NORMAN FOSTER RAMSEY

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NORMAN RAMSEY was a towering figure in the world of physics during the second half of the twentieth century. He was esteemed for his scientific accomplishments, his service as a statesman of science, and his role as a teacher and mentor, and for the friendships he shared with people of all ranks around the world.

Ramsey was born into a military family in Washington DC on 27 August 1915, and he died in Boston, Massachusetts, on 4 November 2011. His father, Norman Foster Ramsey, Sr., had a distinguished career in the U.S. Army, enlisting in the infantry while underage and rising through the ranks to Brigadier General. Norman's education was frequently interrupted when the family was ordered to new locations in the United States and abroad. In spite of these disruptions, Norman graduated from the Fort Leavenworth, Kansas, high school at the top of his class at 15. He wanted to enter West Point or MIT but was too young. Columbia University had no such age restriction, and in 1931, Norman enrolled there as an engineering concentrator. A year later, he switched to his first love—mathematics. For his minor concentration, he chose a subject whose title was a word he had never heard before Columbia—physics.

In his senior year at Columbia, Norman was awarded a Kellett Fellowship. This fellowship was normally used to support studies in the humanities, but the committee was so impressed by Norman that they awarded it to him to study physics at Cambridge University. He arrived in Cambridge in the fall of 1935, and for the next two years, he studied physics at the Cavendish Laboratory. He attended lectures by Rutherford, Dirac, and J. J. Thomson, among others, and was tutored by Maurice Goldhaber, who later became a close friend and Director of Brookhaven National Laboratory. In the fall of 1937, Norman returned to Columbia University armed with two bachelor's degrees, a graduate teaching fellowship in physics, and enough formal training to permit him to move directly into doctoral research. His experience at Cambridge, particularly Rutherford's lectures, had kindled an enthusiasm for experimental physics, and Norman applied to work in I. I. Rabi's laboratory.

Rabi's group measured properties of atomic nuclei using the molecular beams technique in which streams of atoms or molecules are deflected by strong magnetic fields as they fly through a vacuum. Rabi initially discouraged Norman because he felt that that the field was essentially exhausted. Nevertheless, Norman persisted and was permitted to join the group.

Graduate Research with Rabi

Rabi had been seriously mistaken when he spoke to Norman about the future of molecular beam research. A few months later, Rabi invented

FIGURE 1. Norman, Minnesota, 1926.

the technique of molecular beam magnetic resonance, precipitating a scientific explosion whose echoes reverberate today. Rabi's resonance concept opened the way to fundamental measurements and high precision metrology, and it helped to spark atomic clocks, lasers, optical communications, and the Global Positioning System (GPS).

Applying the new resonance technique to study the proton and deuteron was high on the agenda of Rabi's group, but the apparatus, which gave excellent results for alkali-halide molecules, refused to yield data for hydrogen and deuterium. The balky apparatus was turned over to Norman so that the rest of the group could go on to more productive research. Working alone at night, Norman got the apparatus to work and made the most important discovery yet to come from Rabi's laboratory: the deuteron is not spherical. Within a year, this discovery had stimulated 10 publications by theorists, including Hans Bethe, Eugen Wigner, and Gregory Breit. The discovery also launched Norman into a life-long study of magnetic interactions in molecules.

Norman received a Ph.D. in 1940 and accepted a fellowship at the Carnegie Institution in Washington. He switched to particle physics and worked on neutron-proton scattering using the Carnegie's one-million electron volt accelerator. That same year, he married Elinor Jameson of Brooklyn, New York, and accepted a position at the University of Illinois. The newly wed couple arrived at Urbana in mid-September, but war was breaking out and their plans abruptly changed.

The War Years

During the summer and fall of 1940, Germany launched the Battle of Britain, the first battle fought by air. The British employed an early type of radar that turned out to be decisive in their effort. The American inventor/philanthropist Alfred Loomis recognized the significance of radar for the U.S. military and arranged for what became known as the Tizard mission to bring British radar technology—particularly the magnetron—to the United States. The mission arrived late in August. To avoid bureaucratic delay in developing radar, Loomis personally provided the funds for launching the U.S. radar effort. He selected MIT in Cambridge, Massachusetts, as its site, and within days, the MIT Radiation Laboratory was born.

In September 1940, Rabi moved to Cambridge to help organize the new effort and immediately recruited Norman. He and Elinor had just arrived at the University of Illinois at Urbana, but in October, before they were fully unpacked, they moved to Cambridge. Norman expected the job to involve a three-month leave, but they never returned.

Norman was made head of the group charged with developing 3 cm radar. Much of the microwave hardware one sees on radar installations today originated from that group. In the summer of 1941, he flew to England to share technology, crossing the ocean on a PanAm Clipper. He discovered that the British had a decisive invention—a crystal diode detector for microwave radiation that was vastly superior to the vacuum tube triode detectors used in the United States. He carried home a handful of the crystal diodes in his coat pocket, personally transferring the new technology to the Radiation Laboratory. In 1942, Norman was assigned to Washington DC to advise the Secretary of War, Henry L. Stimson, on radar. Stimson was concerned because the Air Force, unlike the Navy and Army, did not recognize the value of radar to its mission. Norman was so successful in generating Air Force interest that he was ordered to advise in creating a budget for bringing radar to the Air Force. Such a massive transformation required many planes, many radar sets, and many ground instillations. The budget, which came to about $2 billion, was put together over the following weekend.

