SPECTROSCOPY **WITH COHERENT** **RADIATION**

World Scientific Series in 20th Century Physics

Published

- Vol. 1 Gauge Theories — Past and Future
edited by R. Akhoury, B. de Wit, and P. van Nieuwenhuizen
- Vol. 2 Scientific Highlights in Memory of Léon van Hove
edited by F. Nicodemi
- Vol. 3 Selected Papers, with Commentary, of T. H. R. Skyrme
edited by G. E. Brown
- Vol. 4 Salamfestschrift
edited by A. Ali, J. Ellis and S. Randjbar-Daemi
- Vol. 5 Selected Papers of Abdus Salam (with Commentary)
edited by A. Ali, C. Isham, T. Kibble and Riazuddin
- Vol. 6 Research on Particle Imaging Detectors
edited by G. Charpak
- Vol. 7 A Career in Theoretical Physics
edited by P. W. Anderson
- Vol. 8 Lepton Physics at CERN and Frascati
edited by N. Cabibbo
- Vol. 9 Quantum Mechanics, High Energy Physics and Accelerators: Selected Papers of J. S. Bell (with Commentary)
edited by M. Bell, K. Gottfried and M. Veltman
- Vol. 10 How We Learn; How We Remember: Toward an Understanding of Brain and Neural Systems — Selected Papers of Leon N. Cooper
edited by L. N. Cooper
- Vol. 11 30 Years of the Landau Institute — Selected Papers
edited by I. M. Khalatnikov and V. P. Mineev
- Vol. 12 Sir Nevill Mott — 65 Years in Physics
edited by N. Mott and A. S. Alexandrov
- Vol. 13 Broken Symmetry — Selected Papers of Y. Nambu
edited by T. Eguchi and K. Nishijima
- Vol. 14 Reflections on Experimental Science
edited by M. L. Perl
- Vol. 15 Encounters in Magnetic Resonances — Selected Papers of Nicolaas Bloembergen (with Commentary)
edited by N. Bloembergen
- Vol. 16 Encounters in Nonlinear Optics — Selected Papers of Nicolaas Bloembergen (with Commentary)
edited by N. Bloembergen
- Vol. 17 The Collected Works of Lars Onsager (with Commentary)
edited by P. C. Hemmer, H. Holden and S. K. Ratkje
- Vol. 18 Selected Works of Hans A. Bethe (with Commentary)
edited by Hans A. Bethe
- Vol. 19 Selected Scientific Papers of Sir Rudolf Peierls (with Commentary)
edited by R. H. Dalitz and R. Peierls
- Vol. 20 The Origin of the Third Family
edited by O. Barnabei, L. Maiani, R. A. Ricci and F. R. Monaco

World Scientific Series in 20th Century Physics – Vol. 21

SPECTROSCOPY WITH COHERENT RADIATION

Selected Papers of Norman F Ramsey
With Commentary

Norman F Ramsey

Harvard University

World Scientific

Singapore • New Jersey • London • Hong Kong

Published by

World Scientific Publishing Co. Pte. Ltd.

P O Box 128, Farrer Road, Singapore 912805

USA office: Suite 1B, 1060 Main Street, River Edge, NJ 07661

UK office: 57 Shelton Street, Covent Garden, London WC2H 9HE

Library of Congress Cataloging-in-Publication Data

Ramsey, Norman, 1915–

Spectroscopy with coherent radiation : selected papers of Norman

F. Ramsey with commentary / Norman F. Ramsey.

p. cm. -- (World scientific series in 20th century physics ; vol. 21)

Includes bibliographical references.

ISBN 9810232500

1. Radiofrequency spectroscopy. 2. Microwave spectroscopy.

3. Quantum electronics. I. Title. II. Series.

QC454.R3R36 1998

537.5'34--dc21

98-16290

CIP

British Library Cataloguing-in-Publication Data

A catalogue record for this book is available from the British Library.

The author and the publisher would like to thank the following organisations and publishers for their assistance and their permission to reproduce the articles found in this volume:

Academic Press (*Recent Research in Molecular Beams*), American Association for the Advancement of Science (*Science*), American Institute of Physics (*J. Chem. Phys.*, *Rev. Sci. Instrum.*), American Physical Society (*Phys. Rev.*, *Phys. Rev. Lett.*, *Rev. Mod. Phys.*), Elsevier Science Publishers B. V. (*Physica*, *Phys. Lett.*), Institute of Electrical & Electronics Engineers, Inc. (*IEEE J. Quantum Electron.*), Nobel Foundation, Royal Swedish Academy of Sciences (*Physica Scripta*), Springer-Verlag (*Appl. Phys.*)

Copyright © 1998 by World Scientific Publishing Co. Pte. Ltd.

All rights reserved. This book, or parts thereof, may not be reproduced in any form or by any means, electronic or mechanical, including photocopying, recording or any information storage and retrieval system now known or to be invented, without written permission from the Publisher.

For photocopying of material in this volume, please pay a copying fee through the Copyright Clearance Center, Inc., 222 Rosewood Drive, Danvers, MA 01923, USA. In this case permission to photocopy is not required from the publisher.

Printed in Singapore.

CONTENTS

Preface	xiii
Autobiographical Notes	xv
Section 1. Methods for Spectroscopy with Coherent Radiation and Atomic Clocks	1
<i>Paper 1.1</i>	3
“The Magnetic Moments of the Proton and the Deuteron: The Radiofrequency Spectrum of H ₂ in Various Magnetic Fields,” J. M. B. Kellogg, I. I. Rabi, N. F. Ramsey and J. R. Zacharias, <i>Phys. Rev.</i> 56 , 728–742 (1939)	4
<i>Paper 1.2</i>	20
“A Molecular Beam Resonance Method with Separated Oscillating Fields,” N. F. Ramsey, <i>Phys. Rev.</i> 78 , 695–699 (1950)	21
<i>Paper 1.3</i>	26
“Phase Shifts in the Molecular Beam Method of Separated Oscillating Fields,” N. F. Ramsey and H. B. Silsbee, <i>Phys. Rev.</i> 84 , 506–507 (1951)	27
<i>Paper 1.4</i>	29
“Nuclear Audiofrequency Spectroscopy by Resonant Heating of the Nuclear Spin System,” N. F. Ramsey and R. V. Pound, <i>Phys. Rev.</i> 81 , 278–279 (1951)	30
<i>Paper 1.5</i>	32
“Use of Rotating Coordinates in Magnetic Resonance Problems,” I. I. Rabi, N. F. Ramsey and J. Schwinger, <i>Rev. Mod. Phys.</i> 26 , 167–171 (1954)	33
<i>Paper 1.6</i>	38
“Resonance Transitions Induced by Perturbations at Two or More Different Frequencies,” N. F. Ramsey, <i>Phys. Rev.</i> 100 , 1191–1194 (1955)	39
<i>Paper 1.7</i>	43
“Resonance Experiments in Successive Oscillatory Fields,” N. F. Ramsey, <i>Rev. Sci. Instrum.</i> 28 , 57 (1957)	44

Paper 1.8	45
“Molecular Beam Resonances in Oscillatory Fields of Nonuniform Amplitudes and Phases,” N. F. Ramsey, <i>Phys. Rev.</i> 109 , 822–825 (1958)	46
Paper 1.9	50
“Shapes of Molecular Beam Resonances,” Norman F. Ramsey, <i>Recent Research in Molecular Beams</i> A29 , 107–119, Academic Press, New York (1959)	51
Paper 1.10	64
“Atomic Hydrogen Maser,” H. M. Goldenberg, D. Kleppner and N. F. Ramsey, <i>Phys. Rev. Lett.</i> 8 , 361–362 (1960)	65
Paper 1.11	67
“Theory of the Hydrogen Maser,” D. Kleppner, H. M. Goldenberg and N. F. Ramsey, <i>Phys. Rev.</i> 126 , 603–615 (1962)	68
Paper 1.12	81
“Large Storage Box Hydrogen Maser,” E. E. Uzgiris and N. F. Ramsey, <i>IEEE J. Quantum Electronics</i> QE-4 , 563–568 (1968)	82
Paper 1.13	88
“Atomic Deuterium Maser,” D. J. Wineland and N. F. Ramsey, <i>Phys. Rev.</i> A5 , 821–837 (1972)	89
Paper 1.14	106
“Precise Measurement of Time,” N. F. Ramsey, <i>American Scientist</i> 76 , 42–49 (1988)	107
Paper 1.15	115
“Experiments With Separated Oscillatory Fields and Hydrogen Masers” (Nobel Lecture), N. F. Ramsey, <i>Les Prix Nobel</i> (1989, The Nobel Foundation) and <i>Rev. Mod. Phys.</i> 62 , 541–552 (1990)	116
Paper 1.16	128
“Successive Oscillatory Fields at Radio to Optical Frequencies,” N. F. Ramsey, <i>Appl. Phys.</i> B60 , 85–88 (1995)	129

Section 2. Radiofrequency and Microwave Spectroscopy Experiments	133
Paper 2.1	135
“An Electrical Quadrupole Moment of the Deuteron: The Radiofrequency Spectra of HD and D ₂ Molecules in a Magnetic Field,” J. M. B. Kellogg, I. I. Rabi, N. F. Ramsey and J. R. Zacharias, <i>Phys. Rev.</i> 57 , 677–695 (1940)	136
Paper 2.2	155
“The Rotational Magnetic Moments of H ₂ , D ₂ , and HD Molecules,” N. F. Ramsey, <i>Phys. Rev.</i> 58 , 226–236 (1940)	156
Paper 2.3	167
“Measurements of Nuclear Quadrupole Moment Interactions,” W. A. Nierenberg, N. F. Ramsey and S. B. Brody, <i>Phys. Rev.</i> 70 , 773–775 (1946)	168
Paper 2.4	170
“Nuclear Radiofrequency Spectra of H ₂ and D ₂ in High and Low Magnetic Fields,” H. G. Kolsky, T. E. Phipps, H. B. Silsbee and N. F. Ramsey, <i>Phys. Rev.</i> 87 , 395–403 (1952)	171
Paper 2.5	180
“Radiofrequency Spectra of Hydrogen Deuteride in Strong Magnetic Fields,” W. E. Quinn, J. M. Baker, J. T. LaTourrette and N. F. Ramsey, <i>Phys. Rev.</i> 112 , 1929–1940 (1958)	181
Paper 2.6	193
“Nuclear Interactions in Deuterium Fluoride,” H. M. Nelson, J. A. Leavitt, M. R. Baker and N. F. Ramsey, <i>Phys. Rev.</i> 122 , 856–859 (1961)	194
Paper 2.7	198
“Rotational Magnetic Moments of Lithium Hydride and Deuteride,” T. R. Lawrence, C. H. Anderson and N. F. Ramsey, <i>Phys. Rev.</i> 130 , 1865–1870 (1963)	199
Paper 2.8	205
“Hyperfine Separation of Ground-State Atomic Hydrogen,” S. B. Crampton, D. Kleppner and N. F. Ramsey, <i>Phys. Rev. Lett.</i> 11 , 338–340 (1963)	206

