studied a new wind tunnel, acquiring information that later enabled him to build a tunnel at MIT.

When he returned to MIT in 1914, Hunsaker set about the construction of this wind tunnel with the assistance of Donald W. Douglas. The wind tunnel was of modest performance, yielding a speed of about 40 miles per hour in a 4-foot-square test section, but it enabled experiments with airplane models for which Hunsaker was awarded MIT's first doctorate in aeronautical engineering in 1916. His work in aircraft stability was published by the NACA as Technical Report No. 1 of the NACA in 1915. As such it was the first of a long series of research reports of very high quality that extended to the melding of NACA into NASA in 1958. In this same time Hunsaker developed a course of study for the degree of master of science. His first course was entitled “Aeronautics for Naval Constructors.” It formed the foundation for the later development of a course in aeronautical engineering that developed into a Department of Aeronautical Engineering.

In 1916 Hunsaker was called back to head the new Aircraft Division in the navy's Bureau of Construction and Repair, and was soon responsible for the design, construction, and procurement of all naval aircraft. By the end of the

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

year a thousand flying boats had been built and shipped to France. In 1918 he was charged with two special projects: to build a Zeppelin and to design and build a flying boat with the capability of crossing the Atlantic. More specifically the aircraft was intended to combat submarines, operating from land bases. This required long-range capability, which enabled the aircraft also to complete a flight across the Atlantic, the longest leg being about 1,330 miles. The flying boat became known as the NC (Navy Curtiss). It was the largest aircraft in the world at the time, with four engines and a crew of six. Of the four built, one—the NC-4—completed the Atlantic crossing by way of the Azores in 57 hours. Two others were left in the Azores. This was in May 1919. On June 14-15, 1919, Alcock and Brown completed a 16-hour nonstop flight from Newfoundland to Ireland in a Vickers Vimy bomber. (It seems that, by these examples, Hunsaker laid the groundwork for modern engineering project management.) After the armistice Hunsaker went to Germany to examine Zeppelins. When he returned he set up a team to design a helium-filled dirigible, to be named the *Shenandoah.* It was the first helium-filled dirigible and operated successfully for two years, breaking up in a violent storm in Ohio in 1925. By this time the Naval Bureau of Aeronautics had been organized. As chief of the Material Division he developed a number of aircraft and methods for launching them from ships. He also worked on radial air-cooled engines, and arresting gear for landing of aircraft on ships. He and Douglas developed a torpedo plane. In this period he argued for and was thus instrumental in the creation of carrier-born aviation. He believed in the need for ship-borne air power to protect ships from attack by aircraft. This brought him into some tension with Col. Billy Mitchell, who forcefully argued that the advent of air power obsoleted warships.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

A new phase of his career as an industrial entrepreneur began in 1926 when Hunsaker joined Bell Telephone Laboratories to develop communication services for aircraft. With the support of the Guggenheim Foundation a model airline, Western Air Express, was established in 1927 by Harris M. Hanshue to provide scheduled daily airline service from Los Angeles to San Francisco. The communications systems developed by Hunsaker were necessary to inform the pilots of weather on the route. In the same vein he urged MIT to develop a meteorology program. In 1928 he joined the Goodyear-Zeppelin Company, which had been newly formed to launch a transatlantic airship passenger service. Two 785-foot dirigibles were built for the navy—the *Akron* and the *Macon* —each of which could store and launch five small fighter aircraft. They were helium filled and heavily strengthened in light of the experience with the *Shenandoah,* but they were nevertheless lost in storms. This incidents was a shattering blow to Hunsaker, who had a friend aboard. They also contributed to the growing sentiment that there was little future in lighter-than-air travel.

In 1933 Hunsaker returned to MIT as head of the Department of Mechanical Engineering. In this position he was also responsible for Course 16, Aeronautical Engineering, which was offered in the Department of Mechanical Engineering. He became head of the Department of Aeronautical Engineering when it was established in 1939, but he continued as head of mechanical engineering as well until June 1947, when he requested that he be relieved of the responsibility for the mechanical engineering department to devote his time to the aeronautical engineering department and its expanding programs in supersonic aerodynamics, aero-elasticity, automatic controls, and jet propulsion. During this time he was very active in national aeronautical affairs, replacing Vannevar Bush as chairman

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of the NACA in 1941 and continuing in this position through the explosive growth of the NACA during the war years and for some years thereafter, until he was succeeded by James Doolittle.

At MIT Hunsaker was engaged in developing the academic substance of aeronautics. Recalling the pioneering studies at MIT, Hunsaker once said, “In the beginning it was not possible to teach the principles of aeronautical engineering because none of us knew them. The principles had to be discovered, which meant that we had to investigate the difficulties of the past and, having a lot of facts, we had to find out what they meant and how to find the principles.” To bring this about Hunsaker hired an able faculty, one of the first being Charles Stark Draper, who succeeded Hunsaker as head of the department in 1951. The Department of Aeronautical Engineering that Hunsaker founded became the Department of Aeronautics and Astronautics under Draper in 1961 and has continued to lead in education and research in aerospace engineering.

In addition to developing the academic program for the new discipline of aeronautical engineering, Hunsaker played an active role in the development of experimental facilities. For the Wright Brothers Wind Tunnel, dedicated in 1938, he obtained the approval of the MIT Corporation and raised funds from prominent figures in aviation. It was the seventh wind tunnel built at MIT, including Hunsaker's first tunnel built in 1914. It could provide a wind velocity of 400 miles per hour at an altitude of up to 37,000 feet. After the United States entered the war it was used almost continuously. It is still in use today as a student tunnel. Later he played a key role with John Markham in the founding of the Naval Supersonic Laboratory with its large supersonic wind tunnel, used initially for work on the Meteor missile.

During the critical years of the Second World War

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Hunsaker served as chairman of the NACA, which played a major role in the explosive military aircraft development driven by the war. At the same time he was also coordinator of naval research and development.

In these positions he involved himself in all matters of technical development and was a party to discussions leading to major policy decisions. He was known to be vigorously opposed to the use of the atom bomb, taking the position that Japan was finished in any case and that Truman, Byrne, and the chiefs of staff had no real understanding of the weapon. There is no doubt that Hunsaker had strong views on the issues of the time. He opposed the development of the jet engine, on the grounds that it could not be brought into useful action before the end of the war. In this, Germany almost proved him wrong. One effect of his position on jet propulsion was that the NACA laboratories, which had been focused on aircraft aerodynamics and structures and piston engines, were far behind the Europeans in the technology of jet propulsion at the end of the war.

During the transition from war to peace Hunsaker served on the Wilson Committee, which made recommendations leading to the establishment of the Research and Development Board of the Department of Defense; the Executive Committee of the Guided Missiles Committee of the Joint Chiefs of Staff; the President's Scientific Research Board; the President's Special Board of Inquiry on Air Safety; and the Industry Advisory Committee of the Atomic Energy Commission.

After his retirement from the MIT faculty in 1952, Hunsaker continued to be very active, serving as chair of the NACA until 1956. He is credited with the development of the Ames and Lewis (now Glenn) Laboratories of the NACA; the modernization and expansion of the Langley Laboratory; establishment of the Pilotless Aircraft Research Station at Wal

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

lops Island, Virginia; and the High-Speed Flight Research Station (now Dryden Research Center) in California. He led the development of the National Aeronautical Research Policy in 1946. In 1954 Hunsaker was honored by the establishment of the Hunsaker professorship in the Department of Aeronautics and Astronautics at MIT. This visiting professorship is offered to persons distinguished in aerospace, who during their term as Hunsaker professor deliver the Minta Martin Lecture. This component of the professorship was endorsed by Glenn L. Martin in honor of his mother. In 1960 he served as a director of McGraw-Hill Publishing, Shell Oil Corp., Goodyear Tire and Rubber, and Sperry Corp., and was a regent of the Smithsonian Institution. On May 26, 1965, he delivered the second annual Sight Lecture at the Wings Club in New York. The first of these lectures had been given by Igor Sikorsky. Hunsaker's lecture is interesting to read for its historical content but also for the humility with which he presents his accomplishments, pointing out that, unlike Sikorsky, he was not an inventor but an organizer of others. In 1969 he was presented by NASA Administrator Thomas O. Paine the award of Honorable Commander of the Civil Division of the Most Excellent Order of the British Empire.

Hunsaker maintained a presence in his office in the Guggenheim Laboratory at MIT for many years, walking there from his home on Beacon Hill, even after he was in his eighties. He often delighted his younger colleagues with his witty commentary. One favorite subject of discussion between him and C. S. Draper at faculty gatherings was the relative importance of aeronautics and astronautics. Hunsaker was never an enthusiast for rockets. On one memorable occasion at the peak of the Apollo Program when Draper reminded him of this, Hunsaker commented, “and I may still be right.” His was a formidable presence to the end.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Hunsaker was married in 1911 to Alice Porter Avery. They had four children: Mrs. Sarah P. Swope, Mrs. Alice H. Bird, Jerome Clarke Hunsaker, and James Peter Hunsaker (who died as a young man).

HONORS AND AWARDS

The awards and honors received by Jerome C. Hunsaker were numerous. Among the more significant were the following.

Member:
National Academy of Sciences
National Academy of Engineering
Fellow:
American Academy of Arts and Sciences
American Physical Society
Honorary Fellow:
Imperial College of Science and Technology
Institute of the Aeronautical Sciences
Royal Aeronautical Society
Honorary Member:
American Society of Mechanical Engineers
Institute of Mechanical Engineers
Honorary Degrees:

1943	D.Sc., Williams College
946	D.Eng., Northeastern University
1955	D.Sc., Adelphi College

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Awards:

1919	Navy Cross
1933	Daniel Guggenheim Medal
1942	Franklin Medal
1946	Presidential Medal for Merit
1949	Legion of Honor
1951	Wright Trophy
1953	Godfrey L. Cabot Trophy
1955	Langley Medal Elder Statesman of Aviation, National Aeronautic Association
1957	Water-Based Aviation Award of the IAS NACA Distinguished Service Medal Gold Medal of the Royal Aeronautical Society
1958	U.S. Navy Award for Distinguished Public Service

CAREER CHRONOLOGY

1909-26	Officer, advancing to commander, Construction Corps, U.S. Navy
1912-16	Instructor of aeronautical engineering, MIT
1916-23	In charge of aircraft design, Navy Department, Washington, D.C.
1923-26	Assistant naval attaché, London, Paris, Berlin, Rome, The Hague.
1926-28	Assistant vice-president, Bell Telephone Laboratories.
1928-33	Vice-president, Goodyear-Zeppelin Corporation
1933-47	Professor and head, Department of Mechanical Engineering, MIT
1933-51	Professor and head, Department of Aeronautical Engineering, MIT
1938-58	Member, National Advisory Committee for Aeronautics (chair. 1941-57)
1951-52	Professor of aeronautical engineering, MIT
1952-84	Professor of aeronautical engineering, emeritus, MIT

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

REFERENCES

Files of the Department of Aeronautics and Astronautics, MIT. New York Times, September 13, 1984.

Pendray, G. E., ed. The Guggenheim Medalists, Architects of the Age of Flight, the Guggenheim Medal Board of Award, 1963.

Bassler, R. E.Jerome Clark Hunsaker, USNA, 1908- Top Man.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1912

With R. T. Hanson. Rudder trials on U.S.S. *Starrett.* Am. Soc. Naval Archs. & Marine Engrs.

1913

Eiffel-The resistance of the air and aviation. Revised and Translated by J. C. Hunsaker. London: Constable.

1914

The present status of airships in Europe. *J. Franklin Inst.* Aeroplane design. U.S. Naval Proceedings.

1915

Inherent longitudinal stability for a typical biplane. Washington, D.C.: NACA.

1916

Dynamical stability of aeroplanes. Smithsonian Miscellaneous Collections and *J. Natl. Acad. Sci.*
Aeronautics. In *Mechanical Engineers Handbook,* Sect. 10. New York: McGraw-Hill.
Stable biplane arrangements, Engineering, London.

1919

Naval airships. *J. Soc. Automot. Eng.*

1923

Transportation by air, Aviation , New York.

1924

With Burgess and Truscott. Strength of rigid airships. *J. R. Aeronaut. Soc.*

1928

Communications as an aid to safe flying. 1st National Aeronautical Safety Conference, New York.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1930

Transoceanic air travel. *J. Soc. Automot. Eng.*

1935

Progress report on cavitation. *Trans. ASME*57: 423.

1938

With J. R. Markham and H. Peters. The Wright Brothers wind tunnel. MIT.

1941

The development of the airplane. Six Lowell Lectures. Boston.

Dimensional analysis and similitude in mechanics. Von Karman Anniversary Volume. Pasadena, Calif.: Caltech.

1945

The NACA—Its contribution to victory. *Army Navy J.*

1947

With B. G. Rightmire. *Engineering Applications of Fluid Mechanics.*New York: McGraw-Hill.

Newton and fluid mechanics. Royal Society Newton Tercentenary. Cambridge University Press.

1951

Aeronautics. *J. Franklin Inst.*251(1).

1952

Aeronautics, some social aspects. *Aeronaut. Eng. Rev.*

*Aeronautics at the Mid Century.*Yale University Press.

1956

*Forty Years of Aeronautical Research.*Smithsonian.

1961

With J. H. Doolitte, G. Loening, and J. F. Victory. Significant twelve. *Pop. Mech.*

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

PHILIP F. LOW

October 15, 1921– January 14, 1997:

BY W. R. GARDNER AND C. B. ROTH

PHILIP LOW'S UNCOMPROMISING honesty, keen intellect, and ability to lead by example resulted in an influence on science and higher education that ranked him among the top scientists in the country. Whatever endeavor he undertook he addressed with a singleness of purpose that never faltered short of achieving his goal. He devoted his career to advancing our understanding of the physics and chemistry of the absorption of water by the soil's clay mineral fraction, which dominates almost all the physical and chemical properties of soils even though it is less than two microns in size. During much of his career his thinking ran counter to the general scientific consensus, however he invariably fielded his scientific arguments with dignity and fairness.

Philip Low was a pioneer in applying thermodynamics to clay-water systems and in elucidating the nature of phosphate fixation, potassium fixation, aluminum release by exchangeable hydrogen, and osmosis and ion diffusion in these systems. Despite almost universal skepticism, Low challenged the concept that double-layer theory described clay swelling and proceeded to prove that this phenomenon is due to long-range interaction between particle surfaces and the water. Also, he developed general equations that relate both

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the swelling pressure and the properties of the water to the thickness of the water films on the particle surfaces. He eventually reduced many clay-water properties to a form that permitted them to be calculated from each other using an absolute minimum number of laboratory measurements. Though his work dominated the field of clay-water phenomena, Phil Low was not a one-dimensional individual. He had a thorough grasp of soil physics, plant physiology, and clay mineralogy as well as soil chemistry. His lectures dealt fairly with theories opposing his own. He had a keen interest in his own students and colleagues, and especially in his later years, he gave unstintingly of his time to assist younger scientists and colleagues.

Philip Funk Low was born on a farm near Carmangay, Alberta, Canada, on October 15, 1921, the son of Philip and Pearl Helena Funk Low. Before 1929 the family enjoyed great prosperity but with the market crash and the onset of the Great Depression, their prosperity vanished, and they became well acquainted with dire poverty. This poverty was made even more painful by marital problems between Philip's father and mother, who eventually separated. There were times when there was no food in the house, and the rent could not be paid. To alleviate the situation, Philip's mother took in boarders. Many times she advised her children to wait at the dinner table until the boarders had been served in order to be sure that there was enough food to go around. Hoping for a better life, the family moved to Calgary, Alberta, in about 1933.

Anyone familiar with Phil's dignified demeanor as an adult may find this hard to believe, but young Philip was a very inquisitive and mischievous child. These traits got him into trouble almost every day, and a spanking or a silent sitting on the stairs was the conventional punishment. Despite warnings, one cold winter day temptation got the best of him,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

and he touched his tongue to an iron gate. It froze fast to the gate. After a few minutes it came loose, but not without leaving some skin behind. On another occasion he tried to emulate the engineer who frequently oiled and greased the big J. I. Case steam engine at threshing time. He took a 5-pound pail of lard from the shelf and greased every door knob and water tap in the house. Later, in a single day, he broke his grandfather's wooden churn, axed his water hose into two pieces, dropped and broke his stove grate, and chased his chickens so they wouldn't lay eggs. These early exploits presaged the imagination and innovation that he displayed later in his scientific career and in an unwillingness to take the unproved for granted.

Philip's mother wanted her children to have a good education in city schools, and despite the continuing poverty, Philip did well there. The twelfth grade in Alberta was so difficult that most students had to spend two years to complete it. At the end of the school year, the government-administered final exams lasted three to four hours and covered all subjects, including mathematics (with beginning calculus), chemistry, English, and French. Most students failed at least one exam the first try and required a second year of study. Philip knew this and he also knew that he could not afford to spend extra time in high school. Consequently, he declined all outside activities to concentrate on his studies. He passed all of his exams the first time, several of them with honors.

While Philip was in the twelfth grade, his sister Gwen enrolled at Brigham Young University in Utah, proving it possible to attend a university on limited finances. Philip and his younger brother Maurice realized that, if they were to find a position with any kind of financial security, they would need a college degree.

Even though Gwen and Philip both worked until the start

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of BYU's fall quarter, it became obvious that there would not be enough money for both of them to enroll at that institution. Gwen selflessly volunteered to continue to work and help support the family so that her brother would not be denied the education he would so need. Also, an aunt kindly agreed to live with the family and to share expenses. Thus, Philip was able to enroll at BYU, and Gwen's sacrifice was eventually rewarded with a very successful career of her own.

In the meantime Mother Low had decided to move her family permanently to Utah. She shipped her household belongings to Salt Lake City, bought bus tickets for herself and her two sons, and presented herself at the immigration office at the U.S.-Canadian border. The $80.00 in her purse was all the money she had in the world, but Divine Providence had never failed her in times of need, and her faith was unshakable. An unfavorable reception at the U. S. Immigration and Naturalization Service awaited her. After three days, as the money decreased and anxiety increased, a kindly immigration officer took interest in her case. With his help, she and her children were granted U.S. citizenship on July 2, 1940.

Because of the rigor of Canadian high schools, BYU granted Philip college credit for the courses he took in the twelfth grade. This was a mixed blessing. Although it reduced the time he had to spend in college, it put him in his sophomore year without prior college experience and without a major field. As a result, he floundered, and his grades were not good for the first quarter. A roommate suggested that he take a course in soils from T. L. Martin, an inspiring and legendary soil scientist (and father of the internationally known soil scientists William and James Martin). This suggestion changed the course of Phil's life. Martin not only inspired Phil but he also guaranteed him an assistantship at

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

one of the best graduate schools in the nation if he would take the courses he recommended. Martin could guarantee this because his reputation for producing outstanding students was well known nationally (e.g., National Academy of Sciences member C. B. Tanner). Thus, an enthusiastic and dedicated soil scientist was created.

Phil's time at BYU was well spent, and he took time from his studies and work to participate in a range of social activities. Most importantly, he met Mayda Stewart, and they were married on June 11, 1942, after more than a year's courtship. This was the day following her graduation with a B.A. degree; Phil had another year to go before he would complete his degree requirements.

The first year of their marriage was an anxious one for Phil and Mayda. Phil was subject to being drafted into military service. On March 2, 1943, while still at BYU he was notified to report to Fort Douglas, Utah, for induction at about the time that Mayda was expected to deliver their first child. Phil was accepted into a technical program designed to train meteorologists for the U. S. Army Air Corps. Immediately after his induction, he was sent to the University of New Mexico in Albuquerque, and Mayda did not know where he was for several days because of military secrecy. This was obviously an anxious time for both of them, as Mayda was obliged to bear their child alone during this separation.

From New Mexico, Phil and his company were sent to the California Institute of Technology in Pasadena, California, for a year's training in practical and theoretical meteorology, including an excellent training in mathematics and physics. In June 1944 Second Lieutenant Philip F. Low was transferred to Fairbanks, Alaska, for six months and Fort Nelson, British Columbia, for another seven months. Even

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

tually he was assigned to Great Falls, Montana, as an instructor.

Phil had received his B. S. degree from BYU in absentia. With that degree and a M.S. degree granted by Cal Tech (for his training there while in the Army Air Corps), Phil was ready to undertake his Ph.D. training immediately after discharge. But few graduate schools were prepared to offer graduate assistantships or fellowships, and Phil returned to BYU, where he helped Martin for an academic year as a laboratory assistant and studied physical chemistry intensely. At the end of that year, Iowa State and Rutgers universities offered him fellowships almost simultaneously. He accepted the offer from Iowa State and began his graduate study in Ames in the fall of 1946.

This was a good time to be a graduate student in soils at Iowa State. Not only did Iowa State have an outstanding soils faculty but it also had extremely good physics, chemistry, and biology teachers to provide the needed foundation for soil science. Phil was assigned to work on phosphorus fixation under C. A. Black, a highly respected scientist and teacher. With Black's full support, Phil was permitted to depart from the usual curriculum and became the first graduate student in soils at Iowa State University to take graduate courses in chemistry. The courses he took emphasized physical chemistry, and thus it was that he developed an intense interest in this subject, especially in thermodynamics. He graduated in June 1949 with the equivalent of a Ph.D. in chemistry.

The U.S. Department of Agriculture offered Phil a job in a new laboratory that the department was establishing at New Mexico State College in Las Cruces. Given the paucity of positions in soil chemistry, Phil felt fortunate indeed. Within four months he received an offer to be an assistant professor of soil chemistry in the agronomy department at

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Purdue University. J. B. Peterson, the new head of that department, had known Phil at Iowa State University and had waited for the opportunity to employ him. Peterson was a man of great vision and believed in giving young scientists the freedom to develop their interests and capitalize on their abilities. He was exceptional in this regard in that many younger faculty members in agriculture at that time were apprenticed to senior faculty members who had definite ideas of the type of applied research that was needed. A farm background was often considered a better guide to success than the course of study followed. As at Iowa State, Phil was fortunate to be surrounded by outstanding colleagues.

Early in his career Phil postulated that clays dissociate into their component ions and have a solubility product constant. Remarkably, this young faculty member was the first to make such a proposal. It then followed, he showed, that soluble phosphates can be “fixed” by forming insoluble compounds with aluminum ions dissociated from the clay and that, as a consequence, the clay decomposes. This work encouraged others to investigate the kinds and stability of phosphates that occur in the soil. Investigation of the solubility products of clays was also stimulated. A new and promising area of soil chemistry had been opened.

Soon afterwards, Low and another team of investigators working independently found that exchangeable hydrogen ions that adsorbed on the surfaces of clay crystals release aluminum ions from these crystals. Hence, the surface charge is partly compensated by the aluminum. A similar finding had been made by others several years earlier, but had not been appreciated. This was not the case with the later work, which caused considerable excitement and resulted in a new concept of soil acidity and in many related investigations.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Thermodynamics was always of special interest to Phil, who applied it to the study of water in clays, phase equilibrium, ionic diffusion, soil swelling and freezing, and to water flow in soils and clays. His thermodynamic treatment of water flow in heterogeneous systems evoked the idea that water flows along a partial molar-free energy gradient and led him to be the first to show that osmosis could occur through thick “membranes”composed of consistent clay gels. He also determined that osmosis occurs by a kind of viscous or laminar flow rather than by diffusion and that the pressure distribution in the membrane was not the primary driving force. However, since the partial molar-free energy of the water was found to decrease continuously in the direction of flow, he concluded that it was the driving force. The relevance of these results to biological membranes is significant.

Phil was a pioneer in elucidating the principles of ion diffusion in clays. He wrote one of the first published papers on this subject. At the time this seminal work was written, it was widely believed that ion diffusion in the soil did not contribute to the nutrition of plants, however in the intervening years there has been a dramatic change in philosophy, and it is now believed that ion diffusion is a major factor in plant nutrition. Interestingly, some of the early papers describing the cloud of ions surrounding the plant roots were groping for this concept. Phil's work helped to clarify and focus the attention of soil scientists on the subject of ion diffusion and contributed to a better understanding of the role of convection, diffusion, and adsorption on ion uptake by plants. In total, he wrote some 15 papers relevant to this subject. His strong training in plant physiology combined with his thorough understanding of physical chemistry gave Phil a distinct advantage in addressing soil-plant relationships.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

In 1960 the editor of *Soil Science,* Fireman E. Bear, requested Phil's help in collecting a group of *Soil Science* papers that would express imaginatively and speculatively in one issue the most advanced thinking of Purdue University soil and plant scientists. In his contribution to this issue (1962), Phil postulated that ordered or quasi-crystalline water near clay surfaces should affect the rate processes involved in plant nutrition. Although, as noted below, he had evidence that water near these surfaces was ordered, he had no evidence that it affected plant nutrition or biological activity. Therefore, he initiated experiments to test his postulate. During the course of these experiments, he found that the arrangement of clay particles in clay-water system affects the properties of the adjacent water and, when the particle arrangement is changed by a mechanical disturbance, biological activity in the system changes correspondingly. For example, seed germination, bacterial thermogenesis, and nutrient uptake by corn seedlings were greater in the disturbed clay-water systems than in the undisturbed ones. These findings have never been tested fully in the field, but they deserve further consideration.

When Phil began his professional career, it was widely believed that water next to the surfaces of all solids behaves like normal bulk water and that all colloidal phenomena, including those involving clays, could be described adequately by electrical double-layer theory. This theory had the advantage of being quantitative and intellectually satisfying. One of its most useful applications was in the prediction of the swelling pressure of clays. Therefore, his hypothesis was not readily accepted when Phil hypothesized instead that interaction between the surfaces of clay particles and the inter-particle water lowers the potential energy of the water and thereby contributes to the swelling pressure of the clay. Nevertheless, he decided to test it by investigating the physical

Copyright © National Academy of Sciences. All rights reserved.

and thermodynamic properties of the water in clay-water systems. His idea was that these properties should deviate from those of normal bulk water if the swelling interaction was strong enough to alter the molecular arrangement (hence, the potential energy) of the water. This line of reasoning led to studies of clay-water interaction that consumed most of the remainder of his career.

As is often the case in science, when evidence favoring his hypothesis accumulated, opposition from those who thought otherwise increased. Most of the objections were based on conceptual arguments or indirect inferences. The complexity of the problem discouraged all but the most intrepid experimentalists. Phil and his students designed and carried out over the years the most elegant and difficult experiments in order to resolve the question of the state of water near particle surfaces based on a quantitative physical model. It turned out that Phil was to spend some 35 years obtaining and refining the data required to clarify the issues. The difficulties inherent in the experimental process dictated that progress be slow and tedious. Eventually the data and its analysis showed unequivocally that inter-particle water differs appreciably from normal bulk water in many physical properties. These properties include supercooling, viscosity, heat and entropy of compression, specific volume, specific heat capacity, specific expansibility, specific compressibility, free energy, enthalpy, and entropy. Moreover, it differs in such spectroscopic properties as molar absorptivity, O-H stretching and H-O-H bending. A careful analysis of the data shows, importantly, that the exchangeable cations cannot account for the differences. Therefore, consistent with his hypothesis, Phil concluded that the particle layer surfaces were responsible for the observed effects. These studies established that the bound water in intimate contact with clay and other minerals in the Earth's

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

crust differs from normal bulk water. Moreover, they indicate the nature and extent of the difference. It is noteworthy that Phil's contributions in this regard are well recognized.

In a remarkable synthesis of his studies of clay-water interaction, Phil discovered that all the properties of the inter-particle water and clay-water system are described by two parameters in an exponential function. The first is a common variable in the exponent representing the average thickness of the water film on the particle surface. The second parameter is a constant that is characteristic of the particular property involved. As a consequence, elimination of the single variable between analogous equations for any two water properties yields an equation that allows one property to be calculated from any other when the respective values of the characteristic constants are known. Since these constants are determinable, the calculation of every property of the water in a clay-water system from the measured value of a single property is feasible. Hence, the aforementioned equation had great practical significance. Its theoretical significance was even greater, however. It showed that every property of the inter-particle water was affected similarly by the interaction of the water with the particle surfaces and that this interaction did not depend on the specific nature of these surfaces.

Once the difference between normal bulk water and the water near clay surfaces had been established, Phil resumed his study of clay swelling in a series of remarkable experiments in which X-ray diffraction was used to measure the distance between superimposed, parallel clay layers as a function of the swelling pressure. He found that the swelling pressure of all expanding lattice clays is described by an exponential equation containing two universal constants and a single variable (i.e., either the interlayer distance or the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

water content). These two variables were found to be proportional to each other. Since the properties of the inter-particle water were also functions of the latter variable, it was evident that these properties and the swelling pressure were related. Double-layer theory does not predict the equation that relates the swelling pressure to the interlayer distance, nor does it predict the relation between this pressure and the properties of the inter-particle water. Thus, Phil showed eventually that electrical double-layer theory effects were overshadowed by the water structure properties and that his initial hypothesis was vindicated.

In an article entitled “The Background to Hydration Forces” published in the proceedings of a conference on hydration forces and molecular aspects of solvation (*Chemica Scripta* 61[1985]:25) the statement is made that “P. F. Low in the West . . . and Deryaguin in the East carried the flag for advocates of in-depth hydration over the difficult period that such views (were) unpopular and indeed heretical.” As is often the case, the heretical became the orthodox.

During the course of his studies on clay swelling, Phil observed that the reduction of octahedral iron in clay crystals reduced the swelling of the crystals by causing their superimposed layers to collapse. Since potassium fixation is due to the entrapment of potassium ions between collapsed clay layers, this observation led to the idea that the reduction of octahedral iron in clays would enhance potassium fixation. When the idea was tested, it was found to be valid. Moreover, it was determined that soil microorganisms could reduce the octahedral iron under the anaerobic conditions in wet soils. Thus, Phil's fundamental research yielded a new and important concept of potassium fixation with many practical ramifications.

One of Phil's last experiments was among the most exciting. He discovered that the frequency of the Si-O stretch

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

ing vibrations in the clay layers changed with water content over the entire range of swelling. The inevitable conclusion is that the structure of the clay changes correspondingly. Further, the Si-O stretching frequency was found to depend exponentially on a single variable—the water content. Since the equation relating the Si-O stretching frequency to the water content has the same form as the equation relating the H-O-H bending frequency and clay swelling pressure to the water content, it follows that the structure of the clay, the structure of the interlayer water, and the swelling pressure of the clay are interrelated. This interrelationship suggested that the Si-O stretching vibrations in the clay are coupled to the H-O-H bending vibrations in the inter-layer water and that, as these vibrations change with inter-layer distance (water content), the energy of the system changes. This, in turn, affects the swelling pressure of the clay. Thus, a much more comprehensive understanding of the phenomenon of swelling now exists.

Phil and Mayda Low had two sons and four daughters. Son Philip S. followed in Phil's footsteps and became a professor of biochemistry on the Purdue faculty. Mayda, a gifted and well-trained violinist, served as concertmistress of the Lafayette Symphony and trained many students. Her standing in the music world gave Phil's life an additional and valued dimension. There was never any question about Phil Low's priorities. First came his family, second his church, and then his work. His ability to concentrate and budget his time was a substantial asset.

Because of his leadership abilities he was called upon by the Church of Latter Day Saints (Mormon) soon after he arrived in Lafayette, and for many years Phil was the ecclesiastical leader for the church throughout Indiana. This responsibility made it difficult for Phil to take the normal academic sabbatical. However, Phil was an excellent admin

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

istrator and delegated responsibility wisely, and therefore could take his summers off. Phil and his family used the summer months to visit a large number of different laboratories. Many of these laboratories were in the oil or related industries and gave Phil a much broader view of applied physical chemistry than that of many colleagues in soil science. Phil spent one summer in one author's (W.R.G.) laboratory in Riverside, California. Phil's thoroughness, tenacity, and objectivity soon became apparent. Over the course of the summer we argued in detail every point, every assumption, and every experiment either of us had ever published. It was clear that he was as rigorous a critic of his own work as that of any colleague, despite the belief of some colleagues that he was a difficult man to dissuade from his own point of view. Phil was an outstanding colleague and free in his praise of the work of others. His religious obligations helped make him into an outstanding public speaker, as well as a compassionate friend. He served as president of the Soil Science Society of America, on the Council of the Clay Minerals Society, and on the Highway Research Board. In all his activities Mayda's hospitality, charm, and warmth aided his career greatly as she supported it in unique and important ways.

He received Purdue University's Herbert Newby McCoy Award, the Soil Science Society of America's Research, and Bouyoucos awards. He was a distinguished member of the Clay Mineral Society and a fellow of the Soil Science Society and the Agronomy Society of America.

Phil was a widely sought lecturer and could fill only a fraction of the requests for lectures. He was one of the very first scientists in any discipline to be invited to lecture at length in the People's Republic of China after improvement of diplomatic relations with that country. As a result he was very influential in bringing a better understanding

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of the United States to the Chinese scientific community and its senior scientific leaders. He and Mayda made four trips to China and hosted a significant number of Chinese scientists, who became fast friends. His influence on science was felt well beyond the 23 Ph.D. and 15 postdoctoral students he supervised directly.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1957

With D. M. Anderson. The partial specific volume of water in bentonite suspensions. *Soil Sci. Soc. Am. Proc.*22: 22-24.

1961

Physical chemistry of clay-water interaction. *Adv. Agron.*13: 269-327.

1962

Effect of quasi-crystalline water on rate processes involved in plant nutrition. *Soil Sci.*93: 6-15.

Influence of absorbed water on exchangeable ion movement. *Clays Clay Miner.*9: 219-28.

1963

With J. D. Oster. Activation energy for ion movement in thin water films on montmorillonite. *Soil Sci. Soc. Am. Proc.*27: 369-73.

With R. J. Miller. Threshold gradient for water flow in clay systems. *Soil Sci. Soc. Am. Proc.*27: 605-609.

1964

With J. D. Oster. Heat capacities of clay and clay-water mixtures. *Soil Sci. Soc. Am. Proc.*28: 605-609.

1967

With D. M. Anderson and E. S. Gafney. Frost phenomena on Mars. *Science*155: 319-22.

1968

With P. F. Hockstra and D. M. Anderson. Some thermodynamic relationships for soils at or below the freezing point. I. Freezing point depression and heat capacity. *Water Resour. Res.*4: 379-94.

With P. Hockstra and D. M. Anderson. Some thermodynamic relationships for soils at or below the freezing point. II. Effects of temperature and pressure on unfrozen soil water. *Water Resour. Res.*4: 541-44.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

With B. G. Davey, K. W. Lee, and D. E. Baker. Clay soils versus clay gels: Biological activity compared. *Science*161: 897.

1972

With I. Ravina. Relation between swelling, water properties and ‘b-dimension in montmorillonite water systems. *Clays Clay Miner.*20: 109-23.

With B. D. Kay. Pressure-induced changes in the thermal and electrical properties of clay-water systems. *J. Colloid Interface Sci.*40: 337-43.

1975

With B. D. Kay. Heats of compression of clay-water mixtures. *Clays Clay Miner.*23: 266-71.

1976

Viscosity of interlayer water in montmorillonite. *Soil Sci. Soc. Am. J.*40: 500-505.

With D. M. Clementz. Thermal expansion of interlayer water in clay systems. I. Effect of water content. *J. Colloid Interface Sci.*3: 485-502.

1979

With J. F. Margheim. The swelling of clay. I. Basic concepts and empirical equations. *Soil Sci. Soc. Am. J.*43: 473-481.

Nature and properties of water in montmorillonite water systems. *Soil Sci. Soc. Am. J.*43: 651-58.

1980

The swelling of clay. II. Montmorillonites. *Soil Sci. Soc. Am. J.*44: 667-76.

1981

The swelling of clay. III. Dissociation of exchangeable cations. *Soil Sci. Soc. Am. J.*45: 1074-78.

With B. E. Viani and C. B. Roth. Direct measurement of the relation between interlayer force and interlayer distance in the swelling of montmorillonite. *J. Colloid Interface Sci.*96: 229-44.

With D. J. Mulla and J .H. Cushman. Molecular dynamics and statis

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
tical mechanics of water near an uncharged silicate surface. *Water Resour. Res.*20: 619-28.

1986

With Y. Sun and H. Lin. The non-specific interaction of water with the surfaces of clay minerals. *J. Colloid Interface Sci.*112: 556-64.

1990

With Z. Z. Zhang, J. H. Cushman, and C. B. Roth. Adsorption and heat of adsorption of organic compounds on montmorillonite from aqueous solution. *Soil Sci. Soc. Am. J.*54: 59-66.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ROBERT FRANKLIN MEHL

March 30, 1898–January 29, 1976

BY C. S. SMITH AND W. W. MULLINS

ROBERT FRANKLIN MEHL played a vital role in the transition of nineteenth-century metallurgy into the much broader field of materials science and engineering, which combines structural and physical approaches to the nature and use of materials with the earlier chemical-analytical framework. His contributions were at several levels: partly in the research he himself did, partly in his effective advocacy of a more fundamental approach to materials, and partly in his establishment of a new concept for a curriculum for the education of metallurgists. According to one of his closest associates, F. N. Rhines, Mehl's strongest points were: “(1) ability to identify and exploit areas ripe for development, (2) ability to inspire deep interest in scientific pursuits, and (3) foresight in developing the curriculum in physical metallurgy.”

Robert Franklin Mehl was born in Lancaster, Pennsylvania, on March 30, 1898. His grandfather had emigrated from the vicinity of Munich following the revolution of 1848. His father, whose formal education terminated before high school, became a manager in a Lancaster department store. His mother, May Ward, was born in Columbia, Pennsylvania, of English and German parentage. On December 27,

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1923, Mehl married Helen Charles. They had three children: Robert F., Jr., Marjorie, and Gretchen. Mehl died on January 29, 1976.

Although of modest means, Mehl's parents encouraged his advanced education and permitted him to have a small laboratory in the basement of their home when he was about twelve, an experience that marks his first recollections of an interest in science. He attended Franklin and Marshall College in his hometown. Living in his parents' home, he worked weekends and vacations in department and drug stores to meet college expenses. He expected to begin a job as an analytical chemist after two years of college, but a teaching assistantship enabled him to graduate near the top of his class in 1919. He participated in athletics and was interested in art; the hobby of oil painting continued throughout his life. His main interests, however, were science and literature. He read widely and he often attributed part of his early interest in science to reading. Although he frequently expressed regret that he did not develop a proficiency in foreign languages, he translated Tammann's book *Aggregation* (Aggregatzustände)[1] from German into English in 1925. He began research in his senior year at college, although a senior thesis was not required at that time. Mehl acknowledged great indebtedness to the then head of the chemistry department at Franklin and Marshall, Professor Herbert Beck, who encouraged him to pursue chemistry as well as his literary interests.

Mehl was granted a research assistantship and fellowship at Princeton University in 1920 and obtained his Ph.D. there in 1924 under Professor Donald P. Smith. Professor Smith had taken his doctoral work at Göttingen University with the renowned Gustav Tammann under whose influence a large fraction of the leaders of metallurgical research beginning in the 1920s were trained. Mehl's thesis topic was

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the electrical properties of aluminum-magnesium alloys. So began Mehl's transition from chemistry to metallurgy. Although he later tended to be somewhat disparaging of his thesis, he described Princeton as a “wonderful place to do graduate work in the 1920s” and, connecting the Princeton ambiance with his later career, he noted that “research and scholarship standards were high, and graduate student interest in and enthusiasm for research were extremely high. Remembering that scene was of immense help in later years at CIT [Carnegie Institute of Technology].”[2]

From 1923 to 1925, overlapping his degree work at Princeton, Mehl taught chemistry at Juniata College, where he also served as department head. In 1925 he was appointed a National Research Council fellow at Harvard University for two years. He worked with T. W. Richards on the relation between the compressibility and chemical affinity of alloys. Although his early papers were almost all published in the *Journal of the American Chemical Society,* his transition toward physical metallurgy continued. It was the interest of A. Sauveur (then also at Harvard) in Widmanstätten structures in meteorites and in medium carbon steels that brought to Mehl's attention the field of orientation relationships in solid state precipitation. His later contribution to this field was the first work of his to be widely recognized.

The U.S. Naval Research Laboratory (NRL), which had been founded a few years earlier, was looking for a head of the new Division of Physical Metallurgy who would be well versed in science and interested in doing basic research rather than practical metallurgy. They selected Mehl for this post in 1927. In building up his small staff at the NRL Mehl brought from Chicago Charles S. Barrett, who was then working on X-ray scattering in gases. Barrett was soon to become internationally famous; his book,[3] whose later

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

editions were coauthored with T. B. Massalski, is still one of the most quoted texts in the field.

Mehl and Barrett collaborated most effectively and the laboratory soon became well known in metallurgical circles for a series of nine papers on the Widmanstätten structure. Although the structure had been observed for a long time in many systems, the mechanism was being newly studied under the impact of Merica's theory of precipitation hardening[4] ; the latter effect had been empirically discovered by Wilm in 1904 in aluminum-copper alloys and was experimentally exploited in alloy systems for many years before any understanding had developed.

The key to the work of Mehl and Barrett on the Widmanstätten structure was the concept of structural matching on the habit plane between the parent phase and the Widmanstätten precipitate. The orientation of the parent grain was determined by X-ray diffraction, which was then an exciting new method of analyzing the crystal structure of metals and alloys. Mehl and Barrett (1931) then were able to deduce the conjugate habit plane in the matrix phase by measuring the number of precipitate plate directions in an individual grain in a polycrystalline material. For example, four distinct directions indicate {111} habit planes in an FCC parent phase. Their work disproved a view long held by metallurgists that precipitate alignment followed “cleavage planes” of the matrix because they showed that the precipitate plane was not necessarily the same for different precipitates. Thus they developed the important concept of structural matching. Although some of their conclusions have been modified, the influence of their approach was enormous. Some years later the field moved to a mature stage with hundreds of papers.

A more practical study in the early 1930s (1930) did much to establish Mehl's reputation, along with that of the NRL.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

This was the use of gamma ray radiography for the in situ study of large steel castings, in particular, the stern post castings of navy heavy cruisers; the poor as-cast structures were causing severe problems. A gamma ray source placed inside the hollow post with film wrapped around the outside yielded photographs showing such casting defects as shrinkage cracks and blowholes as well as failures in welding. This work created a great sensation in engineering and practical metallurgical circles. It earned Mehl immediate recognition in the Society for Non-Destructive Testing. In 1943 he received the Medal of the American Industrial Radium and X-ray Society for the work.

Mehl's real interests, however, were in the science underlying problems of industrial importance. He devoted much time to establishing contacts with the metallurgical industry and with leading industrial metallurgists and he played a very prominent role in the activities of the two principal metallurgical societies in the United States, the American Institute of Mining and Metallurgical Engineers (AIME, from 1956 the American Institute of Mining, Metallurgical and Petroleum Engineers) and the American Society of Metals, (ASM, now the American Society of Metals International).

After four years with the NRL Mehl tried to carry his research philosophy into industry as assistant director of the Research Laboratories of the American Rolling Mill Company in Middletown, Ohio. The effect of the great depression in 1929 and his own interests, however, combined to make him leave in 1932 after only one year. His wife, talking retrospectively of this year in industry, remembers it as an unhappy one, although his colleagues believed that it strongly reinforced his view on the necessity of relating scientific work to industrial problems.

Upon leaving the American Rolling Mill Company, Mehl accepted an appointment as professor of metallurgy at the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Carnegie Institute of Technology (CIT) and director of the reorganized Metals Research Laboratory (MRL) in 1932. The administration at CIT had recognized as early as 1924 the importance of research as well as education. The MRL had already done notable research mainly under the leadership of such men as V. N. Krivobok and Cyril Wells. Mehl increased the scope and made the work attractive to local industry by attracting lively young research people to the MRL and reinforcing the educational function of the laboratory.