In 1943, the Manhattan Project to create an atomic weapon launched under the supervision of General Leslie Groves. Groves selected J. Robert Oppenheimer to direct the scientific effort, and the Los Alamos Laboratory was founded. Rabi was a close friend of Oppenheimer and served as his informal advisor, particularly on issues of personnel. Evidently, Rabi recommended Norman to Oppenheimer because there ensued a somewhat unseemly tug-of-war for Norman between Groves and Stimson, and between Oppenheimer and his counterpart under Stimson, a Dr. Bowles. The conflict was resolved by an unconventional arrangement: Norman would work for the Manhattan Project but continue to be an employee of the War Department. The understanding was that Norman would report to Stimson if he sensed trouble at Los Alamos. He later described how on a few occasions, Oppenheimer prevailed in a disagreement with Groves by threatening to ask him to complain to Stimson.

Norman was put in charge of Project A, the so-called delivery group. As he described it,[1] he was given responsibility:

> . . . for design and procurement of components, which were required to convert a nuclear explosion into a combat bomb, coordination with Air Force activities including the modification of suitable aircraft, supervision of field tests on bombs without active material, planning and establishment of the necessary advance base where the final bombs would be assembled, assembly of active bombs and loading into aircraft, supervision of all tests and actions pertaining to the bomb while aboard the aircraft but prior to release, etc.

His experience in procurement in Washington was evidently of some advantage: to the detailed list of equipment he required, he added "one kit bomb assembly." This kit eventually included, among other things, several buildings, air conditioning, hoists, and a fleet of trucks.

Norman was on hand at the Trinity test site to study final assembly of the bomb. The test was carried out early in the morning of 16 July 1945. Later that day, he drove Rabi and Oppenheimer to Los Alamos (Rabi argued that Oppenheimer was too jittery to drive), and the following morning, Norman left for Tinian, where the delivery group was waiting for him to supervise the deployment.

Although Norman was ever ready with stories, he rarely talked about Los Alamos. In a 1962 oral interview, however, he described his experiences and views in some detail. Paraphrasing this would not do justice to his thoughts, although the interview sheds light on his profound aversion to bureaucracy. The entire scientific community at Los Alamos appreciated the need for speed because of concern that the Germans might develop an atomic weapon. Norman, and undoubtedly others, was constantly frustrated by bureaucratic delays. In reflecting on his wartime experience, he commented on "the many dedicated and hardworking people without whom the war would have ended sooner." In the many years I knew Norman, I never heard him speak unkindly of anyone. This jibe was about as cutting as he got.

Columbia And Brookhaven

Norman left Los Alamos in October 1945 and resumed his career at Columbia University, now as a tenured associate professor. He set about building a molecular beams laboratory by salvaging apparatus that had been cannibalized during the war. The scientific cost of the war to the Columbia physics department had been high: Enrico Fermi and Harold Urey left for The University of Chicago, attracted by Chicago's Metallurgical Laboratory, which was soon to be transformed into the Argonne National Laboratory; other scientists had moved to Oak Ridge National Laboratory and Los Alamos. Rabi and Norman decided that the highest priority for the physics department should be to gain access to a nuclear reactor, preferably nearby. Rabi took his argument to Vannevar Bush, head of the Office of Scientific Research and Development. Bush encouraged them, and in January 1946, they organized a meeting at Columbia with representatives from nearby universities and research laboratories.

Columbia's ambitions were not unique: Harvard and MIT argued that they, too, needed access to such a facility, and other universities became interested. A second organizational meeting was held in March, now with participants from nine universities. The single seriously divisive issue was the location of the new laboratory. Because of its size, the site had to be at a former military base, and each university had a favorite. A working group was appointed to settle on the location, and Norman was asked to head it.

By a combination of patience, good humor, and relentless logic, Norman brought the contentious group to agreement on a site. To put an end to disputes about travel times from airports to sites, for instance, Norman personally timed the travel for each of the final sites, picking up one speeding ticket in the process. The group agreed on the site of Camp Yaphank in Upton Long Island. Worrying that "Yaphank" might not sound attractive to wives in the new community, Norman asked Elinor to pick the best name from a list of nearby communities. She selected "Brookhaven." Shortly before the enabling law would have expired, Brookhaven National Laboratory was signed into existence.

On 1 January 1947, Brookhaven National Laboratory became a reality. Norman agreed to serve as the head of its physics department, splitting his time with the Columbia physics department. During the fall of 1946, he and Rabi tried to recruit Julian Schwinger to Columbia whereas Harvard worked to recruit Schwinger to its physics department. Harvard won and Schwinger joined Harvard's faculty. However, the recruiting battle had repercussions. Harvard was so impressed with Ramsey that they offered him a position. As the year progressed, it

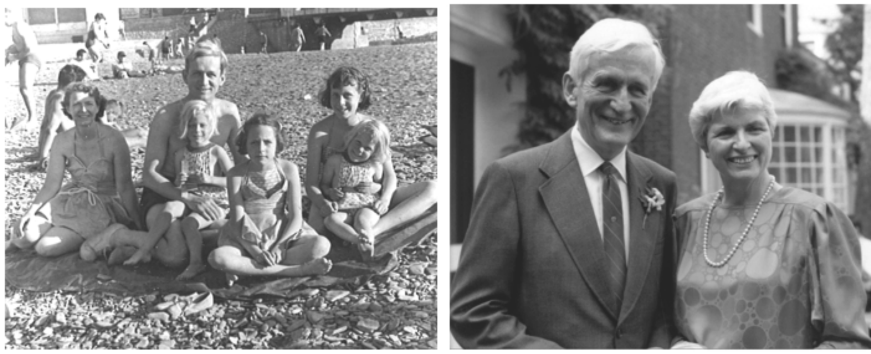

FIGURE 2. Elinor, Norman, Janet, Patty, Margaret, and Winnie, 1955 (left). Norman and Ellie, 1988 (right).

became evident to Norman that his joint appointment with Brookhaven and Columbia was not tenable. A major problem was the Long Island Railroad, whose famous shortcomings finally made commuting impractical. Norman accepted the Harvard position and joined its physics department in the summer of 1947.

