Chadwick, Liverpool and the Bomb

Thesis submitted in accordance with the requirements of the University of Liverpool for the degree of Doctor of Philosophy

by

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December 1997

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CHADWICK, LIVERPOOL and the BOMB

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CHADWICK, LIVERPOOL and the BOMB

ABSTRACT

The author intends to set out in this thesis Chadwick's scientific and diplomatic contribution to the development of the atomic bombs that ended World War Two. The far-reaching consequences of Chadwick's efforts to establish a nuclear physics research programme, both academic and commercial, in austere post-war Britain are also shown.

A brief history of Chadwick's academic career, at Manchester, Cambridge and Liverpool Universities is given, which indicate how his intimate knowledge of atomic and nuclear physics culminated in the building of a state-of-the-art 37" pole diameter cyclotron at Liverpool University. The help that Lawrence and his colleagues generously and freely gave to Chadwick is also acknowledged.

The crucial role of the Liverpool cyclotron in determining the necessary nuclear fission cross-section measurements needed to establish the feasibility of an atomic bomb, is also discussed. Chadwick's precise and penetrating insight of the bomb's feasibility, was presented in the final Maud Report that was sent, prior to the bombing of Pearl Harbour, to the U.S.A. It was this Report that convinced the Americans that an atomic bomb was a feasible and obtainable objective and started, as a matter of urgency, American fission bomb research.

A brief history of fission and the events surrounding its discovery is also presented, as is a comparative discussion on the German and Japanese progress in atomic bomb research in World War Two.

It would not have been possible to give specific details of many of the above events without primary source material to substantiate them. The author has located a large number of previously un-published letters, documents and photographs - only some of which have been included - but all of which are being incorporated into a Chadwick Archive at Liverpool University.

C.D. King
December 1997

ACKNOWLEDGEMENTS

There are a number of people whom I would like to thank for the considerable help given to me in the preparation of this thesis. I would like to mention the following:

Emeritus Professor J.R. Holt; the late Professor B.B. Kinsey; Professor J. Rotblat; the ex-Head of the Department of Physics, Professor P. Twin, and the present Head and Chadwick Professor, E. Gabathuler.

Chadwick's ex-students, particularly Professor W. Williams, Peter Reynolds, Thomas Randle, Eric Sayle and Stanley Rowlands, for the help they have given me in the course of my research.

The kindness shown to me by Chadwick's daughters, Joanna and Judith, and for their willingness to answer my questions on family background.

My Supervisors, David Edwards and Michael Houlden, for their enthusiastic support, and for their many consultations often given at short notice.

The Archivists and Librarians from the U.K. Universities, and the staff at the Public Records Office, in answering my queries.

The staff and students of the History of Science and Technology Course at Liverpool University, particularly to Peter Rowlands for his script readings and sense of humour.

Finally, I would like to thank my family and friends who have been bored, I am sure, on many occasions by my discussions on atomic matters, and I am most grateful to my wife for the consistant support and encouragement she has given me.

INTRODUCTION

> "A myth is, of course, not a fairy story. It is the presentation of facts belonging to one category in the idioms appropriate to another. To explode a myth is accordingly not to deny the facts but to re-allocate them." - Gilbert Ryle, in his Introduction to *The Concept of Mind.*

Myths breed in the half-light of secrecy and censorship and one of the recurring tales is of the Liverpool contribution to the atomic bomb; how James Chadwick used his new cyclotron to get the experimental numbers to convince firstly the British, and then the Americans, that an explosive device was possible.

When the American-born Nobel Laureate Ben Mottleson was awarded an honorary D.Sc. by the University of Liverpool and stated[1],

> Experiments most crucial to the atomic bomb project were done at Liverpool

was he merely flattering his hosts or recounting the received wisdom of his profession?

The Liverpool bomb work was also given extensive media coverage following the award of the 1995 Nobel Peace Prize to Joseph Rotblat, one of the original participants[2]. Myth or fact?

There are many histories of the atomic bomb project, some specifically written to counteract what the authorities or the authors considered the down-playing of the British contribution. The main aim of this thesis is to describe what actually happened in Liverpool in the early part of the war. That much of the experimental data acquired on the Liverpool machine was later repeated and refined in no way diminishes its relevance to the thoughts of the day. Chadwick's feasibility report to the Maud Committee of July 1941 was deliberately passed to the Americans and secured their involvement long before the attack on Pearl Harbour. The author contends that the 're-allocated' facts greatly enhance the stature of Chadwick and his leadership of the U.K. contribution to the Manhattan Project.

The declassification of the politically sensitive documents after fifty years, and the survival of so many of the staff and students from that era, make this an ideal time to investigate this subject. The author has corresponded with or interviewed all of the surviving team; many have provided anecdotes, photographs and documents, some of which are used in this work. I am particularly indebted to Professor Emeritus John Holt, F.R.S., for providing his complete set of lecture and laboratory note books 1935-1950 and the time to comment thereon. The lecture notes, including the course given by Chadwick himself, provide a unique insight into the undergraduate training of the time. The laboratory note books give a detailed day-by-day account of activity on the cyclotron: this was correlated with a complete set of cyclotron operational log books from the departmental archive.

Important declassified documents were unearthed by the author in the Public Record Office, Kew and at the Atomic Energy Authority. Amongst the Chadwick papers at Churchill College, Cambridge was found a series of Monthly Reports detailing the Liverpool contribution as a real time narrative showing both the successes and the dead-ends such endeavours entail. The author has found in the Liverpool departmental archives a file of Chadwick correspondence which greatly clarified the design and the construction of the cyclotron. Some of this correspondence is correlated to letters in the Berkeley Archives.

Contemporary with this research, and overlapping with it, was the work of Andrew Brown on a biography of Chadwick[3]. The Liverpool portions of his book rely heavily on material supplied by the Liverpool team, including the present author, and is acknowledged as such. Brown's book is treated herein as a valuable secondary source and was written for a quite different readership. A number of the important documents which have contributed to this thesis were discovered after the book had gone to the printers.

The first chapter of this thesis gives a brief account of Chadwick's life and work before his arrival in Liverpool. When the forty-four year old accepted the Lyon Jones Chair in 1935, it certainly was not to retire to a backwater at a run-down provincial university. His idea for building a 37 inch diameter cyclotron was at the cutting edge of big science for that time; his research programme, his frustrations and eventual success are described in chapter two. Thereafter, due to the outbreak of war, his planned research had to be completely changed. Chapter four covers the Liverpool work during the war years during which Chadwick found himself increasingly involved in a political and diplomatic role. Despite the temporal overlap with chapter four, the American work is collected in chapter five.

Chadwick returned from America ladened with honours and prestige and threw himself into the reorganisation of post-bomb nuclear physics in Liverpool. He was deeply involved in the establishment of national programmes of both pure research and commercial exploitation (Harwell, Aldermaston and Amersham); he also encouraged the birth of corresponding European programmes (Euratom and C.E.R.N.). This work is covered in chapter six, which also deals with his tragic return to Cambridge.

In order to retain a narrative flow several detailed sections are presented as appendices. The compilation of data for this thesis has created its own archive of correspondence, photographs and taped interviews. Much of this material has not found its way into this work but has been assembled by the present author and can be accessed in the Chadwick and the Rotblat Archives of the University of Liverpool.

A second myth investigated is that the atomic bomb work was clouded in secrecy. The author has found an article by A.L. Lyon from 'somewhere in England' in the *Sunday Express* of 30th April 1939[4] giving a full description of the fission process and how to make a bomb; its only fault was to underestimate the cost by several orders of magnitude. The circumstances surrounding this remarkable article and the establishment's reaction are described in chapter 2 and appendix 4. Chadwick's attitude to security is also considered. Was he aware, for example, that his young research students were taking snapshots of each other fortuitously providing us with the only known picture of Frisch's optical analogy? Why did Chadwick choose to suppress his joint papers on work done on the Liverpool cyclotron with Alan Nunn May, the infamous Russian spy? What was Chadwick's relationship with Paul

Rosbaud the renowned British spy? The author admits to finding no documentary answers to such questions but provides the facts to seed future myths!

References:

Int. 1. Ben Mottleson, 1975 Nobel Laureate for Physics (with Aage Bohr), in a seminar to the Department of Physics, University of Liverpool, on 18th December 1995.

Int. 2. See the Joseph Rotblat Archive, University of Liverpool.

Int. 3. Brown, Andrew, *The Neutron and the Bomb: A biography of Sir James Chadwick* (Oxford University Press, 1997).

Int. 4. See Appendix 2 for the full page article.

CHAPTER 1
Pre Liverpool

James Chadwick was born on the 20th October 1891, at Clark Lane, Bollington, which is in the Registration District of Macclesfield, in the County of Chester. His parents were John Joseph and Anne Mary Chadwick, formerly Knowles, and the 'Occupation of Father' is given as 'a cotton spinner', as noted on a copy of the Registration Entry of Birth. He had two younger brothers and one sister, but she died at an early age.

Chadwick attended the local primary school and presumably had a conventional early childhood. He resided with his grandmother after his father had left Bollington to set up a laundry business, and it was not financially possible for his parents to send the young Chadwick to the Manchester Grammar School even though he did show an early aptitude for scholarship'[1]. Instead he attended the Manchester Municipal School where he took a keen interest in mathematics and also in physics. He was

> much encouraged by a sixth form master, Mr. Wolfenden, to specialise in applied mathematics[2].

After successfully sitting for two scholarships when he was 16 years of age, and, being allowed to hold only one, he took up the award through the Manchester Education Committee with a view to entering Manchester University to read mathematics.

Chadwick attended the initial interviews at the start of term and found himself being interviewed to read physics instead of mathematics. Due to his shyness he did not remedy the situation and found himself enrolled on the physics course.

During Chadwick's second University year, he attended Rutherford's lectures and found them stimulating, which no doubt helped to increase his enthusiasm for physics - this enthusiasm was lacking during his first year! After successfully sitting the main examination at the end of his second year, Chadwick had the choice of reading electrical engineering or physics and he decided to read physics.

Chadwick took his final examination in Honours Physics in 1911, obtaining a First Class Pass. He registered with the University for an M.Sc. and was accepted as a research student. Two years later, in 1913, he gained his M.Sc. His time during those two years must have been stimulating to him as, besides working with A.S. Russell on the separation of various α-particle emitting substances, he took a keen interest in following the progress of the now classic work of Geiger and Marsden on the scattering of α-particles. Also during this period, Rutherford announced his nuclear theory of atomic structure at a meeting of the Manchester Literary and Philosophical Society which 'was naturally attended by the physics research students'[3].

During the period 1911 to 1913, there were many well known scientific people at Manchester University, including Moseley and Hevesy, and for about five months in 1912, Niels Bohr. Chadwick and Bohr remained lifelong friends.

After obtaining his M.Sc. degree in 1913, Chadwick was recommended by Rutherford for an Exhibition of 1851 Senior Research Studentship. One of the Award conditions stipulated that the holder of the award must carry out research work in a laboratory other than that in which he had been working. Chadwick, who wished to continue the study of radioactivity, found there were no other laboratories in the U.K. which had facilities or an interest in this line of research. He therefore looked to the continent and considered the Paris laboratories, under Curie - where the work was mostly on chemistry - and the Berlin laboratories under Geiger. He chose the latter and spent the next year, 1914, at the Reichsanstalt with Geiger. War was declared on the 4th August 1914, and Chadwick was soon interned in the stables of a racecourse at Ruhleben, near Spandau.

A description of the conditions in the internment camp at Ruhleben have been provided by Sir Ernest Campbell MacMillan[4, 5] (1893 -1973), who, according to the *Who Was Who, 1971 - 1980*:

> ... was in Germany in the summer of 1914 and after the outbreak of war (was) interned in Ruhleben Camp: while there, he wrote a setting for chorus and orchestra of Swinburne's "England", which was accepted for the degree of Mus.D., Oxford, 1918: after his release he returned to Canada.

Chadwick suffered from the lack of food and the cold but he involved himself in the camp Scientific Society and the results of one of his experiments showed that thorium oxide, used by an acquaintance of his as a toothpaste, was radioactive! It was during his internment that Chadwick and C.D. Ellis, a trainee British Army Engineer Officer, met and established a lifelong friendship. Ellis's introduction to physics at the camp led him on to a scientific career which 'included outstanding research work at the Cavendish Laboratory'[6].

When the armistice was signed in 1918, Chadwick returned to Manchester. There, Rutherford gave him a job which allowed him to regain his health - although his digestion would always be a problem to him, due no doubt to the poor and inadequate diet he had received - and to earn a little money. He became involved in some of Rutherford's experiments on artificial disintegration by α-particles, and when Rutherford accepted the Cavendish Chair of Physics at Cambridge in 1919, he invited Chadwick to accompany him. Gonville and Caius College offered Chadwick a Wollaston Studentship worth £120 a year and, for the next 16 years, Chadwick remained at Cambridge.

Rutherford had decided to concentrate his research activities at the Cavendish on radioactivity and related subjects - atomic, nuclear or particle physics as it would now be known - and Chadwick was an able and willing assistant. In 1921, Chadwick was elected to a research fellowship at the College which gave him a secure income of £350 per annum. Just over a year later, on a recommendation from the Chairman of the Department of Scientific and Industrial Research (D.S.I.R.) that Rutherford's research work load be relieved by an assistant, and Chadwick was appointed Rutherford's Assistant Director of Research.

In consultation with Rutherford, Chadwick would discuss suitable projects, allocate them to the research students and ensure they had the necessary and essential

Photograph 1.1.1
Chadwick, right, at the wedding with Peter Kapitza, best man.
(ref: *Cockcroft and the Atom,* Hartcup and Allibone, p.33.)
August 1925

equipment to complete their project. As the laboratory had a very small research budget, this was no mean task as every application to spend money on items for the projects was subjected to a very careful scrutiny by Rutherford.

By the end of 1921, Chadwick had published, apart from his Ph.D.[7] and M.Sc. theses, some 12 scientific papers, all of them concerning some aspect of radioactivity, including disintegration and collision of particles, and excitation of various lighter elements.

In 1925, Chadwick married Aileen Stewart-Brown of Liverpool (see photograph opposite). According to *The Times* newspaper of Friday, 14th August 1925, the Chadwick's marriage took place:

> On the 11th August, at St. Anne's Church, Aigburth, Liverpool (and conducted) by the Reverend Stephen Leadley Brown, cousin of the bride. Dr. James Chadwick, of Caius College Cambridge, son of Mr. J.J. Chadwick to Aileen, elder daughter of Mr. and Mrs. Hamilton Stewart-Brown, Oakfield, Grassingdale, Liverpool.