Paper 2.9	209
“Stark Shift of the Hydrogen Hyperfine Separation,” E. N. Fortson, D. Kleppner and N. F. Ramsey, <i>Phys. Rev. Lett.</i> 13 , 22–23 (1964)	210
Paper 2.10	212
“Rotational Magnetic Moments,” J. W. Cederberg, C. H. Anderson and N. F. Ramsey, <i>Phys. Rev.</i> 136 , A960–A961 (1964)	213
Paper 2.11	215
“Absolute Value of the Proton g Factor,” T. Myint, D. Kleppner, N. F. Ramsey and H. G. Robinson, <i>Phys. Rev. Lett.</i> 17 , 405–407 (1966)	216
Paper 2.12	219
“Hyperfine Separation of Tritium,” B. S. Mathur, S. B. Crampton, D. Kleppner and N. F. Ramsey, <i>Phys. Rev.</i> 158 , 14–17 (1967)	220
Paper 2.13	224
“Measurements of the Hydrogen-Deuterium Atomic Magnetic Moment Ratio and of the Deuterium Hyperfine Frequency,” D. J. Larson, P. A. Valberg and N. F. Ramsey, <i>Phys. Rev. Lett.</i> 23 , 1369–1371 (1969)	225
Paper 2.14	229
“Direct Observation of Ortho–Para Transitions in Methane,” I. Ozier, P. N. Yi, A. Khosla and N. F. Ramsey, <i>Phys. Rev. Lett.</i> 24 , 642–646 (1970)	230
Paper 2.15	235
“Hyperfine Structure of $^7\text{Li}^{79,81}\text{Br}$ by Molecular-Beam Electric Resonance,” R. C. Hilborn, T. F. Gallagher, Jr. and N. F. Ramsey, <i>J. Chem. Phys.</i> 56 , 855–861 (1972)	236
Paper 2.16	243
“Molecular-Beam Magnetic-Resonance Measurements of the Anisotropies of the Electric Polarizabilities of H_2 and D_2 ,” K. B. MacAdam and N. F. Ramsey, <i>Phys. Rev.</i> A6 , 898–906 (1972)	244
Paper 2.17	253
“The Molecular Zeeman and Hyperfine Spectra of LiH and LiD by Molecular Beam High Resolution Electric Resonance,” R. R. Freeman, A. R. Jacobson, D. W. Johnson and N. F. Ramsey, <i>J. Chem. Phys.</i> 63 , 2597–2602 (1975)	254

Paper 2.18	260
“Rotational Magnetic Moment Spectra of a Series of Tetrahedral Molecules,” I. Ozier, S. S. Lee and N. F. Ramsey, <i>J. Chem. Phys.</i> 65 , 3985–3993 (1976)	261
Paper 2.19	270
“Measurement of the Neutron Magnetic Moment,” G. L. Green, N. F. Ramsey, W. Mampe, J. M. Pendlebury, K. Smith, W. B. Dress, P. D. Miller and P. Perrin, <i>Phys. Rev. D20</i> , 2139–2153 (1979)	271
Section 3. Parity, Time Reversal Symmetry and Electric Dipole Moments	287
Paper 3.1	289
“On the Possibility of Electric Dipole Moments for Elementary Particles and Nuclei,” E. M. Purcell and N. F. Ramsey, <i>Phys. Rev.</i> 78 , 807 (1950)	290
Paper 3.2	291
“Experimental Limit to the Electric Dipole Moment of the Neutron,” J. H. Smith, E. M. Purcell and N. F. Ramsey, <i>Phys. Rev.</i> 108 , 120–122 (1957)	292
Paper 3.3	295
“A Search for the Electric Dipole Moment of the Neutron,” K. F. Smith, J. M. Pendlebury, K. Green, B. R. Heckel, N. F. Ramsey, S. Lamoreaux, W. Mampe, <i>et al.</i> , <i>Phys. Lett. B234</i> , 191–196 (1990)	296
Paper 3.4	302
“Time Reversal, Charge Conjugation, Magnetic Pole Conjugation, and Parity,” N. F. Ramsey, <i>Phys. Rev.</i> 109 , 225 (1958)	303
Paper 3.5	304
“First Measurement of Parity-Nonconserving Neutron-Spin Rotation: The Tin Isotopes,” M. Forte, B. R. Heckel, N. F. Ramsey, K. Green, G. L. Greene, J. Byrne and J. M. Pendlebury, <i>Phys. Rev. Lett.</i> 45 , 2088–2092 (1980)	305
Paper 3.6	310
“Search for <i>P</i> and <i>T</i> Violations in the Hyperfine Structure of Thallium Fluoride,” D. A. Wilkening, N. F. Ramsey and D. J. Larson, <i>Phys. Rev.</i> A29 , 425–438 (1984)	311

Paper 3.7	325
“Electric Dipole Tests of Time Reversal Symmetry,” N. F. Ramsey, <i>At. Phys.</i> 14 , 3–17 (1995) (AIP Conf., Proc., 323)	326
 Section 4. Theories of Nuclear Magnetic Shielding and NMR Chemical Shifts	 341
Paper 4.1	343
“Magnetic Shielding of Nuclei in Molecules,” N. F. Ramsey, <i>Phys. Rev.</i> 78 , 699–703 (1950)	344
Paper 4.2	349
“Dependence of Magnetic Shielding of Nuclei upon Molecular Orientation,” N. F. Ramsey, <i>Phys. Rev.</i> 83 , 540–541 (1951)	350
Paper 4.3	352
“Temperature Dependent Magnetic Shielding in Ethyl Alcohol,” U. Liddel and N. F. Ramsey, <i>J. Chem. Phys.</i> 19 , 1608 (1951)	353
Paper 4.4	354
“Chemical Effects in Nuclear Magnetic Resonance and in Diamagnetic Susceptibility,” N. F. Ramsey, <i>Phys. Rev.</i> 86 , 243–246 (1952)	355
Paper 4.5	359
“Nuclear Magnetic Anti-shielding of Nuclei in Molecules: Magnetic Moments of F^{19} , N^{14} , and N^{15} ,” M. R. Baker, C. H. Anderson and N. F. Ramsey, <i>Phys. Rev.</i> 133 , A1533–A1536 (1964)	360
 Section 5. Theories of Nuclear Interactions in Molecules	 365
Paper 5.1	367
“Theory of Molecular Hydrogen and Deuterium in Magnetic Fields,” N. F. Ramsey, <i>Phys. Rev.</i> 85 , 60–65 (1952)	368
Paper 5.2	374
“Vibrational and Centrifugal Effects on Nuclear Interactions and Rotational Moments in Molecules,” N. F. Ramsey, <i>Phys. Rev.</i> 87 , 1075–1079 (1952)	375

Paper 5.3	380
“Electron Distribution in Molecular Hydrogen,” N. F. Ramsey, <i>Science</i> 117 , 470 (1953)	381
Paper 5.4	382
“Spin Interactions of Accelerated Nuclei in Molecules,” N. F. Ramsey, <i>Phys. Rev.</i> 90 , 232–233 (1953)	383
Paper 5.5	385
“Interactions Between Nuclear Spins in Molecules,” N. F. Ramsey and E. M. Purcell, <i>Phys. Rev.</i> 85 , 143 (1952)	386
Section 6. General Principles and Theories	387
Paper 6.1	389
“Thermodynamics and Statistical Mechanics at Negative Absolute Temperatures,” N. F. Ramsey, <i>Phys. Rev.</i> 103 , 20–28 (1956)	390
Paper 6.2	399
“Significance of Potentials in Quantum Theory,” W. H. Furry and N. F. Ramsey, <i>Phys. Rev.</i> 118 , 623–626 (1960)	400
Paper 6.3	404
“The Tensor Force Between Two Protons at Long Range,” N. F. Ramsey, <i>Physica</i> 96A , 285–289 (1979)	405
Paper 6.4	410
“Quantum Mechanics and Precision Measurements,” N. F. Ramsey, <i>Physica Scripta</i> T59 , 26–28 (1995)	411
Paper 6.5	414
“Complementarity with Neutron Two-Path Interferences and Separated-Oscillatory-Field Resonances,” N. F. Ramsey, <i>Phys. Rev.</i> A48 , 80–82 (1993)	415

This page is intentionally left blank

PREFACE

When I was invited by World Scientific to add to their excellent *Series in 20th Century Physics*, I was pleased to be able to present my papers in the field of coherent radiation spectroscopy in a coordinated fashion. Having published more than 400 papers ranging from particle physics to resonance spectroscopy, I have been highly selective and have limited the selections to a dominant research area. The *Series* format is favorable: the autobiographical section summarizes my research in all fields, the reprints include my principal papers in resonance spectroscopy and the commentaries provide historical insight and additional references.

The title *Spectroscopy with Coherent Radiation* is appropriate because the field was almost nonexistent in 1937, when I began work with I. I. Rabi developing the then new magnetic resonance method and using it to obtain the first multiple line radiofrequency spectrum and to discover the deuteron electric quadrupole moment. Later papers include my invention of the methods of coherent separated oscillatory fields, the development of the atomic hydrogen maser, and uses of these methods to measure properties of nucleons, nuclei, atoms and molecules and to test parity and time reversal symmetries. My general theoretical papers arising from this work include the first successful theories of nuclear magnetic shielding, NMR chemical shifts, electron-coupled spin-spin interactions and negative absolute temperatures.

A number of my papers are coauthored with former graduate students and other collaborators. I wish to thank them and the many other friends and colleagues who have contributed to this research with both suggestions and stimulating conversations. I am also grateful to the journal publishers who have permitted the reproduction of these papers. Finally, I wish especially to thank my wife, Ellie W. Ramsey, for her constructive criticism and her editorial assistance.