The move to Harvard was so rushed that he and Elinor had only a single day to find a new home. A house in Belmont close to a bus line looked good, and they purchased it on the spot. There, Norman and Elinor raised their family of four daughters: Margaret, Patty, and the twins, Janet and Winnie. The children attended local schools, and family vacations were packed with skiing, sailing, hiking, and travel to centers of physics around the world. Norman and Elinor lived happily in that house until 1983, when Elinor succumbed to cancer.

In 1985, Norman married Ellie Welch, who fully shared his passion for hiking, skiing, and traveling. They lived in Brookline, and until late in his life, Norman commuted to Harvard by bike.

AT HARVARD

When Norman moved to Harvard, he envisioned a traditional career of teaching and research but no sooner had he arrived than his plans were diverted. The physics department had undertaken to construct a cyclotron and appointed a promising young physicist—Robert R. Wilson—to design the machine and supervise its construction. Wilson completed the design but abruptly accepted a faculty position at Cornell, leaving Harvard to build the machine. Norman had acquired a reputation for managing technical projects efficiently and had experience in high-energy physics from his year at the Carnegie Institution, and so it

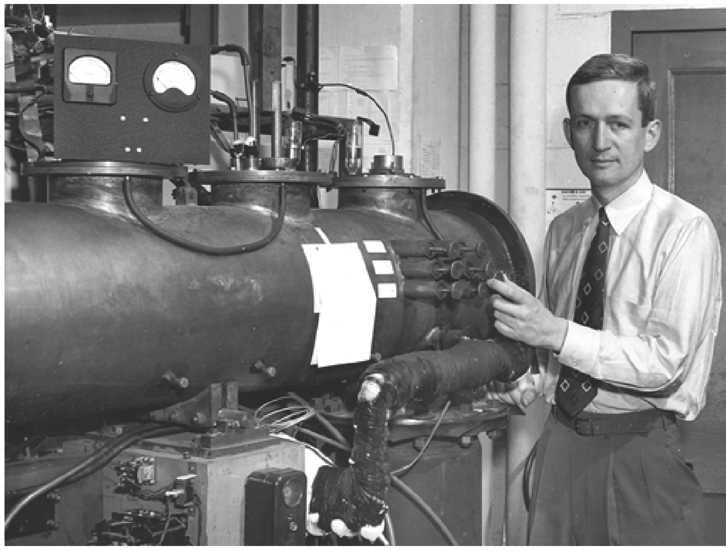

FIGURE 3. Norman with first molecular beams apparatus at Harvard, circa 1955.

was natural for the department to ask him to supervise construction and bring the cyclotron into operation. He agreed and built the cyclotron at the same time that he created his new molecular beams laboratory on the ground floor of Lyman Laboratory. The cyclotron construction proceeded smoothly, and the first beam was accelerated in June 1948. A formal dinner was held in celebration, with notables including the president of Harvard. During dinner, Norman received word of a leak in the cyclotron's vacuum chamber. He rushed off to fix the leak, in full evening dress.

In 1950, Richard Wilson joined the physics faculty and took responsibility for upgrading the cyclotron energy. For about 15 years, Norman collaborated with Wilson and others in experiments on the cyclotron, but his major research program was in the molecular beams laboratory. Norman also collaborated in planning the 6GeV Cambridge Electron Accelerator, working with Richard Wilson, Stanley Livingston at MIT, and several others. (A GeV is one billion electron volts.) The accelerator came into operation in 1962 and was used until 1974.

In the course of designing the molecular beams apparatus for his new laboratory, Norman invented the separated oscillatory field method, which was cited in his 1989 Nobel Prize. To increase the precision of his frequency measurements, he extended the length of the machine, thereby increasing the time the molecules interacted with the oscillating radiofrequency field. While struggling with the design, Norman recalled a curious fact from his undergraduate lectures at

Cambridge University: to obtain a sharp image of a small source through a telescope, paint a black strip across the center of the telescope mirror and use only the edges. The image will be faint but sharper than if you use the whole mirror. Spurred on by this curiosity, he calculated the signal to be expected if he replaced the long coil for the radiofrequency fields by two short coils separated by same overall length. He discovered that the signal was almost twice as sharp and then discovered that there were even more important advantages: Because the frequency of the resonance signal depends on the strength of an applied magnetic field, the field must be extremely uniform. Norman realized that the signal depends only on the average strength of the magnetic field so that fluctuations due to magnetic imperfections are smoothed out. Furthermore, Rabi's method was useful only at relatively low frequencies. In principle, the separated oscillatory method could work at any frequency.