In 1935 Mehl was appointed head of the Department of Metallurgical Engineering at CIT, a post he held until his retirement in 1960. The research of the MRL and the Department centered on the areas of solid-state reactions, diffusion, precipitation, plastic deformation, preferred orientations, and oxidation. In later work, he and his colleagues clearly separated the role of nucleation from that of growth of new phases in solid-state transformations and developed theories applicable as well to recrystallization as a result of plastic deformation. A central conclusion of this work was the now famous Johnson-Mehl-Avrami-Kolmogorov equation set forth in the 1939 paper by Mehl and Johnson (also obtained independently by Avrami[5] and Kolmogorov[6]) describing the volume fraction of a solid transformed in terms of the formation rate and spatial distribution of nuclei and the subsequent growth of the nuclei. All of this work brought international recognition to Mehl and his associates.

Mehl enjoyed doing broad surveys of research fields in both temporal and intellectual frameworks. He wrote annual reviews of theoretical metallurgy in the early 1930s that had a major influence on research undertaken in other laboratories as well as his own. When invited to give the prestigious annual Institute of Metals Division lecture in 1936, he did an in-depth summary on the current status of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

the field of diffusion, which provided many research topics for students and prompted a vast increase in studies of the fundamental processes underlying diffusion in laboratories throughout the world. These review articles and invited lectures as well as his *Brief History of the Science of Metals*[7] were the major publications under his sole authorship after the beginning of the MRL period. His best original research was always done in collaboration with colleagues or students; his contributions to this work were major.

Mehl's great ambition, drive to have an impact, and combative tendencies interfered on occasion with his scientific professional judgment. This seemed to lead him into scientific controversies that often became personal and strident. Two famous examples are the campaigns he waged against the concept of dislocations and against the role of vacancies in diffusion, especially as manifested by the Kirkendall effect (movement of inert markers in a diffusion couple providing evidence for a vacancy mechanism of diffusion). According to associates, he regarded dislocations and vacancies as fanciful inventions of physicists intruding into his domain of metallurgy and discouraged the faculty from mentioning these concepts in the classroom and at meetings.

In the case of vacancy diffusion, he was persuaded by friends of Kirkendall not to reject for publication the now classic work by Kirkendall and Smigelskas,[8] which he had held up for half a year as chief reviewer, but rather to allow publication and to submit discussion to the paper setting forth his objections. Mehl did so and then undertook with a Brazilian graduate student L. C. C. da Silva a study of inert marker movement in metallic diffusion couples of several binary alloy systems. The study proved to be a classic confirmation of the Kirkendall effect. At first, Mehl held up the thesis, still believing the results to be wrong, until

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

colleagues persuaded him to recant. The results of the study were published in 1951. Many years later, when Mehl was confined to bed, he apologized to Kirkendall, who made a personal visit, and whom he told he wished he had an important effect named after him. The history of this controversy is discussed in an article by Hideo Nakajima[9] and a subsequent response by da Silva.[10] A positive benefit of Mehl's passionate stance on these issues was the focused motivation he generated for himself and colleagues to resolve the disputes by incisive research.

Mehl's greatest contribution to his profession was arguably the establishment of new standards for the metallurgical profession both as a whole and particularly in the universities. He took a deep interest in the development of proper curricula, both undergraduate and graduate. The pillars of the curriculum developed under his leadership were fundamental courses in crystallography, phase diagrams and phase transformations, and the mechanical behavior of metals on a macroscopic scale. Although not an adept mathematician himself, Mehl encouraged more advanced mathematical education and analysis on the part of his students. At the time this emphasis on the scientific foundation of the subject constituted a revolutionary approach to university education in metallurgy.

Mehl was widely held in high esteem as an outstanding lecturer, both in university courses and in professional talks around the world. His colleagues described the hours he would often spend in preparation and rehearsal for just one lecture. His delivery was smooth, theatrical, and inspiring.

Mehl's students came to occupy a prominent position in the metallurgical profession and the curriculum he advocated was widely copied. At one time about a quarter of the heads of metallurgy and material science departments in

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the United States and Canada were his former students or faculty colleagues. His laboratory attracted many students and visitors from abroad and his influence grew to a world-wide scale. In retrospect, one can see the impact of the MRL under Mehl's directorship as marking a turning point in the history of physical metallurgy.

He developed a particularly close connection with Brazil, spending a year at Sao Paulo Universidad helping to organize the Brazilian Metallurgical Society and establishing the framework for metallurgical education in Brazil. The Portuguese edition of his lectures were published in book form.[11]

Mehl maintained an active and lucrative consulting business with such corporations as DuPont, United States Steel, Convair, and Thompson Ramo Woolridge. He also served effectively on many governmental advisory committees and professional committees. In 1945 he was attached to the U.S. Embassy in London to work with the Technical Intelligence Investigating Committee of the Joint Chiefs of Staff, and visited various German centers of metallurgical research in the wake of the U.S. Army. For this purpose, he was given the simulated rank of brigadier general with uniform. He took his usual firm stance in arranging these visits and tolerated no barriers.

Mehl was chairman of the Ship Steel Committee at the beginning of engineering and industrial research of the National Research Council in 1950 when the cracking of Liberty ships during World War II service was still an unresolved issue. He was chairman of the Minerals and Metals Advisory Board in 1951. Perhaps his most notable government service was his chairmanship of the Visiting Committee of the National Bureau of Standards, during which he strongly supported Director Alan Astin in 1953 against the commercially motivated attack in the famed battery acid case. Secretary of Commerce Weeks fired Astin, accusing

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

him of interfering with the marketplace by issuing a report stating that the storage battery additive AD-X2 was not effective in reviving old batteries. A major furor in support of Astin arose in the scientific community. Weeks requested the bureau's Visiting Committee to nominate a successor to Astin. In a surprise move, the committee, under Mehl's bold leadership, nominated Astin, which forced Weeks to reverse his position and rehire Astin.

From 1934 to 1958 he received numerous honors beginning with what is now the Matthewson Medal of the Metallurgical Society of the AIME, which he received five times between 1934 and 1947; the Howe Medal of the ASM (1939); the gold medals of both the ASM (1952) and the AIME (1945); the Le Chatelier Medal of the Société Française de Metallurgie (1956); four honorary doctorates; and election to the National Academy of Sciences in 1958. Despite these honors, Mehl seemed to have felt that his great contributions to education were not properly recognized and he was bitterly disappointed not to have been offered the presidency of CIT after Doherty's tenure in that office. A man of strong opinions openly expressed, Mehl had engendered the opposition of key decision makers to his appointment. He did become dean of graduate studies in the College of Engineering and Science at CIT from 1953 to 1960.

In 1960 he left CIT to become consultant to the United States Steel Corporation. He lived in Zurich, where he served as a liaison officer between the company and European metallurgists and industrialists. It is well known that when asked to recommend someone for this post, he recommended himself. His strong personality served to open the doors of many European laboratories that had previously been reluctant to admit visitors from U.S. industry. Returning to the United States in 1966, he became briefly a

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

visiting professor at the University of Delaware and at Syracuse and then returned to Pittsburgh.

During his years as head of the Department of Metallurgical Engineering and the MRL, Mehl's authoritarian style—on the model of a European professor—resulted in a wide range of strong attitudes on the part of faculty and students toward his leadership. He insisted on high standards and on a focus on the core issues in metallurgy. This led to discussions with students, for example, that have been described as exciting, interactive, and crucial to the development of ideas and to making them see the beauty and importance of the developing field of scientific metallurgy. A vignette that gives an additional indication of the quality of Mehl's leadership was related by B. Lustman, one of his distinguished students. Lustman's apparatus for measuring vapor pressures had broken down rather catastrophically, resulting in bad burns on his arms. In response, Mehl spent an entire evening at the bench with Lustman putting the apparatus back together with considerable enjoyment and dexterity, inspiring Lustman to continue with renewed enthusiasm.

On the other hand, students have remarked that once Mehl had studied a field in depth, discussed it with them, and had formed his own opinion as to the importance of certain directions of research and the probable outcome, he tended to oppose continued originality on the part of students. Once the thesis topics had been selected, deviations were discouraged. Further, once he felt he understood a problem well enough for his own satisfaction and was moving on to other things, he became rather impatient with students who deviated from his view.

Similarly, faculty members were encouraged to adopt the Mehl view on research directions and on controversial topics in classroom presentations and at meetings. Neverthe

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

less, he inspired great loyalty. He always prized a cable sent by the faculty to him in London before a major address that read: “Stand up there and give them hell.”

Mehl expected hard work. “You can't be a scientist on eight hours a day” was his stated principle from his Naval Research Laboratory days onward, and he attracted associates who felt the same way. Students referred to themselves as Saltminers as a badge of honor. The Saltminers, comprising present and former faculty and graduate students of the Metallurgy/Materials Department at Carnegie Mellon University (formerly CIT), to this day meet at the annual fall meeting of the AIME for fellowship and a dinner where stories of the old days under Mehl inevitably emerge.

Mehl's view of metallurgy as a connected whole from smelting to the physics of the final use made him unwilling to share the interest of many of his colleagues in materials broadly. Even though his slant of mind was more like that of a physicist than most of the members of the profession, he seemed rather to have resented the intrusion of metal physics into physical metallurgy and did not develop close professional relationships with physicists, either individually or institutionally. He opposed the move toward the newly oriented field of material science and engineering that began to replace metallurgy in universities around 1960, believing this move was both a hollow gimmick to obtain funding and unwise in view of the specialized knowledge required for the study of each major type of material (e.g., metals, ceramics, semiconductors). Nevertheless, he undoubtedly played a central and essential role in preparing the ground for the benefits of this broader view of materials.

Just before leaving for Zurich, Mehl summarized his view of the profession in his Howe lecture, “Commentary on Metallurgy” (1960). He pointed out that throughout history every discipline has drawn from every other whenever possible and acknowledged that metallurgy draws heavily

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

from other disciplines; so, in a sense, Mehl was inter-disciplinary. He nevertheless maintained: "It has its own science; and it has its own rationale interrelating engineering and science."

Charles S. Barrett, with whom Mehl did his first research on alloy transformation and who was closely associated with him from 1933 to 1945, has remarked that "the momentum he generated toward a better basic understanding of physical metallurgical principles will last far longer than the specific findings in individual papers or committee reports." The present writers (especially C.S.S.) can attest to the truth of this not only on the basis of many stimulating discussions at technical meetings but even more in noticing how the viewpoint toward metallurgy first enunciated and demonstrated by Robert Franklin Mehl spread throughout the world and produced an orientation of metallurgists that enabled them to interact effectively with the very cutting edge of physics and chemistry.

Toward the end of his life, Mehl expressed the opinion that universities were inclining too much toward basic research alone and he asked "whether a university ambiance of pure science close to solid state physics could be conducive to interest in an industrial career." And he emphasized the importance of seeing the relationship of science to applied research, "for these two together and neither separately constitute the field and in this union lies the metallurgical mystique." Perhaps at the end of his life his estimate of the proportion of basic science in this union was rather less than those of his younger colleagues, but at the beginning of it he was far in advance of his profession and it was in very large measure his example and his educational innovations that changed the profession into its modern mode.

Mehl suffered from diabetes in later years and for the last decade he was confined to bed and wheelchair because of the amputation of both legs. He faced this hardship with

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

characteristic courage. During his confinement, he wrote a fascinating account of the department and laboratory he led for so many years entitled "A Department and a Research Laboratory in a University."[2] The review included the key people with whom he was most closely associated, their work, and some of Mehl's philosophy. He was visited by many world figures in the field of metallurgy during this period. He died in Pittsburgh on January 29, 1976.

THE AUTHORS ARE INDEBTED to H. I. Aaronson, C. L. McCabe, and H. W. Paxton for the very helpful comments and information they supplied but take full responsibility for the final version. W.W.M. is also indebted to his wife, June Mullins, for editorial suggestions and proofreading.

NOTES

1. G. Tammann. Aggregatzustände. Trans. R. F. Mehl as *States of Aggregation.* New York: Van Nostrand, 1925.

2. R. F. Mehl. A department and a research laboratory in a university. *Ann. Rev. Mat. Sci.* 5(1975):1-26.

3. C. S. Barrett. *The Structure of Metals.* New York: McGraw-Hill, 1943.

4. P. D. Merica, R. G. Waltenberg, and H. Scott. *Trans. AIME* 64(1920):41.

5. M. J. Avrami. *Chem. Phys.* 7(1939):1103; 8(1940):212; 9(1941):177.

6. A. Kolmogorov. Statistical theory for the recrystallization of metals. *Akad. Nauk S. S. S. R. Izv. Ser. Matem.* 1(1937):355.

7. R. F. Mehl. *Brief History of the Science of Metals.* AIME, 1948.

8. A. D. Smigelskas and E. O. Kirkendall. Zinc diffusion in alpha brass. *Trans. AIME* 171(1947):130-42.

9. H. Nakajima. Episode on the discovery of the Kirkendall effect. *J. Met.* 49 (6) (1997):15-19.

10. L. C. C. da Silva. A reflection on R. F. Mehl and the Kirkendall effect. *J. Met.* 50(8) (1998):6-7.

11. Associação Brasilieira de Metals. *Metallurgie do Ferro e do Aco,* 1945.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

The complete works of R. F. Mehl are available in the Mehl Library of Roberts Hall at Carnegie Mellon University, Pittsburgh, Pennsylvania.

1930

With G. E. Dean and C. S. Barrett. Radiography by the use of gamma rays. *Trans. Am. Soc. Steel Test*18: 1192-1237.

1931

With C. S. Barrett. Studies upon the Widmanstätten structure. I. Introduction. The aluminum-silver system and the copper-silicon system. AIME Tech. Pub. No. 353. *Trans. Inst. Met. Div.*93: 78.

With O. T. Marzke. Studies upon the Widmanstätten structure. II. The beta copper-zinc alloys and the beta copper-aluminum alloys. AIME Tech. Pub. No. 392. *Trans. Inst. Met. Div.*93: 123.

1932

With C. S. Barrett and F. N. Rhines. Studies upon the Widmanstätten structure. III. The aluminum-rich alloys of aluminum with copper and of aluminum with magnesium and silicon. *Trans. Inst. Met. Div.*99: 203-33.

1933

With C. S. Barrett and D. W. Smith. Studies upon the Widmanstätten structure. IV. The iron-carbon alloys. *Trans. I. S. D.*105: 215.

1936

Diffusion in solid metals. Annual Inst. Met. Div. Lecture. *Trans. AIME Inst. Met. Div.*122: 11.

With M. Gensamer. Preferred orientations produced by cold rolling low-carbon sheet steel. AIME Tech. Pub. No. 704. *Trans. I. S. D.*120: 277.

1938

With F. N. Rhines. Rates of diffusion in the alpha solid solutions of copper. AIME Tech. Pub. No. 883. *Trans. AIME Inst. Met. Div.*128: 185.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

The physics of hardenability. The mechanism and rate of decomposition of austenite. Reprinted from Hardenability of Alloy Steels, pp.1-65, ASM symposium held October 1938.

1939

With W. A. Johnson. Reaction kinetics in processes of nucleation and growth. *AIME, Iron and Steel Div.*135: 416-42, discussion, pp. 42-58. (Tech. Pub. No. 1089).

With L. K. Jetter. The mechanism of precipitation from solid solution. The theory of age hardening, pp. 342-438. American Society of Metals Symposium on Precipitation Hardening held October 1939.

1941

With C. S. Barrett and A. H. Geisler. Mechanism of precipitation from the solid solution of silver in aluminum. *AIME, Inst. Met. Div.*143: 134-42, discussion pp. 148-50. (Tech. Pub. No. 1275).

The structure and rate of formation of pearlite. Campbell Memorial Lecture. *Trans. Am. Soc. Met.*29: 813-62.

1942

With G. E. Pellissier, M. F. Hawkes, and W. A. Johnson. The inter-lamellar spacing of pearlite. *Trans. Am. Soc. Met.*30: 1049-89.

With F. C. Hull. The structure of pearlite. *Trans Am. Soc. Met.*30: 380-425.

1943

With G. A. Roberts. The mechanism and the rate of formation of austenite from ferrite- cementite aggregates. *Trans. Am. Soc. Met.*31: 613-50.

1945

With W. A. Anderson. Recrystallization of aluminum in terms of the rate of nucleation and the rate of growth. Am. Inst. Min. Eng., Metals Tech. 12. Tech. Pub. No. 1805: 1-28.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1948

With A. G. Guy and C. S. Barrett. Mechanism of precipitation in alloys of beryllium in copper. *AIME Met. Div.* 175: 216-38, discussion pp. 238-39 (Tech. Pub. No. 2341).

The decomposition of austenite by nucleation and growth process. Hatfield Memorial Lecture. *Iron Steel Inst. J.*159: 113-29.

1950

With C. Wells and W. Batz. Diffusion coefficient of carbon in austenite. *Trans. AIME*188: 553.

1951

With L. C. C. da Silva. Interface and marker movements in diffusion in solid solutions of metals. *Trans. AIME*191: 155-73.

1953

With L. Himmell and C. E. Birchenall. Self-diffusion of iron in iron oxides and the Wagner theory of oxidation. *Trans. AIME*197: 827-43.

With R. F. Bunshah. The rate of propagation of martensite. *Trans. AIME*197: 1251.

1956

With W. C. Hagel. The austenite:pearlite reaction. *Prog. Met. Phys.*6: 74-134.

1960

Commentary on metallurgy. Howe Memorial Lecture (invited). *Trans. Met. Soc.* 218: 386-95.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ROBERT SANDERSON MULLIKEN

June 7, 1896-October 31, 1986

BY R. STEPHEN BERRY

ROBERT S. MULLIKEN WAS a quiet, soft-spoken man, yet so single-minded and determined in his devotion to understanding molecules that he came to be called "Mr. Molecule." If any single person's ideas and teachings dominated the development of our understanding of molecular structure and spectra, it surely was Robert Mulliken. From the beginning of his career as an independent scientist in the mid-1920s until he published his last scientific papers in the early 1980s, he guided an entire field through his penetrating solutions of outstanding puzzles, his identification (or discovery) and analysis of the new major problems ripe for study, and his creation of a school—the Laboratory of Molecular Structure and Spectroscopy or LMSS at the University of Chicago, during its existence the most important center in the world for the study of molecules.

Robert's background led him naturally into academic science. He was born in Newburyport, Massachusetts, in a house built by his great-grandfather in about 1798. His father, Samuel Parsons Mulliken, was a professor of chemistry at MIT, which made him a daily commuter between Newburyport and Boston. Samuel Mulliken and his childhood friend and later MIT colleague Arthur A. Noyes were

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

strong influences stirring Robert's interests in science. As a high school student, Robert decided against philosophy as a career and opted for science. He attended MIT as an undergraduate, receiving his B.S. in chemistry in 1917. He then took on a wartime job studying poison gases in a laboratory at American University under the direction of a certain Lieutenant James Bryant Conant, then of the Chemical Warfare Service. Mulliken entered the Chemical Warfare Service himself, rising to private first class, but left the service when he contracted influenza in 1918. When he recovered, he worked for the New Jersey Zinc Company until he entered graduate school at the University of Chicago in the fall of 1919.

As a graduate student in chemistry at Chicago, Robert worked under the direction of W. D. Harkins, first on surface tension and then on isotope separation, particularly of mercury isotopes. The method used in his thesis was “irreversible evaporation” and distillation. Robert found that a dirty surface on the mercury aided the separation considerably; this concept, later called a boundary layer or diffusion membrane, played an integral role in the Manhattan Project. Robert conceived and tried centrifugation, but as he said fifty-five years later, the centrifuge then was simply too crude. He also considered photochemical separation, but never published anything on the subject.

At Chicago, Robert became interested in the interpretation of valence and chemical bonding through the papers of Irving Langmuir and G. N. Lewis. He encountered the old quantum theory through two enthusiastic courses of lectures by Robert A. Millikan, but was uneasy about the theory; “a disorganized chaos” was the description Mulliken used for it in reminiscences written in 1965. Nevertheless, Robert succeeded in applying it in 1924-25 to the interpretation of a particular molecular spectrum, assigned initially

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

by Wilfred Jevons to the boron nitride molecule, BN. Mulliken showed that the spectrum was that of boron oxide despite the preparation involving no apparent oxygen-containing substances. Jevons was pressed by a zealous department head to publish a note insisting on the initial assignment. Then, at the urging of R. T. Birge, Mulliken wrote directly to Jevons, visited him in England in 1925, and the two men settled the matter amicably and remained friends thereafter.

Robert held a National Research Council Fellowship at that time and was working at Harvard after completing his Ph.D. in 1922. He had wanted to study beta-ray spectroscopy with Ernest Rutherford at Manchester, but the fellowship board felt that his physics background was not strong enough and urged him to select a more chemical topic. Consequently he carried out many experiments in molecular spectroscopy largely under the guidance of E. C. Kemble and F. A. Saunders. At that time, a coterie of young, enthusiastic American scientists grew up in Cambridge, a group including Mulliken, Samuel Allison, F. A. Jenkins, J. R. Oppenheimer, John Van Vleck, Gregory Breit, Harold Urey, and John Slater. They were not all in Cambridge at the same time, but for the most part they knew one another, and there were several close friendships among them.

Like most of that group, Mulliken made his early pilgrimage to Europe in the summer of 1925. This was just the threshold time of modern quantum mechanics. Robert, like several of his contemporaries, had been trying to give organization to the states and spectra of diatomic molecules. This subject was, from his later reminiscences, a lively part of many of the discussions he had with colleagues and distinguished senior scientists in London, Oxford, Cambridge, Copenhagen, and perhaps most important, Göttingen. There he met Max Born, James Franck (who, of course, later joined the faculty of the University of Chicago), Otto Oldenberg,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Hertha Sponer, V. Kondratiev, V. I. Semenov, A. Terenin, and especially Born's assistant Fredrich Hund. The relationship between them became one of the most fruitful in the twentieth century in the history of the interpretation of the structure of matter and the nature of chemical bonds.

Even in 1925, a year before the first papers were published on quantum mechanics, Mulliken and Hund began to conceive an analogue for molecules of the “building-up principle” or “Aufbauprinzip” introduced by Niels Bohr to explain the structures of atoms and the Periodic Table. Their notion was that electrons in molecules would have quantized orbits like those introduced by Bohr and developed by Sommerfeld. These orbits would define successive shells like their atomic counterparts. However, the orbits in molecules would extend throughout the molecule, encircling two or more nuclei. After their meeting, Mulliken and Hund corresponded and both published on the subject in 1926 and 1927. But, as soon as they knew of the matrix mechanics of Heisenberg and the wave mechanics of Schrödinger, both realized that would be the correct direction for them. Mulliken probably learned first about Heisenberg's work from a lecture in 1926 by Max Born. He felt quite inadequately trained, especially in mathematics, for this new kind of physics—although it seems now like something he could have learned in a week or two. Schrödinger's formulation, which was based on the second-order differential equations that everybody learned, “was somewhat of a relief that it wasn't so bad.”

Mulliken returned to Göttingen in 1927, after the hydrogen atom had been worked out by Pauli with matrix mechanics and by Schrödinger with wave mechanics. That summer was the time Hund and Mulliken worked out their basic interpretation of the spectra of diatomic molecules and their generalization of atomic orbitals, the standing-

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

wave, stationary states of electrons in atoms, to "molecular orbitals," the molecular counterparts. Robert's strengths were a deep knowledge of molecular spectra and a capacity to invent phenomenological and empirical interpretations; Hund brought quantitative and mathematical insights, a greater mastery of the new theories, and a specific vector model for quantum systems. They shared a view of stationary states of electrons in molecules and of the analogy between atoms and molecules. By 1928 they had both written their first papers that went beyond the old quantum theory, and molecular orbitals were born. Remarkably, especially in light of their long friendship and profound mutual respect, the two men never published a joint paper.

Also during the summer of 1927 in Zürich, Mulliken met Schrödinger, whose chair, Mulliken recalled later, collapsed spontaneously during their conversation. Schrödinger then introduced him to W. Heitler and F. London, who were just developing their electron pair theory of the chemical bond. This approach, close to Langmuir's and Lewis's, was to become a rival to the molecular orbital approach until John Slater, some years later, showed that both were approximations and suitable starting points from which a common, accurate theoretical picture could be achieved. Upon seeing it for the first time, Robert was not enthusiastic. However, he was deeply involved in developing his own ideas and did not care to stop to learn, evaluate, and incorporate such different ideas from others. Linus Pauling and John Slater, however, quickly absorbed the ideas and the Heitler-London-Slater-Pauling valence bond theory became another item in the theorist's bag of tools. There were difficulties inherent in valence bond theory that did not appear in the Hund-Mulliken molecular orbital theory, which Mulliken recognized. Mulliken objected particularly to how "Pauling made a special point in making everything sound as simple

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

as possible and in that way making it [valence bond theory] very popular with chemists but delaying their understanding of the true [complexity of molecular structure]." Mulliken's respect for Hund and Slater endured throughout his life; he felt that his Nobel Prize should most properly have been shared with them.

Between the two trips to Europe, Mulliken became an assistant professor in the Department of Physics at New York University. In 1928 he refused the chairmanship of that department, feeling quite unfit for the job. He also refused a professorship in the Physics Department at Johns Hopkins offered by R. W. Wood. Instead, he accepted an associate professorship in the Physics Department under Arthur H. Compton at the University of Chicago. He acknowledged later that his decision was heavily influenced by the warm feelings he held toward Chicago from his days as a graduate student. The University of Chicago remained his academic home and Hyde Park his domicile until about two years before he died.

In the summer of 1929, Robert met Mary Helen von Noé, the beautiful daughter of a well-known professor of paleo-botany at the University of Chicago, the man who designed the underground coal mine at the Museum of Science and Industry. She, an aspiring water-colorist, and he, the brilliant, rising young physicist, were married on Christmas Eve of the same year. They later became parents of two daughters, Lucia and Valerie.

Robert held a Guggenheim Fellowship at that time and decided to split it into two six-month segments. The first, in the spring of 1930, must have been the honeymoon that Mary Helen claimed to be the birthing time of molecular orbital theory. Our chronology would probably put it almost three years earlier, in 1927. Among the many places on the itinerary, the 1930 trip took the couple to Leipzig, where

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Hund, Heisenberg, Peter Debye, and E. Hückel were, and Edward Teller too, then Heisenberg's assistant. Mulliken talked with them all, especially Hund and Teller, continuing the productive dialogue with Hund and engaging Teller, later a colleague at Chicago, in molecular problems, to which Teller later made a wide variety of very important contributions. Mulliken himself was deeply immersed in interpreting molecular spectra, writing a series of articles on the halogen molecules and another series for *Reviews of Modern Physics,* which gave molecular electronic spectroscopy the coherence he had been seeking since the early 1920s. Mulliken noted in 1965 that he did not bother to go to a "screaming, roaring speech" by Adolf Hitler.

The Mullikens used the second half of that Guggenheim Fellowship during the fall and winter of 1932-33. Heisenberg, Hund, and Teller were still in Leipzig. This time the atmosphere was distinctly more ominous; Hund was predicting the inevitability of Hitler's takeover. The same feeling pervaded the atmosphere in Göttingen and Berlin. A visit to Darmstadt with Gerhard Herzberg ended the German segment of the trip and cemented the long-standing, close relationship between the two men. (Herzberg later came to the University of Chicago before going to the National Research Council of Canada, the position with which he has been most identified.) The Mullikens left Germany and were in Austria on March 5, the day of Hitler's election victory; the next day, they crossed Germany to go to Amsterdam. Mulliken does not mention visiting Hund again until 1953 in Frankfurt; Hund had remained in Leipzig and then moved to Jena, both in East Germany, but was able to move to Frankfurt to accept a professorship there in about 1950.

Robert was engaged in the Manhattan Project at the "Metallurgical Laboratory" at the University of Chicago during World War II. He was one of the members of that group

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

who began early to explore the future consequences of nuclear weapons, and he continued to be active in his concerns regarding the use and control of nuclear energy. He and Eugene Rabinowitch were responsible for the inclusion in the Jeffries Report of a section on the need for international nuclear arms control. He and four other members of the National Academy of Sciences and the faculty of the University of Chicago—A. J. Dempster, James Franck, W. D. Harkins, and Sewell Wright—circulated the famous letter to the President endorsing the Rye Conference report, which took a position strongly opposing the May-Johnson bill to put very tight controls on all information as well as materials concerning nuclear energy. Much later, in the 1970s, he became interested in problems of population growth, arguing for NPG, his acronym for negative population growth.

Robert's profound influence on molecular science evolved partly through the several monumental series of articles he published, beginning in 1926 and continuing until the end of his active life in science in the early 1980s. The first, a series of eight papers from 1926 through 1929, on “Electronic States and Band-Spectrum Structure in Diatomic Molecules,” was designed to organize the subject; the series in *Reviews of Modern Physics,* “Interpretation of Band Spectra,” (1930-32) carries that analysis further, making it more encompassing and more penetrating. That series remains a standard text on the subject. In between, he wrote a three-paper series, “The Assignment of Quantum Numbers for Electrons in Molecules,” which shows the influence of Hund. Mulliken went beyond diatomic molecules with the long series—fourteen papers—entitled “Electronic Structures of Polyatomic Molecules and Valence,” which appeared between 1932 and 1935. A series of ten papers on intensities of electronic spectra appeared during 1939-40. After World War II, he wrote three more series. One dealt with the distribu

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

tion of electronic charge in molecules and its relation to chemical bonding. The next, which overlapped the charge distribution series in time, took Mulliken into an area altogether new for him, the spectra of molecules in solution. A puzzling spectrum of iodine dissolved in benzene was reported in 1949 by Joel Hildebrand and H. A. Benesi; Mulliken was tantalized by the observation and told Hildebrand, "I bet I can explain that spectrum." After one false start, he did explain it, in terms of what is now called a "charge transfer band," an intense spectral band system due to the production by light of two ions bound together from two neutral molecules. The insight that explained the iodine-benzene spectrum led to the series "Molecular Complexes and Their Spectra" and to a book, written with Willis Person. This series has had ramifications for many aspects of photochemistry and photobiology. The last series he wrote became remarkably influential, changing much of the interpretation of molecular spectra in the ultraviolet; this set of seven papers dealt with molecular Rydberg spectra, spectra in which one electron is excited to an orbit (strictly, orbital or standing wave state) large enough to be well outside the core formed by the nuclei and the other electrons.

Certain topics aroused Robert's interest early and intrigued him throughout his career. One pervasive theme was the spectrum and structure of ethylene and species related to it. He pointed out in 1935 that the lowest excited state of ethylene had to be a "triplet," a state in which the molecule is magnetic. The idea was not readily accepted, but eventually became a basic concept for the interpretation of not only the behavior of ethylene but of most small and medium-sized molecules. Mulliken was always adept at seeing connections between seemingly unrelated observations and systems. He recognized the close relationship of the molecules of ethylene, formaldehyde, and oxygen, and the differences

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

and similarities their spectra should (and do) show. He did miss one finding when he was interpreting the spectrum of oxygen in 1932. He left unassigned some weak lines, which W. F. Giauque and H. S. Johnston soon showed were due to the isotopes of oxygen with mass numbers 17 and 18 instead of 16. This led to the award of a Nobel Prize to Giauque. Thereafter, Mulliken was very careful to pay as much attention to weak bands as to strong ones!

His interest in simple olefins rekindled in the late 1970s. To pursue his new ideas, he went back to his own early papers, among others. One day he came to lunch very troubled; he thought he had found an error in one of his own early papers, considered a landmark. Two days later, he came again to lunch, this time much happier, to say, “It was all right after all. I was very clever in those days!”

Mulliken epitomized the eclectic in his scientific style. He considered himself neither a theorist nor an experimentalist—although he carried out both experimental and theoretical research—but an interpreter of observations. With this attitude, he was free to call on whatever techniques, ideas, or approaches seemed best suited for the problem at hand. Until the experimental work of his group closed down with his official retirement, his laboratory always had experimentalists studying electronic spectra of molecules. The basement of Eckhart Laboratory was the spectroscopy laboratory. Its several instruments included a very awkward home-made spectrograph for work in the far or “vacuum” ultra-violet and two other very large instruments, “Paschen circles” with 21- and 30-foot radii for the focal curve, literally using rather large rooms as the interiors of their cameras. These were used for fairly high resolution spectroscopy until the advent of laser techniques, which came into use just when the Laboratory of Molecular Structure and Spectroscopy (LMSS) was discontinuing experimental work.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Although LMSS and Robert Mulliken himself were a bit too early to participate at the leading edge of introducing the experimental methods that now dominate the field, the opposite was the case regarding the role of computations in molecular science. In 1950 Mulliken committed his group to the development of computational methods for finding molecular properties. He foresaw the role that computers could fill in transforming quantum mechanics of molecules from a formal analytic representation and a device for solving simple models into a quantitative tool with powerful predictive capabilities. In an article written in 1958 with his protégé and subsequent colleague Clemens Roothaan, he said, "Looking toward the future, it seems certain that colossal rewards lie ahead from large-scale quantum-mechanical calculations of the structure of matter . . . And gradually, reliable computations even of quantities now inaccessible or poorly accessible to experimental observation will come more and more into the picture . . . We think it is no exaggeration to say that the workers in this field are standing on the threshold of a new era."

The period from 1950 to 1958 saw a qualitative change in the way computations were done. In 1950 almost the only devices available to aid computations were electrically driven mechanical calculators; some laboratories still used hand-cranked calculators. By 1958 machines such as the IBM 650 and the larger, faster Remington-Rand 1103 and Univac Scientific were available to researchers in LMSS and some places elsewhere. This meant, in Mulliken's words in 1958, ". . . the entire set of calculations which took [Charles] Scherr (with the help of two assistants) about a year, can now be repeated in 35 min."; and we know that was only the beginning.

Mulliken was not alone by any means in his belief that computational molecular science was a large part of the

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

future of the field. His close friend from their postdoctoral days in Cambridge, Massachusetts, John C. Slater, was one of these; he founded a group at MIT in friendly rivalry with LMSS. Others with large, active groups included Masao Kotani in Tokyo and Per Olov Löwdin in Uppsala. S. F. Boys worked at Cambridge University in the U.K. environment, which at that time was one of skepticism toward elaborate computations; he had only an occasional student or postdoctoral associate, but made seminal contributions well recognized later. It was LMSS to which the pilgrimages were made. A striking majority of the important contributors to molecular theory and molecular computation spent some period as student, postdoctoral associate, or visiting faculty member in Robert Mulliken's group at the University of Chicago. One of Robert's favorite stories of this phenomenon concerns Professor Saburo Nagakura, later the director of the Institute for Molecular Science at Okazaki, Japan, and then a university president in Japan. Robert had written to Nagakura, already a professor, asking whether the latter had anyone he could recommend to come to Chicago as a postdoctoral associate to do experimental work. Nagakura replied by asking whether it would be all right if he himself came in that capacity. So, in 1965-66, he did!

In that period when Mulliken became completely persuaded of the power of computation from first principles, his allies notwithstanding, there were other strong opinions in opposition. Those who believed in elementary models and simply calculable, semiempirical descriptions expressed deep reservations about the role of “big” calculations. They questioned both the feasibility of accurate computations for all but the simplest molecules and the extent of new physical insight that could be gained from a knowledge of elaborate wave functions and some predicted values of observables. One confrontation of the two factions occurred

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

at a conference in Boulder, Colorado, in 1960 at a conference whose proceedings were published in the October 1960 issue of *Reviews of Modern Physics.* The division of viewpoints now seems shortsighted, because it seems so clear today that both approaches have important uses. But at that meeting, Charles Coulson, professor of theoretical chemistry at Oxford, in his summary talk divided theoretical chemistry into two populations: type 1, which believed the future lay with computations, and type 2, which chose simple and semiempirical models. Coulson, having made important contributions to both aspects, tried to be as tolerant as possible toward both, but his sympathies seemed to us young Americans to be with type 2, the favorite of almost all the British scientists except Boys and a few young iconoclasts. Mulliken, despite his belief in large-scale computation, straddled the field, continuing to carry out simple interpretive studies; there were often people working in LMSS on semiempirical models.

Chicago and LMSS became, ultimately, the world's most important center for molecular computations. The facilities were remarkably good; when I asked Enrico Clementi in the mid-1960s about the quality of the computing facilities at IBM (where he then was) and at Chicago, Enrico said without hesitation that Chicago's were the best in the world; “after all,” he said, “you are customers!” Clemens Roothaan was in charge of the Computation Center; always a zealous believer that users should understand how their machines operate, he was a strong, encouraging influence for aspiring scientists for whom such knowledge would enable them to use computers at the limits of their capabilities, but seemed something of an ogre to users who wanted computers to be black boxes operating with reliable “canned” programs. The students in LMSS typically became very skilled

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

programmers, in addition to well-educated molecular scientists.

Mulliken himself left the programming and the machine operations to others until 1970, when he spent a summer working at the IBM laboratory in San Jose, California. This laboratory had collected several alumni of LMSS—Douglas McLean, Clementi, Yoshimine, and Bowen Liu, a group known as the "Chicago Mafia." Robert learned to write and execute programs that summer, at age seventy-four. He had, of course, done roughly the same kinds of computations by hand years before. But the power of the computer enabled him and everyone else to realize some of the accuracy that he and Roothaan had anticipated. Sometimes the results were counterintuitive, at least counter to the intuitions we had all built up during the pre-computer years. At lunch at the Quadrangle Club early that fall of 1970, shortly after his return, Robert turned to me and said, with the naive wonderment so characteristic of his discussions, "You know, I don't think I understand molecular orbitals very well." This, from one of the three people who did most to develop the concept of molecular orbitals and integrate them into all the thinking about molecular structure since the late 1920s.

The roll of scientists who worked in his group illustrates what an institution Robert Mulliken created. When LMSS was established, Robert was "big boss," Clemens Roothaan was "little boss," and Bernard Ransil was "straw boss" while Ransil was there. Others who were in the group at one time or another included W. C. Price, Christopher Longuet-Higgins, Harrison Shull, Michael Kasha, Klaus Ruedenberg, Yoshio Tanaka, Harden McConnell, Norbert Muller, Robert G. Parr, Gerhard Herzberg, Alf Lofthus, Philip G. Wilkinson (who was primarily responsible for the vacuum ultraviolet spectroscopy), Leslie Orgel, John Platt, Hiroshi Tsubomura,

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

T. Namioka, John Murrell, P. K. Carroll, A. C. Wahl, Paul Bagus, Willis B. Person, Anthony Merer, Joel Tellinghuisen, Marshall Ginter, Paul Cade, Juergen Hinze, and Marshall Ginter, as well as Scherr, Nagakura, Clementi, McLean, and Yoshimine. Robert enjoyed learning equally from all his faculty colleagues, whether roughly contemporaries like Weldon Brown and G. W. (Bill) Wheland or the most junior members. Because he lunched almost every day at the Quadrangle Club, usually with either the physicists or the chemists, he was as much a friend of his youngest colleagues as he was of the most senior members of the faculty. After he retired, he became more and more open and expressive of the feelings toward others that he had been reluctant to reveal in his younger days. He once described to me how he went through a personal realization of this, by saying, “That's when I became human.”

THE AUTHOR THANKS Michael Kasha for his helpful comments and for the photograph that accompanies this memoir.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1921

With W. D. Harkins. The separation of mercury into isotopes. *Nature* 108: 146.

1924

The isotope effect in line and band spectra. *Nature* 113: 820.

1926

Systematic relations between electronic structure and band-spectrum structure in diatomic molecules. *Proc. Natl. Acad. Sci. U. S. A.*112: 144-51 .

1928

The assignment of quantum numbers for electrons in molecules. I. *Phys. Rev.*32: 186-222 .

1930

The interpretation of band spectra. Parts I, IIa, IIb. *Rev. Mod. Phys.*2: 60-115 .

1932

Electronic structures of polyatomic molecules and valence. *Phys. Rev.*40: 55-71 .

1934

New electroaffinity scale: Together with data on valence states and on valence ionization potentials and electron affinities. *J. Chem. Phys.*2: 782-93 .

1942

Structure and ultraviolet spectra of ethylene, butadiene and their alkyl derivatives. *Rev. Mod. Phys.*14: 265-74 .

1947

With C. C. J. Roothaan. The twisting frequency and the barrier height for free rotation in ethylene. *Chem. Rev.*41: 219-31 .

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1949

With C. A. Rieke, D. Orloff, and H. Orloff. Overlap integrals and chemical binding. *J. Chem. Phys.*17: 510 .

With C. A. Rieke, D. Orloff, and H. Orloff. Formulas and numerical tables for overlap integrals. *J. Chem. Phys.*17: 1248-67 .

1950

With R. G. Parr. LCAO self-consistent field calculations of the p-electron energy levels of *cis*- and *trans*-1,3-butadiene. *J. Chem. Phys.*18: 1338-46 .

1951

With R. G. Parr. LCAO molecular orbital computations of resonance energies of benzene and butadiene with general analysis of theoretical versus thermochemical resonance energies. *J. Chem. Phys.*19: 1271-78 .

1959

Conjugation and hyperconjugation: A survey with emphasis on isovalent hyperconjugation. *Tetrahedron*5: 253-74 .

1964

The Rydberg states of molecules. Parts I-V. *J. Am. Chem. Soc.*86: 3183-97 .

1967

Electron-donor acceptor interactions and charge-transfer spectra. *Proc. R. A. Welch Foundation Conf. Chem. Res.*XI: 105-50 .

1969

With W. B. Person. *Molecular Complexes. A Lecture and Reprint Volume.* New York: John Wiley and Sons.

1970

The path to molecular orbital theory. *Pure Appl. Chem.*24: 203-15 .

1971

The role of kinetic energy in the Franck-Condon principle. *J. Chem. Phys.*55: 309-14 .

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1972

The nitrogen molecule correlation diagram. *Chem. Phys. Lett.*14: 137-40 .

1974

Through ZPG to NPG. *Bull. At. Sci.*30: 9 .

1975

*Selected Papers of Robert S. Mulliken.*D. A. Ramsay and J. Hinze, eds. Chicago: University of Chicago Press.

1977

With W. C. Ermler. *Diatomic Molecules. Results of ab Initio Calculations.* New York: Academic Press.

1978

Chemical bonding. *Annu. Rev. Phys. Chem.*29: 1-30 .

1981

With W. C. Ermler. *Polyatomic Molecules. Results of ab Initio Calculations.* New York: Academic Press.

1989 (POSTHUMOUS)

Life of a Scientist. B. Ransil, ed. Berlin: Springer-Verlag.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

WILLIAM D. PHILLIPS

October 10, 1925–December 15, 1993

BY ROBERT G. SHULMAN

WILLIAM DALE PHILLIPS, chemist, nuclear magnetic resonance spectroscopist, and science policy advisor, died at home in St. Louis, Missouri, on December 15, 1993.

Bill Phillips was born October 10, 1925, in Kansas City, Missouri, the first of the two children of Elmer and Mabel Phillips, long-time residents of Kansas City. He graduated from public high school in his hometown at the age of seventeen and entered the U.S. Navy V-12 program in 1943. At the navy's request he studied mechanical engineering at the University of Texas and was commissioned and left active duty in 1946 with the rank of lieutenant (junior grade). Bill returned to finish undergraduate studies at the University of Kansas in 1946 and received a B.A. in chemistry in 1948. For graduate studies he entered the MIT chemistry department. His research was on the vibrational spectra of organic molecules in Richard Lord's laboratory. In this laboratory he learned the nature and basis of spectroscopy, an understanding that enabled him to move freely across different applications of varied spectroscopies. He was in a physical chemistry laboratory that was trying to obtain vibrational and Raman spectra from biological molecules. At that time difficulties arose from the impurities present in

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

solution of “purified” enzymes. The difficulties of seeing through impurity and macromolecular scattering must have impressed themselves strongly on the young researcher; a few years later when he was in a position to obtain well-characterized spectra from biomolecules, he had a historical perspective on the significance of reliable spectra.