The Chadwicks stayed in a furnished house in Cambridge until, after about a year, they were able to move into a house they had had built.

Rutherford and Chadwick worked well together and for this to happen it was necessary for Rutherford to be able to rely on Chadwick and his judgement. The extent of Rutherford's trust in Chadwick, for example, can be seen in the way in which he sent Chadwick to the Radium Institute in Vienna in 1926, after a scientific disagreement had occurred between Kirsch and Pettersson of Vienna and Rutherford and Chadwick in Cambridge. Instead of letters to the scientific journals as many scientists would have done, making matters very public, the visit by Chadwick to Vienna quickly sorted things out. The difference between the two groups came about over the visual counting of scintillation flashes. The Viennese counting was done by three women who were informed and knew what the probable number and rate of the scintillations should be at any time during the experiment. Chadwick soon appreciated what was happening and when he conducted counting experiments without informing the ladies of the type of experiment, the counting numbers and rates agreed with Rutherford's and Chadwick's. The source of the disagreement was thus quickly found and revealed, without undue or adverse publicity. Chadwick, in fact, became a firm friend of Pettersson and they often met when the Chadwicks were on holiday in Sweden.

There were, however, disagreements between Rutherford and Chadwick. These invariably occurred over the provision of equipment for experiments. Rutherford had, during his scientific career, been able to prove, and make, many far-reaching conclusions by utilising his ingenuity in the manufacture of apparatus from every day artifacts, for example, tobacco and cocoa tins, and by thinking hard about the subject. He expected his post-graduate students and others to do the same. While this is an excellent basis for experimental research, it was necessary for some apparatus to be obtained. Chadwick, amongst his other duties, had to balance the needs of the students against the expense of obtaining suitable apparatus for them. Chadwick must have grown increasingly frustrated over the years as physics became more and more 'high tech', as Rutherford was very reluctant to spend money on new equipment.

Chadwick discovered the neutral particle, the neutron, in 1932. Rutherford had predicted the existence of such a constituent of the nucleus in his Bakerian Lecture of 1920 to the Royal Society. It was one of three prophecies made by Rutherford in this lecture. The first was that it was possible for a nucleus to exist having a mass of two units and a charge of one unit which, with its electron, would behave chemically like hydrogen; that is, would be an isotope of hydrogen. It was discovered by Urey 11 years later and now known as heavy hydrogen or deuterium. The second prophecy was of the possible existence of a lighter isotope of helium, which was also discovered later, and the third prediction was of the possible existence of the neutron.

When Chadwick finally was able to prove the existence of the neutron and show the experimental procedure[8, 9], it opened up the possibility of the invasion of the nucleus in a way impossible before. The great advantage of a neutron is that it has no electric charge and is therefore unaffected by the charges in and around nuclei. This means that it is able to penetrate nuclei without having to overcome the intense electric fields normally surrounding them.

It is noted that Chadwick also published a paper the following year, 1933, 'The Neutron and its Properties' in the *British Journal of Radiology*[10]. He was even then thinking of the possible advantages of using neutrons in medical applications.

The Cavendish Laboratory was to have another spectacular success later in 1932 when John Cockcroft and Ernest Walton were, with their high voltage generator, able to produce particles accelerated by potentials of nearly 700,000 volts which were found to be sufficient to penetrate the lithium nucleus. The report in *Nature* on 30th April 1932[11], states:

> the lithium isotope of mass 7 occasionally captures a proton and the resulting nucleus of mass 8 breaks into two alpha-particles, each of mass four and each with an energy of about eight million electron volts. The evolution of energy on this view is about sixteen million electron volts per disintegration.

and the full atomic equation

> for this first man-made artificial disintegration of any atom may be written as:-
> Lithium + Hydrogen = two atoms of helium + some energy
> Li + H = He + He + 17 Mev [12]

The discovery of the neutron opened the flood gates for research in nuclear physics, and it gave a fresh impetus to the workers in this field for a number of years. In 1936, for example, Niels Bohr and his colleagues in Copenhagen were able to make use of not only the result but also the method of production of neutrons, when they explained and demonstrated the capture processes of neutrons in the nucleus[13].

Chadwick was awarded the Nobel Prize in Physics for the discovery of the neutron in 1935, just four months after his arrival at Liverpool University.

The Solvay Conference held in Brussels during October, 1933, was 'one of the few meetings which Chadwick attended'[14]. He did not relish the idea of attending large conferences, preferring to work at the Cavendish and pursuing his research activities. But, at this Conference, Chadwick listened to and met, amongst others, Ernest Orlando Lawrence who enthusiastically expounded the research opportunities which he, Lawrence, was convinced would be opened up by his new, high energy, particle accelerator, a 'cyclotron'[15, 16].

A brief description of the simplified theory and mode of operation of a cyclotron may be found in appendix 1.

When Chadwick returned to Cambridge after the conference, he was more than ever convinced that the way forward in nuclear physics lay in having machines, such as the cyclotron, which would enable penetration of the nucleus and allow the study of its constituents. But, 'Rutherford had a horror of complex apparatus'[17], and Chadwick's request and the arguments put forward to build high energy apparatus were firmly rejected, on the grounds of cost and necessity. It is noted that this, however, had not stopped Cockcroft in obtaining funding, and building, his and Walton's high voltage generator a couple of years previously.

There were four options available at that time, 1933 (other than Cockcroft and Walton's Generator) to enable study of particle interactions to be pursued. Firstly, by using the naturally occurring cosmic rays as a source of energetic particles; secondly, by utilising alpha-particles (with their limited energy), thirdly, the newly discovered Van de Graaff high voltage generator, and finally, the cyclotron.

This author considers that the period between 1933 and 1935 was a very frustrating time for Chadwick. He knew the direction that he wished his research to take and he knew that Rutherford would not allow money to be spent on suitable equipment.

Between the 15th and 21st April 1969, Chadwick gave a daily series of taped oral interviews with Charles Weiner, Director of the American Institute of Physics (A.I.P.), Center for the History of Physics,

> as part of the general program of the AIP Center for History of Physics to document the history of twentieth-century physics and astronomy[18].

Chadwick told Weiner, in relation to the period prior to taking the Liverpool Chair, that

> ... it was becoming very difficult to push on without some new equipment. I couldn't get any further with what I had. I was at an end really with the equipment which I had or could see myself getting, and it was quite clear to me, as it was no doubt to others, that we needed a means of accelerating protons or other particles, particularly protons, at high energies. But that meant more space, particularly more money, and particularly engineering. It meant complicated equipment, and Rutherford had a horror of complicated equipment[19].

And Chadwick recalled in his interview with Merrison in 1973 that he thought Rutherford realised:

> ...that the mere building of a cyclotron would absorb a lot of people's time which would otherwise have been spent on research, real research. And having got a cyclotron, a good deal of time would be spent keeping it in order. And he begrudged time spent in that way[20].

References :

1.1. *Biographical Memoirs of Fellows of the Royal Society* 1976, Volume 22.

1.2. *ibid.* 1.1.

1.3. *ibid.* 1.1.

1.4. D.N. Edwards, in a *Private Communication* to the author, 30th January, 1996, states that,

> MacMillan, a young Canadian music student at the time of his internment, was trapped by the outbreak of war. It was said, in a radio broadcast of some of MacMillan's music in January 1996, that initially all male nationals of belligerent foreign powers had to register with the German police, had their passports confiscated and were required to present themselves each day to their nearest police station. All females and children were repatriated through neutral countries in late August 1914.
>
> The arrest and internment in England of all German nationals triggered retaliation and the Ruhleben racecourse in Berlin, normally used for trotting races, was hastily converted into a camp to accommodate 4,000 men. The biggest proportion of these, well over 1,000, were merchant seamen taken when British ships were seized in Hamburg and Bremen. There were also about 1,000 tourists and business men and approximately 1,000 academics and students (mostly musicians); the latter providing high-class entertainment with the establishment of a Musical Society early in 1915. MacMillan was said to have enjoyed the enforced extension to his studies, describing the food as no worse than that provided to the general public, and the attitude of their guards as sympathetic rather than hostile.

1.5. See also the Bibliography, Books and Papers; J.D. Ketchum, *Ruhleben: a prison camp society*, University of Toronto Press, 1965.

1.6. *ibid.* 1.1.

1.7. Chadwick's annotated and penultimate copy of his Ph.D. thesis is in the Chadwick archive of the University of Liverpool.
A critique of the thesis has been made in a dissertation by R.W. Bibby dated May 1992 for a B.Sc.(Hons) Physics Project and it may also be found in the Chadwick Archive of the University of Liverpool.

1.8. *Nature*, 'Possible existence of a neutron', Volume **129**, p. 312.

1.9. *Proc. R. Soc. Lond.*, 'The Existence of a Neutron', **A136**, p. 692-708.

1.10. *British Journal of Radiology*, 'The Neutron and its Properties', No.6, pps. 24-32.

1.11. *Nature*, Volume **129**, p. 649.

1.12. Hartcut, G.; Allibone, T., *Cockcroft and the Atom* (Adam Hilger Ltd., 1984), p. 52.

1.13. *Neils Bohr; Collected Works*, General Editor L. Rosenfeld, Volume **1**, p. xiii.

1.14. *ibid.* 1.1, p. 21.

1.15. Lawrence and Edlefson, *Science*, **76**, 1930, p. 376.

1.16. Original signed, *Letter*, Lawrence to Chadwick, dated 31st January 1936, Chadwick Archive, University of Liverpool :-

... cyclotron is slang for magnetic resonance accelerator

1.17. *ibid.* 1.1, p. 22.

1.18. Tape recorded interview with Chadwick, 15-21st April 1969, held in the Neils Bohr Library, Center for History of Physics, American Institute of Physics, second page of preface.

1.19. *ibid.* 1.18, page, Chadwick - 81.

1.20. Chadwick/Merrison oral taped interview (1973), transcribed by the present author, 1994, p. 7, in the Chadwick archive, University of Liverpool.

Photograph 2.2.1
The First Three Lyon Jones Professors of Physics
(Oliver Lodge, Lionel Wilberforce, James Chadwick)
c. 1936

CHAPTER 2
Before the Second World War

On the 15 October 1934, Professor L.R. Wilberforce, the Lyon Jones[1] Professor and Head of Department of Physics at the University of Liverpool, indicated to the Faculty of Science that he was going to retire (see photograph opposite).

> The Faculty learned with great regret of the approaching resignation of Professor Wilberforce

and his succession was then discussed. A Committee was set up to find a new Head of Department. Representatives from the members of the Faculty of Science on the Selection Committee were as follows:

Sir Hector J.W. Hetherington:	Vice-Chancellor of the University of Liverpool
Professor Read:	Chairman of the Faculty of Science and George Herdman Professor of Geology
Professor Abell:	Dean of the Faculty of Enginering and Alexander Elder Professor of Naval Architecture
Professor Lewis:	Brummer Professor of Physical Chemistry
Professor Proudman:	Professor of Oceanography
Professor Rosenhead:	Professor of Applied Mathematics
Professor McLean Thompson:	Dean of the Faculty of Science and Holbrook Gaskell Professor of Botany

and two appointed representatives of Senate:-

Professor Martin:	Dean of the Faculty of Arts and King Alfred Professor of English Literature
Professor Wood:	Dean of the Faculty of Medicine and Derby Professor of Anatomy

On 9th March 1935, the Faculty of Science Minutes of the University of Liverpool records the reception of the 'First Report on the Chair of Physics'[2] recommending Dr. James Chadwick as the best candidate.

The above-mentioned First Report of Physics includes the testimonials of Lord Rutherford and Professors Blackett and O.W. Richardson. According to the Dean, J. McClean Thompson, in the preamble to the First Report:

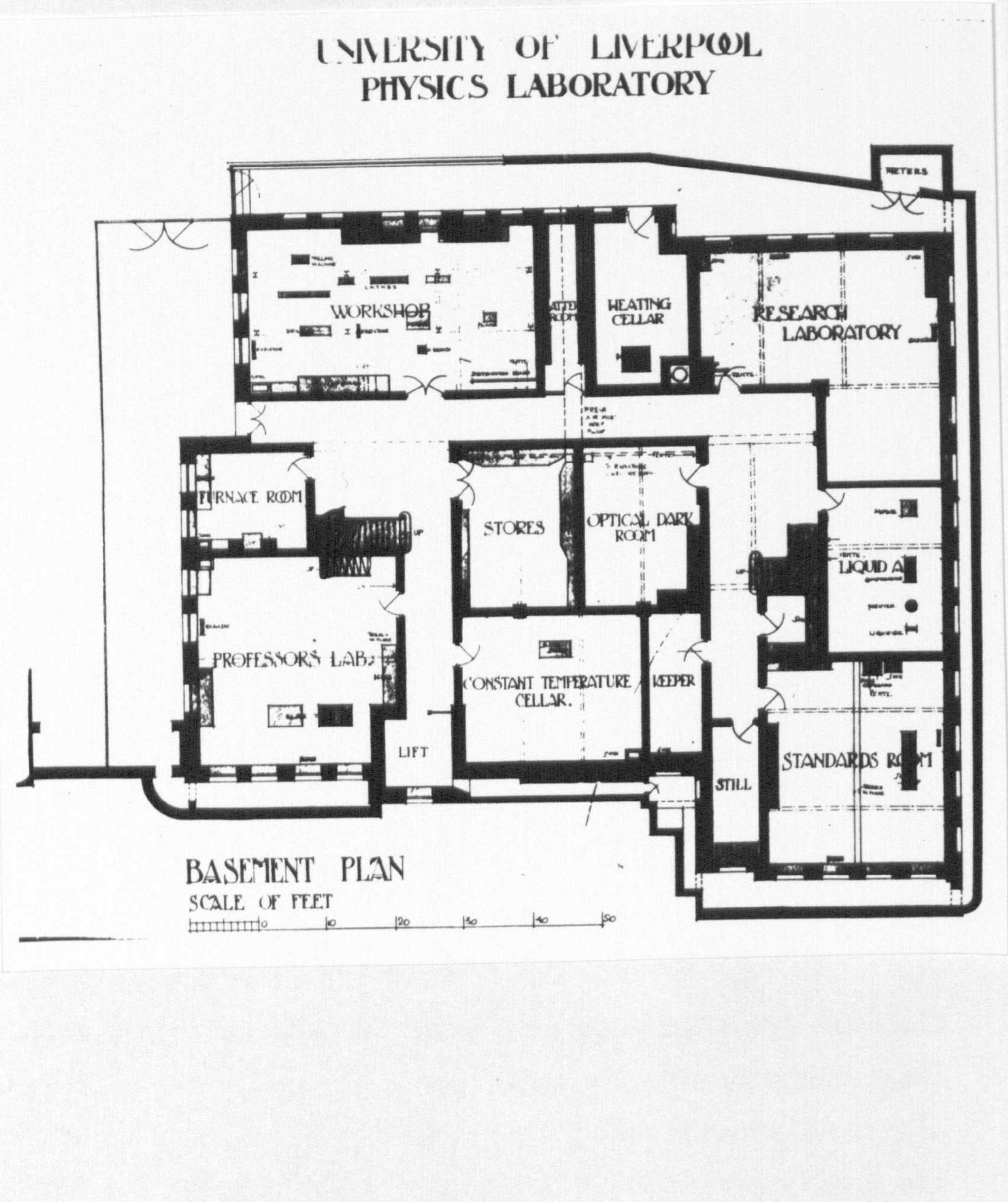
> Preliminary enquiries were accordingly made by Professor Proudman from many sources both at home and abroad as to possible candidates, their standing and their suitability.

