Scintillating Days With Rutherford

May 30, 1988
ETS WALTON

[Ed. note: E.TS. Walton and John Cockcroft made history in the early 1930s by bombarding atomic nuclei with accelerated protons and "splitting the atom." But experimental physics was a low-tech, low-budget enterprise, then compared to today. "We had to make various parts of our apparatus," Walton recalled in a recent interview with The Scientist*'s Bernard Dixon. "But before requesting the necessary materials, everyone was expected to see if items salvaged from unwanted apparatus could be used. I wasted lots of time searching for suitable screws in a large box of rusty ones."]*

Now aged 84, Irishman Walton still drives over from his home in Dublin for seminars at Trinity College, from which he retired as Erasmus Smith's Professor of Natural and Experimental Philosophy 14 years ago. He is principally famed for his work on the structure of the atom, for which he shared the 1951 Nobel Prize with Cockcroft. But Walton's eclectic career also includes important work in fields as diverse as microwaves and hydrodynamics.

Here he describes his exciting early days in Ernest Rutherford's laboratory.

After graduating in physics and mathematics at Dublin's Trinity College in 1926, it was natural that I should want to go to the University of Cambridge, England, which was noted for its preeminence in both of these subjects. Ernest Rutherford was director of the Cavendish Laboratory at Caimbridge and the world leader in studying radioactivity which had been discovered in 1896. Physicists from many other countries were anxious to do research under him, but lack of space and facilities limited the number of research students to about 50. In 1927, I became one of the fortunate few.

Considering his momentous achievements, especially his discovery of the nuclear structure of atoms and the transmutation of elements—Rutherford was surprisingly modest. He used to say that, being a simple person, he believed all basic ideas in physics would prove to be simple too. Most of Rutherford's research was conducted with very simple apparatus—I suspect that he believed more time would be spent keeping complicated equipment in working order than in getting results.

Rutherford was also very reasonable in his dealings with research students He did not expect everyone to be a genius. Realizing that students would be nervous during the PhD oral, he used to begin with some easy questions. On one occasion, a fellow examiner asked an atrocious question, which had the candidate struggling. Rutherford interrupted, "It is obvious that the student cannot answer that question." he said, "I cannot answer it myself. Please tell us your answer." The hapless examiner had to admit that he couldn't do so.

Rutherford's fellow-feeling for students may have come from his own sporadic lapses in completing mathematical calculations when lecturing. On one occasion, he tried to calculate the radius of an electron using classical theory, but failed. He told the class to work it out for themselves, and added they should not worry unduly because the result was not particularly meaningful anyway.

Of course, Rutherford had his faults. He was very jealous of his position as director of the Cavendish, and it was extremely unwise for any research student to suggest ways in which the place could be better run. He was criticized, especially by the assistant director, James Chadwick (discoverer of the neutron), for not throwing his weight around to secure more financial support. The laboratory was certainly poverty-stricken. Simple instruments like ammeters and voltmeters were in short supply. And some research students even had to spend their own money on apparatus they could ill afford.

Sir Hugh Anderson, master of Caius College, Cambridge, knew that additional finance was badly needed but could not be provided by the university. So he persuaded some wealthy friends to give £2,000 per annum over several years. That was a lot of money in those days. Yet without consulting anyone, Rutherford decided to have nothing to do with this generous offer. Chadwick thought that Rutherford was afraid he might have to write long reports, and that he would not be able to justify such a large expenditure.

Soon after I arrived at the Cavendish, Rutherford summoned me to discuss a line of research. He knew that if he decided on a problem that really interested a student, he would get much better work. So he asked whether I had any ideas.

I suggested a method of generating fast electrons by keeping them in a circle that was a line of an electric field (as in a modern betatron). This was lucky, because unknown to me, in his annual address as president of the Royal Society a few weeks earlier, Rutherford had pointed out the need to develop ways of producing fast particles. He was probably pleased to find a student interested in going into this difficult field. Rutherford proposed a modification, and I went ahead. But all of my experiments failed, because at that time no one knew how to keep electrons on a circular orbit.