At MIT Bill met Esther Parker, a Wellesley College student, better known to her friends as Cherry, whom he married in 1951. Cherry was a loving partner and assisted Bill in his many adventures.

Upon leaving MIT in 1951 with his Ph.D. in physical chemistry, Phillips accepted a position in the Central Research Department of E. I. du Pont de Nemours and Company, Inc., in Wilmington, Delaware. Bill is widely remembered by the magnetic resonance community for the path-breaking work he did at the du Pont Co. Du Pont was to be his home base for almost 30 years and he rose through the ranks from research chemist to assistant and associate directorships of research and development.

Looking over Bill's contributions to magnetic resonance during this period, one is struck by the breadth, novelty, and depth of his work. No review would do justice to these original, creative applications of nuclear magnetic resonance (NMR) in its infancy, however a few selective observations may help to illustrate his great body of work.

Bill's first publication out of du Pont, a single authored letter to the editor in the *Journal of Chemical Physics* in 1955, described the use of ^{1}H NMR to demonstrate unequivocally the existence of restricted rotation in amides. The lead sentence reads: “Restricted rotation about the C-N bond of amides has been postulated and is an important consideration in theories pertaining to the structure of protein molecules.” Clearly in 1955, Bill was thinking about NMR as a tool for probing protein structure, an area that

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

would become his central focus in later years. But this was 1955, and his spectrometer ran at 40 MHz.

In 1957, again in the *Journal of Chemical Physics,* Bill announced in a letter to the editor: "We have observed anomalous shifts in the proton magnetic resonance spectra of alcohols complexed with paramagnetic ions." He goes on to postulate bond formation between the alcohol ligand and metal ion leading to subsequent delocalization of the unpaired electrons providing a finite density of unpaired electrons at the resonating nucleus. Chemical shifts induced by paramagnetic metal ions would, in time, become a powerful tool for structural elucidation and a major focus of his research.

He observed large shifts of proton resonances in alcohols complexed with Co^{2+} (or Ni^{2+}) and showed from their strong temperature dependence that they arose from anisotropic dipolar interactions between protons and the paramagnetic electrons (1957). He established this mechanism by extending an earlier analysis to these complexes. The anisotropic dipolar interactions, called by MacConnell "pseudo contact," gave large, easily measured shifts that were proportional to $1/r^3$, where r was the distance between the nucleus and the electron spins of the paramagnetic metal ion. Because the other parameters needed to calculate the observed shifts were generally known, the shifts provided an accurate measure of distances between nuclei in aqueous complexes in solution. This method was used many times in small molecular complexes by Bill, his colleagues, and in other laboratories, where it remains a most valuable structural method. However, in those early days before magnetic resonance imaging (MRI), it found its greatest usefulness in paramagnetic metal complexes of biological macromolecules, particularly proteins and nucleic acids. Since the electron g factors were known with respect to the molecular axes, measurements of the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

shifted positions of numerous proton resonances in a protein/metal complex could be interpreted to determine proton spatial coordinates. In this case the metal ion itself became the origin of the coordinate system, while the valuable information was the protein structure in solution. The ability to form complexes in solution of organic molecules with paramagnetic ions, with anisotropic electronic dipoles, provided the basis for shift reagents that subsequently became so useful (and profitable) in MRI applications. In later years Bill, in charge of research at Mallinckrodt, had to stand by as competitors earned millions from work that was derived from his pioneering studies. After a decade of development by Phillips and others, this method was the basis of a large-scale dedicated effort to determine the structure of the protein lysozyme.

The same 1957 experiments measured shifts in proton resonances when the alcohols were complexed with Mn^{2+} . As Mn^{2+} is isotropic, the shifts could not be explained by anisotropic dipolar fields so that another explanation was required. This Bill saw was the delocalization of spin density from the metal into molecular orbitals, including orbital wave functions of the hydrogens creating true hyper-fine shifts, sometimes called contact interactions. This measured the degree of chemical bonding with the metal and became an important method of describing the degree of covalency, a degree (even though small in accordance with the widely held view that transition metal bonds were ionic) that still measured a significant covalent contribution. In a series of papers in the 1960s, Bill Phillips and coworkers used isotropic contact proton hyperfine interactions to determine the configurations and magnetic properties of paramagnetic bis-nickel(II) chelates of amino-troponeimines. Spin densities from Ni transmitted through N, O, and S atoms connecting conjugated ligands produced

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

large high- and low-frequency chemical shifts of the protons on the ligands. Synthesis and study of compounds with various ligands allowed the determination of spin densities of a large number of aromatic, cyclic 7-carbon, alkyl, and fluorine substituents. Similar studies were made on nickel (II) salicylaldimines.

These findings were parallel to NMR experiments I was doing at Bell Telephone Laboratories on the ^{19}F resonances in transition element fluorides and very directly related to ESR experiments of the hyperfine interactions with protons of organic free radicals done in many laboratories but most significantly by Harden MacConnell. Although there was no occasion when the three of us were together at a meeting (the similarity of the mechanisms were obscured by the differences of techniques [solution NMR, solid state NMR, and ESR]), still in Bill's mind and mine (we acknowledged afterwards) we were watching closely each other's results and, at least in my case, watching with admiration. These earlier studies of paramagnetic complexes were the basis of Bill's future studies of shifted resonances in the paramagnetic proteins such as cytochrome C and plant and bacterial ferrodoxins.

In the early 1960s Phillips realized that modern physical methods had great potential for biological research. He took a leave of absence to go to MIT for postdoctoral work in biochemistry for the academic year 1962-63. This was a significant break from his early emphasis on fundamental electronic and structural properties of inorganic, organic, and organometallic compounds (he had published approximately 30 papers, mostly NMR and ESR, on the earlier work). Collaborators who made significant contributions included J. J. Drysdale, C. E. Looney, E. L. Muetterties, H. C. Miller, J. Foster, D. B. Chestnut, R. E. Benson, D. R. Eaton, D. R. Josey, and R. E. Merrifield. The move into biological mol

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ecules had been clearly his intention even as early as his first NMR experiment, but the direction was consolidated by NMR studies on nucleic acids finished after a year at MIT.

With the highest-frequency high-resolution NMR spectrometers and the newly available computer of average transients (CAT), Bill Phillips with his long time collaborator C. C. McDonald and his biological collaborator, former physicist Sheldon Penman, started his explorations of the NMR spectra of biological molecules. The CAT gave the sensitivity needed for studying the dilute solutions obtainable for macromolecules. In this he profited from the development of CAT by Oleg Jardetzky and by L. C. Allen and L. F. Johnson, and he expressed appreciation of their advances, which made his work possible. Also he generously acknowledged the pioneering work by Jardetzky, who had been studying proteins and their constituent amino acids for several years and who had recently reported studies of nucleic acids similar to those of the Phillips group.

The 1964 *Science* article entitled “Nucleic Acids: A Nuclear Magnetic Resonance Study” showed how NMR can serve as the method of choice for studying the structure and dynamics of nucleic acids in solution. Starting with solutions of the single base polynucleotides (i.e., poly A), they showed the resonances observed at room temperature shifted to lower fields as the temperature was increased to 60°C but did not shift further at still higher temperatures. They interpreted this to result from the conversion of an ordered configuration to a random coil, with the shifts at lower field coming from ring currents of the neighboring stacked bases. Other experiments, taking advantage of their interpretation that the shift reflected structural order, were made of different compositions of poly A and poly U. The results showed that “at 25°C poly (A+U) is formed when the mole

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

fraction of U is less than 0.50 and that poly (A+2U) is formed when the mole fraction of U is greater than 0.66."

In addition to the structural information they obtained kinetic information about the changes with temperature or the "melting." Raising the temperature of the poly A-poly U complex through the melting transition brought out sharp resonances of poly A and poly U, which appeared suddenly without any gradual sharpening, indicating that the melting was an all-or-nothing process without significant rapid exchange between melted and ordered forms. This kind of information, particularly easy to obtain from NMR melting experiments dominates folding or melting data to this day.

In subsequent experiments, Bill and his colleagues extended melting studies to several small proteins, concentrating on ribonuclease and lysozyme. Their subsequent studies were the fulfillment of the promise in his 60-MHz studies of nucleic acids in which narrow lines were observed after destroying the ordered 3-dimensional structure.

These results were inspired by Bill's stay in the biology department at MIT for a sabbatical. There he interacted with Alex Rich and Sheldon Penman, like himself trained in physics and chemistry, who had already successfully switched to biological studies. Upon his return to DuPont, Bill pursued the goal of obtaining a higher field NMR spectrometer than those previously available. The new model would use a superconducting magnet. The first commercially available high-resolution NMR spectrometer was made by Varian Associates and was installed in the DuPont Central Research Laboratory in 1966. This spectrometer in Bill Phillips's hands was a turning point in the study of biological macromoles. Although sharp lines had been seen previously in melted nucleic acids, as discussed above, and although Oleg Jardetzky and his colleagues had been studying histidine protons and titrating them, the 220-MHz spectra

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

that Bill introduced to the world at the second meeting of the International Society of Magnetic Resonance in Biological Systems in Stockholm in 1966 created a revolution in our thinking.

This society, which meets every two years, was formed by a group of attendees at a Gordon Conference on Magnetic Resonance in the summer of 1962. The organizing committee consisted of Bill Phillips, Oleg Jardetzky, Mildred Cohn, Josef ("Terry") Eisinger, and me. The first meeting was held in 1964 at the old stately headquarters of the American Academy of Arts and Sciences, which was to elect Bill to membership some years later. The society has continued to the present, holding meetings every two years around the world. Each meeting is organized by a different group of three or four scientists who volunteer to raise the money and do all the work. Bill was one of the organizers of two of the meetings held in the United States, the first being his participation in the 1968 meeting held in Fairly House, Virginia. The history of these meetings, held in an unselfish scientific spirit almost always in inexpensive out-of-season schools and universities, captured the spirit of innovative, commemorative science that Bill epitomized, which was unique to those times.

Into this small world Bill Phillips gave a revolutionary talk at the Stockholm meeting in 1966. He showed the 220-MHz NMR spectra of several proteins, lysozyme, ribonuclease, cytochrome C, and Fe-S proteins as well as of nucleic acids. In previous high-resolution NMR studies of proteins at 100 MHz, Bill and others, particularly Oleg Jardetzky, had shown some well-resolved lines that were separated by interaction with their unique environment from the broad hump containing the hundreds of unresolved lines. Although the separated resonances of histidine protons usually gave reasonably narrow lines, it seemed as if the large majority

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of the proton resonances were too broad to ever be resolved. In one spectacular spectrum after another Bill Phillips's presentation blew away this pessimism and opened a broad, unlimited future of high-resolution NMR studies of macromolecules.

Inasmuch as imitation is the sincerest form of flattery, I can report that I rushed home to Bell Telephone Laboratories to report Bill's results, a story so convincing that Bell Labs authorized us within 10 days to order the second 220-MHz spectrometer. Bill Phillips's 220-MHz spectra were one of the eye openers of my scientific life. He had the vision to convince DuPont to purchase the first of its kind, based upon his earlier studies of nucleic acids and proteins at lower magnetic fields.

During the next several years, the NMR studies by Bill with his long-time collaborator Cam MacDonald dominated the field. Particularly his studies of the paramagnetically shifted resonances in ferrodoxins and cytochrome C set new standards of resolution and elegance. These were supported by two particularly gifted postdocs, Jerry Glickson and Martin Poe.

Retiring as assistant director of research and development in 1978, Phillips returned to his native Missouri and assumed the positions of chair and Charles Allen Thomas professor of chemistry at Washington University, where he led the chemistry department to national prominence. Bill's six-year stint at Washington University led to the rebirth and growth of the Department of Chemistry. He was, however, clearly uncomfortable with the politics and limited resources of the academic environment. In 1984 he returned to the private sector as senior vice-president of research and development at Mallinckrodt, Inc., in St. Louis.

Highly respected internationally as a scientist of the first tier, Phillips was also deeply involved in science policy is

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

sues. He was science advisor to governors Bond and Ashcroft of Missouri and president of the Missouri Advanced Technology Institute prior to accepting a role on the Bush administration's Science Advisory Board. Moving to Washington, D.C., he chaired the National Critical Technologies Panel, whose first biennial report, presented to President Bush in 1991, became a blueprint for government action. In clear, lucid, direct prose the report convincingly advocated enhancing and securing for the United States those technologies they identified as critical to national security and economic competitiveness.

His skill as a science and technology advisor was widely recognized, and he served on the advisory committees of numerous academic and governmental programs, as well as on the editorial boards of many prestigious scientific journals. Phillips was particularly active in the science and technology of the St. Louis region, serving on the boards of directors of Mallinckrodt, Inc., Sigma-Aldrich, Inc., the Missouri Corporation for Science and Technology, the St. Louis Science Center, the St. Louis Technology Center, as well as Celgene Corporation of Warren, New Jersey. He was actively pursuing many of these interests at the time of his death.

Friends remember Bill Phillips for his great honesty and integrity and for the encouragement and support he gave young scientists. Phillips was renowned for his far-reaching, global assessments of issues and policies both in and outside the technology arena. There is no secret summary of an individual's life; there are always in any simplified representation loose ends and inconsistencies that multiply with familiarity. But in Bill's life, as known to his professional colleagues and friends, there were two well-characterized directions that he followed, often simultaneously. The first was his thrill in scientific innovation. He loved the first

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

appearance of a discovery, the observation and explanation of a novel scientific result. He sought new directions such as his NMR results and always reached out beyond what was known to be possible. His novel early discoveries were of shifted resonance in paramagnetic samples, of shifted and well-resolved resonances in biomolecules, and of their dependence on the solution conformation. He was truly delighted by these insights and he unrolled sheets of spectra for visitors like a dedicated oenophile escorting a visitor through his cave or a creative jeweler displaying his rings and necklaces. With his broad happy smile, his cigar, and his sense that these data were a good thing in life, he communicated warmth and pleasure. Beautiful data were not the results of a dedicated attack with modern weapons in the scientific war on nature's heavily guarded secrets. They were the lovely stones picked up on a walk (although too strenuous a walk for many) exploring a new part of the world.

A similar gentle acceptance permeated Bill's other main professional direction. Despite his enduring love of science, because the description above is truly that of love and interest, Bill was continually drawn into a more worldly administrative direction. He went somewhat up the administrative ladder at DuPont, took upon himself the job of synthesizing plant food fit for humans, came back to his beloved Missouri to rebuild the Chemistry Department at Washington University where he served many administrative responsibilities, went to Mallinckrodt as senior vice-president for science and technology and finally went to Washington to serve as associate director of the Office of Science and Technology Policy.

At different times in his life we met and we would talk about new science and the hope of the field we shared. Often Bill would have to express his regrets of not having

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

been able to commit himself more completely to research. But the demands for his attention to the more administrative responsibilities needed the same somewhat delicately poised curiosity and intellectual ease as scientific research. In no case was it a harsh world, nor a strongly challenging environment, but both to science and to administration Bill Phillips brought a similar sense. They were part of a fine enterprise; they reached out to worthwhile goals; and he could do his share. He never valued his share highly enough. Bill was modest and kind so that the extremes of building a large scientific group to hammer out a field and a career or to fight his way up administratively were not in his lexicon. Bill was pleased that he was able to contribute to what he considered worthy activities, and we who loved him were always thrilled to share with him. These two professional directions brought out similar fine human qualities in him.

Phillips is survived by his wife of 43 years, Esther ("Cherry") Parker, their two children, Katherine Daniels of St. Louis and Edward D. Phillips of Virginia Beach, Virginia, and two grandchildren. His ashes are buried in Kansas City, Missouri.

I WOULD LIKE TO THANK J. J. H. Ackerman and J. Glickson for their helpful suggestions.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1955

Restricted rotation in amides as evidenced by nuclear magnetic resonance. *J. Chem. Phys.*23(7): 1363-64 .

1957

With C. E. Looney and C. K. Ikeda. Influence of some paramagnetic ions on the magnetic resonance absorption of alcohols. *J. Chem. Phys.*27(6): 1435-36 .

1958

Studies of hindered internal rotation in organic molecules by nuclear magnetic resonance. *Ann. N. Y. Acad. Sci.*70: 817 .

1959

With E. L. Muetterties. Structure and exchange processes in some inorganic fluorides by nuclear magnetic resonance. *J. Am. Chem. Soc.*81: 1084 .

1961

With R. E. Benson, D. R. Eaton, and A. D. Josey. Electron spin density distributions in conjugated systems by NMR. *J. Am. Chem. Soc.*83: 3714 .

1962

With others. Unpaired electron distribution in Ti-systems. *J. Am. Chem. Soc.*84: 4100 .

1963

With C. C. McDonald. A nuclear magnetic resonance study of structures of cobalt (II)-histidine complexes. *J. Am. Chem. Soc.*85: 3736 .

1964

With C. C. McDonald and S. Penman. Nucleic acids: A nuclear magnetic resonance study. *Science*144(3623): 1234-37 .

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1965

With D. R. Eaton. Spin delocalization in mixed tetrahedral Ni II complexes. *J. Chem. Phys.*43(2): 392-98 .

With D. R. Eaton. Nuclear magnetic resonance of paramagnetic molecules. *Adv. Magn. Reson.*1: 103 .

With C. C. McDonald and J. Penswick. NMR study of the secondary structure of s-RNA. *Biopolymers*3: 609 .

1967

With R. C. Ferguson. High-resolution nuclear magnetic resonance spectroscopy. *Science*157: 257-67 .

With C. C. McDonald. Manifestations of the tertiary structures of proteins in high-frequency nuclear magnetic resonance. *J. Am. Chem. Soc.*89: 6332 .

With C. C. McDonald and J. Lazar. Nuclear magnetic resonance determination of thymine nearest neighbor base frequency ratios in deoxyribonucleic acid. *J. Am. Chem. Soc.*89: 4166 .

1969

With C. C. McDonald. Perturbation of the PMR spectrum of lysozyme by Co^{+2}. *BBRC*35(1).

1970

With others. Proton magnetic resonance, magnetic susceptibility and Mossbauer studies of *clostridium pasteurianum* rubredoxin. *Nature*227: 574-77 .

1971

With C. C. McDonald and J. D. Glickson. Nuclear magnetic resonance study of the mechanism of reversible denaturation of lysozyme. *J. Am. Chem. Soc.*93: 235 .

1972

With others. Structure and properties of a synthetic analogue of bacterial iron-sulfur proteins. *Proc. Natl. Acad. Sci. U. S. A.*69(9): 2437-41 .

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1973

With C. C. McDonald. Proton magnetic resonance studies of horse cytochrome C. *Biochemistry*12: 2170 .

With others. Synthetic analogs of the active sites of iron-sulfur proteins. Structure and properties of bis[o-xylyldithiolato-:$_2$-sulfidoferrate (III)], an analog of the 2Fe-2S proteins. *Proc. Natl. Acad. Sci. U. S. A.*70: 2429 .

Nuclear magnetic resonance spectroscopy of proteins. *Methods Enzymol.*27: 825 .

1974

With C. C. McDonald and J. LeGall. Proton magnetic resonance studies of desulfovibrio cytochromes C_3. *Biochemistry*13: 1952 .

1975

With R. C. Burns and R. W. F. Hardy. Azotobacter nitrogenase: Mechanism and kinetics of allene reduction. From nitrogen fixation by free-living microorganisms. *Int. Biol. Prog.*6: 447 .

With W. E. Cooke and D. Kleppner. Magnetic moment of the proton in Bohr magnetons. *Phys. Rev. Lett.*35: 1619 .

1976

With L. F. Liebes and R. Zand. The solution behavior of the bovine myeline basic protein in the presence of the anionic ligands. Binding behavior with the red component of trypan blue and sodium dodecyl sulfate. *Biochem. Biophys. Acta*427: 392 .

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

EDWARD MILLS PURCELL

August 30, 1912–March7, 1997

BY ROBERT V. POUND

EDWARD MILLS PURCELL, Nobel laureate for physics in 1952, died on March 7, 1997, of respiratory failure at his home in Cambridge, Massachusetts. He had tried valiantly to regain his strength after suffering leg fractures in a fall in 1996, but recurring bacterial lung infections requiring extended hospitalizations repeatedly set back his recovery.

Two of the best known of Purcell's many outstanding scientific achievements are his 1945 discovery with colleagues Henry C. Torrey and Robert V. Pound of nuclear magnetic resonant absorption (NMR), and in 1951 his successful detection with Harold I. Ewen of the emission of radiation at 1421 MHz by atomic hydrogen in the interstellar medium. Each of these fundamental discoveries has led to an extraordinary range of developments. NMR, for example, initially conceived as a way to reveal properties of atomic nuclei, has become a major tool for research in material sciences, chemistry, and even medicine, where magnetic resonance imaging (MRI) is now an indispensable tool. Radio spectroscopy of atoms and molecules in space, following from the detection of the hyperfine transition in hydrogen as the first example, has become a major part of the ever-expanding field of radio astronomy.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Purcell made ingenious contributions in biophysics, as exemplified by his famous analysis of life at low Reynolds numbers, which described the locomotion of bacteria in water. In astronomy, he made important contributions to the study of the alignment of interstellar grains. As a teacher he had a great influence on many students whom he advised and who sat in his beautifully crafted courses at Harvard. His introductory textbook on electricity and magnetism set a new standard of scholarship. Finally, Purcell was looked to as a most valued advisor and consultant throughout his professional life, having served on innumerable committees, including two periods of service on the President's Science Advisory Committee in the administrations of Presidents Eisenhower, Kennedy, and Johnson.

EARLY YEARS

Edward M. Purcell was born on August 30, 1912, in Taylorville, Illinois, the older of two boys. His father, Edward A. Purcell, was manager of the local telephone exchange in Taylorville, and moved, when the boy Edward was fourteen years of age, to Matoon, Illinois, some 60 miles southeast to become general manager of the Illinois Southeastern Telephone Company, an independent regional company. Ed's mother, Elizabeth Mills Purcell, was a graduate of Vassar College and taught Latin in the Taylorville high school before her marriage. Both of Ed's parents had strong influences on his lifetime interests, his father's profession leading to his pursuit of technology and science and his mother's to his interest in literature, writing, and the humanities.

His father's connection with the telephone company played an important role in his youthful interests. It made available to him discarded equipment, such as lead-jacketed cable and copper wire and the old hand-cranked bell-ringer magnetos replete with horseshoe magnets. Of even greater

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

influence were the periodic issues of the highly scientific *Bell System Technical Journal.* Although his father's company was not a part of the Bell System, it did maintain the area lines for the system and received the journal. These publications were a source of inspiration to the young Edward and he held onto and referred to them into his college years. In an interview recorded at Harvard by Katherine Sopka in 1977, transcribed at the Center for the History of Physics of the American Institute of Physics, Purcell is quoted as stating, “They were fascinating because for the first time I saw technical articles obviously elegantly edited and prepared and illustrated, full of mathematics that was well beyond my understanding. It was a glimpse into some kind of wonderful world where electricity and mathematics and engineering and nice diagrams all came together.”

COLLEGE YEARS

From this background, Ed elected to enter Purdue University in 1929 to pursue a course of study in electrical engineering. Ed described his training in the old style of electrical engineering, which included learning about such practical things as the design and construction of the armatures of electric motors, as continuing to interest him and to influence his later work. While he was a Purdue undergraduate, he discovered that his true interest was physics. It was the arrival of Karl Lark-Horovitz as the new head of the physics department there that led to an increased visibility of that subject. The department that previously did not even offer a major undergraduate program began to include graduate students engaged in research. During his junior undergraduate year, although technically still an engineering student, Ed signed up with the physics department for a newly offered course of independent study. His supervisor initially was Professor Walerstein, who put him

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

to work refurbishing a spectrometer based on a Rowland grating. He then went on to building an electrometer to measure nuclear half-lives. With that experience, Ed became enamored with physics, and in his senior year he worked with H. J. Yearian, then a graduate student finishing a Ph.D. thesis project in electron diffraction. Ed stayed on for the summer after he graduated to participate in writing his first two papers, which grew from this experience, the first dealing with electron diffraction (1934) and the second reporting a method for making the required thin films (1935). In that connection he once told me he believed he, luckily, must not be allergic to beryllium oxide. His evidence was that, in connection with preparing samples for the electron diffraction studies, he had manually swept the BeO from the smoke produced by an arc with a beryllium electrode running in air. This was well before beryllium toxicity was widely recognized.

Ed graduated from Purdue University in the spring of 1933 with the B.S.E.E. degree, and with the support of Lark-Horovitz he was awarded an exchange fellowship that took him to the Technische Hochschule in Karlesruhe, Germany, for the following academic year. It was an awkward time to be in Germany, because the effects of the Nazi Party's coming to power were beginning to be felt. He expressed regret that he did not take more interest while there in the history of Heinrich Hertz, who first produced radio waves at Karlesruhe. In traveling to Germany by ship that autumn Ed had met Beth C. Busser, another exchange student, from Bryn Mawr College, who was to visit Munich. Although she was studying German literature, he persuaded Beth to attend a lecture by the distinguished physicist Arnold Sommerfeld at Munich. She did so and took notes for him, although the subject had little meaning to her. Beth and Ed were married

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

four years later in Cambridge, Massachusetts, where she survives him.

Again with the support of Lark-Horovitz, Ed joined the physics department of Harvard University as a graduate student in the fall of 1934, where he would remain for the rest of his life, except for various leaves of absence for special purposes. Of particular influence on his interests in later years was the course on electric and magnetic susceptibilities of Professor John H. Van Vleck. Ed and Malcolm Hebb, then an advanced student of theory with Van were the only students in the course. Van Vleck had just joined the Harvard faculty after some years at the University of Minnesota and the University of Wisconsin. He persuaded them to publish their joint term paper in the then quite new *Journal of Chemical Physics.* This work (1937) was an analysis of experiments in cooling by adiabatic demagnetization then being carried out mostly in The Netherlands and Great Britain. Ed has emphasized that this experience and his study with John Van Vleck had an important influence on his later interests in physics.

As a thesis project, after a couple of unrewarding tries, Ed undertook an experiment suggested to him by Professor Kenneth T. Bainbridge, who was then especially concerned with the focusing of charged particles by magnetic and electric fields in connection with his work with mass spectrographs and nuclear physics. This project was to study, both theoretically and experimentally, the focusing properties of the electric field in the space between two concentric metal spheres forming a spherical condenser. (It now would be called a capacitor!) The experimental aspect of this project involved a complex glass-blowing effort to construct the device, and Ed was always grateful for the expert help of the department's resident glass blower, Mr. H. W. Leighton. The experiment, which basically confirmed Ed's analysis of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

the three-dimensional focusing properties for electrons, was published (1938) and provided him with his dissertation. Some thirty years later the concept resurfaced for refocusing low-energy electrons in X-ray-induced electron emission experiments.

With the completion of his thesis project, Ed joined the teaching staff of the Harvard physics department, becoming a Faculty Instructor, a rank with a five-year term that was created in Harvard's reconstruction of its faculty structure and tenure policy. (The title reverted to the more widely recognized Assistant Professor in later years.) Ed continued to collaborate with Kenneth Bainbridge in the completion, bringing into operation, and initial research in nuclear physics of the pre-war Harvard cyclotron. In this connection he developed current-carrying shim coils for adjusting the shape and homogeneity of the field of the cyclotron magnet. In the autumn of 1940, when the Radiation Laboratory was established at the nearby Massachusetts Institute of Technology, Ed was asked to follow Bainbridge there to undertake emergency work to develop microwave technology for military radar.

THE MIT RADIATION LABORATORY

Ed went on a leave of absence from Harvard from the beginning of 1941 to join the MIT project and continued it until July 1946. For most of that time he headed the advanced developments group, which was responsible mainly for moving the radar systems to shorter wavelengths, initially from 10 cm to 3 cm and then to 1.25 cm. A major gain from these moves was the improved resolution, especially from aircraft, where space seriously limited the antenna dimensions and therefore the sharpness of the beams. After sufficient progress had been made, serious preparations were made for production of the 1.25-cm systems, the principal one being

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

code named H_2K. However, as the warmer weather of the spring of 1944 arrived, a disappointing decrease in the detection range was found. This was soon recognized to be associated with the increasing atmospheric humidity that brought with it a serious absorption of that particular wavelength by water molecules. This effect had been anticipated because of the complex structure of the energy states of the free water molecule, but its strength and the wavelength had not been established. The choice of 1.25 cm for the so-called K-band radar systems, made in the absence of prior data, turned out to be unfortunate.

With the ending of World War II on V-J Day, August 14, 1945, the MIT Radiation Laboratory prepared to close down, but several of its members, including Purcell, were asked to stay on. Most of the staff members were leaving to resume their pre-war careers or to begin new ones. Some of us stayed to contribute to the writing of a series of books that would preserve the technology developed over five years at the laboratory and its collaborating organizations under conditions of military secrecy. During this period, in the autumn of 1945, Ed proposed to his two friends and colleagues Henry C. Torrey and me that we three should jointly design and undertake in our spare time an effort to detect resonant absorption of radio-frequency energy by atomic nuclei in solid matter held in a strong magnetic field. He was led to think along those lines by his writing for the series about the absorption of microwaves by water vapor (1951). The absorption was attributed to two energy states of the free H_2O molecule that happened to have an energy difference corresponding to the quantum energy of the K-band radio waves. The concept of nuclear magnetic resonance in molecular beams had recently been highlighted with the award of the 1944 Nobel Prize for physics to our colleague I. I. Rabi for his pre-war research. Rabi and sev

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

eral of his associates from his laboratory at Columbia University had played important roles in microwave developments, both at the Radiation Laboratory at MIT and its counterpart of the same name at Columbia University. Henry Torrey was one of the Rabi laboratory veterans, and this influenced Purcell to seek Torrey's view about the possibility of detecting NMR as an absorption of radio-frequency energy. An experiment was improvised largely from inactive Radiation Laboratory equipment and was moved to the Research Laboratory of Physics at Harvard to use a magnet built in the 1930s by J. Curry Street to provide bending of tracks of cosmic rays in his cloud chambers. Our three-member team worked mostly evenings and weekends and, on Saturday afternoon, December 15, 1945, succeeded in detecting the absorption of radio-frequency by protons in paraffin wax by magnetic resonance (1946).

AFTER WORLD WAR II

In July 1946, anticipating his return to Harvard—where he had been promoted to the tenured rank of Associate Professor—Ed undertook to develop this newly opened field of magnetic resonance of nuclei. In February 1946 graduate student Nicolaas Bloembergen, just arrived from Utrecht, The Netherlands, joined in the effort. As a research assistant, Bloembergen helped initially to develop more sensitive instrumentation for the NMR studies. With it the team of Bloembergen, Purcell, and Pound carried out a series of fundamental experiments in 1946 and 1947. The one that has become widely known by the initials BPP was the important study and explanation of thermal relaxation of the nuclear spins and of collision narrowing of NMR resonance lines in liquids and gases and in solids with certain internal motion. The report of this work was the basis of Nicolaas Bloembergen's Ph. D. thesis, which was submitted to Leiden

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

University in 1948. The article in *Physical Review* (1948) established a record for its citations, according to the publishers of the Citation Index.

Another field of research that was greatly influenced by the wartime development of new electronic technology was radio astronomy. Beginning in the late 1940s Purcell encouraged graduate student Harold I. Ewen to look for an astronomical spectral line based on the hyperfine splitting of the ground state of the interstellar atomic hydrogen in the galaxy. As known from laboratory experiments, the quantum energy of that splitting corresponded to a wavelength of 21 cm, or a frequency of 1420 MHz. Purcell applied to the Rumford Fund of the American Academy of Arts and Sciences for support in the amount of $500, with which he was able to construct the horn antenna and carry out the project. The spectral line was first detected, as an emission, on March 25, 1951. In a gentlemanly gesture, Purcell sent information about the technique and the success to two astronomers he knew were interested. One was Professor Jan Oort in Leiden, The Netherlands, and the other was J. L. Pawsey of the Radio Physics Laboratory in Sydney, Australia. He delayed the publication of the news in *Nature* until confirming reports from those two teams could join it (1951). Professor Oort had much earlier, after learning of the discovery of radio signals from space by Carl Jansky and then by Gerth Reber before World War II, hoped to study the details of the galaxy by observing the strength and Doppler shifts of some radio spectral lines. The suggestion of looking for the hydrogen hyperfine line had been made to him by H. G. van de Hulst in 1944. Only after receiving the description of Ewen and Purcell's success was Oort also successful. He reported early data on galactic structure in that first article and went on to do so in impressive detail in later reports. Atomic and molecular spectroscopy has grown

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

to become a major activity in radio astronomy in the years since that seminal experiment of Ewen and Purcell. Prior to that discovery, radio astronomy had mainly dealt with sources emitting broad noise, mostly at lower frequencies.

Throughout his professional career, Edward Purcell was continuously sought out as a consultant and advisor. He spent time on a variety of studies for agencies of the U.S. government. Following almost immediately from the period at the MIT Radiation Laboratory he served for many years on the Air Force Science Advisory Board at the request of Lee Dubridge. In the fall term of 1950 Ed took a leave of absence from his duties at Harvard to join Project Troy, a secret study based at MIT for the U.S. Department of State.[1] This was also a critical period in the development of the search for the astronomical atomic hydrogen line, and I became more closely involved in its progress in Ed's absence. Through this and later studies he developed a close friendship with Edwin H. Land, founder of the Polaroid Corporation and inventor of its instant photography techniques. They both served on the original President's Science Advisory Committee that began under President Eisenhower in response to the Soviet *Sputnik* revelations. There, Purcell chaired the subcommittee on space and he and Land wrote, with the participation of Frank Bello, formerly of *Fortune* magazine, a pamphlet sometimes called the “Space Primer” to educate as many people as possible about the possibilities of space exploration.[2] Ed was proud of the degree to which their projections proved correct as the program developed in the following years, including the moon landings, whose possibility they had described. He and his committee colleagues had important influences on the organization of the National Aeronautics and Space Administration (NASA), the whole developing space exploration program, and the later conduct of the Apollo mission. One such contribution

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

was their persuading NASA to provide the astronauts with specially designed color stereo cameras to make photographs of the undisturbed lunar surface around the landing site on the initial and later missions. Another outgrowth of one of the studies for national defense was the invention of a long-distance communication system (1952) for very short wavelengths, using scattering from turbulence in the troposphere.

In 1952 the award of the Nobel Prize for physics (1953) recognized Purcell's role in the founding of NMR and its even then rapidly increasing range of applications. He shared that honor with Felix Bloch of Stanford University, who had reported a successful detection of nuclear induction with collaborators William W. Hansen and Martin Packard a few weeks after the publication of our group's report.

In the first few post-war years after returning to Harvard, Purcell directed thesis research in aspects of magnetic resonance by a distinguished group of able young men. Following almost in parallel with the well-known work of Nicolaas Bloembergen, George E. Pake joined in. His name has been immortalized in NMR by the term “Pake doublet” (1949), which results from the pairing of protons in water of crystallization. Charles P. Slichter studied a related relaxation question in electronic paramagnetic resonance in crystals. Walter Brown used NMR to calibrate the magnetic field of a specially constructed β-ray spectrograph to establish a better value for the absolute energy of an internal conversion electron from radium, often used as a calibrational reference. A new way to understand so-called spin echoes, which had been invented by Erwin Hahn at University of Illinois was developed with Herman Carr into a method of measuring molecular self-diffusion, using an applied magnetic field gradient.

In 1949 I described to Ed some experiments I had just

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

carried out with a pure LiF single crystal. I had found the nuclear spin systems to require several minutes to build up their magnetization to thermal equilibrium at room temperature when placed in a strong magnetic field. They retained that polarization even if they were removed for a few seconds from the strong magnet and then replaced. I also found that reorienting manipulations on the demagnetized crystal had little effect on its remagnetized state. Ed and I then saw an opportunity to try to invert that magnetization by applying a specially designed magnetic transient to the demagnetized crystal, followed by a return to the strong polarizing field. Ed constructed a simple device to provide such a transient pulse, which indeed resulted in an inverted state relative to the normal thermal polarization when the crystal was subjected to this transient and then returned to the polarizing field. That inverted magnetization decayed back to the normal equilibrium state with the several-minute time constant of its spin-lattice relaxation. When probed by the NMR detector in the inverted condition, stimulated emission rather than absorption was observed. Charles Townes has indicated that his reading about that initiated his thoughts that led to his invention of masers and lasers. Our experiment involved complete demagnetizations at several steps in the process, with the evidence that the inversion persisted without serious loss; so Purcell and I described the resulting situation (1951) as a spin system at negative temperatures. Initially, that concept met with resistance and even antagonism from some well-established thermodynamicists, but with time it became a significant textbook item illuminating aspects of thermodynamics and statistical mechanics special to systems with a bounded set of energy states. Negative temperatures are even hotter than infinite temperatures, not colder than absolute zero, as might naively be supposed.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

In 1950 Purcell and N. F. Ramsey questioned whether the argument was valid that nuclei could not possess electric dipole moments. They pointed out (1950) that the basis of the argument was the belief that nuclei obeyed the laws of parity symmetry conservation, for which they could find no direct experimental evidence. As a follow-up of this query they undertook an experiment at the Oak Ridge National Laboratory with graduate student James H. Smith, seeking—but only setting an upper limit to—an electric dipole moment in the neutron (1957). Norman Ramsey has continued this search with other collaborators, setting ever-lower limits, mainly at the ultra-cold neutron facility at the Institut Laue Langevin in Grenoble, France.

Ed's name is applied to the phenomenon known as the Smith-Purcell effect (1953). Recently that concept has been viewed by some as a precursor of the free-electron laser (FEL). In their experiment, Steve Smith, as a graduate student guided by Purcell, sent an energetic beam of electrons very closely parallel to the surface of a ruled optical diffraction grating, and thereby generated visible light. Ed was not happy with the connection recently made to the FEL. He held that the electrons, as they sped past the grating, individually induced moving image charges, and the light produced by those image charges would be completely incoherent. He showed there was negligible effect on the trajectory of the inducing electrons. Later experimenters have, however, tried to produce coherent radiation by using much higher electron energies and optical feedback, thus attempting to develop the scheme into an FEL.

The new strong-focusing accelerator at the Brookhaven National Laboratory, the alternating gradient synchrotron, was to begin in the early 1960s to run a beam of protons having the unprecedented laboratory energy of 3 $\times 10^{10}$ electron volts (30 GeV). Ed became excited by the possibil

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ity that a so-far unobserved magnetic monopole might be created by collisions at such energies. He and four Brookhaven-based collaborators designed a simple but elegant detector and carried out over the course of a year and a half in 1960-62 an unsuccessful search for evidence of the Dirac monopole at the alternating gradient synchrotron (1963).

In the early years after World War II, Ed, much involved in teaching the introductory physics course, joined with his Harvard colleagues J. Curry Street and Wendell Furry to write a serious introductory textbook for students intending to concentrate in the physical sciences. This book, which began as an updated edition of a well-known text, became much more changed than anticipated and was published under its own title.[3] Many years after this initiation into textbook writing, Ed, through his membership in the newly established Commission on College Physics, undertook while on a leave of absence at Berkeley to write a new textbook on electricity and magnetism; this became volume II of the Berkeley Physics Series organized by Charles Kittel and supported by the National Science Foundation (1965). In that elegantly structured text he made particular use of the interdependence of electric and magnetic phenomena in moving frames of reference as established by the special theory of relativity. He took special pains with the clarity and simplicity of the illustrative figures, as well as the problems. He supplemented the textbook with a separate book of problem solutions, reproduced directly from his carefully prepared manuscripts and lettered out in his own recognizable hand. When he discovered that the publisher and some users of the text were keeping its existence secret from the students, he was angry. He felt that the problems and their solutions made a major contribution to the pedagogical content of his course. He brought out a revised

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

second edition in 1985 in which he gave in, to some extent, to the mounting pressure to use SI (Systeme International) units by including alternative versions of the equations in those units, but still retaining his preferred Gaussian units.

ASTROPHYSICS AND BIOPHYSICS

Although Edward Purcell's contribution to radio astronomy brought him into contact with astrophysics, it was in the early 1960s that he moved most of his creative research energies into astrophysics. He had always been strongly attracted to theoretical modeling and analysis, and the problem of understanding the mechanisms of the interactions of interstellar dust and light propagating through the galaxy consumed a large part of his efforts in his later years. In 1969 he published his initial article in this field under the title “On the Alignment of Interstellar Dust,” and followed up with an article authored jointly with the astrophysicist Lyman Spitzer, Jr., of Princeton University entitled “Orientation of Rotating Grains” (1971). He went on to develop the image of interstellar grains brought into states of high-speed rotation—which he entitled “Suprathermal Rotation”—by their interactions in space. In 1982 Purcell was invited to give the Halley Lectures at Oxford University, England, on this subject. He told me that our mutual friend there, Brebis Bleaney, suggested that his title referring to interstellar dust would be better received in England if he avoided the word dust in his title, because the English use it for less pretty materials. Witness Mr. Doolittle, Eliza's father, the dustman in G. B. Shaw's *Pygmalion.* Such is a problem of our shared language.

One of Ed's academic roles had been to serve for several years as a senior fellow of Harvard Society of Fellows, where he became closely acquainted with some of the fascinating projects of the junior fellows. One particular junior fellow

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

was a physicist converting to biophysics—Howard Berg. Ed and Howard devised an elegant machine based on concentric rotating cylinders for separating molecules in liquid states according to their masses. Ed expressed astonishment at the heavy response of the biological community to this publication in the *Proceedings of the National Academy of Sciences.*

A little later, when Howard was studying the locomotion of *E. coli* bacteria, he was able to prove that they moved by continuous rotation of their corkscrew-like flagella, rather than by flapping them. With this model, Ed developed the description of their hydrodynamic situation. This led to the publication of a transcript of his talk at a symposium, held in honor of Victor Weisskopf, under Ed's title “Life at Low Reynolds Numbers” (1977). He compared the bacteria's problem of generating thrust to that of a man trying to swim in a tank of thick molasses. He demonstrated the inefficiency of their mechanism by scaling up to small spiral wire coils that he dropped through a viscous fluid, observing the small rotation induced in the coils by their falling velocity. He thus showed the reciprocal effect of the rotation-to-thrust model and developed a matrix to describe the situation quantitatively. Initially he used corn syrup as the viscous medium, but was very happy when I suggested to him that silicone fluids of great optical transparency were available in a wide range of viscosity. He once mentioned to me that his tank of silicone fluid cost him about the same as that amount of Jack Daniels. Ed was able to project this demonstration onto a lecture screen, giving graphic evidence of his analysis. It demonstrated the little thrust the bacteria could produce in water, because their rotation mainly carried the viscous fluid round with it out to a quite large distance. The *E. coli* had no other way to pursue needed nourishment, and so had to put up with it. Ed and Howard

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Berg were jointly awarded the Biological Physics Prize of the American Physical Society in 1984 for their work.