It soon emerged that there was no dearth of excellent men of whom not a few had already distinguished themselves as teachers, administrators and investigators. Following a careful scrutiny of the opinions provided by many authorities Professors Proudman and Lewis greatly aided the Committee by preparing a list of twenty-five physicists for consideration.
The Committee chose eight names for further review, and submitted these names to a selected group of external authorities. On the basis of the information thus furnished the Committee came provisionally to the conclusion that the most satisfactory appointment would be that of Dr. J. Chadwick, Fellow of Caius College, and Assistant Director of the Cavendish Laboratory, Cambridge ... With the exception of the Chairman of Faculty the Committee met Dr. Chadwick and unanimously confirmed its previous opinion as to his suitability for the Chair of Physics ... Dr. Chadwick has now intimated that he would be prepared to accept.

March 9th, 1935.

Two days later, on 11th March 1935, a 'Special Meeting' of the Faculty of Science was held in the Senate Room where 23 members of the Faculty were present, but with the notable absence of Wilberforce. It was there agreed to recommend that the First Report of the Committee on the Chair of Physics be accepted and forwarded to Senate and Council, and that Dr. James Chadwick, M.Sc., Ph.D., F.R.S., be invited to the Lyon Jones Chair of Physics as from 1st October 1935: his salary to be £1350 p.a.[3].

Chadwick accepted the post and arrived in Liverpool during September 1935. Why did Chadwick, who was in the most prestigious laboratory in Europe - at least in atomic or nuclear physics - come to a rather run-down provincial University? This is a difficult question to answer and a number of possible reasons may be given. His wife, Aileen, may have wanted to return to her family home in Liverpool, or Chadwick might have been frustrated at the lack of available funding at Cambridge. The promise of a substantial financial contribution towards the cost of a major piece of scientific apparatus - the cyclotron - together with staff and a free hand to run it, may possibly have helped in his decision. The quarrel that occurred between Rutherford and Chadwick concerning the building of a cyclotron at Cambridge might also have been significant. Chadwick was also, to some extent, in Rutherford's 'shadow' as Rutherford's 'Assistant', and may have felt that the time was right to take the opportunity to build and lead his own team.



Plan 1.2.1
Plan View of the George Holt Laboratory Basement
(Later used to house the cyclotron)
c. 1935

In 1935, the physics department at Liverpool University was housed in the George Holt Building on the North East side of the Victoria Building complex (the original 'red-brick' University), and it comprised four floors of offices and laboratories to include the basement (see basement plan opposite).

Holt has said [4, 5] that as soon as Chadwick arrived, he immediately set about updating the physics department. For example, he had alternating current installed to replace the batteries and direct current that were in use. Much of the student experimental apparatus was very old and Chadwick was 'dismayed' when looking in the apparatus cupboards used by the students for their practical laboratory work.

Wilberforce had concentrated on teaching physics, not researching it, and his undergraduate lectures and lecture demonstrations were models of their kind. The post-graduate and research work does not appear to have been actively encouraged in the years leading to Chadwick's arrival, although Wilberforce had communicated in 1934 and 1935 - the year of his retirement - two papers to the *Philosophical Magazine* by C.A. Beevers and H. Lipson.

The research for higher degrees that was carried out in the Department of Physics from 1930 to 1937 covering the successful completion of higher degrees started in the year of Wilberforce's retirement, were as follows:

Seven M.Scs. and one Ph.D.

in subjects ranging from 'Dissociation of Nitrous Oxide by H.F. discharges' to 'Magneto-rotary Dispersion of Cerous Sulphate' with no research at all in particle or atomic physics.

From 1938 to 1990, there were 341 successfully completed theses in the Department of Physics (including both Ph.D. and M.Scs.), over 95% of which were on atomic, nuclear or particle physics. The remaining higher degrees were awarded in acoustic and geo-physical subjects[6].

As Professor and Head of the Physics Department in the University of Liverpool, Chadwick automatically had a seat on the Committees of the Faculty of Science and the Faculty of Medicine, partly due to the conditions imposed on the holder of the Lyon Jones Chair. This was originally for the endowment of a physics Chair in the Medical Faculty, but had become one which entailed the teaching of a course of physics to first year medical undergraduates[7]. There was a close liaison between the Departments of Physics and Medicine at that time.

Chadwick's first Faculty of Science meeting was on Monday, 14th October 1935, at 2.15 p.m. in the Medical School Common Room, when the Chairman, Read, expressed a welcome on behalf of the Faculty. Towards the end of the meeting, it was agreed to recommend that the title of Professor Emeritus be conferred upon Wilberforce. It is noted that the minutes of the Faculty made no recorded comment on the award to Chadwick of the Nobel Prize for Physics, 1935. However, the Chairman of the Faculty of Medicine, Channon, conveyed congratulations on behalf of the Medical Faculty on 15th November 1935. Chadwick replied to the Chairman by letter, as the Dean of the Medical Faculty reported on 29th November 1935.

At a meeting of the University Senate held on the 27th November 1935, the Vice-Chancellor, in the name of the Senate, congratulated Chadwick on the honour conferred upon him, and the University, by the award of the Nobel Prize for Physics.

Chadwick attended eight Faculty of Science meetings during his first academic year at Liverpool and eight Faculty of Medicine Meetings.

During the academic year, 1935/6 - prior to Faculty agreement - Chadwick was actively engaged in obtaining staff with particle and atomic physics interests (but see later). In particular, it was agreed to recommend at the Faculty of Science Meeting of 30th June 1936:

> That Mr. S.F. Adams and Mr. B.B. Kinsey be appointed temporary Demonstrators in the Department of Physics for a period of one year as from 1st October, 1936[8].

The author has been in correspondence with Bernard Kinsey who confirmed that his appointment was taken up with effect from 'the summer of 1936'[9]. Kinsey's recruitment started in the early part of March 1936. The 'file' copy of the letter to him, in which Chadwick asked him to come to Liverpool[10], includes alternative proposals for terms of employment. In it, Chadwick also stated that the arrangements for Kinsey's employment had the agreement of the University Vice-Chancellor, Sir Hector Hetherington, thus pre-empting Faculty's permission!

One reason for stating the information given above concerning Kinsey's recruitment is to strengthen the hypothesis that Chadwick, when offered his position at Liverpool, had reached an 'understanding' with the University authorities that he would be able to build a cyclotron and have the staff to run it. He would also have to have Faculty permission to recruit staff, but obviously did not think this would be a problem.

Funding for Chadwick's cyclotron at the University had also been considered prior to his appointment and, as is shown below in his funding application to the Royal Society dated '*March,* 28, 1936'[11],

> Towards this apparatus I have been granted £2,000 by the University of Liverpool.

The above information confirms the hypothesis that Chadwick had knowledge of the financial support that the University were prepared to give, and this then enabled him to start the process of finding external financial support - once a knowledge of the total cost of a cyclotron was known.

The full funding application to the Royal Society from Chadwick is included in Appendix 3. This author considers the funding application document to be of importance because it shows the experimental programme that Chadwick was considering *at the time of the application.* The programme was extensively modified when the Second World War started, and major sections of Chadwick's proposed experimental programme, the medical and biological parts for example, were discarded and a programme of nuclear measurements that were required for the uranium atomic bomb project was undertaken. The medical aspects of the proposed cyclotron programme were discussed in a paper given by this author to the Liverpool Medical History Society[12].

Minute 5 of the printed Council Minutes of the Royal Society of London for 18th June 1936, shows that the Council,

> Read the following Report of the Government Grant for Scientific Investigations Committee:-
> The Committee submit their report as follows:-
> ...
> (iii) That the application from Professor J. Chadwick for an apparatus grant of £2,000 be referred to the Council.
>
> Resolved - That the report be received and that it be ordered accordingly.

The Report referred to above has been located in the 1936 Royal Society Government Grant Applications and is included as part of Appendix 3. This funding from Government sources via the Royal Society is:

> For Scientific Investigations undertaken with the Sanction of a Committee Appointed for the Purpose.

The Committee in this case was the Board A Committee, comprising the following Fellows of the Royal Society:

Professor C.G. Darwin	Professor E.A. Milne
Professor L.N.G. Filon	Professor H.C. Plummer
Professor G.H. Hardy	Professor J. Proudman
Professor D.R. Hartree	Professor G.N. Watson

From the layout of the Report - the Funding Application that Chadwick made - it appears that seven questions were asked, and answers given to the set questions. It is headed as shown below; the '5' in this case being the fifth submitted Application,

> 5. J. Chadwick, F.R.S...........................£2,000
> *March*, 28, 1936.

and it goes on to show,

1. I wish to build a magnetic resonance accelerator of the type devised by Professor E.O. Lawrence of Berkeley, California....

In reply to Question 2, Chadwick stated that:

> The cost of building this magnetic resonance accelerator or cyclotron is difficult to estimate closely. Professor Lawrence has given me a figure of 12,000 dollars, ... and it seems that I must be prepared for a total expenditure between £4,000 and £5,000.

Chadwick duly received a positive reply and so this left him to find approximately £1,000. [A present day conversion would leave him to find approximately £50,000 - a not insignificant amount!]

One of the main components of a cyclotron is a magnet which can supply a suitable magnetic field (see next chapter). The iron core of the magnet is wound with coils of wire to carry the high currents that are necessary to produce the magnetic field. Copper conductors can be used to advantage in the coils due to the low electrical resistance and ease of manufacture; that is, the ability to soften and then shape the copper to a required pattern.

The business acquaintances and friends that were part of Chadwick's wife's family circle now probably stood him in good stead as the author has discovered a letter[13] from Dr. Nisbett, the Managing Director of B.I.C.C. Ltd., Prescot, Lancs., to Chadwick. Nisbett informed him that his Board had been consulted and they had agreed to supply the eight tons of copper strip for the cyclotron coils without charge and requested Chadwick to inform Cambridge that they would supply similar strip to Cambridge at cost price.

The same file of documents that contains this letter also contains four 'Advice Notes' for, respectively, 2374, 4803, 7526 and 2697 pounds weight of 1" x 0.2" copper strip in 10, 20, 31 and 11 coils; that is, almost eight tons of copper strip, to be delivered to a Mr. P.P. Starling of Metropolitan-Vickers Co., Ltd., of Trafford Park, Manchester (Met.Vics.). This largesse saved Chadwick an estimated £600 [present day, £30,000]. He wrote to Kinsey[14] on 27th May, 1936:

> I have not yet made arrangements about power supply. I am hoping to get the power very cheaply by approaching the City Engineer. Before I can do this I must know exactly what (power) we want and how much we are going to use. I do not anticipate any great difficulty here. We have D.C. 230+ neutral 230-, and three phase A.C. 230 volts.

Chadwick presumably had a favourable reply from the Liverpool City Engineer as he then was able to place the necessary orders to commence construction of his cyclotron.

The funding was in place but it was necessary to have experimental physicists and engineers who had suitable experience to help in the design and the construction of his cyclotron, which was at the leading edge of technology of the time.

The Berkeley Laboratory of the University of California, under Lawrence, was the world centre of cyclotron expertise. It was the 'Mecca' to which aspiring 'cyclotroneers' were drawn. Kinsey, an ex-Cambridge graduate, is listed as a

> British post-doc for the years, 1933/4, 1934/5 and 1935/6[15]

and, as early as the 29th December 1935, in a letter from Chadwick to Lawrence[16], thanking Lawrence for his congratulations on the award of his Nobel Prize, Chadwick said,

> In connection with the accelerator there was a further matter I wanted to mention. Kinsey is obliged to return to England at the end of the year. I was thinking it might be possible to offer him a temporary post here to help with the accelerator ... should be very grateful to have your opinion of Kinsey.

Lawrence in the letter to Chadwick dated 31st January 1936[17] replied,

> I think Kinsey would be an awfully good man for you. He is a very hard worker and ... is very eager to have an opportunity to help with the construction and use of one in England. I mentioned casually to him you might be building one ... (cyclotron)

These letters indicate that Chadwick was 'head-hunting' staff for his cyclotron just two and a half months after taking the Liverpool Chair. There is no record of permission for this action contained in University Committee Minutes over that period, which again helps to substantiate the possible 'gentleman's agreement' given to Chadwick as part of an inducement to accept the Chair.

Kinsey was an experimental physicist with an expertise in the field of cyclotrons gained at the Berkeley Laboratories, and was held in high regard by Chadwick and by Lawrence. It is therefore rather surprising to this author that in Heilbron and Seidal's book *Lawrence and his Laboratory; a history of the Lawrence Berkeley Laboratory*[18], the following statement was made:

> (cyclotron) ... machines started by foolhardy types without Berkeley experience and finished with the help of one or more men from the Laboratory, as at Cambridge, ..., Liverpool ... .

They are therefore implying that Chadwick (and Cockcroft) amongst other U.K. scientists were:

> daring without sense of judgement, foolishly bold; rash, reckless[19].

The quotation from the above book *Lawrence and his Laboratory*, in the opinion of this author, gives a wholly wrong impression of the thought and expertise that Chadwick and Kinsey gave to the cyclotron project at Liverpool and is also proven untrue in Cockcroft's case as Cockcroft had had experience at Berkeley. See, for example, page 61 of *Cockcroft and the Atom*[20], where it is stated that Cockcroft took the opportunity to spend time with Cooksey at Berkeley 'getting the hang of the cyclotron'.