Norman's group quickly put the separated oscillatory field method to use, and it was soon taken up in laboratories in the United States and abroad. Today it is often referred to simply as the "Ramsey method." In addition to playing a crucial role in Norman's own studies of magnetic interactions in molecules, the Ramsey method made atomic clocks possible. In 1944, Rabi pointed out that, in principle, one could use the internal frequency of an atom to control the rate of a clock. Unfortunately, his magnetic resonance technique could not be used at the high frequency required for a clock. Within a few years of Norman's discovery, a group at the National Physical Laboratory in England demonstrated the feasibility of an atomic clock by applying the Ramsey method to observe a microwave transition in the cesium atom. A few years later, Jerald Zacharias, an MIT professor who had been a member of Rabi's group, led the engineering of a practical atomic clock, the *Atomichron*. During the following five decades, the accuracy of the cesium beam atomic clock was incrementally improved by about a factor of 10 each decade, with the Ramsey method at the heart of each new generation of clocks.

Applications of atomic clocks exceeded everyone's imagination. Atomic clocks made GPS possible; they keep communication and power transmission system synchronized; they are widely used in precision measurement experiments; and they are to be found in astronomical observatories that employ very long baseline interferometry. In recent years, the Ramsey method has been employed in a new generation of atomic clocks that work at optical frequencies and achieve unprecedented accuracy. New applications continue to evolve—the Ramsey method is now employed on research involving entangled quantum states and quantum information theory, and it may open new paths for geodesy.

A Golden Age of Physics at Harvard

Norman's era at Harvard was a golden age for the physics department. His colleagues included Percy Bridgman, Edward Purcell, Julian Schwinger, Roy Glauber, John Van Vleck, and Nicolaas Bloembergen (in the adjacent Division of Engineering and Applied Physics), all of whom were awarded the Nobel Prize, and also Robert V. Pound, who was awarded the National Medal of Science. Norman's own Nobel Prize was awarded belatedly in 1989 for the separated oscillatory field method and the hydrogen maser.

Norman and Ed Purcell had adjacent offices in the Lyman Laboratory for Physics, and they talked daily. Purcell was shy and soft spoken, whereas Norman was gregarious and loud-voiced. However, they were intellectually and temperamentally matched and clearly enjoyed each other's company. For instance, when Norman created a graduate course on molecular beams research, Purcell sat in. Purcell asked penetrating questions, which Norman took care to anticipate. One lecture involved the issue of parity conservation, which was generally taken for granted. Norman guessed that Purcell would ask what evidence he had for this. Looking around, he found none. Rather than risk embarrassment in the classroom, Norman took the initiative and asked Purcell if he knew of any evidence. When Purcell could not find any, they surmised that parity might not actually be conserved. They published a paper suggesting tests to see whether parity is conserved, thus launching a series of experiments searching for a breakdown of parity conservation, and also time-reversal conservation, in the neutron. The first experimental test was reported in 1957. He reported ever more sensitive tests roughly every five years until about 1990, with work carried out at Brookhaven and Oak Ridge and in a collaboration at Institut Laue-Langevin. Unfortunately, as Norman liked to explain, Ed and he had proposed searching for parity breakdown under the so-called strong interaction, using neutrons. The effect was promptly seen for the so-called weak interaction, using muons, and led to Nobel Prizes for T. D. Lee and C. N. Yang.

In the fall of 1955, I entered the Harvard physics department as a graduate student, and the following spring I joined his group. My apprenticeship that summer was spent on the ground floor of the Lyman Laboratory on hands and knees with ruler and pencil, locating the center of resonance curves recorded on long rolls of recorder paper.

FIGURE 4. Outdoor group meeting, ca. 1960. At center, Norman standing on his head.

During the 1950s, Norman had organized a biannual conference on molecular beams physics that was held at Brookhaven. We traveled with him by car, crossing Long Island Sound in a military landing craft that was so cramped we had to stay seated in our car for the voyage, hoping that the craft would not spring a leak. In the 1960s, the conference became the biannual International Conference on Atomic Physics (ICAP), which continues today to be the major conference in atomic physics.

All of Norman's students took his course on molecular beams. From the early years of that course, he wrote a monograph, *Molecular Beams*, that became the bible of the field. Like many of Norman's students, I recall the years with him in a golden glow. There was an intellectual excitement with a combination of solid scientific progress on magnetic interactions in molecules and forays on speculative ideas. The group was dominated by Norman's exuberant, ever-friendly personality and booming voice. (A colleague recalled that on a bus from Oxford to London, Norman was telling a story so loudly in the rear that the driver stopped, walked back, and asked Norman to lower his voice because the story was disturbing his driving!) Norman's Friday group meetings were legendary, as were the travel gymnastics he sometimes executed to get there. I recall one scenario: Thursday morning, fly to DC to meet with the Department of Energy; Thursday afternoon, fly to Chicago—taking advantage of the time difference—to preside at a meeting of University Research Associates; Thursday evening, return to Cambridge and prepare for Friday class; Friday morning, teach class; Friday noon, group meeting.

Norman's group meetings mixed serious science with playful ideas and endless anecdotes. He kept a collection of toys and puzzles that displayed scientific paradoxes that baffled us, and sometimes him. Norman was a famous storyteller and he could enjoy a story even if he were the butt of its humor. One evening at a Ph.D. party, Norman was in the midst of a story when my wife, thinking I was dozing, nudged me. I whispered to her, "I'm not sleeping, I'm listening. This is a wonderful story and I've never even heard it before!" At that moment, there was a lull in the room and everyone heard my comment. Instead of an awkward silence, however, the room was filled by a roar of laughter from Norman.