In the years between 1983 and 1988 Purcell conducted a pedagogical column in the *American Journal of Physics* under the heading “Back of the Envelope.” In this he posed problems for thought, which he could solve quantitatively by a few lines on the back of an envelope. In the next issue he gave his consistently educational and insightful solution. This was yet another way Ed enjoyed playing what he felt to be his primary role, that of educator. Of course, he received many honors over the years, including membership in the National Academy of Sciences in 1951, foreign member of the Royal Society, the National Medal of Science in 1979, and membership in the many other scholarly academies. He was particularly happy with the recognition of his teaching conveyed by his award of the Oersted Medal of the American Association of Physics Teachers in 1968.

To conclude, Edward Purcell contributed strongly to the advance of many sciences and taught a large number of students and colleagues his special insight for explaining complex phenomena in simple ways. It was a universal reaction of those who encountered his way of analyzing a new problem to marvel at his quick understanding. In addition to his deep interest in so many aspects of science and his interest in teaching others, he had a deep wish to avoid wars and especially to control weapons of mass destruction. It was his distaste for our involvement in Vietnam that brought him to resign from the President's Science Advisory Committee in 1965 and sever his ties with all military advisory panels from then onward. In September of 1967 he was persuaded as our member who was acquainted with Johnson from his role in the advisory committee to serve as spokesman for a group of Harvard faculty members. We had been invited to meet with President Johnson at the White House

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

to present our views in opposition to the continuing war in Vietnam that had been presented to him in a letter earlier. Ed accepted the role with reluctance. From my seat on Johnson's left side at the big table in the Cabinet Room, I watched as he wrote on his pad "improvement relations Harvard University. " Otherwise he only talked for over an hour of rambling defense and was quite unmoved by the arguments Ed presented.

Testimony to Ed's evident wisdom and clarity of thought and expression is presented in the autobiographical work of James R. Killian, Jr., the first chairman of the President's Science Advisory Committee.[4] Killian writes, "When Eisenhower was later to speak in memorable tribute of 'my scientists' he was surely recalling among others this quiet, modest, lucid man. Robert Kreidler, in an interview I had with him in preparing for this memoir, spoke almost with awe of his impact on PSAC. 'Ed Purcell did not speak often,' he said, 'but when he did there would be enormous silence in the room, because everybody knew that whatever he said was going to be worth listening to with careful attention.' "

Through many of these years Ed and Beth dedicated much of their energies to their role as parents of their two boys, Dennis W. and Frank B., born in the early 1940s. In more recent times they have enjoyed watching their three grandchildren grow to adulthood. Ed was happy to know a great-grandchild in his last years. For most of my own professional life I have benefited greatly from Ed's unstinting and wise advice and support. It is difficult to accept that it is no longer there for my colleagues or me.

ALTHOUGH I HAVE HAD a close personal and professional relationship with Edward Purcell for almost sixty years, I found two recent biographical sources helpful with some details. In 1991 the IEEE held a special session of its meeting in Boston commemorating the fifti

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

eth anniversary of the Radiation Laboratory. In connection with that meeting about forty interviews were held with Radiation Laboratory alumni. John Bryant interviewed Ed, and these, entitled *Rad Lab: Oral Histories Documenting World War II Activities at the MIT Radiation Laboratory,* were published by the IEEE in 1993 (ISBN number 0-7803-9968-4, Center for the History of Electrical Engineering, Piscataway, N.J.). Another source of biographical information appeared in a chapter that amounts to an authorized biography, written with considerable consultation with Ed and many other sources, by James Matson in the volume *The Pioneers of NMR and Magnetic Resonance in Medicine: The Story of MRI* (Bar-Ilan University Press, Ramat Gen, Israel; published in the U.S.A. by the Dean Book Co., Jericho, N.Y., 1996). That book is sometimes seen as seeking particularly to promote the role of one contributor to the development of MRI. Nevertheless, Matson has captured much interesting detail in his biography of Edward Purcell.

NOTES

1. The origin and purposes of Project Troy and Harvard Provost Paul Buck's promise to have strong Harvard participation are described by James G. Hershberg in his *James B. Conant,* p. 511, New York: Alfred A. Knopf, 1993.

2. Reprinted in James R. Killian, Jr.'s autobiographical memoir *Sputnik, Scientists, and Eisenhower,* as Appendix 4, pp. 288-99, under the title *Introduction to Outer Space,* with a foreword by President Eisenhower. Cambridge, Mass.: MIT Press, 1977.

3. W. H. Furry, E. M. Purcell, and J. C. Street. *Physics for Science and Engineering Students.* New York: Blakiston, 1952; New York: McGraw Hill, 1960.

4. *Sputnik, Scientists, and Eisenhower,* op. cit., p. 123.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1934

With K. Lark-Horovitz and H. J. Yearian. Electron diffraction from vacuum-sublimated layers. *Phys. Rev.*45: 134(A) .

1935

With K. Lark-Horovitz and J. D. Howe. Method of making extremely thin films. *Rev. Sci. Instrum.*6: 401-403 .

1937

With M. H. Hebb. A theoretical study of magnetic cooling experiments. *J. Chem. Phys.*5: 338-50 .

1938

The focusing of charged particles by a spherical condenser. *Phys. Rev.* 54: 818-25 .

1946

With H. C. Torrey and R. V. Pound. Resonance absorption by nuclear magnetic moments in a solid. *Phys. Rev.*69: 37-38 .

Spontaneous transition probabilities in radio-frequency spectroscopy. *Phys. Rev.*69: 681 .

With R. V. Pound and N. Bloembergen. Nuclear magnetic resonance absorption in hydrogen gas. *Phys. Rev.*70: 986-87 .

With N. Bloembergen and R. V. Pound. Resonance absorption by nuclear magnetic moments in a single crystal of CaF_2. *Phys. Rev.*70: 988 .

1948

With N. Bloembergen and R. V. Pound. Relaxation effects in nuclear magnetic resonance absorption. *Phys. Rev.*73: 679-712 .

With C. G. Montgomery and R. H. Dicke. *Principles of Microwave Circuits.* Radiation Laboratory Series, vol. 8. New York: McGraw-Hill.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1949

With G. E. Pake. Line shapes in nuclear paramagnetism. *Phys. Rev.*74: 1184-88; erratum 75: 534 .

1950

With N. F. Ramsey. On the possibility of electric dipole moments for elementary particles and nuclei. *Phys. Rev.*78: 807 .

1951

With H. I. Ewen. Observations of a line in the galactic radio spectrum. *Nature*168: 356.

With R. V. Pound. A nuclear spin system at negative temperature. *Phys. Rev.*81: 279-80 .

With J. H. Van Vleck and H. Goldstein. *Atmospheric Attenuation.*Radiation Laboratory Series, vol. 13 ed. D. E. Kerr. New York: McGraw-Hill.

1952

With D. K. Bailey and others. A new kind of radio propagation at very high frequencies observable over long distances. *Phys. Rev.*86: 141-45 .

1953

Research in nuclear magnetism. *Les Prix Nobel en 1952,*pp. 97-109, Stockholm; *Science*118: 431-36 .

With S. J. Smith. Visible light from localized surface charges moving across a grating. *Phys. Rev.*92: 1069 .

1954

With H. Y. Carr. Effects of diffusion on free precession in nuclear magnetic resonance experiments. *Phys. Rev.*94: 630-38 .

1963

With G. B. Collins, J. Hornbostel, T. Fujii, and F. Turkot. Search for the Dirac monopole with 30-BeV protons. *Phys. Rev.*129: 2326-36 .

1965

Electricity and Magnetism. Berkeley Physics Course, vol. II. New York: McGraw Hill.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1969

On the alignment of interstellar dust. *Physica*41: 100-127 .

1971

With L. Spitzer, Jr.Orientation of rotating grains. *Astrophys. J.*167: 31-62 .

1977

Life at low Reynolds numbers. *Am. J. Phys.*45: 3-11 .

With H. C. Berg. Physics of chemoreception. *Biophys. J.*20: 193-219 .

1983-88

The back of the envelope. *Am. J. Phys.* vols. 51, 52, 55, and 56.

1988

Foreword. In *Magnetic Resonance Imaging,* eds., C. Leon Partain and others. Philadelphia: W. R. Saunders.

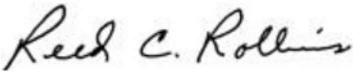
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.



Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

REED CLARK ROLLINS

December 7, 1911-April 28, 1998

BY IHSAN A. AL-SHEHBAZ

REED ROLLINS, ASA GRAY professor of systematic botany emeritus at Harvard University, was one of the outstanding and most insightful scientists of this century. He was one of the founding fathers and second president of both the International Association for Plant Taxonomy and the Organization for Tropical Studies. His 30-year leadership as the director of the Gray Herbarium elevated Harvard to one of the world's top centers for studies in systematic and evolutionary botany. He was one of the pioneering botanists who promoted the extensive use of genetics, anatomy, and cytology to solve taxonomic problems.

Reed, the eighth of thirteen children (ten boys and three girls) of Mormon parents, was born in Lyman in southwestern Wyoming on December 7, 1911. His father, Clarence Rollins, was a rancher and at one point a deputy sheriff of Uinta County, who along with his family and neighbors, leased the land from the federal government. The children were always encouraged to study hard and read the various books at home and the daily newspaper, as well as sing and play the piano at night. During the evenings, the family spent many good times singing, while the girls took turns playing the piano.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Reed attended elementary and high schools in Lyman. He enjoyed plants as a child, and at the age of fifteen, he attended a presentation by Aven Nelson, then director of the Rocky Mountain Herbarium at Laramie, about plants in general and those of the Rocky Mountains in particular. The summer after finishing high school, Reed, his brother Bill, and their cousins worked during the day on a gas pipeline, but at night their band, The Happy Jack Jazz Band, played in neighboring towns. Reed played the trumpet.

In 1929 Reed entered the University of Wyoming to become an agricultural teacher. His travel expenses from Lyman to Laramie were covered by a scholarship from the Union Pacific Railroad; his tuition was paid by a scholarship from the University of Wyoming. After finishing his freshman year, Reed decided to major in botany. He worked as a mounter in the herbarium during the academic year and waited on tables in the university dining hall. During the summers of the following three years, he worked for the U.S. Department of Agriculture, conducting plant disease surveys designed to trace the movement of smuts and stem rust on cereals in eastern Wyoming.

Reed's first introduction to the flora of Wyoming was in a summer camp class that he took with Nelson in 1930. Reed was the top-ranking student in his class. After receiving his A.B. degree with honors in 1933, he worked for a year at the University of Wyoming in the Public Works Administration examining the records of farms and ranches. The following year, he attended Washington State University in Pullman and received his M.S. degree in 1936 under the supervision of Lincoln Constance. Reed joined Harvard University in the fall of 1936 and received his Ph.D. in mid-1940 under the supervision of Merritt Lyndon Fernald. He worked on the mustard genus *Arabis* for his masters and

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

doctorate dissertations (1941); he continued working on the genus throughout his life.

Shortly after joining Harvard as a student, Reed impressed his mentors by his innovative approach and courage to use various fields of biology, especially cytology (with Karl Sax) and anatomy (with Ralph Wetmore), to understand the evolutionary relationships and to solve some difficult taxonomic problems. In April 1937 he was unanimously chosen by the senior fellows to the Society of Fellows and was awarded a three-year Junior Fellowship (1937-40), which allowed him to take classes free of tuition and provided him with an annual salary of $1,250, a three-room suite, and free meals.

Reed took courses in cytology, biochemistry, and related fields, and he divided his time between working on taxonomy in the Gray Herbarium and on cytology in Karl Sax's lab. Reed was among the first North American taxonomists to use chromosome data in monographic studies, as evidenced by his account of the genus *Physaria* (1939).

Perhaps among the most important thing Reed advocated in his classes and even in his writings was that, to have a better understanding of the group at hand, taxonomists should use whatever knowledge they can get from any field. This was elegantly stated in his (1952) presidential address to the American Society of Plant Taxonomists (ASPT): “Thus viewed, taxonomy becomes an integrative and synthesizing subject, in a way rising to the shoulders of its sister disciplines.”

In his presidential address to the International Association for Plant Taxonomy, Reed (1959) emphasized the use of population genetics as an important field in enriching the knowledge of a taxonomic group, and stated that “with our natural interest in the dynamics of variation, survival and migration, we not only need the information that can be provided by genetic studies of populations, but we are in a position to exploit this area of research as part of the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

usual taxonomic research program." In fact, his work on the genetic evaluation of a taxonomic character in *Dithyrea* (1958) led the way to questioning the validity of numerous cruciferous species, subspecies, and varieties that were based primarily on differences in the presence or absence of trichomes (hairs). That work showed that the occurrence or lack of trichomes in *Dithyrea* is controlled by a single gene with two alleles that follow simple Mendelian inheritance.

Reed continued to promote the use of new approaches in evolutionary and systematic studies (1957, 1965), and his extensive use of scanning electron microscopy in the study of pollen (1973) and trichomes (1975, 1979) was instrumental in understanding the evolution of the mustard genus *Lesquerella.* Unlike the previously prevailing thought that the Brassicaceae universally have uniform pollen, he (1979) discovered that a number of western North American genera have pollen with more than three colpi, thus providing solid evidence of the direct relationships among them.

Reed joined Stanford University as assistant professor in January 1941, and two years later he took leave of absence to join a U.S. Department of Agriculture team of plant breeders and geneticists in Salinas, California, to work as an associate geneticist on the Guayule Rubber Research Project, a position that he held through 1945. At that time the United States and Japan were at war, and the Japanese had full control of the *Hevea* rubber production in Malaysia and southeastern Asia, whereas the rubber in Brazil and elsewhere in the New World was suffering from rust diseases. Reed continued working on guayule (*Parthenium argentatum*) until late 1950, and his research on rubber resulted in 15 papers published between 1944 and 1951. Among his most important discoveries (1944, 1946, 1949, 1950) was the first genetic evidence for apomixis in *Parthenium,* coupled with

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

detailed study of natural and artificial interspecific hybridization in the genus.

Reed returned to Harvard as associate professor and director of the Gray Herbarium (effective July 1, 1948) and became the fifth director (1948-78) of that prestigious herbarium, succeeding his mentor, Fernald, who followed Benjamin Robinson, Sereno Watson, and Asa Gray.

At the international level, Reed was one of the founders, the first vice-president (1950-54), and the second president (1954-59) of the International Association for Plant Taxonomy (IAPT). Since its foundation on July 18, 1950, Reed's thirty-one-year membership of the IAPT council continued uninterrupted through the Thirteenth International Botanical Congress (IBC) that was held in 1981 in Sydney. With his untimely death, we lost the last survivor of the “founding fathers” of IAPT and one of the truly outstanding systematists of this century.

Reed was the key person in organizing the Eleventh International Botanical Congress that was held in 1969 in Seattle. Prior to the selection of Seattle as the city for that congress, he did all of the initial home work to receive the support of the president of the University of Washington (Seattle), the National Academy of Sciences, and the Botanical Society of America. He also had to select members of the congress's national committee, chairman of that committee, and president of the IBC.

At the national scene, Reed was one of a six-person executive committee that founded, in February 1963, the Organization for Tropical Studies (OTS), a consortium of research institutions and universities, to educate, promote, and coordinate studies in tropical biology of students and scholars in the United States. Following the death in February 1964 of Norman E. Hartweg, the first president of OTS, Reed was elected by mail ballot as the second president.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

During his two-year presidency, he was instrumental in resolving a number of the organizational and funding problems that faced the young organization, and his actions marked a turning point in the establishment of a clear identity and stability, especially in the development of an infrastructure to supervise and run the graduate courses in Costa Rica. Reed served as a member of the executive committee from 1963 to 1968 and continued to attend the OTS board meetings through 1973.

At the Harvard level, Reed played a major role in unification of the plant and library material of the five botanical institutes (Gray Herbarium, Arnold Arboretum, Farlow Herbarium, Botanical Museum, and Harvard Forest). As a result of his dedication, patience, and leadership, unification of the collections became a reality, and he took on the responsibility of supervising and coordinating the entire process. These collections were later renamed the Harvard University Herbaria. He served as the Harvard's chairman of the Institute for Research in General Plant Morphology (1955-65), chairman of the Institute of Plant Sciences (1965-69), supervisor of the Bussey Institution (1967-78), and chairman of the Administrative Committee of the Farlow Library and Herbarium (1974-78). During his thirty-year (1948-78) tenure as the director of the Gray Herbarium, systematic and evolutionary botany reached a “golden era” and was the strongest in the history of the Harvard. Although Reed supervised twenty-one Ph.D. students between 1954 and 1980, he did not publish a single joint paper with any of his students based on their Ph.D. theses.

Reed conducted extensive field work throughout his professional career and concentrated primarily on collecting the mustards of the Pacific, mountain, southwestern, and southeastern states, as well as of northern Mexico. Although such work spanned nearly sixty years (1934 through 1993),

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the most productive period was between 1974 and 1993, when he and his second wife, Kathryn, covered all of the continental United States minus the northeastern and central states. Reed did not concentrate on the mustards of Greenland, Alaska, the Caribbean, Central and South America, or Canada, though he studied a few of the taxonomically difficult South American genera, especially *Menonvillea.*

During the 1950s, Reed concentrated his field research in Tennessee, Alabama, and neighboring parts of Texas, Mississippi, Georgia, and Kentucky, where he focused primarily on the genera *Lesquerella* and *Leavenworthia.* His principal goals were to study the extensive interspecific hybridization in the former genus (1954, 1957, 1973) and the sympatric isolation and breeding systems of the later (1963, 1977). His fifteen years of research and field work on the lesquerellas of the Nashville Basin, Tennessee, were quite rewarding. Not only did he discover several new species, but his painstaking and intensive field work, which was followed by extensive crossing experiments in Cambridge, led to publication of one of the classic studies on natural interspecific hybridization. It is interesting to note that Reed discovered and named *L. densipila* in 1952, but when he found several populations of suspected hybrid origin at the fork of Stones River, Tennessee, he immediately postulated that there ought to be yet another undescribed species that, with *L. densipila* of the western fork, was hybridizing at the fork of Stones River. Further continuous search along the east fork lead to the discovery of *L. stonensis,* which was described in 1955. He later discovered that the latter species also hybridizes with *L. lescurii* and that all of the auriculate-leaved species are interfertile in experimental hybridization, though they were initially isolated geographically, coming into contact subsequently due to human clearing of the areas where their ranges did not overlap.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

I took Reed's graduate seminar course in plant biosystematics in the fall of 1968, and I was very much impressed by his elegant reasoning and soft-spoken approach in discussing any taxonomic problem. Although students did most of the talking and discussions, ideas did not become crystallized until Reed gave his authoritative opinions. He used his extensive collections of *Parthenium, Lesquerella,* and *Leavenworthia* as models to introduce diverse taxonomic problems (e.g., hybridization, apomixis, polyploidy, allopatric and sympatric speciation, phenotypic plasticity) and to show how critical systematic methodology would lead to sound solutions. Reed's impressive approach provided us with a wealth of knowledge and helped us develop into young systematists with clear concepts. Perhaps the best reward that I and two of my colleagues (James E. Rodman and Charles Schnell) received from that course was to accompany Reed in his car for a one-week drive from Cambridge to the Nashville Basin, Tennessee, to study the populations of various species of *Leavenworthia* and *Lesquerella.* Unfortunately, that was the only time I joined Reed in any field work.

When the time came to select a thesis problem in 1968, I wanted to work on the Cruciferae. The Middle East, where I intended to work after my degree, is one of the richest centers of the family and I wanted to learn from Reed's extensive thirty-five-year experience with the family. Reed introduced me to several potential subjects and carefully pointed out where the problems were. He then told me to take as much time as I wanted to decide, and when the choice was made, he was instrumental in securing funds for me to conduct field work in mountain and Pacific states during the summers of 1968 and 1969. Reed gave the same absolute freedom to his other twenty Ph.D. students in deciding what to do, and he was always successful in find

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ing the financial resources to support the field studies of his students.

Reed's pioneering work on the guayule rubber plants in the late 1940s provided him with first-hand experience in genetics, hybridization, and asexual reproduction (especially apomixis) that he applied so elegantly in studies of the mustard family. Most of Reed's field work was specifically designed to solve a number of taxonomic problems, though he concentrated primarily on *Arabis, Physaria,* and *Lesquerella,* the three genera closest to his heart, the study of which resulted in numerous discoveries of new scientific novelties.

Shortly after publishing his first book (1973), Reed started compiling data on his second book, *The Cruciferae of Continental North America,* but the hard work on this project did not start until about the mid-1980s. Accompanied by Kate, he intensified the field work for sixteen years (1974-90), and the actual writing of his monumental work (1993) occupied them for about six years ending in March 1991. I had the chance to read every part of the book soon after Kate typed the first draft. During that period, the discussions I had with Reed about the various ins and outs were most enlightening and educational. The one thing I did not agree with was the alphabetical instead of the taxonomic or phylogenetic arrangement of taxa. The phylogeny of North American mustards was not much known in the late 1980s and early 1990s, and only during the past four years have we started to use molecular data and gain enormous insight about generic lines and relationships in the family. Now that I have been using Reed's book on a daily basis, I find the alphabetical arrangement most practical to search for any given plant.

Reed's research career on the Cruciferae spanned more that sixty years. Of the 778 species recognized in *The Cruciferae*

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

of Continental North America, 101 were weedy species introduced primarily from Eurasia. Of the 677 native North American species, Reed's name was attached to 203 species, or a remarkable 30%. The book also included 243 infraspecific taxa, and Reed's name was attached to 121 subspecies and varieties, or 50%. In short, Reed described 156 new species and 56 new varieties, and he made 47 new combinations at the specific rank and 65 at the subspecific and varietal ranks. Following the publication of the book in 1993, Reed described four species in *Lesquerella,* thereby bringing the total North American species of the genus to 87.

Reed served as a member of the editorial committee of the International Code of Botanical Nomenclature for nearly thirty years. In addition, he was the editor in chief of *Rhodora* from 1950 to 1963, *Contributions from the Gray Herbarium* from 1950 through July 1978, *Occasional Papers of the Farlow Herbarium of Cryptogamic Botany* from June 1974 through July 1978, and *Publications from the Bussey Institution of Harvard University* from 1975 through 1979. Kate Rollins was co-editor of *Contributions from the Gray Herbarium* from January 1971 through November 1984 and co-editor of *Occasional Papers of the Farlow Herbarium of Cryptogamic Botany* from June 1974 through April 1980.

My fondest memories of Reed date back to 1969-73, when he, Kate, and the graduate students met weekly for two hours in his office. Not only were those meeting most memorable and educational but they were such great fun. The subjects ranged from science to politics, history of botany, and sports. Birthdays were never missed. Reed created such a wonderful bond with his students, and he was an exemplary friend, academic father, and true gentleman. After returning to Harvard in 1981, my wife and our two boys strengthened our bond with Reed and Kate, and we enjoyed

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

visiting them in their summer home in Maine and kept in close touch until five days before Reed's death.

Reed enjoyed good health basically throughout his life, but his heart problems started about two years before his death. He is survived by a daughter, Linda White, and a son, Richard, from his first marriage; a sister, Aileen Carter; his second wife, Kathryn; and five grandchildren and two stepdaughters. His memorial service was held on May 22, 1998 in the Appleton Chapel of Memorial Church (Harvard University).

Reed received several distinguished awards, including the Centenary Medal of the French Botanical Society (1954), Certificate of Merit of the Botanical Society of America (1960), medals of the Ninth (1952), Eleventh (1969), and Twelfth (1975) International Botanical Congresses, Congress Medal on the 25^{th} anniversary of the International Association for Plant Taxonomy (1975), Gold Seal of the National Council of State Garden Clubs (1981), Asa Gray Award of the American Society of Plant Taxonomists (1987), and the Twenty-fifth Anniversary Medal of the Organization for Tropical Studies (1988). Two genera, *Reedrollinsia* J. W. Walker and *Rollinsia* Al-Shehbaz, eight species, and two varieties were named in honor of Reed.

In addition to membership of the National Academy of Sciences and the American Academy of Arts and Sciences, Reed was a member of many societies, including the Society for the Study of Evolution, the Genetic Society of America, and the Linnaean Society of London. He was president of the American Society of Plant Taxonomists (1951-52), president of the International Association of Plant Taxonomists (1954-59), president of the New England Botanical Club (1955-57), president and chairman of the board of the Organization for Tropical Studies (1964-65), and president of the American Society of Naturalists (1966).

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1939

The cruciferous genus *Physaria* . *Rhodora*41: 392-415 .

1941

A monographic study of *Arabis* in western North America. *Rhodora* 43: 289-325, 348-411, 425-81 . Also published in *Contrib. Gray Herb.* vol. 138 (same pagination as in *Rhodora*).

1944

Evidence for natural hybridity between guayule (*Parthenium argentatum*) and mariola (*Parthenium incanum*). Am. J. Bot. 31: 93-99 .

1946

Interspecific hybridization in *Parthenium.* II.Crosses involving *P. argentatum, P. incanum, P. stramonium, P. tomentosum* and *P. hysterophorus. Am. J. Bot.* 33: 21-30 .

1949

Sources of genetic variation in *Parthenium argentatum* Gray (Compositae). *Evolution*3: 358-68 .

1950

The guayule rubber plant and its relatives. *Contrib. Gray Herb.* 172: 1-73 .

1952

Taxonomy today and tomorrow. *Rhodora*54: 1-19 .

1954

Interspecific hybridization and its role in plant evolution. *Eighth Internatl. Bot. Congress, Rapp. & Comm.* Sec. 9 & 10: 172-80 .

1955

A revisionary study of the genus *Menonvillea* (Cruciferae). *Contrib. Gray Herb.*177: 3-57 1.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1957

Taxonomy of higher plants. *Am. J. Bot.* 44: 188-96 .

Interspecific hybridization in *Lesquerella. Contrib. Gray Herb.*181: 3-40 .

1958

The genetic evaluation of a taxonomic character in *Dithyrea* (Cruciferae). *Rhodora* 60: 145-52 .

1959

Taxonomy and the International Association. *Taxon* 8: 277-79 .

1963

The evolution and systematics of *Leavenworthia* (Cruciferae). *Contrib. Gray Herb.*192: 3-98 .

1965

On the basis of biological classification. *Taxon*14: 1-6 .

1967

The evolutionary fate of inbreeders and nonsexuals. *Am. Natur.*101: 343-51 .

1973

With O. T. Solbrig. Interspecific hybridization in*Lesquerella*. *Contrib. Gray Herb.*203: 3-48 .

With E. A. Shaw. *The Genus* Lesquerella *(Cruciferae) in North America*. Cambridge, Mass.: Harvard University Press.

1975

With U. C. Banerjee. Atlas of the trichomes of *Lesquerella* (Cruciferae). *Publications of the Bussey Institution of Harvard Univ.* 48 pp.

1977

With O. T. Solbrig. The evolution of autogamy in species of the mustard genus *Leavenworthia*. *Evolution*31: 265-81 .

1979

With U. C. Banerjee. Pollen of the Cruciferae. *Publications of the Bussey Institution of Harvard Univ.,* pp. 33-64

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1982

A new species of the Asiatic genus *Stroganowia* (Cruciferae) from North America and its biogeographic implications. *Syst. Bot.* 7: 214-20.

1983

Interspecific hybridization and taxon uniformity in *Arabis* (Cruciferae). *Am. J. Bot.*70: 625-34.

1986

With I. A. Al-Shehbaz. Weeds of south-west Asia in North America with special reference to the Cruciferae. *Proc. R. Soc. Edinburgh*89B: 289-99.

1993

The Cruciferae of Continental North America. Stanford, Calif.: Stanford University Press.

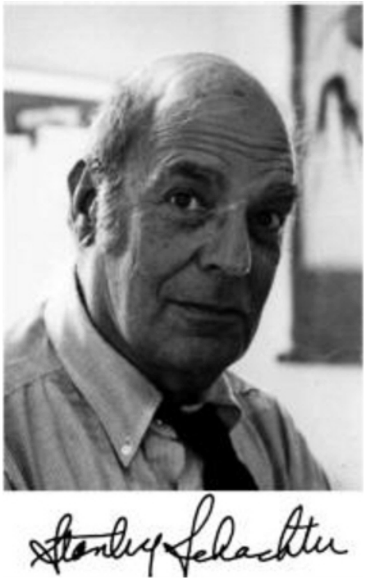
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.



Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

STANLEY SCHACHTER

April 15, 1922–June 7, 1997

BY RICHARD E. NISBETT

STANLEY SCHACHTER was one of the very few social psychologists ever elected to the National Academy of Sciences (in 1983). His contributions ranged across the study of communication and social influence, group processes, sources of the affiliation motive, intellectual and temperamental correlates of birth order, nature of emotional experience, people's ability to correctly attribute the causes of their behavior to external versus internal factors, causes of obesity and eating behavior disorders, the addictive nature of nicotine, psychological reactions to events that affect stock market prices, and the proper interpretation of “filled” (“uh,” “er”) pauses in speech. Few, if any, social psychologists ever made contributions over a wider range of topics. Remarkably, the diverse content of the contributions was tied together by a small number of powerful theoretical concepts.

Stanley Schachter was born on April 15, 1922, to Nathan and Anna Schachter in Flushing, then a semi-rural part of Queens, New York. Knowing that he wanted to go away to school, but knowing nothing of the rarefied and preppy atmosphere he was about to enter, he chose Yale, where he initially majored in art history. He stayed on for a master's degree in Yale's psychology department, which he found

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

far more to his liking than the undergraduate school. The main intellectual influence on Schachter at Yale was Clark Hull, one of the founding fathers of learning theory.

After a stint working on vision in the Aero-Medical Laboratory of the Armed Services during World War II, Schachter found he was eager to work on pressing social problems. This took him to MIT in 1946 to work with the great German social psychologist Kurt Lewin, who had just set up his Research Center for Group Dynamics for the theoretical and applied study of social issues at that school. The other younger faculty members were Dorwin Cartwright, Leon Festinger, Ronald Lippitt, and Marion Radke, all to become distinguished social psychologists. The first two-year cohort of students included many who were to become eminent social psychologists, including Kurt Back, Morton Deutsch, Murray Horwitz, Harold Kelley, Albert Pepitone, John Thibaut, and Ben Willerman. On Lewin's death in 1947, the Research Center for Group Dynamics moved to the University of Michigan, where it became a part of the Institute for Social Research. Schachter received his Ph.D. from Michigan in 1949.

Schachter's dissertation adviser and most influential mentor was Leon Festinger. With Festinger, Schachter studied communication and social influence and, together with Henry Riecken, they wrote a book entitled *When Prophecy Fails* (1956), describing what happened to a millenial group that had predicted the end of the world on a date certain. The appointed hour came and went, but the group's adherents did not give up their beliefs. On the contrary, they decided their faith had saved the world and began to proselytize for converts! Though this finding might seem a mere curio, it gave rise to much interesting social psychology, including Festinger's celebrated cognitive dissonance theory. It also

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

played a key role in showing Schachter how powerful social influence could be.

Schachter's first job was at the University of Minnesota, and he remembered both the city of Minneapolis and the university with great fondness. In 1961 Schachter moved to Columbia, the university from which he ultimately retired. Schachter and his wife, Sophia Duckworth, loved the city of New York, as well as their summer residence on Long Island. The couple had a son, Elijah.

The effect that Schachter had on people was very much the same whether they were his fellow eminent scientists or the lowliest of beginning graduate students. He was charismatic, funny, a wonderful gossip (but never in a malicious way), thought provoking, and unpretentious. He encouraged his students to be equally unpretentious, by his example and by his habit, after a student had just produced a particularly sententious observation, of insisting that the student repeat the observation, but this time in language that would be used for the student's grandmother.

Schachter's non-professional interests were as protean as his professional ones. He loved art, literature, the theater, the beach, tennis, backgammon, and offbeat scientific facts from fields as diverse as geography and medicine. Partly for esthetic reasons, he was incapable of conducting boring research—including the sort of potboilers that even the best scientists are likely to conduct to make sure they are productive. His esthetic sense and his capacity to enjoy himself at play prevented Schachter from being the sort of workaholic that many great scientists are. He enjoyed himself enormously outside of work, and probably in part because of that, in his work as well.

Schachter had the great good fortune to work briefly with Kurt Lewin, and then with Lewin's student Leon Festinger. Both men understood that social psychology could

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

be an experimental science like any other. Schachter's dissertation, published in 1951, became one of the most famous experimental demonstrations of a social process ever conducted up to that point. It showed the massive pressures to conform that are brought to bear on deviates from a group norm and the sorts of punishment that are administered to those who fail to toe the line. The study also showed the “carrot” side of group pressure. Deviants who join the opinion fold may be fully forgiven for the error of their previous ways.

The dissertation was inspired by work on social influence in MIT married student quarters done earlier in graduate school with Festinger and the sociologist Kurt Bach. This work indicated that people were defining the same objective situation in very different ways, depending on their accidental exposure to people having one set of views versus another. This work laid the groundwork for one of the major theoretical themes of Schachter's career, namely the great power of social factors in determining people's understanding of reality. The project incidentally showed the remarkable importance of physical distance and functional distance (e.g., proximity to the same staircase as another individual versus proximity to another staircase) in determining who communicates with whom. This work is by now known to every architect, and presumably influences the way they construct environments.

Schachter brought some aspects of the experimental techniques created by his dissertation to the study of the bases of group affiliation (1959). He was able to show that people sometimes affiliate to find out what emotions to experience in a given situation. When the situation is ambiguous, but potentially threatening, people seem to require knowledge of other people's emotional states to help them decipher their own. Moreover, this turns out to be particularly true

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of first-born children, who seem to have needs for conventional, and conventionally admired, behavior. These facts have implications, demonstrated by Schachter and his students, for levels of educational attainment and for professional success in a variety of occupations that are extreme in the extent to which one must be alone or together with others.

One of the most interesting aspects of the affiliation work was that it implied that emotions are sometimes “constructed” cognitively rather than produced directly by a given stimulus situation. One of Schachter's most influential projects, carried out with Jerome Singer, Bibb Latané, and Ladd Wheeler, was the study of the construal processes underlying emotional experience (1962). They showed that people who are aroused from some unknown source (for example, from an injection containing adrenaline) can be influenced to experience anger, euphoria, or fear, depending on the situation in which they are placed. Schachter argued that the physiological substrate of all strong emotion, or at any rate the peripheral, non-central nervous system substrate, may be the same. It is the construal of situations, often aided by cues from other people, that determines precisely which emotion will be experienced.

An even more important outgrowth of the research on emotions was a generalization of the point about the construal of causes. Attribution of causality for one's own emotions and behaviors is a far more subjective matter than had previously been assumed. It is possible to arouse people by some purely physiological means and have them attribute the arousal exclusively to some external source, such as a social situation that can be interpreted as threatening. Contrariwise, it is possible to prevent people from having the emotion they would normally experience in response to an arousing situation by having them mistakenly attribute the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

arousal to a drug (actually a placebo) that they have been given. In essence, all of the arousal is attributed to the drug, and less emotion is experienced than if participants had not been told that they had been given a drug that would cause arousal. Thus causal attributions for one's own behavior are far more subjective, and the malleability of emotions is far greater, than had previously been supposed.

The work on the attribution of emotions was one of the centerpieces of research on causal attribution, which dominated the field of social psychology in the 1970s. The work was also of substantial practical importance, in part because it showed that the placebo effect counted on by physicians could sometimes backfire. For example, to tell insomniacs, whose worries prevent them from sleeping, that they have been given a drug (actually a placebo) that should help them to sleep, could have the paradoxical effect of convincing patients that they are particularly upset on the nights they take the drug. Assuming they are as aroused as usual, they can only surmise that, since the drug is proving ineffective, they must be particularly distressed. Greater insomnia should result—a finding actually obtained by researchers working from the Schachter-Singer theoretical position.

The work on emotional states gave rise to two provocative lines of research. With Bibb Latané and Stuart Valins (1964), Schachter studied primary sociopaths (individuals who show low affect and are often criminals caught doing things that any normal person would be too frightened to do). Sociopaths had been found by David Lykken to learn anxiety-mediated avoidance behavior more slowly than normals. Contrary to what one might expect, however, sociopaths had higher levels of chronic arousal than normal individuals. Despite this, Schachter and Latané found that when they injected sociopaths either with saline solution or adrenaline, subjects in the latter group learned how

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

to avoid shock more readily than subjects in the former group.

In other inventive work conducted with, among others, Larry Gross, Richard Nisbett, Patricia Pliner, Judith Rodin, and Lee Ross, Schachter (1968) reasoned that, since physiological arousal symptoms are so plastic and can be attached to so many emotional states, perhaps they can even be interpreted as organismic states not normally regarded as emotions. Perhaps, he guessed, they can even be interpreted as hunger signals. If a person were burdened with a habit of interpreting arousal in that way, that person, given a life of even ordinary stress, might become obese. Schachter found no evidence of such a tendency on the part of the obese, but what he did find was even more interesting.

The obese turned out to be relatively unmotivated to eat by food deprivation, but highly motivated to eat by external cues, such as the taste and availability of food. Schachter guessed that the hyper-responsivity to external cues, in a world where such cues abound, would leave the individual prey to the temptations of overeating. This interpretation subsequently gave way to the view, advocated by students of Schachter who continued to work on the problem, that the cart had been placed before the horse in the theorizing. The obese in our society are typically food-deprived because they are attempting to keep their weight down, hence minor manipulations of short-term deprivation are far less important to them than to normal-weight individuals who are regulating their intake in part on the basis of short-term changes in caloric need. But precisely because the obese are so hungry, they can be led to eat large amounts by external cues—in effect, to get off the wagon when good-tasting food lies close at hand.

Continuing his interest in the relation between biological and cognitive states, Schachter (1978), along with stu

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

dents who included Lucy Friedman, Neil Grunberg, Peter Herman, and Lynn Kozlowski, studied the addictive properties of nicotine. It should be recalled that as late as the 1970s, it was still controversial whether nicotine was addictive. In a double-blind experiment, Schachter found that people asked to smoke low- or high-nicotine cigarettes on alternate weeks reported smoking more on low-nicotine weeks, thus indicating that they were titrating to a degree their exposure to the drug. More importantly, because nicotine is an alkaloid, and its rate of excretion is determined by the pH, or degree of acidity, of the urine, it is easy to manipulate urinary pH with bicarbonate of soda or fruit juices. Schachter found that stress increased the acidity of the urine. When Schachter decreased the acidity of smokers' urine, he found that this reduced their smoking under stressful conditions.

In addition to all the other claims that can be made about the importance of Schachter's career, it can be argued on the basis of his work on the attribution of emotions, sociopathy, obesity, and smoking that he was the founder of modern health psychology. This field applies the findings of social, personality, and cognitive psychology to problems of physical and mental health. Much of the early work in the field made explicit reference to Schachter's research.

In later work, perhaps because human subjects review boards were making it difficult for psychologists to conduct research in which they deceived their subjects or placed them in uncomfortable situations, Schachter became interested in aggregate-level phenomena. He found, for example, that department store sales were off the day after a widely publicized crime, presumably because people were temporarily hesitant to go out. He was able to apply these findings to such practical matters as the behavior of the stock market. For example, the number of stories about violence

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

in the newspapers or the recent occurrence of an airline catastrophe had predictable, though usually small and ephemeral, effects on the market.

Aside from the important content contributions made in each of these research areas, Schachter bequeathed an orientation toward the conduct of research that revolutionized the study of "soft" topics in psychology—those questions of social and personality and clinical psychology that seem intrinsically to be so difficult to study that only suggestive progress can be made. Schachter's approach consisted in part of devising control conditions that were so similar in all respects to the experimental condition—save the theoretically crucial one—that alternative explanations were difficult to sustain. Most prior research in the soft areas of psychology used a methodology that simply showed that individuals of type X do behavior A more than individuals of type Y. Schachter showed that it was often possible to show, whereas X do more A than Y in Situation 1, there is no difference between X and Y in Situation 2, where, according to his theory, there should be no difference. It is hard today to recognize just how much cleverness was required to invent such manipulations, so routine have they become. An equally important aspect of Schachter's orientation to research was to combine elegant experimentation with yeasty real-world tests of theory. In general, the sorts of objections that apply to laboratory experiments do not apply to real-world studies, and vice versa. The combination of the two types of research made Schachter's work uniquely convincing. (Though it must be admitted that his work was sometimes more convincing than it might have been simply because of the charm of his prose style! Example: "I don't think I have ever seen a hypothalamus, though I'm pretty sure I've eaten one in a French restaurant.")

Almost equally striking was Schachter's contribution to

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the nature of theorizing in social and personality psychology. Prior to his entrance on the scene, much theorizing was highly complex and was derived from large, overarching frameworks such as psychoanalytic theory and learning theory. In contrast, Schachter's theorizing was ad hoc, in the sense that the theory was designed to generalize from the facts at hand about a particular phenomenon rather than to find some way to bend a pre-existing theory to fit the particulars. His theories were of the sort that the sociologist Robert Merton has called approvingly “theories of the middle range.” By comparison to most researchers, Schachter's theorizing was always to a very simple account, one that often seemed odd and implausible at first encounter but eventually began to seem commonsensical. His theorizing was perhaps sometimes oversimplified, and his hedgehog stance nearly always annoyed the foxes of the field, but it has proved far easier to build upon and when necessary to correct Schachter's simple and commonsensical theories than to work with their more “sophisticated” competitors.

Among Schachter's most important contributions to psychology was his training of graduate students. It is doubtful that any social psychologist ever trained so many distinguished people. Through a combination of charm and a sense of adventure, he made the conduct of psychological research exciting. He elicited as much from his students as there was to be drawn from them. His students, in turn, have been successful in operating in a similar way with their own students. A remarkable fraction of the most highly regarded social psychologists in the country are the intellectual children, grandchildren, and now even great-grandchildren of this multiply talented investigator with his protean interests.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

NOTES

This memoir is based in part on many discussions with Schachter's students over the years. The author is indebted to Julian Hochberg and Lee Ross for comments on an earlier version of this memoir.

There is a fascinating and useful autobiography by Schachter in *A History of Psychology in Autobiography,* ed. G. Lindzey. Stanford: Stanford University Press, 1989. Other useful material about Schachter can be found in a festschrift edited by N. E. Grunberg, R. E. Nisbett, J. Rodin, and J. E. Singer: *A Distinctive Approach to Psychological Research: The Influence of Stanley Schachter.* Hillsdale, N.J.: Lawrence Erlbaum Associates, 1987.

Schachter's papers are archived at the Bentley Historical Library of the University of Michigan.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1950

With L. Festinger and K. Back. *Social Pressures in Informal Groups.* New York: Harpers.

1956

With L. Festinger and H. Riecken. *When Prophecy Fails.* Minneapolis: University of Minnesota Press.

1959

The Psychology of Affiliation. Stanford: Stanford University Press.

1951

Deviation, rejection and communication. *J. Abnorm. Soc. Psychol.* 46: 190-207 .

1962

With J. Singer. Cognitive, social and physiological determinants of emotional state. *Psychol. Rev.*69: 379-99 .

1963

Birth order, eminence and higher education. *Am. Sociol. Rev.*28: 757-68 .

1964

The interaction of cognitive and physiological determinants of emotional state. In *Advances in Experimental Social Psychology,* ed. L. Berkowitz, pp. 49-79 . New York: Academic Press.

With B. Latane. Crime, cognition and the autonomic nervous system. In *Nebraska Symposium on Motivation,* ed. D. Levine, pp. 221-73 . Lincoln: University of Nebraska Press.

1968

Obesity and eating. *Science*161: 751-56 .

1971

Some extraordinary facts about obese humans and rats. *Am. Psychol.*26: 129-44 .

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Emotion, Obesity and Crime. New York: Academic.

1974

With J. Rodin. *Obese Humans and Rats.* Hillsdale, N.J.: Erlbaum.