This page is intentionally left blank

Autobiographical Notes

1. *Early Life (1915–1935)*

I was born on August 27, 1915, in Washington, D.C. My mother, the daughter of German immigrants, had been a mathematics instructor at the University of Kansas. My father, descended from refugees of the Scottish clan wars, began his military career as a 16-year-old volunteer in the Spanish–American War and later went on to West Point to become an officer in the Army Ordnance Corps. His frequently changing assignments took us from Washington, D.C., to Topeka, Kansas; to Paris, France; to Picatinny Arsenal near Dover, New Jersey; to Fort Leavenworth, Kansas; to Governor’s Island in New York City; and to Rock Island Arsenal, Illinois, where he was Commanding General during World War II and retired soon thereafter.

The death from pneumonia of my older brother John when he was ten and I six was a traumatic childhood experience. From that time on my mother wore black clothing, taught a large enthusiastic Bible class, and worried about me to the point of being excessively protective. At the time these actions embarrassed and annoyed me, but in retrospect I feel they did no harm. Although my parents were strict disciplinarians, often giving spankings, I realized the punishments were out of concern for my welfare, and not anger. My parents were supportive of my work in school, but were particularly concerned that I be well-behaved and a hard worker. They argued that it wasn’t my fault if I were stupid, but it was my fault if I did not work hard.

When I was between the ages of six and ten, we were at Picatinny Arsenal, near Dover, New Jersey. Since it was a high explosives manufacturing arsenal with few families, I commuted five miles by bus to the Dover public school. Only six or seven children under twelve lived within a radius of two miles from our house; not enough for one — much less two — baseball or football team. As a result, even now I am awkward at throwing a ball. On the other hand, there was a tennis court behind our house and a nine-hole golf course in front. I became a very good ten-year-old golfer and quite a good tennis player.

Reading *Popular Mechanics* and whatever science books came my way stimulated an early interest in science. I clearly remember, when ten, reading a fascinating chapter on the theory of the atom in one of my father’s chemical engineering texts.

Around 1922, before skiing was a popular U.S. sport, I read about it in *Popular Mechanics* and made skis from two barrel staves which functioned more like snow-shoes. The next year I persuaded my parents to give me a pair of skis, but these had only toe strap bindings and were without ski poles, which my parents considered too dangerous. Despite these initial restrictions, I loved skiing and have been an enthusiast to this day.

An exciting childhood episode was a series of huge explosions during a fire started by lightning in the neighboring Lake Denmark Ammunition Depot. The largest single explosion was 600 tons of navy depth bombs — larger than any previous explosion in the U.S. Our home, about half a mile away, was damaged and my father, though injured by flying glass, led rescue work, for which he received the Soldier’s Medal. I still have a vivid memory of the red hot steel girders, in burning magazines, bending under their own weight. For a few weeks we stayed with friends in Dover,

since smaller explosions continued in neighboring magazines for more than a week.

Just as I turned 11, my father was transferred to Fort Leavenworth, Kansas, which housed the Army Command and General Staff School, where he was a student for one year and an instructor for four years. It differed from Picatinny in many ways, having more officers' families, a local school up to the eighth grade and a three-mile commute to Leavenworth Senior High School. Although there was a beautiful golf course at Fort Leavenworth, no one under 18 was allowed to play. Thus my golfing career came to an abrupt end, never to be resumed.

In two of our moves, I skipped a school grade and consequently was two years younger than my friends. Although some argue this hinders development, I believe it did not in my case. I was tall for my age, so my friends were usually unaware of our age difference, but age did blight my potential career as a Boy Scout, for which there then was a strict 12-year age requirement; by the time I was eligible I was uninterested. My age saved me from many teenage temptations; I tried smoking a cigar when six and had so much difficulty concealing my nausea from my parents that I never tried again. In the long run I clearly benefited from my two skipped years: I received one of the last physics Ph.D.'s before World War II. Otherwise I would have had one of the first after the War — a difference of six or more years.

I remember two outstanding high school teachers. Miss Landis was a strict disciplinarian but she understood and loved math and knew how to teach it. Dr. Wiegand was also the city chemist and spent much of his time in his lab, leaving the class alone to read or solve problems, but for me he was great since he understood both chemistry and physics and willingly let his better students experiment on their own. Despite my age, I did well in high school and was elected Senior Class President.

My parents presumed that I would follow my father's footsteps to West Point, but I graduated from high school at 15, much too young to enter. Soon thereafter, we moved to Governor's Island in New York Harbor, so I applied to Columbia College. The term had started two weeks earlier, but I was admitted immediately.

There I particularly enjoyed the course in Contemporary Civilization with Professor Harry Carman, who later became dean. He often invited students to dinner for discussions on world, national and personal problems. Not realizing as a freshman that physics could be a profession, I started as an engineering concentrator, but I wanted a deeper understanding of nature than was then expected of engineers, whose courses emphasized rules and tables; so I shifted to mathematics. My undergraduate physics courses were generally good, but mostly on pre-quantum-physics — classical mechanics with George Pegram, electricity and magnetism with Shirley Quimby. There were some excellent physics students, considering the small size of the college and the unpopularity of physics concentrations at that time. They included Herbert Anderson (later Fermi's principal research associate at Chicago), Robert Marshak (an excellent theorist at Rochester and later President of the City University of New York), Henry Primakoff (another excellent theorist later at the University of Pennsylvania) and Arthur Kantrowitz (the inventor of jet sources for molecular beams and subsequently the President of Avco Everett). To compensate for our lack of exposure to relativity and quantum mechanics, we organized an informal physics club to study relativity theory. By winning yearly competitive mathematics contests, I was honored in my senior year with the

mathematics teaching assistantship, normally reserved for a graduate student. I tutored in solid geometry as an assistant to the Dean of Columbia College, Herbert Hawkes, and shared an office with the Department Chairman, Willard Fite, who introduced me to the Appalachian Mountain Club Sunday hikes in the Ramapo mountains near New York City.

The Depression years were distressing, especially in New York City, where many unemployed people lost their savings and their homes and the federal government, at first, did little to help, other than supporting a few public construction projects. Thus, left wing groups who offered the hope of change attracted many student supporters. Although I did not join the Socialist Party, I sympathized with its ideas and went to some of the meetings. A number of friends, especially in the Columbia Social Problems Club, joined the Communist Party, which emphasized the contrast between the 1931 conditions in the U.S.A. and the conditions they hoped would exist in the U.S.S.R. many years later — a comparison that was appealing but misleading. Having known the appeal of the Communists to sensitive and conscientious students in those difficult times, I was later supportive of former Communists who came under vicious attack during the McCarthy era.

Roosevelt's election in 1932 brought an improved capitalistic system with significant government supports, controls and the Social Security Act of 1935. Although Roosevelt was hated by those who felt there should be no government intervention, I believe he did much to save and improve democracy and capitalism.

2. *Cambridge University (1935–1937)*

Upon graduation, I received Columbia's two-year Kellett Fellowship to Cambridge University, England. At Columbia I had been a mathematics major, but at Cambridge I converted to physics. Matthew Fitzsimons, who had received a similar Columbia Fellowship to Oxford, and I traveled by slow boat to Europe and toured in Germany. We were impressed by the beauty of the countryside and cities, but were appalled by the anti-Jewish signs and newspapers — especially by *Der Stürmer*, the official newspaper of the Nazi Storm Troopers, and by the posters "*Juden nicht erwünscht*" on many buildings. After several weeks in cities with their museums and concerts, I left Matthew in the Munich beer halls and hiked alone in the Bavarian Alps: a beautiful trip marred by the customary Bavarian greeting of "*Grüss Got*" being frequently replaced by the Nazi "*Heil Hitler*."

My two years in Cambridge at Clare College were a great experience. The Cavendish Laboratory was an exciting center for physics with a stellar array of physicists including Sir Joseph (J. J.) Thomson, Lord Rutherford, Sir James Chadwick, John Cockcroft, A. S. Eddington, Max Born, R. H. Fowler, Edward Bullard, Maurice Goldhaber, Paul Dirac, P. I. Dee, C. T. R. Wilson and M. Oliphant.

In the United States most advanced physics courses are taken during the first two years of graduate study, while in Cambridge they were part of the undergraduate program. I had the option either to start immediately on research for my Ph.D. or to take the last two years of the undergraduate program including the final tripos exam. Although it seemed a waste of two years to obtain a second B.A., I realized that otherwise I would miss advanced subjects like quantum

mechanics. As it turned out, I lost no time since, on my return to Columbia, I merely took exams in these subjects and finished all of my Ph.D. requirements in just two additional years.

In retrospect, I am particularly delighted to have taken a course from J. J. Thomson. He had retired as the Cavendish Professor but continued as Master of Trinity College and as an occasional lecturer. He seemed to be a very old man, often losing his false teeth during lectures, though he was then 79, younger than I am now. Despite his age and somewhat dull lecture style, J. J. Thomson had astute physical insight and an intense interest in physics. At the start of his research, atomic physics was so undeveloped that he participated in discovering the electron and in making the first model of the atom, the invalid "plum pudding" model. Considering our present state of knowledge, I am astounded at the progress made during the span of just two overlapping scientific careers.

I mark my conversion from mathematics to physics by my reactions to Lord Rutherford in two successive years. The first year I was terribly disappointed. In his lectures on *The Constitution of Matter*, his mathematical derivations were terribly weak and his derivation of the Rutherford scattering formula fell apart so completely that he asked us to work it out ourselves after class. Likewise, in seminars he frequently asked questions that were so naive that even the undergraduates could answer them. I wrote my parents that Lord Rutherford met my expectation of a typical British lord, not a great physicist. But the following summer, in writing an essay for my tutor, I looked up the notes I had dutifully recorded despite my disappointment in Rutherford's course. To my pleasant surprise, I not only found the answers to my questions, but also was so impressed by his physics remarks that I attended the course again and found it excellent, even though he still had trouble in deriving the Rutherford scattering law. I was impressed by his innovative ideas and realized that among Rutherford's naive questions in seminars were a number of original and profound remarks which stimulated much of the Cavendish Laboratory research program.

John Cockcroft gave excellent lectures on classical electricity and magnetism. Max Born, a refugee from Germany, did likewise on classical optics. Dirac's lectures were disappointing, since he was quite shy and essentially read to the class his excellent book *Quantum Mechanics*. Edington's lectures on relativity were the most disappointing; he was the author of a beautifully written treatise, *Mathematical Theory of Relativity*, but in class he hesitantly tried to repeat his book word for word from memory.

Maurice Goldhaber, also a refugee from Germany, was my tutor one term. An article I wrote for him on nuclear spins and magnetic moments stimulated my interest in molecular beams and subsequently my Ph.D., research with I. I. Rabi at Columbia. My study with Maurice began a long friendship which has continued to this day and includes our being colleagues at the University of Illinois, my hiring him and his wife at Brookhaven while I was head of the Physics Department, there, and his being a Trustee of the Universities Research Association while I was President.

