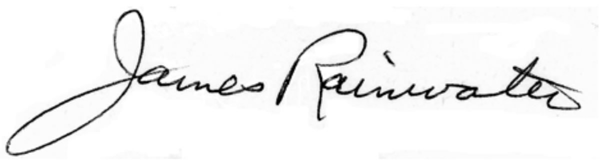
NATIONAL ACADEMY OF SCIENCES

JAMES RAINWATER
1917—1986

A Biographical Memoir by
VAL L. FITCH

Biographical Memoir

Photograph Courtesy AIP Emilio Segré Archives.



JAMES RAINWATER

December 9, 1917–May 31, 1986

BY VAL L. FITCH

I. I. RABI, THE COLUMBIA University physics department's leading researcher, chairman, and then after his retirement, wise old man, disliked the notion that physicists had divided themselves into two groups: experimental and theoretical. "There is only Physics," he said, "with a capital P." His strong feeling always manifested itself in his insistence that those who did experimental theses have a rigorous grounding in theoretical subjects and that theorists know something about experiment. He had two outstanding examples of such people in the department. One was Willis Lamb, who had done his thesis with Robert Oppenheimer and after a series of notable theoretical papers had won the Nobel Prize for an experiment. Rabi never forgave Lamb for leaving Columbia and going back to his native California. And then there was Jim Rainwater, the subject of this memoir, who had done his thesis with John Dunning, a consummate experimentalist, and had gone on to win the Nobel Prize for theoretical work. Rainwater spent his entire career at Columbia, first as a graduate student and then as a member of the faculty. He enjoyed Rabi's highest accolades.

Leo James Rainwater was born in Council, Idaho, on December 9, 1917. Leo he never used; he was always Jim. Council, Idaho, today has a population of about 1,000 with the main activities centered around ranching and lumbering.

As to the town in 1917 I have no special information but as these things go, I suspect the town has changed very little in the last 90 years. Council is about 90 miles north of Boise and 20 miles to the east of the Oregon-Idaho boundary, defined by the well-known tourist attraction, Hell's Canyon of the Snake River. One might describe the area around Council as rough country, thinly populated.

Earlier in the century Jim's parents, Leo J. and Edna E. (Teague) Rainwater, had moved from California where his father worked as a civil engineer, but for reasons unknown he changed careers and operated a general store in Council. (The name Rainwater suggests a Native American heritage. In fact, the progenitor was Robert Rainwater who came from England to work as an indentured servant in 1705.) Late in 1918 the whole family succumbed to the great flu pandemic. Jim's father died and it was nip and tuck for a while as to whether the infant, Jim, would survive. When Jim was well again and the father buried, his mother moved back to California, to Hanford, in the San Joaquin Valley. It is there that Jim grew up. After a time his mother married a widower and through that marriage Jim acquired two older stepbrothers and a half brother, George Fowler. George graduated from the Naval Academy and became a career officer.

As might be expected Jim was an excellent student in mathematics, physics, and chemistry in high school. And because of his high performance on an open chemistry competition he was admitted to the California Institute of Technology. This was in the middle of the depression, 1935. At Caltech he took courses from notable people (e.g., Carl Anderson and Thomas Hunt Morgan) and graduated in 1939.

At the time it must have been unusual for a Californian to go to Columbia University for graduate work. But the Columbia University's physics department was becoming a powerhouse. Peagram, department chairman, had enticed Enrico Fermi to come to the United States and Columbia. In the first two years Jim had courses from I. I. Rabi, Enrico Fermi, Edward Teller, and John Dunning. Dunning and Booth had just completed the Columbia cyclotron in the basement of the Pupin Laboratories and neutron studies were in high gear. Later, Dunning, Booth, Slack, and Von Grosse developed the gaseous diffusion process to enrich uranium with U^{235}, and they hold the basic patent. In his research for his Ph.D. thesis Jim and Bill Havens along with Dunning developed the technique of pulsed neutron spectroscopy. Later, with C. S. Wu joining them, that was the principal activity of the group throughout World War II. When his thesis was finally declassified in 1946, Jim received his Ph.D. Many papers dealing with the energy dependence of neutron cross-sections were declassified at this time, all with the names of Dunning, Havens, Rainwater, and Wu.

