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World Scientific Series in 20th Century Physics – Vol. 15

ENCOUNTERS IN **MAGNETIC** **RESONANCES**

Selected Papers of Nicolaas Bloembergen
(with commentary)

Editor

Nicolaas Bloembergen

Harvard University

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PREFACE

When World Scientific suggested I contribute to their series of reprint collections, I was happy to do so as I had a favorable impression of the volumes in their previously published series on twentieth century physics and on atomic, molecular and optical physics. I chose the title "Encounters in Magnetic Resonance" for one volume, and "Encounters in Nonlinear Optics" for another, as the subject matter of my scientific work was to a large extent determined by a mixture of haphazard and planned opportunities during my life in physics. "Encounters in Magnetic Resonance" begins with an autobiographical sketch to provide a frame of reference of the circumstances under which these papers originated. I hope some readers will find it of historical interest as no scientific idea is born in a vacuum.

While working in the field of magnetic resonance, I concentrated on spin relaxation phenomena in nuclear magnetic resonance as well as in electron spin paramagnetic and ferromagnetic resonance. Furthermore, I studied the interaction of nuclear spins in metals, alloys and imperfect crystalline lattices, together with the influence of external parameters such as temperature, pressure and applied electric, magnetic or acoustic fields. The reprints fall naturally into three sections:

1. Nuclear Magnetic Relaxation
2. Paramagnetic Relaxation and Masers
3. Magnetic Resonance Spectroscopy of Solids.

Since the papers date, with few exceptions, from the period 1946–1964, they are primarily of historical interest. Preference is given to papers published in less accessible places, for example review papers in conference proceedings. Longer papers, published in the *Physical Review* or in the *Journal of Chemical Physics*, are only included in exceptional cases. The most cited paper in magnetic resonance, often referred to as BPP (*Physical Review*) is not included, because it has been reprinted in many previous collections, including the volume *Resonances* published by World Scientific in 1990 in honor of my 70th birthday. Instead, a reprint of my PhD thesis, "Nuclear Magnetic Relaxation", Leiden, 1948 is included. This thesis was reprinted and published by W.A. Benjamin, Inc. in 1961 and was available from Addison-Wesley until 1978. It has since been out of print. The material covered is essentially the same as that in the BPP paper, but is probably somewhat easier to digest as it contains more introductory passages than the *Physical Review* paper which was written for professional researchers.

Personal comments accompany each paper, providing historical insight or hindsight. Most of the papers are co-authored. It is a pleasant duty to thank all my former co-workers, graduate students, research fellows and colleagues for their enormous contributions to the results contained in this volume. I also thank the publishers of various journals or books for their permission to reproduce these papers.

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Autobiographical Notes

1. Early Childhood

I was born on March 11, 1920 in Dordrecht, Netherlands, as the second child of Auke Bloembergen and Sophia Maria Quint. My father was a chemical engineer who managed a chemical fertilizer plant which had been founded by his father. My mother had a degree to teach French, but she devoted nearly all of her energy to the rearing of our large family. Four younger siblings — two boys and two girls — soon joined my elder brother and myself. We moved to Bilthoven, a residential suburb of Utrecht, before I entered grade school.

My father rose through the ranks to become an executive director of the chemical company, but did not enjoy the work at business headquarters as much as the work in the plant, where he had more direct contact with the labor force. My maternal grandfather, with a Ph.D. in mathematics, had been a high school principal in The Hague. My mother believed in the Protestant work ethic, and our family life was much more frugal than the family income might dictate. My father concurred wholeheartedly with this modest life style, characteristic of the Dutch provinces.

My childhood was nonetheless rather comfortable and was probably considered to be a happy one. Nevertheless, I cannot recall either being happy or very unhappy during the first decade of my life. I was apparently a rather slow starter and was dubbed “Dumb Nick” by relatives. This nickname was suddenly dropped when, at the age of four, I beat an uncle at checkers.

I was often bored. When I was six, I remember, I learned the Greek alphabet, forward and backward, from an old engineering almanac belonging to my father. In grade school I often did the homework twice, for lack of something better to do. The educational philosophy in the school system was strict and did not encourage initiative. Being obedient, if not meek, I was content with getting high scores. My mother was always very busy running the large household and taking care of the smaller children. Consequently, I was often entrusted to the supervision and authority of my older brother. My parents had the wisdom to send us to different high schools.

2. High School Days

At the age of twelve I entered the prestigious municipal gymnasium in Utrecht. It was founded as a Latin school in 1474, and nearly all its teachers held Ph.D. degrees. The curriculum emphasis was, of course, on the humanities. Latin, Greek, French, German, English, History, Dutch and Mathematics were all obligatory. In the final three grades, there was, however, very good instruction in the sciences, which included laboratory sessions in Physics, Chemistry and Biology. With a friend, I set up a small private chemical laboratory in a barn. The strenuous curriculum was traumatic for the majority

of pupils, but I enjoyed most of it. I liked Latin as much as Mathematics, Physics and Chemistry were on the high end of my scale of appreciation, Dutch literature on the low end. I had time to take an active part in extracurricular activities, the school theater club and a debating society. The latter displayed an unusual degree of intellectual snobbism. We discussed natural philosophy from Leibniz and Kant to Einstein's concept of relativity, and we read poetry from Homer to Rilke. By tradition the gymnasium graduation ceremonies were held in the Pieterskerk, a church dating from the 12th century. This old tradition was discontinued in the seventies as political egalitarianism prevailed in the Netherlands. There, I delivered a valedictorian address on the reconciliation of scientific facts with spiritual values in natural phenomena. I had to wear a white tie and tails. I wore this costume to many student parties, as well, little anticipating that I would once again wear the same suit in 1981 in Stockholm.

My preference for science became evident only during my last years in the gymnasium. The emphasis on humanistic values in the tradition of Erasmus did not encourage studies in science and technology. I believe that the choice of Physics as a field for further study was at least partly motivated by the fact that I found it the most difficult and challenging subject. I was intrigued by the combination of mathematics and physical facts. Even now I find this uncanny adaptability of abstract mathematics to describe physical phenomena ever more fascinating.

Sports were not organized by the schools, but in private clubs or on an individual basis and most of my free time during weekends and vacations was taken up by them. My father, in an escape from his executive duties, took us on camping trips by kayak through most of the Dutch waterways, canals, lakes and rivers. We also made a trip in Germany and one in Sweden. We had a cabin on a lake near Utrecht where we did a lot of sailing. These activities were especially helpful during a period of two years when I suffered from osteomyelitis in a heelbone. A successful operation finally cured me completely from this disease which was dreaded in pre-penicillin days. Outside the summer season, the principal activity on weekends was field hockey. My brothers, myself and one sister took part in various national competitive leagues. During periods of frost, which are not as frequent in the Netherlands as is commonly believed, we did a lot of skating. We biked, of course, everywhere. The commute from our suburban home in Bilthoven to the school in Utrecht was five miles (8 kilometers) each way.

Although at that time I believed myself to be a well-rounded educated person, in retrospect it is clear that there was a complete void in my understanding of social relations, psychology, government and politics. I was also not exposed to music until later.

3. College and the War Years

I entered the University of Utrecht as a student of Physics in the fall of 1938. The choice was not so much governed by academic reasons as by social and family traditions and by geographic proximity. The State universities in Holland all have the same academic

standards. I joined a student fraternity-type organization, complete with hazing, and I participated in the usual drinking parties as well as the traditional student sport of rowing. The rigorous training procedures for the latter put a stop to my alcohol consumption. To the surprise of many, including myself, our lightweight (150 lbs.) crew won two regattas in its class. The traditional fraternity student life came to an abrupt end when the German troops overran Holland in May 1940. Jewish professors, including L.S. Ornstein, a professor of experimental physics whose course I had taken, were fired in 1941. All Jewish students were expelled shortly afterwards. Ornstein died a natural death in the following year. These were only the forerunners of much harsher measures of persecution and deportation that were soon to follow. Those of us left held agonizing discussions about what form of protest to mount. The University of Leiden adopted a strong stand against these measures in 1941 and was closed by the German authorities. Most of the Leiden students transferred to other universities. These, including Utrecht, remained open and functioning until the spring of 1943. The declaration of loyalty to the German authorities, which every student was required to sign at that time — but the majority refused to do so — was the last phase in the process of forced academic strangulation. At the same time, the process of physical starvation was gaining momentum. Until the closing of the university, I had been a member of the student committee that ran a central kitchen for the students. With the aid of Dutch government officials, portions of hot vegetable and potato soup were distributed, for which no ration coupons were required.

When the war broke out, my laboratory teammate, J.C. Kluyver, who later became a professor of physics at the University of Amsterdam, and I were permitted as sophomores to assist an advanced graduate student in his thesis research project. This special treatment perhaps came about due to the fact that J.C. Kluyver's father was a famous microbiologist who was a good friend of Ornstein. Together, they had founded the biophysics institute at Utrecht which was supported by the Rockefeller Foundation in New York. Jan Kluyver truly deserved the advanced standing, as he had already spent a year in physics at the University of Lausanne before coming to Utrecht. For me, it was just a lucky break. This work resulted in my first scientific publications, "On the Straggling of Po-a-particles in Solid Matter", by G.A.W. Rutgers, N. Bloembergen and J.C. Kluyver, *Physica* 7, 669-72 (1940). We were thrilled to have this contact with "real science" and to see our names in print.