In reflecting on his career, Norman said:

> "During the time when I had graduate students, if I were asked to name my 15 best friends, at least half of them would have been my current graduate students. That is the way we operated, on a first name basis. I learned a lot from them, and they learned a lot, I hope, from me."

Norman's students not only shared in the intellectual excitement of his group—they shared in a good deal of physical excitement. Norman was an avid skier, both downhill and cross-country, and he introduced many of his group to skiing. He was also an avid hiker. We often went on hikes in New England or at conferences in the mountains. He skied and hiked throughout his long career. In his '70s, he trekked in the Himalayas, the oldest visitor to have done so. Only after the trek did Norman notice that he had ruptured his Achilles tendon. In his '80s, Norman walked the week-long Milford Trek in New Zealand and the 200-mile, coast-to-coast path in England. In New Zealand, Ellie had to dissuade him from bungee-jumping into a gorge. In his '90s, he managed to visit both Antarctica and Alaska in the same summer. His Catholic connections in science and his warm personality generated walking friends worldwide.

At the start of my second year at Harvard, in the fall of 1956, Norman told me about a new idea that sounded screwball (I recall that was his word) but might work. The goal was to create a new type of atomic clock that operated on the hyperfine transition in hydrogen. The key idea was to obtain a sharp resonance signal by making the flight path of the atoms long while keeping the apparatus short. The atoms would be confined in a bulb with an inert surface. The ammonia maser had been invented a few years earlier by Charles H. Townes, and we speculated about whether it could be possible to employ the maser principle with hydrogen. Success would depend on the effect of the wall collisions in the storage bulb that could be understood only by experiment. We carried out an atomic beam experiment to study surface collision using cesium. Fortunately, the results suggested that the hydrogen maser had a reasonable chance of succeeding. In the fall of 1959, I started constructing the maser. At that time, Norman was on leave from Harvard, serving as the first science advisor to NATO, but he returned from time to time and we had chances to talk. A new graduate student, Mark Goldenberg, joined the project in the spring of 1960, and that summer the maser worked, generating a pure signal at 1420 MHz. We reported the success in a letter written by candlelight in Norman's home in Belmont, Massachusetts. A hurricane had knocked out the power.

The maser was later developed into a practical atomic frequency standard. Hydrogen masers are now to be found in most international time and frequency laboratories, as well as radio-astronomy observatories involved in very long baseline interferometry. In Norman's own laboratory, the advent of the hydrogen maser created a new line of research. His group generated a continuous stream of results in basic atomic and molecular physics until he wound down his experimental program in the mid-1980s. In addition, he collaborated in particle physics experiments with colleagues at Harvard and elsewhere until the 1960s, when he became involved with the governance of Fermilab. Norman's research program can be roughly summarized as:

> *Magnetic Moments and Magnetic Interaction in Molecules*: nuclear spins and magnetic moments; magnetic moment of the neutron; rotational magnetic moments; magnetic interactions in diatomic and polyatomic molecules; magnetic susceptibilities and magnetic shielding. These studies laid the foundation for understanding the chemical shifts that are the key to using nuclear magnetic resonance for chemical analysis. Norman later commented that among all of his research, his studies of magnetic interactions in molecular hydrogen had given him the greatest intellectual satisfaction.
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> *Atomic Physics*: fundamental measurements in atomic hydrogen; deuterium and tritium; hydrogen maser theory and performance; applications of the separated oscillatory field method in various contexts; atomic time-keeping and metrology.
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> *Particle Physics*: proton-proton, proton-neutron, and electron-proton scattering at energies up to 4; proton-proton triple scattering; electron-deuteron scattering; nuclear form factors; accelerator design.

> *Other*: breakdown of parity and time-reversal symmetries; search for the electric dipole moment of the neutron; negative absolute temperature.

Over the years, Norman supervised a total of 73 Ph.D. theses. Generations of his students and his students' students now occupy positions in colleges, universities, federal and industrial research laboratories, and other positions both in the United States and abroad. Prominent on the Ramsey scientific tree are five Nobel Prize winners. In addition to David Wineland, who was his graduate student and received the prize in 2012, there are four prize winners in the next scientific generation: Bill Phillips (1997), and Eric Cornell, Wolfgang Ketterle, and Carl Wieman, who shared the prize in 2001.

Teacher

Norman was an enthusiastic and popular teacher. In addition to presenting his graduate course in molecular beams and mentoring his graduate students, Norman was a hit with freshmen in Harvard's Natural Science II course and in the undergraduate course in quantum mechanics. In the 1970s, freshmen seminars were introduced to Harvard, and Norman took up this new mode of teaching with his usual energy and enthusiasm. For the first half of the term, Norman talked about frontline research from all areas of physics; in the second half, the students did the talking. The seminar was so popular that it was seriously over-subscribed every year.

Norman was widely appreciated as a guest speaker for colloquia, seminars, and other occasions, and he invariably accepted invitations if he could make room in his jammed schedule. Wherever he went, he tried to make opportunities to talk with students, and his podium was often mobbed following a talk. To coordinate his labyrinthine schedule of travel, meetings, classes, and talks, he relied on his principle of minimum bureaucracy: he simply recorded everything in a tiny notebook distributed annually by the Harvard Coop.

