THE EARLY DAYS OF THE CANADIAN AND BRITISH ATOMIC ENERGY PROJECTS

by

Sir John Cockcroft

The interest of scientists in Canada and Britain in atomic energy goes back to Rutherford's days in the McGill University in Montreal when he first speculated on the enormous amount of "Atomic Energy" locked up inside the nucleus of the atom. Throughout his days at the Cavendish Laboratory he was sceptical about the possibility of releasing this energy and it was a tragedy that he died just before Hahn's discovery of nuclear fission, the key to the release of atomic energy.

Serious work on the release of atomic energy by nuclear fission started in Britain late in 1939 in Chadwick's laboratory in Liverpool and was reinforced by calculations of Frisch and Peierls in Birmingham. This work led in due course to the atomic bomb. A second line of work started in Britain with the arrival in June 1940 of Halban and Kowarski from Joliot's laboratory, carrying with them 180 liters of Norwegian heavy water, and I was able to obtain primitive accommodation and facilities for them in the Cavendish Laboratory. They were joined by a few physicists who had not gone off to work on radar and they concentrated on establishing the possibility of a chain reaction in a heavy water pile. Halban and Kowarski's experiments in Cambridge in 1940 showed that a divergent chain reaction, maintained by slow neutrons, could be produced in a mixture of uranium oxide and heavy water.

In the High Voltage Laboratory of the Cavendish, Bretscher and Feather worked on the production of trans-uranium elements. McMillan and Abelson in America had shown the existence of a new element 94 formed from U^{238}. Bretscher and Feather predicted at the end of 1940 that this new element would show a capacity for fission - by both thermal and fast neutrons - even bigger than that of U^{235} and might form a very powerful explosive. The Cambridge equipment was not powerful enough to isolate 94 and I sent off a cable asking Ralph Fowler, our scientific representative, to urge Ernest Lawrence to arrange for a study of the fissile properties of 94. This work, however, had already been started by Seaborg, and by May 1941 he had experimental proof that plutonium-239 was fissile by slow neutrons.

The work of both groups was co-ordinated by the so-called MAUD Committee, and by July 1941 the Committee reported that the atomic bomb and the so-called "boiler" were feasible. The report on the

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"boiler" stated that the Halban-Kowarski experiments indicated that it should be possible to develop a nuclear energy reaction using uranium and heavy water, that such a "boiler" could be used as a prime mover, and that it promised to have considerable possibilities for peace-time development. It was not thought to have great value in the war, though the work of Bretscher and Feather on substances produced in a "boiler" might be important for a bomb. At that time, in mid-1941, not having received the news of the experimental proof of the fissile properties of plutonium, the MAUD Committee physicists considered that only the U^{235} bomb was likely to have any real war-time importance and the "boiler" had in their minds a much lower importance. It was therefore considered that in view of the great load on British war effort the "boiler" project should be moved to the United States or Canada. After the MAUD Committee reported, it was somewhat abruptly dissolved on the formation of the Tube Alloys project and most of the UK nuclear

physicists concerned with the work of the Committee lost touch with the work for over two years, though I learned early in 1943 through unofficial channels of the successful start-up of Fermi's graphite reactor.

During this period exchanges with the USA were cut off on the formation of the Manhattan Project in September 1942 and were not resumed until the end of 1943, following the Quebec Agreement. In January 1943 the British-French group had moved to Montreal and were joined by George Laurence and other Canadian scientists and by Auger, Paneth, Placzek, Seligman, Newell (an ICI engineer) and others in the half-empty University of Montreal. For the first year no uranium and no Canadian heavy water was available since both were now controlled by the Manhattan Project; so little progress could be made. After the Quebec Agreement of August 1943 collaboration with the USA again became possible, and when on a mission in November 1943 I met Chadwick and Oliphant, they suggested that I might move to Montreal. By 13 April 1944 approval was given by the "Combined Policy Committee" set up under the Quebec Agreement to construct jointly a 5 MW heavy water pile in Canada. On 19 April I was asked by Sir John Anderson, Minister in charge of the Tube Alloys project and Chancellor of the Exchequer, to fly at once to Montreal to take charge of the joint project, and on 26 April I was met in pouring rain at Montreal airport by G. Placzek, the distinguished theoretical physicist. On 6 May General Groves, Dr. C. J. Mackenzie and I looked hopefully at a delightful site for the reactor at Nobel on Georgian Bay taken up by a factory used for making dynamite, but unfortunately a change in the war situation prevented its release and we had to search again.

On 4 May I visited the Metallurgical Laboratory at Chicago, directed by Arthur Compton, and discussed and agreed on the future relations between Chicago and Montreal. Interchange of information was arranged on reactor physics and problems of building a heavy water pile. Uranium metal and heavy water were to be provided. Information on the chemical separation process for plutonium was not to be provided, but it was agreed to provide Montreal with irradiated uranium "slugs" from Oak Ridge so that we could work out our own chemical separation process. I was also allowed to visit Oak Ridge on 17 May to see the Clinton Laboratory 2 MW graphite reactor; this visit was of great value for the future programme. On my way back from Oak Ridge I called on General Groves and he agreed to let us have an additional 5 tons of heavy water to build a zero energy heavy water reactor to study heavy water lattices. Kowarski took charge of this project, and with National Research Council engineering help the reactor known as ZEEP was brought into commission in 15 months and has been in use ever since, except for three years from 1947 to 1950 due to insufficient heavy water for both NRX and ZEEP.