Dunning was Rainwater's thesis adviser and in turn George Peagram had been Dunning's adviser. Peagram's thesis, 1903, was entitled *Secondary Radioactivity in the Electrolyses of Thorium Solutions.* I have not been able to determine Peagram's adviser so that particular professional genealogy stops there. In that Rainwater was this author's Ph.D. adviser I can claim Peagram as my professional great-grandfather. The American Mathematical Society maintains a mathematics genealogy. A few theoretical physicists are included. In tracking the history of Richard Feynman, for example, one encounters the name of Carl Freiderick Gauss. It's hard to beat that for a professional ancestor. But more spectacular is the genealogy of Alan Turing, the computer pioneer, a student of Alonzo Church at Princeton. After five generations the advisers that follow come, in order, Poisson, LaGrange, Euler, Johann Bernouli, Jacob Bernouli, and then Leibnitz.

Jim continued on at Columbia after the war as an instructor, rising to professor in 1952. I entered the physics department as a graduate student in the fall of 1948. I had been working at Los Alamos the previous summer. J. M. B. Kellogg, a former collaborator of Rabi's in the molecular beams group, was my division leader at Los Alamos and he had written a letter on my behalf to Rabi, then the department chair. When I checked in at Rabi's office, he dug out Kellogg's letter, picked up the phone, and called Rainwater, letting him know that he was sending me to be his graduate assistant. I spent the next six years working with Jim, five as a graduate assistant and one as an instructor. I had learned electronics skills at Los Alamos and I was immediately put to work developing circuits of use to Rainwater's research. Shortly, I had an assistant working for me, perhaps the first graduate student who had his own technician.

Jim was teaching a course in advanced nuclear physics, which I took in 1949-1950. I still have the notes for the course with Rainwater's handwriting on ditto paper. He was very thorough, the first chapter was devoted to the quantum mechanics that would come into play in the rest of the course. Also in the course was Leon Cooper of Bardeen, Cooper, Schreifer theory of superconductivity fame. Leon once mentioned to me that Rainwater thought like a theorist. I was never quite sure what he meant by that but it was said admiringly. Subsequently, Leon wrote a theoretical paper with Rainwater. I got to know Jim as a perceptive experimentalist with a deep theoretical understanding of physics.

From the course I remember his great interest in the shell model proposed by Marie Mayer in 1949. As he has written,

"It" fitted my belief that a nuclear shell model should represent a proper approach to understanding nuclear structure. Combined with developments of Weizsaker's semi-empirical explanation of nuclear binding, and the Bohr-Wheeler 1939 paper on nuclear fission, emphasizing distorted nuclear shapes, I was prepared to see an explanation of large nuclear quadrupole moments. The full concept came to me in late 1949 when attending a colloquium by Professor C. H. Townes who described the experimental situation for nuclear quadrupole moments." Jim immediately wrote the famous paper quantifying his ideas. He had a perfect sounding board as he shared his office with Aage Bohr that year. They had many discussions of the implications. Subsequently Bohr and Mottleson, following the lead of Rainwater, successfully exploited the basic ideas in their highly successful collective model of the nucleus. Rainwater, Bohr, and Mottleson shared the 1975 Nobel Prize for their work on nuclear models.

The following is an excerpt from the presentation speech given by Professor Sven Johansson of the Royal Swedish Academy of Sciences on the occasion of the awarding of the Nobel Prize:

It was soon found that the nucleus has properties, which cannot be explained by these models. Perhaps the most striking one was the very marked deviation of the charge distribution from spherical symmetry, which was observed in several cases. It was also pointed out that this might indicate that certain nuclei are not spherical but are deformed as an ellipsoid, but no one could give a reasonable explanation of this phenomenon.

The solution of the problem was first presented by James Rainwater of Columbia University, New York, in a short paper submitted for publication in April 1950. In this, he considers the interaction between the main part of the nucleus, which forms an inner core, and the outer, the valence nucleons. He points out that the valence nucleons can influence the shape of the core. Since the valence nucleons move in a field which is determined by the distribution of the inner nucleons, this influence is mutual. If several valence nucleons move in similar orbits, this polarizing effect on the core can be so great that the nucleus as a whole becomes permanently deformed. Expressed very simply, it can be said that as a result of their motion, certain nucleons expose the "walls" of the nucleus to such high centrifugal pressure that it becomes deformed. Rainwater also attempted to calculate this effect and got results that agreed with experimental data on the charge distributions.