Statesman of Science

During his many years of research and teaching, Norman also pursued what was effectively a second career, which included positions such as president of the American Physical Society in 1978, chair of the Board of the American Institute of Physics from 1980 to 1986, and president of Phi Beta Kappa from 2001 to 2003. Beyond these public activities, Norman was widely sought as a confidant. His scientific insight,

FIGURE 5. Norman on *Meet the Press*, 20 December 1953.

relentless common sense, and remarkable personal skills made him the natural person to turn to when a tough problem arose. A colleague commented: "Beyond Norman's brilliance, friendliness and other attractive qualities, the quality I most appreciated was his absolute determination to be fair."

Norman first adopted a two-career pattern when he collaborated with Rabi to create Brookhaven National Laboratory and organize its physics department while simultaneously building his own laboratory at Columbia. The pattern continued after retirement when he had wound down his laboratory but continued to teach at various colleges while continuing his advisory work. He did not slow down until very late in life when travel became impractical.

An early incident of civil engagement occurred in 1953 when the nation was swept by anti-communist hysteria generated by Senator Joe McCarthy of Wisconsin. Wendell Furry, a professor in Harvard's physics department, had the misfortune to fall under McCarthy's spotlight. McCarthy accused Furry of being a communist, which he wasn't, and demanded that he give names of his friends, which he wouldn't. McCarthy, who got headlines in practically every newspaper in the nation, attacked Harvard for harboring communists. The Harvard Corporation put pressure on Nathan Pusey, the new president, to fire Furry. Norman led a few other colleagues in defending Furry from the Corporation. The Corporation eventually agreed not to fire him, but Senator McCarthy's attacks continued.

Shortly before Christmas 1953, Pusey was invited to defend Harvard on the TV show *Meet the Press* by its moderator Larry Spivak. Because it was felt that McCarthy's charges should not be dignified by a response from Pusey, the job fell to Norman. In preparing for his national TV debut, Norman went deeply into the law with Erwin Griswold, head of Harvard's Law School. At the end of the show McCarthy phoned to ask Norman and Spivak to come to his apartment for dinner. McCarthy, who could be charming, was so enthusiastic about Norman's presentation that he attempted to hire him! In spite of Norman's success in defending Furry, McCarthy called for a public hearing so that he could cite Furry for contempt for not revealing names. A conviction meant a jail sentence. Norman and Griswold spent long hours advising Furry. Eventually, the courts threw out the contempt citation. A few months later, McCarthy fell from power, and shortly thereafter he died.

Early in 1954, Norman became engaged in a second incident of anti-communist hysteria when J. Robert Oppenheimer asked Norman to testify at a hearing about his loyalty by the Atomic Energy Commission (now the Department of Energy). Norman knew Oppenheimer well from Los Alamos and had absolute confidence in his loyalty and a good understanding of the politics behind the attack: principally, the enmity of Lewis Strauss, a Commissioner of the AEC whom Oppenheimer had made look foolish in public, and the distrust of Edward Teller, who had policy disagreements with him. Norman, I. I. Rabi, and Enrico Fermi traveled to Washington together to brief Oppenheimer's lawyer, and all three later testified at the hearings before the so-called Gray Committee hearings. Its procedures appalled Norman. The purpose of a hearing is to establish facts, not to be a trial for judging guilt or innocence. Norman later reported that the Committee's lawyer behaved like a prosecuting attorney. Worse, that same lawyer wrote the report, thus also serving as the judge. It was clear from these tactics that Oppenheimer would be discredited. Norman was deeply distressed by the affair and considered refusing to consult for the government. Fortunately, he reconsidered because a few years later, he took on an assignment that ended happily.

In 1958, Detlev Bronk, President of the National Academy of Sciences, asked Norman to serve as the first science advisor to NATO. The duties were unspecified: they were for Norman to determine. Norman accepted the position but for only one year. Harvard granted leave, and Norman moved to Europe, returning once or twice a month for the Friday group meetings and to see his family and students. During that year, he established a postdoctoral fellowship program that enabled young scientists to pursue research in different NATO countries. He also came to the rescue of two institutions that were in serious financial difficulty: the Les Houches School in France and the Varenna School in Italy. Both of these institutions had been supported by their governments, but they attracted so many international students that the governments eventually decreed that someone else should pay the expenses. Les Houches offered the only graduate education in physics in France at the time, and Varenna played a similar role for Italy. In providing the support to prevent Les Houches from going under, Norman transferred a lump sum to the director, Cecil DeWitt. When she inquired about the reports and other paperwork that would be required, he replied that there would be only a single piece of paper: a summary of the scientific program. The Les Houches and Varenna schools flourish today with programs devoted to specialized topics that vary from year to year. Sadly, the paperwork has grown.

The Founding of Fermilab

In the 1960s, particle physicists recognized it should be possible to construct accelerators that could achieve energies of tens or hundreds of GeV. Lawrence Berkeley Laboratory and Brookhaven National Laboratory—the two leading laboratories—started to plan new machines, and other groups became involved, including Western Accelerator Group (WAG), a West Coast group centered at Cal Tech, and Midwest University Research Associates (MURA) in the Midwest. The Atomic Energy Commission ruled that only one major new accelerator could be supported. The political opportunity for providing the funding was fragile, and the window would be shut unless the community came to agreement on a common proposal. Nevertheless, the competition between the laboratories was so acrimonious that there was a real possibility the opportunity would be lost. In this increasingly dire situation, President Kennedy's science advisor, Jerome Wiesner, and the General Advisory Committee of the Atomic Energy Commission asked Norman to chair a panel to develop a single proposal. Norman was uniquely matched to the task: he was known to the particle physics community, was personal friends with many of the leaders, was not personally involved with any of the contending laboratories, and had the community's respect as a scientist and an established reputation for fairness.