1977

Nicotine regulation in heavy and light smokers. *J. Exp. Psychol.*106: 5-12 .

1978

Pharmacological and psychological determinants of cigarette smoking. *Ann. Intern. Med.*88: 104-14 .

1980

Nonpsychological explanations of behavior. In *Retrospective on Social Psychology,* ed. L. Festinger, pp. 131-57 . New York: Oxford University Press.

1982

Recidivism and self-cure of smoking and obesity. *Am. Psychol.*37: 436-44 .

1985

With D. C. Hood and P. Andreassen. Random and non-random walks on the New York Stock Exchange. *J. Econ. Behav. Organ.*6: 331-38 .

1989

Stanley Schachter. In *A History of Psychology in Autobiography,* ed. G. Lindzey. Stanford: Stanford University Press.

1991

With N. J. S. Christenfeld, B. Ravina, and F. R. Bilous. Speech disfluency and the structure of knowledge. *J. Pers. Soc. Psychol.*60: 362-67 .

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

GLENN THEODORE SEABORG

April 19, 1912–February 25, 1999

BY DARLEANE C. HOFFMAN

GLENN T. SEABORG WAS a world-renowned nuclear chemist, educator, scientific advisor to 10 U.S. presidents, humanitarian, and Nobel laureate in chemistry. He is probably best known for his leadership of the team that in 1941 accomplished the first chemical separation and positive identification of plutonium and his "revolutionary" actinide concept (1944) in which he placed the first 14 elements heavier than actinium in the periodic table of elements as a *5f* transition series under the lanthanide *4f* transition series. He went on to be codiscoverer of 9 elements beyond plutonium, culminating in 1974 in the production of element 106, later named seaborgium in his honor.

Seaborg was also well known as an educator and for his tireless efforts to improve U.S. science education at all levels. He served as chancellor of the University of California at Berkeley from 1958 until 1961 when he was called by President-elect John F. Kennedy to chair the U.S. Atomic Energy Commission, a position he held until 1971. Seaborg led the negotiations resulting in the limited nuclear test ban treaty prohibiting the testing of nuclear devices in the atmosphere or under the sea, approved by the U.S. Senate in 1963. He strongly supported the use of nuclear energy as

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

a source of electricity, and led delegations to some 63 countries, including the Soviet Union to promote the peaceful uses of atomic energy.

1912-1942: EARLY LIFE, EDUCATION, AND MARRIAGE

Glen Theodore Seaborg was born of Swedish ancestry in Ishpeming, Michigan, a small iron-mining town on the Upper Peninsula of Michigan. His father, Herman Theodore Seaborg, whose parents had come from Sweden to Ishpeming in their youth and met and married there in 1872, was born in 1880 in Ishpeming. His mother, Selma Olivia Eriksson (changed to Erickson), was born in Grängesberg in the southern Dalarna region of Sweden, and came to Ishpeming in 1904, when she was seventeen years old. Glen's parents met at a picnic on Swedish Midsummer's Day (June 24, 1908) and were married three years later on Swedish Midsummer's Day (June 24, 1911). Glen was born in Ishpeming on April 19, 1912; his only sibling, Jeanette, was born two years later.

Ishpeming had typical sections that were nearly all Swedish and it was in one of these that the Seaborgs lived. Since Glen's father was fluent in Swedish and it was his mother's native tongue, the Swedish language was spoken in the home, and he learned to speak and understand Swedish before English. He started kindergarten in the High Street School in Ishpeming in September 1917 and continued there through the first three grades. Glenn was nicknamed “Lanky” because he was so much taller than his classmates.

When he was ten years old, the family moved to Home Gardens, now a part of South Gate, near Los Angeles, California. (At this time he changed the spelling of his name from “Glen” to “Glenn.”) This move to California was made primarily because his mother wanted to broaden her children's horizons beyond the limited opportunities avail

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

able in Ishpeming. But Glenn Seaborg never forgot his roots in Ishpeming and was always very proud of his Swedish ancestry. Unlike in Ishpeming, where his father would have been guaranteed employment for life as a machinist in the iron works, in California his father never found permanent employment in his trade, and the family finances were in rather poor condition. Glenn early on earned his own spending money by taking paper routes, mowing lawns, and performing other odd jobs.

Glenn attended high school in the Los Angeles suburb of Watts and developed a special interest in chemistry and physics, which he attributed to his inspiring high school chemistry and physics teacher, Dwight Logan Reid. Seaborg graduated as valedictorian of his class in 1929. He first obtained work in a warehouse as a stevedore and then found summer employment as a night laboratory assistant in the Firestone Tire and Rubber Company to earn money for his freshman year at the University of California, Los Angeles (UCLA). This made it just barely possible for him to enter UCLA in the depression year of 1929, since it was a tuition-free public university and he could live at home and commute with friends some 20 miles to UCLA. He also worked at a variety of jobs to help earn his way. He decided to major in chemistry rather than physics because he felt it would provide him with more career opportunities if he were unable to find a position as a university teacher. During his last year, he became particularly interested in the exciting new developments in nuclear physics and chemistry. After receiving his A.B. degree in chemistry in 1933, he stayed on a fifth year (1933-34) to take a number of courses in physics, which had just that year been started at the graduate (master's degree) level. Because graduate work had not yet been instituted in the Department of Chemistry, he then went to Berkeley to pursue graduate work in

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

chemistry, hoping to work near the great Professor Gilbert Newton Lewis, dean of the college of chemistry, and the rising young nuclear physicist Ernest Orlando Lawrence, who invented the cyclotron in the early 1930s, for which he received the Nobel Prize in physics in 1939.

Seaborg described the atmosphere that existed at the University of California at Berkeley (UCB) when he entered as a graduate student in August 1934 as “exciting and glamorous,” and he took formal courses in chemistry from many eminent professors. Seaborg earned his Ph.D. in chemistry in the spring of 1937 with a thesis on the inelastic scattering of fast neutrons. It was the depth of the Depression and suitable positions were difficult to find, but he was soon asked by Lewis to stay on at Berkeley to serve as his personal research assistant. Together they published several papers; Seaborg always regarded Lewis as one of the scientific geniuses of our time and often expressed his admiration for him as his great teacher and mentor. In 1939 Seaborg became an instructor at Berkeley and in 1941 he was promoted to assistant professor. During this period he collaborated with the physicist J. J. (Jack) Livingood to use the newly completed 37-inch cyclotron to produce and discover several dozen new isotopes. Many of these, including iodine-131, are still widely used in nuclear medicine procedures. These experiences as a “radioisotope hunter” led eventually to the exploration of the transuranium elements, his life-long research interest.

During Seaborg's graduate years and later, he closely followed the developments from Enrico Fermi's group in Italy, which was bombarding uranium with neutrons and producing what they thought were transuranium elements, and the research of Otto Hahn, Lise Meitner, and Fritz Strassmann in Berlin on these so-called transuranium elements. These results were widely discussed in Berkeley at the weekly nuclear

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

seminars and physics journal club meetings. In January 1939 the exciting news of the discovery of nuclear fission by the Berlin Group came through to Berkeley by word of mouth. Edwin M. McMillan and Philip H. Abelson then set out to study the fission process in bombardments of uranium with neutrons at Berkeley's new 60-inch cyclotron. Quite unexpectedly, they produced and identified the first “real” transuranium element, which they chemically separated and identified as element 93, for which they proposed the name neptunium. McMillan then began a search for the next heavier transuranium element (atomic number 94), but was soon called to wartime research at the Massachusetts Institute of Technology. Seaborg received McMillan's permission to continue this search. In February 1941 Seaborg led a team consisting of fellow instructor Joseph W. Kennedy and Seaborg's first graduate student, Arthur C. Wahl, in performing the first chemical separation and positive identification of plutonium. It was produced as the isotope plutonium-238 in deuteron bombardments of uranium. Soon after, the new isotope plutonium-239 was produced and was found to be highly fissionable. (Because of potential military applications in nuclear weapons, these results were voluntarily withheld from publication until 1946, after World War II.) These discoveries led to the U.S. decision to undertake a crash program to develop nuclear reactors for plutonium production to be used in the U.S. atomic bomb project. In April 1942 Seaborg took a leave of absence from Berkeley to go to the University of Chicago Metallurgical Laboratory to direct the work on the chemical extraction and purification of the plutonium produced in the reactors.

As soon as the decision was made in March 1942 that Seaborg should move to Chicago for work on the plutonium project, he immediately proposed to Helen Lucille

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Griggs (then E. O. Lawrence's secretary), who he had been dating since the fall of 1941, and she accepted. The understanding was that he would soon come back to Berkeley and they would be married. In June 1942 Seaborg did return from Chicago to Berkeley and took Helen to visit his parents in South Gate. He then persuaded her to return with him to Chicago by train, promising they would be married en route. They disembarked from the train at Caliente, Nevada, and were subsequently married at Pioche, Nevada, on June 6, 1942, but not without some interesting “misadventures.” These have been charmingly described by Helen Seaborg. (See Helen's account quoted by Seaborg in his preface to the book *The Transuranium People: The Inside Story* by D. C. Hoffman, A. Ghiorso, and G. T. Seaborg, pp. 42-44.) Helen and Glenn's marriage was to last for more than 56 years and Seaborg often fondly referred to Helen as “his best discovery of all,” an assessment with which I totally agree!

They had six children: Peter Glenn, who died in 1997, Lynne Annette Seaborg (Mrs. William B. Cobb), David Michael, Stephen Keith, John Eric, and Dianne Karole, all of whom survive Glenn. Helen was his constant companion and advisor and accompanied him on most of his many trips, faithfully attending the scientific and other symposia in which he was involved. I personally was always most gratified to have her support in the audience when I spoke at some of these meetings.

1942-1961: SEPARATION OF PLUTONIUM AND DISCOVERY OF ELEMENTS 95-102

Seaborg headed the Metallurgical Laboratory chemistry group, which was responsible for devising plant processes for chemical purification of plutonium for the World War II Manhattan Project to develop an atomic bomb. Remark

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

ably, these plant procedures, which were developed and later used in the manufacture of kilograms of plutonium at Clinton, Tennessee, and Hanford, Washington, were devised on the basis of experiments with milligrams or less of plutonium. This represents a scale-up of more than six orders of magnitude, leading to much initial skepticism about the success of the project! By 1944 the process chemistry for plutonium was essentially worked out, and Seaborg and his coworkers began attempts to produce and identify the next transuranium elements with atomic numbers 95 and 96 (americium and curium). They were unsuccessful until Seaborg came up with the actinide concept of heavy-element electronic structure in which the 14 elements heavier than actinium (atomic number 89) are placed in the periodic table of elements as a *5f* transition series under the lanthanide *4f* transition series. A new periodic table incorporating this concept was published in *Chemical &Engineering News* in 1945. It was viewed as a "wild" hypothesis because at that time it was commonly believed that thorium, protactinium, uranium, neptunium, plutonium, and the following elements should be placed as the heaviest members of groups 4 through 10. But Seaborg postulated that the heavier actinides, like their lanthanide counterparts, would be extremely difficult to oxidize above the trivalent oxidation state. This concept was verified when chemical separations based on separating elements 95 and 96 as trivalent homologues of the lanthanides were successfully used in 1944 to separate and identify these new elements, subsequently named americium and curium.

After the end of World War II in May 1946, Glenn Seaborg returned to Berkeley from Chicago as full professor of chemistry, bringing with him some of his associates, including Isadore Perlman, Burris B. Cunningham, Stanley G. Thompson, and Albert Ghiorso. In the following years up to

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1958, Seaborg, Thompson, Ghiorso, and coworkers, including many graduate students and postdoctoral fellows, went on to synthesize and identify the next six transuranium elements with atomic numbers 97 through 102. The first of these, berkelium (97) and californium (98), were produced at the Berkeley 60-inch cyclotron in 1949-50. Shortly thereafter, in 1951, Seaborg and McMillan shared the Nobel Prize in chemistry for their research on the transuranium elements. Seaborg, who was only thirty-nine years old at the time, still remains the youngest winner of this honor.

Elements 99 and 100 were most unexpectedly produced in the debris from the first thermonuclear device, which was designed and tested by the Los Alamos Scientific Laboratory on Eniwetok Atoll in the South Pacific on November 1, 1952. Its huge yield of some 10 megatons created such an instantaneous high neutron flux that at least 17 neutrons were captured by the ^{238}U in the device. Seaborg's group at Berkeley was the first to separate and obtain evidence for these new elements, working together with scientists from Argonne National Laboratory and Los Alamos to confirm these results. The group proposed the names einsteinium and fermium for these elements in honor of the great scientists Albert Einstein and Enrico Fermi. Professor Seaborg and coworkers then produced mendelevium (101) in 1956 using the 60-inch cyclotron and nobelium (102) in 1958 using the heavy ion linear accelerator at the Berkeley Radiation Laboratory. According to the actinide hypothesis, it was expected that nobelium should have a relatively stable 2+ state by analogy with ytterbium, which can be reduced from 3+ to 2+ with strong reducing agents. However, it was found that not only is the 2+ state of nobelium achievable, it is the most stable oxidation state of nobelium in aqueous solution.

During the period 1946-58, Seaborg served as director of

Copyright © National Academy of Sciences. All rights reserved.

the Nuclear Chemistry Division and in 1954 became an associate director of the Berkeley Radiation Laboratory. In addition to the research on the production and chemical properties of the transuranium elements, the division discovered dozens of new isotopes and furnished much of the data on alpha-particle radioactivity and nuclear energy levels needed for the evolution of modern theories of nuclear structure.

Seaborg also began to broaden his horizons to national public service and served from 1947 to 1950 on the first General Advisory Committee to the Atomic Energy Committee. Consistent with his lifelong interest in athletics, he accepted Chancellor Clark Kerr's invitation to serve as Berkeley's faculty athletic representative from 1953 to 1958 and played a leading role in organizing the Athletic Association of Western Universities. When Kerr became president of the University of California in 1958, Seaborg was asked to serve as chancellor, which he did until 1961, when President-elect John F. Kennedy asked him to come to Washington, D.C., to chair the U.S. Atomic Energy Commission.

1961-1971: THE WASHINGTON, D.C. YEARS

Seaborg and his family moved to Washington, D.C., somewhat reluctantly, but he was eager to accept this new challenge as chairman of the U.S. Atomic Energy Commission (AEC), and he was granted a leave of absence from the University of California. Seaborg's tenure from 1961 to 1971 was longer than any other chairman's and spanned the presidencies of John F. Kennedy, Lyndon B. Johnson, and Richard M. Nixon. Seaborg led the negotiations resulting in the limited nuclear test ban treaty prohibiting the testing of nuclear devices in the atmosphere or under the sea, which was approved by the U.S. Senate in 1963. He strongly supported the use of nuclear energy as a source of electricity

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

and led delegations to some 63 countries, including the Soviet Union, to promote the peaceful uses of atomic energy. During the Johnson and Nixon administrations, the AEC played a significant role in the negotiation of the non-proliferation treaty and took the lead in instituting national and international safeguards to assure that nuclear materials were not diverted from peaceful uses to weapons purposes. He was a strong advocate of a comprehensive test ban treaty. As AEC chairman he continued his interest in transuranium element research and the National Transplutonium Production Program was established at the High Flux Isotope Reactor, which was commissioned at the Oak Ridge National Laboratory in the mid-1960s. That reactor and its associated transuranium processing facility were essential to the production of rare heavy-element isotopes used in the synthesis of new heavy elements and in heat sources for space exploration. Other radioactive isotopes for applications in biology, nuclear medicine, and industry were also produced. During his tenure the support for basic research in the physical sciences, biology, and medicine nearly doubled. Seaborg also became very interested in the improvement of teaching in science and mathematics and in attracting young people to careers in science.

1971-99: RETURN TO BERKELEY

Seaborg returned to Berkeley in 1971 and was appointed university professor of chemistry by the regents of the University of California. He continued to teach until 1979 and supervised the Ph.D. research of more than 65 students. In 1982 he became the first director of the Lawrence Hall of Science, which he founded. He served as associate director at large of the Lawrence Berkeley National Laboratory until his death in 1999. He was active in many international organizations for fostering the application of chemistry to

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

world economic, social, and scientific needs. He continued to speak out for better education in science and mathematics and was appointed in 1998 to the Commission for the Establishment of Academic Content and Performance Standards by California Governor Pete Wilson. In addition to the 1951 Nobel Prize in chemistry, he received a host of other honors and awards. These include selection in 1947 as one of America's ten outstanding young men by the U.S. Junior Chamber of Commerce; election to the National Academy of Sciences in 1948; American Society of Swedish Engineers' John Ericsson Gold Medal in 1948; AEC's Enrico Fermi Award in 1959; 1971 Nuclear Pioneer Award of the Society of Nuclear Medicine; Order of the Legion of Honor of the Republic of France, Decoration, 1973; 1979 Priestley Medal; Swedish Council of America's Great Swedish Heritage Award in 1984; University of California's 1986 Clark Kerr Medal; National Science Board's 1988 Vannevar Bush Award; 1991 National Medal of Science; and many other major awards from the American Chemical Society; 50 honorary degrees from various universities and election to a dozen foreign national academies of science. Seaborg is listed in the *Guinness Book of World Records* for having the longest entry in *Who's Who in America*!

The name “seaborgium” for element 106 was officially approved by the International Union of Pure and Applied Chemistry in 1997, an honor that Seaborg cherished more than the Nobel Prize. He held more than 40 patents, authored more than 500 scientific articles and numerous books, including his journals, which he faithfully kept throughout his career. These formed the basis for a number of books, including an autobiography published in 1998 entitled *A Chemist in the White House: From the Manhattan Project to the End of the Cold War.*

Seaborg was active in the American Chemical Society

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

throughout his career, serving as its president during its centennial year of 1976. One of his last accolades was being voted one of the top 75 distinguished contributors to the chemical enterprise over the last 75 years by the readers of *Chemical &Engineering News.* It was this award that he accepted at a huge ceremony and reception at the August 1998 American Chemical Society meeting in Boston the evening before his stroke.

Glenn loved to hike, and he and Helen laid out an interconnected network of 12-mile trails in the East Bay Hills above Berkeley extending to the California-Nevada border that forms a link in a cross-country trek of the American Hiking Society. Glenn was also a strong supporter of the Berkeley athletic program. Football was his favorite spectator sport and he liked to point out that during his tenure as chancellor the Berkeley football team went to the Rose Bowl!

SOME PERSONAL REMINISCENCES

Glenn Seaborg had a tremendous influence on me—both before and after I met him. Of course, as a nuclear chemist, I knew of his leadership in the legendary discovery of plutonium in 1941, the development of the actinide concept, his receipt of the Nobel Prize in 1951, and the discovery of 8 more transplutonium elements by 1958. It was not until his tenure as chairman of the AEC (1961-71) that I actually began to learn first hand about the “real” person behind these awesome accomplishments. One of my early vivid memories of him was from the 1969 Welch Conference on “The Transuranium Elements—The Mendeleev Centennial,” which I attended. He presented the opening lecture on “From Mendeleev to Mendelevium”; he gave the evening address commemorating the twenty-fifth anniversary of the discovery of americium and curium, he slipped out during one lunch hour to visit the M. D. Anderson

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Hospital to see the program to develop the use of californium-252 for cancer therapy; and he introduced all the speakers and presided at all the sessions, except for the final day, when he was at the Manned Spacecraft Center at 5 a.m. to observe the deployment by our astronauts on the moon of the SNAP-27 (plutonium-238) generator to power the instrument package to be left there. He was also instrumental in seeing that scientists from the Soviet Union were included on the program. The breadth of his interests, his skill in communicating with both scientists and the general public and press, and his energy in doing all this even while he was AEC chairman still boggles my mind!

In 1971 while I was at the Los Alamos Scientific Laboratory (now Los Alamos National Laboratory), I became better acquainted with him when my colleagues and I found minute quantities of primordial plutonium-244 in nature. He was immediately interested in our progress and announced it during his trip to Europe for the United Nations Conference on Peaceful Uses of Atomic Energy. We kept in touch after that, especially after I was appointed in 1974 to be a member of a committee of the international unions of pure and applied chemistry and of physics to consider competing claims to priority of discovery of elements 104 and 105 by Berkeley and Dubna (Soviet Union) scientists, and I visited Berkeley several times. Later, in 1976, we had many long discussions both in person and by telephone concerning the drafts and wording of an article on criteria for the discovery of new chemical elements, which was published in *Science*. In 1978-79 I spent a sabbatical year as a Guggenheim fellow in Berkeley with his group, leaving a few months early to return to Los Alamos as leader of the Chemistry-Nuclear Chemistry Division. I frequently returned to Lawrence Berkeley National Laboratory for experiments on spontaneous fission and searches for superheavy

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

elements. Glenn was still taking graduate students at that time, ultimately being advisor for 65 Ph.D. students. However, he wanted to ensure that heavy-element research would continue at Berkeley. He and his colleagues convinced the faculty of the Berkeley Chemistry Department, and then me, that I should return in 1984 as professor of chemistry and succeed him as group leader of the heavy-element nuclear and radiochemistry group. Thus began a very close association for more than 15 years. I learned so many things from him just by observing how he ran the weekly brown bag lunches with his graduate students and later mine—listening with great interest as they described their research progress. He asked insightful and penetrating questions, but not in a threatening manner, made suggestions, and frequently went to visit the labs late in the day to see what was going on. He also hosted many undergraduate research students. He was devoted to education and student training and would prepare as carefully for lectures to freshman chemistry classes as for presentations to prestigious assemblages of scientists.

Glenn was very concerned with history and had kept a diary or journal since he was eight years old. After his return to Berkeley from Washington in 1971, he continued the tremendous undertaking of putting them into book form, which occupied him and several helpers for many years. His journals also formed the basis for books on his years as chancellor at Berkeley, as chairman of the AEC, and many other topics. On the rare occasions that he did not remember something that one of us might ask about he would look it up in his journals. He had a fabulous memory and was able to synthesize and apply and keep track of what he knew so it could be applied to the situation at hand. One might almost say in the parlance of our time that he was a “parallel processor”!

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Seaborg had previously organized and published the proceedings of symposia commemorating the twenty-fifth anniversaries of the discoveries of the individual transuranium elements. In 1990 he decided that a symposium must be held to commemorate the fiftieth anniversary of the first chemical separation and proof of the discovery of plutonium on February 23, 1941. He asked me to help plan and organize this grand undertaking and we worked very closely on the project. Quite appropriately, at the symposium banquet in February 1991 the announcement of the initial establishment of the Glenn T. Seaborg Institute for Transactinium Science at Lawrence Livermore National Laboratory was made by its former director and Seaborg Ph.D. student (in 1951) Roger Batzel. The institute is devoted to the study of the transactinium elements with special emphasis on the education and training of future generations of scientists in heavy-element research.

Glenn Seaborg's legacy as a citizen-scholar was also commemorated by the establishment in 1998 of the Glenn T. Seaborg Center for Teaching and Learning Science and Mathematics and the new Seaborg Science Complex. They are located at Northern Michigan University in the Upper Peninsula, not far from his birthplace in Ishpeming. Their stated mission is to reach out to children young and old to invite them to share the excitement of learning that Seaborg held so dear.

Glenn continued to travel and lecture widely in the United States and abroad; I never ceased to marvel at his energy and ability to keep going. He maintained an active interest in issues such as the first studies of the chemical properties of the transactinide elements (atomic number > 103), the search for superheavy elements, science education, the nuclear test ban treaty, non-proliferation and the use of nuclear power, as well as hiking and sports. What a wonder

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ful advisor and resource he was, one I almost came to take for granted!

Our final collaborative effort, together with Albert Ghiorso, resulted in a book entitled *The Transuranium People: The Inside Story* published in late 1999. Of course, in typical fashion, he had his sections completed long ahead of time with appropriate figures and pictures, but Al and I worked long and hard to get all our pictures in and have him read our final texts. Somehow I was obsessed with getting this project completed and sent to the publisher before we left for the August 1998 American Chemical Society national meeting in Boston at which he suffered the stroke and fall that led to his death. One of my great regrets is that he did not live to participate in the joy of discovery of the new superheavy elements 118, 116, and 114 by our Heavy Element Nuclear and Radiochemistry Group in May 1999 (Ninov et al. in 1999).

In spite of his legendary accomplishments, Glenn Seaborg always had time for family members, colleagues, students, and even non-scientists who wanted to visit with him. We have lost a treasured advisor, colleague, mentor, resource, and friend. But he will live on through his prolific writings and in the cherished memories of the hosts of students, scientists, colleagues, and lay people that he influenced.

I would like to close this memorial by quoting from his statement upon being appointed the Berkeley chancellor in 1958.

There is a beauty in discovery. There is mathematics in music, a kinship of science and poetry in the description of nature, and exquisite form in a molecule. Attempts to place different disciplines in different camps are revealed as artificial in the face of the unity of knowledge. All literate men are sustained by the philosopher, the historian, the political analyst, the economist, the scientist, the poet, the artisan and the musician.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

GENERAL REFERENCES

Journals of Glenn T. Seaborg published in eight volumes spanning the years 1927-76.

R. L. Kathren, J. B. Gough, and G. T. Benefiel, eds. *The Plutonium Story: The Journals of Professor Glenn T. Seaborg, 1939-1946.* Columbus, Ohio: Battelle Press, 1994.

G. T. Seaborg, ed. *Modern Alchemy: The Selected Papers of Glenn T. Seaborg*.Singapore: World Scientific Publishing Co., 1994.

D. C. Hoffman, A. Ghiorso, and G. T. Seaborg. *The Transuranium People: The Inside Story.* Singapore: World Scientific Publishing Co., 1999.

The Seaborg Center, Northern Michigan University. Available at http://seaborg.nmu.edu.

E. O. Lawrence Berkeley National Laboratory. Available at http://seaborg.lbl.gov and http://sheiks.lbl.gov.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1938

With J. J. Livingood. Radioactive iodine isotopes. *Phys. Rev.*53: 1015.

1939

With J. W. Kennedy. Nuclear isomerism and chemical separation of isomers in tellurium. *Phys. Rev.*55: 410.

1944

Table of isotopes. *Rev. Mod. Phys.*16: 1.

1945

The chemical and radioactive properties of the heavy elements. *Chem. Eng. News*23: 2190-93.

1946

With E. M. McMillan, J. W. Kennedy, and A. C. Wahl. Radioactive element 94 from deuterons on uranium. *Phys. Rev.*69: 366 and 367.

With J. W. Kennedy, E. Segré, and A. C. Wahl. Properties of 94(239). *Phys. Rev.*70: 555.

1950

With S. G. Thompson and A. Ghiorso. Element 97. *Phys. Rev.*77: 838-39.

With S. G. Thompson, K. Street, Jr., and A. Ghiorso. Element 98. *Phys. Rev.*78: 298-99.

With A. Ghiorso, R. A. James, and L. O. Morgan. Preparation of transplutonium isotopes by neutron irradiation. *Phys. Rev.*78: 472.

1951

The transuranium elements, present status (Nobel lecture, Stockholm, December 12, 1951). Stockholm: P. A. Norstedt and Soener.

1955

With others. New elements einsteinium and fermium, atomic numbers 99 and 100. *Phys. Rev.*99: 1048-49.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

With A. Ghiorso, B. G. Harvey, G. R. Choppin, and S. Thompson. New element mendelevium, atomic number 101. *Phys. Rev.*98: 1518-19.

1958

With A. Ghiorso, T. Sikkeland, and J. R. Walton. Element no. 102. *Phys. Rev. Lett.*1: 17-18.

1963

*Man-Made Transuranium Elements.*Englewood Cliffs, N.J.: Prentice-Hall.

1964

With E. K. Hyde and I. Perlman. *The Nuclear Properties of the Heavy Elements, vol. I, Systematics of Nuclear Structure and Radioactivity; vol. II, Detailed Radioactivity Properties.*Englewood Cliffs, N.J.: Prentice-Hall.

1969

With I. I. Rabi, R. Serber, V. F. Weisskopf, and A. Pais. *Oppenheimer.*New York: E. P. Dutton.

1972

*Nuclear Milestones.*San Francisco: W. H. Freeman.

1974

With others. Element 106. *Phys. Rev. Lett.*33: 1490-93.

1978

Ed. *Elements—Products of Modern Alchemy,*Benchmark Papers in Physical Chemistry and Chemical Physics, vol. 1. Stroudsburg, Pa.: Dowden, Hutchinson & Ross.

1981

With B. S. Loeb. *Kennedy, Krushchev, and the Test Ban.*Berkeley: University of California Press.

1985

With others. Attempts to produce superheavy elements by fusion of

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

^{48}Ca with ^{248}Cm in the bombarding energy range of 4.5-5.2 MeV/μ. *Phys. Rev. Lett.* 54: 406.

1986

With J. J. Katz and L. R. Morss, eds. *The Chemistry of the Actinide Elements,* 2nd ed. New York: Chapman and Hall.

1988

With others. Atom-at-a-time radiochemical separation of the heaviest elements. *J. Radioanalyt. Nucl. Chem.* 124: 135-37.

1993

With W. D. Loveland. *The Elements Beyond Uranium.* New York: John Wiley.

Overview of the actinide and lanthanide (the *f*) elements. *Radiochim. Acta.* 61: 115-22.

1994

With R. C. Colvig. *Chancellor at Berkeley.* Berkeley: University of California Institute of Governmental Studies Press.

1995

Transuranium elements: Past, present, and future. *Acc. Chem. Res.* 28: 257-64.

Gilbert Newton Lewis—Some personal recollections of a chemical giant. *Chem. Intell.* July:9-13.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

GEORGE FREDERICK SPRAGUE

September 3, 1902-November 24, 1998

BY ARNEL R. HALLAUER

GEORGE F. SPRAGUE conducted research in the genetics and breeding of corn for nearly seventy years. His career spanned the interval from the initial studies on the potential of the inbred-hybrid concept for the development of inbred lines to produce hybrids in the 1920s to the potential of molecular genetics for the improvement of lines and hybrids in the 1990s. Throughout his seventy-year career, he always had an active research program. Sprague was a dedicated biologist who had an interest in all facets of maize genetics and breeding. He was trained in classical genetics, but he also made significant contributions to understanding the inheritance of quantitative traits; experimental methods for evaluation of lines for use in hybrids; relative efficiency of evaluating hybrids over locations and years; development and improvement of germplasm resources for the extraction of lines for use in hybrids; types of genetic effects important for the expression of heterosis; and identification of transposable elements that contributed to genetic variation.

Sprague was primarily interested in basic research, but he always emphasized that the applied aspect of research needed to be integrated with the basic research. Based on

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

his experience and the successful development of the inbred-hybrid concept for providing hybrids to producers, Sprague had great foresight in the future direction of corn research during the last half of the twentieth century. He recognized there would be a rapid transition from the prominence of publicly supported breeding programs before World War II to the rapid expansion of the commercial hybrid-seed-corn industry after World War II. He encouraged and supported the publicly supported agencies to emphasize high-risk long-term research agendas; the applied aspects of line and hybrid development would be the main focus of the commercial seed industry.

Sprague also realized that development and improvement of germplasm resources would be necessary to maintain the genetic improvement of lines and hybrids. He was a strong proponent of the recurrent selection methods developed in the 1940s for genetic improvement of maize germplasm. He developed synthetic cultivars during the 1930s and 1940s that were the basis of his selection studies. Continued selection within the synthetic cultivars was conducted and they were integrated with line and hybrid development programs. One of these synthetic cultivars, Iowa Stiff Stalk Synthetic, became an important germplasm source of inbred lines that were, and continue to be, prominent in the pedigrees of hybrids in the U.S. Corn Belt during the last fifty years. It is estimated that 40% to 45% of U.S. hybrids include germplasm whose origin traces to Iowa Stiff Stalk Synthetic.

George F. Sprague is recognized nationally and internationally for his contribution to the successful implementation of the inbred-hybrid concept for developing superior corn hybrids. In the developed areas of the world, hybrid corn is grown on nearly all of the maize-growing area and the percentage of hybrid maize has increased in the lesser

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

developed areas. Sprague's vision and research contributions have impacted all areas. He was a leader and active supporter of maize research throughout his career. He will always be recognized for his prominent role in the science of corn genetics and breeding during the twentieth century.

PERSONAL HISTORY

George Frederick Sprague was born at Crete, Nebraska, on September 3, 1902, the second youngest of seven children. His parents were Elmer Ellsworth and Lucy Manville Sprague. Both parents were graduates of Doane College at Crete, Nebraska. His father was a minister operating under the Congregational Home Missions Board.

Sprague attended grade school at Thedford and Butte, Nebraska, and graduated from Lincoln High School in Lincoln. Although the economic situation of the family was very poor, the seven children grew up with the expectation they would attend college. After he graduated from high school, Sprague enrolled in the College of Agriculture at the University of Nebraska in Lincoln for the 1920 fall semester. His choice of college was based partly on convenience but also because he was raised in rural and small-town environments. Also, in high school he became very interested in botany because of the influence of his teacher, Miss Rice.

In college, Sprague obtained employment with the Animal Husbandry Department tending experimental feed trials of swine and sheep. During the summers he worked on farms as a hired hand to earn funds to pay the fall semester tuition fees. After his sophomore year, he transferred to the Agronomy Department, where he was a laboratory assistant for both the cereals and forage crops courses. This was a very fortunate move because Sprague came under the influence of Professor F. D. Keim. Professor Keim was an unusual and

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

perceptive individual who had an impact on many of the undergraduate students at the University of Nebraska. Professor Keim took a keen interest in his students. He had an uncanny ability to evaluate students and suggest various career opportunities. Some students were steered toward SmithHughes teaching and county agent work and a few toward research; Sprague was one of the latter. At least four of Professor Keim's students were elected to the National Academy of Sciences.

Sprague's schooling was delayed one semester at the University of Nebraska because of the influenza outbreak of 1918. If not for the delay, he could have graduated mid-year. He continued taking graduate courses that would apply toward a M.S. degree. Sprague graduated from the University of Nebraska in 1924 with a B.S. degree in agriculture. Upon graduation he obtained a position as a junior agronomist with the U. S. Department of Agriculture (USDA) at the North Platte substation in Nebraska. His responsibilities were to initiate a program of cereal improvement. He continued his graduate studies at the University of Nebraska, but the materials for his M.S. thesis problem were grown at North Platte. He fulfilled the requirements for his M.S. degree in agronomy in 1926.

During the course of his M.S. studies, Sprague realized the need for additional training. Accordingly, he applied for a leave of absence from the USDA and for entrance to the Graduate School at Cornell University. Both requests were approved, and he enrolled at Cornell in the fall of 1926. Sprague considered it very fortunate that he was able to obtain Professor R. A. Emerson, a corn geneticist, as his major advisor. Soon after his arrival at Cornell, the bank at North Platte was closed, and funds that he had planned to use for his residency were no longer available. Fortunately, through Professor Emerson's influence, Sprague obtained

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

a field assistant appointment, which provided adequate funds for him to complete the two-year residency requirement. Sprague completed his preliminary examinations and returned to North Platte in the spring of 1928 as an assistant agronomist in the USDA. In 1929, he was transferred to Arlington Farms, Virginia. His Ph.D. dissertation studies were continued at North Platte and at Arlington Farms. The Ph.D. dissertation was submitted and accepted, and the Ph.D. degree in genetics was granted in 1930 from Cornell University.

Sprague was elected to the National Academy of Sciences in 1968. He was affiliated with its Section 32, Applied Biology, which became Section 62, Plant, Soil, and Microbial Sciences. He was an active member and served on several ad hoc committees. From 1972 to 1975 he served as chair of the section on applied biology. One of his major activities was with the committee on genetic vulnerability of major crops (1970-72). He had a prominent role in the preparation of the monograph *Genetic Vulnerability of Major Crop Plants,* which influenced expansion of germplasm research in the United States.

George F. Sprague died on November 24, 1998, of natural causes at his home in Eugene, Oregon. His memory was lucid, and he maintained an interest in the genetics and breeding of corn up to the time of his death. He is survived by two daughters (Phyllis Lance of Iowa City, Iowa, and Judy Sprague of Eugene, Oregon), two sons (Don of Ames, Iowa, and George of Eugene, Oregon), and two grandchildren.

PROFESSIONAL HISTORY

After the completion of his graduate studies, Sprague continued his professional career as an assistant agronomist with the USDA at Arlington Farms. He collaborated with F. D. Richey, who was the senior agronomist in charge

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

of corn investigations for the USDA, and initiated studies on the genetics and breeding of corn. This was the start of a forty-eight-year career with the USDA. His associations with prominent geneticists and breeders at Cornell University and Arlington Farms were very influential in the development of his philosophy of the importance of research that included both theoretical and applied aspects. While he was at Arlington Farms, he also selected sixteen inbred lines that were considered to have above average root and stalk strength. He inter-mated the sixteen inbred lines to form a synthetic cultivar, designated as Stiff Stalk Synthetic. Stiff Stalk Synthetic became an important cultivar in his future professional career.

Sprague was transferred to Columbia, Missouri, in 1934 to develop a cooperative USDA-Missouri University corn improvement program. He continued his genetic and breeding studies as an agronomist. He was stationed at Columbia until 1939, when he was transferred to Ames, Iowa, as an agronomist to assume leadership of the cooperative USDA-Iowa State University corn improvement program. His transfer to Ames was influenced by the prominence of the program that had been initiated there by Merle T. Jenkins in 1922. Jenkins and Sprague had a high regard for each other and had similar philosophies and ideas for developing productive research programs. Jenkins emphasized breeding research to identify methods to effectively and efficiently develop superior inbred lines for the U.S. Corn Belt. Sprague continued and strengthened the program during the years 1939-58 with the support and encouragement of Jenkins.

The twenty years that Sprague was stationed at Ames were probably the most satisfying and productive years of his long career with the USDA. He was a collaborator in the Department of Agronomy at Iowa State University and became an active participant in research, teaching, and training of

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

graduate students with majors in plant breeding. Although he was not obligated to teach while he was a USDA employee at Iowa State, he organized a graduate-level corn-breeding course, which he taught for eighteen years. He had a strong interest in the graduate training of students, and the course became a core component of students with majors in plant breeding. His excellence as a teacher was recognized with a Gamma Sigma Delta Award for teaching at Iowa State. His teaching interests, however, were not restricted to the classroom. He served as major advisor to more than thirty students who completed the requirements for M.S. and Ph.D. degrees with majors in plant breeding. He supplemented classroom instruction with informal discussions that could occur anytime during research activities in the laboratory or field. Sprague maintained an open-door policy; he was always willing to discuss plant breeding with graduate students and faculty.

Sprague developed and conducted a broad research agenda while he was stationed at Ames. It was an active and exciting time in corn research, because the inbred-hybrid concept was becoming a reality, and several of the more widely grown hybrids (e.g., U.S.13, IA939, IA13) included inbred lines developed by the publicly supported breeding programs. In 1936, only 3.1% of the U.S. corn acreage was planted with hybrid seed. The superiority of the hybrids was evident during the drought years of the 1930s, and the use of hybrid seed increased rapidly with all of the corn acreage in Iowa and Illinois planted with hybrid seed by 1950. Sprague initiated a broad range of research projects to strengthen the methods used in developing hybrids and alternative uses of corn (e.g., higher amylose and increased oil content) during World War II.

Sprague was very successful in interrelating basic studies in genetics and quantitative genetics theory with applied

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

plant breeding. He was at an ideal location to enhance his interests to apply theory to plant breeding because of his interactions and discussions with Joseph O'Mara (geneticist), Jay L. Lush (animal quantitative geneticist), and Oscar Kempthorne (statistician and quantitative genetics theorist). Each had unique talents and interests, but they formed a solid core for advancements made in animal and plant breeding at Ames.

When Sprague transferred to Ames in 1939, he brought seeds of Stiff Stalk Synthetic, a cultivar he had started intermating at Arlington Farms. Hybrid seed corn had become a reality, but Sprague believed that development and improvement of germplasm resources were necessary to ensure consistent, incremental genetic improvement over time. Based on a previous study by Merle T. Jenkins, Sprague initiated selection, based on half-sib families with U.S.13 as a tester, in Stiff Stalk Synthetic in 1939. The source population and selection methods were very effective. From this selection, came the widely known Iowa Stiff Stalk Synthetic. Two inbreds, B14 and B37, were developed from the initial cycle of the selection started in 1939 and were released to the seed industry in 1953 and 1958, respectively. B14 and B37 were used extensively to produce hybrid seed in the United States and in other temperate areas of the world. Continued selection within Iowa Stiff Stalk Synthetic eventually became the source germplasm for inbreds B73, B84, B104, and B110.

Because of the debate on the relative importance of different types of genetic effects in the expression of heterosis observed in corn hybrids, Sprague initiated, in 1949, another selection study that included Iowa Stiff Stalk Synthetic and Iowa Corn Borer Synthetic No. 1, also based on half-sib family selection. Initial response for development of inbred lines was not as rapid as for the program started in 1939. But the program was continued, and inbred lines B89, B94,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

B105, and B111 were developed from Iowa Stiff Stalk Synthetic and inbred lines B90, B91, B95, B97, B99, and B112 were developed from Iowa Corn Borer Synthetic No. 1.

The selection programs initiated by Sprague in 1939 and 1949 were based on the concepts of recurrent selection methods. Objectives of the recurrent selection methods were to genetically improve the base cultivars and to maintain genetic variation for continued future selection. Several recurrent selection programs were initiated at different locations throughout the United States during the 1950s to determine response to selection for different types of populations; types of progenies evaluated; and types of genetic effects important in selection. Sprague initiated selection in Krug High I Syn. 3 in 1953 to compare the relative effectiveness of inbred and half-sib family selection for population improvement; selection for specific combining ability in Kolkmeir and Lancaster open-pollinated cultivars was initiated in 1943 to compare the relative importance of dominance and overdominance in yield heterosis; and selection for specific combining ability was initiated in 1949 in the Alph open-pollinated cultivar and the F2 generation of the WF9 x B7 cross to determine the relative importance of dominance and overdominance effects in yield heterosis. In addition to developing improved germplasm through recurrent selection, Sprague designed each selection study to provide information on the relative importance of different types of genetic effects in selection response and yield heterosis. Most of the studies were continued until about 1980. General conclusions were that observed response to selection for increased grain yield was 2% to 6% per cycle, regardless of the population and selection methods used, and additive genetic effects with partial-to-complete dominance were of greater importance than overdominance and epistatic effects.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Sprague's research goals in corn breeding and genetics were primarily long-term and fundamental for development of more effective methods for corn improvement. His contributions to corn research were pervasive and well documented, having ranged from the genetics of scutellum color (1927) and heterofertilization (1932), effects of mutagenic agents (1936), aberrant ratios caused by virus infection (1971), and mutability in the a-ruq, Uq system (1984), to estimates of number of plants required to sample a corn cultivar (1939), relative importance of general and specific combining ability (1942), early testing of inbred lines (1946), inheritance of oil and protein (1949), estimates of rates of mutation (1955), effectiveness of recurrent selection methods (1952, 1961), and cytoplasmic-genic interaction (1983). Probably the most frequently cited paper is the one Sprague and Tatum published in 1942 in which the terms “general” and “specific” combining were introduced and defined. Combining ability of an inbred line was a phrase used to define the potency of an inbred line in hybrids. Sprague and Tatum partitioned the combining ability among diallel crosses of inbred lines into 1) average performance of a line in crosses with all other lines (general combining ability), and 2) performance of the cross of specific pairs of lines relative to their average performance (specific combining ability). The concepts of general and specific combining abilities stimulated theoretical research for the types of genetic effects important for each and a method to evaluate inbred lines in hybrids. The terms “general and specific combining abilities” are commonly used throughout all of plant breeding. Sprague also made lasting contributions to the importance of early testing for the evaluation of inbred lines in hybrids and to the effectiveness of the recurrent selection methods to develop germplasm resources. His stature in the corn research community was evident with

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

his selection to serve as editor of the corn monograph *Corn and Corn Improvement* published in 1955 (1st ed.), 1977 (2nd ed.), and 1988 (3rd ed.). This monograph became one of the best selling monographs ever published by the American Society of Agronomy.