The undergraduate curriculum also required some exposure to machine shop techniques and instrument making. We had to design a small transformer and build it as well. Such work did not come naturally to me, and it taught me respect for the nature of matter. These encounters with the realities of the laboratory were as much of a challenge as the mathematical equations of physics. The introductory brochure for students of experimental physics contained a warning that prospective students should not think that success was assured just because they had "played around with radios". I was worried about the obverse of this statement, as I had never been exposed to electronics or to any other physical gadgets during my classical education at the gymnasium. Although I had

no natural aptitude in this direction, I found that many skills can be acquired by perseverance, which was the same situation I had encountered in sports. I have no natural athletic abilities, but persistence in physical exercise yields results, satisfaction and enjoyment.

I made good use of the continental academic system, which leaned heavily on self-study with a minimum of formal courses. I obtained the equivalent of the Bachelor's degree (Phil. Cand.) in 1941, and became a part-time research assistant. I was able to learn something about vacuum tube electronics and the problems of fluctuations and noise in physical measurements. I wrote the lecture notes for a seminar on Brownian motion, given by Professor J.M.W. Milatz, Ornstein's successor.

Beginning graduate students in experimental physics were assigned to participate in ongoing research programs. A specialty of the Utrecht lab had always been the quantitative measurement of spectral intensities. Ornstein, Moll and Burger wrote a monograph on this subject in the mid-twenties and they were the first to determine the intensity ratios occurring in spectral multiplets, which were explained by the quantum theory of angular momentum. While most measurements were carried out by calibrated photographic plates, I was assigned to build a photoelectric amplifier. The photocell had to be cooled to suppress thermal current noise. The light was modulated by a rotating disk, the a.c. photo-current was amplified and detected in a phase-sensitive manner by a synchronous a.c. galvanometer. This work was completed in 1943 and written up for publication. It finally appeared in print a year after the war was over (J.M. Milatz and N. Bloembergen, *Physica* **11**, 449, 1946).

There was only one chair in theoretical physics. Professor Leon Rosenfeld, who had succeeded George Uhlenbeck in 1939, gave a four year cycle consisting of Classical Mechanics, Electromagnetic Theory, Statistical Mechanics and Quantum Mechanics. I entered graduate studies in the third phase and enjoyed a beautiful course in Statistical Mechanics. The graduate courses in mathematics emphasized very rigorous, purely formal procedures and existence theorems, so I taught myself some Applied Math, Mechanics and Electromagnetism from standard German textbooks. I passed tests (nearly all individual oral examinations) in these subjects as quickly as I could, because the threat of the university closing was very real. The next to last test was conducted at the home of a lecturer in mathematics, 40 miles from Utrecht. It was unsafe for young men, especially students, to be in or near the city. The police tried to pick me up one night at home, but I had been warned of the raid. I stayed in hiding with an old aunt in the countryside for many weeks in February and March 1943. I obtained my M.A. degree (Phil. Doctorandus) in April, 1943, despite not having passed the last required test in quantum mechanics. A few weeks later, the university was formally closed. This liberal gesture of my professors had the important consequence that I was not a student anymore in a legal sense. I was not required to sign a declaration of allegiance to the occupying forces nor to bear the consequence of a refusal, which was deportation or going "underground". On the contrary, I became a member of the University laboratory fire brigade, which was duly noted on

my identity card. Every person had to carry such identification at all times. In this capacity, I could move about in relative safety. Scientific work had, however, come to a virtual standstill. The laboratory was unheated and there were no new supplies; we were reduced to fiddling about with some obsolete equipment and with a clandestine radio. Nevertheless, I managed to squeeze out a brief research note which was also published after the war ("Note on internal secondary emission", N. Bloembergen, *Physica* **11**, 343, 1945).

At this point, I realized I had to learn quantum mechanics on my own. I had attended Professor Rosenfeld's course on the subject in the fall of 1942. He began with a thorough review of Hamiltonian mechanics and Poisson brackets, but by the time he was about to quantize those relationships in February 1943, formal courses were interrupted by the action of the German authorities. I submitted myself for the missed test in quantum mechanics in the summer of 1943. After an hour of oral discussion, Rosenfeld was satisfied I had some understanding of the basics of the subject. When he started to fill out the slip that I had passed, I reminded him that I had already received the degree without it. He said good-naturedly that it would have saved time if I had told him beforehand.

In the winter of 1944, the "hunger-winter", every male between the ages of 16 and 50 was likely to be picked up in the street and to be deported. I survived at home, or rather its substitute in the suburban town of Bilthoven, because my parents' house had been requisitioned by the military occupation forces. The official food ration amounted to 500 calories a day per person. My father did a fantastic job of foraging on his bicycle at far-away farms. My mother tended an ailing brother and other sick members of the family. I remember eating bitter tulip bulbs to fill my stomach. They were hard and indigestible despite of hours of boiling. I read through the book *Quantum Theorie des Elektrons und der Strahlung* by H.A. Kramers by the light of a storm lantern. Some left-over gallons of No. 2 heating oil were burned in the lantern because there was no kerosene for the kerosene lamp. The soot had to be cleaned every hour.

At the end of the war, about ten percent of the members of my fraternity had perished. Some, while active in the underground resistance, were caught by the Nazi SS or Dutch collaborators, some were shot as hostages, some were exterminated as Jews and a few died from illness and malnutrition.

4. From Utrecht to Harvard

Liberation came in May 1945. I had made up my mind that I wanted to get out of the badly ravaged country and do some "real physics". I had caught the spirit of this endeavor in spite of the war years. Where could one turn, however, since all of Europe was devastated? My elder brother suggested I take the initiative and write to some universities in the United States. This turned out to be sound advice. I wrote to the

University of California in Berkeley, the University of Chicago and Harvard University. After a few months the reply came from Berkeley that to their regret they could not admit foreign students to their campus at that time because of government security regulations. The war with Japan was still going on, a fact easily ignored by Europeans at that time. The University of Chicago never answered, but Professor Otto Oldenberg, then chair of the Harvard Physics Department, wrote that I should submit further credentials and letters of recommendation. I was duly admitted to the Graduate School of Arts and Sciences at Harvard. (At that time I did not anticipate that years later these same universities would write me letters offering me faculty positions). It was September 1945 and the problem was how to obtain foreign exchange and passage to the United States. My name was among those of 20 or 30 promising young scholars on an official dutch government Department of Education and Culture list; we were to receive priority in scholarship funds. The scholarships made available from the United States were, however, restricted to the Humanities at that time and none were in the politically sensitive subject of physics. The two physicists on the list, Abraham Pais and I, would have to wait for dollar funds.

My father was willing to put up the necessary funds in Dutch currency, but permits for conversion into dollars at that time were nearly impossible to obtain. I argued successfully with Dutch government officials, however, that since I was accepted by Harvard University and was on the selected preference list of the Dutch Ministry of Education, they should give me a dollar permit. I traveled to Amsterdam in a freight box car, because the railroads, as everything else, were still in a deplorable state. When I presented my permit to purchase \$1,800 at the Dutch National Bank Headquarters, the only place authorized to handle such transactions at that time, the official asked me who I was. He said the only people who got permits — and these were for a few hundred dollars at most — were tulip bulb dealers and professors for short business trips. Such was the state of the economy at that time. I told him I was a graduate student going for one full year. I had to wait until the end of January 1946 before a Liberty freighter, carrying grain to Europe and returning in ballast to the United States, could take me across. As we went through the British Channel, a seaman kept watch for mines, pointing to the bridge when a change in course was advisable. It was a surprisingly primitive operation.

Since these mass-produced 8000 ton ships had a ballast of only 2000 tons of sea water, they were in danger of breaking in the middle when the distance between two wave crests in an Atlantic gale was about equal to the ship's overall length. Then the middle was unsupported. Our captain avoided this danger by deviating far from the great circle route and passing by the Azores rather than by Iceland. The crossing took seventeen days and because the Port of New York was in the middle of its first postwar longshoremen strike, we sailed up the Delaware river to Philadelphia in early February 1946.

I took the train from Philadelphia to New York, where I paid a brief, unannounced visit to Columbia University. European scientists were not a common sight at that time.

Professor George Pegram, secretary-treasurer of the American Physical Society, deemed the event important enough to take me to the faculty club for lunch. In the afternoon, J.R. Dunning showed me a fission experiment and a neutron time-of-flight spectrometer. It was way over my head, but exciting after three very lean war years, both in the material and the spiritual sense. On the evening train from New York to Boston, I got into a conversation with a neighboring passenger. When he heard that I was going to do physics at Harvard University, he became very interested, stating that he was a nephew of Professor P.W. Bridgman. He was concerned where I would go on arriving at Boston's South Station so late in the evening. I had not the foggiest notion myself. He convinced me to get off at New Haven and spend the night in his home, where I met his wife and a four-year old daughter. This spontaneous hospitality in the United States was most remarkable.