The Ramsey panel (as it came to be known) first met in December 1962. A few months later, it recommended that the Federal Government authorize construction of a 200 GeV accelerator at Berkeley and, as a second priority, that Brookhaven start planning for an accelerator in the 600–1000 GeV range. These proposals triggered a series of initiatives that extended across the nation, involved the political needs of several powerful states and three presidents—Eisenhower, Kennedy, and Johnson—and culminated in 1967 with the creation of the National Accelerator Laboratory in Weston, Illinois, not far from

Chicago, as well as the commissioning of a 500 GeV accelerator. The laboratory name was later changed to Fermi National Accelerator Laboratory, or Fermilab. In 1984, the energy was doubled, and the machine became known as the Tevatron.

To manage the new laboratory, a consortium of universities joined in 1965 to form the Universities Research Association (URA). The consortium elected Norman as its president, and he was annually re-elected for 16 years. Among his principal achievements was selecting and then personally recruiting Robert R. Wilson to be Fermilab's director. Norman skillfully steered the laboratory through a series of scientific and political crises. To make time for URA, Norman took half-time leave from Harvard.

For more than two decades, Fermilab assured the nation's leadership in particle physics. In gratitude for his role in creating Fermilab, in 1981, the year of his retirement from URA, Fermilab's auditorium was named in Norman's honor.

The Kennedy Assassination Study

In 1979, the National Academy of Sciences asked Norman to chair a study requested by the House Committee on Assassinations. The Committee requested the evaluation of purported evidence for an additional shot from a different site at the John F. Kennedy assassination, which would establish the existence of a conspiracy. The evidence was the recording of a shot-like noise from an open microphone on a parked police motorcycle. Norman assembled a panel of a dozen physicists, acoustical engineers, and other specialists to analyze the evidence. After a laborious analysis, they found that the purported shot came about a minute after the fatal shots.[2]

It is impossible to prove the absence of a conspiracy, and as Norman had anticipated, the study did not put an end to conspiracy theories. In 2005, questions were raised about the validity of the study and the possibility of an additional gunshot. He recruited several members of the study team, and they reanalyzed all of the evidence, taking advantage of the significant advance in data processing over the years. The results confirmed the accuracy of the earlier analysis and reinforced the study's conclusions.[3]

Late Years

The Age Discrimination in Employment Act had not yet taken force when Norman reached 70, and he was compelled to retire in 1986. He had wound down his molecular beam laboratory, but his career carried

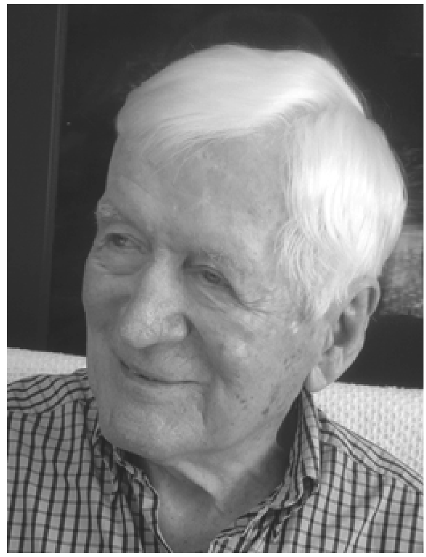

FIGURE 6. Norman, August 2010.

so much momentum that he seemed as busy as ever. He continued his collaboration in the neutron electric dipole moment experiment at the Institut Laue-Langevin in Grenoble, finally withdrawing from the collaboration in 1990 when he felt that he could no longer judge the data critically. Over the years, word of Norman's popularity as a teacher had spread, and he accepted invitations to teach as a visiting professor at Middlebury, Williams, Colby, and Mt. Holyoke colleges, and at the universities of Virginia, Chicago, and Colorado.

By this point in his career, Norman had been showered with prizes, honorary degrees, and other awards. Among these were the National Medal of Science (1989) and membership to the American Academy of Arts and Sciences (1950), the National Academy of Sciences (1952), and the American Philosophical Society (1958). The capstone to his honors was, of course, the award of the Nobel Prize in 1989. His colleagues felt that the award was much belated: the work—the separated oscillatory field method and the hydrogen maser—had taken place 30 years earlier, but Norman viewed the timing as just right because he was free to accept the deluge of invitations to visit and speak that befall Nobel laureates. He accepted whenever possible and never lost his enthusiasm for engaging the students who often mobbed the podium after his talks. But travel became increasingly difficult, and as he moved into his '90s, he gradually slowed his activities.

Toward the end of his life, Norman became physically limited and eventually confined to a wheelchair. His personality, however, never altered: he never complained, never spoke unkindly of anyone, and was cheerful and ever thoughtful. In his final days, a visitor entered his room to find him dozing, totally unable to move. Norman looked up with a smile and said, "How kind of you to come. Let me get you a chair!"