The holders of Kellett Fellowships were expected to travel in Europe, but insufficient travel funds were provided. However, in France the cost of living was then so low and the quality of inexpensive meals so high that I could save money by going to France if I went for more than two weeks. I also saved on hotel bills by third class train travel at night. For my first Christmas vacation I went skiing at Mjofjell in Norway near Finse, the high point on the Bergen-to-Oslo railroad. It was ideal for studying and skiing — too dark to ski before 10:00 A. M. or after

2:00 P. M., so I had plenty of time for my physics books. For the following spring vacation, Matthew and I went to Spain, both to see that beautiful country and to warm up after the damp cold of our college rooms in Oxford and Cambridge. Spain, at the time, was politically unstable and showed signs of a threatening civil war, which did indeed start less than a year later. Pairs of police and Civil Guards carrying submachine guns were everywhere. A low point on that trip was running out of money the day before we were to catch a boat back to England and having to explain to heavily armed police in our nonexistent Spanish why we were spending the night sleeping in the park. Another vacation was spent sailing on the beautiful Norfolk Broads with the family of Alex Wood, a Cambridge physics lecturer.

The 1935–37 period in Europe was politically disturbing. Mussolini occupied Ethiopia and the Spanish Civil War started. The Conservative government of Stanley Baldwin was unpopular, as reflected by the oft-repeated words “Before Baldwin England muddled through, now it just muddles.” Winston Churchill, a lone voice warning of the Nazi threat and the need to prepare for war, was dismissed as an archaic old fool by most students, including myself, since the possibility of a real fighting war seemed incredible. The antiwar movement was strong and many of the British students signed the Oxford Pledge, never to fight for King or country; but later, when war came, most of these students did fight and many died. The coronation of Edward was to occur during my second year at Cambridge, but he abdicated in favor of George VI. The Coronation Celebration in Cambridge was a festive town and gown party with concerts and dancing in the streets, as well as a competition to catch a greased pig.

3. *Columbia Graduate School and Early Research (1937–1940)*

(a) *Research with I. I. Rabi; Early Magnetic Resonance Experiments and the Discovery of the Deuteron Electric Quadrupole Moment*

With an honors B.A. from Cambridge University, I returned to Columbia to pass the required exams and to begin my Ph.D. research. I did, however, take one advanced Columbia course, on beta ray theory, with Enrico Fermi, who had joined the Columbia faculty soon after my return. He had brought his partly Jewish family to Stockholm when he received the Nobel Prize and continued to travel westward to Columbia because of the Italian anti-Jewish campaign. His course on beta ray theory was excellent; he had a superb ability to explain and calculate complicated frontier problems in terms of relatively simple quantum mechanics.

When I discussed the possibility of doing research with I. I. Rabi, he was rather discouraged about the future of molecular beam studies, but agreed to let me work with him. He and his associates were measuring nuclear magnetic moments by deflecting beams of neutral atoms with an inhomogeneous magnetic field, but he was pessimistic about the prospects of major improvements. I began as an apprentice to Sidney Millman and Jerrold Zacharias, who were measuring new interactions within the indium atom during what Jerrold called the “indium summer.” Then I worked with Jerome M. B. Kellogg to measure the magnetic properties of neon and argon in a magnetic

deflection experiment. We were just getting started when Rabi invented the molecular beam magnetic resonance method, which dramatically changed the research in our laboratory, and eventually in many laboratories.

For a long time Rabi's laboratory had been reorienting nuclear and atomic spin angular momenta by passing them through a region in which the direction of the magnetic field reversed from one position to another. Later Rabi developed the theory for such reorientations. For simplicity he assumed the field changes were equally spaced so each atom in effect was exposed to a simple oscillating magnetic field. Although his result had a resonance character, it was obscured by the form in which the result was written and totally lost when the results were averaged over the different atomic velocities. Although his paper [*Phys. Rev.* **51**, 652–656 (1937)] provides the theoretical basis for all magnetic resonance research, Rabi was so concerned with fields changing in space that he did not initially recognize the resonance in time. Seven months later, following a visit by C. J. Gorter, who described his published unsuccessful calorimetric experiments using an oscillating field, Rabi invented the molecular beam magnetic resonance method and immediately urged two of his experimental groups to prepare different magnetic resonance experiments.

As was expected, the first resonances were seen by Rabi, J. R. Zacharias, S. Millman and P. Kusch [*Phys. Rev.* **53**, 318–320 (1938)], who were studying easily detectable alkali atoms. Rabi, Kellogg, Zacharias and I were studying the H_2 molecule, which is more interesting but much more difficult to detect. We soon obtained our first H_2 resonance, but were terribly disappointed by its featureless shape; instead of the expected single resonance peak, we found a number of closely spaced overlapping small peaks that were hard to distinguish from noise. We then tried D_2 and HD, and found dominant peaks from which we could determine the nuclear magnetic moments of the proton and deuteron despite some noiselike background.

Columbia then had a rule that each Ph.D. thesis should have a single author; so I was assigned the investigation of the backgrounds and the peculiar H_2 and D_2 resonance patterns since we thought the peculiar shapes were probably apparatus effects that were interesting, but not so fundamental that all of us should share the authorship. While the others in our group planned future apparatus modifications to improve resolution, I continued measurements with the old apparatus, and soon found that we had been using much too strong an oscillatory field. Through a reduction in the oscillatory fields, I found that our confusing pattern became six well-resolved resonances. It became clear that the multiple resonances in H_2 were a radio frequency spectrum of the molecule with the protons interacting magnetically, not only with the external magnetic field, but also with each other and with the magnetic field generated by the rotation of the molecule. Likewise, in weaker oscillatory fields, the ill-defined D_2 and HD backgrounds were suggestive of additional weak resonance peaks. The frequency spread of these peaks in D_2 was even greater than in H_2 , which was surprising, since the nuclear magnetic moments were less and the molecule rotated more slowly so the magnetic interactions should have been less. This suggested that there must be an additional nonmagnetic interaction in the molecule, such as that from an unexpected deuteron electric quadrupole moment.

The four of us then agreed that the structure of the multiple resonances was too important for a single-authored thesis, since all had built the apparatus and discussed the interpretations. So I

switched my thesis to the study of the resonances I had found associated with the rotational magnetic moment of the molecule (Paper 2.2). The four of us, with the improved apparatus, went on to measure accurately the spin-spin magnetic interactions in H_2 and to confirm the existence of the deuteron quadrupole moment interactions in D_2 and HD (Papers 1.1 and 2.1). These experiments were the first instances of multiline spectra being observed with coherent electromagnetic radiation.

My thesis, "Rotational Magnetic Moments of H_2 , D_2 and HD Molecules" (Paper 2.2), became the first of many theses based on nuclear magnetic resonance. My rotational results agreed quite well with those from our nuclear experiments, with the only free parameters being the rotational magnetic moments, but the agreement was not perfect. This discrepancy bothered me; thinking about it while on a train to the University of Illinois, I realized that in the rotational experiments the molecules changed their orientation so there should be an additional contribution due to the differences in molecular diamagnetic susceptibility in the different orientations. When I added a diamagnetic susceptibility term to my theory, the agreement between theory and experiment was excellent. From the results I determined an additional molecular parameter, the dependence of the molecular diamagnetic susceptibility on orientation, and presented this new result at the Illinois colloquium.

There were no federal funds to support research at that time; the Physics Department provided a machinist and Rabi had a grant of about \$10,000 from the Research Corporation. Graduate students were unpaid for doing research, but I earned \$800 as a teaching assistant in my first year and received the same from a Columbia scholarship the next year. Postdocs received no compensation for working in the lab and there was no extra pay for summer work. I later learned that we had been partially supported by a \$3,500 contribution from Harold Urey, the discoverer of deuterium, from his own \$7,600 Carnegie grant — an act of extraordinary generosity.

Rabi was a great thesis supervisor; he had fascinating original ideas on fundamental physics as well as on a variety of other subjects. He encouraged creativity and gave his students and collaborators lots of freedom. He had high standards for the quality of research and for its fundamental interest. His critical remarks about uninteresting research annoyed many scientists, and he was sometimes wrong in his criticisms, but he was usually right; these remarks often stimulated his friends and associates to even higher standards.

I had the good fortune to be associated with other excellent physicists. Zacharias was a professor at Hunter College, where there were no research facilities, so he voluntarily worked in Rabi's lab as an unpaid visiting postdoc, and Millman did the same from Brooklyn. Kellogg, Willis Lamb and Arnold Nordsieck were Columbia instructors. Ugo Fano, from Italy, was a frequent visitor. Herbert Anderson, Donald Hamilton, Julian Schwinger, Nicholas Renzetti and Andrew Lawson were among my student friends. Julian Schwinger, though still an undergraduate, did theoretical work associated with our experiments. Five of the six members of my Ph.D. examining committee eventually won Nobel Prizes — Rabi, Fermi, Urey, Lamb and Kusch.

(b) *Nuclear Scattering Experiments at the Carnegie Institution of Washington*

After completing my thesis but before receiving my degree, I became a Research Fellow at the Carnegie Institution of Washington, where E. O. Salant, J. Van Allen and I [*Phys. Rev.* **57**, 1069, 1075 (1940)] measured a number of neutron cross sections. N. P. Heydenburg and I measured proton-helium scattering to compare with known neutron-helium scattering as a test of charge symmetry in nuclear forces [*Phys. Rev.* **60**, 42–45 (1941)].

For a short time in Washington I was the owner of one third of a 16-cylinder Cadillac, before the idea of collecting antique cars had been conceived. My share cost only \$50 and I should have held on to it, because that car is now worth about half-a-million dollars.

(c) *Marriage and the University of Illinois*

While a graduate student, I was offered, but turned down a blind date with Elinor Jameson, sister of the Barnard physics teaching assistant, Margaret Jameson. This proved to be unfortunate: later, when I did meet Elinor and was greatly attracted to her, I had moved to Washington and had to commute weekends between Washington and Brooklyn, N.Y., for most of a year until our marriage on June 3, 1940. We had a three-day honeymoon in Connecticut, and then set out with Jerrold Zacharias on a trip to Seattle, W.A., where Jerrold and I were to give invited talks at the American Physical Society Meeting. The trip was concurrent with the Nazi conquest of France and it was discouraging to read of the speed with which the Panzer divisions crossed France. We guessed that this trip might well be our last pleasure trip for years to come. A similar shadow was cast at the Physical Society Meeting when several papers on klystrons were withdrawn, presumably for security reasons.