The next morning my host put me on the train to Boston with directions on how to get to Harvard by subway. This was the end of our brief encounter. That same afternoon, the office of the Graduate School of Arts and Sciences helped find me a dormitory room in Weld Hall in Harvard Yard. They noted that I was late for spring term registration, but agreed to waive the penalty and I had only to pay the two hundred dollar tuition fee for spring semester. The next day, Railway Express delivered my trunk to the dormitory and I went to see the chairman of the Physics Department. Professor O. Oldenberg invited me for lunch and suggested that I meet with Professor J.C. Street and Professor E.M. Purcell, who were both ready to accept research students. My calling card in each case was a carbon copy of the manuscript on the photoelectric amplifier which was to appear in *Physica*. Since it showed I had some laboratory experience, I was given the choice of working on cosmic ray physics with Street or on nuclear magnetic resonance with Purcell. I sensed that the recent discovery of the latter effect in condensed matter might lead to very interesting follow-up research. I was also vaguely aware that the topic was of interest to Professor C.J. Gorter at the University of Leiden. I believe I had heard a discussion about it at a war-time meeting of the Dutch Physical Society. I asked further advice from two senior Dutch physicists who had also been associated with the MIT Radiation Laboratory. Sam Goudsmit was worried that my finances would not stretch far enough in Cambridge and suggested that it might be better for me to carry out my PhD research at a midwestern institution such as the University of Michigan in Ann Arbor. George Uhlenbeck told me that I could confidently cast my lot with Purcell. This proved to be an understatement. After the vagaries of war, I had arrived at the right place at the right time. The Dutch academic system and my own efforts during the unfavorable conditions had nevertheless prepared me well for the stimulating challenges at a leading US academic research institution. The friendly reception I had experienced during those first weeks in the USA had also been extremely helpful.

5. Graduate Studies at Harvard

A large class of extremely bright graduate students had entered the Physics Department at the beginning 1946 spring semester after their discharge from military service. However, most of them were not immediately ready for laboratory research, as they had to take a full load of formal graduate courses, and Edward M. Purcell and Robert V. Pound needed an extra pair of hands immediately. They were still employed at the MIT Radiation Laboratory where they were writing volumes for the MIT Radiation Laboratory Series which was to provide a permanent record of the US effort in radar development during World War II. Their workstation was in a wooden shack adjacent to the Lyman Laboratory, which housed the large magnet borrowed from Professor Street. My first task was to remove the paraffin from the 30 MHz resonant circuit they had used in their first successful experiment to detect the proton magnetic resonance in condensed matter, carried out about six weeks before my arrival at Harvard. I refilled the cavity with a mud pie consisting of a mixture of mineral oil and calcium fluoride powder. A few days later, within two weeks after my arrival, I was able to witness the detection of both the proton and the F^{19} magnetic resonance in this sample. Bob Pound kept the electronic circuit tuned and balanced, while Ed Purcell controlled the magnet current. Henry Torrey, who had made major contributions to the theory and design of the first experiment, was also present that evening. It was an exciting and auspicious beginning of the work for my PhD thesis.

My next task was to build a lock-in amplifier and an audio-generator to drive the modulation coils which were to be installed in a smaller magnet to be used in subsequent experiments. The large cavity was replaced by a more conventional LC resonant circuit at 30 MHz. Future samples would be on the order of 1cm³ rather than one liter. I had to gain familiarity with RCA vacuum tubes (instead of the Philips tubes I had used in the Netherlands), as well as with other components, English technical terms and workshop procedures. Again, I received plenty of help, especially from Gordon Voorhis, a graduate student working with Professor Street. Ed Purcell advised me to enroll in two regular graduate courses, which would enable him to get a calibration on me. I chose Statistical Mechanics taught by E.C. Kemble and Electromagnetism given by Julian Schwinger. During the war years, Professor Schwinger had made significant contributions to electromagnetic wave guide theory and to the theory of synchrotron radiation. He was about the same age as the average student in his class of 40 or 50 members. I got acquainted with Walter Kohn, C. Luttinger, R.J. Glauber, J. Blatt, R. Karplus, F. Rohrlach and many others who were later to become well-known physicists.

Later that year, I was asked to present a joint Harvard-MIT seminar on a solid state particle counter. This device had been built during the war at the University of Utrecht by Pieter J. van Heerden. He was five years ahead of me, but I had followed his work, which was based on photoconductivity of carriers created by radioactive radiation in insulating crystals (diamond), and had brought a copy of his PhD thesis with me. This

work was of interest and caught the attention of Professors Bruno Rossi, Ken Bainbridge, Victor Weisskopf and other nuclear physicists. My presentation of his work turned out to be helpful to Pieter van Heerden, as well. On the basis of his PhD thesis, he was offered an assistant professorship at Harvard.

Physics colloquia at Harvard and MIT also helped increase my English scientific vocabulary, but I was unable to understand the English of colloquium speaker Niels Bohr. In later semesters, I audited additional courses by Julian Schwinger, John H. Van Vleck and others.

My main activity and interest remained focused on the nuclear magnetic resonance experiments. Purcell was at first reluctant to define and circumscribe the scope of my thesis. When I suggested that I would like to concentrate on nuclear magnetic relaxation phenomena, he agreed wholeheartedly. The equipment was located in a basement room of the Lyman Laboratory of Physics. I spent long hours there, often working until midnight and on weekends. It became a "home away from home".

During the spring of 1946, Purcell and Pound still spent most of their time at the MIT radiation lab. In addition to building needed electronics, I made improvements in the radiofrequency bridge. When I.I. Rabi visited Harvard in April 1946, balancing the radio frequency bridge was still a tricky art. Purcell was disappointed that we were not able to show Rabi the proton NMR signal in water or mineral oil on the lock-in output meter. After they left, I tuned all the controls from scratch and within an hour or so was able to get the signal back. I rushed upstairs to Purcell's office. Fortunately, Rabi was still there and they were both happy to see the NMR signal.

In the summer of 1946, Purcell and Pound had returned from the MIT Radiation Laboratory and were able to devote more time to the project. When I once mentioned to Bob Pound that the signal appeared to change in magnitude after reinsertion of the probe in the magnet gap, he immediately moved the probe around the gap in a systematic manner. We found the narrowest resonance with the largest peak signal at the location away from the center of the gap near the edge of a pole piece. This was caused by inhomogeneities in the magnet material. The minimum line width in water and some other fluid samples was about 0.14 gauss at this "sweet spot" of minimum inhomogeneity of the magnetic field. We realized that we could not measure the true width, which was masked by the magnetic field inhomogeneity, but we could measure the relaxation time T_1 . Somehow, the motion in the fluids "averaged out" the local magnetic field variations produced by neighboring nuclear spins. Very soon the term "motional narrowing" was coined, as the three of us moved our index fingers relative to one another in space to visualize the time dependence of dipolar interactions. The variations of the local field with time were the important clue to magnetic relaxation problems.

Fortunately, I had followed a seminar on Brownian motion and noise given by J.M.W. Milatz in 1942 at Utrecht. I had written the lecture notes for it which had been distributed in mimeographed form and was therefore familiar with the concept of power spectral densities of random variables. Purcell made the valuable suggestion that the book

by Debye, *Polar Molecules*, would be relevant. He was obviously aware of the high frequency dielectric losses of polar liquids. The dielectric relaxation in polar liquids is caused by the random rotation of the electric dipole moments as described by Debye. During the fall of 1946 I worked out the spectral density of the various terms in the dipole-dipole interaction between two proton spins due to the random rotation of a water molecule. I also calculated the contributions to the local field spectral density of translational Brownian motion of spins in different molecules. I realized that the Fourier components in the local field at the Larmor frequency and at twice the Larmor frequency were responsible for the longitudinal relaxation process and determined T_1 . I remember I obtained explicit equations for T_1 in water between Christmas 1946 and New Year, in my dormitory room, when a bad cold prevented me from going to the lab.

The transverse relaxation process described by T_2 also had a contribution from Fourier components near zero frequency. I correctly surmised that a self-consistent description required that motional narrowing start when the local field spectral density has a correlation time shorter than T_2 for the static case. It need not be short compared to the resonant period.

These insights could, of course, be tested by varying the viscosity of the fluid samples. I prepared scores of samples with hydrocarbons from light to heavy oils and water-glycerin mixtures, and measured their viscosities. The experimental data for T_1 for all these samples fit the theory nicely.

All this experimental and theoretical activity took place during the last three months of 1946. During this period, I evolved from a lab assistant to a full-fledged research partner. Purcell decided that I should present the results on NMR relaxation in liquids at the annual meeting of the American Physical Society in New York City in January 1947. The abstract was published in the *Physical Review*. My mandate was to get the essential ideas of T_1 in fluids across in ten minutes. Thanks to a rehearsal at Harvard before a critical audience consisting solely of Purcell and Pound, I was able to finish the presentation in accented English before a packed room at Columbia University a few seconds before the chairman's bell rang.

