DISCOVERY STORY

Search for the Neutron

The Researcher's Personal Account

by
Sir James Chadwick

A few months after his Bakerian Lecture of June 1920, in which he first mentioned what had been in his mind for some time -- the possible existence of a neutral particle formed by the close combination of a proton and an electron -- Rutherford invited me to join him in following up the experiments on the artificial disintegration of nitrogen which he had made in Manchester.

There were a number of reasons for this invitation, so welcome to me. Among them was that I had made some improvements in the technique of counting scintillations, better optical arrangements and a strict discipline; but also he wanted someone to talk to, to while away the tedium of working in darkness.

It was during the periods of waiting to begin counting that he expounded to me at length his views on the problems of nuclear structure, and in particular on the difficulty in seeing how complex nuclei could possibly build up if the only elementary particles available were the proton and the electron, and the need therefore to invoke the aid of the neutron. He freely admitted that much of this was pure speculation, and, being averse to speculation without some basis of experiment, he seldom mentioned these matters except in private discussion. Indeed, I believe that only on one occasion after the Bakerian Lecture did he again refer publicly to his views on the role of the neutron. He had not abandoned the idea, and he had completely converted me. From time to time in the course of the following years, sometimes together sometimes myself alone, we made experiments to find evidence of the neutron, both its formation and its emission from atomic nuclei. I shall mention some of the more respectable of these attempts; there were others which were so desperate, so far-fetched as to belong to the days of alchemy.

Immediately after the Bakerian Lecture, Rutherford had asked J.L. Glasson to look for the production of neutrons when an electron discharge was passed through hydrogen, and a little later J.K. Roberts made a somewhat similar experiment. He did not really expect that any evidence of the neutron would turn up in this way, but it had to be tried. Both the mass of hydrogen and the voltages used in these experiments were quite trivial.

It seemed to me not too unreasonable to look at hydrogen in the normal state, notwithstanding its apparent

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stability. If a close combination of proton and electron were possible at all, it might take place spontaneously; and the neutron so formed might break up again under the action of the cosmic radiation. With Rutherford's approval, I tried in 1923, to detect the emission of γ radiation from the formation of neutrons in a large mass of hydrogenous material, using an ionization chamber and a point counter as the means of detection.

A few years later in 1928, Geiger and Müller devised what is now universally called the Geiger counter, which enormously increased the ability to detect γ radiation. Geiger very kindly sent me two of his new counters, as well as instructions for making them. Immediately, Rutherford and I used this new instrument to repeat the experiment with hydrogen. We went to all manner of tricks in the hope of finding some trace of the neutron. We also examined in the same way some of the rare gases, and any rare element we could lay our hands on, just in case any sign of the formation of the neutron or its emission might turn up. I mention these experiments only in a general way because some were quite wildly absurd.

After my first attempt in this way I considered the possibility that the neutron could be formed, or exist, only in a strong electric field; and that perhaps one might find some evidence by firing fast protons into atoms, especially those of higher atomic number where some electrons were tightly bound. This was the vague idea behind the remark, in a letter to Rutherford, which is quoted in Eve's Life* (p. 301): "I think we shall have to make a real search for the neutron. I believe I have a scheme which may just work..." I thought that at least 200,000 volts would be necessary for the acceleration of the protons. No suitable transformer was available and, although Rutherford was mildly interested, there was no money to spend on such a wild scheme. (I might mention that the research grant of the Cavendish was about 2,000 pounds sterling a year, little even in those days for the amount of work which had to be supported.) I persisted with the idea for a year or two, and in the intervals between other work I tried to find a way of applying Tesla voltages to the acceleration of ions in a discharge tube. I had quite inadequate facilities, and no experience in such matters. I wasted my time -- but no money.

During our work on the disintegration of the lighter elements by α particles, Rutherford and I had not been unmindful of the possibility of the emission of neutrons, especially from those elements which did not emit protons. We looked for faint scintillations due to a radiation undeflected by a magnetic field. The only specific re-

* Eve, Arthur S. "Rutherford - Being the Life and Letters of the Right Honorable Lord Rutherford, O.M." (McMillan, 1939).

ference to the search for the neutron in this way was made in a paper published in 1929, some years after the experiments.

