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MARTIN DEUTSCH
1917—2002

A Biographical Memoir by

LEE GRODZINS

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MARTIN DEUTSCH

January 29, 1917—August 16, 2002

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IN 1951 MARTIN DEUTSCH discovered positronium through a study of the behavior of positrons in gases. He then measured the decay mode and the hyperfine structure of this most elementary atom of an electron and a positron, a particle and its antiparticle. These elegant experiments were the crowning achievements of his wide-ranging, seminal contributions to nuclear science using radioisotopes in tabletop experiments. In the 1960s Martin turned his attention to elementary particle studies, starting with experiments at the newly completed 6 GeV Cambridge Electron Accelerator. He was actively participating in neutrino physics when he died on August 16, 2002.

I met Martin in 1955 during one of his periodic visits to Brookhaven National Laboratory. In 1959 not long after we collaborated on a test of parity violation he invited me to join the faculty of MIT, where for decades we had adjoining offices. I have tried in this memoir of Martin's contributions to illuminate the personality of this gifted, erudite, multifaceted man through personal reminisces as well as anecdotes that Deutsch himself wrote in a collection of fragments he called "Opusculi" (Deutsch, n.d.)

Martin Deutsch was born on January 29, 1917, in Vienna, Austria-Hungary. His father, Felix Deutsch, had been Sigmund Freud's physician when he met and married Helene Rosenbach in 1912. Helene was Freud's last student and stayed a member of his inner circle for decades. She became internationally known for her many contributions to psychiatry, especially her 1944 two-volume masterwork, *Psychology of Women,* which remains a primary reference. Felix, while never achieving Helene's eminence, was a pioneer in psychosomatic medicine. Martin's upbringing in a concentrated atmosphere of psychiatric conversations had a lasting effect. In conversations with me he would often conjecture on the underlying reasons behind his students' and colleagues' decisions, both personal and professional.

Martin was an only child, and precocious. "My mother," he said, "was sure I was a singular point in the universe." He went to private schools in Vienna, Zürich, and Berlin, getting a deep education in the classics and science. From an early age he was fluent in English, French (his favorite language), and various dialects of German. In later years he became fluent as well in Italian and learned sufficient Swedish and Russian to "get by," as he put it.

Martin's teen years coincided with the antifascist resistance movement, which he joined. He never discussed his role beyond saying that he acted as a courier between Germany, Switzerland, and Vienna and that it was dangerous. In 1935 while Helene was on a professional trip to America, Mussolini invaded Ethiopia. Martin, who accompanied her, was more aware than his parents that the anti-Semitic events in Germany were a grave danger to Jews in Austria. He insisted that they must not return to Austria. Felix and Helene settled in Cambridge where she had friends and colleagues. Martin entered MIT, still uncertain as to whether he wanted to be a chemist or a physicist. In the end he chose physics, graduating in two years.

During the summer of 1937 he acquired a 1932 Ford coupe for $70 and set out to see more of the country than just the East Coast. Arriving in Glacier National Park, he found a collection of books and journals on theoretical physics on the table in the office of the lodge. In his own words from Opusculi:

I was only a college Junior and an experimentalist, not a theorist, and could really not benefit from this opportunity, but the evidence of the ubiquity of physics in beautiful places was encouraging. It proved to be only a preamble to what was to happen two days later.

When I returned to the hut for lunch on the third day of my sojourn (after hiking for a day above the timberline) I found two couples sitting at a table, conversing in German. I joined them out of curiosity and learned that they were on a vacation trip driving through the mountains, had climbed from the Going-to-the-Sun camp in the morning and wanted to take a brief hike from the pass before returning the same evening. I offered to guide them on a particularly scenic trail and this led to the usual question about my occupation and hence, when I said that I was an undergraduate student, about the subject of my studies. When I answered "Physics" the older of the two men said "What a coincidence! We are also physicists. This is Edward Teller and I am Hans Bethe." In 1937 physicists were as rare as Edelweiss. As I recall, the membership of the American Physical Society was under 4000, yet this was my second encounter in three days! Unfortunately, my education had not progressed to the point where the names of Teller and Bethe meant anything to me. Eight year later, in Los Alamos, we [Martin and his wife Suzanne] had quite a bit of contact with the Bethes, somewhat less with the Tellers. At one occasion—it must have been in the late sixties—I remarked to Bethe that the age difference between us seemed so much smaller than it had in Glacier Park. He conceded that this was true but '"in a few years it will grow again."