During the spring of 1947, I rounded out the experimental data for my PhD thesis. The bull sessions with Purcell and Pound became less frequent. They concentrated on the design of other NMR experiments, while they confidently left further work on magnetic relaxation in fluids to me. I worked happily in the unventilated basement room, pervaded by a strong smell of acetone and sulfuric acid. I used acetone and dry ice to cool my samples to lower temperature. The acid vapor was caused by the nightly recharging of the bank of truck batteries that supplied the current to our magnet. No environmental regulations existed to retard the progress of my research. I enjoyed frequent conversations through a hole in the wall with George Pake, who had started work as Purcell's second PhD student in the next basement room. He used a permanent magnet designed by Purcell. We decided to move this magnet to the Physics Lecture Hall for a public demonstration of NMR when I was invited to present a Harvard Physics Colloquium in

April 1947. The topic was my thesis work, which combined the experimental work with the theory of relaxation based on time-varying internal magnetic fields.

Before starting to write my thesis, I made a trip by bus and train to Denver, where I stayed with the parents of Herbert Broida, with whom I shared a room in the graduate dormitory Perkins Hall. The vastness of the American plains and the grandeur of the Rocky Mountains were a new experience. On the return trip, I spent a few days in Chicago where I gave a talk at the University of Chicago. There I met Herbert Anderson, who was involved with the He3 magnetic resonance. Enrico Fermi was in the hospital with an eye problem, but he wanted to hear about the Harvard NMR results, so I paid him a visit. Back at Harvard, I met C.J. Gorter for the first time. A professor of experimental physics at the University of Leiden, Gorter was a visiting lecturer during the 1947 summer term at Harvard. I followed his lectures with great interest. They were based on his monograph "Paramagnetic Relaxation" written during the last years of the war. He suggested that the Heisenberg scalar spin-exchange coupling might also cause line narrowing, as rapid exchange of spin orientation would cause rapid variations in the internal or local field. J.H. van Vleck, had, however, calculated that the Heisenberg interaction does not change the second moment of a magnetic resonance line. He was therefore reluctant to accept Gorter's argument, which had appealed to me. I was fascinated by the discussions between Gorter and van Vleck, which were resolved when the latter showed that spin exchange greatly enhances the fourth moment of the line. Thus the magnetic resonance has a very sharp center, combined with extremely broad and usually unobservable wings.

During the summer of 1947, I presented handwritten copies of the draft of various chapters of my thesis to Gorter and Purcell, and then incorporated their suggestions for improvement in the presentation. Purcell retained a copy of my notes, which would serve as the basis for a comprehensive paper in the *Physical Review*. After I left Harvard at the end of August, I paid a brief visit to my elder brother who at that time lived with his family in Quebec City. I then returned by ship, the Veendam of the Holland America line, from New York to Rotterdam, where I was met by my anxious and proud parents. They had had to wait a long time on the pier as I was detained for many hours by Dutch customs. A suitcase full of clothing, bought by my brother in Canada, was part of my luggage. Such merchandise in the Netherlands was still scarce and rationed, and I was suspected of black market activities. The suitcase was kept by customs, but was later cleared by my father after payment of a fine.

6. Research in Leiden, 1947–1948

During our discussions at Harvard, Gorter and I agreed that we should introduce NMR investigation techniques to the European continent and extend the relaxation investigations to liquid helium temperatures. We decided we could make a quick start by

duplicating the Harvard setup. We therefore went to the Boston Radio Shack and bought all the needed electronic equipment and components, which were shipped to Leiden. The Kamerlingh Onnes lab had good electromagnets and cryostats, and I had the NMR setup going within one month after my arrival in Leiden. The machine shops there were supervised by highly professional master technicians and instrument makers. It was not appreciated, or even permitted, for scientists to do an amateurish job, as I had done at Harvard. If one went through the established channels, however, beautiful gadgets were delivered on a surprisingly fast schedule. The liquefaction of hydrogen and helium was entirely in the hands of the technicians and no mortal scientist was allowed to touch a helium dewar. Liquid helium was only available on Thursdays, so experiments requiring its use had to be scheduled well in advance. All helium gas was recycled, as supplies were scarce and expensive. All of these traditions dated back to Kammerlingh Onnes, who had organized schools for instrument making and glass blowing as part of the laboratory. Government funding was available for these purposes, but not for research. Research was carried out on the financial coattails of these schools.

Gorter was very busy as director of the large Kamerlingh Onnes laboratory, concentrating on experimental research in all subfields of low temperature physics. He left me complete freedom in the choice, design and execution of the experiments, and showed strong interest in the progress of nuclear magnetic relaxation phenomena at low temperature. I demonstrated that this relaxation in ionic crystals proceeds via paramagnetic impurities present in low concentrations. In this process, nuclear spin diffusion was an essential new concept which I introduced. At Gorter's request, I measured the relaxation time in the LiF crystal which he and Broer had used in their unsuccessful attempt to detect NMR in 1942. The progress of my research in Leiden proceeded at the same pace as it had at Harvard.

In May of 1948, I defended my PhD thesis, "Nuclear Magnetic Relaxation", in a grand public ceremony in the centuries-old aula of Leiden University. I arrived in white tie and tails according to an ancient ritual which has since succumbed to modern efficiency. I had five hundred copies of the thesis printed. About half of them were distributed among Dutch physics faculty and colleagues, family and friends. The remaining copies sold quickly through the offices of the publishing house Nyhoff in The Hague. The proceeds were just sufficient to recover the printing costs.

In the summer of 1948 I felt that the basic nuclear spin relaxation mechanisms in solids, liquids and gases were understood. I was invited to speak on this topic at an international conference in Zurich devoted to postwar advances in high energy and nuclear physics. The international renown of Bloch and Purcell was already high, but in 1948 transatlantic travel was still predominantly by ocean liner and considerations of time and expense prevented them from attending. I was honored to introduce the emerging field of NMR to a critical audience, which included, among others, Heitler, Pauli and Weisskopf. In the hallway afterwards, I heard someone comment that the experimenters had gotten ahead of the theoreticians. This reinforced my view that a combination of these two

aspects of physics is more than the sum of the separate parts.

I had experienced a great deal of cooperation from senior staff members at the Kamerlingh Onnes, including K. Taconis and J. van den Handel. I also interacted daily with younger postdocs and a number of graduate students. One of these, N.J. Poulis, was assigned to me as a graduate assistant. This resulted in a joint paper on NMR in an antiferromagnetic crystal. The paper opened up a large field of investigation, which was exploited by Poulis and others. Poulis stayed on in Leiden, and in the sixties, occupied a chair in experimental physics.

I had fairly frequent discussions with H.A. Kramers, often at his home. He held the chair of Theoretical Physics and his office was located in the H.A. Lorentz Institute adjacent to the Kamerlingh Onnes Laboratory. He presided over the Ehrenfest colloquia, which were traditionally held on Wednesday evenings. There, speakers were often interrupted by a lively and very critical audience. After I gave a colloquium there, I was allowed to add my signature on the wall of the lecture room. Among many famous names, I recognized the signatures of A. Einstein, M. Planck and F. Bloch. A. Fokker, to whom I was distantly related, and whom I addressed as "Uncle", was also a regular attendant at the Ehrenfest colloquia. Younger members of the theoretical physics group with whom I interacted included J. Koringa and N. van Kampen. There was also a graduate student organization named after Christiaan Huyghens, and I participated regularly in their social get-togethers. At a summer sailing camp they organized, I was to meet my future wife. Weekends, I often returned home to my family and friends in Bilthoven and Utrecht. I again joined the field hockey club and played with my two younger brothers on the same team in a competitive national league.

Late in the spring of 1948, I received a note from J.H. van Vleck, then chairman of the Harvard Physics Department. He informed me that I had been elected as a Junior Fellow in the Society of Fellows. This would be a three year term of complete scientific freedom. I estimated that I could finish the research projects I had underway in Leiden by the end of 1948. Although my experiences in Leiden had been very positive, life in the Netherlands was still subject to many post-war restrictions. The excitement of the academic life I had experienced at Harvard beckoned to me and the long-range opportunities opened for me by the offer from the Society of Fellows, proved irresistible. Therefore, I wrote to Van Vleck that I accepted the offer, but would like to postpone the starting date of the fellowship from July 1948 to January 1949.

When I informed Gorter of my decision, he was quite unhappy. He had secured a position as "Wetenschappelijk Medewerker van wege de Stichting voor Fundamenteel Onderzoek of het gebied van de Materie" for me. This FOM Foundation was new at the time and my position had been quite satisfactory, but Gorter could not offer any further prospects. I pointed out to him that I would return to the United States for three years and that my whole future beyond that was undetermined. At a farewell party in December 1948, he expressed the hope that I would eventually return to the Netherlands.

My departure was delayed by visa problems, about which I wired the Society of Fellows. I learned later that James Fisk, who at that time was a professor at Harvard, a senior fellow in the Society of Fellows and also a member of the Atomic Energy Commission, interceded on my behalf at the State Department. The US consulate in Rotterdam informed me that I must appear for a hearing on my visa application. The consul had received a list of instructions from Washington DC. He asked me what kind of physics I did. I responded that I was a radiofrequency spectroscopist. He smiled and joked, 'How do you spell that?', but then continued.

"What is the title of your PhD thesis?"

When I said, "Nuclear Magnetic Relaxation," I had to explain I was not a "nuclear physicist". He next asked what party I had voted for during Dutch elections. I explained I had never voted because there were no elections in the Netherlands during the German occupation and during the first post-war election I happened to be at Harvard in the United States. The consul then asked, "Had you been here, how would you have voted?" to which I replied, "Your guess is as good as mine. Probably somewhere in the center." These questions were evidence of burgeoning McCarthyism in US policy. My answers did not provide any basis on which the visa could be refused, so I was able to set sail on another freighter — a faster Victory ship this time — from Rotterdam three years after my first voyage.