Following the meeting, Robert Oppenheimer and Karl Darrow, the Secretary of the Physical Society, drove with us to Berkeley. It was a beautiful trip along the rugged coast road of Oregon and California. We stayed in Robert's home, where our delightful visit was marred by the depressing news from the European war.

Our return included Yellowstone Park and a week in Glacier National Park, where Jerrold's wife, Leona joined us for a trip to Banff and the newly opened Banff-Jasper highway. Later all of us visited my parents at Rock Island Arsenal in Illinois, staying in the commanding officers' quarters, a magnificent old mansion built in the late 19th century. The Zacharias's departed after a few days, but Elinor and I stayed for two more weeks and were entertained by many of my parents' friends, at the Arsenal and in the neighboring cities of Davenport, Rock Island and Moline. My father was expecting to retire, but instead was promoted to Brigadier-General and remained in command at Rock Island for the duration of World War II.

In 1940 the Great Depression still lingered, with few physics jobs, so I enthusiastically took a teaching position at the University of Illinois offered by Wheeler Loomis, Chairman of the Physics Department. Elinor and I rented an attractive half-house on the edge of Urbana across the street

from the university alfalfa farm. Since we hoped to spend the rest of our lives there, we settled down comfortably and bought much of our furniture.

By this time, France had been overrun by the Nazis and it appeared probable that England would soon face the same fate, so there was much discussion as to what American scientists should do to help. That summer the British had sent a scientific mission to inform U.S. scientists and government officials about the highly effective use of radar (then called “radiolocation”) in the defense of Britain against aerial bombing. With radar, the defending Spitfires could be well deployed in advance to attack the incoming bombers. Thus the British thought they could defeat the German daylight bombing attacks, but feared the Nazi bombing would then change to attacks at night when the defending pilots could not see the bombers. The British had developed crude radars at longer wavelengths for night fighters and were developing higher resolution radar at 10 cm wavelength, but wanted U.S. help in developing better short wavelength radar. Alfred Loomis [then Chairman of the U.S. Microwave Committee of the National Defense Research Committee (NDRC)] called a meeting of a number of leading physicists, including Ernest Lawrence, I. I. Rabi and Wheeler Loomis. As a result of this meeting, the NDRC decided to support a laboratory for radar research at MIT, with scientists from many universities being encouraged to work there for at least three months. When Wheeler Loomis returned from the meeting, he urged me to go to MIT; he believed the opportunities for making valuable contributions were great. He also remarked that scientists drafted into the Army during the First World War were not effectively used. Elinor and I left promptly after one of the shortest teaching careers ever (less than six weeks) at the University of Illinois.

4. World War II

(a) *Radar Research at MIT*

Elinor and I were among the first to arrive at the new laboratory. After renting an apartment in Cambridge, I left Elinor waiting for our rented furniture while I went to MIT, expecting to return early in the afternoon. But with all the interesting problems in the laboratory, I returned long after dark to find the furniture undelivered, the electric power not turned on and Elinor sitting on the toilet seat — the only seat in the apartment — reading a book with a flashlight. This was a rough beginning to our life in Cambridge, but it prepared her for the many subsequent evenings when I would return from the laboratory later than I had expected.

Initially I was a member of the Magnetron Group headed by Rabi. When he became Deputy Director I headed the Magnetron Group, but soon became head of the Advanced Development Group, which could undertake long range projects that might be finished too late for use during the War. We chose as our first project the development of a 3 cm magnetron for higher resolution radars. The work progressed so well that we expanded to developing other components that would be useful in a 3 cm radar system, such as waveguides, low loss choke connectors for attaching one waveguide to another, crystal detectors and TR boxes (the TR — Transmit/Receive — box is a device that prevents the crystals from burning out when a radar transmits and receives on a single antenna).

By early summer of 1941 we had a 3 cm radar system operating on the roof of MIT and it was decided that I should go to England to alert the scientists there about our successful technology. During my month in England, I visited radar research laboratories, told them of our work, learned more about their work at 10 cm and visited one of the long wavelength operating fighter control stations. I also helped Jim Lawson and Dale Corson, who were making comparative tests of British and U.S. 10 cm radars, which showed that the British crystal detectors were much more sensitive than the U.S. vacuum tube detectors. On my return to the U.S., I brought with me several British crystals and T/R boxes, which stimulated the U.S. programs for crystal detectors and for solid state electronics.

By the time I returned, our 3 cm developments were progressing so well we were ready to run a test aboard an airplane. The first was a dramatic failure (we flew by the *Queen Mary* — one of the largest ships afloat — and did not see it on our radar screen). We soon fixed the trouble and the 3 cm wavelength proved superior for distinguishing small objects. We also observed that radar images of coast lines and features such as Cape Cod or Nantucket were dramatically sharper at 3 cm than at 10 cm and thus an effective navigational aid along the coast, which encouraged us to try our 3 cm radar as a navigational aid over land. E. Luebke and I had several miserable trips trying to navigate over land by comparing our radar images to ordinary road maps. Despite our failure, we realized that there were a number of distinguishing features on our radar display that might make radar navigation over land possible if the radar operator had a special map highlighting radar reflecting objects. This proved to be correct and led to a 3 cm radar navigation and bombing system that was used extensively in Europe and the Pacific.

(b) *Work in the Pentagon*

When the U.S. became directly involved in the war, the Radiation Laboratory's activities accelerated. The technical developments were impressive, but the Army Air Force badly needed help on radar procurement decisions, so I went to Washington as an Expert Consultant to the Secretary of War. Dr. Edward Bowles of MIT had moved to the Pentagon Building, where he headed an office of consultants who were assigned in turn to different parts of the War Department needing technical help. I worked in the Air Force Military Requirements Branch and helped establish the Air Force radar requirements. During one weekend a young lieutenant in the procurement office and I prepared the first draft of what eventually became the Air Force radar procurement plan for the duration of the war.

(c) *Los Alamos and Tinian*

In the early spring of 1943, I received a call from Robert Oppenheimer saying he and Bob Bacher wished to meet Ken Bainbridge and me for lunch. They related the progress that had been made on the U.S. uranium fission project, including the success of the Stagg Field Reactor under

Fermi's supervision and plans to construct large scale isotope separation plants and plutonium production reactors. General Leslie Groves headed the Manhattan District to develop nuclear weapons and Robert, as the newly appointed Director of the Los Alamos Laboratory in New Mexico, wanted us to join the project. He said we should continue in our present jobs and we would soon receive further word. In my case the further word was long delayed because of a disagreement about my assignment between Dr. Bowles and General Groves, which could only be resolved by the Secretary of War since each of them reported directly and independently to the Secretary. A compromise was eventually reached by which I continued officially as an Expert Consultant paid by Bowles' office, but otherwise worked at Los Alamos as a regular staff member, beginning in September of 1943.

Most of the Los Alamos housing was in ugly temporary apartment buildings, but the old Ranch School apartments were attractive log structures on what was called "Bathtub Row," since they were the only houses with bathtubs. Most of the old Ranch School apartments had been taken long before we arrived, but we found one unclaimed because of its small size and lack of storage space. We were compensated by its beautiful view of the Sangre de Cristo Mountains.

I headed the Delivery Group, which was responsible for aircraft modifications as well as ballistic and reliability tests involving aircraft. I was also Deputy to Captain W. S. Parsons, who headed the Ordnance Division. At the end of the war, I was Chief Scientist for the Los Alamos technical unit headed by Captain Parsons on Tinian Island, where the 509th Bombardment Group was based.

During the first half of the war, I had been on leave from the University of Illinois, but later I accepted a professorship at Columbia and went on leave from Columbia for the remainder of the war. As soon as the war was over and we were allowed to leave Tinian and Los Alamos, Elinor and I drove to New York City. I was eager to return to peacetime fundamental research as soon as possible.

Although radar and atomic bombs had helped defeat the Nazis and had reduced human suffering by bringing the war to an earlier end, I was and still am deeply distressed by the misery and loss of human life directly caused by both radar and nuclear bombing. It is my sincere hope that nuclear weapons will never be used again, that major wars of all kinds will be avoided and that even minor wars will eventually disappear.

5. Columbia and Brookhaven

(a) *Return to Columbia*

During the war, I had been so fully occupied that I had given little thought to the research I would do when peace returned, but one evening in 1944 while I was visiting MIT, Rabi and I did some postwar planning. The discussion was fruitful and we invented two good experiments. For the first, we calculated the possibility for detecting magnetic resonance transitions by the effect on the oscillating field, rather than on the molecules, as in our previous experiments. To our pleasant surprise the calculations were favorable. But Rabi remembered that in 1943 Gorter had tried such an experiment and failed for unknown reasons, so we gave it a low priority. The second experiment

was to use the molecular beam magnetic resonance method to measure accurately the atomic hydrogen hyperfine separation, the energy difference between states with the electron and nuclear spins being parallel and antiparallel. This experiment had the advantage that the answer presumably could be predicted from the Dirac theory and the measured values of the proton and electron magnetic moments. We believed the comparison between theory and experiment would provide a fundamental test of the theory. Following this one evening we returned to our wartime work, with little opportunity to think further about postwar research.

On my return to Columbia I found that most of our old molecular beam laboratories had been taken apart to make room for wartime research. However, one old molecular beam apparatus on the fifth floor, used for alkali molecules, was still in existence, buried under five years of dust. A graduate student, William Nierenberg, who had done underwater acoustic research during World War II, expressed the desire to work with me for his Ph.D. thesis, so I started him off cleaning up the fifth floor apparatus and preparing it for operation. At the same time I searched diligently through the Columbia attic for other pieces of old molecular beam apparatus. I found a number of miscellaneous components on shelves, in boxes and even in old wastebaskets. Fortunately I found our old Pirani detector for molecular and atomic hydrogen and a number of other parts of our hydrogen apparatus. Since Rabi was quite busy with national policy issues in Washington at this time, I helped two Columbia graduate students, J. E. Nafe and E. B. Nelson, use these components for the atomic hydrogen hyperfine structure experiment that Rabi and I had conceived two years earlier.

As these research projects proceeded, I needed to prepare the course I was to give the next term on a subject I had not even reviewed in the past five years. Although all this was time-consuming, I found I very much enjoyed the combination of teaching and research.