Sprague's career changed abruptly in 1958. Merle T. Jenkins retired from the USDA, and Sprague was asked to assume leadership of corn and sorghum investigations. The change was from an active participant in an individual research project to largely one of administrator, advisor, and coordinator of the work of several research units distributed throughout the United States. He assumed the new assignment in Beltsville, Maryland, with the same intensity that he had showed in the classroom and laboratory. He wanted to expand research and attract the best possible new scientists into the corn and sorghum research programs of the USDA. He was successful in establishing new positions that emphasized basic research for the improvement of corn and sorghum. Although his primary responsibilities were as an administrator, he continued research on the genetics of corn, which he considered a release valve for the burdens of administration. He continued as investigations leader from 1958 until his retirement in 1972. At his retirement, Sprague had completed forty-eight years of employment with the USDA, starting as a junior agronomist at North Plate, Nebraska, and finishing as the leader of corn and sorghum investigations at Beltsville, Maryland.

Upon retirement, Sprague accepted an appointment as professor of plant breeding and genetics at the University of Illinois, Urbana. He resumed a more active research program and was an active participant in the corn research program. Although he did not have a formal teaching appointment, he was a willing listener and advisor of graduate students. None of the graduate students, however, was

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

able to either hand plant or hand harvest corn as quickly as Sprague. Sprague continued at the University of Illinois until 1994, and he made positive contributions until his final retirement. Although more than ninety years old, he was always looking to the future to determine what methods, techniques, and germplasm could be used for the genetic improvement of hybrids provided to the producers.

PUBLIC SERVICE

Research and administrative responsibilities were Sprague's primary interests, but he was active in his professional societies: the American Society of Agronomy, Crop Science Society of America, American Genetic Association, and the National Academy of Sciences. In addition to being an active member of several committees of these societies, he served as vice-president and president of the American Society of Agronomy (1960), president of the Crop Science Society of America (1961), and chairman of the section on applied biology (1972-75) of the National Academy of Sciences. He was a charter member of the National Plant Genetics Resources Board, formed in 1975 as an advisory committee on plant germplasm to the U. S. Secretary of Agriculture. Sprague was reappointed twice and served three consecutive terms, the maximum permitted by law. While a member of the board, he strongly advocated that the collection and preservation of germplasm must be accompanied by evaluation and enhancement to realize the full potential of the program. He also served about ten years on the Maize Crops Advisory Committee, a committee to develop and provide guidance for a national plan for corn germplasm enhancement.

FOREIGN SERVICE

Sprague actively assisted with food production in other areas of the world. He was a consultant to the Rockefeller

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Foundation for many years. He worked with E. J. Wellhausen on corn-breeding and training programs to assist corn breeders in Latin America. He served on a special mission for the development of hybrid corn-breeding programs in Europe sponsored by the Marshall Plan and assisted the USDA advisement on the hybrid corn program for Yugoslavia. In 1963, he was given the responsibility by the U. S. Agency for International Development to lead and organize an effort for increasing production of major cereal crops in Africa. A research station was established at Kitale, Kenya, to assist Kenya and surrounding countries. He was leader of the joint USAIDAR project from 1963 to 1972. Sprague believed hybrid corn could be used in developing countries, and a program was planned that included germplasm enhancement and development of adapted hybrids. Hybrids were grown in 1964 in Kenya, and with improved production practices, corn production was doubled. Sprague also was a member of the first U.S. delegation of agricultural scientists to be invited to visit China in the early 1970s. Sprague's advice and counsel were widely sought internationally, and he visited all areas of the world where corn was an important crop.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

HONORS AND AWARDS

1947	Fellow, American Society of Agronomy
1957	Gamma Sigma Delta Award, Iowa State University Crop Science Research Award, American Society of Agronomy
1958	Faculty Citation, Iowa State University Alumni Association Honorary Doctor of Science Award, University of Nebraska
1959	Distinguished Service Award, U. S. Department of Agriculture
1960	Academica Correspondente, Academia Nacionale de Agricoltura, Bologna, Italy
1963	Fellow, American Association Advancement of Science
1965	Superior Service Award, U. S. Department of Agriculture
1968	Fellow, Washington Academy of Sciences Member, National Academy of Sciences
1972	Crop Breeding Award, National Council of Commercial Plant Breeders
1978	Wolf Prize in Agriculture, Wolf Foundation, Israel
1980	DeKalb Career Award, Crop Science Society of America
1984	Fellow, Crop Science Society of America
1997	Honorary Member, American Society of Agronomy

THREE SOURCES OF information were excellent aids to summarize the long career of George F. Sprague: *Der Züchter-Genetics and Plant Breeding 37 (4):149-50 by H. F. Robinson; Maydica* 29(4):351-55 by A. R. Hallauer; and *Plant Breeding Reviews* 2:1-11 by W. A. Russell. Personally, I first met George F. Sprague in 1956 when he accepted me as a graduate student in corn breeding. He was my advisor for my M.S. degree and was my supervisor as a USDA employee from 1958 to 1972. His years as my mentor were instructive, positive, and enlightening, and provided me a unique insight to his thinking for my research in corn breeding and genetics.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1927

Heritable characters of maize XXVII colored scutellum. *J. Hered.*XVIII: 41-44 .

1929

Heterofertilization in maize. *Science*LXIX: 526-27 .

1931

With F. D. Richey. Experiments on hybrid vigor and convergent improvement in corn. USDA Technical Bulletin 267.

1932

The inheritance of colored scutellum in maize. USDA Technical Bulletin 292.

1934

Experiments on iarovizing corn. *J. Agr. Res.*48: 1113-20 .

1936

Hybrid vigor and growth rates in a maize cross and its reciprocal. *J. Agr. Res.*53: 819-30 .

1939

An estimation of the number of top-crossed plants required for adequate representation of a corn variety. *J. Am. Soc. Agron.*31: 11-16 .

1942

With L. A. Tatum. General vs. specific combining ability in single crosses of corn. *J. Am. Soc. Agron.*34: 923-32 .

1943

With B. Brimhall and R. M. Hixon. Some effects of the waxy gene in corn on properties of the endosperm starch. *J. Am. Soc. Agron.*35: 817-22 .

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1945

With M. L. Kinman. Relation between number of parental lines and theoretical performance of synthetic varieties of corn. *J. Am. Soc. Agron.*37: 341-51 .

1946

Early testing of inbred lines of corn. *J. Am. Soc. Agron.*38: 108-17 .

The experimental basis for hybrid maize. *Biol. Rev.* 21: 101-20 .

1947

With W. T. Federer. A comparison of variance components in corn yield trials. 1. Error, tester x line and line components in top cross experiments. *J. Am. Soc. Agron.*39: 453-63 .

1949

With B. Brimhall. Quantitative inheritance of oil in the corn kernel. *Agron. J.*41: 30-33 .

1950

With B. Brimhall. Relative effectiveness of two systems of selection for oil content of the corn kernel. *Agron. J.*42: 83-88 .

1951

With W. T. Federer. A comparison of variance components in corn yield trials. II. Error, year x variety, location x variety and variety components. *Agron. J.*43: 535-41 .

1952

Early testing and recurrent selection. In *Heterosis,* ed. J. W. Gowen, pp. 400-417 . Ames, Iowa: Iowa State College Press.

1955

With J. F. Schuler. Natural mutations in inbred lines of maize and their heterotic effect. II. Comparison of mother line vs. mutant when outcrossed to unrelated inbreds. *Genetics*41: 281-91 .

Corn breeding. In *Corn and Corn Improvement,* ed. G. F. Sprague, pp. 221-92 . New York: Academic Press.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1956

With W. A. Russell. Some evidence on type of gene action involved in yield heterosis in maize. In *Proceedings of the International Genetics Symposia,* pp. 522-26 . Tokyo: *Cylologia*.

1971

With H. H. McKinney. Further evidence of the behavior of AR in maize. *Genetics*67: 533-43 .

1974

Plant breeding, molecular genetics and biology. NAS-NRS Conference on Genetic Improvement of Seed Proteins.Washington, D.C.: National Academy of Sciences.

1976

Plant breeding, molecular genetics and biology. In *Genetic Improvement in Seed Proteins*. Washington, D.C.: National Academy of Sciences/ National Research Council.

1980

With D. E. Alexander and J. W. Dudley. Plant breeding and genetic engineering: A perspective. *BioScience*30: 17-21 .

Germplasm resources of plants: Their preservation and use. *Annu. Rev. Phytopathol.*18: 147-65.

1983

Heterosis in maize: theory and practice. In *Heterosis: Monographs on Theoretical and Applied Genetics,* ed. R. Frankel, Chapter 2. Berlin: Springer-Verlag.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ROBERT JULIUS TRUMPLER

October 2, 1886–September 10, 1956

BY HAROLD F. WEAVER

ROBERT JULIUS TRUMPLER was born on October 2, 1886, in Zurich, Switzerland. He was the third child in a family of ten children. The Trumpler family, which traced its genealogy back to 1384, was well established and was active in business and manufacturing. A structured family life was regulated by a strict businessman father and was softened by a kind and loving mother. From a very early age the children were encouraged to be industrious, and they engaged in handicrafts that ranged from making puppets to embroidery. School was important and church was a significant activity. All these family characteristics were apparent throughout Trumpler's life.

CHILDHOOD AND EDUCATION

In a quite remarkable autobiographical sketch and self-analysis written before he finished the Gymnasium at age nineteen, Trumpler recounted some of his childhood memories and the succession of interests that occupied him up to that time. The document itself is an indicator of his character. The handwritten manuscript resembles a copper plate engraving. He was a perfectionist.

Growing up in a very large family (and having many cousins and relatives) made friends of secondary importance. Though

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

he spoke of getting to know his fellow students at school, Trumpler had few close friends in his early years. He described himself as reserved, and often found it difficult to participate in a group conversation. He did well in school, but his classes seem not to have interested him, particularly in the earlier grades, since he had already learned to read and write at home. From summer excursions in the Swiss Alps with his father and two older brothers Trumpler gained a great love of nature, particularly the high mountains.

An important part of growing up in the Trumpler family was to decide what profession to enter after finishing school. The problem of deciding on a profession flows throughout Trumpler's account. As a quite small child he liked to do errands for the family and to learn about Zurich. He enjoyed getting to know the stores and the businesses of the city. He decided he would become a businessman. During an outbreak of smallpox in Zurich when school was dismissed, he substituted for an assistant in his father's office in order to learn more about his future profession, which seemed settled from early childhood. But as he grew older his interests began to change.

After the first years in the Gymnasium Trumpler began to formulate problems for himself—to educate himself, as he described it. He read extensively and explored art and literature, especially poetry. Eventually, he felt that he had too little imagination ever to write or to become a poet or an artist. However, photography gave him an artistic outlet that he enjoyed all his life. He developed a major and continuing interest in science and the scientific method. He could work in science by himself and the careful systematic development of data and evidence was much to his liking. Early on, he studied astronomy, but found that his knowledge of physics and mathematics was insufficient to get him deeply into the subject, and he did not have a telescope.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

He became interested in biology and zoology. He described at length his intense interest in the dissection of a pigeon and his investigation of the internal organs and the detailed structure of the skeleton. Classes, he felt, interrupted these more interesting experiences.

At age seventeen Trumpler was confirmed in the church. He had gone to Sunday school regularly and had religious instruction at school. For a while after confirmation he seemed satisfied with what he had been taught, but he began to have doubts. He listened carefully to discussions, he said, and made his own observations of the world around him. He observed contradictions between what religion taught and what he observed. He believed what he himself observed; he became a skeptic. After much thought, he formulated three major questions that he would try to answer for himself:

Does God exist?

Does man have an immortal soul?

Does man have free will?

He finally equated God with the totality of physical laws that govern the universe. To the second question he could find no fully satisfactory answer. To the third question his answer was, he said, uncertain, but he was inclined to deny the existence of free will. He did, however, accept the moral teachings of Christianity as the rules to live by.

Trumpler's growing interest and joy in science made him question his early decision to go into business. He began to think about a career in science. He finally decided he was totally unsuited for business. He loved seclusion and thought, he wrote, not the constant contact with people that business required. He might become a science teacher in the Gymnasium, but again, his retiring nature would make it

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

difficult or impossible to be a successful teacher. He tentatively decided that he would become a doctor. That would permit him to be involved in some science and would be a useful occupation. His parents argued against the plan. There were enough doctors, they said, and it would be difficult to find a practice. In the end, Trumpler gave up the idea. His paternal grandparents argued strongly that he should go into business and have science as a hobby. Trumpler respected his grandparents greatly and accepted their advice. On the last night of 1905 and in the first hour of 1906, Trumpler announced his decision to his parents. They were pleased. His father proposed that he should spend a year as an apprentice in a bank and that he then should study jurisprudence so that he could become a bank director. Trumpler noted that he hated jurisprudence, but he could console himself with some science.

Trumpler graduated from the Gymnasium first in his class and became an apprentice at a bank in Zurich. Within a year the mismatch between banking and his interests became unbearable. With parental approval he left the bank and in 1906 entered the University of Zurich to study astronomy, physics, and mathematics. He had found his life interest. He began to participate in student activities at the university. He joined the Academic Alpine Club of Zurich and with friends from the club climbed many of the highest peaks of the Swiss Alps. During a week-long trip on skis through the glacier region of the Berner Oberland, Trumpler and friends from the Alpine Club made one of the first winter attempts to climb some of the high peaks.

In 1908 Trumpler transferred to Gottingen, where he studied with some of the leading scientists of the time—Klein, Hilbert, Voigt, Schwarzschild—and completed a Ph.D. degree magna cum laude in 1910 under the direction of Professor J. Ambronn. His thesis involved experiments in

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the photographic recording of the meridian transits of stars. He remained at Gottingen as an assistant until he joined the Swiss Geodetic Commission in 1911.

At a meeting of the Astronomische Gesellschaft in Hamburg in 1913 Trumpler took the opportunity to meet many of the leading American astronomers. With Frank Schlesinger he discussed a plan he had developed to determine the proper motions of the Pleiades. He had become interested in that cluster when he observed it in the course of his thesis work at Gottingen. Schlesinger was interested in the plan and thought it was feasible.

War interrupted Trumpler's work at the Geodetic Commission. In 1914 he was called up for military duty as a first lieutenant in the Swiss Army. In 1915 Schlesinger invited Trumpler to become an assistant at the Allegheny Observatory. Fortunately, Trumpler received a leave of absence from the army with permission to leave Switzerland to accept the position at Allegheny. He arrived in the United States in May 1915. In the summer of 1916 he returned to Switzerland for his marriage to Augusta de la Harpe. Together with his bride, Trumpler returned to the United States, crossing the Atlantic in a military convoy.

Trumpler was invited to the Lick Observatory as a Martin Kellogg fellow in 1919 and was appointed assistant astronomer in 1920. With a position in a major American observatory, Trumpler decided that his future was in the United States. He became a naturalized citizen in 1921.

SCIENTIFIC WORK

The bulk of Trumpler's scientific work falls into two categories: (1) positional astronomy, or (2) the study of star clusters and the Milky Way. His earliest work was all in positional astronomy, his thesis field. His first paper, published in 1910, related to the determination of the latitude

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

of Gottingen; his work with the Swiss Geodetic Survey consisted of the accurate determination of longitudes of the Swiss observatories. At the Allegheny Observatory Trumpler published the parallaxes of 23 stars and the proper motion of Nova Aquila.

He was also very actively at work on topics that fell in the second category. In 1915 he published a paper on the relative motions of the Pleiades and in 1920 a second one on the constitution of that cluster. Perhaps his most significant work at Allegheny, a forerunner of things to come, was a study of the classification of star clusters.

Trumpler's approach to the study of a phenomenon or class of objects was to gather all available data (including his own observations) and compile a very detailed catalog that he could then use to show the relationships of various features of the objects or phenomenon. His creation of the extensive catalog of star clusters, on which he spent many years, was an outgrowth of the early work at Allegheny. It provided the basis for the most important papers he published during his years at Mt. Hamilton.

At Lick, Director W. W. Campbell recognized Trumpler as an exceedingly accurate and skillful observer and chose him as his collaborator for the 1922 Lick-Crocker Eclipse Expedition to Wallal, Australia. The expedition's principal objective was to measure the deflection of light at the limb of the Sun in order to test Einstein's theory of the deflection of light in a gravitational field. Eddington had measured the deflection at the 1919 eclipse as 1.61 +/− 0.3 seconds of arc, a value determined from five stars measured on each of two plates. Campbell wanted a much stronger test of the effect.

Trumpler approached the project in his usual thorough and detailed way. The two eclipse cameras (of focal lengths 5 feet and 15 feet) were set up on Mt. Hamilton in the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

form they would be used at the eclipse and were thoroughly tested on the stars. Their field errors were carefully examined. A method of differential measurement of stellar images was devised and the apparatus necessary for its use was constructed. The literature on the derivation of the deflection of light at the limb of the Sun was researched and some errors in the earliest discussions were corrected. Four months before the eclipse the cameras were set up in Tahiti just as they were to be set up and used in Australia. Photographs of the eclipse field of stars were taken with telescope pointings as nearly as possible identical to those that would exist at the time of the eclipse. These formed the standards against which the stars photographed during the eclipse would be measured. The eclipse expedition was a complete success. Ten plates were obtained. Depending on the camera, approximately 70 or 140 stars were measured on each plate. The final result determined for the deflection of light at the limb of the Sun was 1.75 +/− 0.09 seconds of arc. This was taken as strong confirmation of the Einstein theory, which predicted 1.75 seconds of arc.

In 1925 Trumpler used the catalog of star clusters he was developing to write a paper entitled “Spectral Types in Open Clusters.” It presented data from the HR diagrams for 52 clusters. Trumpler found that there were two types of clusters. Type 1 contained no giant stars and had a main sequence in which the spectral types might extend from spectral type O or B down the main sequence as far as the observations went. Type 2 contained red giant stars and had no stars earlier than A or F on the main sequence. No cluster was found that contained both O or early B stars and red giants. Nowadays, the explanation of the two cluster types is immediately evident: they are the result of stellar evolution. In 1925 these observations were challenging. Trumpler surmised that the difference of the cluster types

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

was caused by a difference in the mass distribution among the stars when the cluster was formed, but it was not possible for him to reach a satisfactory solution to the puzzle with the then current idea that small-mass stars evolved more rapidly than large-mass stars.

In 1930 Trumpler published "Preliminary Results on Distances, Dimensions, and Distribution of Open Star Clusters." It was an extraordinary paper and represented an immense amount of labor. HR diagrams determined for 100 clusters were used to infer distances that were then used to derive linear diameters for the clusters in the sample. The diameters covered a wide range from 2.3 to 21 parsecs. Next the clusters were classified according to central concentration, range of brightness of stars, and richness of the cluster, and their linear diameters were re-discussed. The expectation was that clusters of the same classification would have the same linear diameter. They did not; distant clusters of any one type seemed to have diameters larger than nearby clusters of the same type. Exhaustive analysis of the data for possible causes of this discrepancy left Trumpler with only two possibilities: clusters did increase in size with distance (a situation that is not physically reasonable) or the distances determined from the HR diagrams were wrong because of absorption of light in the Milky Way. Trumpler showed that on average the absorption is 0.67 (photographic) magnitudes per kiloparsec and that the absorption is selective, since distant stars appeared redder than nearby stars of the same spectral type. Finally, he showed that the absorbing material is concentrated primarily in a thin layer in the galactic plane.

Trumpler then went on to present his catalog of 334 clusters for which he computed distances from their diameters. These 334 objects were used to determine the space distribution of the clusters and to determine the plane of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

the galaxy. But, from that point on the broader conclusions about the galactic system reached from these data went badly astray. Though Trumpler had discovered interstellar absorption, he did not at the beginning clearly perceive its overpowering influence on observations of distant objects. In 1930 ideas about the size and nature of the galaxy, especially its size, were at a very early stage of development. At that time Trumpler believed that the 334 open clusters he was investigating defined the galaxy, the Milky Way system. According to his analysis, the system was at most 10,000 parsecs in diameter; the Sun was roughly near its center. Earlier investigators had proposed very different models of the galaxy. In 1918 Shapley first delineated the quasi-spherical system of globular clusters and identified the center of the globular cluster system with the center of the galaxy at a distance of 16,000 parsecs from the Sun. In 1927 Lindblad and in 1928 Oort explained the systematics of stellar motions as arising from rotation of the galaxy around a center located at a distance of 10,000 parsecs in the direction of the center of the system of globular clusters. It was a time of great confusion.

One decade later, in a paper presented at the dedication of the McDonald Observatory and published in 1940 with the title “Galactic Star Clusters,” Trumpler showed how star clusters could be used in the solution of a variety of galactic problems. In one section of the paper, he analyzed the visibility of galactic star clusters of representative types located in the plane of the galaxy at different distances from the Sun. Interstellar absorption was assumed to be present. He demonstrated that at distances of 5,000 parsecs from the Sun even the brightest and most favorable galactic clusters would have been missed in all the observational surveys that had been made up to that time, and fainter clusters would be undetectable at distances of 2,000 parsecs or less.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Trumpler then acknowledged that all 334 clusters he had studied in the 1930 paper were located within a few kiloparsecs from the Sun; they did not outline the galaxy. One may note from the drawing in Trumpler's paper that, by 1940, the accepted distance to the center of the galaxy was settling down at 10,000 parsecs and its location was in the direction to the center of the system of globular clusters.

In a 1935 paper entitled “Observational Evidence of a Relativity Red Shift in Class O Stars” Trumpler analyzed seven clusters that contained O-type stars for systematic differences in radial velocity between their O stars and stars of later types. The O stars all showed positive residual velocities. The average red shift (absolute value) was 10.1 km/s. As a confirming test of such a red shift, Trumpler determined the solar motion from O-type stars. Any red shift present should show as a K-term. The K-term found from 69 stars was +6 km/s. The measured red shifts in the clusters were used to infer the masses of the O stars. Trumpler determined distances to the clusters from their HR diagrams and used measured magnitudes of the O stars along with standard bolometric corrections and temperature scales to compute their radii. He calculated individual O star masses that ranged from 75 to 340 times the mass of the Sun. These results for the “Trumpler Stars,” as these O stars were called in the literature, were and still are controversial. Stellar masses that are 100 or more times the mass of the Sun are incompatible with modern ideas of stellar formation and instability. That there is a gravitational red shift present in the O stars is certain, but the shift measured by Trumpler is excessive by a factor of at least three and possibly more. The source of the large values and the inferred masses is unknown and presents a continuing problem in need of a solution. A variety of data, not only from the clusters but from other sources as well, indicate that an

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

additional as yet unknown effect is very likely present in the clusters or the O stars.

As early as 1924 Trumpler started to measure radial velocities for a selection of open clusters. Originally, it seems that these were simply to supply statistical information for his extensive catalog of clusters. As time went on, it became apparent that radial velocities were useful for the solution of many problems and the program became the observational focus of Trumpler's scientific career. Radial velocities can, for example, provide a means of separating cluster members from non-members as in Trumpler's 1938 investigation of the star cluster in Coma Berenices. This study is an excellent example of the huge effort required to produce the information Trumpler would have liked for each cluster: its position, distance, proper motion, and radial velocity, along with a complete list of cluster members and a full set of observable properties for each member. These data, in turn, could be used to provide a picture of the fundamental physical properties of the cluster: its motion in space, its linear size, the space and velocity distributions of the cluster members as a function of mass or luminosity, etc. Trumpler would have liked to establish all these functions for many nearby clusters.

During the period from 1940 to his retirement in 1951, Trumpler was fully occupied by teaching and working on his extensive program of radial velocities. He did continue to observe spectra with the 36-inch telescope and he gave various public lectures and participated in a few symposia, but he produced no important papers on clusters; it was a period of data gathering. Unfortunately, even though he continued to work on the radial velocity program after retirement, he did not live to complete the task he had set for himself. It was an overwhelming project for one individual, and would have been a very large task even for a

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

group. His plan to use the information in a major study of galactic rotation was never realized. Some data from the radial velocity program in manuscript form have been supplied to individual investigators; it is hoped that all the data can be made generally available.

Trumpler undertook two projects at the Lick Observatory well afield of his major work on clusters. One, at the request of the Solar Parallax Commission of the International Astronomical Union, involved observations of Eros at the opposition of 1931 as part of the international campaign to measure the solar parallax. The other was a program of the Lick Observatory to observe Mars at the opposition of 1924.

There were two phases to the plan for Mars: (1) a color survey of the planet made photographically with filters and matched photographic emulsions at the Crossley reflector and (2) combined photographic and visual observations made with the 36-inch refractor in the relatively small wavelength range (yellow and red) in which it could be used effectively. W. H. Wright made the color survey and Trumpler carried out the photographic-visual survey. He made about 1,700 photographs of Mars directly at the focus of the 36-inch refractor during the 1924 opposition. He analyzed some 150 of these taken at moments of best seeing. They provided determinations of the diameter and polar flattening of Mars, the heights of the visible atmosphere in yellow and red light, and the position of the planet's north pole. Trumpler then went on to determine the areographic longitudes and latitudes of 228 markings on the planet. Each marking was measured on from 3 to 17 photographs. From these positions a map of Mars was drawn and analyzed. In his discussion of the map Trumpler always described the so-called canals as a network and pointed out that though they were generally drawn as uniform, they were, in fact,

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

quite irregular. He concluded that they were natural features in the topography of Mars. The bright areas that appeared for short periods he suggested were snow or frost. When he compared his maps of the dark areas with those of earlier observers there seemed to be changes in extent in latitude of some features. He thought that, if this were the case, the most likely explanation was that the dark areas represented vegetation that varied in coverage from year to year.

When Trumpler's chart is compared with a modern map made from *Viking* photographs, there is a remarkable similarity, particularly in the delineation of dark areas. One can readily see that the network, as Trumpler called it, is made up of edges of craters, areas between adjacent craters, and small spots that happen to be approximately in line, all natural features in the landscape.

LIFE ON MT. HAMILTON AND AT BERKELEY

As he correctly described himself in his early autobiographical sketch, Trumpler was slightly reserved, but he was broadly intellectual, a person with many interests. Augusta Trumpler shared his many intellectual interests, but was much more outgoing. While at Pittsburgh at the Allegheny Observatory, they found and joined the Unitarian Church, which fitted well with the philosophical views developed by Trumpler when he was a young student. Activities of the Unitarian Church formed an important part of their lives.

The Trumplers' first daughter was born while they were in Pittsburgh. Coming to the isolation of Mt. Hamilton with a small child must have been a daunting experience. For the first year they had rooms in the dormitory and took their meals at the boarding house. Eventually, they did move into an observatory house, the first of many different ones

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

they occupied until one was built specifically for them in 1928.

Today the isolation of Mt. Hamilton during the first quarter of the century is hard to imagine. It was a small community of 40 to 50 people—the families of the five or six astronomers plus those of the staff that maintained the instruments and houses plus the observatory secretary, graduate students, assistants, and the school teacher who taught in the one-room Mt. Hamilton school. In the 1920s cars were not the commonplace items they are today. For many residents of Mt. Hamilton, the Trumplers among them, communication with the outside world was by the stage, which six days each week made the 25-mile trip between San Jose and the observatory, bringing the mail as well as food and supplies and carrying passengers. A trip to San Jose was an event that required careful planning and at least an overnight stay.

The family of an astronomer on Mt. Hamilton had an unusual life. Each astronomer normally worked one, two, or more nights a week at the telescope and then slept during daylight hours. Children had to learn to play quiet games; dogs were not allowed on the mountain. Social events were infrequent. Occasionally there were movies in the schoolhouse. The families took turns renting the films. Saturday was Visitors' Night, a special occasion when the children could “go up top” to see all the visitors who came from San Jose and the Bay Area to look through the 36-inch telescope. There were some sports on the mountain. There was a tennis court and hiking was popular. A dam on Isabel Creek some 5 miles from the observatory provided swimming on hot summer days. In summer there were graduate students from Berkeley and sometimes visiting astronomers. All would join the residents of the mountain at the post office at noon when the stage arrived from San Jose. The

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

astronomer who served as postmaster would deliver the mail, and the custodian would set out for each family the supplies just brought up.

Trumpler became interested in gardening and undertook the development of a garden at a natural spring on the east side of Mt. Hamilton about a mile from the road. During World War I the site had been used by some of the astronomers to grow vegetables, but it was later abandoned. The development undertaken by the Trumplers and their five children was a weekend activity that lasted for many years. After reclaiming the older space and enlarging it, they rebuilt a high fence around the garden to keep out the deer. They built a small cabin and picnic area next to the garden; the lumber for the structure was carried to the site piece by piece. There was a small swimming pool and play area (called Monkey's Paradise) for the children. With picks and shovels the family dug out and made a road that finally allowed them to drive to the garden. In this extensive work they were often helped by summer graduate students from Berkeley, who enjoyed the exercise as well as the hospitality of the Trumpler family. Though the cabin and the area are again falling into decay, the residents of the mountain still speak of Trumpler's Garden.

An arrangement between the Lick Observatory and the Berkeley Department of Astronomy made it possible for periodic exchanges of personnel to occur. An astronomer from Lick would spend a semester teaching at Berkeley; a professor from Berkeley would spend a semester doing research at Lick. The Trumplers got to know several families from Berkeley and found that they shared many interests. Trumpler exchanged with Berkeley in 1924 and 1930. The family enjoyed being in a university town and Trumpler found that he enjoyed teaching, and was successful at it in spite of his much earlier doubts.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

With five children, the Trumplers were facing an important problem. The one-room school on Mt. Hamilton went through only the eighth grade. One by one the children would have to leave the mountain to continue their education. In 1935 when two children were living away from home, Trumpler arranged to continue as a Lick astronomer but with residence in Berkeley, where the family would again be united. Trumpler commuted to Lick to use the 36-inch telescope during the school year and the family returned to their house on Mt. Hamilton during the summer.

At Berkeley, Trumpler had an office in the Department of Astronomy where he carried on his research and writing. Occasionally he would give some specialized classes in galactic structure. He found that he enjoyed teaching and working with the graduate students. In 1938 he transferred permanently to the Berkeley campus as professor of astronomy, but he retained the house at Mt. Hamilton where the family spent the summers and Trumpler observed with the 36-inch telescope.

Trumpler was a very successful teacher. Periodically, he gave the introductory course for non-majors, as did all the faculty members. He expanded and modernized the upper division course in practical astronomy, a field in which he had worked throughout his career: setting up and testing instruments, measuring photographic plates, etc. He also developed a graduate course in statistical astronomy (galactic structure), which all the graduate students took. It was Trumpler's specialty and was very popular with the students. Many chose him as their thesis adviser.

In the summer of 1939 Trumpler had an accident at the 36-inch telescope that had an important unforeseen consequence. Trumpler was alone at the telescope and for the first observation of the night needed to reverse the instrument from one side of the pier to the other. This operation

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

is performed slowly from a platform near the top of the mount by locking the telescope and turning two wheels that slowly move it to the desired location. The operation can also be performed quickly from the floor by holding the telescope at the eye end and swinging it around by hand. The instrument is very heavy and, once moving, has a great deal of momentum. At the critical moment when the instrument is just moving over the pier around the polar axis, the operator must move the telescope around its second axis or else the eye end of the telescope will move directly into the floor, potentially damaging the telescope and the equipment mounted on it. Trumpler missed the moment to swing the telescope around the second axis and it started to go into the floor. He was partially under the telescope desperately trying to get the clamp to stop the motion when the telescope struck him on the knee, driving his heel into the floor. His heel was crushed. The telescope was undamaged, but the experience was painful for Trumpler, who spent a long period of time with his leg in a full cast.

During recuperation, when Trumpler was learning to walk normally again, the doctor advised him to do a good deal of walking in sand. The Trumplers started spending time at Santa Cruz and Rio del Mar, where they would walk on the beach. On one occasion, when they were returning their house key to the rental agent, they learned that a house on the cliff overlooking the beach at Rio del Mar had just been put up for sale at a very favorable price because the owner was fearful that the Japanese were about to attack. (They had attacked Pearl Harbor a few days earlier.) The Trumplers looked at the house and bought it on the spot. It became a home that they very much enjoyed and to which they retired.

Trumpler often spoke about writing a book based on his course in statistical astronomy. In 1951, the year he retired,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

he and a former student who was then on the staff of the Lick Observatory completed the book. *Statistical Astronomy* was published by the University of California Press in 1953 and was reprinted by Dover in 1962. It was Trumpler's last publication.

The Trumplers retired to Rio del Mar where he continued to work on his radial velocity data and to develop the large garden he so much enjoyed. They were founding members of the Unitarian Universalist Society in which they were very active. Trumpler's health began to fail rapidly after being diagnosed with leukemia. He died unexpectedly on September 10, 1956, after a few days in the hospital.

Trumpler was a member of the International Astronomical Union and many astronomical societies. He had been a councilor of the American Astronomical Society and twice was president of the Astronomical Society of the Pacific (1932 and 1949). He was elected to the National Academy of Sciences in 1932 and was a fellow of the American Academy of Arts and Sciences. To honor him as an outstanding teacher who guided many students through their theses, the Astronomical Society of the Pacific established the Trumpler Prize for the most outstanding Ph.D. thesis of the year; it has been given annually since 1974.

I AM GRATEFUL TO Dorothy Schaumberg, librarian at the Mary Lea Shane Archives at Lick Observatory, and to Jenny Mun at the National Academy of Sciences for information on several dates. I thank Alar Toomre for helpful suggestions relating to early drafts of this biographical memoir.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1915

The relative proper motions of the Pleiades. *Astron. Nachr.*200: 217-30 .

1920

A study of the Pleiades cluster. *Publ. Astron. Soc. Pac.*32: 43-49 .

1921

Physical members of the Pleiades group. *Lick Obs. Bull.*10: 110-19 .

1922

Comparison and classification of star clusters. *Publ. Allegheny Obs.*6: 45-74.

1923

With W. W. Campbell.Observations of the deflection of light in passing through the Sun's gravitational field during the total solar eclipse of September 21, 1922. *Publ. Astron. Soc. Pac.*35: 158-63 .

Historical note on the problem of light deflection in the Sun's gravitational field. *Publ. Astron. Soc. Pac.*35: 185-88 .

1924

A method for differential measurement of stellar photographs. *Publ. Astron. Soc. Pac.*36: 9-14 .

Preliminary results on the Einstein test from observations with the five-foot camera. *Publ. Astron. Soc. Pac.*36: 221-34 .

Visual and photographic observations of Mars. *Publ. Astron. Soc. Pac.*36: 263-69 .

1925

Spectral types in open clusters. *Publ. Astron. Soc. Pac.*37: 307-18 .

The cluster M11. *Lick Obs. Bull.*12: 10-16 .

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1927

Visual and photographic observations of Mars made at the opposition of 1926. *Publ. Astron. Soc. Pac.*39: 103-11 .

Observations of Mars at the opposition of 1924. *Lick Obs. Bull.*13: 19-45 .

1928

Final results on the light deflection in the Sun's gravitational field from observations made at the total eclipse of September 21, 1922. *Publ. Astron. Soc. Pac.*40: 130-34 .

1929

The relativity deflection of light. *Publ. Astron. Soc. Pac.*41: 23-34 .

1930

Preliminary results on distances, dimensions, and distribution of open star clusters. *Lick Obs. Bull.*14: 154-88 .

Absorption of light in the galactic system. *Publ. Astron. Soc. Pac.*42: 214-27 .

Spectrophotometric measures of interstellar absorption. *Publ. Astron. Soc. Pac.*42: 267-74 .

1932

The deflection of light in the Sun's gravitational field. *Publ. Astron. Soc. Pac.*44: 167-73 . *Science*75: 538-40 . *Z. Astrophys.*4: 208-20 .

1935

The constitution of the star cluster in Coma Berenices. *Publ. Astron. Soc. Pac.*47: 219 .

Observational evidence of a relativity red shift in class O stars. *Publ. Astron. Soc. Pac.*47: 244-56 .

1937

With F.J. Neubauer, P. H. Hutchings, C. E. Smith, and K. P. Kaster. Observations of Eros made at the Lick Observatory during the opposition of 1931 with a preliminary determination of the solar parallax. *Lick Obs. Bull.*18: 93-107 .

1938

The star cluster in Coma Berenices. *Lick Obs. Bull.*18: 167-95 .

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

1940

Galactic star clusters. *Astrophys. J.* 91: 186-201 .

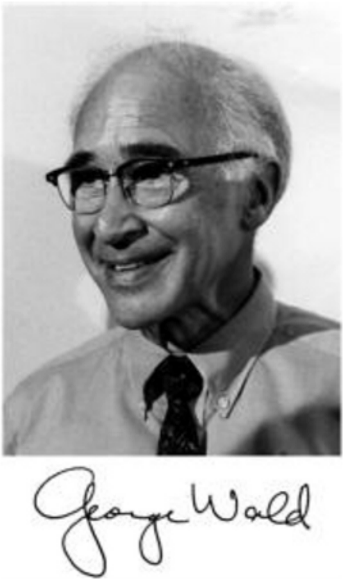
1953

With H. F. Weaver. *Statistical Astronomy.* Berkeley and Los Angeles: University of California Press.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html



About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

GEORGE WALD

November 18, 1906–April 12, 1997

BY JOHN E. DOWLING

BIOLOGY LOST ONE of its towering figures of the twentieth century with the passing of George Wald. A student of Selig Hecht, the major researcher in visual physiology of his generation, Wald unraveled the nature of the light-sensing molecules found in photoreceptor cells and was the dominant force in his field for over forty years. Beginning with postdoctoral research in the early 1930s, Wald showed that the visual pigment molecules consist of a protein (termed opsin) to which is bound a derivative of vitamin A (vitamin A aldehyde, now termed retinal). Retinal serves as chromophore for these molecules, absorbing the light and initiating conformational changes in the protein that lead eventually to the excitation of the photoreceptor cells. Wald's findings represented the first instance that a biochemical role for a fat-soluble vitamin was established and were widely recognized. Wald was elected to the National Academy of Sciences in 1950 and was awarded the Nobel Prize in physiology or medicine in 1967 for his monumental contributions to our understanding of the molecular basis of photo-reception.

In addition to being a superb scientist, Wald was a marvelous teacher, lecturer, and writer. *Time* magazine named

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

him "one of the ten best teachers in the country" in a cover story published in 1966. He wrote and lectured on a wide variety of topics from the "Origin of Life" and "Life and Mind in the Universe" to political issues. The Vietnam War horrified him and, beginning in the mid-1960s until shortly before his death, he was deeply involved in anti-war and anti-nuclear activities. He considered his political actions as part of being a biologist: one who is concerned with life.

George Wald was born in New York City on November 18, 1906. The son of immigrant parents, he grew up in Brooklyn in a working-class neighborhood. His mother was from Germany, his father from Poland. He showed an aptitude for mechanical things and science from his youngest days. An early triumph was the successful construction of a crystal detector radio that enabled him and his neighborhood friends to listen to the 1919 World Series.

George went to Manual Training High School, now the Brooklyn Technical High School, which trained students to use their hands and to build things. He later felt this training was especially useful for his scientific career, as it enabled him to design and even to help build a variety of specialized equipment. Two interests stand out from his high school days: electricity and vaudeville. For a while he thought of electrical engineering as a career but a visit to Western Electric in New Jersey soured him on that path. With a high school friend, he organized a vaudeville act that they took to nearby Jewish community centers. His success as a performer suggested law as a possible career and so he entered college as a pre-law student at Washington Square College of New York University.

College was especially exciting for George; it introduced him to art, classical music, and literature and he became enamored of them all. In later years, he gathered a first-class collection of Rembrandt etchings and much indigenous

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

art from Africa and Central and South America. His college years were also broadening in another way: for two summers he worked onboard a passenger ship that traveled between New York and Buenos Aires. His pre-law studies, however, did not interest him. He felt he needed something "more substantial, more natural, more organic," and so he became a pre-medical student. By the time George was a college senior, medicine as a career also had lost its luster, but he happened upon Sinclair Lewis's *Arrowsmith* and was smitten by the possibility of doing biological research. He applied to Columbia University for graduate studies in zoology and was accepted.

The first year as a graduate student was again a watershed year for George. He took a genetics course with T. H. Morgan and met Selig Hecht, his future mentor. Hecht was well known for his studies on photosensory systems of both simple organisms, such as the worm *Ciona* and the clam *Mya,* as well as man. His quantitative measurements of the effects of light and dark on various organisms demonstrated that visual mechanisms conform to photochemical laws. Hecht (1919) introduced the notion that in photosensory systems, a photosensitive substance S is decomposed by light into products P and A (light adaptation) and that in the dark, P and A combine to reform S (dark adaptation). Wald and his co-workers would eventually set many of Hecht's concepts into precise molecular terms.

Not only did Hecht introduce Wald to vision but he had a profound influence on him. Upon receiving the Proctor Medal from the Association for Research in Ophthalmology in 1955, eight years after Hecht had died, Wald remarked,

Hecht was a great teacher and physiologist. Also, he was one of those rare persons who sets a standard both at work and at leisure. I was fortunate in

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

having his instruction and later his friendship. I saw too little of him after leaving his laboratory, but I felt his presence always. What I did or said or wrote was in a sense always addressed to him.

As a graduate student, Wald worked on the visual performance of *Drosophila.* He found that in many ways fruitfly visual function resembles that of other animals, including man. With Hecht, he also investigated human dark adaptation. Although Hecht's research was concerned with the essential features of the photoreceptor process, he was not especially interested in the underlying molecules themselves, only their physicochemical relationships. Wald, on the other hand, very much wanted to lay his hands on these substances and for postdoctoral work chose to go to the laboratory of Otto Warburg in Berlin. Warburg was one of the great biochemists of the day and had just won a Nobel Prize.

Supported by a National Research Council Fellowship, Wald and his wife, Frances, arrived at Warburg's laboratory in 1932. This was the beginning of his *Wanderjahre,* which would take him to three distinguished laboratories and provide him the first and key insights into the structure of the visual pigments. Franz Boll had discovered the rod visual pigment in 1876. Boll (1877), as well as Willy Kühne (1878) in Heidelberg, described the effect of light on the substance. They showed the native pigment has a reddish-purple color, termed visual purple by Kühne and later called rhodopsin. In light the pigment bleaches to a yellowish-orange product (visual yellow) and then with time it fades to a colorless substance (visual white). Kühne also solubilized rhodopsin, with bile salts and showed it was a protein.

In Warburg's laboratory, Wald surmised that rhodopsin is a carotenoid-linked protein based on its absorption spectrum. He, therefore, took some retinas, extracted them with chloroform, and reacted the extract with antimony trichlo

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

ride. The solution turned a bright blue color and had an absorption curve typical of vitamin A. Earlier work had established a link between vitamin A deficiency and nutritional night-blindness, but the nature of the link was unknown. Could it be that vitamin A played a direct role in the chemistry of rhodopsin and, thus, in the visual process?

Upon seeing these findings, Warburg suggested that Wald should go to Paul Karrer's laboratory in Zürich to confirm the result. Karrer had just elucidated the structure of vitamin A and β-carotene, showing that β-carotene consists of two end-to-end molecules of vitamin A minus two water molecules. In Zürich, Wald collected retinas from cattle, sheep, and pigs; extracted them with organic solvents; and with Karrer confirmed the presence of vitamin A in all of them. In three months the job was done and it was time to move on, now to Otto Meyerhof's laboratory in Heidelberg. Meyerhof was an expert in muscle biochemistry and had been awarded a Nobel Prize in 1922.