7. Harvard Society of Fellows, 1949–1951

As a junior fellow in the Society of Fellows, I had three years of complete freedom to pursue my own interests. I chose to broaden my base in experimental physics by learning microwave techniques. I worked in the Vanserg building, a wooden structure dating back to World War I. It was the site of Harvard's Radio Research Laboratory during World War II, where anti-radar measures were developed. Under the direction of Professor E.L. Chaffee, the Harvard Joint Services Electronics Program was located there from 1946 on. I became a member of a group working on microwave devices. The members of the group were more oriented toward engineering and applied physics and were worried that as a foreign physicist I would be a misfit. They also had a culture of practical jokes and sometimes foul language. They were relieved when I readily adjusted to their customs. When I had to undergo gallbladder surgery in April 1949, probably a delayed consequence of my wartime hepatitis, they were kind and helpful. So were members of the Society of Fellows. I recovered rather quickly and could again enjoy the weekly lunches and dinners. The conversations were stimulating, covering all fields of academic and human endeavor. Of course, I also retained my contacts with the physics department, but these were much reduced from two years ago when my whole life was concentrated there.

I quickly learned the basics of microwave circuits and measurements. I decided to study the transition from paramagnetic to ferromagnetic resonance. This involved the

construction of a cavity with a wall of metallic nickel which could be heated above the Curie point of this metal. This switch from low temperature radiofrequency spectroscopy to high temperature microwave spectroscopy was educational and resulted in a publication in the *Physical Review*. Van Vleck read my manuscript and his favorable written comments eliminated the necessity of further refereeing. The physics community was still relatively small in those years.

During the summer of 1949, I attended a physics summer school in Ann Arbor, Michigan. I enjoyed my first airplane journey from Boston to Detroit. The school had become famous during the thirties and was still going strong. The lecturers in 1949 included L. Alvarez, R.P. Feinman and F. Seitz. I presented a seminar on magnetic resonance.

After a year, I decided to broaden my physics base still further and do some work on nuclear physics at the Harvard cyclotron. I teamed up with my friend from Utrecht, Pieter van Heerden. We decided to measure the range and straggling of the proton beam in condensed matter with energies between 35 and 120 MeV. This topic harked back to my first publication ten years earlier on the range and straggling of Po α -particles. Our paper was accepted for publication in the *Physical Review*.

During my year and a half at the Harvard cyclotron, I also became friends with another junior fellow, Karl Strauch. We have remained very good friends and colleagues at Harvard. I worked briefly on a radioactivity problem with M.S. Raben, MD. He suggested that an assay of β -radioactivity could be carried out by dissolving the source directly into a fluorescent fluid. A brief joint paper published in *Science* on a β -ray counter based on terphenyl attracted quite a bit of attention outside the physics community.

I also used my term as a junior fellow to get my first exposure to academic teaching. I joined Clayton Swenson in a special topics course on low temperature physics. He concentrated on the characteristics of liquid and solid helium, while I discussed electrical conductivity and magnetism. I realized I was definitely aiming for an academic career in the United States. I also decided that I should return to the field of magnetic resonance. I preferred the smaller scale experiments, where an individual remained familiar with all aspects of the problem, over large machine experimental physics. The field of magnetic resonance was then growing by leaps and bounds, but it was not too late for me to get back in.

There was no position available in the Physics Department at Harvard, however, where three tenured faculty members, R.V. Pound, E.M. Purcell and N.F. Ramsey, were all actively pursuing nuclear magnetic resonance. I was offered a position as assistant professor in a newly formed entity at Harvard, the Division of Engineering and Applied Physics. Professor J.H. van Vleck was chosen to be its first dean.

Fortunately, I also received an offer of an assistant professorship at the University of Chicago at a base salary about thirty percent higher than the Harvard offer. Ed Purcell advised me to remain loyal to Harvard. I argued that I knew the Harvard scene and that Harvard knew me and that the time had come for me to move on. Van Vleck and also

Harvey Brooks, who had just become a Gordon McKay Professor of Applied Physics, recognized the logic of this argument. Van Vleck said he would propose to the administration to offer me as associate professorship with academic tenure in the Division. This decision had to be made by Harvard president James B. Conant, and could not be expected before the end of May 1951. With Van Vleck's help, the situation was explained to Dean Cyril Smith at Chicago, who agreed to extend the deadline for my reply to his offer. It thus came to pass that I assumed a tenured faculty position in the Division of Engineering and Applied Physics at Harvard in the summer of 1951.

This new Division was generously supported by funds from the Gordon McKay endowment and it had been decided that the burgeoning field of solid state physics could find a home there. Thus, the limited funds available to physics could be concentrated on the needs of nuclear, high energy and fundamental particle physics. I remember reactions from some fellow physicists who expressed surprise that I was leaving "the cutting edge of fundamental physics". Personally, I never paid much attention to the distinctions between "intrinsic and extrinsic" or "pure and applied" or "curiosity-driven and goal-oriented" physics. In my opinion, Pauli's definition in the thirties of solid state physics as "Schmutzphysik" or dirty physics, was inappropriate. The debate concerning the relative merits of various subfields of physics, and of various scientific disciplines in general, continues unabated to the present day. In the current political climate, the physics community strives for more unity.

The partial separation between solid state physics and the mainstream of activities in the physics department which existed at Harvard in the fifties never bothered me. I was interested in many basic problems that presented themselves in the physics of condensed matter, and it turned out that magnetic resonance techniques were extremely useful in addressing many of these problems. In retrospect, it may have been to my advantage that I did not join the Harvard Physics Department in 1951. There I might have remained in the shadows of Pound, Purcell and Ramsey. In the Division of Applied Physics and Engineering, I could develop my own line of research at my own pace, while still benefiting from the proximity of my colleagues in the Physics Department.

The early professional recognition by Harvard culminating in the offer of a tenured faculty position in 1951 was crucial. It was not until 1956 that I was offered a professorial chair in the Netherlands. By that time, our family life and my research were already sufficiently well established that we preferred to remain in the United States. I considered the issue seriously and told Dean John H. Van Vleck that I had to make a decision from a professional point of view whether to remain "a small fish in a big pond," or to become "a big fish in a smaller pond." My scientific reputation was increasing rapidly at that time. In 1957 I was promoted to Gordon McKay Professor of Applied Physics at Harvard.

C.J. Gorter was very unhappy about my decision to stay at Harvard, and he expressed his displeasure in a letter. I explained that the Dutch offer was too little too late. It is true that in the early fifties the number of chairs in experimental physics at Dutch universities was still very limited. It is curious, however, that a second chair at Leiden, next to

Gorter's, remained vacant for years. Professor van Itterbeek, who had tenure at the University of Leuven in Belgium, served on a part-time basis in Leiden. Gorter was apparently not keen on filling it permanently. He was very generous and helpful to his subordinates, but he also wanted to remain the uncontested leader at the top. Gorter and I remained on good speaking terms afterwards, when he realized that he could not change my mind about staying at Harvard. We met frequently at international meetings, and I paid regular visits to Leiden.

Other propositions to lure me back to the Netherlands included the directorship of planned new research institutes. In one case, I was even offered the opportunity to supervise the design and construction. I made it clear that the last thing I wanted was to become involved in administrative duties associated with the directorship of a large institute. The organization at Harvard, where I was responsible solely for my own research group of moderate size, suited me perfectly. When I received feelers to join other universities in the United States, I explained that I did not want to interrupt my research and family life by a long distance move. Other locations might have other attractions, but there were never cogent reasons to leave Harvard.

Academic tenure at the age of thirty-one provided financial security for raising a family in an adopted country. My wife fully concurred with the plans for my scientific career and she has always been a source of strong support. A brief description of our family life should provide some backdrop against which my research was carried out.

8. Family Life in the United States

In the summer of 1948, I joined a sailing camp organized by the Graduate Science Student Society Christiaan Huyghens at the University of Leiden. There I met my future wife, Deli. Huberta Deliana Brink was born in 1928 in Indonesia and grew up in East Java. Her parents were Dutch, and her father, who held a PhD in Biology, was an expert in tropical agriculture and the production of sugar, coffee and rubber. Deli spent the war years in a Japanese concentration camp in Java. She was re-patriated to Holland after the war where she began her pre-medical studies at the University of Amsterdam. She was lively and full of initiative. My travels in the United States intrigued her. A mutual attraction quickly developed. I did not want to commit myself before returning to Harvard in January 1949, but she managed to join a student exchange to the United States organized by students of Vassar College in the summer of 1949. We succeeded in getting in touch with each other by mail despite considerable delays, as I had spent three weeks at the Ann Arbor summer school while she was traveling from one Vassar student address to another. Finally, she arrived in Boston after a bus ride of two and a half days from Minneapolis. We were engaged within hours after our happy 6 am meeting at the bus station. She had to return to the Netherlands soon afterwards.