My second suggestion was the method of the linear accelerator, in which high-frequency' voltage is applied to a series of hollow cylindrical electrodes. While I was trying to get the method to work, toward the end of the 1920s, two papers appeared, one by George Gamow in Leningrad and the other by Edward Conon and R.W. Gurney in Princeton. Both applied the new wave mechanics to the problem of the emission of alpha particles from radioactive substances. Gamow subsequently lectured on this in Cambridge, and during a conversation with John Cockcroft they realized that the same theory could be used to calculate the probability of getting fast particles into the nuclei of atoms.

Cockcroft pointed out to Rutherford that the new theory predicted that the protons, accelerated by a few hundred thousand volts, had a significant chance of getting into the nuclei of light atoms such as lithium. Until that time, we all believed that millions of volts would be required. So it was decided that Cockcroft and I should build equipment to produce protons accelerated by up to 300,000 volts.

Initially we adopted vacuum tubes like those made to produce X-rays. But these would not stand up to the high voltages we wanted. Straight cylinders proved much better. Our first ones came from an unusual source: the measurement mechanism in gasoline pumps. We were able to obtain larger tubes later because the making of glass cylinders was a stage in one process for manufacturing sheet glass. Four of these cylinders could be stacked into a column 12 feet high, giving us four rectifiers evacuated by one pump—an important saving.

On April 14, 1932, we placed a lithium target and a fluorescent screen at the bottom of the tube in which protons were to be accelerated. We then focused a microscope on the screen in the hope of seeing some scintillations, which would indicate that a nuclear reaction had occurred. The introduction of the target and screen necessitated letting air into the apparatus. Due to the high voltage and high vacuum, more than an hour was needed to get the apparatus working properly again. As this could be done by one person, John Cockcroft left to give some help to Petr Kapitza in the Magnetic Laboratory.

On completing the conditioning, I left the control table and looked through the microscope. To my delight, there were scintillations, which appeared like those I had read about in books on alpha particles. After carrying out various checks to make sure I was not imagining things, I called Cockcroft. He came over at once, and he too satisfied himself that the scintillations were genuine. Rutherford was then informed, and he joined us immediately. Few things pleased him more than seeing a worker get results. In our case, he was especially interested because of his great love of alpha particles.

Rutherford was a big man, but we managed to maneuver him into the little hut we had built under the accelerating tube to protect us from the X-rays. He peered into the microscope and gave instructions about varying the voltage and proton current, but made no comment on what he saw. Finally, he told us to switch everything off.

"Those scintillations look mighty like alpha-particle ones," were his first words. "I should recognize an alpha-particle scintillation when I see one. I was in at the birth of alpha particles and have been observing them ever since." Indeed, Rutherford was present at their christening too, for it was he who gave them their name. He had also used them as tools in his two greatest discoveries, the nuclear structure of atoms, and the transmutation of nitrogen into oxygen.

Rutherford returned the next day and did some counting of the alpha particles. He insisted that no one be told about our success. If the results became known, he said, a lot of people would want to see the apparatus, and we would get no work done. Over the next four days, this wise precaution enabled us to amass a lot of information about the disintegrations and even to observe the particle tracks in a Wilson cloud chamber.

A few weeks later, Rutherford reported our results at a meeting of the Royal Society in London. The announcement was given headlines in newspapers all over the world. In part, this was because of a play then running in London, the plot of which involved a scientist who had discovered how to split atoms. The power that this gave the fictional scientist enabled him to hold governments at ransom.

Editor's Note (October 15, 2018): Since the original publication of this article, it has come to light that it was not Rutherford, but a research fellow in his lab named Patrick Blackett, who was responsible for conducting, analyzing, and publishing the experiment that first confirmed artificial nuclear transmutation.