Martin stayed at MIT for his Ph.D. In 1939 in the midst of his studies he married Suzanne Zeitlin just after she finished her master's degree in social work at Simmons College. His thesis, "A Study of Nuclear Radiations by Means of a Magnetic Lens Beta Ray Spectrometer," was submitted in 1941. With the strong support of Robley Evans, his thesis supervisor, Martin stayed at MIT for two postdoctoral years, exploiting the new technique he had invented for measuring the energy of gamma rays, an invention that is still listed in the *World Almanac's* list of significant inventions. In 1943 he finally got the necessary clearance to work at Los Alamos, where he was assigned to Emilio Segre's group. On his very first day he showed off the deductive powers that would continue to be admired throughout his career. In his own words from Opusculi:

Segre had brought (his) counters and associated electronics from Berkeley to Los Alamos with the intention to continue his experiments but found that the background counting rate was more than twice that it had been in California. It was expected on the basis of my previous experience that I would design and build an anti-coincidence shield of counters to reject particles arriving from outside the counters.

Martin's clearance badge had not yet arrived so he stayed within the security area while the others went offsite for lunch. If he went out, he might have difficulty coming back in.

I pondered my assignment and decided to try a quick-fix that might, at least, get us started on a solution. My reasoning was as follows: Cosmic rays arrive from extraterrestrial space approximately isotropically, but the thickness of atmosphere that they have to traverse before reaching the surface depends on their angle of arrival. I had never worked on cosmic ray questions but, since my problem was due to the difference in air absorption between Berkeley and Los Alamos, it seemed plausible that the radiation at Los Alamos' altitude would be predominantly close to vertical. I noted that the Geiger counters were mounted horizontally. I upended one of the sixty pound lead "pigs" containing the counters and counted the background. To my surprise, it was reduced to a rate approximately equal to the Berkeley value. When the colleagues returned with my sandwich, they naturally wanted to know whether I had thought about the problem. No, I had not, at least not very hard, but had, instead solved it. Then I bowed to acknowledge the applause.

Shortly after this initial tour de force I was given the opportunity for another virtuoso performance. This time the assignment came from a committee charged with radiation monitoring at the laboratory. The instruments were calibrated at the Manhattan District "Metallurgical Laboratory" in Chicago. I was confronted with the disturbing observation that attempts to verify the standards indicated smaller amounts of radium than certified by Chicago. Fortunately, having grown up in an alpine country, I was attuned to looking for effects of altitude. "Was the discrepancy," I asked, "by any chance 20%?" "Yes it was." "Are we measuring the radiation with an air ionization chamber?" I never got a formal answer. People were either laughing or shaking their heads because the solution of our problem was obvious.

Out of such little triumphs chutzpah and laziness are created. I have told these stories not only to boast what a very smart young man I once was but also to draw some morals about problem solving. One is the great power of a fresh approach.

The Manhattan Project ended in 1945. The Deutsches returned to Cambridge and Martin returned to MIT. At about the same time Victor Weisskopf, whom Martin had known at Los Alamos, also joined the faculty. When these two Viennese physicists compared their histories, they found that they had had the same *gymnasium* teacher. One of Deutsch's favorite jokes was that their teacher "told each of us that we would end in the gutter."

The Deutsches bought a handsome house in Cambridge, a house in which he and Suzanne raised their two sons, L. Peter Deutsch and Nicholas Deutsch, a house within bicycling distance of MIT, the house in which Martin died.