(b) *The Establishment of Brookhaven National Laboratory*

While getting the molecular beam laboratory restarted, Rabi and I had many discussions about the future course for the Columbia Physics Department. We were rather frustrated by the way Columbia Physics had suffered during the war. At its beginning, Enrico Fermi and Harold Urey were Columbia professors and Fermi had done his initial research for the development of nuclear reactors there, with the assistance of my former classmate Herbert Anderson and others. During the war, this work and the staff were transferred to the University of Chicago, so, at the war's end, Chicago had Fermi, Urey, Anderson and the Argonne Laboratory while we had none. We needed nuclear facilities like Argonne Laboratory, either at Columbia or nearby, but we knew the Columbia Physics Department would always be relatively small and we did not wish to operate a large facility that would distract us from basic research. We concluded that the responsibilities should be shared with other institutions, so we asked Dean Pegram to call a series of meetings with New York institutions, later expanded to other northeastern universities. Eventually, representatives of nine northeastern universities formed the Initiatory University Group (IUG), which later became Associated Universities, Inc. (AUI). We proposed that AUI, under Manhattan District and later Atomic Energy Commission contracts, should establish and operate a nuclear and particle physics

laboratory in the northeast. L. A. DuBridge was elected Chairman and I was elected Secretary of IUG and Chairman of the Site Committee.

After a year of intense activity and many crises, Brookhaven National Laboratory was established at Upton, N.Y. I was appointed the first Chairman of the Physics Department. During the next six months, I hired many of the Brookhaven staff, including two who later were Laboratory Directors — Lee Haworth and Maurice Goldhaber.

While serving as Chairman of the Brookhaven Physics Department, I continued on a half-time basis as a professor at Columbia, where I supervised my first graduate student, Bill Nierenberg, in his thesis studying the radiofrequency spectra of the sodium halides (Paper 2.3). At that time, Rabi and I were trying to persuade the rest of the Columbia Physics Department to hire Polykarp Kusch, but the others argued that, with Rabi, Lamb and myself all being in the field of atomic and molecular physics, we should not hire another physicist in that field. To make Kusch's appointment possible, I agreed that half of my research would be in nuclear and particle physics at Brookhaven and that I would give up my participation in the atomic hydrogen hyperfine structure experiment. My agreement was a great contribution to the department since Kusch did come and was a valuable member of the department as well as a winner of the Nobel Prize. However, it was a terrible loss for me since the atomic hyperfine results disagreed with the Dirac theory and the experiment I had to miss became the primary stimulus for Julian Schwinger's development of relativistic quantum electrodynamics (QED).

Although I enjoyed my work at both Columbia and Brookhaven, I worried that my administrative duties were giving me insufficient time for thinking and research. Consequently, when Professor J. H. Van Vleck offered me a tenured associate professorship at Harvard, I accepted, and in September of 1947 Elinor, our two daughters and I moved to Belmont, MA, where we lived for the next 36 years.

6. *Harvard University (1947–1997)*

At Harvard, I was usually involved in a number of concurrent activities. There was undergraduate and graduate teaching, supervision of Ph.D. candidates and research, often in quite different areas. As a result, a sequential account is confusing, so I shall discuss my Harvard activities by topics.

(a) *Teaching*

I taught at Harvard for 40 years except for visiting professorships at Middlebury College, Mt. Holyoke Colleges, the University of Virginia and Oxford University. Although I taught a wide variety of subjects, ranging from classical mechanics and electromagnetic theory to nuclear physics, the two courses I most enjoyed, and taught most frequently, were "Introduction to Quantum Mechanics" and a Freshman Seminar, usually on "Topics in Contemporary Physics." The

pleasure of classroom teaching was enhanced by the high quality of Harvard students, many of whom became famous physicists.

My most enjoyable teaching was associated with the 84 students who obtained their Ph.D. degree under my supervision. We worked closely together, more as collaborators than professor and student. I encouraged my students to be creative, to take the initiative and to assume maximum responsibility for their work, and in turn I was gentle when they made mistakes. At any one time, I felt that half of my very best friends were my current graduate students. Usually I had eight to a dozen graduate students working in my group. Our research was mostly in the fields of molecular beam, atomic, particle or nuclear physics. I tried to have a mix of experiments: some that were important, but fairly sure to succeed, and others that were risky and speculative, but potentially of great significance, like early tests of parity and time reversal symmetry or searches for new kinds of forces. Much of this research depended on spectroscopy with coherent radiation, mostly radiofrequency and microwave radiations. Our experimental and theoretical results were published in the physics literature, most often in *Physical Review*, *Physical Review Letters* or *Journal of Chemical Physics*.

Some of the excellent Harvard and MIT faculty who contributed to my knowledge and pleasure were D. Kleppner, D. Larson, E. M. Purcell, J. H. Van Vleck, J. Schwinger, K. T. Bainbridge, V. Weisskopf, S. Glashow, S. Weinberg, N. Bloembergen, W. Furry, I. Silvera, R. Wilson, R. V. Pound, F. Pipkin, K. Strauch, S. Coleman, H. Georgi, G. Feldman, R. Glauber, I. Shapiro, M. Tinkham, J. C. Street, G. Gabrielse, P. Bridgman, P. Horowitz, M. Prentiss, P. Martin, D. Nelson, A. Jaffe, T. Chupp, J. Doyle, E. Heller, M. Franklin, D. Pritchard, B. Halperin, W. Press, C. Papaliolios, P. Pershan, T. Wu, C. Vafa and G. Holton.

(b) *Nuclear Interactions in Molecules*

My students and I established a molecular beam laboratory for spectroscopy with coherent radiation at radio and microwave frequencies. We measured nuclear spins, magnetic moments and nuclear quadrupole moments. We also measured molecular rotational magnetic moments, electron distributions in molecules and various nuclear interactions in molecules including nuclear spin-spin magnetic interactions, electron-coupled nuclear spin-spin interactions, nuclear quadrupole moment interactions and the dependence of molecular magnetic shielding and diamagnetic susceptibility on molecular orientation. In some cases we measured the dependence of these parameters on the isotopic composition of the molecules and on their vibrational and rotational states. Some of our experiments were with magnetic oscillatory fields and others with electric. Since molecular beam resonance apparatus was not commercially available, we built our own. We concentrated on diatomic molecules since they were more suitable for comparing theory and experiment, but we also studied more complicated molecules such as methane (CH_4). Our most extensive experimental measurements were with H_2 , D_2 , and HD , since these were most fundamental and interpretable. Several new kinds of interactions were first seen in these experiments. In many cases, we had to develop new theoretical structures for the interpretation of the

experiments, as in the cases of molecular hydrogens and spherical top molecules such as methane for which there were no previous theories including interactions between nuclear spins and magnetic moments. These experiments are described with greater detail in Sec. 2.

(c) *Electron-Coupled Spin-Spin Interactions*

Most of the above nuclear interactions cannot be observed in NMR experiments with gases due to the frequent molecular collisions causing the interactions to average to zero (collision narrowing). However, Gutowsky, Slichter, Hahn and others found that in some molecular spectra there were unanticipated long range spin-spin scalar interactions proportional to $\mathbf{I}_1 \cdot \mathbf{I}_2$. Purcell and I (Paper 5.5) showed this could be attributed to magnetic coupling between the nuclear and electron spins. The two nuclear spins are each magnetically coupled to nearby electron spins, but the electrons are exchange-coupled together by being fermions in a molecule with no net nuclear spin angular momentum.

(d) *Nuclear and Particle Physics*

While supervising our molecular beam research, I was also Director of the 150 MeV Harvard Cyclotron, designed by Robert R. Wilson, who left Harvard for Cornell in 1947 before it was constructed. I agreed to be Director during its construction and early use and was well assisted by Deputy Director Lee Davenport and other staff members. My associates, graduate students and I used the cyclotron in a number of experiments, including proton-proton scattering, and increased its energy to 250 MeV. The Harvard Cyclotron was particularly well designed for ease of operation and maintenance; partly for this reason, it is still used for proton cancer therapy, making it the oldest large cyclotron still in operation.

(e) *Invention of the Separated Oscillatory Fields Method*

With our first molecular beam magnetic resonance apparatus, we sought to obtain ten times better resolution than in any previous apparatus by increasing the length of the resonance region to 1.5 m (a factor of ten), but we failed to obtain magnetic fields of the uniformity required to prevent the resonances in the different parts of the magnet occurring at different frequencies. Inspired by this failure, I invented (Paper 1.2) the separated oscillatory fields method, which permitted the desired accuracy with the available magnets since the resonance frequencies were determined only by the *average* energies of the states between the two separated oscillatory fields. As discussed in the commentary for Paper 1.2, the separated oscillatory fields method has been extended in a number of different ways and has many advantages over the single oscillatory field method, including applicability to much higher frequencies and elimination of first order Doppler shifts.

(f) *Parity Tests*

In 1950, when the assumption of parity (P) symmetry was universally accepted, I was lecturing on molecular beams with Ed Purcell sitting in. I was about to give the then-standard proof that in the absence of degeneracy a particle whose orientation was determined by its spin angular momentum could have no electric dipole moment, a proof that depends on the assumption of P symmetry for nuclear forces. I had already discovered that, if I lectured on a topic I did not thoroughly understand, I could count on Purcell asking me an astute question that would reveal my ignorance. In anticipation of such a question I tried to find experimental evidence for the assumption of parity symmetry in nuclear forces but found none. An electric dipole moment interacting with an external electric field, for example, would show a P failure but most experiments were on charged particles which would be accelerated by an electric field into a zero field region or out of the apparatus. On the military principle that, if about to be attacked, counterattack, I asked Purcell for the evidence for P symmetry with nuclear forces, and after some effort he decided he could find none either. Thus, in 1950, we published Paper 3.1, pointing out the absence of experimental evidence for the assumption of P symmetry, and we collaborated with a graduate student, J. Smith, to test P by looking for a neutron electric dipole moment; we found none, but did set a low limit to its value (Paper 3.2).