Aage Bohr, working in Copenhagen, but at this time on a visit to Columbia University, had, independently of Rainwater, been thinking along the same lines. In a paper submitted for publication about a month after Rainwater's, he formulates the problem of the interaction of a valence nucleon with the core in a general way.

These relatively vague ideas were further developed by Bohr in a famous work from 1951, in which he gives a comprehensive study of the coupling of oscillations of the nuclear surface to the motion of the individual nucleons. By analyzing the theoretical formula for the kinetic energy of the nucleus, he could predict the different types of collective excitations: vibration, consisting of a periodic change of the shape of the nucleus around a certain mean value, and rotation of the whole nucleus around an axis perpendicular to the symmetry axis. In the latter case, the nucleus does not rotate as a rigid body, but the motion consists of a surface wave propagating around the nucleus.

Up to this point, the progress made had been purely theoretical and the new ideas to a great extent lacked experimental support. The very important comparison with experimental data was done in three papers, written jointly by Aage Bohr and Ben Mottelson and published in the years 1952-53. The most spectacular finding was the discovery that the position of energy levels in certain nuclei could be explained by the assumption that they form a rotational spectrum. The agreement between theory and experiment was so complete that there could be no doubt of the correctness of the theory. This gave stimulus to new theoretical studies, but, above all, to many experiments to verify the theoretical predictions.

This dynamic progress very soon led to a deepened understanding of the structure of the atomic nucleus. Even this further development towards a more refined theory was inspired and influenced in a decisive way by Bohr and Mottelson. For example, they showed together with Pines that the nucleons have a tendency to form pairs. A consequence of this is that nuclear matter has properties reminiscent of superconductors.

Drs. Bohr, Mottelson and Rainwater, in your pioneering works you have laid the foundation of a theory of the collective properties of atomic nuclei. This has been an inspiration to an intensive research activity in nuclear structure physics. The further development in this field has in a striking way confirmed the validity and great importance of your fundamental investigations" (*Nobel Lectures, Physics 1971-1980,* ed. S. Lundqvist. Singapore: World Scientific, 1992).

I saw Rainwater only occasionally during this period. Not only was he writing a trailblazing theoretical paper but he was also working on the notes for his course on advanced nuclear physics. No suitable texts were available in those early days and he was in effect supplying one for his students. Furthermore, he had been active in the design and construction of the Nevis synchrocyclotron, the RF (Radio Frequency) system in particular, which was then just coming into operation. Columbia University had been willed a DuPont family estate located about 20 miles north on the shore of the Hudson River at Dobbs Ferry. It was a beautiful setting with a mansion, a carriage house, and plenty of space on the extensive grounds. The mansion had been constructed by the son of Alexander Hamilton. The name, Nevis, is the island in the Caribbean where Alexander Hamilton was born. It was here that the Columbia machine had been built. Columbia faculty whose research was tied to the cyclotron bought homes in the area, Jim was one of those.

In Rainwater's Nobel speech, 1975, he said that his main experimental contribution after 1950 was in the muonic atom X-ray studies started with Val Fitch. These studies involved several groundbreaking developments that I will shortly describe.

One day in the spring of 1950 Rainwater called me into his office. After introducing me to Bohr, he handed me a paper, a preprint from John Wheeler on what might be learned about nuclei from studies of the spectra of mu-mesonic atoms. Said Jim: "This might make a good thesis topic for you." I took it and ran.

A number of important technical developments crucial to this experiment were taking place. RCA had just developed an end-window photo tube, the 5819. Robert Hofstaeder had discovered at Princeton that thallium (Tl) activated sodium iodide (NaI), and it was an efficient scintillating material with the iodine being of sufficiently high atomic number to make the material ideal for sensing gamma rays and measuring their energy up to a few MeV. In addition Lederman and Tinlot had plotted the trajectories of negative pions through the fringe fields of the cyclotron after they originated from internal targets placed in opportune positions. The shielding wall was being cast with horizontal apertures to roughly collimate particle beams of various momenta.