This author will show in the course of this thesis that neither Chadwick nor Cockcroft were of the 'foolhardy' type. They were in the foremost rank of atomic physicists in the U.K. and were not given to 'rash, reckless' work, although they could be classed as 'bold' in pushing back the frontiers of science - but certainly not 'foolishly bold'.

Cockcroft had started his career intending to become an engineer with the backing of his employers, Met.-Vics., but eventually went to the Cavendish and recommenced a career in physics[21, 22], although retaining close links with the firm.

Shortly after Chadwick took the Chair in Liverpool, Rutherford agreed to let Cockcroft commence the building of a cyclotron at the Cavendish Laboratory. It is probable that Rutherford bowed to the inevitable as his initial reluctance to allow the construction of a cyclotron was overcome. He possibly realised that to keep Cambridge in the forefront of atomic physics, some such machine had to be constructed. Money was available at the Cavendish through the sale of Peter Kapitza's apparatus to the Russians (£30,000) and also through the generous bequest (£250,000) of the motor magnate, Sir Herbert Austin, who, a few weeks later surprisingly became Lord Austin!

Chadwick and Cockcroft were initially in the Cavendish as supervisor and student respectively. Cockcroft was elected a Fellow of the Royal Society in 1936, and the two scientists became colleagues and friends.

They decided to join forces in the manufacture of the component parts of their cyclotrons. In doing so, besides possible advantageous pricing, Chadwick could draw on Cockcroft's engineering experience and also on his close ties with Met.-Vics, who were, incidentally, one of only two commercial enterprises in the U.K. capable of the manufacture of this experimental machine (the other company was I.C.I.. Ltd.).

There was, therefore, a close liaison between Chadwick and Cockcroft with the manufacture of the cyclotrons and this author has found in Departmental files a number of typed, and hand-written, signed letters from Cockcroft to Chadwick giving many examples of this close cooperation[23].

In May 1935, the Trustees of the Estate of Lord Leverhulme agreed to fund a new lectureship in the Physics Department of the University, the recipient to be known as a Leverhulme Fellow. Although this author has not located correspondence, it is highly likely that Chadwick was consulted over the new appointee. It was offered to, and accepted by, Norman Feather who had been a student at Cambridge University under Chadwick and was well known to him. Feather commenced at the University at the same time as Chadwick, in October 1935. However, Feather was enticed back to Cambridge after Rutherford offered him a Trinity Fellowship, returning in October 1936.

Rutherford died on the 19th October 1937 and Chadwick attended the burial of his ashes in Westminster Abbey. He wrote Rutherford's obituary in *Nature*[24].

Chadwick made a number of visits to Met.-Vics. in connection with the manufacture of the cyclotron at Old Trafford and, in late autumn of 1936, was introduced there to a young Irishman from Cork, M.J. (Mike) Moore, who had recently completed a craft apprenticeship with Met.-Vics. Moore's selection, and compatability with Kinsey resulted in his commencing employment at the University of Liverpool in January 1937 (see next Chapter and Biography, Staff; M.J. Moore).

There was one further source of skilled, although inexperienced, (wo)man-power available. This additional staffing was in the form of post-graduate students, including T.G. Pickavance, F.C. Thompson, and in 1938, Holt. Stanley Rowlands graduated in 1939. They had all benefited from the new courses introduced by Chadwick in atomic physics[25]. Holt won the 'Wilberforce Silver Medal' at the end of his third (of four) years as an undergraduate, and the 'Isaac Roberts Scholarship' plus the 'Oliver Lodge Prize' in Physics after he was awarded a First Class pass in his finals. He was pleased to be accepted as a post-graduate to do research on the cyclotron. Chadwick described him in the Chadwick/Weiner interviews[26] as,

> ... one of the best students I ever had.

Holt was appointed a student demonstrator for the session 1938-39 and was paid an honorarium of £15 for his services! This was the start of his long and distinguished academic career.

Stanley Rowlands was a student demonstrator appointed for the session 1939 to 1940. Rowlands was a recipient of the Oliver Lodge Prize in Physics in 1939 and started his Ph.D. research in October of the same year. He completed his Ph.D. on "A Study of β-Radioactivity using a Wilson Expansion Chamber" in 1942 and a doctorate was conferred on him, and Holt, in March 1942. Holt has said in a private communication that,

> By the summer of 1940 I had completed all the measurements for my Ph.D. thesis. Although still registered as a post-graduate student I wished to become involved with the work of the Department while writing my thesis.

A large part of the two young demonstrators' work load consisted of conducting experiments on the cyclotron.

It will be shown in the next chapter that the considerable help given freely by Lawrence and others in the U.S.A. enabled Chadwick and Cockcroft to base their design of machines on proven and tried methods. This did not stop either of them trying innovative ideas but Chadwick noted in his Weiner interview that some time had been wasted in trying them out. The main delay in the building of the cyclotrons, however, was due to the U.K. re-armament programme for which Met.-Vics were contracted, and the secondary nature of all civilian contracts.

While Kinsey was in the States, prior to his return to England in October 1936, Chadwick asked him to obtain as much information about the design of cyclotrons in general and various other design details of specific parts - more information will be in the following chapter. One of the items in which Chadwick had an interest was the cloud chamber, of similar design to C.T.R. Wilson's. Chadwick in a letter to Kinsey confirming his appointment as a 'Temporary Demonstrator' dated 28th July 1936, said that as a Temporary Demonstrator it would help,

> to eke out the grant from the D.S.I.R. and to give you something to do.

He also said in the same letter that,

> I believe Bonner is in your neighbourhood. You may remember that Bonner and Mott-Smith built a high pressure expansion chamber. Could you find out for me the design of the release valve for producing the expansion?[27]

E.J. Williams was appointed to the Liverpool Physics staff for a two year appointment as a Leverhulme Foundation Lecturer with effect from 1st October

1936[28]. His research project was to build a cloud chamber at Liverpool and examine the cosmic particle tracks made therein. Williams resigned from the University in 1938 to take a Chair at Aberystwyth University. Using the cloud chamber, Williams (in association with Eric Pickup) produced the first direct photographic evidence of the existence of a meson[29,30]. This photograph, together with a compilation of many other historic photographs, was published by G.D. Rochester and J.G. Wilson of the University of Manchester[31].

Chadwick, who was in the fortunate position in the summer of 1936 of having found the necessary funding to complete a cyclotron, could call on the services of an engineer/physicist (Cockcroft) and was ready to employ an experimental physicist (Kinsey). He was also about to engage the services of Williams to commence research work on the cloud chamber for cosmic ray investigation.

On the 28th February 1938, there is recorded in the Faculty of Science Minutes,

> That the following be the amounts allocated to the departments named as special grants for non-recurrent purposes in the session 1938-39:-
> Physics £100 { High pressure expansion chamber and apparatus.

A number of further decisions were required in order to complete Chadwick's plans for the Department. One was whether to make an appointment of a theoretical physicist following the unexpected death of Associate-Professor Rice (who, according to the Faculty of Science Minutes for 7th May 1935, had just been recommended to a Readership in the Department). Chadwick realised it was necessary to have one but did not get permission to fill this post until, according to the Minutes of the Faculty, 25th May 1936. The post, at Readership level, was offered to and accepted by Dr. M.H.L. Pryce:

> for a period of 6 years as from 1st January 1939, at a salary of £600[32].

One other experimental physicist was appointed by Chadwick for research on the cyclotron, and that was Harold J. Walke. The initiative to appoint Walke appears to have commenced with a letter that Lawrence wrote to Chadwick[33] about one year after Kinsey had started at Liverpool. In this letter dated 7th July 1937, Lawrence said,

> ... the thought comes to me that you might find it agreeable to provide some sort of scholarship

for Walke as his

> ... Commonwealth Fund Fellowship comes to an end this summer and he is returning to England in August and has not yet lined up a job.

Walke had been awarded a two-year Commonwealth Funded Fellowship in 1935 and worked under Lawrence for the two years at the Radiation Laboratory, Berkeley. Kinsey and Walke must have known each other as their appointments in Berkeley had overlapped. Chadwick, in a letter to Lawrence dated 7th August 1937[34], said,

> I received your letter about Walke just after I had written to him. Kinsey and I had been thinking about the matter for some time, but as I had no definite post to offer I did not want to prejudice Walke's chances in any way ... I think I shall be able to fix him up for one year, but prospects after that are rather vague at present.

Walke joined the Liverpool group in October 1937 after being funded with an 1851 Exhibition Senior Studentship and was at Liverpool until his untimely death on the 21st December 1939[35].

As 1938 ended, Chadwick was fully prepared to start his research programme on the cyclotron as soon as it was completed. But the war clouds were gathering in Europe and the German Nazi regime were embarking on their quest to conquer and annex their European neighbours. Nuclear research had been progressing in a number of European laboratories, and early in 1939 publication of investigations into the newly discovered fission processes were appearing in scientific journals and were freely available to all who wished to read them.

Within a few months of the publications appearing it was apparent that when fission of uranium took place a large amount of energy was released. The description of events leading to the discovery of fission is given in Appendix 4.

The exodus of European peoples under threat from the Nazi regime, mainly, but not always, Jews, resulted in the 'acquisition' by the U.K. of a number of scholars. There had been set up in the U.K. Academic Assistance Councils which helped to place emigrés in various government and University laboratories; Rudolph Peierls and Otto Frisch for example. In the spring of 1939, Jóseph Rotblat came to Liverpool from Warsaw University intending to stay for a year, to learn about the cyclotron. The Faculty Minutes for 1st May 1939 state that:

> Agreed to Recommend
> (ii)That Jóseph Rotblat, a Polish subject, be admitted for research in the Department of Physics and that his fees be remitted.

Poland was invaded by German Forces on the 1st September 1939, effectively trapping Rotblat in England - he was not able to return during the war and never saw his wife again. The funding from Warsaw University for Rotblat therefore ceased and Faculty Minutes of 9th October 1939, state:

> That the following awards be made:-
> Oliver Lodge Fellowships each £120 } T.G. Pickavance, Dr. J. Rotblat

Rotblat has said[36] that if Chadwick had not organised some such financial help he would have been destitute.

The log books[37] that the cyclotron teams kept from March 1938 to 1958 indicate that a 'beam' from the cyclotron is first mentioned on 12th July 1939.

The cyclotron came on stream almost four years after Chadwick's arrival at Liverpool.

References:

2.1. The Lyon Jones Chair of Physics is one of the original endowments of 1881, and founded (through Edward Whitley) by the trustees of the Roger Lyon Jones Trust Fund.

2.2. University of Liverpool, Faculty of Science Report Book Number 17, Archive S2478.

2.3. *ibid.* 2.2.

2.4. Booklet printed of a History of Science and Technology Seminar by J. Holt, *Sir James Chadwick at Liverpool. Personal reminiscences of the nobel laureate with archive film*, 8th February 1988, Department of Physics, University of Liverpool, in the Chadwick Archive, University of Liverpool.

2.5. Chadwick Centenary Lectures, Liverpool, October 1991.

2.6. Compiled from *Liverpool Who's Who in Physics (1881 - 1992)* by D.N. Edwards.

2.7. See Rivlin, J., "The Liverpool Medical School and its Alumni (1882 - 1902)", M.Sc. dissertation, February 1997, Department of Physics, University of Liverpool.

2.8. See Bibliography, Staff; Kinsey, B.B.

2.9. *Personal communication*, Kinsey to author, 22nd October 1994.

2.10. *Letter*, Chadwick to Kinsey, 11th March 1936, Document 61, File 1, Chadwick Archive, University of Liverpool.

2.11. See Appendix 3.

2.12. King, C.D., 'Sir James Chadwick and his Medical Plans for the Liverpool 37" Cyclotron', published in the *Medical Historian*, No. 9, 1997.

2.13. *Letter*, Nisbett to Chadwick, 25th September 1936, Document 7, Chadwick Archive, University of Liverpool.

2.14. *Letter*, Chadwick to Kinsey, 27th May 1936, Document 64, Chadwick Archive, University of Liverpool.

2.15. See Table 5.4a, Heilbron, J.L. and Seidal, R.W., *Lawrence and His Laboratory; a history of the Lawrence Berkeley Laboratory*, published by University of California Press, 1989.

2.16. The Lawrence/Chadwick correspondence, Bancroft Library, University of California. A copy of the correspondence is now held in the Chadwick Archive of the University of Liverpool.

2.17. Original signed, *Letter*, Lawrence to Chadwick, 31st January 1936, Document 27, Chadwick Archive, University of Liverpool.

2.18. Heilbron, J.L. and Seidal, R.W., *Lawrence and His Laboratory; a history of the Lawrence Berkeley Laboratory*, published by University of California Press, 1989.

2.19. Cassell, *Concise English Dictionary* (1995), p. 518.

2.20. Hartcup, G. and Allibone, T.E., *Cockcroft and the Atom*, Adam Hilger Ltd., 1984.

2.21. Hendry, J., *Cambridge Physics in the Thirties*, Adam Hilger Ltd., 1984 (see particularly Chapter 3.6, *Met-Vic Electrical Cpy. and the Cavendish Laboratory* by T.E.Allibone).

2.22. *ibid.* 2.20.

2.23. See the Chadwick Archive, University of Liverpool.

2.24. *Nature* **140,** November 1937, pps. 749-50.

2.25. *ibid.* 2.7.

2.26. Tape recorded interview, Chadwick and Charles Weiner, 15-21st April 1969, held in the Neils Bohr Library, Center for History of Physics, American Institute of Physics, page, Chadwick-98.

N.B. According to the information received from the A.I.P.:-

INTERVIEWER: Charles Weiner was born in 1931 in Brooklyn, N.Y. He received a B.S. in Metallurgy from Case Institute of Technology in 1960 and a Ph.D. in History of Science and Technology from Case in 1965. From 1958 to 1962 he worked on editing science-related publications; in 1964 he began work at AIP as Director of the Project on History of Recent Physics in the U.S.; and from 1965 to 1974 he was the Director of the AIP Center for History of Physics. His research interests include the history of American physics and the history of nuclear physics.

2.27. *Letter*, Chadwick to Kinsey, 28th July 1936, Document 66, Chadwick Archive, University of Liverpool.