NUCLEAR SPECTROSCOPY

The cyclotron at MIT had just been constructed by Stanley Livingston when Martin began his Ph.D. thesis work under the supervision of Robley Evans. Much was known at that time about the ground states of stable nuclei, but the vast territory of the quantized excited states of stable and unstable nuclei was uncharted. The study of the level structure and the radiations from them required new approaches, new techniques. Martin Deutsch developed many of them,

Martin set out to determine the disintegration schemes of radioisotopes that were being used for biological purposes. That goal required a quantitative knowledge of the beta ray emanations, which could be studied by existing spectrometers, and the energies and intensities of the gamma radiations. Until Martin invented the short, magnetic-lens electron spectrometer the gamma rays were detected by Geiger counters and the energies determined from measurements of their mean free paths through absorbers. That method, which could only be applied to nuclear decays with one or two dominant gamma rays, gave crude, often incorrect values for the energies and intensities. Martin's magnetic spectrometer determined the energy of the photoelectrons emitted when the gamma rays from the radioactive source interacted with a thin, high Z absorber. The energy of the gamma ray was the sum of the measured energy of the photoelectron plus its known binding energy. Martin's results were accurate. He could assign quantitative values to branching ratios and disentangle moderately complex decay schemes.

In 1941, the year of his PhD, Martin and Arthur Roberts published "Energies of Gamma Rays from Br^{82}, I^{131}, I^{130}, Mn^{58}, Mn^{54}, As^{74}," which reported the first results of the spectrometer. Martin's second publication, also in 1941, was on the disintegration scheme of a radioisotope of yttrium. This paper was the first of a series that ended in 1946 with "Disintegration Schemes of Radioactive Substances. IX. Mn^{52} and V^{48}." Every one of the series used his spectrometer to measure the energies and intensities of the gamma rays and beta rays. From these data Martin would construct the first accurate decay scheme for that isotope. Martin rarely, if ever, came to a false conclusion. I recall a conversation with a well-regarded experimentalist at Brookhaven National Labs who had been repeating and greatly extending, with far more sophisticated apparatus, some of Martin's work. Martin, he said, never drew a wrong inference in the dozens of Physical Review papers he wrote in the 1940s and 1950s.

Martin wrote some 60 papers from 1941 to 1961. Examining the list recently, I was struck by the fact that the lens-spectrometer paper is the only one devoted to technical innovation. His other technical advances were always described in separate sections of papers that focused on the physics. Martin was an old-timer. He made apparatus to answer a physics question. He made most of the apparatus himself because that is what a physicist did. His hero was Rutherford.

Throughout his career Martin was a student of the forefront techniques useful for his experiments. In the 1940s he mastered the techniques of nuclear chemistry in order to make his own radioactive sources. In the mid-1950s he would periodically visit Brookhaven Labs to spend a few days consulting with Willy Higgenbottom, director of instrumentation, on the latest in the rapidly emerging world of transistors and transistor-based circuits. On returning to MIT he would set aside time each day to design and test circuits that would replace the vacuum tube circuits that he had previously built.

I also learned in an offhand way that Martin had made the lens spectrometer that he invented, and until about 1970 it still sat in a prominent place in his lab. On a visit to a major nuclear facility I was shown an experiment using what appeared to be an exact copy of Martin's lens spectrometer. His comment on my observation was that as far as he knew, every short-lens spectrometer in the world replicated the 6-inch-diameter brass tubing described in his paper. The community seemed to be content with mimicking his exact dimensions even though Martin's paper indicated that a larger diameter would have been more effective. Martin said that he used the 6 inch tubing because it was lying around the lab and that he was in a hurry. My own view is that Martin, who could be quite frugal, probably weighed the cost of a new tube against the gain the larger diameter would bring and found the latter wanting.

The papers he and his students wrote during that period were mainly concerned with energy levels and branching ratios. Unraveling that taxonomy was necessary for practical applications of the radioisotopes but was insufficient to test nuclear models or to make predictions. To gain further insight required knowledge of the level's angular momenta, magnetic moments, lifetimes, and reflection symmetries. Measuring these properties was the essential next step to which Martin turned his faculties of invention, his mastery of techniques, and his full energies. He needed them because in 1946 there were no experimental methods for measuring a nuclear state's angular momenta or its parity; no methods fast enough to measure the submicrosecond lifetimes of nuclear states; no methods even suggested for measuring the magnetic moments of excited states.

Don Hamilton published the theory of the directional correlation of gamma rays in 1960. He showed that the correlation between the direction of emission of the first gamma ray and the direction of emission of the second can be used to deduce the intrinsic angular momenta of the involved states. Attempts to verify the predictions failed, however, and it was theorized that the failure was caused by the masking perturbations from internal electromagnetic fields. In 1947 Brady and Deutsch measured the correlation that others had failed to find.