Most physicists then attributed this result to the assumed validity of P symmetry, but I continued to believe a failure P symmetry in nuclear forces might be found with a sufficiently sensitive experiment. At that time such forces were usually discussed together as nuclear forces rather than the two separate categories of weak and strong forces. Our electric dipole experiment was a very sensitive test for P failure in the strong, but not in the weak, force. As a result, in 1946, when I heard C. N. Yang in a colloquium suggest the possibility of a P failure in the weak force, I immediately wanted to test the weak interaction and proposed, in the colloquium discussion and subsequent correspondence, a method for doing so. I knew that L. Roberts of Oak Ridge had cryogenic facilities for polarizing nuclei and that radioactive ^{60}Co had been polarized, so I arranged to do an experiment with him to see if more decay electrons came out in one direction relative to the spin than in the opposite. Roberts agreed to provide the polarized ^{60}Co and I the electron-counting equipment. Unfortunately, Roberts soon discovered that the angular distribution of the neutrons in fission was different than expected theoretically, so the theory advisory group at Oak Ridge urged him to concentrate on exploiting this new discovery and postpone our highly speculative test of parity. By the time I was told of this decision, C. S. Wu and E. Ambler had already started preparing their own experiment, which later showed a maximal failure of parity symmetry in the radioactive decay of ^{60}Co . The absence of an electric dipole moment in our neutron experiment and the forced postponement of our ^{60}Co experiments were the greatest disappointments in my research career. But, by then, I had realized that research scientists have both good and bad luck and productive scientists do not allow the bad luck to discourage them from further research.

(g) *Nuclear Magnetic Shielding and Chemical Shifts*

Although we accurately measured nuclear magnetic resonance frequencies, the accuracies of the nuclear magnetic moment determinations were reduced because the nuclei were surrounded by the rest of the atom or molecule and the external magnetic field induced a circulation of the electrons, which in turn produced a magnetic field partially canceling the original field, i.e. provided magnetic shielding. The shielding factors had been calculated by W. Lamb for atoms where the induced electron circulations were in simple circles due to the central symmetry of the Coulomb force. However, with polyatomic molecules, that theory no longer applied due to the multiplicity of Coulomb centers. Since I was eager to extract better nuclear magnetic moment values from our experiments, I attacked this theoretical problem and eventually found a general theory of magnetic shielding applicable to molecules (Papers 4.1, 4.2 and 4.4).

The theory implied that magnetic shielding would be different in different molecules. From the point of view of measuring nuclear magnetic moments, these chemical shifts are a nuisance, but they provide the molecule identifications which are the basis for the widespread use of NMR in chemistry, biology and medicine.

(h) *Thermodynamics and Statistical Mechanics at Negative Absolute Temperatures*

In his NMR research at Harvard, R. V. Pound found a pure LiF crystal for which spin-lattice relaxation times were as large as 5 min at room temperature while the spin-spin relaxation time was less than 10^{-5} sec. Pound, Purcell and I did some experiments on this crystal and noted the results could be interpreted in terms of the nuclear spins forming a valid equilibrium thermodynamic system, well isolated from its surroundings, and that the spin system could be at negative absolute temperature since there was an upper as well as a lower limit to the allowed energies of the constituents (see commentary for Paper 1.4). The first publication by Purcell and Pound described negative temperature phenomena only briefly, so I later (Paper 6.1) developed and published detailed theories of thermodynamics and statistical mechanics at negative absolute temperatures. Negative temperature systems have several unusual characteristics. The high energy states are occupied more than the lower energy states, as can be seen from the Boltzmann factor, $e^{-W/kT}$. Negative absolute temperatures are not colder than positive absolute zero but are hotter than positive infinity since heat will flow from a negative temperature system to one at positive temperature if they are brought into thermal contact. Although most formulations of the second law of thermodynamics are the same with positive and negative absolute temperatures, the Kelvin-Planck form must be modified as discussed in Paper 6.1 and its commentary. Resistances at negative absolute temperatures are intrinsically negative, so they are effectively amplifiers.

Although our first work was done on negative temperature systems long before the invention of the maser and laser, negative temperature systems are the ones that most closely approximate masers and lasers even though most masers and lasers are not in thermodynamic equilibrium.

C. Townes has told me that a colloquium I gave at Columbia on “Negative Absolute Temperatures” helped stimulate his invention of the maser.

(i) *Significance of Potentials in Quantum Mechanics*

When Aharonov and Bohm’s paper on the effects of potentials in quantum mechanics first appeared, many physicists were incredulous, until Furry and I (Paper 6.2) showed that without these effects one could observe a two-slit interference pattern even when the path of the electron had been determined, observations that are contrary to the principle of complementarity; we showed that these observations are prevented by the Aharonov–Bohm effects. If a sufficiently sensitive circuit is provided to determine the path, the quantum-mechanical fluctuations of the potentials through the Aharonov–Bohm effect will destroy the interference. Much later, in Paper 6.5, I showed that similar considerations apply to two-path neutron interference experiments except that the interferences are destroyed through the Aharonov–Casher effect. In the same paper, I pointed out that the Aharonov–Casher effect also makes it impossible to observe separated oscillatory field resonances if one can determine the spin state of the neutron between the two separated oscillatory fields.

(j) *Atomic Clocks and Frequency Standards*

Although the separated oscillatory fields method was originally invented for improved precision in radiofrequency spectroscopy, it was clear that the high precision and reproducibility of the spectral frequencies made them particularly suitable for atomic clocks and frequency standards. The earliest atomic cesium clocks used the separated oscillatory fields method and were made by J. R. Zacharias, L. Essen, V. L. Parry and others. I consulted on some of this atomic clock work and did theoretical studies on possible frequency shifts and distortions that need to be eliminated to achieve high accuracy (Papers 1.9 and 1.14). There have been further improvements in such clocks by laboratories throughout the world, especially those responsible for time and frequency standards. Cesium atomic beam clocks now have accuracies better than 1×10^{-14} and are the basis of the internationally approved definitions of both the second and the meter. Atomic hydrogen masers (described in Paper 1.10) are ten times more stable for periods of one day, which makes them particularly useful in very long baseline interferometry (VLBI) in radioastronomy, as described in Paper 1.14.

(k) *6 GeV Cambridge Electron Accelerator and Electron Scattering Experiments*

While Harvard and MIT were building the 6 GeV Cambridge Electron Accelerator, I was Chairman of the Harvard–MIT Joint Committee overseeing this project, which was directed by

M. S. Livingston. Later, my associates and I used the accelerator for electron scattering experiments including measurements of proton electromagnetic form factors at high momentum transfer.

(l) *Atomic Hydrogen Maser*

The sharpness of the separated oscillatory field resonances were limited by the short time the atoms were in the atomic beam apparatus. This time could be increased by lengthening the apparatus but eventually would meet limits due to cost as well as to the deflection of the atomic beam by gravity. To overcome this, I proposed an atomic trap in which cesium atoms would pass through the first half of a separated oscillatory field resonance apparatus and then be admitted to an evacuated container with suitably coated walls where they would bounce around for a period of time before emerging through a small hole into the second half of the beam apparatus. Two of my thesis students, D. Kleppner and P. Fjelstadt, and I undertook this experiment [*Phys. Rev. Lett.* **1**, 232–233 (1958)]. To the surprise of most of our friends, the experiment was a partial success, in that the atoms could bounce off hydrocarbon-coated walls a few hundred times without losing their phase coherency. Later, we realized that light atoms with low electric polarizability should stick least on the walls and this suggested the use of atomic hydrogen. However, atomic hydrogen was then particularly difficult to detect in molecular beams, so we calculated the possibility of detecting it through measurements of the microwave radiation. In the course of the calculations we realized that the system could spontaneously oscillate if the atoms were introduced only in the higher hyperfine state. Although the smallness of the frequency and oscillation matrix elements made spontaneous oscillation more difficult in our atomic hyperfine case than in C. Townes' previous ammonia maser, we concluded that these difficulties should be fully overcome by the our much narrower resonance line. My graduate student, H. M. Goldenberg, Kleppner and I constructed the first atomic hydrogen maser (Paper 1.10). It functioned well and was extremely stable, particularly when the interior of the atomic hydrogen storage bottle was coated with Teflon.

(m) *Limit to the Tensor Force Between Two Protons at Long Range*

In our accurate experiments with molecular hydrogen, we determined what we originally described as the magnetic nuclear spin–spin tensor interaction. Later, I realized it was an assumption to attribute the entire effect to the nuclear magnetic moments. What we actually observed was the total nuclear spin–spin tensor interaction, which is the sum of that magnetic interaction and any other tensor interaction that may exist. Furthermore, we could accurately calculate the magnetic interaction from the measured magnetic moments of the nuclei, since the average value of R^{-3} was known. Although a determination of the latter required a suitable averaging over the vibrational motions and rotational stretching of the H_2 molecule, these had been determined by a combination of theory and experiments, including our own comparisons of molecular beam resonance measurements in the first rotational states of H_2 and HD and in the second rotational state of D_2 . These

calculations showed that our observed tensor interaction could be fully accounted for by known magnetic effects within experimental error. From this result and a determination of the theoretical and experimental errors, I could set an upper limit to the nonmagnetic tensor force between two protons as in Paper 6.3.

(n) *Tests of Time Reversal Symmetry*

Soon after the experimental demonstration of the failure of parity symmetry in the weak interaction, most of the leading theorists, including L. Landau, T. D. Lee, C. N. Yang and E. Wigner, predicted there could still be no neutron electric dipole moment because of CP conservation, which would imply T symmetry if there were CPT symmetry, as was generally believed. I then wrote the theoretical Paper 3.4 pointing out that CP and T symmetries were assumptions which required experimental testing and that a search for a neutron electric dipole moment provided such a test. Subsequently, various collaborators and I have carried out successive searches with increased sensitivity, but we have yet to find a nonzero electric dipole moment. The attitude of theorists toward these experiments changed markedly in 1964 with the discovery of CP nonconservation in the decay of the long-lived neutral kaon. Most theories soon after that discovery predicted neutron electric dipole moments of sizes comparable to our limits. As our experimental limit lowered, most of these early theories were abandoned and replaced by new theories predicting smaller neutron electric dipole moments.

Improved experiments continue. Our latest published experiment (Paper 3.3) at the Institut Laue-Langevin (I.L.L.) in Grenoble, France, utilizes ultracold neutrons stored for about 100 sec in a totally reflecting beryllia cylinder with beryllium top and bottom. We conclude, with 95% confidence, that the neutron electric dipole moment is less than 9×10^{-26} e cm. New modifications to our apparatus, including a ^{199}Hg magnetic field monitor in the same volume with the neutrons, should enable us to set a limit better than ten times lower. Although the standard model predicts too small an electric dipole moment to be seen even in these experiments, most theories that go beyond the standard model, such as supersymmetric (SUSY) theories, are compatible with our finding an electric dipole moment in this range.

(o) *Parity-Nonconserving Spin Rotations of Neutrons*

In experiments at Grenoble, B. Heckel, other collaborators and I made the first observations of parity-nonconserving spin rotations of neutrons passing through various materials (Paper 3.5 and accompanying commentary). In some materials the effects were much larger than we had expected, due to possible low-lying P states of the neutron-nucleus compound state. This simplified the experiments but complicated their interpretations by mixing particle and nuclear effects. Heckel hopes soon to make such measurements in liquid H_2 and helium, which should be free of such complications. The difference between the two measurements should provide the best determination of the neutron-neutron parity-nonconserving interaction.