2.28. University of Liverpool, Faculty of Science Minute Book No.4, Archive Location S3027, entry 15th June 1936.

2.29. E.J. Williams, *Proc. Roy. Soc.*, A.**172**, p. 194 (1939).

2.30. Williams, Pickup, *Nature*, London, **141**, p. 684 (1938)

2.31. G.D. Rochester, J.G. Wilson, *Cloud Chamber Photographs of the Cosmic Radiation* with foreword by P.M.S. Blackett, London, Pergamon Press (1952).

2.32. University of Liverpool, Faculty of Science Minute Book No. 4, Archive Location S3027, entry 25th May 1936.

2.33. *Letter*, Lawrence to Chadwick, dated 7th July 1937, Bancroft Library, University of California.

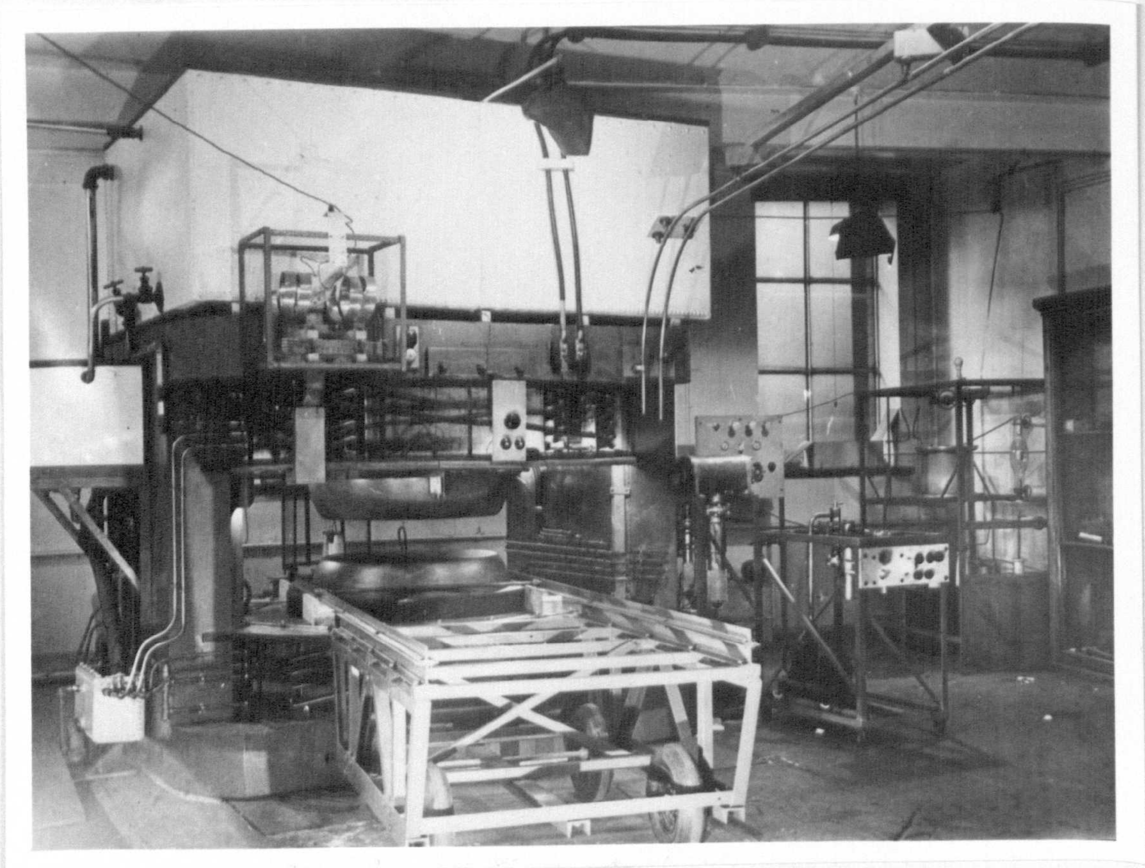
2.34. *Letter*, Chadwick to Lawrence, dated 7th August 1937, Bancroft Library, University of California.

2.35. See Biography, Staff; H.J. Walke.

2.36. See Rotblat/Edwards/King taped interview, London, 28th October 1992, transcribed by the author, Rotblat Archive, University of Liverpool.

2.37. Cyclotron Log Books held in the Department of Physics, University of Liverpool:

Book No.	Date	Remarks
1a	21st March 1938 - 12th July 1941	
1b	20th February 1939 - 13th June 1939	R.F. measurements
2	15th July 1941 - September 1944	
3	1st November 1945 - 21st February 1946	Power Amplifier
4	12th March 1945 - 2nd April 1948	
5	3rd April, 1948 - 28th October 1950	
6	30th October 1950 - 7th July, 1954	
7	8th July 1954 - 30th December 1958	



Photograph 3.3.1
The Liverpool 37" Cyclotron
c. 1941

CHAPTER 3
Building the Liverpool 37" Cyclotron

In this chapter the author will discuss the extent of the assistance given to Chadwick after his arrival at Liverpool University regarding the design, building, costing and safety aspects of the cyclotron that he intended to build (see photograph opposite). His research project was at the leading edge of technology for its time and a number of the techniques then used were novel although they have now been superseded or are obsolete. Production of vacuum, its practices and techniques, is a prime example and Chadwick's team had many vacuum problems, particularly in the early stages of testing the cyclotron. Electronics was also in its infancy and the high power electronic valves were manufactured in house by glassblowers and were continuously evacuated. New light can be shed on these issues by the discovery of a series of 13 previously unknown letters between Chadwick and Kinsey[1]. These letters were found in departmental files covering this time period. In them is outlined how some of the difficulties and problems that were encountered in the design of a machine at the forefront of existing technology could be overcome. The research for this chapter has been in the detailed analysis of these documents and also in other letters and documents, found in departmental files, between Chadwick and leading cyclotron researchers in the U.S.A., including Lawrence, Newson, Henderson and Cooksey.

Medical and biological effects of high energy particles and waves had only recently been investigated and cyclotron operators' safety was one of Chadwick's considerations. Medical uses of the products of irradiation from the cyclotron and its directed beams were of great importance. Up to the late 1930s, the application of X-rays with the possibility of surface treatment using radium were, apart from surgery, the only means of regressing tumours and lymphocytes. The first recorded information on the treatment of a leukemia patient using radio-phosphorus made by a cyclotron beam is mentioned in a letter Chadwick received from Lawrence of the Berkeley Laboratory written on the 30th April 1938[2].

Chadwick started a collaboration which involved Lawrence and others in the United States of America, together with U.K. manufacturers and J.D. Cockcroft in Cambridge University. The methods of communication were normally by letter and only exceptionally by telephone or by cablegram, so that orders for goods to be manufactured, answers to queries or questions that were required from the States, or from manufacturers in the U.K., often took days to obtain.

Chadwick's collaboration with Cockcroft almost certainly involved discussions on the future uses to which the two machines - the one proposed for Cambridge and the one in Liverpool - would be put. The only evidence that the author has located on the envisaged research programme at Cambridge was in a letter that Cockcroft sent to Chadwick on the 30th March 1936[3]. In this letter Cockcroft discussed the plane of the air gap between the magnetic pole faces and said,

> Most convenient vertical for Cambridge. Most convenient horizontal if only required for ion spinning. No use for cosmic rays and magnetic work then.

As Chadwick in his Royal Society Funding Application (see appendix 3) did not mention 'cosmic rays and magnetic work', it seems probable that the two machines would have complementary research programmes with Chadwick's doing the 'ion spinning'. It would obviously have been a waste of resources if the two programmes had been identical but some overlapping would be unavoidable.

On the 27th November 1935, just seven weeks after Chadwick took up his appointment at Liverpool University, he received a letter from Lawrence[4] offering congratulations for the award of the Nobel Prize in Physics, 1935. It was stated in the letter that,

> Dr. Fleming of the Metropolitan Vickers, and his son were here not long ago and discussed the possibility of building a magnetic resonance accelerator for you. I told him I should be very glad to help with the project in any way possible and doubtless we could be helpful in the way of sending detailed recommendations, drawings and specifications.

This magnanimous gesture and 'warmest felicitations' in the same letter must have given Chadwick enormous encouragement in the project which he intended to start within the Physics Department. Lawrence, who, with Edlefson in 1930 had demonstrated the idea of circular particle acceleration in combined magnetic and electric fields, was the world leader in devices of this kind. He had built up a strong team, including a number of visiting research fellows, within the University of California and had at least six years' expertise and an extensive lead within this field. Many physicists from the U.S.A., Europe and the Far-East had had contact with or spent time at Lawrence's Laboratory and amongst many others were those who were going to take a leading part in the building of the Liverpool machine. Not least, as has already been mentioned, was B.B. Kinsey[5]. Another experimental physicist recruited for Liverpool by Chadwick from California after a period with Lawrence's team, was H.J. Walke[6], as already mentioned.

Whilst Kinsey was working at Berkeley , he was able to get 'hands on' experience of the operation of Lawrence's cyclotron. This was one of the obvious reasons for Chadwick recruiting him. After Chadwick's offer and the subsequent acceptance of a post at Liverpool by Kinsey, there was an exchange of letters between them, and the information contained in them enabled Chadwick to consider incorporating the latest working practices and type of equipment into his proposed cyclotron.

A convenient place to start consideration of the Chadwick/Kinsey correspondence is the one of the 24th March 1936[7], when Kinsey replied to a letter from Chadwick, and, after thanking him 'for your most excellent offer' of employment at Liverpool, said:

> I am very pleased to see that the possibilities of the cyclotron are being appreciated in England. Even here, we are amazed every week at the huge field which is being opened up for nuclear work, with its aid. We think, that so far, the potentialities of this machine have scarcely been touched on. With many microamperes of deuterons available at six million volts, with the regularity of clockwork for the eighteen hours or so of the working day, we do not see any limit to the things that we can do with it.

Kinsey wrote a three page letter dated 7th April 1936[8], containing details of his existing work on lithium and beryllium targets and, in order to gain as much experience on the Berkeley machine as possible, asked for the latest time he could return to the U.K. He also queried the cost of a magnet to be built in the U.K. as he goes on to say:

> ... Henry Newson's 50 inch pole-face magnet is not going to cost more than $7000 or so, all told. ... But then, as far as I know, Henry and the Ann Arbor crowd have obtained specially low prices on their steel.

Another comment comes from the same letter when Kinsey states :

> Then, how about the location of the magnet? Used as a source of neutrons, the intensity is so fierce, that the present distance of 40 feet or so between target and control table is really insufficient, and most members of the laboratory have low blood counts, ... someone here has worked out that the maximum safe daily dose of neutrons is recieved (sic) at the control table in about an hour.

On the 23rd April 1936, Chadwick wrote back to Kinsey[9]:

> ...that you ought to get as much information as you can while in the States. ... I am not sure when work on the magnet will start, probably soon. ... The general design of the magnet will probably follow that of Svedberg in Upsala. ...I am setting a practical limit of 10 million volts for the irons (sic). The pull (sic) pieces will then be about 150cms. diameter, tapered to 90cms. at the gap. The gap is about 9cms. wide. The field will be uniform over a diameter of about 72cms. and we should get fairly comfortably 17000 - 20000 gauss. The weight of the iron will be about 50 tons and the weight of copper about 8 tons. The iron may cost about £30 a ton and the copper about £100 a ton, so you see where the mondy (sic) goes. Fleming says that iron is about the same price here as in the States. I do not think we can machine the magnet parts in the University, and in any case the cost of machining is not a large part of the total cost.
> Cockcroft is building a similar magnet for magnetic work in the Mond laboratory. For this work and for cosmic ray work the plane of the gap must be vertical. I do not see any harm in having the gap vertical for the ion spinning. Do you?
> ... I think I have ample room for the whole apparatus in the basement, and that we shall be able to get away from the neutrons. I do not think it should be very difficult to arrange some form of protection. The floor is on solid sandstone, so it should carry the weight comfortably, the only difficulty may be to get the magnet into the laboratory.
> As we may be starting on the magnet shortly, I shall be glad to have any views you may have. I should also be glad if you could explain why the Princeton and Chicago magnets cost relatively so little, and what field they are going to get with 50 inch pull (sic) pieces.

(Chadwick's secretary, Gwendaline Lloyd-Jones, also had a lot to learn about physics terminology!)

In response to the above letter, Kinsey must have contacted Professor H.W. Newson of the 'George Herbert Jones Laboratory, Department of Chemistry, University of Chicago,' as Newson wrote a letter to Chadwick dated 13th May 1936[10], that:

> At the request of Mr. Kinsey, I am enclosing a copy of my letter to him relative to the cost of the cyclotron magnet which I am building here in association with Professor Harkins. ... I shall be glad to furnish any other information which will be helpful to you.
>
> Very truly yours,
> Henry W. Newson (signed)

The enclosed letter mentioned above is addressed to 'Dear Bernard,' and dated 13th May 1936. Newson starts by saying that:

> I heard the good news about your job from Ernest and Don.

This author presumes 'Ernest' is E.O. Lawrence and 'Don' is Donald Cooksey, and Newson then proceeds to give a detailed breakdown of the cost of the yoke, pole pieces, copper coils and (vacuum) tank giving a

> ... total of about $10,000. Although the final details are not settled, I think these prices are pretty accurate.

Newson ends the letter:

> Meta and I are looking forward to seeing you in September.

The total cost in the States would be about £2,500. If one uses the figures that Chadwick quoted in his letter of 27th April 1936, to Kinsey, of steel £30 per ton and copper £100 per ton and the respective weights being 50 and 8 tons, then the comparable U.K. cost would be £2,300 plus the cost of the vacuum tank and machining of same, say £3,000.

As can be seen from the above, Chadwick had received a good appreciation of building the cyclotron both from the physics and the engineering point of view, but he was not a trained engineer. Cockcroft, with his engineering background and experience in large machine works (Met.-Vics.), was available to discuss and advise

Chadwick on many of the engineering aspects of the cyclotron. Cockcroft not only was an engineering physicist but had had practical experience in working with cyclotrons. After Cockcroft and Walton had 'split the atom' in 1932 with their 700 kV generator at the Cavendish, Cockcroft had been invited to attend the American Association for the Advancement of Science at Chicago held in June 1933. (Cockcroft had earlier written to Lawrence asking to visit him and to see his 11" cyclotron in which almost 1 Mv ions were produced). It was then that Cockcroft

> learned from Lawrence's colleagues how to make adjustments
> (to the cyclotron)[11]

on Lawrence's machine and, later, another occasion arose (1937) when Cockcroft was invited to give a course of lectures at Harvard, and he took the opportunity to spend time with Donald (Don) Cooksey - Lawrence's deputy - at Berkeley, where he spent time

> getting the hang of the cyclotron[12].