The Germany Wald returned to was fast becoming a hostile country, especially for Jews, and both Meyerhof and Wald were Jewish. Hitler had come to power at the end of January 1933, and the National Research Council soon decreed that George must return to the United States by the end of the summer. In the middle of the summer, however, a fortuitous situation arose that enabled George to make a quantum leap forward in the understanding of visual pigment biochemistry.

Everyone was away on holiday when a shipment of 300 frogs arrived in the laboratory. The assistant was about to release the frogs when George asked for them. Extracting the retinas with various solvents, he found that from dark-adapted retinas and retinas bleached to the visual yellow stage, he could detect a novel carotenoid that was similar to but distinct from vitamin A; for example, it was yellow in

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

color and when reacted with antimony trichloride had a different absorption spectrum. This substance he called retinene. From retinas in the visual white stage, no retinene was found; rather, there was abundant vitamin A. He then proposed a visual cycle: that retinene was bound to protein in native rhodopsin and was released by light to yield the visual yellow product. He further surmised that retinene was gradually converted to vitamin A, resulting in visual white, and that regeneration of rhodopsin represented the reverse process. He wrote a note to *Nature* suggesting these relationships and returned to the United States for a second year of fellowship in the Department of Physiology at the University of Chicago.

Wald assumed his first academic position as tutor in biochemical sciences at Harvard in 1934. He remained at Harvard his entire academic career, becoming instructor and tutor in biology in 1935, faculty instructor in 1939, associate professor in 1944, and professor of biology in 1948. His research at Chicago and initially at Harvard was to confirm in other vertebrates the visual cycle he had found in the frog. Wald was aware that Kühne and others had observed that the dark-adapted retinas of certain fish were a darker purple than were frog retinas, and so in the mid-1930s he began to study this and other questions in the summers at the Marine Biological Laboratory at Woods Hole. He returned to Woods Hole virtually every summer, teaching for many years in the famous physiology course and becoming eventually a trustee of the laboratory.

In Woods Hole, George found that the rod visual pigment of marine fishes is similar to that of frogs: their retinas contain rhodopsin and, when exposed to light, the rhodopsin releases retinene that is converted to vitamin A. Freshwater fish, he discovered, were somewhat different. Their rod visual pigment absorbed maximally at longer wave

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

lengths of light and, when bleached, yielded a different form of retinene and vitamin A. He called this novel visual pigment porphyropsin, and the new carotenoids, retinene $_2$ and vitamin A $_{2.}$

From this work came an exploration of fishes that go back and forth between fresh and salt water. Salmon live in salt water but spawn in fresh water, whereas eels do the opposite. The result he found was that the vitamin A used and the visual pigment produced goes with the spawning environment. Salmon use vitamin A_2 and have porphyropsin in their retinas, whereas eels use vitamin A_1 (the more usual form of the vitamin) and make rhodopsin. Frogs, which spawn in freshwater but live on land, appeared to be different: their retinas contain rhodopsin. When George looked at tadpoles, he discovered that their retinas contain vitamin A_2, which is changed over to A_1 at metamorphosis. These experiments led him to start thinking about biochemical evolution and the molecular transformations that occur during metamorphosis, and about the origin of life. In later years, he wrote provocative articles on “The Significance of Vertebrate Metamorphosis,” “Life in the Second or Third Periods; Or Why Phosphorous and Sulfur for High Energy Bonds”, and “The Origin of Optical Activity.”

Also in the mid-1930s, Wald turned his attention to cone vision. How do cone pigments differ from the rod pigments? Using chicken retinas, which have abundant cones, he was able to extract a visual pigment that absorbed maximally at longer wavelengths: a red-sensitive visual pigment. He called it iodopsin, but because it was always mixed with rhodopsin, he could not show unequivocally that iodopsin bleached to retinene and opsin. It was not until the mid-1950s that Wald and co-workers showed that iodopsin bleaches to retinene$_1$ and opsin. The conclusion could then be drawn that this cone pigment, like rhodopsin, uses vitamin A_1 and ret

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

inene$_1$; it must differ from rhodopsin, therefore, in its opsin protein.

Wald's experiments on the visual pigments were interrupted by World War II, during which time he worked with Donald Griffin and others on applied vision research projects for the U.S. Army Board of Engineers. Infrared viewing devices were being developed to allow soldiers to see in the dark, but the prototype infrared searchlights gave off a dim red glow. Were the filters used in the searchlights defective or could humans see in the near-infrared? They discovered that, although human visual sensitivity falls rapidly at wavelengths beyond 700 nm, humans can see intense infrared light. At about 1000 nm, a warming of the skin is felt at the same time that the red glow is seen! Wald and Griffin also studied the spherical and chromatic aberration of the human lens.

At the conclusion of the war, Wald returned to studying visual pigment molecules. He was joined in this work by two individuals who were to play essential roles in his laboratory until he retired and who themselves became distinguished investigators; Ruth Hubbard joined the laboratory as a graduate student and was eventually to become George's second wife and Paul Brown became Wald's research assistant and long-time co-worker.

The next major advance in the visual pigment story came from the laboratory of R. A. Morton in Liverpool, who first surmised and then showed that retinene is vitamin A aldehyde (Ball et al., 1946). Eventually, retinene was renamed retinal and vitamin A retinol, the terms we now use. Shortly thereafter, Hubbard in Wald's laboratory worked out the enzymatic interconversion of retinal and retinol as her Ph.D. thesis, while Brown showed that rhodopsin could be generated simply by mixing retinal and opsin. This was a spontaneous reaction; no enzymes or energy source were required.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

At this point, it was possible to assemble the components needed to synthesize rhodopsin in a test tube, including retinol, opsin, and the appropriate enzymes. An immediate puzzle was observed: whereas a light-sensitive, rhodopsin-like pigment was produced when retinol from fish oil was used, nothing happened when synthetic retinol was tried. What is different between retinol in fish oil and synthetic retinol? The answer turned out to be *cis-trans* isomerization. Carotenoids can assume different shapes depending on whether their double bonds are in a *cis* or *trans* configuration. Adding a trace of iodine to synthetic vitamin A in light promotes isomerization, and such retinol would form a light-sensitive pigment with opsin.

Wald, Hubbard, and Brown, working with several organic chemists, were able to show that one cis isomer, the 11-*cis* isomer, was precursor to all visual pigments. This isomer is not only bent, it is sterically hindered and twisted, which makes it particularly light sensitive and optimal as a visual pigment chromophore. A second *cis* isomer, 9-*cis,* will form a light-sensitive pigment, but with lower light sensitivity and a different absorption maximum. No other isomer works at all. This part of the story was completed by 1955 and was satisfying. Not only did it represent the first instance of a role for *cis-trans* isomerization in biology, it also meant that rhodopsin and other visual pigments could be quantitatively synthesized in the laboratory.

The focus of the Wald Laboratory then turned to the bleaching of rhodopsin. What does light do to rhodopsin and how does it excite the photoreceptor cell? Hubbard led the research team studying these problems, working first with Robert St. George and then with Allen Kropf. By studying rhodopsin cooled to low (down to liquid nitrogen) temperatures, they showed that what light does in the visual process (and the only thing it does) is to isomerize

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

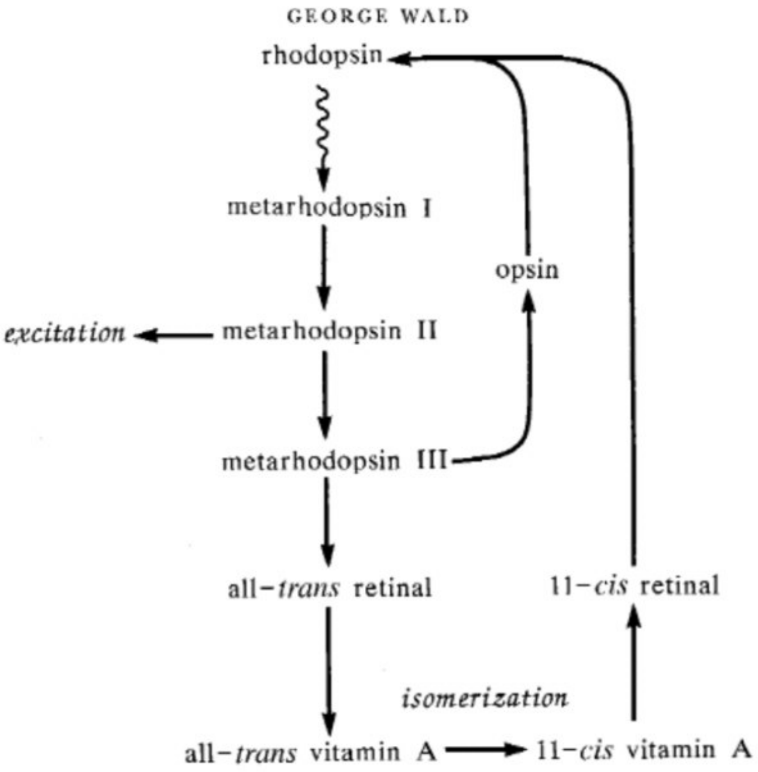
11-*cis* retinal to the all- *trans* form. Thus, a cycle of stereoisomerization is part of the visual cycle, as all-*trans* retinal or retinol must be isomerized back to the 11-*cis* form for regeneration of rhodopsin to occur. These experiments also demonstrated that rhodopsin goes through a series of molecular transformations as illuminated rhodopsin is warmed in the dark from liquid nitrogen to room temperature. These transformations reflect conformational changes in the protein, and we now know that one of these bleaching intermediates, metarhodopsin II, leads to excitation of the photoreceptor. A summary of the sequence of events that occurs from the absorption of a quantum of light by rhodopsin to its resynthesis is shown below.

How does metarhodopsin II lead to excitation of the photoreceptor cell? This was not solved until the mid-1980s, nearly a decade after Wald's retirement in 1977. It was, however, a question that vitally interested him. His mentor Hecht had shown in the late 1930s that the absorption of a single photon was sufficient to excite a rod. This suggested that a large amplification must occur when rhodopsin is excited. In 1965 Wald wrote a prescient paper, published in the journal *Science,* suggesting that light-activated rhodopsin might trigger a cascade of enzymatic reactions, much like those occurring in blood clotting, to account for the amplification. We now know this is exactly what happens: metarhodopsin II interacts with a G-protein, transducin, which activates phosphodiesterase molecules that regulate cyclic GMP levels in the photoreceptor cell (see Stryer, 1986). Membrane voltage of the photoreceptor cell relates to cyclic GMP levels and modulates neurotransmitter release from the cell. The amplification factor between one light-activated rhodopsin molecule and changes in numbers of cyclic GMP molecules is about 10^{6}. The story is now virtually complete and represents one of this century's triumphs of

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html



Scheme of the sequence of events that occurs following the absorption of a quantum of light by the rod visual pigment, rhodopsin. Light initiates the conversion of rhodopsin to retinal and opsin through a series of metarhodopsin intermediates. Metarhodopsin II is the active intermediate leading to excitation of the photoreceptor cell. Eventually, the chromophore of rhodopsin, retinal, separates from the protein opsin and is reduced to vitamin A (retinol). For the resynthesis of rhodopsin, the vitamin A must be isomerized from the all-*trans* to the 11-*cis* form, and this isomerization takes place in the pigment epithelium overlying the receptors. Vitamin A is replenished in the eye from the blood.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

biochemistry. Receptor molecules that interact with G-proteins and enzymatic cascades are ubiquitous in biology, and rhodopsin is arguably the best understood of these proteins.

As the rhodopsin biochemistry story began to wind down in the early 1960s, George turned much of his attention to color vision. Using a modification of psychophysical methods developed by W. S. Stiles in England, he determined the spectral sensitivity functions of the red-, green-, and blue-sensitive cones in normal and color-blind human subjects. At the same time, Paul Brown built a microspectro-photometer in the laboratory that enabled the measurement of the absorption spectrum of the human fovea and then of single cones. Since all of these pigments could be regenerated with 11-*cis* retinal, this showed that all had the identical chromophore but were different in their opsin structure. Jeremy Nathans and his colleagues (1986) eventually described the precise differences in the amino acid composition of the three opsin types.

I joined the Wald laboratory in 1956 as an undergraduate. It was an exceptionally lively and heady place for an undergraduate. In addition to George, Ruth, and Paul, Timothy Goldsmith was there, working on insect visual pigments, along with Norman Krinsky, who was studying the esterification and storage of retinol in tissues. Patricia Brown, Paul's wife, was also in the laboratory helping both Paul and George with various experiments. Allen Kropf joined the laboratory a year or so later.

My research dealt with vitamin A deficiency and night blindness, an old interest of George's. Indeed, George had carried out two studies on vitamin A deficiency in humans in the late 1930s, but a number of questions remained, particularly the question of whether prolonged vitamin A deficiency caused loss of opsin and degeneration of photo-

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

receptor cells. Thus, he proposed I map out the course of vitamin A deficiency in rats, focusing on the ocular manifestations of the deficiency. The project eventually formed the basis of my Ph.D. thesis and led to the discovery that vitamin A acid (now retinoic acid) could substitute for all the functions of vitamin A, except for its role as precursor to the visual pigments. Retinoic acid now appears to be a key molecule in the development of many tissues and is under active investigation the world over.

Lunch in the Wald Laboratory was an event. We took turns buying bread, deli meats, cheese, cookies, and drinks. At 12:30 p.m. we gathered in the lunchroom to be joined by John Edsall, Alex Forbes, Don Griffin, and occasionally Jim Watson, as well as their colleagues and students. George presided and we discussed science, politics, or whatever, and all joined in, from undergraduates like myself to professors emeriti such as Alex Forbes. The discussions were entertaining, provocative, and invariably stimulating.

George was one of Harvard's best teachers. He introduced biochemistry to generations of undergraduates, teaching always with great wit and clarity. In 1960 he began his famous introductory biology course entitled “The Nature of Living Things,” which he taught until his retirement. This latter course was part of the general education program at Harvard, and was taken by both students intending to concentrate in biology as well as non-concentrators. He started the course with his marvelous “Origin of Life” lecture and the second semester with an “Origin of Death” lecture. Thousands of undergraduates were enthralled by his excursions into cosmology, atoms, and molecules and, of course, all aspects of contemporary biology. More than one student altered direction to become a biologist or physician because of the fascination of Nat Sci 5, as the course was called and numbered. From the course came a popular

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

laboratory manual that George titled “Twenty-six Afternoons of Biology.” George gained national attention from the course, and as noted earlier was named one of the country's 10 best teachers by *Time* magazine in 1966. Wald was also a superb writer. A manuscript given to him for review invariably came back with more red pencil marks on it than typed words. But it was remarkably better, and those of us in his laboratory always strove to write as elegantly as did he.

The Vietnam War had a profound effect on Wald and was to lead him away from science. He was one of the first in academia to speak out against the war and was one of the signers of an open letter to *The New York Times* protesting the war in 1965. He won the Nobel Prize in 1967, and that provided him the recognition to speak out effectively on political issues. In 1969 he participated in a teach-in at the Massachusetts Institute of Technology and gave a speech called “A Generation in Search of a Future” that was published in the *Boston Globe, The New Yorker* magazine, and numerous other newspapers and publications. It eventually was translated into over 40 languages and was even released as a phonograph album.

From that moment on, politics became his primary interest: He was a forceful spokesperson against the Vietnam War, nuclear arms proliferation, and the military-industrial complex. In this effort, his wit and clarity served him well. When challenged that the arms race and the peacetime draft were “facts of life,” he retorted, “No, those are the facts of death. I don't accept them and I advise you not to accept them.” After his retirement from Harvard, Wald gave up laboratory research to devote his time entirely to political causes. He traveled widely until the last two years of his life.

Wald is survived by four children: Michael and David from his first marriage to Frances Kingsley in 1931 and

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Elijah and Deborah from his second marriage to Ruth Hubbard in 1958. At the time of his death, there were nine grandchildren and three great-grandchildren.

Over the years, Wald won numerous awards in addition to the Nobel Prize, including the Eli Lilly Award in 1939, the Lasker Prize in 1953, and the Rumford Prize of the American Academy of Arts and Sciences in 1959. The opening lines of his Nobel Prize lecture typify his approach to science and are a fitting close to this memoir.

I have often had cause to feel that my hands are cleverer than my head. That is a crude way of characterizing the dialectics of experimentation. When it is going well, it is like a quiet conversation with Nature. One asks a question and gets an answer, then one asks the next question and gets the next answer. An experiment is a device to make Nature speak intelligibly. After that, one only has to listen.

TIMOTHY GOLDSMITH, DONALD GRIFFIN, and particularly Ruth Hubbard and Elijah Wald provided material for this memoir. The piece that Ruth Hubbard and Elijah Wald wrote for the Novartis Foundation Symposium held in Japan in 1998 in honor of George Wald was particularly helpful (R. Hubbard and E. Wald. 1999. George Wald Memorial Talk. In *Rhodopsin and Phototransduction,* pp. 5-20. Chichester: Wiley [Novartis Foundation Symposium 224]). Ruth Hubbard read the memoir and made many useful comments. The memoir was drafted while I was a visiting fellow at the Bellagio Study and Conference Center of the Rockefeller Foundation.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

REFERENCES

Hecht, S. 1919. Sensory equilibrium and dark adaptation in *Mya arenaria. J. Gen. Physiol.*i(5): 545-58.

Boll, F.1877. Zur anatomie und physiologie der retina. *Arch. Anat. Physiol.* Physiol. Abt 4-35. Translated by Ruth Hubbard in *Vision Res*. 17(1977): 1249-65.

Kühne, W.1878. The photochemistry of the retina. Translated and edited by Michael Foster in *Dr. W. Kühne on Photochemistry of the Retina and on Visual Purple,* London: Macmillan and Co.

Ball, S., T. W. Goodwin, and R. A. Morton. 1946. Retinene$_1$-vitamin A aldehyde. J. *Biochem.* 40: lix.

Nathans, J., D. Thomas, and D. S. Hogness. 1986. Molecular genetics of human color vision: The genes encoding blue, green, and red pigments. *Science*. 232: 193-202.

Stryer, L.1986. Cyclic GMP cascade of vision. *Annu. Rev. Neurosci.*9: 87-119.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1934-1935

Vitamin A in eye tissues. *J. Gen. Physiol.*18: 905.

1935-1936

Carotenoids and the visual cycle. *J. Gen. Physiol.*19: 351.

1937

Photo-labile pigments of the chicken retina. *Nature*140: 545.

1938-1939

On the distribution of vitamins A_1 and A_2. *J. Gen. Physiol.*22: 391.

The porphyropsin visual system. *J. Gen. Physiol.*22: 775.

1941-1942

The visual systems of euryhaline fishes. *J. Gen. Physiol.*25: 235.

1945

Human vision and the spectrum. *Science*101: 653.

1948-1949

With R. Hubbard. The reduction of retinene_1 to vitamin A_1 *in vitro. J. Gen. Physiol.*32: 367.

1950

With P. K. Brown. The synthesis of rhodopsin from retinene_1. *Proc. Natl. Acad. Sci. U. S. A.*36: 84.

1951

With R. Hubbard. The mechanism of rhodopsin synthesis. *Proc. Natl. Acad. Sci. U. S. A.*37: 69.

1952

With R. Hubbard. *Cis*-trans isomers of vitamin A and retinene in the rhodopsin system. *J. Gen. Physiol.*36: 269.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1955

With P, K. Brown and P. H. Smith. Iodopsin. *J. Gen. Physiol.*38: 623.

1956

With P. K. Brown. The neo-b isomer of vitamin A and retinene. *J. Biol. Chem.*222: 865.

1957

The metamorphosis of visual systems in the sea lamprey. *J. Gen. Physiol.*40: 901.

1958

With P. K. Brown. Human rhodopsin. *Science*127: 222.
The significance of vertebrate metamorphosis. *Science*128: 1481.

1960

With J. E. Dowling. The biological function of vitamin A acid. *Proc. Natl. Acad. Sci.* U. S. A. 46: 587.

1963

With T. Yoshizawa. Pre-lumirhodopsin and the bleaching of visual pigments. *Nature*197: 1279.

1964

With P. K. Brown. Visual pigments in single rods and cones of the human retina. *Science*144: 45.

1965

Visual excitation and blood clotting. *Science* 150: 1028.

1966

Defective color vision and its inheritance. *Proc. Natl. Acad. Sci. U. S. A.*55: 1347.

1967

With T. Yoshizawa. Photochemistry of iodopsin. *Nature*214: 566.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1968

Les Prix Nobel en 1967. The molecular basis of visual excitation: Nobel Lecture. Stockholm: The Nobel Foundation.

1971

With T. E. Reuter and R. H. White. Rhodopsin and porphyropsin fields in the adult bullfrog retina. *J. Gen. Physiol.*58: 351.

1984

Life and mind in the universe. *Int. J. Quant. Chem.*11: 1.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

JOHN C. WARNER

May 28, 1897–April 12, 1989

BY TRUMAN P. KOHMAN

JOHN C. WARNER, ALMOST universally known to friends and even professional associates as "Jake," was proficient as a scientist, an educator, and an administrator. He was affiliated for most of his career with the Carnegie Institute of Technology in Pittsburgh, Pennsylvania, and as president he was responsible for its growth to the status of a major technological and research university, preparing the way for its metamorphosis into Carnegie-Mellon University. In addition, he was prominent in many civic activities of Pittsburgh and in many national and international scientific activities.

Jake, born on May 28, 1897, on a farm near Goshen, Indiana, was the second of five children of Elias and Addie Warner. His grandfather William Warner emigrated from Saxony about 1850, settled in Indiana, and married Elizabeth Enders. His father operated a farm and in winters worked as logger and lumberman. When Jake was eight his father died. He recalled, "Mother, who was left with few financial assets, was a loving, determined person who convinced us of the work ethic and somehow kept her family together." Jake and his older brother worked before and after school and during summers.

His mother, Addie Plank Warner, had been a country schoolteacher before marriage, and was always a source of encouragement for scholarly achievement. His primary edu

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

cation was in one-room rural schools, followed by high school in Goshen. His work outside school shifted from farm to furniture factory, where he became a cabinetmaker during high school. He decided to prepare for a career in science because of the influence of an able and inspiring teacher of chemistry and physics in Goshen High School, Mr. G. W. Warner (no relation).

Entering Indiana University, Jake earned most of his college expenses, continuing as a cabinet worker the first two summers and concentrating in chemistry. He began his career as a chemist in 1918 with the Barrett Company in Philadelphia, returning to the university to complete his undergraduate work for the A.B. in 1919 and the M.A. in 1920. That year he joined the Cosden Oil Company in Tulsa, Oklahoma, as a research chemist. The following year he again returned to the university as a graduate student (1921-23) and instructor in chemistry (1922-23). Under the supervision of Professor O. W. Brown he earned the Ph.D. degree in 1923. In 1924 he became a research chemist for Wayne Chemicals Corporation in Fort Wayne, Indiana.

On June 17, 1925, in Huntington, Indiana, Jake Warner married Louise Hamer, a daughter of William Hamer of that city. They had two sons, William Hamer Warner and Thomas Payton Warner, three grandchildren, and five great-grandchildren. Louise died in 1981, and Jake followed her eight years later.

According to Warner, “My principal avocations seem to center around music—a good listener and a poor performer—and activities in support of civic and cultural enterprises.” He was also fond of golf, the theater, and family vacations on the Maine coast.

Warner began his academic career at the Carnegie Institute of Technology in Pittsburgh in 1926 as an instructor in chemistry. He rose through the ranks to assistant professor

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

(1928-33), associate professor of theoretical chemistry (1933-36), associate professor of metallurgy (1936-38), and professor of chemistry and head of the department (1938-1949).

From 1943 to 1945 he took leave from the Carnegie Institute of Technology to supervise research on the chemistry and metallurgy of plutonium for the Manhattan Project. I met him on a visit to Glenn Seaborg's Plutonium Chemistry Division of the Metallurgical Laboratory at the University of Chicago while he was coordinating inter-site research.

Returning to Carnegie Tech in 1945, he undertook a vigorous rebuilding and expansion of the chemistry department, and I, among others, was brought aboard in 1948. He also served as dean of graduate studies from 1945 to 1950. In 1949 Warner, in anticipation of the retirement of President Doherty, was appointed vice-president and president-elect, and on July 1, 1950, he became president, which office he occupied until February 1, 1965. During his presidency the institute experienced a remarkable development of the quality of its academic work and of its reputation, especially in engineering, the physical sciences, and the social sciences. Major factors were Warner's deep understanding of science, insistence on excellence, and ability to appoint strong academic and administrative leaders.

Likewise, there was a considerable academic and physical expansion. This included the establishment in 1950 of the Graduate School of Industrial Administration, which developed into a source of significant innovations in American and European business education. In 1948 a nuclear research center with a large synchrocyclotron was established at neighboring Saxonburg. Chemistry and metallurgy continued to grow in strength along with physics, and the engineering departments were developing the application of computers to teaching, research, and design. It is no accident that during Warner's presidency six faculty members were ap

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

pointed who subsequently were awarded Nobel Prizes on the basis of work done at least in part at Carnegie Tech or its successor Carnegie-Mellon University.

Warner recognized the opportunities and challenges of the computer revolution, and in 1956 he established the Computation Center, one of the earliest in academia. This soon became one of the principal government centers for the development of computer science, and it has continuously pioneered in computer science and applications.

A number of new academic buildings were also constructed during his presidency, including the Graduate School of Industrial Administration, the Hunt Library, and the campus activities center known as Skibo, the third campus building to bear the name of Andrew Carnegie's estate in Scotland. Early in the 1960s the Scaife family pledged a sizable sum toward the construction of a new administration building on the campus. Warner actively participated in its planning, of course not realizing that it was to be named Warner Hall at its dedication in 1966.

Warner was active in a number of international educational projects. In 1962 he was influential in the establishment of the U. S. Agency for International Development's Kanpur Indo-American Program with a consortium of nine U. S. universities and institutes of technology to assist in the development of the Indian Institute of Technology in Kanpur. In 1963 I was a participant from Carnegie Tech as visiting professor of chemistry. Jake and his wife visited the Kanpur IIT and my family and me during that year.

In 1960 there had been some discussions among Paul and Richard Mellon, Warner, and James Board, chairman of the CIT Board of Trustees, about a possible merger between CIT and the Mellon Institute of Fundamental Research, located about a half-mile from the CIT campus. These discussions proceeded intermittently for several years,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

involving also CIT Senior Vice-President Edward Schatz. They ultimately led to an agreement in 1966 between Guyford Stever, who had become president of CIT following Warner's retirement, Allen Fischer, the new chairman of the Board of Trustees of CIT, and Paul Mellon, chairman of the Board of Trustees of the Mellon Institute, to effect a formal merger. This was accomplished on July 1, 1967, when the newly merged institution took the name Carnegie-Mellon University.

Warner's door was always open to staff and faculty and even students. With his infectious, cheerful smile he retained the open and direct manner that was his heritage from his boyhood experiences. He preferred informality and a minimum of paperwork. These attributes explain why it was so natural for his colleagues to call him “Jake.” His wife, Louise, was an excellent partner to the president, active on the campus scene, and warmly regarded by faculty, students, and the Pittsburgh community.

Warner retained a keen interest in the further development of the university after his retirement and until his death in April 1989 at the venerable age of ninety-one.

SCIENTIFIC ACTIVITY

Following his graduate studies, all of Warner's published scientific work was done at the Carnegie Institute of Technology.

Organic Chemistry. Warner's Ph.D. thesis work under the supervision of Professor O. W. Brown at Indiana was on the electrolytic preparation of ortho- and para-amidophenol. In work with W. J. Svirbely it was determined how the electric moment of a mono-substitute benzene derivative influenced the position of the next substitution.

Physical Chemistry—General. Several studies had to do with physical properties of compounds and solutions. With D. S.

Copyright © National Academy of Sciences. All rights reserved.

McKinney and P. Fugassi he co-authored a review of the definition of pH and extension of the acidity scale to non-aqueous systems.

Physical Chemistry—Equilibrium. Several studies were made of binary and ternary organic systems to determine conditions under which ideal behavior is approached.

Physical Chemistry—Kinetics. Warner considered that his most important contributions to science were those dealing with electrostatic effects on the rates, activation energies, and other aspects of chemical reactions in solution. A study with F. B. Stitt of the classic conversion of ammonium cyanate into urea found that the negative salt effect is of the magnitude predicted by Brönsted theory for univalent ion-ion collisions, indicating that collisions between NH_4^+ and OCN^- are responsible. With E. L. Warrick it was found that in mixed solvents the primary salt effect and dielectric constant influence are in good agreement with theory.

With several graduate students the kinetics of a number of reactions in aqueous, nonaqueous, and mixed solvents were studied and rate constants determined, with particular attention to salt and medium effects. By varying the temperature, the "critical increment" or activation energy was determined for many of them. In 1940 Warner published a review of activation energies in solution reactions. These studies continued until 1953, yielding mechanisms, solvent effects, rate constants, activation energies, frequency factors, and effects of structure, with the objective of determining the validity and limitations of various theories.

Metallurgy and Materials. Warner's first publication, with O. W. Brown and S. V. Cook, was on the effect of grinding on the apparent density of lead oxides, an apparently trivial matter but of importance in the construction of storage batteries. His interest in metals was principally in their corrosion, including theoretical aspects and practical matters

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

such as inhibitors and passivators. In 1942 he edited and wrote, with colleagues in his department, a complete revision of Leighou's *Chemistry of Engineering Materials.* In 1951 he was invited to give the Perrin Memorial Lectures at the Indian Institute of Metals in Calcutta. The six lectures on iron, steel, corrosion, etc., and engineering education were published in book form.

Manhattan Project. In 1943 Warner was asked to serve on the Manhattan Project to coordinate the work on plutonium chemistry and metallurgy, and in February 1944 he moved to Chicago with his family. With an office in the so-called Metallurgical Laboratory he made frequent visits to the various laboratories there and to the Clinton Laboratory at Oak Ridge, Tennessee (Site X), the Los Alamos Laboratory in New Mexico (Site Y), the Monsanto Research Laboratory in Dayton, Ohio, and the Ames Project at the University of Iowa.

Warner became intimately involved in an effort to prepare plutonium metal of extreme purity with respect to light elements that emit neutrons under bombardment with alpha particles, which are emitted in profusion by ^{239}Pu. When this objective was virtually achieved it was discovered that ^{240}Pu, which is produced along with ^{239}Pu by second-order neutron capture, emits copious neutrons accompanying its spontaneous fission. The fission bomb design would have to cope with this, so that the neutron emission from impurities became moot. At the request of General L. R. Groves, director of the Manhattan Project, a survey volume of this phase of the project was prepared with C. A. Thomas as editor, Warner as assistant editor, and many contributors. According to Thomas, "The compiling and rewriting of the original manuscripts was largely the work of Dr. J. C. Warner."

Warner was then given responsibility for directing the

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

writing of the proceedings of the Metallurgical Project, which ultimately became a major part of the National Nuclear Energy Series. With J. E. Vance, Warner edited and wrote three chapters of a volume on uranium technology. He was the chief editor of a volume on metallurgy of uranium and its alloys, writing one chapter. These activities engaged him for several years.

The Warner family returned to Pittsburgh in September, 1944, but Warner continued his close involvement until October, 1945, when he resumed his professorship and chairmanship of the Department of Chemistry and became dean of the Graduate School at the Carnegie Institute of Technology. From 1952 to 1964 Warner served as a member of the General Advisory Committee to the Atomic Energy Commission.

EDUCATIONAL ACTIVITY

Many of Warner's later publications were addresses or writings on education at all levels. He believed that preparation for careers in science and engineering must begin in the secondary schools, and he attributed his successful beginning to the inspiration of his high school science teacher. Unfortunately, he felt, many otherwise good teachers are hampered and restricted in methodology and objectives by school administrations. Too many science courses are superficial surveys, and mathematics is commercial arithmetic. To insure adequate scientific and engineering manpower we must provide gifted young men and women with competent and inspiring teachers by paying attractive salaries. Science instruction should emphasize basic principles rather than extent of coverage, scientific methodology, and application of the basics to the solution of problems in new situations. Mathematical training should emphasize the state

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ment of real problems in mathematical terms rather than mere memorization of formulas and rules.

At the college level, Warner was concerned that scientific education was often too specialized. He felt that technical instruction should be accompanied by a major component of humanities and social sciences, to provide a philosophical outlook and breadth of knowledge and ability to deal with the human, economic, and social aspects of the scientist's work. Accordingly, he became a strong advocate of the Carnegie Plan of Professional Education, pioneered by his predecessor President Robert Doherty. This had been developed to correct the former almost exclusive emphasis on technology in engineering education, but was extended to include education in science and then undergraduate education generally. Warner emphasized that professional education must be aimed at equipping students to continue learning after graduation and to grow throughout their lives in professional and personal stature. Analytical thinking, problem solving, and communication skills should be vital components of professional education. Research should be incorporated into the undergraduate curriculum to enable the student to solve progressively larger problems and to provide incentive to go on learning.

In the course of his teaching of chemistry at the Carnegie Institute of Technology Warner authored or co-authored several textbooks and problem and laboratory manuals. The most significant was *General Chemistry* by T. P. McCutcheon, H. Seltz, and J. C. Warner, which went through several editions.

CIVIC ACTIVITIES

Another theme of many of Warner's later publications was the relationship between science and society. He believed that the scientific method is the most efficient yet

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

devised for the generation of knowledge and its application to the betterment of mankind through solutions to complicated social, economic, and political problems. Properly trained professional people could help to provide these solutions, but all educated persons should possess familiarity with scientific methodology. Accordingly, society must provide adequate support for educational institutions, and intellectual freedom must be ensured. Only then can individuals develop to the limits of their capacity.

Throughout Warner's presidency of the Carnegie Institute of Technology he was concerned with the Cold War, and felt that maintenance of adequate military strength was essential. In the long run, however, he thought that the winner would be the nation with the best scientists and engineers. He declared, "Perhaps we must set our sights on a world culture, a worldwide expanding economy, and a world of peace under world law . . . One price of freedom may involve some sacrifice of national sovereignty."

He was quite active in civic affairs, particularly in Pittsburgh, serving as a director of the Regional Industrial Development Corporation and a member of the Pennsylvania State Planning Board. He served as a board member of several corporations, a trustee of several cultural and educational institutions, and a consultant to the United States government and those of several foreign countries.

HONORS AND AWARDS

Warner was active in numerous scientific organizations. Among offices held was the presidency of the Pittsburgh Chemists Club (1940-41), the Electrochemical Society (1951-52), the American Chemical Society (1952-53), the Pennsylvania Association of Colleges and Universities (1954-55), and the Universities Research Association (1965-67). He was elected to the National Academy of Sciences in 1956. Among

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

many awards received were the Pittsburgh Award of the Pittsburgh Section of the American Chemical Society (1945), the Gold Medal of the American Institute of Chemists (1953), the Pittsburgh Junior Chamber of Commerce Man of the Year Award (1958), and the Pennsylvania Award for Excellence in Education (1966). He received honorary degrees from fourteen universities.

I AM GRATEFUL for assistance from several colleagues, particularly Robert Parr, Herbert Simon, and Guy Berry, from Warner's sons, William and Thomas, and from Jennifer Aronson, archives and art inventory specialist at Carnegie-Mellon University Libraries.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

WITH GRADUATE STUDENTS

With H. H. Lee. The systems (I) diphenyl-diphenylamine, (II) diphenyl-benzophenone, and (III) benzophenone-diphenylamine. *J. Am. Chem. Soc.*55: 209-14 (1933). The ternary system diphenyl-diphenylamine-benzophenone. *Ibid.* pp. 4474-77. The system biphenyl-bibenzyl-naphthalene. Nearly ideal binary and ternary systems. *J. Am. Chem. Soc.*57: 318-21 (1935).

With F. B. Stitt. The conversion of ammonium cyanate into urea. Mechanism and kinetic salt effect. *J. Am. Chem. Soc.*55: 4807-12 (1933).

With R. C. Scheib and W. J. Svirbely. The solubility of biphenyl in non-polar solvents. *J. Chem. Phys.*2: 590-94 (1934).

With W. J. Svirbely and J. E. Ablard. Molar polarizations in extremely dilute solutions. The dipole moments of d-limonine, dpinene, methyl benzoate, and ethyl benzoate. *J. Am. Chem. Soc.* 57: 652-55 (1935).

With W. J. Svirbely. The directive influence of the electric moment on substitution in the benzene ring. *J. Am. Chem. Soc.* 57: 655-56 (1935). The critical increment of ionic reactions. Influence of dielectric constant and ionic strength. *Ibid.* pp. 1883-86.

With E. L. Warrick. Kinetic medium and salt effects in reactions between ions of unlike sign. Reaction between ammonium ion and cyanate ion. *J. Am. Chem. Soc.* 57: 1491-95 (1935).

With S. Eagle. Kinetic medium effects in the reaction between bromoacetate and thiosulfate ions. *J. Am. Chem. Soc.* 58: 2335-37 (1936). Kinetics of the reactions of ethyl iodide with bases in ethyl alcohol-water mixtures. *J. Am. Chem. Soc.* 61: 488-95 (1939).

With D. S. McKinney and C. F. Leberknight. The infrared absorption of liquid and gaseous 14-dioxane between 1.4 and 14.0 μ. *J. Am. Chem. Soc.* 59: 481-89 (1937).

With L. O. Winstrom. Kinetic salt and medium effects in the reaction between ethylene chlorohydrin and hydroxyl ion. *J. Am. Chem. Soc.* 61: 1205-10 (1939).

With J. E. Ablard and D. S. McKinney. The conductance, dissociation constant, and heat of dissociation of triethylamine in water. *J. Am. Chem. Soc.* 82: 2181-83 (1940).

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

With A. Alberto Colón. Mecanismos y cinética de la hidrólisis de esteres. *Bol. oficial asoc. quim. Puerto Rico* 2(2): 15-17 (1943).

With J. E. Stevens and C. L. McCabe. Kinetics of the reaction between ethylene chlorohydrin and hydroxyl or alkoxyl ions in mixed solvents. *J. Am. Chem. Soc.* 70: 2449-52 (1948).

With C. L. McCabe. The kinetics of the reaction between the ethylene halohydrins and hydroxyl ion in water and mixed solvents. *J. Am. Chem. Soc.* 70: 4031-34 (1948).

With H. D. Cowan and C. L. McCabe. A kinetic study of the neutral hydrolysis of ethylene fluoro-, bromo-, and iodohydrin. *J. Am. Chem. Soc.* 72: 1194-96 (1950).

With W. C. Woodland and R. B. Carlin. Acid-base levels in methanol-water and 1, 4-dioxane-water solutions. *J. Am. Chem. Soc.* 75: 5835-39 (1953). The relationship between acid-base level and the rate of alkaline hydrolysis of halohydrins in methanol-water and dioxane-water systems. *Ibid.* pp. 5840-41.

With K. H. Vogel, A. Alberto Colón, and R. B. Carlin. The hydrolysis of some alkyl lactates. I. Alkaline hydrolysis. *J. Am. Chem. Soc.* 75: 6072-74 (1953). *idem* II. "Neutral" and acid hydrolyses. *Ibid.* pp. 6074-75. *idem* III. Ethyl o-acetyllactate and o-acetyllactic acid. *Ibid.* pp. 6075-79.

OTHER SCIENTIFIC PUBLICATIONS

With O. W. Brown. Electrolytic preparation of ortho-amidophenol. *Trans. Am. Electrochem. Soc.* 41: 225-38 (1922). Electrolytic preparation of ortho-amidophenol. Effect of cathode materials. *J. Phys. Chem.* 27: 455-65 (1923). Electrolytic preparation of para-amidophenol. *Ibid.* pp. 652-73.

Activation energies in solution reactions. *Ann. N. Y. Acad. Sci.* 39: 345-54 (1940).

Editor, with T. R. Alexander, P. Fugassi, D. S. McKinney, H. Seltz, G. H. Stempel, Jr., and K. K. Stevens. *Leighou's Chemistry of Engineering Materials,* 4th ed. New York: McGraw-Hill (1942).

Thermodynamic considerations in the corrosion of metals. *Trans Electrochem. Soc.* 83: 319-33 (1943).

With D. S. McKinney and J. P. Fugassi. Definition of pH and extension of the acidity scale to non-aqueous systems. American Society for Testing Materials Technical Publication No. 73, pp. 19-30 (1946).

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Perrin Memorial Lectures. Calcutta: Indian Institute of Metals (1951). (Six lectures on iron, steel, corrosion, etc., and engineering education.)
Editor, with J. Chipman and F. H. Spedding, assoc., eds. M*etallurgy of Uranium and Its Alloys.* Vol. 12A, Division IV – Plutonium Project Record, Manhattan Project Technical Series, National Nuclear Energy Series. Oak Ridge, Tenn.: U. S. Atomic Energy Commission (1953) (originally classified secret; declassified 1965).

ON EDUCATION AND SOCIETY

America's opportunity as a center of learning. *Chem. Eng. News* 29: 4668-70 (1951).
Contributions of science to the goal of civilization. *Chem. Eng. News* 29: 108-10 (1951).
Freedom, scholarship, and centers of learning. *Trans. Am. Soc. Metals* 45: 32-38 (1953).
To teachers of science and mathematics in the schools. *Sch. Sci. Math.* 54: 340-44 (1954).
New responsibilities for members of the professions. *Chemist* 30: 277-81 (1953).
National goals and the university. *Science* 142: 462-64 (1963).

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.
Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

JEROME BERT WIESNER

May 30, 1915–October 21, 1994

BY LOUIS D. SMULLIN

JEROME WIESNER—JERRY to almost everybody—led an exciting and productive life and, more than most, he made a difference. His career, the offices he held and the honors he received are spelled out in the MIT obituary notice at the time of his death. As interesting and impressive as is the list of offices and honors, even more interesting is his transformation from a young engineer just out of college to an "electronic warrior" during World War II, to a "cold warrior" during the early days of the "missile gap," and finally to a leading spokesman for the nuclear test ban and a worker for nuclear disarmament.

Jerry and his younger sister, Edna, were the children of Joseph and Ida Wiesner, each of whom had come to the United States at about the turn of the century. To escape having to take violin lessons, at age nineteen, Joseph had run away from his parents in Vienna in about 1892 and had shipped out to places as far away as Alaska and the California gold fields before landing in New York. (Edna remembers her father telling stories about meeting and drinking with Jack London in Alaska.) Ida had come from Romania to New York with her younger sister. She worked in the garment industry and then as a housekeeper until she and

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Joseph met and married in 1914. Shortly thereafter, they moved from New York to Dearborn, Michigan. Jerry was born in nearby Detroit on May 30, 1915. In the early days of their marriage Ida and Joseph helped Ida's four brothers immigrate to the United States. Sometime during the 1920s, Ida and Joseph opened a small “mom and pop” dry goods store in Dearborn, and the two children grew up and went to the public schools there.

Jerry went on to study electrical engineering and mathematics at the University of Michigan in Ann Arbor. While there, he met Laya Wainger, a fellow student majoring in mathematics. They married in 1940, and developed a deep and abiding lifelong partnership. Besides being the mother of their four children—Steven, Zachary, Lisa, and Joshua—Laya was one of Jerry's primary intellectual companions. Together they explored issues and ideas. She critiqued many of his most important documents (beginning with his Ph.D. thesis). Laya was very active in community and national civic affairs, and they entertained and worked with leaders from the academic, arts, media, business, civic, and governmental arenas.