On a second visit to Chicago in June we discussed reactor physics data and methods of calculating graphite lattices. After this visit many of us learnt our reactor physics in detail from nine o'clock lectures in Montreal by Volkoff. I also visited Fermi and Zinn and saw Fermi's Mark II graphite reactor. During the same visit I discussed reactor siting policy with Arthur Compton and came to the conclusion from consideration of possible accidents that our reactor should be sited at least 100 miles from any large city. So we searched for a site satisfying this criterion and with plenty of cooling water. By mid-July a good site had been found at Chalk River with a delightful village site at Deep River, and a week later we were able to settle site layouts and a McGill University town planner laid out the village with very good results. We then started the design of the NRX reactor and decided to design for 10 MW on a conservative basis. The basis was so conservative that the reactor later operated up to 40 MW. The general features of the design were worked out by our small group of three ICI engineers together with the reactor physicists, and the detailed design and construction were carried out by Defence Industries Limited. The NRX reactor came into commission three years later and has since been one of the most successful research reactors in the world. In particular, it has provided a good deal of experience on irradiation damage in uranium metal. The first 10 feet of rolled uranium rods used to fatten and shorten by several inches. Later batches would lengthen by several inches. With experience and understanding, the growth problems of uranium were overcome and burn-ups of 3000 MWD/ton were achieved with surface temperatures of about 70°C. This experience was the somewhat dubious origin of target burn-ups for UK power reactors.

During the next year Goldschmidt - and later Spence - worked on the chemical extraction of plutonium from irradiated uranium. Goldschmidt selected triglycol dichloride as a solvent and this was used in the Chalk River pilot plant. Spence carried out a wider survey and worked out the flow sheet using dibutyl carbitol which was later adapted for the Windscale chemical separation plant. Work on radiological protection started with the arrival of Joseph Mitchell and W. V. Mayneord from Britain. A 1000-curie polonium-beryllium neutron source, obtained from the USA, enabled the biological effects of neutrons to be studied, and for the first time I gained from them a quantitative appreciation of radiation hazards.

By May 1945 we were discussing breeder piles, convertor piles and power piles, though little effective work was done. We also formed a "Graphite Group" to study the design of a graphite research reactor which we thought might be built in Britain in due course.

I flew to Britain in a flying boat in October 1945 and met Sir John Anderson and discussed the found-

ation of a UK Atomic Energy Research Establishment, and - with Oliphant - searched for a suitable airfield to give us a "flying start". We thought that we should be not too far from a major university so that we could use its libraries, schools, hospitals and other amenities important to scientific staffs. My interest in Duxford near Cambridge was vetoed by my colleagues and we finally chose Harwell and it was ceded to us on 1 January 1946 by the Royal Air Force. I also agreed with Sir Alan Barlow, a senior official of the Treasury, on a charter which gave the Establishment an exceptional degree of freedom in its formative years.

On 1 February 1946 I paid my first visit to the windswept deserted airfield together with Dr. Skinner and Dr. Marley, and Colonel Fisher in charge of the construction plans. The Harwell airfield contained four hangars which were used for housing reactors and accelerators, and workshops and the other buildings were converted quickly to laboratories or living accommodation. We also imported 200 prefabs for urgent housing needs. Design of the alterations and new buildings started in Canada and was quickly transferred to the UK. We decided to build two graphite moderated research reactors. The first, GLEEP, was largely designed by a New Zealand group under Watson-Munro and was completed in about 15 months. It developed 100 kW of heat and was our first source of radioisotopes. It has since been used continually for measurements on nuclear materials by the oscillator method and also for some biological work on the effects of slow neutrons. Our second reactor, BEPO, was designed by our Industrial Group at Risley to develop 6 MW of heat. It came into commission in July 1948 during a meeting of our Technical Committee - the Committee members pushing in the last uranium bars to make it critical. It became under Henry Seligman's leadership and inspiration a principal source of radioisotopes for the world. Today the Radiochemical Centre at Amersham uses and processes isotopes from BEPO and DIDO and 40 000 consignments a year are now despatched to users all over the world.

A good deal of the early Harwell effort was devoted to helping Risley to design the two Windscale air-cooled graphite-moderated plutonium production piles and their associated chemical separation plants and the fuel element production facilities. By 1949 we had some effort to spare to consider nuclear power plants and studied the numerous possible combinations of moderators, coolants and fuel elements. Several early projects were still-born.

In September 1950, however, a Harwell Power Conference attended by many influential industrialists recommended the adoption of a natural uranium-fuelled CO_2-cooled graphite-moderated reactor as a source of heat for a nuclear power station. We made this decision because suitable graphite was available in England, we had little enriched uranium and we had experience of the technology of this type of reactor. Our engineers also reported that electricity should be produced from such a system at less than 1d. per unit.

The decision taken at this meeting was followed by three years of design study and technological research on "Project Pippa". We had to study fuel element problems, graphite problems and reactor physics problems, and for this a combined effort of chemists, engineers, metallurgists and physicists was required. By 1953 Goodlet and Moore put forward their paper on "Project Pippa" - designed to produce 50 MW of electricity from a single reactor at a cost of not more than 1d. per unit.

In April 1953 a Government decision was taken to build Calder Hall as a combined plutonium-power producer and our Industrial Group led by Sir Christopher Hinton took charge of design and construction. The power station was completed within $3\frac{1}{2}$ years and within the estimated cost of £$16\frac{1}{2}$ million - a remarkable achievement for a pioneer venture. Since then three more identical units have been built and will produce about $2\frac{1}{2}$ billion units of electricity this year. They have proved to be remarkably reliable in operation and achieve a 95 per cent load factor between refuelling.

Their successful operation has been the basis for the construction by the Electricity Boards of 8 commercial nuclear power stations of outputs increasing from 275 MW to 580 MW, whilst negotiations have started for a 1000 MW station at Wylfa Head. So, including Wylfa, the total installed nuclear capacity should be about 5000 MW by about 1968.