The seminal Brady-Deutsch paper was a tour de force, well described by Hans Frauenfelder in his 1955 monograph on the angular correlation between nuclear radiations.

These first experiments by Brady and Deutsch were done with Geiger counters. A major improvement was their introduction of the scintillation counter in 1948. It is difficult today, only seven years later, to realize how tedious were the gamma ray-gamma ray correlation measurements with Geiger counters. A run which now takes a day would have taken more than ten years of continuous work with Geiger counters.

Martin introduced scintillation counters and their uses into nuclear spectroscopy (1947). He always deprecated his role: "I was not the creator," he said, "just the mid-wife." True enough, but Martin's widely discussed physics with scintillation crystals was instrumental in popularizing many of the now common scintillators.

Martin's innovative use of the new scintillators reminds me of a conversation I had with Henry Kendall that gives a taste of what it must have been like to be one of Martin's students during those years. Henry, who had recently received his Ph.D. under Martin's supervision, was collaborating with me during the summer of 1955. He remarked that Martin only respected a physicist who was more capable than he in some area. "Henry," I said, "I know that Martin respects you. What can you do better than he?" Henry's reply: "I can make better sodium iodide detectors than he can."

In his third paper with Brady, Martin made the suggestion that the magnitude and sign of the magnetic moment of an excited state could be determined by applying an external magnetic field to the nucleus and measuring the angle through which the angular correlation rotated. The magnitude of the angle would be the product of the Larmor precession frequency times the mean life of the state. Martin did not follow up his own suggestion, but the much cited paper opened up a subfield of nuclear experimentation.

POSITRONIUM

Throughout the 1950s Martin's group continued the study of radioisotopes and Martin himself continued to introduce new techniques; his method for measuring subnanosecond lifetimes is still in use. But Martin's primary focus in the early 1950s was the investigation of a positron's interactions as it stops and annihilates with an electron. His goal was to find positronium.

Theorists predicted that prior to annihilation, the positron would bind to the electron to form an atom whose ground state would have zero or unit spin, depending on whether the spins of the electron and positron were antiparallel or parallel. The former, a singlet state with spin zero, would decay in about a tenth of a nanosecond into back-to-back, 511 keV gamma rays. The lifetime of the triplet state would be about a thousand times longer since its one unit of spin required that it decay into three gamma rays. Positronium, as it was named, would be the simplest of atoms, more amenable to theoretical calculations than the hydrogen atom with its heavy, complex-structured proton. Papers were written predicting the properties of the ground and excited states, and the transitions between them. In the late 1940s a lifetime of less than a microsecond was a daunting obstacle to identifying positronium and determining its properties. The breakthrough came when Martin realized that the properties of positronium are more easily observed in gases than in solids. He reasoned that the longer-lived triplet state probably did not survive its predicted lifetime in a solid, but might in a gas.

Theory predicted that the probability of the singlet's two-photon mode of annihilation should be proportional to the density of the electrons in the medium. In 1949 Deutsch and his student Shearer confirmed the pressure-dependent prediction. Martin's later series of experiments unraveled the phenomena occurring in the slowing down of the positrons. It was this understanding that was instrumental in Martin's elegant, unequivocal demonstration of positronium in 1951.

Martin chose the radioactive nucleus Na^{22}, which decays by emitting a positron followed within a few picoseconds by a 1.27 MeV gamma ray. His tabletop apparatus had five basic elements: a chamber filled with a gas to a known pressure; the radioactive source (that he prepared himself) attached to an inside wall of the chamber; a scintillation counter outside the chamber wall and directly behind the source to detect the 1.27 MeV prompt gamma ray; a scintillation counter inside the gas chamber, and shielded from direct radiation from the source so that it only detected the 511 keV radiation when the positron annihilated after moving past the shield; and a timer. The timer started when the high-energy gamma ray was detected by the outside detector and stopped when an annihilation gamma ray was detected by the inside detector.

In some fraction of the radioactive decays the following sequence took place: a gamma ray was detected in the outside detector signaling that a positron had been emitted a few picoseconds earlier. The timer started and continued to run until the inner detector fired, signaling that the positron had annihilated. The duration measured the total time it took for the positron to slow down as it moved passed the shield, come to rest near an electron, and annihilate.

