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SOME PERSONAL REMINISCENCES OF MY YEAR WITH CHADWICK AT THE CAVENDISH*

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It is an honor to take part in this commemoration of James Chadwick and his great discovery made half a century ago. I have been asked to give you some personal reminiscences perhaps because I was one of Chadwick's last students before he left the Cavendish for Liverpool. I spent over a year working closely with him. I learned a lot from him, and I shall tell you a little about our interaction.

Why should you listen to personal reminiscences? The older among you may have nostalgic reasons, wanting to hear again about the "good old days." The younger among you might learn that some things were done differently and perhaps better long ago, and, though it may be difficult to bring back some of the old ways of doing business, we should try to do so when we are convinced that they were better. Also, young physicists today hear so much about future physics that they may find that past physics is sometimes more exciting to listen to for a change. Historians of science among you may have come here to learn of some raw material for a more definitive history. They must keep in mind, though, that memory is selective and protective of the ego. There are usually contradictions between the reminiscences of different scientists which they have to sort out to get, if not the unattainable goal of "ultimate historical truth," at least a more consistent picture. In personal reminiscences we can mention ideas and experiments which did not succeed and often did not lead to publications. We can tell of near misses because one was either too "clever" or had too insensitive a detector or too weak a source, and we can talk of motivations and other

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relevant information not usually mentioned in journal papers. Historians of science might appreciate it if authors would send along with the papers they submit to scientific journals a note in a sealed envelope giving their motivations, how their ideas arose, who affected their thinking, etc. This should not be made generally available, say, for 50 years.

Well before Chadwick discovered the neutron he had already distinguished himself in nuclear physics. Among his most important contributions was the discovery he made when he was only 23 years old, that the beta spectrum is continuous, rather than a collection of discrete lines as was then believed. He was at that time working with Geiger in Berlin. It was 1914, and when the first world war started the Germans interned him in a civilian camp where he stayed for four years, amusing himself by doing experiments on radioactivity. He also interested another internee, C. D. Ellis, - later famous for his work on beta-rays - in the subject. They investigated a German toothpaste, advertised as radioactive, and for a while they thought they had discovered a new radioactive series. The bad food at the Camp ruined Chadwick's digestion, and he suffered from this for the rest of his life.

Chadwick was 20 years older than I, and he in turn was 20 years younger than Rutherford. Twenty years is a long time in the development of physics. It covers something like five successive generations of research students, with each generation confronting physics in a different state of development.

I came to Cambridge 49 years ago and thus missed the excitement at the Cavendish when the discovery of the neutron was made. At that time I was still a student at Berlin University, where I had taken a course in nuclear physics given by Lise Meitner. When Chadwick's discovery became known, Lise Meitner reported on it in an extremely well attended colloquium. She was so excited that she talked of the neutron hitting a "brass nucleus," but after all you might say that this "nucleus" has fewer "isotopes" than the tin nucleus!

Early in '33 it became clear that I had to interrupt my studies in Berlin. I had gone as far as to talk to Schroedinger about a possible theoretical thesis, but soon we both decided that the time had come to leave Germany. I wrote to a number of physicists. Rutherford was the first to answer, accepting me at the Cavendish. I came up to Cambridge, I believe in August, to find out more details about the life of a student here. I had heard that the cheapest way to study was to become a member of Fitzwilliam House, but when I met Chadwick and told him about this he made the somewhat cryptic remark: "If I were you, I would join a college. They do things for you." Then I met David Schoenberg and asked him to tell me about some good colleges. Pointing to Trinity Street, he said, "In this direction you will find Trinity, St. John's, and Magdalene."

I walked over to Trinity and was told they were already full up. At St. John's I heard that they would let me know in six weeks. At Magdalene the Senior Tutor, V. S. Vernon-Jones, said, "Ah, you are a refugee; I suppose we ought to have one." Then he added, "I suppose you have no money. We better give you a hundred pounds," which was about one half of what a research student needed then for a year. Chadwick had given me good advice. Some people find it hard to believe now that there was so little red tape in those days.

In the spring of 1934 I worked on the role of nuclear spin in the disintegrations studied by Cockcroft and Walton and also wrote a note on what is now known as "delayed neutrons." I went to see Chadwick on two occasions to discuss my work with him. On one of these occasions I found the courage to suggest the photodisintegration of the deuteron (then called the diplon at the Cavendish). The photodisintegration of the deuteron seemed a way to obtain a precise mass for the neutron, and this interested Chadwick. At that time the Joliots claimed a rather high mass for the neutron, Ernest Lawrence argued for a small one and Chadwick's value was in between, though lighter than the proton, apparently confirming Rutherford's 1920 suggestion of a proton tightly binding an electron to make a neutron.