We first received a sample of NaI (Tl) from a government laboratory in downtown New York. It was immersed in mineral oil since NaI is a highly deliquescent material. With this sample and with further crystals obtained from the Harshaw Chemical Company we were able to establish the possible use as a gamma-ray spectrometer. We simply dipped the cathode end of the 5919 into a beaker filled with mineral oil. This provided excellent optical coupling between the crystal and the photo cathode.

Jim had grown up in the depths of the Great Depression. It is said that anyone who grew up in those times never learned to spend money. Correspondingly, he was always looking for ways to save money. A good example was his building two bending magnets, C-shaped, by having the shop flame cut 1-inch-thick slabs of steel into the C profile and then bolting them together. He made the electrical coils from ordinary 1/4-inch copper refrigerator tubing covered with insulating tape.

The annual meeting of the American Physical Society in January 1952 was held at Columbia University in the Pupin Physics Building. Jim chaired a session on the apparatus of nuclear physics. One of the 10-minute contributed papers was entitled “A New Method of Focussing Ion Beams.” To my knowledge it was the first use of magnetic quadrupole focusing. A quadrupole will focus and defocus equally. However, when two magnets are combined with an appropriate separation between and with the poles at 90 degrees, the net effect is focusing. A group from Princeton had applied the technique to a beam from their small cyclotron. They had produced the quadrupoles using bending magnets with wedge-shaped pole tips. The inventor of this scheme was Roy Britten who, after getting his Ph.D., in nuclear physics, went on to became a distinguished biologist.

Jim immediately had pole tips cut to be 90-degree wedges and had them installed on his homemade magnets. We spent a Sunday setting up the magnet system and, using the hot wire method for simulating particle trajectories, found the optimum conditions for focusing the meson beams of around 140 MeV/c. By this time it had been determined that muons comprised about 10 percent of the meson flux from the cyclotron; these originated from pion decay in flight. For a given momentum, muons have a greater range in material than pions and thereby can be separated. Scintillating materials made from organic crystals became available, in particular, stilbene. For this experiment our standard beam counters were stilbene cystals about 2 inches in diameter and 1/4 inch thick, viewed by side-window 1P21 RCA photo multiplier tubes.

In the spring of 1952 most of the gear was in place for searching for muonic X rays: the focused beam from the cyclotron, beam counters, and the NaI (Tl) gamma-ray detector (the NaI was now supplied by the Harshaw Chemical Company already encapsulated with a transparent window for viewing by a 5819 photo multiplier). We had also constructed all the necessary coincidence circuits as well as a multichannel pulse-height analyzer into which the NaI pulses were fed. In striking contrast to today, every piece of electronics was home designed and constructed. With the exception of mechanical counters preceded by scaling circuits, nothing was available commercially, not even power supplies.

In his original paper Wheeler had calculated the 2P-to-1S transition energies for lead (Pb), and it was this material that we initially used to search for the radiation. Modeling the nucleus as a uniform ball of charge with a radius of 1.4 $\times 10^{-13} A^{1/3}$ centimeters, the then generally accepted nuclear size, Wheeler had come up with 4.5 MeV for the transition energy. The principal interaction of gammas of this energy in the crystal would be through the production of positron-electron pairs. The positron would eventually come to rest and annihilate with an electron producing two 500 KeV gammas. With a large crystal there was a good probability of capturing both gammas resulting in a full 4.5 MeV deposited. With the smallish crystal such as we had available it was more likely that none or one of the annihilation quanta would be captured. Correspondingly, we expected to see a peak at 3.5 MeV with a smaller one at 4 MeV. After many frustrating hours of searching for energy peaks in this range, Rainwater suggested we search with an expanded energy scale. Behold! A peak appeared at 5 MeV corresponding to a transition energy of 6 MeV rather than Wheeler's predicted 4.5 MeV. This was the first indication that the nucleus was significantly smaller than had been previously thought. We measured the X rays from several other elements and the results were consistent with a smaller nucleus.