When Martin increased the gas pressure in the chamber, he discovered that the measured lifetimes depended on the composition of the gas. For some gasses, for example NO (nitrous oxide), a molecule with an odd number of electrons, the lifetime diminished in inverse proportion to the gas pressure, as predicted for the two-photon mode of decay. In other gasses, such as N_2 (molecular nitrogen), which has an even number of electrons that pair to a total spin zero, he found a strong component of 1.4×10^{-7} seconds that was independent of pressure. This independent component had the anticipated lifetime of positronium of spin one. Martin then proceeded with an unequivocal proof. He introduced a small amount of NO into the chamber that contained N_2 and the pressure independent component disappeared; a 3 percent addition of NO reduced the pressure-independent state by three orders of magnitude.

In sum, Martin had quenched the long-lived, parallel-spin positronium by introducing molecules with an electron that could readily convert the excited state into the lower-energy positronium with antiparallel spins and a short lifetime. Martin was especially proud of this experiment. Years later he said to me, "This one time I did a Rutherford experiment."

Martin's paper of March 1951 announced his discovery. A few months later he reported his accurate measurement of the lifetime of the three-quantum decay of positronium, confirming its predicted value. A third paper that year, by Deutsch and Dulit, reported the first measurement of the fine structure of positronium, obtained by measuring the quenching of the long-lived state as a function of an applied magnetic field. Just a few months later, and just days after Martin turned 35, Brown and Deutsch reported the radio-frequency-based measurement of the hyperfine structure separation to an accuracy of about 10 percent, sufficient to reveal the effect from quantum-electrodynamic radiative corrections (1952). Two years later R. Weinstein, M. Deutsch, and S. Brown reported the measurement of the hyperfine separation to a precision of a part in 5,000, confirming the most accurate prediction of QED (1954).

Martin's magical year at the age of 34 did not win the Nobel Prize though he was nominated at least twice. Isidor Rabi, reminiscing in 1985, told me that Martin would no doubt have won if he had continued his studies of positronium. Perhaps so, but it was not Martin's style to feed long at a table he had set. For him it was enough to break the first ground, plant the first seed, taste the first fruits. And so with positronium, as he had with unraveling disintegration schemes of radioactive nuclei and measuring spins by angular correlation of gamma cascades, Martin pioneered the basic measurements, uncovered the first physics, and left for others the continuing feast.

THE LATER YEARS

Leaving the field of nuclear physics was not a difficult decision. Sometime during this period he confided to me that when he returned from Los Alamos he wrote in his journal a list of nuclear phenomena he wanted to investigate, questions he wanted to answer. He went on to say that he had looked at that list recently and found that he had answered all the questions he had set for himself. "It was time," he said, "to move on."

The question of what research to do next, however, was not an easy decision. He thought about it and discussed his options. My own view, which I shared with him, was that his expertise and interests fit well in the still nascent field of biophysics. He was, after all, expert in physics and chemistry, in electronics and computer programming. And he was brilliant at making correct deductions from minimum information. In the end he decided to join the Cambridge Electron Accelerator consortium (CEA) to do experiments with GeV electrons and photons.

Martin led experiments on neutral-pion photoproduction and on proton-Compton scattering. The latter, he said, would be the most informative study that could be done at the CEA. Martin and his group spent several years carrying out a detailed investigation of Compton scattering of GeV photons off protons, but there was no breakthrough physics to uncover within the capabilities of the facility. The payoff of this line of research came a few years later at SLAC, the high-energy linear accelerator at Stanford. When J. Friedman, H. Kendall and R. Taylor studied the electron scattering off protons at large momentum transfer, they found unequivocal evidence for quarks. Suzanne Deutsch, Martin's wife of 63 years, recalled that Martin received a call from Kendall on the day the Nobel informed him that he had won the prize. Kendall wanted to thank Martin, not only for his mentoring as a Ph.D. student, for bringing him to MIT, and for supporting him strongly but also to thank him for insisting that the electron-proton scattering problem was the most important one to study at that time.