About six weeks after this conversation, Chadwick said to me: "Were you the one who suggested the photodisintegration to me? Well, it works; it worked last night. Would you like to work with me on this?" I immediately said yes, and then got Fowler's blessing. Since Rutherford was formally in charge of all research students, whether they were doing theoretical or experimental work, I assume that Chadwick discussed this change with him at one of their daily meetings. It was generous of Chadwick to include me. We worked intensely for about two months, and found that the neutron was definitely heavier than the proton, even heavier than the hydrogen atom. This has remained a puzzle now "transferred" to the masses of the quarks which are believed to make up the nucleons. I remember being shocked by the realization that an "elementary particle" like the neutron might decay by β-emission. I estimated its half-life as $\sim$30 min, using the empirical energy-lifetime relation of Sargent. This was of course an overestimate since it used data from complex nuclei.

When we started writing a note to Nature, sometime in the latter part of July, we noticed an interesting conflict between our results for the photodisintegration cross section ($D + \gamma \rightarrow p + n$) and observations made by D. E. Lea earlier. He had bombarded paraffin with fast neutrons and discovered γ-emission which he ascribed to the inverse reaction $p + n \rightarrow D + \gamma$.

We realized that our measured photodisintegration cross-section would lead—regardless of specific assumptions—to a prediction for the cross-section for the inverse reaction smaller by many orders of magnitude than that reported by Lea. Thus, the effect Lea observed could not be due to the primary neutrons from his source. Early drafts of our paper—which I still have in Chadwick's and my handwriting—contained a speculative interpretation in which we assumed that the fast neutrons are first slowed down and then captured as slow neutrons, but Chadwick said a day or so later, "Let us not speculate." Because of Rutherford's influence speculation was somewhat frowned upon at the Cavendish. By that time I had absorbed enough of the Cavendish spirit against speculation to agree. We merely pointed out the puzzle we had encountered, and ended with the bland statement: "It therefore seems very difficult to explain the observations of Lea as due to the capture of neutrons by protons, for this effect should be extremely small. A satisfactory explanation is not easy to find and further experiments seem desirable." A few months later, when Fermi and his collaborators discovered slow neutrons, the paradox we had noticed was resolved. Rutherford sought me out, uncharacteristically agitated about the fact that this discovery had gotten away from the Cavendish. A psychohistorian of science might suspect that it was he who talked Chadwick out of our speculation.

After the discovery of slow neutrons, we were mentally prepared to exploit it. We started by searching for the disintegration

$$^{6}Li + n \rightarrow {}^{4}He + {}^{3}H$$

and found a good yield. This reaction is sometimes called a fusion reaction and sometimes a fission reaction. We then bombarded many more elements and found reactions in boron and nitrogen.

When we exposed uranium to slow neutrons, we hoped to find long-range α-particles (a phenomenon which was later observed). We therefore "cleverly" covered the uranium target with an aluminum foil thick enough to stop the naturally emitted α-particles, i.e., also thick enough to stop fission particles!

The Li, B, and N reactions have had an impact out of proportion to the ease with which they were found. Like most discoveries they can be used for good or ill. Reactors have made it possible to use the Li reaction to make large quantities of ^{3}H, important as a tracer and for fusion reactions; its daughter product ^{3}He is important in low temperature research—a "cold war surplus." The B reaction is important as a neutron detector (especially in the BF_3 counter, of which we built one). This reaction also holds some promise in cancer treatment. The N reaction leads to the important tracer ^{14}C, made in reactors and naturally by cosmic-ray neutrons in the atmosphere.

Together with H. J. Taylor I studied the boron and lithium reactions with a new technique: "impregnated" photographic emulsions (now called loaded emulsions). Our studies added to other discrepancies between the measured nuclear reaction energies and the values deduced from Aston's atomic masses and led me to suspect that his $^{12}C/^{4}He$ ratio, which was an important standard, was in error. Freed from this "holy" ratio Bethe, as well as Rutherford and Oliphant, recalculated the atomic masses from nuclear reactions and obtained more definitive values.

When Chadwick was packing up in the Summer of 1935 to leave for Liverpool, I visited him at his house where he rewrote our final paper on the photodisintegration investigations for the Proceedings of the Royal Society in his beautiful handwriting (secretaries were still a rarity then). He told me that he used a simple principle to decide what he would take along. If he would not need it for the next two weeks, he would leave it behind. He came across the paraffin wax he had used to knock out protons when he discovered the neutron and said to me: "Do you want it; I don't need it." I was glad to get this historical memento. It is now on loan to the Science Museum in Kensington and is being shown at the exhibition at this Conference.

The Cavendish has the distinction of having started the electronic age with the discovery of the electron in 1897 by J. J. Thomson and what one might call the "neutronic age" which started there in 1932 with Chadwick's discovery of the neutron. The electronic age extends our senses, and the neutronic age may still bring us to our senses.