In his three-page letter of 7th May 1936[13] to Chadwick, Kinsey detailed the dimensions of a suitable air gap for the magnet and also stressed the fact that the magnet should not have vertical pole pieces:

> <u>Please do not build a vertical magnet on any account</u>

Kinsey must have also written to Malcolm C. Henderson - who was then at the Palmer Physical Laboratory, of Princeton University, New Jersey - as well as Newson. Henderson wrote back to Chadwick in a letter[14] dated 1st June 1936:

> Bernard Kinsey has asked me to write you the details of the magnet that we have just built here in Princeton, and in particular to give you the prices of the various materials used in it.

Henderson must have held Chadwick in some esteem[15] as he goes on to say:

> It is with some diffidence that I do so, since in the past it was you who used to give me advice and information. Still, I don't pretend not to enjoy the chance to repay in some small part your labors over me.

In his three-page letter, Henderson was able to give Chadwick not only the cost of the various magnet parts made for the Princeton cyclotron, but also a comparison of the different costs of various grades of steel and the various methods of manufacture that could be employed in the magnet. For example, the 'blue print'[16] that is mentioned in the letter is labelled:

> Cyclotron Electro-Magnet Princeton University Princeton, N.J.

and dated 9th December 1935. This blue print is a working drawing showing the main dimensions and method of construction of the Princeton magnet as described by Henderson. Incidentally, the manufacturer and supplier's name are given in the bottom right hand corner as:

> Carnegie-Illinois Steel Corp. Sales Engineering Dept. Pittsburgh, Pa.

Henderson says,

> You will notice that the thickest slab is 8" thick and that the heaviest piece is just over 5 tons. Except for the pole pieces, which are forgings, the magnet is made of "rolled plate" the cheapest sort to manufacture.

The steel was a

> " low-metalloid" steel, at 2.34 cents a pound rough rolled. ... The pole pieces were 6.1 cents a pound finished, and weigh 9,000 lbs. apiece. The total cost of the steel worked out at about $3,600 delivered in Princeton. ... Their ordinary steel costs something like 1.8 cents a pound, base price, compared to 2.34. ... Armco iron, the standard material for magnets in the U.S. is much more expensive and not worth the price.

He also gives the details of the copper windings that were used on his magnet, their output being:

> 300,000 ampere turns at 40 KW.

and says,

> Our windings are made of 1¼ by 1/16 inch strip copper - "slit copper", not drawn, which costs about 1 to 2 cents more per pound - wound in flat pies, 180 turns to a pie, three half inch gaps in the course of the winding. ... The total amount of copper is 8.4 tons, 3,600 turns, in 20 pies, ten to each pole. ... Inner radius 22", outer radius about 38". The Revere Copper Co. supplied us at 12½ cents a pound. That was at a time when raw copper was 8½ cents a pound "delivered in the Connecticut Valley". The cost was thus $2,100 for the copper. Winding the coils was done for us by a small electrical motor winding concern in Hoboken, N.J. They asked $975 delivered.

And in a hand written note under the paragraph:

> Bogue Electric Corp. Lipton Bldg. Hoboken N.J.

It can be seen then that the Princeton magnet and coils cost $6,675 plus, of course, the cost of the vacuum tank, power generator and oscillator, which compares with Newson's:

> ... total of about $10,000.

The message coming back strongly to Chadwick was a cost, in the States, of the main mechanical components of a comparable cyclotron being of the order of £2,500.

Henderson built, as did Cockcroft, a scale model magnet in order to check out the effects of the magnet pole shape on the air gap field uniformity, the saturation curves and the iron and copper losses associated with the design. The stray fields were also checked to enable the magnet efficiency to be maximised. Henderson, discussing the model's results, in this letter to Chadwick, states,

> ... We aimed throughout at making the magnet saturate everywhere approximately simultaneously. I enclose a set of curves taken on the model and big machine. The latter curve was taken only yesterday and one reason for the delay in writing to you was that I wanted to send you the final result as well as those taken on the model. ... Compared to the Berkeley magnet there is a lot of stray field, probably because at Berkeley the yoke is way below saturation even at 18,000 (gauss) in the gap.

The final page of this document is a sheet of graph paper (24 x 18 cms) showing 'Kilogauss' versus 'Thousands of Ampere-Turns per cm of air gap' and, on the same x-axis, power marked at '10 KW' and '40 KW'. The four curves are drawn freehand in coloured pencil and labelled:

> Big Magnet 35" pole 4¼"gap
> 3.5" Complex Pole
> 3.5" Cylinder Pole
> 4.0" Cylinder Pole

The last three labels were bracketed together and labelled:

> Model .385" gap

The four B-H curves shown give the scaled up results obtained from the model with three different pole configurations, and then compared with the actual result from the full sized magnet. Plotting B-H curves for a magnetic circuit is a recognised method of finding the saturation field of the magnet, and in this case was slightly over 18,000 gauss.

Kinsey, in the meantime, had been in touch with Chadwick, again sending him a letter dated 15th May 1936[17], enclosing

> ...blue prints of oscillators and oil pumps, and a memorandum concerning the former

but, unfortunately, the author has not located them. Kinsey goes on to tell Chadwick that he would like to draw up:

> ... semifinal plans for the tank and the D's over here, before I go home, and get the criticism of the people over here. But it will not be possible to do this unless I know what gap between the pole faces, and what diameter pole faces have been decided on.

He stressed the value of having maximum distance between the pole faces:

> Every fraction of an inch is worth its weight in gold

and goes on to tell Chadwick that localised hotspots occur which could be overcome by putting ¼" water cooled copper plate sandwiched between the D's and the iron covers of the vacuum tank to:

> absorb the energy of the electron (sic) beam

Another idea that Kinsey asked about in this letter was:

> Also, have you thought of the possibilities of windings with few turns, high current? Say, 30 turns, excited by 20,000 amperes, and driven by a homopolar machine. ... minimum copper, ... and power consumption ... greater rapidity in the decrease of the field with distance from the pole faces, ... This would be of great value in getting the beam out. With this magnet here, the field is still thousands of gauss at a foot away from the pole faces.

Kinsey ends his letter by asking if he could delay his return to England as,

> There are a number of things I want to do before I get back, ... and time is getting very short. ... I wish you could spare me a little extra time.

The author believes that Kinsey had written to at least a third 'Cyclotroneer' in the States, although no document has yet been located, as Chadwick in a further letter to Kinsey dated 27th May 1936[18] told him that:

> I received yesterday a letter from Thornton and a blue print showing the design of the Ann Arbor magnet.

In response to Kinsey's request of details of magnet gap and pole diameter (see the previous letter above), Chadwick states:

> The gap of 5 inches which I mentioned was after allowing for the iron plates of the acceleration tank. We were thinking of a total gap of about 7 inches.

The magnetic field leakage which Henderson had mentioned in his letter to Chadwick had been given some consideration by Chadwick, as he goes on to say that:

> In both the Ann Arbor and Princeton magnets there seems to be a weakness in design. There is not enough iron where the pole pieces join the yokes to take the leakage from the coils. They must be aware of this themselves. If it is possible to improve this point without too much cost I shall do so.

One of the reasons that minimum leakage was required was to enable accelerated particles from the cyclotron to enter a field free region for further study, and, secondly, all losses reduce the magnet efficiency and consequently increase the power requirements and, therefore, increase the overall running cost.

Chadwick answered a number of queries that Kinsey had put to him concerning:

> 1. Pyrex. ... Our pyrex is not the same as American pyrex. I believe our pyrex tube has the same composition as the glass made for domestic use.
> ... According to measurements made in the Electrical Engineering Laboratory here the power factor is about double ... Pyrex has been tested up to about 10^7 cycles per second when the power factor is 0.5%.
> 2. Sylphon. I have no sylphon, but it is made by the Crosby Valve and Engineering Co.Ltd. I enclose their pamphlet. .... .
> 3.There are two firms which make high frequency insulators; Taylor and Tunnicliffe, Stoke-on-Trent is one, the other, about which I will let you know later, makes the insulators for Marconi.
> 4.I have not yet made arrangements about power supply. I am hoping to get the power very cheaply by approaching the City Engineer. ... I do not anticipate any great difficulty here. We have D.C.230+ neutral 230-, and three phase A.C. 230 volts.

N.B. In correspondence with Kinsey, the author received the following definition of a 'sylphon':

> It is (or was) just a very short length of flexible metal tubing, made of copper, which could be easily soft soldered to a pipe.

In his letter to Chadwick of 6th July 1936[19] is given provisional travel arrangements saying that he hoped:

> ... to leave here about the middle of August, and I want to have - if possible - a couple of months before I get back to England, to look around on business.

Kinsey also details a few design ideas concerning the electrical supply from the oscillator, as:

> I have been looking into the oscillator problem during the last couple of months, and we have come to a number of conclusions. We think (i.e. at the moment) that it is probably best to build a master oscillator and power amplifier outfit, and drive the tank through a half wave double concentric tube transmission line. ... Of course, a master oscillator and amplifier outfit is more expensive from the point of

view of transformers and rectifiers but since it is easy to get a very high efficiency in class C amplifiers, there is not much likelihood of using more power, all told.

Because of the plural used in the above letter, 'We think ...' etc., Kinsey had discussions with his colleagues at Berkeley before writing back to Chadwick. He was obtaining as much useful information as he could from them. Other design problems were also being considered, as Kinsey informed Chadwick that:

I am still concerned about this question of pyrex insulators to support the D's. ... The principle concern is this question of 'cathode ray' punctures of the glass. Every time a puncture takes place the entire tank has to be taken down, and cleaned, ... The people here are just afraid to raise the high frequency voltage beyond a certain amount - which, incidentally, no one has ever measured, but is generally believed to be in the region of 40 to 50 kv. between the D's. This is a pity, seeing that the intensity of the beam increases as a rapid function of this voltage. ... ('the cathode ray' puncture) ... is probably a combination of the effect of the high pressure in the tank - of the order of 10^{-4} mm or more - and hysteresis heating of the glass.

Kinsey goes on to suggest that Kruger, at Illinois, who had built a small cyclotron appeared to overcome this problem by using guard rings, or:

... metal rings, which slip inside the glass insulator, are insulated from one another, and interlock so as to hide the glass from the high frequency electrodes.

In Chadwick's reply of the 16th July 1936[20] and after giving Kinsey the details of the financial arrangements of his employment[21], tells him that:

... you need not be here until the 6th.(October). I do not think I ought to stretch beyond this unless it is absolutely vital.
I have some good news in prospect which will enable me to go right ahead with the cyclotron and will, if everything comes off, give us ample supplies for a few years.

The 'good news in prospect' mentioned in the paragraph above, has not, from the correspondence the author has studied, been explained. In the same letter Chadwick states:

We have begun to think about the problem of supporting the D-plates. ... Also, can the D-plates be placed asymmetrically in the tank, nearer the top than the bottom?

and then asks a number of detailed design queries, such as, What is the maximum high frequency voltage between the D-plates and ground? What is the power input for the tank and how near the edge of the poles does the tank come ? ... will you duplicate the parts you are getting for the tanks so that Cockcroft will have everything he needs for his?

He goes on to say that he has:

... a little more information about insulators for high frequencies. The insulator Marconi's use for 6 metre work are mycalex, porcelain, mica and frequentite. Vitreosil, provided it is smoothly finished, is also very good.
... It is supplied by Steatite and Porcelain Products Limited, Stourport-on-Severn, Worcester, and their London Office address is 56,Victoria Street, London, S.W.1.
... Can you let me have an address which will find you after the middle of August?

Kinsey replied on the 5th August 1936[22] in which he said that:

... if you wish to communicate with me, ... write to me at the Commonwealth Fund offices, who will send it on to where I happen to be at the moment. Their address is: The Commonwealth Fund
41 East 57th. Street,
New York City, N.Y.

In the same letter he gave detailed answers to the questions raised and again gave valuable design guidance on such matters as the symmetry of the D's 'about a horizontal plane' and the fact that:

Insulating feet such as you mention were tried some time ago. Some little quartz supports were tried, but became white hot with only a very meagre voltage between D's to ground.

He then lists and describes six 'troubles and defects' on the cyclotron at Berkeley, after stating that:

In the last few years, and particularly this year, here, 80% of the time has been spent on repairs.

This statement does appear to be at odds with Kinsey's previous optimistic remarks of only a few months earlier that:

With many microamperes of deuterons available at six million volts, with the regularity of clockwork for the eighteen hours or so of the working day, ...

Maybe Kinsey had been working those 'eighteen hours or so of the working day' and was feeling the effects of pushing himself so hard. He finishes this three-page letter on a much more optimistic note giving Chadwick further design details of the oscillator system and the methods of its manufacture. The letter concludes by saying,

> Amazing to relate, the cyclotron actually started to work a few days ago, and is now delivering 10 microamperes at 5.5 million volts, out in the air. A drove of biologists have descended on the laboratory, headed by Lawrence's brother, and are hard at work investigating the effects of the neutrons from beryllium on tumours in mice.

The mention here of the biological effects of neutrons reminds us that already in 1936 there was sufficient knowledge of the potential dangers of neutron irradiation to have important implications for the design and physical layout of the machine and its controls.

The red and white blood cell quantities in healthy humans were measured and checked when workers became involved with radiation beams. The red cell count[23] (RBC) ranges from 4.5-6.5 million/mm^3 in men and 3.9-5.6 million/mm^3 in women, and the white cell count (WBC) in the adult is in the range 4,000-10,000/mm^3. The author has detailed the above information on blood cell levels as Kinsey, in reply to a request from Chadwick (of 28th July 1936) for further information on low blood counts of the workers at Berkeley, replied on the 11th August 1936[24] that:

> ... The information which you ask, has been measured recently with the 10 microampere beam at 5.8 million volts, now available here. Measuring r-units with an aluminum ionisation chamber as for X-rays, ionisation given by neutrons are as follows (in air):
>
> | | | | |
> |---|---|---|---|
> | At 5 cms. from target | .... | 6 | r/min./microamp. |
> | 1 metre " " | .... | 0.015 | " " |
> | ~ 20 metres (the distance of our control table from the target) | ... | 0.00004 | " " |
>
> I understand that the maximum safe daily dose for X-rays is about 0.1r per day. John Lawrence (E.O. Lawrence's medical doctor brother) tells me that experiments on the lymphocytes of mice show that, for equal ionisation in air, neutrons are just about 5 times as effective as hard million volt X-rays. Hence it is assumed here, that the maximum safe dose for neutrons is 0.02r per diem (sic). If this is so, we all get this maximum dose during an evening's work at the control table, and it may also be obtained by simply walking past the magnet, a few feet from the target. So you see things are pretty bad. During the months of last Spring ... several people had very low blood counts. About half the laboratory had red

> blood counts lower than 4 million per mm^3. ... Nobody has suffered anything so far. I suggest that John Lawrence could tell you more; he is here now, but normally works at the Yale Medical School, or possibly Zirkle, who is also here now.