With the demise of the CEA in the early 1970s Martin's total immersion in research declined. In 1973 he accepted the position of director of MIT's Laboratory of Nuclear Science, which administered all of the research carried out by the experimental and theoretical groups. But administration was not that attractive, and in 1979 he returned to the laboratory and to an extent not seen before he focused on teaching.

Teaching had not been a high priority for Martin during his most productive years when his time, energy, and concentration centered so completely on his research. In the early 1960s I sat in on his graduate nuclear physics course, enjoyed it immensely, and learned much. And so did those students who enjoyed the flood of insights that could not be found in texts or the literature. But most of the students were turned off by Martin's tacit assumption that he did not have to cover what they could read for themselves. Martin was well aware that his style of teaching was not for the majority and so when he returned to the classroom, it was to teach in the two-semester, advanced physics laboratory course, and to teaching special topics in physics to freshmen and sophomores.

The laboratory course with its one-on-one interactions suited Martin, and he and his students thrived. Even more enjoyable to Martin were the after-hours short-course electives that freshman could sign up for. Martin would select a subject he was interested in but had never studied in depth. The course would be announced and Martin would find himself studying the subject while teaching it to small groups of smart, involved students. Their sharp questions and his physically based, often extemporaneous answers were a constant source of conversation. I recall the time he came into my office to get my reaction to his insight into how to think about nuclear magnetic resonance, the topic of the course. I do not recall the insight but I do recall thinking how lucky those students were, and how much I would like to be taking that freshman course.

In his later years Martin often said that he considered his teaching of undergraduates and his mentoring of graduate students to be on a par with his contributions to physics. And he considered his ability to judge talent to be on a par with his ability to work at the cutting edge. After all, both Henry Kendall and Samuel Ting, the two people that he brought on the MIT faculty, won the Nobel Prize.

Martin's formal retirement from the MIT faculty in 1987 had no visible effect on his research, which had turned to a study of neutrinos. Raju Ragahaven, then at Bell Labs and now at Virginia Tech, had invited him to join a group to study low-energy neutrinos from the sun. The experiment, called Borexino, would be set up at the Gran Sasso National Laboratory situated inside a mountain about 160 km northeast of Rome. Martin, who had spent a sabbatical at the high-energy accelerator in Frascati in 1960-1961, enjoyed Italy and had friends there. He accepted Raju's offer and for the next 15 years, as the complex, very difficult installation was developed, he traveled periodically to Italy. He was still working closely with Raju at the time of his death. Raju (private communication, Aug. 2008) telephoned Martin on August 15, 2002, to arrange their next visit together to Gran Sasso. The discussion was to continue with a telephone conversation the next day, but when Raju called it was to find that Martin had died suddenly that morning.

In 1997 Martin summarized his own career to an interviewer from the Boston Globe. "The next five or six years [after returning from Los Alamos in 1946] were harvest years for me. Ideas that germinated during the war years led to others, and somehow, it seems that everything I touched turned to gold."

Gold indeed! Over a span of years following his work on the Manhattan Project, Martin Deutsch and his students pioneered the understanding of disintegration schemes of radioactive nuclei by inventing, developing, and using many of the fundamental methods of nuclear spectroscopy:

• Invented gamma-ray photoelectron spectroscopy that quantified gamma-ray energies and intensities from complex nuclear decays.

• Introduced the use of scintillators, in particular naphthalene and sodium iodide, for scintillation counting of gamma-ray energies and intensities.

• Proved the feasibility of measuring spins of excited states by angular correlation of gamma-gamma and electron-gamma ray cascades.

• Made the quantitative estimates that showed that magnetic moments of excited states could be determined from the perturbation on the angular correlations by a known magnetic field.

• Introduced polarization methods to determine the multiplicity of gamma rays.

Martin Deutsch was already a dominant figure on the world stage of nuclear spectroscopy when at the age of 34, he capped these innovations with a one-year burst of creative experimentation during which he proved the existence of positronium, quantified its modes of decay, and determined its hyperfine structure with such accuracy that he detected the shift due to quantum-electrodynamic, radiative corrections. Martin moved on to the new physics of elementary particles studied at high energy. He put behind him one of the most productive chapters written by any experimental physicist in the 20th century. Martin was elected to membership in the National Academy of Sciences in 1958.

SUZANNE DEUTSCH, Martin's wife for 63 years, was especially helpful with this memoir.

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