(p) *The Universities Research Association and Fermilab*

For 16 years I was on leave half-time from Harvard and served as President of the Universities Research Association (URA), which managed the construction and the operation of Fermilab, which now has the world's highest energy particle accelerator. During this period, I terminated all my personal high energy research to concentrate on Fermilab, but I continued my low energy research with graduate students at Harvard. At Fermilab, I had the good fortune to work with two excellent directors, R. R. Wilson and L. Lederman. The accelerator was completed under budget and reached more than 400 GeV, double its original 200 GeV design energy; and by now it has been upgraded with superconducting magnets to provide colliding beams at 1 TeV each.

(q) *Other Research, Administrative and Policy Activities*

In addition to the research activities discussed, I participated in other experiments, too numerous to list here but reported in the scientific journals. I was also involved in various administrative activities and was a member or chairman of a number of science policy committees, including the following three, which I considered particularly effective.

From 1958 to 1959, I was on leave from Harvard as the first NATO Science Advisor (Assistant Secretary General for Science) and Chairman of its Science Committee, to "increase the cooperation in science among NATO nations." We established the NATO programs of Fellowships, Cooperative Research Projects and Advanced Study Institutes (Ecole d'Ete). To the credit of my successors, these programs continue, with the number of Advanced Study Institutes having increased from the original two in theoretical physics to about 160 in all fields of science.

For a number of years, I was a member of the General Advisory Committee (GAC) of the AEC and later of the Department of Energy. From 1962 to 1963, I was Chairman of the Joint High Energy Physics Panel of the GAC and of the President's Scientific Advisory Committee (PSAC). This panel recommended the future high energy program for the United States including a 200 GeV proton accelerator to be followed by one at 800 GeV, colliding electron beams at the Stanford Linear Accelerator (SLAC) and colliding 35 GeV protons at Brookhaven. This program was adopted except that the 200 GeV accelerator at Fermilab was later upgraded to a 1 TeV collider and Brookhaven preferred to improve its existing 35 GeV accelerator rather than to build the proposed collider.

From 1981 to 1982 I was chairman of the National Academy of Sciences Committee on Ballistic Acoustics to investigate the claim that there was a police recording of a broadcast from a motorcycle with a stuck-open microphone that indicated shots were fired at President Kennedy by two assassins, one of whom was on the Grassy Knoll portion of Daley Plaza. We were able to show that the claimed acoustical evidence for the extra assassin was faulty. In addition, there was cross-talk between the two police communications channels which showed conclusively that the recorded noise claimed as shots occurred approximately one minute after the assassination was over and the President's car had left Daley Plaza.

(r) *Defense of Wendell Furry from Attacks by the House Un-American Activities Committee and by Senator Joseph McCarthy*

For several years I spent much of my time time defending my colleague Wendell Furry before the Harvard administration, the government and the public when he was viciously attacked by the House Committee on Un-American Activities and later by Senator Joseph McCarthy. I had the opportunity on a *Meet the Press* TV program to point out how Senator McCarthy was using the "area rule," a legal technicality associated with the use of the Fifth Amendment, to mislead the TV-watching public into believing he was "exposing Communist spies." A former member of the Communist Party was entitled, under the Fifth Amendment, to decline to name friends who 25 years earlier had innocently attended a Communist Party meeting during the Great Depression, but under the "area rule" he had to decline to answer *all* questions in the "area" of Communism with the statement "I decline to answer on the grounds that the answer might tend to incriminate me." The "area rule" for Congressional Hearings is the analog of the familiar Fifth Amendment court trial ruling that if a witness chooses to testify in his own defense he is subject to cross-examination. In closed hearings McCarthy would ask the witness to reveal the names of others at the meetings. If the witness declined to do so, McCarthy would warn him of the "area rule," but in the subsequent open hearing McCarthy would ask questions like "Did you ever give secret information to Communist spies?". The witness, to protect his Fifth Amendment privilege, would by the "area rule" be required to decline on the grounds that the answer "*might tend* to incriminate me." In his summary of the proceedings McCarthy would then misquote the technical phrase by saying he had just exposed more Communist spies, because what better evidence could there be that a person was a spy than his statement that an answer *would* incriminate him?

After the *Meet the Press* program I had an unexpected dinner and long discussion with Senator McCarthy and a few of his friends, during which I emphasized the harm he was doing and the misleading nature of his TV hearings while he tried unsuccessfully to convince me he was doing a valuable national service. Although Senator McCarthy was then at the peak of his popularity, everything soon went downhill for him. E. R. Murrow broadcast a series of TV clips showing the untruthful and self-contradictory nature of McCarthy's own statements. Furry and another Harvard teacher, Leon Kamen, were acquitted of contempt of Congress charges brought by McCarthy. The extensively televised Army-McCarthy hearings in the Senate clearly demonstrated the misleading and harmful character of McCarthy's campaign. McCarthy eventually was censured by Congress and he soon died of sclerosis of the liver due to alcoholism.

(s) *Personal and Family Life*

I have thoroughly enjoyed my teaching, research and writing activities, but they reduced time for my wife and children. However, we had a very happy family life with four daughters and eventually six grandchildren. Although we consulted with each other and agreed on major decisions, Elinor took on the primary responsibility of raising our children and did it very well. We

enjoyed most Sundays together, our two-week summer vacations and our Christmas week skiing holidays. Our longest family trip was the year I had a Guggenheim Fellowship at Oxford, when we lived in the nearby village of Dorchester-on-Thames and were able to take family trips to Austria, France, Germany, the Netherlands and Denmark. Later, we greatly enjoyed the year I was the Eastman Professor at Oxford, but by then our daughters were so dispersed that only one could be with us.

We were all greatly saddened when Elinor died from ovarian cancer in 1983. I was deeply distressed since Elinor had become such a central part of my life.

A year later, on an Appalachian Mountain Club hike, I had the good fortune to meet Ellie Welch, a graduate of Boston University, a retired Social Service Administrator and then the owner of a Bed and Breakfast Reservation service. A widow for many years, she had supported and raised two sons and a daughter. Ellie and I were immediately attracted to each other, married in May of 1985 and have lived happily ever since. We had no free time for a honeymoon trip immediately after our marriage, but soon had a long trip in Europe including a hiking trip in the Austrian Stubai Alpen, going from one alpine hut to another for ten days, followed by trips to Grenoble, France and Russia.

On our return, we first lived in Ellie's four-floor Brookline town house and later moved across the street to a smaller, one-floor contemporary home with a beautiful garden. We now have a combined family of five adult daughters, two sons and eight grandchildren. Even though our children were adults when Ellie and I first met, she has been remarkably effective in drawing our families together, as illustrated by 19 of us being in Vermont, Brookline or both over the Christmas holidays. We visit our far-flung family as our travels permit.

(t) *Retirement*

I retired from Harvard in 1986, about a month before my 71st birthday. During my active period, I usually had 10 or 12 graduate students working with me at any one time and I felt they learned so much from each other that I stopped accepting new graduate students a few years before my retirement while I still had a strong group. However, with my Harvard office and partial use of a secretary, I continue my great interest in physics and devote much time to continuing neutron experiments, theoretical research, writing, participating in seminars and studying to keep up with the many exciting new scientific developments.

Ellie and I have enjoyed my periods as visiting professor at the University of Chicago, Williams College and the University of Michigan and as a visiting scientist at the Joint Institute for Laboratory Astrophysics in Boulder, Colorado, where I am still an Adjunct Fellow. I am frequently invited to give advice and lectures at international meetings and universities in different countries; I like doing so and we both enjoy the trips. Ellie and I enjoy downhill and cross-country skiing, hiking, bicycling, trekking and traveling as well as musical and cultural events. Most trips originate in my being invited to give lectures or advice in foreign countries, but Ellie is resourceful in suggesting adventuresome additions, for which we now have more time. We have taken a 28-day trans-Himalayan

trek in Kashmir, India, an 8-day trek in Nepal, a 14-day coast-to-coast hike across England, and have visited Australia, New Zealand, Vietnam, Korea, China, Japan, Thailand, Cambodia, Brazil and Chile, among other interesting places.

A delightful event was winning half of the 1989 Nobel Prize in Physics. We had a fabulous time in Stockholm with the impressive ceremony, elegant banquets and numerous meetings with students and Scandinavian scientists as well as the other laureates. The Prize came at a fine time for me, just as I was ending my classroom teaching, so I had extra time for the additional activities that come with being a Nobel laureate. Instead of interfering with my research, the Prize helped extend my active physics career.

I continued full involvement with the Grenoble search for a neutron electric dipole moment until 1996, when I concluded that, beyond the age of 81, I should no longer be responsible for such an important and difficult experiment. Although I shall not be a coauthor of future publications of our group, which is just starting a new search, I continue a friendly association with the experiment and try to anticipate problems and pitfalls.

I have greatly enjoyed my years as a research physicist, teacher, husband, father and retired professor. My close friendships with my collaborators, associates and 84 graduate students have given me deep pleasure.

(u) *Honors*

E. O. Lawrence Award, 1960; Trustee, Carnegie Endowment for International Peace, 1962–86; Davisson–Germer Prize, 1974; Trustee of Rockefeller University, 1977–89; President of the American Physical Society, 1978–79; President of Universities Research Association, 1955–1982; Senior Fellow, Harvard Society of Fellows, 1971–82; Chairman, Board of Governors of American Institute of Physics, 1980–86; President of Phi Beta Kappa Society, 1984–88; IEEE Medal of Honor, 1984; Rabi Prize, 1985; Rumford Premium, 1985; Chairman, Board of Physics and Astronomy of National Research Council, 1985–1989; Compton Medal, 1986; Oersted Medal, 1988; National Medal of Science, 1988; Nobel Prize in Physics, 1989; Foreign Associate, French Academy of Science, 1990; Erice Prize for Science and Peace, 1992; Einstein Prize for Laser Science, 1993; Vannevar Bush Award, 1995; and 12 honorary degrees.

(v) *Publications*

More than 400 articles in scientific journals, and four books — *Experimental Nuclear Physics*, with E. Segre (John Wiley and Sons, 1953); *Nuclear Moments* (John Wiley and Sons, 1953); *Molecular Beams* (Oxford University Press, 1956, 1985); and *Quick Calculus*, with D. Kleppner (John Wiley and Sons, 1965, 1985).