Returning to the actual design of the machine, Kinsey makes the following observation in this same letter:

> By the way, the objection to the vertical cyclotron does not really exist. Anyway, people here do not object any longer. I think that our objection was rather the natural reaction to heresy, than scientific in basis.

This letter also gives details of suitable materials, and methods of manufacture that could be used in making the tank. Some non-magnetic steels could be welded, but:

> Some arc welded joins from brass to brass on the present tank cracked right out, I am told, because of strains, etc. Much depends on whether one is lucky enough to find a really good technician. But by the use of a non-magnetic steel, welded joins would be very easy and guarantee-able, and would give us far greater strength for the same thickness.

After telling Chadwick that he had:

> ... completed measurements on the stiffness and inductance of coils to support the D's,

he says that,

> ... Lawrence agrees with me that this is obviously the best way to build a cyclotron.
> (by utilising D support coils as mentioned above)

It is clear that Kinsey was getting as much information as he could and giving Chadwick many suitable hints on the best way forward, and this letter may have crystallised Chadwick's ideas on obtaining 'a really good technician' as mentioned above. It is possible that Moore[25] owes his employment in the University to this statement as Chadwick became interested in recruiting him from Met.-Vics. of Manchester during the next two months of 1936.

During the exchange of letters with Kinsey, Chadwick was also corresponding with Lawrence at the Berkeley Laboratory. It has been previously mentioned that this series of documents are held in the Bancroft Library Archive at Berkeley and they comprise some 28 letters and four cablegrams with an extract from *Nature*, London, over the time period November 1935 to January 1947. The first 17 documents relate, in the main, to the building and design of the Liverpool 37" cyclotron, and the events surrounding it.

The generous help that Lawrence, and of course Lawrence's laboratory colleagues, extended to Chadwick in the design and later stages was of immense importance to Chadwick. There were minor pieces of information:

> ... a cyclotron (slang for the magnetic resonance accelerator)

for example[26,27], to four pages of detailed information sufficient for Chadwick so that[28]:

> Towards the end of May I ... (can) ... give a Friday evening discourse at the Royal Institution on the cyclotron and its applications.

In fact, Chadwick felt uneasy about asking so much of Lawrence, as he goes on to say in this letter:

> I feel very guilty in asking so many questions but the answers are of interest not only to me but to many working on these lines - and also to some who are not interested particularly in physics but more in the other applications of the cyclotron.

In Lawrence's return letter of 30th April 1938[29] was information of his present cyclotron, from which Chadwick would have gained a good overview of the machine. There is, for example, the fact that Lawrence had made provision,

> for two magnetic gaps (of) 6 and 7½ inches, ¾ inch plates being inserted in the vacuum chamber for the narrower gap.

and, with suitable adjustment,

> ... we obtained for short intervals of time 175 microampres of 6.2 million volt deuterons and for steady operations many days we had 150 microamperes.

He goes on to say that,

> The limit of the current reaching the target under these conditions seemed to be determined by the warping of the dee lip produced by the large ion current striking it. ... perhaps I should mention ... that when the cyclotron is delivering 50 microamperes of 8 million volt deuterons, the power input to the oscillator is about 40 kilowatts and I judge that the radio-frequency power into the cyclotron circuit is about half the input to the oscillator

thus implying that they had a well matched load for the oscillator. Both Kinsey and Walke who were, by this time, fully occupied with the construction of the Liverpool cyclotron, had been kept up to date on the latest information at Berkeley as Lawrence mentions in this letter:

> I presume that Dr. Cooksey and others in the laboratory in their letters to Kinsey and Walke have described the improvement of the cyclotron oscillatory circuit which has been made here recently ... .

He then explained how, by various techniques,

> The Q of the tank circuit has been greatly increased. With no ion load the voltage can be produced on the dees with less than half the power formerly required, ...

and says,

> This suggests that under normal operating conditions 5 or 10 kilowatts of high speed ions are produced when the high speed ion power delivered to the target is between half and one kilowatt, indicating that we are getting to the target only a small fraction of the high speed ions within the dees.

In other words, the extraction of the particles from the dees was causing him some concern, as this efficiency was only of the order of 10%. The

> ... radio-activity produced in targes (sic) such as copper and iron and tungsten. ... it is quite all right to insert a target between the dees.

where approximately, 'under present conditions', we have 'a milliampere of high energy deuterons.'

This new way, of inserting targets into the dees to obtain a 10-fold increase in irradiation, was one solution to a difficult problem for Chadwick although it meant that there were then limitations on target size and accessibility. No problem is overcome without introducing others! Lawrence also passed on information concerning the physical dimensions of the new magnet that he was then building which was to have a 72" pole diameter and 58" pole face:

> The new magnet consists of about 190 tons of steel and 22 tons of copper, more material than needed for the intended voltage and current output because it has been designed for the primary purposes of medical research, requiring openness and accessibility.

He then details the output of the new cyclotron - 15 million volt deuterons and 30 million volt alpha particles - as,

> We are interested, for the medical purposes, in the production of neutrons and the manufacture of large quantities of radioactive materials, in large currents as well as high voltages, but of course the program of biological work has in now (sic) way dimmed our desire to get on up to as high voltages as possible.'

On the 11th May 1936[30], Chadwick wrote to Lawrence and gave him an update on the progess in Liverpool of the proposed cyclotron. Chadwick says,

> We have not proceeded very far with the design of the magnet for the cyclotron. There are, at the moment, three magnets to be considered, one for the Cavendish, one for Fleming and one for me. The Cavendish will have a very large magnet indeed. They have plenty of money from the sale of Kapitza's apparatus. ... Fleming intends to build a small cyclotron for Metro-Vick.

He goes on to inform Lawrence that,

> ... for my part I must build for ten million volts. Fleming, Cockcroft and I are discussing the design of the magnets together, chiefly from the point of view of doing it as cheaply as possible. The first design, which was a variation of the one built for Svedberg in Upsala, proved to be too expensive. We are now considering one somewhat on the Princeton pattern. If we can use air cooling the cost of this should be about £2,500, if we can get the steel at £20 per ton. But this appears to be a low figure, and may prove too optimistic.

Also in this letter, his opinion was asked whether or not it

> ... would be some advantage for the cyclotron ... (if) the plane of the gap of the magnet ... be vertical. Another suggestion has been made, that the high frequency coil be put inside the D-box to avoid dielectric losses. Is there any serious objection to this? (adjustment?) I have some hope of getting the copper for the

magnet cheaply, ... Time is running short, for our orders for the steel will have to be sent in by the middle of June. After this date all the steel works will be busy on government work. The die must soon be cast. I think that if the cost of the magnet does not exceed £2,500 I shall take the chance of finding the money to provide the oscillator system. I am writing to Kinsey asking him to send along his description of the oscillators, so that we can estimate the cost of this part.

In his reply to Chadwick, written on the 26th May 1936[31], and referring to Met.-Vics. building 'a small cyclotron', Lawrence says that,

> ... if the matter is worth their attention at all, they should not hesitate in building at the beginning a large enough magnet for ten million volts.

He also puts forward the suggestion,

> ... that a vertical magnetic gap would be much more awkward.

and that,

> The suggestion of putting the high-frequency coil inside the vacuum to avoid dielectric losses and insulator trouble is very good. We have had it in mind for several years, ...

He agreed with Chadwick over costing and said,

> ... I think you are wise in spending all of your present available funds for the magnet. Kinsey is resourceful and could, if necessary, make the oscillator equipment with a small expenditure of funds. At the moment he is away on a holiday to Mexico, but he will be back in a few days and doubtless will write you immediately on these matters.

The foregoing section has helped to show that Chadwick relied heavily on the expertise that was available in the United States and was so freely given. It is convenient, at least for historians, that e-mail was not available at that time as letters are a much more permanent record of events, and many details of present day activities will not, in the future, be so readily found!

Chadwick had, of course, during the first year of his tenure at Liverpool, been corresponding with a number of British engineers and physicists while writing and receiving mail from the States. He was in frequent correspondence with Cockcroft and with business associates in Met.-Vics. and elsewhere, all giving him information in one way or another concerning the construction and building of 'his' cyclotron.

It is noted that during the first four years of Chadwick's leadership in the Department of Physics at Liverpool, the main thrust of his energy was directed toward building the cyclotron and introducing his students to a more 'leading-edge' knowledge of atomic physics[32].

The careful preparation and collaboration that Chadwick started, commencing with the fruitful cooperation of Lawrence in the States, continued in the U.K. with Cockcroft, Met.-Vics. and other industrial concerns. There were correspondence and meetings between three groups of involved personnel; firstly, Chadwick's Liverpool group, which included Kinsey and Walke (and later Moore); secondly, Cockcroft and the Cambridge group; and thirdly, the industrialists which included Met.-Vics., British Insulated Cable Company (B.I.C.C.), and the steel manufacturers and suppliers.

The correspondence, between early 1936 and late 1939, that the author has located gives an insight to the amount of preparation and planning that Chadwick devoted to the Liverpool cyclotron.

On the 30th March 1936[33], Cockcroft wrote to Chadwick saying that he had looked more closely into the magnet design, considering the air gaps, iron circuit, plane of the gap, windings and the magnetic field produced. Chadwick, who had had no heavy engineering experience, relied a lot on Cockcroft's expertise and had to make his decisions only after much careful thought. Cockcroft, in this letter, told Chadwick that in Paris their magnetic field figures gave between 17,500 and 19,850 gauss for 228,000 and 376,000 ampere-turns respectively and that if Chadwick used a specified and dimensioned copper tube (5/8" x 5/8") for the water cooled field windings (giving a resistance of 0.4 ohms), then 40 Kilo-watts (kW) of power would give the required magnetic field.

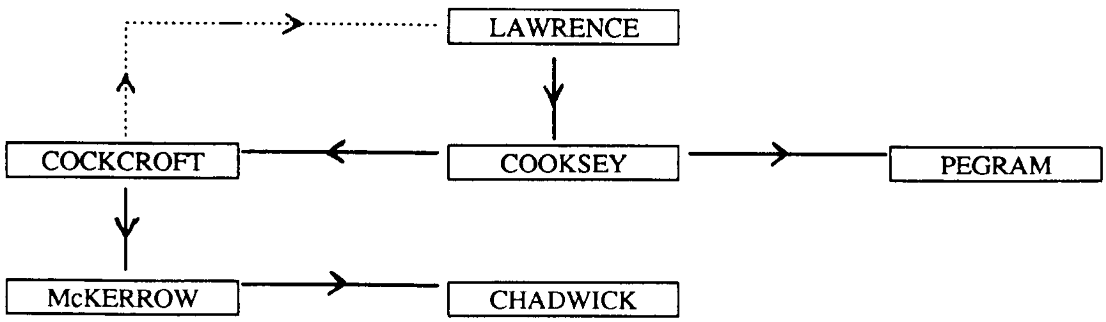
Cockcroft had also taken advantage - in the best sense of the word - of the freely available Berkeley Group information and practical experience. He had, as he wrote to Chadwick on the 21st April 1936[34], received a

> ... complete set of photographs and 20 pages of description from Lawrence for the cyclotron unit. ... I intend to send them on to McKerrow in two or three days so that you could see them after that if you wished.

The author has located a copy of this document[35]. The document took a rather roundabout route to reach Chadwick as McKerrow, from Met.-Vics., sent the copy to Chadwick after he had received a copy from Cockcroft, who, in turn had received it from Cooksey. Cooksey had originally composed the 'memorandum' for Professor Pegram of Columbia University and had dated it 'January 20 1936', but at Lawrence's suggestion, Cooksey also sent it on to Cockcroft. He, Cooksey, had also included 'ADDITIONAL REMARKS' for Cockcroft and a 'TECHNIQUE OF SOLDERING THIN WINDOWS' by Dr. McMillan which spelt out that procedure (See the Table 1 below). The detail included in this memorandum would have been invaluable for a cyclotron designer and constructor in those times.

TABLE 1

The route of Cooksey's memorandum



The contacts that either Chadwick or his wife's family had with industrial concerns, and also the industry's hope of commercial gain, gave him the opportunity to accept a gift from Nisbett of sufficient copper strip to wind the coils for the Liverpool cyclotron. Nisbett wrote[36] on the 25th September 1936, that,

Following on to our interview on the 21st inst., and discussion relative to the large electro-magnet that you are about to manufacture, I have consulted my Board on the question of the supply of the necessary copper - which I understand to be 8 tons in weight and to consist of 1" x . 2" strip.
Under the circumstances you mentioned to me, and which I put before them, they have agreed to supply the quantity of copper without charge.

Later in the same letter, Nisbett reports,

I am also authorised to inform you that we should be willing to supply Cambridge University with a similar quantity of strip on the terms of cost price to ourselves.

This generous offer from Nisbett and B.I.C.C. Ltd., probably reduced the overall costing of the cyclotron by approximately £800 and gave Chadwick some relief from the ever present financial difficulties of the Department.

Chadwick wrote to McKerrow of Met.-Vics., on the 1st October 1936[37], with the news regarding the gift of copper and he also wrote back to Nisbett on the 17th October 1936[38] in which he said,

I hope that the Acting Vice-Chancellor has now written to accept formally your gift of copper, and to express the thanks of the University Council to you and to your Board of Directors.

Chadwick in the same letter informed Nisbett of the coil requirements as,

I have settled the design of the coils with Metropolitan Vickers, and they are now ready to begin the winding.

The specification and '... the essential details ...' being as follows,

Specification of Copper Strip for Professor J. Chadwick, University of Liverpool

Bare copper strap, 1 inch by 0.2 inch, corners rounded, grade according to specification No.444 BSS. Cross sectional area 0.195 sq.inches. Total length required 22,600 ft.
To be supplied to Mr.Starling, Metropolitan-Vickers Electrical Co. Ltd., Trafford Park, Manchester.

A letter which Chadwick wrote to Mr. Starling of Met.-Vics.[39], dated 24th October 1936, states,

> I have heard from Mr. Nisbett of B.I.Cables that he has put the order for copper into the Factory, and that they will communicate with you directly about the most suitable lengths for the work. We ought to have the copper there promptly.