The case of beryllium was interesting for two reasons. It did not emit protons under α particle bombardment; and -- though a false argument -- the mineral beryl was known to contain an unusual amount of helium, suggesting that perhaps the Be nucleus split up under the action of the cosmic radiation into two α particles and a neutron.

This matter intrigued me off and on for some years. I bombarded beryllium with α particles, with β particles and with γ rays, generally using the scintillation method to detect any effect. In those days this was the only method of much use in the presence of the strong γ radiation of the radium active deposit, the chief source of radiation available to me. Quite early on, too early perhaps, I tried to devise suitable electrical methods of counting. I failed. Later, when the valve amplifier method had been developed by Ercinacher, and put into use in the Cavendish by Wynn Williams, I was also able to make a polonium source, small but just enough for the purpose. With Constable and Pollard, I had another look at beryllium, and for a short but exciting time we thought we had found some evidence of the neutron. But somehow the evidence faded away. I was still groping in the dark.

The first indication of the neutron came in the work of H.C. Webster on the γ radiation excited in beryllium by α particle collisions. I had had such work, the excitation of γ rays by bombarding light elements with α particles, in mind for some years. An attempt had been made by L.H. Bastings, but this failed, because the polonium source was too weak and the instrument of detection, the electroscope, too insensitive. When the Geiger counter became available Webster took up this quest, but his first efforts were not very rewarding -- I was still short of polonium.

This deficiency was overcome by the kind intercession of Dr. Feather, then in Baltimore, and the generosity of Dr. C.F. Burnam and Dr. F. West of the Kelly Hospital. They sent me, first by the hand of Dr. Feather and later by post, a number of old radon tubes which together contained what was, for me, a very large quantity of radium D and its product polonium. This gift was of immense value both immediately and later on.

In the meantime, Bothe and Becker had taken up this matter and they were the first to publish results. But Webster made a most interesting observation, that the radiation from beryllium which was emitted in the same direction as the incident α particles was more penetrating than the radiation emitted in the backward direction. This fact, clearly established, excited me; it could only be readily explained if the radiation consisted of particles, and, from its penetrating power, of neutral particles. Believing that a neutral particle would produce tracks, though very sparsely ionized, I suggested that he should pass the radiation into an expansion chamber. To our dismay, for we were convinced that a neutral particle of some kind was involved, no such tracks were to be seen. We were very puzzled; we did not know how to reconcile the observations.

This near-miss occurred in June 1931. Shortly afterwards, Webster left Cambridge for Bristol. I decided to take up the matter afresh, but my preparations were delayed, perhaps fortunately, by a change of my working quarters to another part of the laboratory. Then one morning I read the communication of the Curie-Joliots in the Comptes Rendus, in which they reported a still more surprising property of the radiation from beryllium -- a most startling property -- that of ejecting protons from matter containing hydrogen. Not many minutes afterwards, Dr. Feather came to my room to tell me about this report, as astonished as I was. A little later that morning I told Rutherford. It was a custom of long standing that I should visit him about 11 a.m. to tell him my news of interest and to discuss the work in progress in the laboratory. As I told him about the Curie-Joliot observation and their views on it, I saw his growing amazement; and finally he burst out: "I don't believe it." Such an impatient remark was utterly out-of-character, and in all my long association with him I recall no similar occasion. I mention it to emphasize the electrifying effect of the Curie-Joliot report. Of course, Rutherford agreed that one must believe the observations; the explanation was quite another matter.

It so happened that I was just ready to begin experimentation, for I had prepared a beautiful source of polonium from the Baltimore material. I started with an open mind, though naturally my thoughts were on the neutron. I was reasonably sure that the Curie-Joliot observations could not be ascribed to a kind of Compton effect, for I had looked for this more than once. I was convinced that there was something quite new as well as strange. A few days of strenuous work were sufficient to show that these strange effects were due to a neutral particle and to enable me to measure its mass. The neutron postulated by Rutherford in 1920 had at last revealed itself. □