Elected 1958

Daniel Kleppner[a]
Lester Wolfe Professor of Physics, Emeritus
Massachusetts Institute of Technology

Endnotes

1. N. F. Ramsey, *Report of Project A*, Los Alamos National Laboratory Archives, Unclassified 17 July 1979.

2. N. F. Ramsey (Chairman), L. W. Alvarez, H. Chernoff, R. H., et al., *Report of the Committee on Ballistic Acoustics (John F. Kennedy Assassination)* (National Academy Press: Washington DC, 1982); also *Science,* 218 (1982): 127–33.

3. R. Linsker, R. L. Garwin, H. Chernoff, P. Horowitz, and N. F. Ramsey, "Synchronization of the acoustic evidence in the assassination of President Kennedy," *Science and Justice* B45 (2005): 207–26.

Sources

Norman F. Ramsey, Jr. *The Reminiscences of Norman F. Ramsey* (Oral History Research Office: Columbia University, 1962).

Interview of Norman Ramsey by Katherine Sopka on 23 November 1976, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA.

Interview of Norman Ramsey by Lillian Hoddeson on 19 December 1978, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA.

Interview of Norman Ramsey by Ursala Pavish on 20 September 2006, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA.

John S. Rigden. *Rabi: Scientist and Citizen* (Basic Books: New York, 1987).

Robert P. Crease, *Making Physics: A Biography of Brookhaven National Laboratory, 1946-1972* (Chicago: The University of Chicago Press, 1999).

Lillian Hoddeson, Adrienne W. Kolb, and Catherine Westfall, *Fermilab: Physics, the Frontier & Megascience* (Chicago: The University of Chicago Press, 2008).

a From Kleppner, Daniel (2015), "Norman F. Ramsey, 1915-2011," in *Biographical Memoirs*, National Academy of Sciences, Washington, D.C. Access at http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/ramsey-norman.pdf.

Selected Bibliography

1940 With J. M. B. Kellogg and I. I. Rabi. "An Electrical Quadrupole Moment of the Deuteron," *Phys. Rev.* 55: 318–9. "Radiofrequency Spectra of HD and D_2 Molecules in a Magnetic Field," *Phys. Rev.* 57: 677–94.

"Rotational Magnetic Moments of H_2 and D_2 and HD Molecules," *Phys. Rev*. 58: 226–36.

1950 "Molecular Beam Resonance Method with Separated Oscillating Fields," *Phys. Rev.* 78: 695–9.

"Magnetic Shielding of Nuclei in Molecules," *Phys. Rev*. 78: 699–703.

With E. M. Purcell. "On the Possibility of Electric Dipole Moments for Elementary Particles and Nuclei," *Phys. Rev*. 78: 883–6.

1951 With R. V. Pound. "Nuclear Audiofrequency Spectroscopy by Resonant Heating of the Nuclear Spin System," *Phys. Rev*. 81: 278–9.

"Radiofrequency Spectra of H_2 and D_2 by a New Molecular Beam Resonance Method," *Phys*. 17: 388–91.

With R. W. Birge et al. "Proton-Proton Scattering at 105 MEV and 75 MEV," *Phys. Rev.* 83: 274–80

1956 "Thermodynamics and Statistical Mechanics at Negative Absolute Temperatures," *Phys. Rev*. 103: 20–8.

1957 With J. H. Smith and E. M. Purcell. "Experimental Limit to the Electric Dipole Moment of the Neutron," *Phys. Rev*. 108: 120–2.

With Henry R. Lewis. "Theory of Hydrogen Deutoride in Magnetic Fields," *Phys. Rev.* 108: 1246–50.

1960 With W. H. Furry. "On the Significance of Potentials in Quantum Theory," *Phys. Rev.* 118: 623–6.

With H. M. Goldenberg and D. Kleppner. "Atomic Hydrogen Maser," *Phys. Rev. Lett*. 8: 361–2.

1963 With S. B. Crampton and D. Kleppner. "Hyperfine Separation of the Ground State of Atomic Hydrogen," *Phys. Rev. Letters*. 11: 338–40.

With K. W. Chen et al. "Electron-Proton Scattering at High Momentum Transfers," *Phys. Rev. Letters*. 11: 561–4.

1964 With E. N. Fortson and D. Kleppner. "Stark Shift of the Hydrogen Hyperfine Separation," *Phys. Rev. Letters*. 13: 22–3.

1965 With D. Kleppner et al. "Hydrogen Maser Principles and Techniques," *Phys Rev*. 138: A972–A983.

1966 With T. Myint et al. "Absolute Value of the Proton g Factor," *Phys. Rev. Lett*. 7: 405–7.

1972 With R. C. Hilborn and T. V. Gallagher. "Hyperfine Structure of $Li^7Br^{79,81}$ by Molecular Beam Electric Resonance," *Jour. Chem. 1 Phys.* 56: 855–91

With D. J. Wineland. "Atomic Deuterium Maser," *Phys. Rev. D* A5: 821–97.

1977 With W. B. Dress et al. "Search for an Electric Dipole Moment of the Neutron," *Phys. Rev.* D15: 9–21.

1980 M. Forte, B. R. Heckel, N. F. Ramsey, K. Green, et al., "First Measurement of Parity-Nonconserving Neutron Spin Rotation: The Tin Isotopes," *Phys. Rev. Lett.* 45: 2088–92.

1989 "Experiments with Separated Oscillatory Fields and Hydrogen Masers," Les Prix Nobel, Nobel Foundation, 1989.

1990 "Electric Dipole Moment of the Neutron," *Ann. Rev. Nucl. Part. Sci.* 40: 1–14.