Jim always insisted that his students perform some significant theoretical calculations. I began solving the two-component Dirac equation for the energy levels, assuming a nucleus with a uniform charge distribution with various radii. This work was done numerically on a Marchand calculator. Five years later this would have been a relatively trivial job on an early IBM computer. As it was, both the Marchand and I had a real workout. The conclusion was that the nuclear radii were nicely fitted to $R = R_0A^{1/3}$ where R_0 was 1.18×10^{-13} cm in contrast to the previously accepted value of 1.4×10^{-13} cm.

The paper describing this work was published in November 1953, more than a year after the X rays were first observed. It constituted my Ph.D. thesis. Along with our paper, that of John Wheeler, which had stimulated all the activity, appeared together. In addition, Leon Cooper and Ernest Henley had inquired of possible polarization effects of the muon on the nucleus and their paper was included. This was to be Cooper's Ph.D. thesis.

Shortly afterward Sam Koslov joined Rainwater and me in using a novel method of showing the existence of rather large energy shifts due to vacuum polarization, an effect that is small in the usual Lamb shift but dominant in the case of muonic atoms.

By now I was an instructor at Columbia with every possibility for promotion. However fruitful my experience working with Rainwater, I decided that it was probably in my best professional interest to move on and with a lot of arm twisting from John Wheeler I changed my allegiance to Princeton in 1954. With other students Rainwater continued the muonic atom work for a few years, considerably refining the technique. Rainwater also served two periods as the director of the Nevis Laboratory during the middle 1950s. He also returned to his initial research interest at Columbia, neutron cross-sections as a function of energy. The Nevis cyclotron made it possible to substantially extend the energy scale, and the same time-of-flight techniques were possible with much greater neutron flight paths. Jim continued with this work until the cyclotron was shut down in 1978. Following this he devoted himself to teaching.

After six years working with Jim Rainwater, what were my impressions of the man? I had never seen anyone so devoted to physics. In 1942 Jim married Louise Smith, a graduate of Barnard whom he had met at a mixer. They had three sons, James, Robert, William, and a daughter, Elizabeth Ann, who died from leukemia at the age of nine. Jim had few social interests nor did he engage in any athletic activity except those involving his sons. One of my fellow graduate students Aihud Pevsner remembers Rainwater's reaction to the news that Rabi had to cancel the annual department holiday party. "Good," he said, "now if only Quimby would cancel his New Year's Eve party, we could get a lot of work done."

It is characteristic of people who are firstrate in their field to be the first to know of their superiority. But Rainwater was unusually modest and you would never learn from him of his many accomplishments. However, he was enormously respected by his colleagues.

Jim was a superb experimental physicist. He took great joy is totally understanding how nature works and in devising and inventing the equipment required to probe and learn. He was not one for attacking the latest and most fashionable problems. The hot items in the marketplace of physics were not for him. He was too reflective for that. He had to understand things at too deep a level. He eschewed the superficial, he disdained the dilettante. It was not obvious disdain. He simply observed and walked away. He was not given to small talk. He assumed every question to be thoughtful and one that deserved a thoughtful response. Jim established for himself the highest standards of professional performance and was, in this regard, the ideal role model for his students. I trust it is obvious, but for a person to contribute as much as he did to physics, the unfailing dedication, love, and support from home is a necessity, and he had his wife, Louise, to thank for that.

I always had the impression that Rainwater felt his Nobel Prize-winning paper had not been properly recognized by nuclear theorists. This is apparent from what he has to say in his Nobel lecture. "I was surprised and dismayed to hear one or more respected theorists announce in every Nuclear Physics Conference which I attended through approximately 1955 some such comment as 'Although the Nuclear Shell Model seems empirically to work very well, there is at present no theoretical justification as to why it should apply.'"

Though he never discussed it, over a number of years it became apparent that his health was slowly declining, his vigor was waning. He continued to teach but it took heroic effort. One day in early 1985 on leaving Pupin Laboratory after a lecture he collapsed to the ground. Fortunately, a student who had CPR training was nearby and revived Jim. However, everafter his health continued to deteriorate. On May 31, 1986, at age 68 he died in a hospital in Yonkers.