We got married in Amsterdam in 1950. Her parents came over from Indonesia and our families and friends celebrated together. They speculated that we might return to Holland a year later when my term in the Society of Fellows ended. Deli and I agreed, however, that I should pursue my academic career in the United States. She preferred life there over the more confined social structures in the Netherlands, where she had never felt at home. Deli's life in Indonesia before the war had similar qualities of freedom to the ones we experienced in the United States. We had no family constraints there, but the lack of nearby relatives meant that we had no support in raising our own family. Three children were born in Cambridge, Massachusetts: a daughter, Antonia, in 1951, a son, Brink Auke in 1953, and another daughter, Juliana, in 1955. Deli showed enormous strength in taking care of the babies and me on a very tight budget. We saved up to buy a secluded, wooded lot in the town of Lexington, in a neighborhood where young professionals lived in houses designed by the Architects Collaborative under the direction of Walter Gropius. We moved into our new home in 1955, and still enjoy living in it after forty years. We became well integrated into the small community called Five Fields. Several of our neighbors have also remained there since the early "pioneering" years of the fifties. The community has a common playground, a swimming pool, and a small skating pond. Our children spent many happy hours there with their pals. They attended public schools in Lexington. Deli kindled an active interest in music in each of the three children, while I took them hiking and skiing.

Deli is a generous hostess and an excellent cook. She regularly gave dinner parties for colleagues and visiting scientists, as well as for my graduate students and postdocs. The guests often numbered between ten and twenty. In retrospect, it is difficult to overrate her accomplishments in raising the family as an immigrant, while I was devoting nearly all of my efforts to the world of physics. We became naturalized US citizens in 1958.

When our children were teenagers, we sometimes discussed our war year experiences and life as young immigrants in the US at the family dinner. Our elder daughter remarked critically, "Dad, you never emigrated or immigrated, you just moved from one physics lab to another." The underlying partial truth is, of course, that academic life has some universal international character and that scientific research knows no boundaries. Our children found their own ways and did not pursue careers in either science or academics. The eldest obtained a combined MA degree in political science and demography. The other two got MBA degrees. All three hold jobs in the corporate business world.

With the children out to college and beyond, Deli used her newfound freedom to pursue her own interests in music, creative writing and handicrafts. She regrets she did not have the opportunity to create her own professional life at an earlier age. Fortunately, we share an interest in travel and meeting people of foreign countries. During my forty years on the Harvard faculty, we enjoyed five sabbatical leaves. The first one we spent in Paris, while we parked our pre-school age children with relatives in the Netherlands. The second sabbatical we spent a whole year in Berkeley, California, where our children attended public schools. We postponed the third sabbatical to 1972–73, when our youngest

child went off to college. We traveled around the world for three months, visiting colleagues and former research associates in Arizona, Hawaii, Japan, Hong Kong and India. We also visited Indonesia, where I saw the places where Deli grew up and where she was interned by the Japanese. We also renewed contact with a family who had been friends of Deli's father for a long time. The second half of this sabbatical we spent in the Netherlands, where I was Lorentz visiting lecturer at the University of Leiden. I also worked at the Philips Research Laboratories in Eindhoven (Waalre).

We had been so intrigued with our visit to India that we decided I should accept an invitation to become Raman Visiting Professor during our fourth sabbatical leave in 1979. We stayed in a guest house on the grounds of the Raman Institute in Bangalore. From this base we traveled all around India from Kashmir in the North to Madurai and Trivandrum in the South. We traveled to Bombay, Bubhaneshwar, Calcutta, Kanpur and Dehli where we saw a multitude of temples, mountains, forests and seashores.

The second part of this sabbatical, in the spring of 1980, we spent in Munich, Germany. I was invited as a Humboldt Senior Fellow by Professor Herbert Walther, a director of the Max Planck Institute for Quantum Optics. My emotional attitude towards Germany had been frozen in a hostile state when I left Holland right after the German occupation. My relatives and friends in Holland had much more relaxed feelings as the years passed, and I decided I should get rid of my historically determined prejudices. Our stay in Munich was a success. We made many friends and we enjoyed the cultural life of the city. In fact, we decided to return to Munich on part of our last sabbatical, seven years later. Some years earlier we lived three months in Pasadena, CA, where I was Fairchild visiting lecturer. We also lived one month in Paris in an apartment that belongs to the Ecole Normal Supérieure, and one month in Pisa, where I was visiting lecturer at the Scuola Normale Superiore. This Italian counterpart to the French ENS was established by Napoleon around 1805. In addition to these more extended stays in different countries, we made a large number of shorter trips, usually during the summer months, to international conferences and summer schools in many European countries, to Japan, China, Taiwan, Brazil, Israel and elsewhere. Travel opportunities still abound after my retirement from the Harvard Faculty in 1990. We keep in touch with colleagues and friends around the world. We remain interested in different cultural manifestations, including architecture, art, literature, music, food and drink. We are more than ever convinced that human population growth presents the most serious challenge to humankind. We share an interest in planned parenthood activities, in educational opportunities for girls and woman and in world-wide population politics.

This tale of our travels should not obscure the fact that we feel most at home in Lexington, Massachusetts. We are firmly rooted there, and we are by no means perpetual wanderers. We enjoy our house, which has served as a permanent abode for forty years. We keep in close touch with our children and two grandchildren. We see local friends and colleagues on a regular basis. Deli uses her creative instincts in art and music, while I

keep in touch with science and use my Harvard office frequently. We have not retired in the literal sense of the word.

9. Member of the Harvard Faculty, 1951–1990

In 1957 I was promoted to Gordon McKay Professor of Applied Physics. In 1974, I became the Rumford Professor of Physics, and in 1980 I succeeded my former mentor E.M. Purcell as Gerhard Gade University Professor. At this point it appears appropriate to deviate from a strictly chronological order in the description of my professional life and instead to treat various aspects of it separately. Research activity has always been most important. The finding of new phenomena, the interpretation of new data and the formulating of a mathematical description for them in the elegant framework of theoretical physics has always fascinated me.

10. Outline of my Research

The logic of my decision to concentrate on magnetic resonance in condensed matter has been explained in Section 5. Solid state physics had been established as an important subfield of science and much attention was being paid to imperfections in crystal lattices. These include impurity atoms, vacancies, color centers, dislocations, etc. In 1951, magnetic resonance techniques had not yet been applied to the study of such imperfect solids. Our first program was concerned with NMR in alloys. It led naturally to several novel phenomena, including the variation and the anisotropy of the Knight shift, the influence of quadrupole interactions in solid solutions and the demonstration of both nuclear spin exchange broadening and narrowing in isotopic mixtures of metallic thallium and thallium oxide. Our research group also made early investigation on the influence of external parameters, including temperature, hydrostatic pressure, acoustic strain and externally applied electric fields on magnetic resonance spectra.

In tandem with these NMR experiments, I also pursued electron spin resonance at microwave frequencies. I was aware that virtually no attention had been paid to the problem of relaxation and saturation in ferromagnetic resonance. My familiarity with issues of relaxation and microwave magnetic resonance nicely converged in 1956 in the proposal to obtain continuous-wave maser action by saturation of a transition between non-adjacent energy levels. A flurry of activity in masers and optical masers ensued.

I made a conscious decision not to join the race to build the first laser, based on the proposals by Schawlow and Townes. We did not have enough optical know-how nor technical support to compete with concentrated efforts launched by industrial research laboratories. It is a historic fact that these laboratories succeeded in the first demonstration of many different types of lasers. When the ruby laser was first realized by T.H. Maiman at the Hughes Research Laboratories in 1960, it was clear that new fields of research in

optics were ready for exploitation. I was particularly impressed by the demonstration of second harmonic generation by a pulsed ruby laser, reported by P.A. Franken and coworkers in 1961. I recognized the birth of a new subfield of physics, soon to be designated as nonlinear optics. It is concerned with the properties of matter at high light intensity. I was aware that the Bloch equations had described nonlinear phenomena and that they were eminently suited to describe concepts in nonlinear optics. Since we didn't have a serviceable ruby laser available at Harvard in 1961, we first concentrated on the development of a general theoretical framework for nonlinear optics. When we got our ruby laser a year later, I faced the question of how to compete with the enormous activity unleashed in many industrial and governmental laboratories. I found a niche by concentrating on the nonlinear optical properties of absorbing materials such as semiconductors and metals. These were clearly less suitable for device applications and therefore had not yet received attention elsewhere.

In the years between 1960 and 1965, the activities in my laboratory switched completely from magnetic resonance to nonlinear optics. The underlying philosophy was simple. I followed the examples set by H. Kamerlingh Onnes at Leiden and P.W. Bridgman at Harvard. The former measured many familiar physical characteristics at low temperatures. When he measured the resistivity of metals, he discovered superconductivity. Bridgman measured all kinds of material properties at high pressures. We took Born and Wolf's textbook and asked how the laws of linear optics would be modified at high light intensities.

Nonlinear optics turned out to be very rich and it became an integral part of the field of quantum electronics and quantum optics, which were all spurred on by further laser developments. Many research problems presented themselves. My main task became to introduce and guide graduate students and young postdoctoral fellows in this exciting, but also highly competitive, endeavor. Through their efforts, many significant contributions reproduced in the reprint volume "Encounters in Nonlinear Optics" were achieved.