Throughout his life, Jerry's glass was always at least half full, as is illustrated by his sister's recollection of his teenage efforts as a radio ham in Michigan. Just after he got his first receiver working and on the air, he burst out of his bedroom to announce that he had picked up a station from a foreign country. Not much later, he came out to amend his announcement. He had, in fact, tuned into a Polish language station in Hamtramck, a small city about 10 miles from Dearborn.

The story illustrates his abiding optimism about the power and possibilities inherent in technical things, and in his later years about the possibility of saving the world from a nuclear disaster. As noted by MIT's news office, Jerry was “a

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

leading voice for decades in international efforts to control and limit nuclear arms . . . a key figure in the establishment of the Arms Control and Disarmament Agency, in achieving a partial nuclear test ban treaty, and in the successful effort to restrict the deployment of antiballistic missile systems."

WORLD WAR II AND THE POSTWAR YEARS

Jerry's life, as for so many engineers and scientists of his generation, was shaped by his World War II experience in the MIT Radiation Lab, where he worked on the development of radar, and by his work at Los Alamos directly after the war. Through these experiences, he learned new scientific and engineering ideas and techniques, he learned about the needs of the military, he learned how to deal with the military, he learned how to manage large group projects, and he met the people who helped shape his career.

In the Radiation Lab, he began his weapon systems work on a 3-cm radar for a Navy night fighter. This was followed by his taking over the job of directing the final stages of development of Project Cadillac, an airborne early warning (AEW) radar for the Navy. It was a very big and complex system that brought him into close contact with the highest levels of the Navy command structure. The AEW system played an important role in the Battle of the Pacific.

At the end of the war, Professor Jerrold Zacharias was recruited from the Radiation Lab to set up a new nuclear weapons engineering group at Los Alamos; among those he brought with him was Jerry Wiesner. In his autobiographical notes, Jerry indicates that due to the postwar letdown at Los Alamos, he was not very gainfully employed, but he was able to attend many of the heated discussions about the future peacetime control of atomic weapons and of atomic energy. These made a deep and lasting impression on him: "It was for me an interesting and sometimes exciting period.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

In later years I realized that the understanding I gained, both about the bomb and the controversy surrounding its use, had provided me with a valuable education on issues that were to occupy a large part of my life . . . ."

Before it was known as such, the Cold War began almost immediately after the end of World War II. Poland, Hungary, and Czechoslovakia were put under Soviet control. The Marshall Plan and the North Atlantic Treaty Organization came into being, and the attention of the military was now directed towards the new situations. The wall was erected between East and West Germany, and the Berlin blockade and airlift were symbols of the growing danger of conflict between the Soviet Union and the West.

In 1945, as MIT was moving into its peacetime mode of operation, Professor Harold Hazen, head of the Department of Electrical Engineering asked that Wiesner be appointed to the faculty. President James R. Killian sent the official letter. In his book *The Education of a College President* Killian wrote:

The last paragraph of my letter was more dramatically prophetic than I realized in 1945. Herewith the letter:

Dear Dr. Wiesner:

. . . We look forward with much pleasure to having you join us at the Institute. We believe opportunities here over the coming years in your field of interest will be of ample scope to your work.

Jerry accepted the invitation and returned to MIT in 1946 as a faculty member in electrical engineering, conducting his research in the Research Laboratory of Electronics (RLE), the peacetime successor to the Radiation Lab.

After his return to MIT, Jerry was a participant in many of the summer studies on military problems (anti-submarine warfare, the distant early warning [DEW] line, etc.). In these studies a selected group of scientists and engineers from

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

industry and academia were briefed by appropriate military officers and civilians on the detailed nature of the problems, and various solutions were proposed and discussed. Many important recommendations were made and implemented. In this way Jerry (and his colleagues) began the move from purely technical matters into the broader domain of policy making.

The beginnings of the Cold War were symbolized by a number of important technical events. Among these were the race between the United States and the Soviet Union to copy the German ballistic missile technology, the explosion of the first Soviet atomic bomb, and the successful launching of *Sputnik.*

Faced with the threat of the Soviet atomic bomb, the MIT Lincoln Laboratory was established in 1951 to work on the problems of air defense of the continental United States. One of its projects was to set up a distant early warning chain of radar stations to warn of an attack by Soviet bombers coming across the North Pole. To get the maximum warning time, the line would be located along the shore of the Arctic Ocean. However, radio communication in these high latitudes was uncertain and could be blacked out for long periods due to intense electrical activity in the ionosphere. In about 1953 Wiesner and some of his colleagues proposed the use of forward scattering from the troposphere for reliable (but narrow band) communication. For many years, these systems provided secure communication from the DEW line to the North American Air Defense Command in Colorado.

In 1952 Wiesner became the third director of RLE, succeeding professors Jay Stratton and Albert Hill in that role. Under their successive leaderships, RLE developed into a multidisciplinary, interdepartmental center. The joint services (Army, Navy, and Air Force) contract under which

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

RLE was organized, specified that it should "do research in the field of electronics, physics, and communication, and publish."

Inspired by Professor Norbert Wiener, Jerry recognized that communication included psychology, language, and sensory physiology. Thus it was that from its earliest days, the lab brought in linguists, psychologists, and neurophysiologists, as well as information theorists, radio engineers, and physicists. Much later, when he became provost and then president of MIT, he played a similar role in stimulating the growth of the humanities and the arts at MIT.

THE WASHINGTON YEARS

Concern about the threat implied by the Soviet A-bomb was very real in Washington. In 1954 the Office of Defense Management set up a Technical Capabilities Panel (TCP) to study the capabilities of the Air Force. MIT President James Killian was asked to head the study, and he invited Wiesner to join the panel as head of the Overseas Military Communications group. The launching of *Sputnik* produced a scare in Washington that the Soviet Union already had an arsenal of ICBMs at a time when we had almost none. This awareness and fear was summarized in one short phrase: the missile gap. In 1957 President Eisenhower set up a committee (later known as the Gaither Committee) to study the implications of a nuclear war. Both the TCP study and the Gaither study showed that an all-out nuclear war would be catastrophic, and that there would be no winners. Both sides would suffer millions of deaths, destruction of their cities and infrastructures, and would be ruined beyond repair.

In his book *Making Weapons, Talking Peace* Herbert York describes the early steps towards achieving a nuclear test ban (pp. 116-18) in the Eisenhower administration. There was a special meeting of the President's Scientific Advisory

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Committee (PSAC) at Ramey Air Force Base in Puerto Rico. Eisenhower had specifically asked for advice about the proposed test ban. Except for York, there was general agreement for the idea of a test moratorium. York says,

I argued that the matter was essentially a political and strategic issue and that a group made up entirely of scientists wasn't appropriate for dealing with such matters . . . After the meeting, Jerry Wiesner took me aside and patiently explained several things to me. One was that the President could ask anyone any question he wished. Another was that there really was no one else; it was us or no one, be that plausible or not . . . I later learned that Wiesner himself actually did have such doubts . . . In a speech made two years later in 1960, he said, “I've been billed as an expert on arms control, and I think I am an example of what's wrong with the American posture in this field . . . My background is primarily in the field of military technology . . . I come to the arms control field with all the biases, prejudices, and skepticism of someone who has been working very hard on military weapons.”

In his own (unpublished) autobiographical notes, Jerry later commented about the Gaither Report:

As we began to write the report, it became clear that a part of the group regarded its ingredients as recommendations that they felt very strongly about while others of us (certainly myself) found it impossible to differentiate between a war that had only 60 million casualties instead of 100 million at a cost of many billions of dollars. I also wondered whether such an increased spending for war preparations might make a war more likely. The Gaither study just made me even more certain than I had been before that nuclear weapons were not useable weapons.

The following paragraphs by MIT Professor Carl Kaysen summarize this period:

Jerry Wiesner, like so many of “the best and brightest” in science and engineering, spent World War II in contributing to the development of military technologies. He worked on both of the two largest scientific technological efforts of the time: first on radar at the Radiation Lab at MIT, then on the atomic bomb at Los Alamos. Out of these experiences grew his

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

lifelong concern with, and action on, the technologies of war and their redirection to peace.

At first, he continued to direct his efforts to improving the U. S. arsenal. In 1954 he served on the Technical Capabilities Panel chaired by James Killian, which recommended to the Pentagon the initiation and vigorous pursuit of an intercontinental ballistic missile program. When in response to the Soviet launch of *Sputnik* in 1957, President Eisenhower appointed Killian to the newly created post of science advisor to the President, and made him chairman of the President's Scientific Advisory Committee (PSAC), Wiesner was among its initial members.

In 1957 a panel of PSAC, the Gaither Committee, was convened to survey the comparative military capabilities of the United States and the Soviet Union for offense and defense. Wiesner served as its staff director. The Committee came to a pessimistic conclusion. That experience convinced Wiesner that the intense competition with the Soviets in improving weapons technologies and procuring arms would reduce rather than increase America's security. Only programs of arms limitations and other kinds of arms control could make us more secure.

These conclusions were reinforced in the summer of 1958, by Wiesner's participation in the Geneva Conference on Preventing Surprise Attack. There he also experienced the difficulty of securing any kind of agreement on arms limitations or arms control in the face of the profound mutual hostility and suspicion between the United States and the Soviets.

In 1961 the newly elected President Kennedy chose Wiesner to be his science advisor. During his tenure in that post for the three years of the Kennedy Administration and the first year of Johnson's, arms control in various forms was at the center of the agenda. Wiesner had both successes and failures.

Wiesner's outstanding success was his role in the achievement of the Partial Test Ban Treaty of 1963, by which the United States, the United Kingdom, and the Soviet Union agreed to cease nuclear weapons testing in the atmosphere and underwater, and to ban testing in space. The failure to achieve a complete test ban ending underground tests as well was a great disappointment to Jerry and to the administration. The two sides never succeeded in bridging the gap on what quota of on-site inspections in their respective territories was adequate to provide effective verification of a complete test ban.

An equally great, but not then public, disappointment was Wiesner's inability to persuade the President and the Defense Department that the

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

rapid build-up of ICBMs and submarine launched BMs proposed in the President's first full budget was both unnecessary and undesirable: unnecessary because the new satellite reconnaissance systems showed that the Soviet missile forces were very much smaller than the United States had believed; undesirable because our build-up would be matched by the Soviets, with the result of greater insecurity for both sides.

Three other areas in which Wiesner's arms control efforts succeeded were in civil defense, the installation of permissive links on nuclear weapons carried by aircraft and ICBMs and the non-deployment of anti-ballistic missile systems.

Wiesner helped to persuade the President in 1961 to call for a very modest civil defense program of fall-out shelters rather than the large program of blast shelters that many in the military and some of Kennedy's political adversaries were calling for. Wiesner saw the desirability and technical feasibility of installing electronic locks—permissive action links or PALs—on nuclear warheads. These made more certain positive control that prevented launches without presidential authorization. Further, he drove the program through to realization in a remarkably short time, although he never succeeded in overcoming the Navy's resistance to the use of PALs on submarine launched missiles.

PSAC's strong technical criticisms of the Anti-Ballistic Missile (ABM) systems available in 1961 helped lead to a decision against deployment. Wiesner argued forcefully for the undesirability of deploying even much improved systems, pointing out that technological improvements would continue to give the advantage to the offense. He continued his advocacy after he left Washington to return to MIT and made a major contribution to the achievement of the ABM treaty of 1972.

More broadly, Wiesner's leadership of PSAC was outstanding. His closeness to the President, and his ability to persuade its distinguished and busy members to contribute significant time and effort led to PSAC's effective engagement with a broad range of issues.

Wiesner remained active on arms control and disarmament issues for the rest of his life. A member of the MacArthur Foundation's Board of Trustees from the time of its organization, he co-chaired its Committee on International Security. In that capacity, he was instrumental in getting the Foundation to fund the establishment and operation of programs of research and teaching on arms control in a number of universities, including Harvard, Stanford, MIT, Michigan, Cornell, and Illinois. He participated in the Soviet-American Disarmament Study Group between 1965 and 1975; it was an

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

important forum for the discussion of ABM and other arms control issues. He wrote and spoke publicly on these issues whenever opportunity offered.

Following the death of President Kennedy, and after serving as President Johnson's science advisor for about a year, Jerry's three-year leave from MIT came to an end, and in 1964 he returned with his family to Cambridge and to their home in Watertown. His re-entry into academic life began with his appointment as dean of the School of Science. However, his connections to national and world affairs were too numerous to be severed, and there were too many unsolved problems that demanded his attention. Primary among these was the continuing danger of nuclear war.

PROVOST AND PRESIDENT OF MIT

When Howard W. Johnson became MIT's twelfth president in July of 1966, he appointed Wiesner as provost, beginning a number of years of close teamwork. These were the years of international student unrest, and of the growing protest against the war in Viet Nam. Johnson and Wiesner by dint of their cool but steady style were able to keep open communications with the protesting students and faculty. Unlike the experience at many other universities in this period, it was never necessary to call the police onto the campus.

In July of 1971 Wiesner became the thirteenth president of MIT, succeeding Howard Johnson, who became chairman of the MIT Corporation. When Jerry became president, he had many unusual assets in addition to his technical competence. He, of course, had a thorough knowledge of MIT gained during the postwar decades. He knew personally many members of the faculty and administration, as well as of the national executive and congressional branches.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

By the time he became president, the troubles of the 1960s had subsided sufficiently to allow him to turn his attention to the job of building and developing MIT. As president, Jerry pursued educational reforms and the cultivation of fields not previously represented or represented at less than MIT's potential level of excellence. Professor Paul E. Gray served with Jerry as the Institute's chancellor during this period, and later succeeded him as president. Reflecting on that era, Gray wrote:

As dean, provost, and president, Jerry expanded MIT's teaching and research programs in health sciences, humanities, and the arts. And he strengthened the Institute's undergraduate educational programs through creative employment of a fund for educational innovation, which had been provided by his close friend Edwin H. Land, the founder of the Polaroid Corporation.

This resource was used in 1970 to enable the late Professor Margaret L. A. MacVicar to start the Undergraduate Research Opportunities Program. This program, now used year after year by approximately three-quarters of the undergraduates at MIT, is widely regarded as the most important educational innovation at the Institute in this half-century.

He sought new ways to bring MIT's expertise in science and engineering to bear on social issues, such as health care, urban decay, mass transportation, and housing. He was instrumental in establishing the MIT Program in Science, Technology, and Society, which focuses on ways in which science and technological and social factors interact to shape modern life. This interdisciplinary program has become a highly respected component of the Institute's academic structure.

In his later years, Jerry was centrally involved, together with Professor Nicholas Negroponte, in the creation of the Program in Media Arts and Sciences and the Media Laboratory, which are housed at MIT in the Jerome and Laya Wiesner Building.

He was deeply committed to the goals of this nation's civil rights movement, and the period of his leadership of MIT produced the greatest progress up to that time in bringing women and minorities to the student body and the faculty.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Jerry served as president until June 30, 1980, when he retired and resumed the title of institute professor, a position reserved for a handful of the Institute's most distinguished faculty, and which he had held from 1962 to 1971. Paul Gray summed up the influence of Jerry's leadership of MIT in this way: “This special place has benefitted beyond acknowledgment from his fierce belief in the value of racial, ethnic, and gender diversity in this community, from his insistence on intellectual quality in our programs, and from his vision of the ways in which science and technology and the arts and humanities reinforce each other.”

Laya Wiesner (1918-98) was an important partner in these latter areas in particular. She provided the inspiration and leadership for the MIT Workshop on Women in Technology that took place in May 1973 with grants from the Carnegie Corporation, the GE Foundation, and the Alfred P. Sloan Foundation. This led to the WITS Project (Women in Technology and Science), a program that recruited MIT faculty members and engineers and scientists from Massachusetts industry to speak to high school students on science and technology careers. Subsequently, the project worked intensively with Boston schools to foster minority participation in scientific and technical education endeavors.

In civil rights Laya's activities covered much ground, but two of them bear special mention. She was one of the original organizers of METCO, a system for busing black students from Boston to schools in the more affluent suburbs. She was one of a group of women from around the country who went to Mississippi in the 1960s to observe and report on voter registration activities in that state. They hoped that their presence would lessen the danger of police violence, and it did.

Starting in about 1970 a progressive muscular disease polymyositis began to seriously affect Laya's ability to travel and

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

even to move around. At the time of Jerry's death she was already confined to a wheel chair, but she continued to maintain a lively interest in her family and in world and public affairs. When Laya died in September 1998, Catherine M. Stratton, widow of MIT's eleventh president, Julius, said: “Laya Wiesner was a remarkable woman with an indomitable spirit. She was incredibly courageous, allocating her energy to the causes which mattered most to her—civil rights, mentoring MIT women students in the fields of science and engineering, and being a sparkling, creative partner to her husband.”

THE MACARTHUR FOUNDATION

MIT was not the only institution to benefit from Jerry's special talents. Throughout his career, Jerry was active in community affairs and was a frequent advisor to public and private agencies, foundations, educational institutions, and industry. The following recollection by Ruth Adams (first director of the MacArthur Foundation's Peace and Security Program from 1984 to 1994) describes the range of Jerry's activities and contributions as a member of the board of the MacArthur Foundation:

The John D. and Catherine T. MacArthur Foundation was created in 1978. Its founder, John D. MacArthur, bequeathed his large estate to the foundation without specifying its purposes. A year later five members were added to the originally narrow corporate board of the Foundation, among them was Wiesner. This fortuitously brought together a foundation in search of a comprehensive vision of its larger purposes and a man who would supply one.

From 1979 until his death in 1994, Jerry persuasively engaged his colleagues on the board in discussions leading to the opening of many innovative new programs and ideas. A suggestion of the ambitiousness of his vision is provided by a memo he wrote to the board at the time: “I believe humanity is living through a crisis of vast dimensions brought on by the scientific and technological revolution which is reshaping relationships between

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

individuals and their societies, and between nations. The opportunities are enormous and so are the dangers."

Consistent with this, Jerry often spoke of the duties as well as privileges of foundation directors. Their responsibility, he felt, was to guide a foundation in responding both creatively and flexibly to a wide range of social as well as political needs and problems. Indicative of his approach was the way in which the Mental Health Program was created during the Foundation's first years. As Jerry wrote: "We spent hundreds of hours listening to experts in many areas of mental health before deciding that mental health problems were perhaps the most serious and least understood health problems of the world. When we finally decided to do something about it, we realized that people in related areas were not communicating effectively, so we began an imaginative plan of networking, an idea that has infused the entire Foundation . . . ."

By any reckoning, the MacArthur Fellows Program quickly became an important feature of the American intellectual landscape, and Jerry was proud to have been one of its architects. The same could be said of other major undertakings in which he had a central part, including the establishment of the World Resources Institute, the Energy Foundation, and the Chicago Education Initiative.

In 1983 the MacArthur Board appointed a committee to explore prospects for an international program on security issues, with Jerry as its chair. Other members included Jonas Salk, Murray Gell-Mann, and two staff members James Furman and Ruth Adams. As a first step, the committee invited McGeorge Bundy to head an independent study of existing programs in the field and to enlist the advice and counsel of specialists. Its report led to the establishment of an International Security Program that, in its first decade, contributed more than $200 million in support of young researchers, educational institutions, international collaborative projects, and public interest organizations. Wiesner's collaboration with Andrei Sakharov and other Russian scientists led to the establishment of the Foundation for the Survival and Development of Humanity, the first private foundation in the Soviet Union. That, in turn, paved the way for the creation of a MacArthur program in Russia, directed by a Russian scholar, and the unprecedented opening of a permanent Moscow office for the Foundation.

FINAL YEARS

In 1988 Wiesner suffered a heart attack and stroke. The

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

stroke affected his left side and left him with almost no speech. Jerry took his rehabilitation seriously and worked very hard at the various available therapies. Within about a year and a half he had recovered much of his speech, and he was able to drive a suitably modified car. The recovery was enough to allow him to attend meetings, to speak in public, and to travel. He was able to make several trips from Watertown to Chicago for MacArthur Foundation board meetings. During this period, he devoted part of his time to writing a biographical memorandum.

Former MIT President Howard W. Johnson, at the time of Jerry's death, said: “Jerome Wiesner was a creative force at MIT for the last half century. With his great technical and social inventiveness he made notable contributions in a number of fields as a professor, an administrator, and a corporation member. Beyond MIT he made significant impacts in arenas ranging from arms control to the arts. He will be deeply missed at MIT. And for those of us who worked with him closely for many years, the loss is immeasurable.”

I have tried to capture here some of the ways Jerry Wiesner made a difference in the world, and to trace his transformation from young engineer to a leader of international stature. Sometimes prose just doesn't do it. Jerry had a long friendship with the poet Archibald MacLeish. They first met when Jerry came to the Library of Congress in 1940 to work with Alan Lomax on the recording of indigenous American folk music. As Jerry said in his memoirs, “My title was chief engineer of the Library of Congress. I was also its entire engineering staff.” At the time of Jerry's inauguration as MIT president in 1971, MacLeish wrote a poem in tribute to his old friend. It is a fitting conclusion to this memoir as well:

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

A good man! Look at him against the time!
He saunters along to his place in the world's weather,
Lights his pipe, hitches his pants,
Talks back to accepted opinion.
Congressional committees hear him say:
"Not what you think: what you haven't thought of."
He addresses presidents. He says:
"Governments even now still have to govern:
No one is going to invent a self-governing holocaust."
The Pentagon receives his views:
"Science," he says, "is no substitute for thought.
Miracle drugs perhaps: not miracle wars."
Advisor to presidents, the papers call him.
Advisor, I say, to the young.
It's the young who need competent friends, bold companions,
Honest men who won't run out,
Won't write off mankind, sell up the country,
Quit the venture, jibe the ship.
I love this man,
I rinse my mouth with his praise in a frightened time.
The taste in the cup is mint,
Of spring water.

IN THE PREPARATION OF this essay we received help, critical advice, and encouragement from the following people:

Mildred Dresselhaus, MIT

Joshua Wiesner

Edna Wiesner McNeal

Walter and Judy Rosenblith (Walter and Jerry had been very close friends after Walter came to MIT in 1948; he served as MIT provost under Wiesner. Their advice about the organization of this memoir was extremely helpful.)

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

Kathryn A. Willmore, vice-president and secretary of the MIT Corporation

Edith Ruina, who worked with Laya Wiesner on women's issues at MIT

Kosta Tsipis, retired director of the Program in Science and Technology for International Security, MIT

J. Y. Lettvin, professor emeritus, MIT

Frances Tenenbaum, a close friend of Laya's since University of Michigan days

Peter Bartes, Library of Congress

Anna Kariotakis, Alan Lomax's daughter.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1942

The recording laboratory in the Library of Congress. *J. A. S. A.* 13.

1950

With T. P. Cheatham and Y. W. Lee. The application of correlation functions in the detection of periodic signals in noise. *Proc. IRE*38(10).

1952

With D. K. Baily, H. G. Booker, L. V. Berkner, H. G. Booker, G. F. Montgomery, E. M. Purcell, and W. W. Salisbury. A new kind of radio propagation at very high frequencies observable over long distances. *Phys. Rev.* 86(2).

1958

Electronics and the missile. *Astronautics* 3(5).

1960

A study of comprehensive arms control systems. *Daedalus.*

1962

Peace could be achieved in our lifetime if . . . . *Daedalus.*

1964

With H. F. York. National security and the nuclear test ban. *Sci. Am.* 211(4).

1966

Disarmament and European security. Testimony before the Senate Committee on Foreign Relations,June 28, 1966.

1967

The Cold War is dead but the arms race rumbles on. *Bull. At. Sci.*June 1967.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1969

With D. Brennan, W. O. Douglas, L. Johnson, and G. S. McGovern. ABM: Yes or no?Center for Study of Democratic Institutions 2(2).

With A. Chayes, eds. *ABM: An Evaluation of the Decision to Deploy an Anti Ballistic Missile System.*New American Library.

1970

Arms control: Current prospects and problems. *Bull. At. Sci.* May 1970

1971

The information revolution and the bill of rights. *Comput. Autom.* 20(5).

1974

Information and a free society. *Technol. Rev.* 78(8).

1987

The rise and fall of the President's Science Advisory Committee. 24th Cosmos Club Award.

1993

With P. Morrison and K. Tsipis. Beyond the looking glass: The U.S. military in 2000 and later.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Alfred P. Wolf

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

ALFRED P. WOLF

February 23, 1923–December 17, 1998

BY JOANNA S. FOWLER AND MICHAEL J. WELCH

ALFRED P. WOLF WAS born in Manhattan on February 13, 1923. Al was the son of Margarete and Josef Wolf, who had emigrated from Germany before World War I. Josef Wolf had been a pastry chef on a German cruise ship, and when World War I broke out his ship could not go back to Germany. So he and Margarete, who was a dressmaker, settled in Manhattan raising Al and his older brother John. Al grew up to be the quintessential New Yorker, drinking in the culture and becoming a connoisseur of food and wine, and the arts. His first love was music; in fact, his chosen profession was to be a concert pianist, but as he would later comment, “I would go to Carnegie Hall and hear Artur Schnabel play the piano, and I quickly realized that I could never be any good.”

What Al did possess was a keen aptitude for science, particularly chemistry. During his long career, he pioneered the development of labeling techniques that used the reactions of hot atoms (i.e., atoms with very high translational energies produced by recoil from nuclear reactions). He used this as a springboard to develop labeling methods to produce organic radiotracers that enriched the field of nuclear medicine and allowed the field of human neuro-

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

science to germinate and to blossom through the use of positron emission tomography, or PET. He is most well known for the role he played in the development of 2-deoxy-2-[^{18}F]fluoro-D-glucose (^{18}FDG), a radiotracer that stimulated more than two decades of progress in the use of human neuroimaging to study mental illness. ^{18}FDG remains the most sensitive tracer ever developed to image tumors and tumor metastases, and it has provided the means of directly studying the effects of drugs on the human brain.

Al's education at Columbia College was interrupted by World War II, when at eighteen years of age he enlisted in the army and spent some time working on the Manhattan Project in Los Alamos, where he worked on the initiator of the atomic bomb. While at Los Alamos in 1941-42, he worked on metal X and metal Y, which he later found out were uranium and plutonium. His group leader at Los Alamos was Richard W. Dodson, who would later become the first chair of the Chemistry Department at Brookhaven National Laboratory, a new national laboratory established in 1947 and dedicated to the peacetime uses of atomic energy. After the war, Al returned to Columbia to finish his education. He joined the group of William Doering as a graduate student and worked on the fenchol β-fenchene rearrangement. In an American Chemical Society symposium honoring Al in 1998, Doering described Al's elegant early mechanistic studies by characterizing him as a man having a “pride of craft,” and the only one of his graduate students to have his thesis bound in full morocco leather. He set high standards for himself and for others. It was a pattern that he carried through his entire career.

Al Wolf's early studies involved research on the chemical fate of carbon atoms. Initially in collaboration with Carol Redvanly and R. C. (Andy) Anderson, he produced carbon-14 using the Brookhaven research reactor. He rapidly real

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

ized that in producing carbon-14, he was studying not only the chemical fate of the carbon atoms but also the radiation chemistry of the target compound. He continued his work using the 20-minute-half-life nuclide carbon-11, which could be produced with much lower radiation doses even though it challenged the chemist in the analysis of the products. Using carbon-11, Al and his colleagues probed unusual reactions of carbon that were not possible to study by other means at that time. When the research required new analytical tools, Al designed and built them, including a flow proportional counter in 1967 and a GLPC instrument designed for short-lived isotopes in 1969. These studies led in a major way to our understanding of these basic systems.

Al's curiosity also drove him to study other systems, and here he made noteworthy contributions to problems in organic chemistry. He developed non-nuclear techniques to study the chemistry of carbon atoms and in the late 1960s he attacked one of the most challenging problems in organic chemistry, that of the synthesis of tetrahedrane. With colleague Philip Shevlin, he made a significant dent in the problem by demonstrating the probable intermediacy of tetrahedrane in the synthesis. Another highlight in Al's career was his study of the selectivity of the reactions of elemental fluorine with aromatic compounds. With his colleague and close friend Fulvio Cacace, he was able to show that aromatic fluorination with elemental fluorine showed the characteristics of electrophilic substitution of low regioselectivity.

Throughout his career, Al continued to build a strong research group at Brookhaven, and many came from around the world to work with him. This led to what came to be known affectionately as the “Wolf Pack,” which consisted of Al, a core group of Brookhaven scientists, including David Christman, and an ever-changing group of postdoctoral fel

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

lows and students who were stimulated by Al's continual challenges to go beyond observation to understand the phenomena at the fundamental level.

Al carried this challenge more and more into the area of biology and medicine, and by the mid-1960s his fundamental studies had laid the groundwork for the synthesis of simple molecules labeled with the short-lived positron emitters in pure form and high specific activity for tracer applications in medicine using a PET. The chemistry of two of these isotopes, carbon-11 and fluorine-18, became a focus because they lent themselves to incorporation into organic compounds. However, their chemistry was dominated by their short half-lives: carbon-11 has a 20.4-minute half-life and fluorine-18 has a 110-minute half-life. Al and his group precisely measured the excitation functions of many nuclear reactions and produced important medical isotopes, including C-11 and F-18. These measurements on C-11 and F-18 in particular are standards around the world. In parallel with these measurements Al and his group (including David Christman and Ronald Finn) made a major breakthrough in the development of nitrogen gas targets producing C-11-labeled precursors. The bombardment of nitrogen gas with protons produces carbon-11 and an alpha particle ($^{14}N(p,\alpha)^{11}C$). If nitrogen with a trace of oxygen is bombarded with protons of sufficient energy, C-11 is produced in the chemical form of carbon dioxide. If hydrogen is present during the bombardment, methane is produced. This can rapidly be converted to carbon-11-labeled cyanide in the presence of ammonia and Pt at 1000°C. Al made similar contributions to the development of F-18-labeled precursors and with Richard Lambrecht developed a neon gas target for the production of F-18-labeled elemental fluorine, which was first presented in 1973. Here a target of neon gas is bombarded with deuterons producing F-18 and

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

an alpha particle. When a small amount of fluorine gas is present, F-18-labeled elemental fluorine is produced. Later when F-18 in the form of hydrogen fluoride was needed for the synthesis of high specific activity tracers for neuroreceptor studies, he and Tom Ruth measured the ^{18}O (p,n) ^{18}F excitation function and with Bruce Wieland developed a small-volume enriched water ($H_2{}^{18}O$) target. Early on, Al became an expert on cyclotrons and generously provided advice to dozens of institutions worldwide that were starting PET programs.

Interestingly, the basic studies of fluorine-18 not only led to the labeling of biological compounds but to new knowledge in the area of atmospheric chemistry. Al's long-time friend F. Sherwood Rowland, originally a halogen-hot-atom chemist, used his background in this area to understand the decomposition of ozone by species formed from freon.

Although producing isotopes like carbon-11 and fluorine-18 is a challenge, creating organic compounds from simple labeled compounds like cyanide, carbon dioxide, fluoride, and fluorine gas was an equal problem. To prepare an organic compound labeled with carbon-11 one has about 45 minutes; otherwise all is lost to radioactive decay. Success hinges on the availability of large quantities of synthetically useful labeled precursor molecules. Al's studies gave the organic chemist carbon-11-labeled carbon dioxide and cyanide and fluorine-18-1abeled fluoride ion and elemental fluorine from which to synthesize complex radiotracers; this knowledge formed the basis of a new interest in developing positron-emitter-labeled radiotracers so that the tracer method could be applied to visualize biochemical transformations in living systems, including the human body.

Positron decay is at the heart of PET. When a positron emitter decays, it results in the production of two high-energy (511 KeV) annihilation photons emitted at approxi

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

mately 180 degrees. These are energetic enough to penetrate the body barrier, and they can be imaged using coincident detection. Prototype PET scanners were developed in the early 1960s. By the early 1970s, radiotracer chemistry and PET instrumentation were growing hand in hand with new discoveries in one area, stimulating growth in the other. Al realized that advances in chemistry would be a major driving force in the field, and this drove him to solidify radiotracer chemistry in its own right. He along with several other leading chemists from around the world (Monte Blau, Yves Cohen, Dominique Comar, W. Maier-Borst, Aldo E. A. Mitta, Tadashi Nozaki, Gerhard Stöcklin, Michael J. Welch, and David Silvester) established the International Symposium for Radiopharmaceutical Chemistry for chemists to come together and grapple with the unusual problems of working with very short-lived isotopes at a sub-micromolar reaction scale. The first symposium was held at Brookhaven in 1976; the meeting, held every two years since, has grown in size and breadth, with an increasing emphasis on understanding and probing the interactions between chemical compounds and living systems, while focusing on the central science, chemistry.

Al had a passion for knowledge. He believed in the power of the tracer method and he developed the tools to apply it in the human body. At the same time he set the standards for PET research throughout the world. Nowhere is his vision and leadership more powerfully illustrated than in his role in the development of ^{18}FDG in 1976. This was a remarkable collaboration between Al and his group and David Kuhl, Martin Reivich, and Louis Sokoloff. It began in 1973 to develop a method for measuring brain glucose metabolism in the living human. Al successfully led the effort to synthesize 2-deoxy-2-[^{18}F]fluoro-D-glucose (^{18} FDG). He coined the term “metabolic trapping” as a principle of radiotracer

Copyright © National Academy of Sciences. All rights reserved.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

design to describe the trapping of ^{18} FDG -6-phosphate in cells as a marker of glucose metabolism in a paper published in 1978 (*Journal of Nuclear Medicine* 19:1154-1161). The first synthesis of ^{18}FDG was carried out at Brookhaven in 1976 by Tatsuo Ido. It took one half-life (110 minutes), and the product was flown by private plane to the University of Pennsylvania, where the first brain and body images were made on the Mark II scanner developed by David Kuhl. This was the very first time that brain glucose metabolism was mapped in the living human. This is well recognized to have been a watershed in the current worldwide growth of PET for basic and clinical research and diagnosis. Al went on to push the use of ^{18}FDG and other radiotracers in neurology and in psychiatry (with New York University colleagues Jonathan Brodie, Tibor Farkas, and Mony DeLeon). The fruits of his efforts abound, including an internationally recognized research program in imaging in substance abuse research at Brookhaven.

Al derived great satisfaction from teaching, and for 30 years (1953-83) he taught organic chemistry at night at the Columbia University School of General Studies. Though this involved a long trek from Brookhaven, which is about 60 miles east of New York City, the environment and energy of the city and his strong rapport with students were powerful reinforcers. Though he bemoaned the fact that a majority of the students were pre-med candidates, when bright students were challenged to pursue chemistry, Al happily declared victory. He also enjoyed his group at Brookhaven, holding wine tastings, walking tours of New York City, and ski trips to Vermont. He was an extraordinary travel guide, and it was not unusual for members of the Chemistry Department to seek his travel advice and to use his extensive collection of maps of cities throughout the world.

Al's favorite city was Rome, and he had a passion for all

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

things Italian. In 1969, when he began consulting for the National Research Council of Italy, he learned Italian and on more that one occasion prepared pasta and espresso for the group in the chemistry laboratory at Brookhaven. Though Al loved cities, he also appreciated the wilderness, including hiking, rock climbing in California, and camping in the Adirondacks. He never compromised on food, however, and carried fresh eggs packed in a special padded egg case on his mountain treks. No matter where he was in the world, Al could be counted on to seek out the finest restaurants, museums, music, ballet, or architecture.

Not surprisingly, Al Wolf received many honors during his long career at Brookhaven. He was a member of the National Academy of Sciences (elected in 1988). He received the Nuclear Chemistry Award of the American Chemical Society (1971), the Society of Nuclear Medicine Paul Aebersold Award (1981), the Hevesy Nuclear Medicine Pioneer Award (1991), and the Melvin Calvin Award of the International Isotope Society (1997), to name a few. He chaired the Chemistry Department at Brookhaven from 1983 to 1987. During his long career, he published over 325 papers on basic and applied research in chemistry and nuclear medicine.

Al was not only a strong scientific leader and a mentor to many; he was also our friend. Those of us who knew him well also appreciated his passion, his drive, and his willingness to stand up and fight for what he believed. We could not imagine a stronger or a more articulate ally. Though he left us with much new knowledge, more importantly, he left us with the tools to grow more knowledge. He stimulated and inspired the dozens of scientists around the world who worked with him over the years. Indeed most of the world's cyclotron-PET centers have one or more individuals who,

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

to their great advantage, spent part of their careers at Brookhaven working with Al Wolf.

Al died on December 17, 1998, after a long illness. His wife, Elizabeth (Helga), died in April 1998. He is survived by his son, Roger, an architect living in Santa Monica, California, and two granddaughters, Allison and Madeline. He is sorely missed by his friends throughout the world.

THE AUTHORS ARE GRATEFUL to David Schlyer, Richard Ferrieri, Carol Redvanly, and Nora Volkow for their help in preparing this memoir.

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

SELECTED BIBLIOGRAPHY

1955

With C. S. Redvanly and R. C. Anderson. Benzene-$^{14}{}_{1}$C from the neutron irradiation of the clathrate with ammoniacal nickel cyanide. *Nature* 176: 831.

1958

With B. Suryanarayana. Chemical effects of the nuclear transformation, $C^{12}(n,2n)C^{11}$, in benzene: Influence of phase, temperature and radical scavengers. *J. Phys. Chem.* 62: 1369.

1962

With F. Cacace. The effect of radiation on the reactions of recoil carbon-11 in ammonia. *J. Am. Chem. Soc.*84: 3202.

1963

With G. Stöcklin. Phase dependence of carbon-11 recoil products in ethane and propane: Evidence for methylene insertion. *J. Am. Chem. Soc.*85: 229.

1966

With H. J. Ache. Bond energy effects and acetylene production in the reactions of energetic carbon atoms with alkyl halides and propane. *J. Am. Chem. Soc.*88: 888.

1968

With M. J. Welch. Reaction intermediates in the chemistry of recoil carbon atoms. *Chem. Commun.*3: 117-18.

1970

With P. B. Shevlin. On the probable intermediacy of tetrahedrane. *J. Am. Chem. Soc.*92: 406-408.

With D. R. Christman and R. M. Hoyte. Organic radiopharmaceuticals labeled with isotopes of short half life. I. Dopaminehydrochloride-1-^{11}C. *J. Nucl. Med.*11: 474-78.

With E. Y. Y. Lam, P. Gaspar, and A. P. Wolf. States of atomic

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

carbon produced in decomposition of organic compounds in a microwave plasma. *J. Phys. Chem.*75: 445-47.

1972

With D. R. Christman, E. J. Crawford, and M. Friedkin. A new means of detecting DNA synthesis in intact organisms with positron-emitting [methyl-^{11}C] thymidine. *Proc. Natl. Acad. Sci. U. S. A.*69: 988-92.

1975

With D. R. Christman, R. D. Finn, and K. I. Karlstrom. The production of ultra high activity ^{11}C-labeled hydrogen cyanide, carbon dioxide, carbon monoxide, and methane via the ^{14}N (p,a) ^{11}C reaction. XV. *Int. J. Appl. Radiat. Isot.*26: 435-42.

1978

With T. Ido, C.-N. Wan, V. Casella, J. S. Fowler, M. Reivich, and D. E. Kuhl. Labeled 2-deoxy-D-glucose analogs. ^{18}F-labeled 2-deoxy-2-fluoro-D-glucose, 2-deoxy-2-fluoro-D-mannose and ^{14}C-2-deoxy-2-fluoro-D-glucose. *J. Label. Compd. Radiopharm.*14: 175-84.

1979

With others. The (^{18}F) fluorodeoxyglucose method for the measurement of local cerebral glucose utilization in man. *Circ. Res.*44: 127-37.

With T. J. Ruth. Absolute cross sections for the production of ^{18}F via the ^{18}O(p,n) ^{18}F reaction. *Radiochim. Acta*26: 21-24.

With P. Schueler, R. P. Pettijohn, K.-C. To, and E. P. Rack. Evidence of Walden inversion in high energy chlorine-for-chlorine substitution reactions. *J. Phys. Chem.*83: 1237-41.

1980

With others. A fluorinated glucose analog, 2-fluoro-2-deoxy-D-glucose (F-18): Nontoxic tracer for rapid tumor detection. *J. Nucl. Med.*21: 670-75.

With others. The application of [^{18}F]-2-deoxy-2-fluoro-D-glucose and positron emission tomography in the study of psychiatric conditions. In *Cerebral Metabolism and Neural Function,*J. V. Passonneau, R. A. Hawkins, W. D. Lust, and F. A. Welch, eds., pp. 403-408. Baltimore, Md.: Williams and Wilkins

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

With F. Cacace and P. Giacomello. Substrate selectivity and orientation in aromatic substitution by molecular fluorine. *J. Am. Chem. Soc.*102: 3511-55.

1983

With M. Attina and F. Cacace. Labeled aryl fluorides from the nucleophilic displacement of activated nitro groups by ^{18}F-F^{-}. *J. Label. Compd. Radiopharm.*20: 501-14.

1987

With others. Mapping human brain monoamine oxidase A and B with ^{11}C-suicide inactivators and positron emission tomography. *Science*235: 481-85.

With M. L. Firouzbakht, R. A. Ferrieri, and E. P. Rack. Stereochemical consequences of thermal F-for-Cl atomic substitution with 2(S)-(+)-chloropropionyl chloride. *J. Am. Chem. Soc.*109: 2213-14.

1988

With others. Serial [^{18} F]-N-methylspiroperidol PET studies to measure changes in antipsychotic drug D_2 receptor occupancy in schizophrenic patients. *Biol. Psychiatry*23: 653-63.

1989

With others. Mapping cocaine binding in human and baboon brain in vivo. *Synapse*4: 371-77.

1990

With others. Positron emission tomography (PET) studies of dopaminergic/cholinergic interactions in the baboon brain. *Synapse*6: 321-27.

With Y.-S. Ding, C.-Y. Shiue, J. S. Fowler, and A. Plenevaux. No-carrier-added (NCA) aryl[^{18}F] fluorides via the nucleophilic aromatic substitution of electron rich aromatic rings. *J. Fluorine Chem.*48: 189-205.

With others. Effects of chronic cocaine abuse on postsynaptic dopamine receptors. *Am. J. Psychiatry*147: 719-24.

Copyright © National Academy of Sciences. All rights reserved.

Biographical Memoirs V.78
http://www.nap.edu/catalog/9977.html

About this PDF file: This new digital representation of the original work has been recomposed from XML files created from the original paper book, not from the original typesetting files. Page breaks are true to the original; line lengths, word breaks, heading styles, and other typesetting-specific formatting, however, cannot be retained, and some typographic errors may have been accidentally inserted. Please use the print version of this publication as the authoritative version for attribution.

1993

With M. L. Firouzbakht, D. J. Schlyer, and S. J. Gatley. A cryogenic solid target for the production of [^{18} F]fluoride from enriched [^{18} O] carbon dioxide. *Int. J. Appl. Radiat. Isot.*44(8): 1081-84.

1995

With D. L. Alexoff, C. Shea, J. S. Fowler, P. King, S. J. Gatley, and D. J. Schlyer. Plasma input function determination for PET using a commercial laboratory robot. *Nucl. Med. Biol.*22 (7): 893-904.

1996

With others. Inhibition of monoamine oxidase B in the brains of smokers. *Nature*379: 733-36.

1997

With J. S. Fowler. Working against time: Rapid radiotracer synthesis and imaging the human brain. *Acct. Chem. Res.*30: 181-88.

Copyright © National Academy of Sciences. All rights reserved.