His fellow graduate student, Bill Havens, who was probably in the best position to know him well, observed of Jim that he found the beauty of physics in its "orderly nature." "He was an original thinker; he looked at things differently and came up with interesting new suggestions." I. I. Rabi said of him, "He had a quiet nature, he was a person of extraordinarily solid integrity who was never satisfied until he got to the bottom of a problem. He had no glib answers and worked at things until he understood them thoroughly." Professor E. T. Booth, in charge of the design and construction of the Nevis cyclotron came to know him extremely well. He observed of Jim, “He was extremely gifted as both a theorist and as an experimentalist, he was a dedicated physicist who left a lasting mark on the field through his thorough research of the selective reactions of neutrons and through his rigorous training of students.” The author of this biographical memoir couldn’t agree more with all of these accolades, especially the very last one.

He received the Nobel Prize in 1975 and the Ernest Orlando Lawrence award in 1963. In addition to membership

in the National Academy of Science (elected in 1968) he was also a fellow of the American Physical Society, the New York Academy of Sciences, the IEEE, and a member of the American Institute of Physics, the American Association of Physics Teachers, and the Optical Society of America.

IT IS A PLEASURE TO THANK those who assisted me in preparing this memoir. I am especially grateful to Ann Therrien, manager of Columbia’s Nevis Laboratory, and Aihud Pevsner and Bruce Knapp provided illuminating remembrances. The Columbia Physics Department administrative coordinator, Lalla Grimes,

expeditiously provided material such as publication lists. I am indebted to Jim's youngest son, Bill, who supplied details of Jim's family and provided the fine photograph of Jim and his wife that graces this memoir. It was taken in 1971 on the occasion of the wedding of Jim's oldest son. It is not the custom to include a photograph of the spouse, but in this case Louise's devotion to Jim had a great deal to do with his high accomplishments.

SELECTED BIBLIOGRAPHY

The first two entries consitute the Ph.D. Theses of William Havens and James Rainwater.

1946

With W. Havens. The slow neutron cross sections of indium, gold, silver, an timony, lithium and mercury as measured with a neutron beam spectrometer. *Phys. Rev.* 70:154-173.

With W. Havens. Neutron beam spectrometer studies of boron, cadmium and the energy distribution from paraffin. *Phys. Rev.* 70:136-153.

Twelve publications devoted to slow neutron spectroscopy were published by Rainwater and co-authors in 1946-48. Of special note are:

1946

With J. Dunning, W. Havens, and C. S. Wu. Neutron scattering in ortho and parahydrogen and the range of nuclear forces. *Phys. Rev.* 69:236-237.

1947

With W. Havens and I. I. Rabi. Interactions of neutrons with electrons in lead. *Phys. Rev.* 72:634-636.

The Nobel Prize winning theoretical paper can be found at:

1950

Nuclear energy level argument for a spheroidal nuclear model. *Phys. Rev.* 79:432-434.

Jim's first paper describing research at the Nevis Cycloron with his student H. L. Friedman followed by his pioneering work on mu-mesic atoms.

1951

With H. L. Friedman. Experimental search for beta decay of the Pi^+ meson. *Phys. Rev.* 81:644.

1953

With V. L. Fitch. Studies of X-rays from mu-mesic atoms. *Phys. Rev.* 92:789.

1954

With S. Koslov and V. Fitch. Experimental study of the mu-meson mass and the vacuum polarization in mesonic atoms. *Phys. Rev.* 95:291.

Two theoretical papers followed.

1954

With L. N. Cooper. Theory of multiple Coulomb scattering from extended nuclei. *Phys. Rev.* 95:1107.

1955

With A. Pevsner. Phase shift optical model calculations for the elastic scattering of pions on aluminum. *Phys. Rev.* 100:1431.

Five experimental papers describing experiments on the elastic scattering of pions from various materials done with graduate students come next. After this Rainwater's interest turned again to neutron spectroscopy. Using the Nevis cyclotron as a source of measurements to much higher energies. This work resulted in more than 30 papers and continued until cyclotron was finally shut down in 1978.

Jim published a major review paper in 1957, viz.

1957

Mu meson physics. *Annu. Rev. Nucl. Sci.* 7:1-30.