11. Graduate Students and Research Fellows

In the fall of 1951, my first semester as professor, it became clear to me that my time to carry out laboratory research was severely restricted. Furthermore, I had to start from scratch to assemble some viable NMR spectrometers. I was fortunate that very soon a young graduate student who had an interest in the structure of metals walked into my office. His name was T.J. Rowland and he said he wanted to do something different from X-ray analysis which he had been doing with an assistant professor interested in crystallography. Ted Rowland had valuable experience in the preparation of metallic samples and I introduced him to radiofrequency electronics. Thus, I was relieved from most laboratory work and data taking. Rowland obtained his PhD degree in 1954. His thesis was entitled, "Nuclear Magnetic Resonance in Metals and Alloys", and he became

a professor of engineering at the University of Illinois in Urbana. Along with Rowland, I also assumed the supervision of two of Professor E.L. Chaffee's graduate students, as Professor Chaffee was close to retirement and had extensive administrative duties. The two students had started on an experimental investigation of microwave properties of ferromagnetic materials and the work of one of them, R.W. Damon, matched nicely with my interest in ferromagnetic relaxation phenomena. Another of Prof. Chaffee's former students, Shyh Wang, had completed a thesis on high power vacuum tube oscillators and he now wanted to gain experience in the emerging field of solid state electronics. He became my first postdoctoral research fellow.

My second postdoc was A.G. Redfield. He had obtained his PhD degree at the University of Illinois in a different area of solid state physics. When he wanted to broaden his education with exposure to the burgeoning field of magnetic resonance, Professor C.P. Slichter recommended he go to Harvard. Al Redfield was a highly original and independent individual. He worked out a fundamental improvement for magnetic relaxation in solids by describing the saturation of magnetic resonance in a rotating frame of reference. Since he did everything himself, no co-authored publication resulted. For this reason, his work is not included in this selection of reprints. His presence was, however, stimulating for our growing research group.

As our work became better known through presentations at colloquia and at national and international conferences, more graduate students and postdocs became interested in joining my research group. By 1955 it already consisted of half a dozen graduate students and two research fellows. Ten years later, its maximum size of twelve PhD candidates and five or six postdocs was attained. During my entire academic career, I supervised a total of fifty-seven PhD theses, and the list of research fellows and visiting scientists also numbered about sixty at the time of my retirement.

I was most happily surprised when about eighty former associates came together for a reunion at Harvard in May 1990 to celebrate my 70th birthday and my retirement from the Harvard faculty. A volume, *Resonances*, with contributions by many of them, was edited by M.D. Levenson, E. Mazur, P.S. Pershan and Y.R. Shen, and published by World Scientific. It is clearly impractical to mention all these individuals by name and to describe their various accomplishments. My wife and I enjoy our continuing contacts with many of them. They form a worldwide network of friends, spreading throughout the United States and Europe, to Israel, India and the Pacific Rim, and back to Latin America. I shall just mention here the names of the last three of my former graduate students, all of whom obtained their PhD degrees in 1983. They are Michael D. Downer, who is now a Professor of Physics at the University of Texas in Austin, Lewis J. Rothberg, who holds a research position at AT&T Bell Laboratories in Murray Hill, New Jersey, and Thomas B. Simpson, who is a scientist with Jaycor Inc. in San Diego, California.

I was told by my wife and others that I made a mistake in not having any more graduate students from 1983 onwards. It is true that the students have kept me young and alert for decades, but I have also found that I had plenty of other ways to keep me busy

since then. My last postdoctoral coworker was Cheng-Zai Lu, a professor from Fudan University in Shanghai. I met him there in 1975, when he had just built the first dye laser in the People's Republic. He came to Harvard as a visiting scholar in 1987 under the Chinese American exchange program sponsored by the American Physical Society. He stayed in our laboratory for four years, as he did not want to return to Fudan University. He managed to have his wife and two sons join him in the United States and he now holds a position as supervisor in the laser laboratories at the Massachusetts General Hospital in Boston.

All of my former PhD's and young postdocs have found suitable employment without much difficulty. They are about equally divided between academic, industrial and governmental institutions. In retrospect, I may nevertheless have contributed to the current problem of overpopulation of research scientists. I am still writing letters on their behalf when they wish to change jobs or when they are recommended for promotion or for a distinctive award. I consider myself fortunate to have lived in the golden age of US academic research and to have benefited from my association with so many young, talented scientists.

12. Teaching at Harvard

Teaching and research are the main duties of a professor. It is often asserted that at famous universities, teaching is considered less important. This statement overlooks the fact that guiding graduate students and even young postdocs in how to carry out research is an advanced form of teaching. But undergraduate courses at Harvard are also taught frequently by senior professors. The official formal teaching obligation in the department of Physics and the Division of Engineering and Applied Physics at Harvard amounts to one half course per semester. This usually consists of three hours of classroom lectures per week. I have, as have most of my colleagues, taken this obligation seriously, and have taught both undergraduate and graduate courses. The best way to really learn a subject well is to have to teach it to a critical audience of students. In 1951 a new undergraduate course, "Introduction to Solid State Physics", was called for. I volunteered to teach this course and spent a large fraction of my time on it during my first semester on the faculty. There was no suitable textbook available at that time. I believe the first edition of C. Kittel's text with the same name was published in 1953. I read more advanced books, including F. Seitz's *Solid State Theory*, *The Theory of Metals and Alloys* written by Mott and Jones, W. Shockley's book on semiconductors and J.C. Slater's book *Chemical Physics*, to extract the notes for my introductory course. Obviously this effort greatly benefited my magnetic resonance research in solid materials. I also taught "Electron Physics", originally developed by my mentor, Professor E.M. Purcell. Later, I taught an advanced course on magnetism, which J.H. van Vleck and I developed jointly. I also gave an undergraduate course on alternating current circuits theory and so learned some useful fundamentals of electrical engineering.

In the sixties, I developed a new course, "Fluctuation Phenomena", together with my colleague (and former student), P.S. Pershan. Our plans to transform our lecture notes into a book, however, never materialized. In 1963, I gave a special graduate course on the newly emerging field "Quantum Electronics". This course attracted many auditors, including colleagues from other institutions in the Boston area. Over the following decades, the course was updated and renamed, "Modern Optics". It concentrated on the physics of lasers and nonlinear optics. Well-known textbooks such as those of A. Yariv, Y.R. Shen and others, were of course not available in the sixties and early seventies. Again, teaching and research were mutually beneficial. For many years I also taught the advanced courses on Maxwell's Electromagnetic Theories, listed as Physics 232A and B in the Harvard catalogue of courses. In the last decade of my teaching career, I tried my hand again at some undergraduate courses. I brushed up on a classical subject, thermodynamics, that I had largely forgotten, and I also taught introductory quantum mechanics of atoms and molecules. I always found that teaching a course for the first time required a lot of work and was also rough going for the students. The second year the lectures were much improved, and the third time around it was smooth going, but it also became boring. Then I would volunteer for a different course.

I took my teaching seriously and kept interference of my teaching schedule with outside travel engagements to a minimum. If absence was unavoidable, I would ask a colleague to replace me, or I would reschedule the lecture for another time. I was fortunate that I never missed a lecture due to illness during my forty years of teaching at Harvard. I graded all exams and term papers personally, even if the enrollment in the class was forty or fifty students. A teaching fellow would grade and discuss homework problem sets. My teaching duties also helped me to give clearer expositions on our research work at scientific meetings and at colloquium talks.

13. Administrative Duties at Harvard

In addition to the central themes of teaching and research, closely related activities demanded increasing amounts of time. Fortunately, the administrative aspects related to my research were minimal during the fifties and sixties.

My research space occupied one half of the third floor of the Gordon McKay research laboratory, about five thousand square feet. Since I did not wish to expand further, space was never a serious problem. Neither was the funding of my research. I started out in 1949 as a participant in the US Joint Services Electronics Program at Harvard, which was then directed by Professor E.L. Chaffee. The program gladly accepted additional research in magnetic resonance under its umbrella. I never had to write research proposals, only brief progress reports. When the solid state maser came along five years later, this liberal policy of the US Department of Defense was amply justified. The switch from magnetic resonance to optics research in the early sixties was again never the

subject of a formal refereed proposal; we simply presented our results at the annual reviews of the Joint Services Electronics Program. Additional funding came from another umbrella grant, the DARPA Materials Research Program. The fifties and sixties were, of course, the golden times for governmental research support in the physical sciences.

I had to do some proposal writing in the seventies for additional smaller contracts from the Office of Naval Research and from NASA, and also for the annual renewal proposals of the umbrella contracts. This was not too onerous, as I proposed to do things for which we had already obtained some preliminary results in the preceding year. The funding agencies were happy because our "proposed" research turned out to be successful, and we could use part of the funds to try out new things, not yet officially proposed, to be incorporated in next year's proposal. From 1967 to 1983, I served as director of the Harvard Joint Services Electronics Program and I obviously had to spend more time on administrative matters, including annual renewal of proposals and reviews. I received a plaque from the service representatives in November 1983 with the commendation, "In recognition of his outstanding professional and administrative leadership in directing Harvard University's important share of the Joint Services Electronics Program for the past sixteen years."

I never received financial support from the National Science Foundation. Academic research funding by the Department of Defense was in the hands of highly knowledgeable professionals with a broad view. They were not restricted by tight congressional supervision. I was, for example, permitted to support a visiting scholar from the Soviet Union in the sixties, and scholars from the People's Republic of China in the eighties. National Science Foundation funds could not be used for non-citizens. Some of my US graduate students received partial support from their respective countries.

With my predilection for small scale experiments, our equipment budget remained modest. Some of the most basic results were obtained with rather primitive instrumentation, harking back to my early laboratory days with "string and sealing wax". My associates had to convince me that periodic upgrading was essential.

Harvard University has a strict policy that no classified research can be carried out on its premises. Our research was open and could be published without prior approval. I have never been apologetic or uneasy about the source of my research funding being the Department of Defense. It is true that research in magnetic resonance and lasers had and still has military significance. It is, however, a more important fact that applications coming out of this type of research have on balance been extremely beneficial. Magnetic resonance imaging and all kinds of laser surgery save the lives and increase the well-being of millions of individuals. Optical fiber communications improve worldwide contacts between individuals, between governments and between business corporations.

Other duties at Harvard involved service on committees to select new prospects for faculty appointments and to consider promotions. These tasks were time consuming but important. I also served on many an examination committee for the oral qualifying and final PhD thesis defense involving my own students as well as those of colleagues. This

was often followed by writing letters of recommendations for job applications. Deans and colleagues realized that I was not keen on doing more administration than strictly necessary. I was a member of the Division's library committee, which involved minimal work.

When the student revolts of the sixties reached Harvard in 1967, I did take an active part in the faculty deliberations about student grievances. A Faculty council was established and I served on it during the first critical eighteen months of its existence under the chairmanship of Dean of the Faculty John Dunlop. Positive action was taken on several legitimate requests, but student demands for direct participation in the governance of the university and in new faculty appointments were resisted. The limits of academic freedom of expression were defined and distinguished from academically unpermissible behavior. The student unrest at Harvard was brief compared to that of many other institutions, and led to several improvements in the curriculum and in student-faculty interaction. There were no long-term damaging consequences at Harvard.

Toward the end of my academic career, I was asked by the Harvard University's legal counsel to serve on a very special committee of two. A complaint had been filed against the president of the university, Derek Bok, by a faculty member who had been denied promotion to tenure. The rules of the Faculty council, adopted by the Faculty of Arts and Sciences, had no provision for hearing such an exceptional case. Two senior members with the rank of University Professor were chosen to investigate the complaint. Helen Vendler, a highly respected scholar in English Literature, and I had the demanding but interesting task of interviewing the members of the appointment committee, high level administrators, including the president himself, and ranking members of the department involved with the promotion. Helen Vendler and I agreed on our conclusions. She edited my contributions and wrote the final draft of our report. The Harvard Corporation, the top governing body of the university, accepted our findings and was able to settle the dispute internally without the intervention of courts of law. The university now has this workable mechanism in place should a similar situation arise in the future.

14. Other Professional Activities

Professional activity outside Harvard rose with my research reputation. I served as Associate Editor of the *Physical Review* (1956–1958), the *Journal of Chemical Physics* (1960–1962), the *Journal of Applied Physics* (1964–1967) and the *IEEE Journal of Quantum Electronics* (1965–1970). In addition, I was Advisory Editor of *Optics Communications* (1969–1989), *Physica* (Amsterdam, 1975–1990), *Nuovo Cimento* (1979–), and of the *International Journal of Nonlinear Optical Physics* since its inception in 1990.

I had to serve as referee for scores of manuscripts submitted to these and other journals. I also received, for review, countless research proposals from various funding organizations. Gradually these tasks became burdensome and I made it clear when I no

longer felt up-to-date or lacked interest. Now I only receive requests for review in exceptional cases; when two or three referees are in disagreement about the merits of a proposal or paper, for example.

Invitations to speak at major national and international scientific meetings also began to arrive in the early fifties and increased as my research group's work expanded. Unless the timing seriously interfered with my teaching duties at Harvard, I accepted most of these, as I found the contacts with colleagues interested in the same or related subject matter most valuable. Attending these meetings stimulated my train of thought and was often very beneficial to the progress of our own work. I realized more and more that physics is an international enterprise and that recognition outside Harvard was important to me.

I enjoyed being a colloquium speaker at many universities in the United States with prominent physics departments. Even though my active involvement in research has almost vanished during the past decade, I still receive many invitations to speak. I understand this has more to do with name recognition. Keynote lectures and historical overviews are, however, also important. My lectures are now more oriented towards general audiences rather than to peers in a research specialty.

Another outside activity related to my research involved consulting for industrial and governmental organizations. This started in 1953, when I had a close association with the MIT Lincoln Laboratories where the division on microwave radar research included an active group in magnetic resonance under the supervision of Professor B. Lax. I remained in touch with the group for several decades during the development of masers and lasers. In 1953 I also became a consultant for Schlumberger Well Surveying Corporation. Professor Purcell, who was not interested in the job, had suggested my name. Schlumberger studied the possibility of using nuclear magnetic resonance techniques in bore holes to distinguish between oil and water in geological formations. This consulting job involved travel by train and car to Ridgefield, Connecticut, where the company had a nice modern research facility. In later years, I occasionally had to travel to their headquarters in Houston, Texas. The company was of French origin and I enjoyed the many French touches of the competent leadership. The financial remuneration was an essential supplement to my academic salary and was used to make the down payment on our house in Lexington. I was fascinated by the relationship between scientific research and engineering applications. Unfortunately, the project of NMR logging of oil wells turned out to be impractical at that time. My services were not too useful and were terminated in the early sixties. I understand that modern techniques, similar to those used for MRI, magnetic resonance imaging in medicine, have made NMR logging oil wells important. These are based on differences in T1 and T2, the longitudinal and transverse relaxation times, of protons in oil bearing formations.

Consulting activities since the early sixties were mostly related to my research with lasers and will be referred to in the volume "Encounters in Nonlinear Optics." I found my industrial and governmental consulting activities stimulating and I enjoyed the

differences in style between business corporations, governmental institutions and academia. I consider myself fortunate that my two major fields of interest, magnetic resonance and nonlinear optics, have led to so many practical applications. The consulting activity undoubtedly had a positive influence on my research program at Harvard.

15. Professor Emeritus, 1990 —

Retirement has not brought sudden change, but it has provided even more freedom than academic life had already permitted. As I reminded my Harvard colleagues at my retirement dinner, "A professor can do as he pleases, but a professor emeritus can do as he damn well pleases." I still keep my Harvard office and enjoy the continuing contact with Harvard colleagues. Since I have no teaching obligations and no longer provide guidance to laboratory research, I have time to read about many new developments in nonlinear optics. I still get many invitations to attend conferences. Instead of reports on new research results, I now present keynote lectures or historical overviews. They are adapted to the main thrust of each individual conference and therefore require a fair amount of background reading.

During the year of 1991, I served as president of the American Physical Society. I was elected as vice president in 1989, served as president elect in 1990, and as past president in 1992. This four-year stint involved quite a bit of work. During my forty years of membership in the Society, its functions have changed and grown enormously. While in early years the presidency was mostly an honorific and ceremonial position, it now requires a substantial amount of time. The APS has over forty thousand members, of which over ten percent are from other countries. Its journals are still growing and becoming the favored channel of communication for physicists throughout the world. The society is concerned with the unity of physics and promotes interaction between different subfields. It also has established contacts with physical societies in many other countries. It is now actively involved with social issues, including arms control, environmental problems and minority participation. Employment issues and job opportunities for physicists of all ages demand increased attention. I found it interesting to become an active participant in the affairs of the society. It provided the opportunity to get acquainted and to interact with a very broad spectrum of individuals from all subfields of physics in a wide variety of institutions. I was glad, however, that this service to the society was postponed until the end of my professional career, as earlier it would have caused an undue interruption in my research activities. During my tenure as president, the Council of the American Physical Society voted to start a fund raising campaign jointly with the American Association of Physics Teachers. The main purpose of this Campaign for Physics is to improve education in the physical sciences at the pre-college level. I was asked to chair the Campaign Advisory Group and as a consequence am still involved with this new venture. I have learned about the realities of fund raising. In the process, I have had the

opportunity to meet with a number of high level executives at various corporations and foundations.

Opportunities for travel continue, so far unabated. Since my retirement, my wife and I have made trips to the Netherlands, Indonesia, France, Japan, Mexico, Hong Kong, the People's Republic of China, Sweden, Germany, Italy and Taiwan. In addition, I have traveled without her to Korea, Egypt and Greece. In all cases, meetings with old and new colleagues and sightseeing are combined with lectures at scientific gatherings. We spent the months of February and March 1995 in Orlando, Florida where I had a temporary job at the Center for Research and Education in Optics and Lasers. I found it fun to interact with faculty and graduate students there, and to re-immense myself in current research problems in nonlinear optics. Looking forward to 1996, we already have tentative plans to visit Tucson, Arizona, and Ireland.

My wife and I continue to pursue our interests in arts and science respectively. We have, of course, more leisure time for reading and sports, and for seeing children and grandchildren. We know we are slowing down, but we keep body and mind in motion and have avoided boredom. We hope our state of health will permit us to remain active for years to come.