'Advice of Despatch' consignment notes[40] from British Insulated Cables, Ltd., have been located and some of the information contained therein is shown below in Table 2.

TABLE 2

Date	Number	Consigned	Goods	Quantity	Weight lbs.
4.11.36	WM 62537	Starling	Soft Copper Strip	10 coils 3790 ft. 1" x 0.2"	2374
16.11.36	WM 63389	Starling	Soft Copper Strap	20 coils 6230 ft. 1" x 0.2"	4803
20.11.36	WM 64375	Starling	Soft Copper Strap	31 coils 9763 ft. 1" x 0.2"	7526
23.12.36	WM 65948	Starling	Soft Copper Strap	11 coils 3528 ft. 1" x 0.2"	2697

(The total weight of copper delivered to Met.-Vics. was 7.77 imperial tons.)

It is now apparent that by September 1936, Chadwick had decided on air cooling for his cyclotron magnet after further discussions with Nisbett and the Research Department of Met.-Vics. (Mr. P.P. Starling), and not water cooling as originally suggested by Cockcroft. He, Cockcroft, had himself taken advice concerning forced air cooling and used this for the Cambridge cyclotron.

In his hand-written note to Chadwick[41] dated '26.8.36' on stationery printed with the Cockcroft's home address - Stansfield Hall, Todmorden - Cockcroft says,

> I am taking up the question of air flow with the Farnborough people to eliminate the hot spot behind the flow. G.I. Taylor tells me it is all worked out. I have asked for a slow speed fan to keep down noise.

As well as the copper (approximately eight tons) in a cyclotron of this size, there is a steel structure needed to contain and direct the magnetic flux. During August and

September of 1936 both Chadwick and Cockcroft were discussing and designing the magnet steel with industry and between themselves. In the same letter as above (26.8.36), Cockcroft informed Chadwick that,

> I went to M.V. yesterday and discussed the magnet. I had a letter from Hatfield offering to do the steelwork plus all machine work in a 0.1% carbon steel for £1500. Starling had a previous price from English Steel of £1100 and thinks £1200 ought to be ample. We had in the steelwork expert and he thought that the English Steel forgings would be quite good enough for the job.

Dr. W.H. Hatfield - mentioned in the above letter - was the Managing Director of the Atlas & Norfolk Steel Works of Sheffield, and it was on the 29th April 1936, that Cockcroft entered into discussions with him over the supply of steel forgings for the cyclotron magnets. In Cockcroft's letter to Chadwick dated 21st April 1936[42], he informed him that after seeing Hatfield on the 29th April he was going

> ... on to Metropolitan-Vickers on the 30th. Perhaps you could arrange to be there some time during that day, preferably in time for me to get the 3 o'clock train back to Cambridge?

Four days later, on the 25th April, 1936[43], Cockcroft wrote again to Chadwick to say that,

> I have heard from Hatfield that they would provide the steel part of a magnet exactly similar to Henderson's design for £1250, this of course includes machining.

In reply to questions asked by this author, concerning the Cambridge cyclotron, Donald Hurst said[44] that,

> I arrived at Cambridge in late September 1937; at that time the magnet poles and large iron slabs were lieing (sic) on the floor and other bits of equipment were scattered about; ... Cockcroft was in charge of the cyclotron, and W.B. Lewis was involved, ... In addition to a couple of technicians, there were three of us initially working on the machine - myself, Robert Latham and A.K. Solomon. ... Sometime later we were joined by Dr. A. Kempton;

As the previous information has suggested that the Cambridge and Liverpool magnet steel orders, and other components, were placed at the same time, the inference is that the steel for the Liverpool magnet had been delivered to Met.-Vics. by September 1937, for assembly and winding of the coils.

There was a third cyclotron planned in Britain and that was a 60 inch magnetic pole diameter, air cooled cyclotron at Birmingham University, designed again with much help from Lawrence and colleagues in the U.S.A. Professor (later Sir) Marcus Oliphant, who had taken the Poynting Chair of Physics at Birmingham in 1937, commenced the construction in 1938 after receiving a £60,000 bequest from Lord Nuffield. There appears to have been no active collaboration between Chadwick/ Cockcroft and Oliphant. The lack of collaboration is borne out in a communication from Professor W. Burcham, F.R.S., that the author received[45],

> ..., Oliphant did like to be independent.

The Birmingham machine did not come on stream until early in 1950, as work was interrupted by Oliphant's other war work and also most of his staff were transferred to America and Canada for the atomic bomb project and associated research. A description of the design and construction of the Birmingham machine may be found in a 1952 article in *Nature*[46] and also in a 1967 unpublished article by Oliphant sent to the author by Burcham and attached as appendix 6.

Chadwick, in a letter to Starling of 24th October 1936[47], informed him that Hatfield says,

> that he will put the work in hand as soon as he has the final drawings from you (for the steel slabs for the magnet). Will you let him have the drawings as soon as possible, otherwise if he has to wait for the drawings his 20 weeks (delivery time) will probably become 20 years, and I shall have to consult my successor on the matter.

Delivery of the copper to Met.-Vics. was indicated in the 'Advice of Dispatch' Notes in Table 2 (that is, November/December 1936) and is also referred to in correspondence between Chadwick and H.S. Carter of the 'Research Department' of Met.-Vics.[48] where Chadwick said that the magnet coils were ordered 'last August or early September' (1936). In the same letter, Chadwick told Carter that he had not yet sent an order for the vacuum tank 'which you are making' and would do so if required.

Kinsey helped Chadwick in overseeing the manufacture of the cyclotron parts at Met.-Vics. and it becomes apparent in the correspondence between Chadwick and Met.-Vics. from about the middle of 1937 that he was getting increasingly agitated over delays and 'slip-shod' work. For example, at the end of May 1937, McKerrow, who had been complaining of the alterations both Kinsey and Cockcroft were requiring, said that the time involved to incorporate the modifications 'will hold up delivery'. He also said in the last paragraph of this letter[49],

> A draughtsman got Mumps, there was a Coronation and people round here want a Whitsun holiday (which is rather incomprehensible to anybody from further N), but these are only small matters with which I won't worry you.

The author regards this statement as one which would have been written only if McKerrow and Chadwick knew each other very well!

The 'chamber' (that is, the vacuum tank and drum) was delivered to Liverpool by the 21st June 1938, and Chadwick informed Starling of Met.-Vics. that he was not satisfied with it. A list of 24 defects that the Liverpool team had found in the received items[50] from Met.-Vics. was finally corrected by a combination of replacement parts from Met.-Vics., local machining services and 'in-house' modifications by Moore and the physics department machine-shop mechanics.

The invoices for £1,200 and £529 for the manufacture of the exciting coils and the manufacture and supply of the vacuum tank respectively, were finally paid by the University at the end of May 1939. Invoices for the magnet, the motor generator and power oscillator have not been located. These items must have been installed, in the basement of the physics department, prior to the arrival of the excitation coils as the contract with Met.-Vics. included testing on site, and without the testing of the components the invoices would not have been paid.

It is also noted that a document was located dated the 5th April 1938[51], annotated in Moore's handwriting, showing the cooling inlet and outlet temperatures of the magnet top plate cooling pipe with the 'generator run at 200 volts, 200 amps - 40 Kw', so that the generator and the coils must have been 'in situ' by then.

In a letter to Starling dated 30th September 1939[52], Chadwick asked him to supply soft iron sheet for 'shimming' the magnet (to obtain a uniform magnetic field) and told Starling,

> ... that we got quite a good beam in August, and I think we can improve it considerably with good shimming ...

This statement by Chadwick confirms the cyclotron log book entry of July 1939 (reference 2.37) that indicated a 'beam' had been obtained.

In the course of research for this thesis, the author visited what was the Met.-Vics. works at Old Trafford, Manchester. Through company take-overs and modernisation, it is now G.E.C. Power Engineers Ltd., and they generously allowed the author access to their library and archive. The author was unable to locate any correspondence or other information relating to the building of the 37" cyclotrons.

The research done in this chapter has shown that the design and construction of the Liverpool cyclotron took approximately four years to complete. Chadwick had received considerable help from the States not only by encouragement but by practical assistance in the form of information and specifications. He had assembled a team of experienced 'cyclotroneers', technical and research staff, and had eventually overcome the difficulties of a commercial company that was struggling to meet re-armament contracts as well as its peacetime commitments. He was seeing graduates who had benefited from the introduction of new atomic physics courses and who were eager to start their careers doing research on the cyclotron. He was ready to start the research for which he had come to Liverpool; however, World War Two started between Germany and the U.K. on the third of September 1939. Publications from many of the Continental research laboratories and Universities ceased as they became occupied by the German armed forces. A number of foreign scientists had come to Britain, and others went to the U.S.A.: both countries gained from this influx of scientific manpower.

In his first four years as the Head of the Department of Physics at the University of Liverpool, Chadwick brought the department from a provincial teaching establishment to a leading research laboratory of its time. The new documents that were located and then discussed in this chapter have shown the detailed planning and care that Chadwick made in building the 37" cyclotron. He also established a collaboration with prominent nuclear physicists in the U.S.A. which were beneficial to him and the U.K. over the following war years and beyond.

References:

3.1. The documents may be found in File 1, Chadwick Archive, University of Liverpool.

3.2. *Letter*, Lawrence to Chadwick, dated 30th April 1938, Bancroft Library Archives, University of California.

3.3. Original signed, *Letter*, Cockcroft to Chadwick, dated 30th March 1936, Document 47, File 1, Chadwick Archive, Department of Physics, University of Liverpool.

3.4. *Letter*, Lawrence to Chadwick, dated 27th November 1935, Bancroft Library Archives, University of California.

3.5. See Biography, Staff; B.B. Kinsey.

3.6. See Biography, Staff; H.J. Walke.

3.7. *ibid.* 3.2, Document 10.

3.8. *ibid.* 3.2, Document 11.

3.9. *ibid.* 3.2, Document 62.

3.10. Original signed, *Letter*, Newson to Chadwick, dated 13th May 1936, Document 1, File 1, Chadwick Archive, University of Liverpool.

3.11. Hartcup and Allibone, *Cockcroft and the Atom*, Bristol, Adam Hilger Ltd, 1984, p. 61.

3.12. *ibid.* 3.11, p. 70.

3.13. *ibid.* 3.2, Document 12.

3.14. Original signed, *Letter*, Henderson to Chadwick, dated 1st June 1936, Document 22, File 1, Chadwick Archive, University of Liverpool.

3.15. From the statement made by Henderson it would appear that he had previously known Chadwick. On inquiry to the Cambridge University Archivist Dr. O. Green, the author was informed that Henderson had gone to Cambridge in 1925 from Yale, and obtained his Doctorate on the 19th June 1928. His Ph.D. thesis was titled 'The Scattering of ß Particles and the Heating Effects of Radium and Thorium Products'. Henderson would have, in his three years at Cambridge, come to know Chadwick quite well.

3.16. *ibid.* 3.14, also in Document 22.

3.17. Original signed, *Letter*, Kinsey to Chadwick, dated 15th May 1936, Document 13, File 1, Chadwick Archive, University of Liverpool.

3.18. *Letter*, Chadwick to Kinsey, dated 27th May 1936, Document 64, Chadwick Archive, University of Liverpool.

3.19. Original signed, *Letter*, Kinsey to Chadwick, dated 6th July 1936, File 1, Document 14, Chadwick Archive, University of Liverpool.

3.20. *Letter*, Chadwick to Kinsey, dated 16th July 1936, Document 65, File 1, Chadwick Archive, University of Liverpool.

3.21. See Biography, Staff; B.B. Kinsey.

3.22. Original signed, *Letter*, Kinsey to Chadwick, Document 15, File 1, Chadwick Archive, University of Liverpool.

3.23. *Communication*, Occupational Health Department to author, University of Liverpool, dated June, 1995.

3.24. Original signed, *Letter*, Kinsey to Chadwick, dated 11th August 1936, File 1, Document 16, Chadwick Archive, University of Liverpool.

3.25. See Biography, Staff; J.M. Moore.

3.26. Original signed, *Letter*, Lawrence to Chadwick, dated 31st January 1936, Document 27, Chadwick Archive, University of Liverpool.

3.27. *Letter*, Lawrence to Chadwick, dated 30th April 1938, Bancroft Library, University of California.

3.28. *Letter*, Chadwick to Lawrence, dated 16th April 1938, Bancroft Library, University of California.

3.29. *ibid.* 3.27.

3.30. *Letter*, Chadwick to Lawrence, dated 11th May 1936, Bancroft Library, University of California.

3.31. Original signed, *Letter*, Lawrence to Chadwick, dated 26th May 1936, Document 29, Chadwick Archive, University of Liverpool.

3.32. Perrin, G.K., *100 Years of Physics Teaching: Liverpool University Physics Department, (1881-1981)*, M.Sc. dissertation, February 1997, Department of Physics, University of Liverpool.

3.33. Original signed, *Letter*, Cockcroft to Chadwick, dated 30th March 1936, Document 47, Chadwick Archive, University of Liverpool .

3.34. Original signed, *Letter*, Cockcroft to Chadwick, dated 21st April 1936, Document 49, Chadwick Archive, University of Liverpool.

3.35. File 3, Chadwick Archive, University of Liverpool.

3.36. Original signed, *Letter*, Nisbett to Chadwick, dated 25th September 1936, Document 7, Chadwick Archive, University of Liverpool.

3.37. *Letter*, Chadwick to McKerrow, dated 1st October 1936, Document 15, File 2, Chadwick Archive, University of Liverpool.

3.38. *Letter*, Chadwick to Nisbett, dated 17th October 1936, Document 15, File 2, Chadwick Archive, University of Liverpool.

3.39. *Letter*, Chadwick to Starling, dated 24th October 1936, Document 67, File 1, Chadwick Archive, University of Liverpool.

3.40. *Communications*, B.I.C.C. to Starling, dated 4th November 1936, Document 3.
ditto , dated 16th November 1936, Document 4.
ditto , dated 20th November 1936, Document 5.
ditto , dated 23rd December 1936, Document 6, File 1, Chadwick Archive, University of Liverpool.

3.41. Original signed, *Letter*, Cockcroft to Chadwick, dated 26th August 1936, Document 38, File 1, Chadwick Archive, University of Liverpool.

3.42. *ibid.* 3.34.

3.43. Original signed, *Letter*, Cockcroft to Chadwick, dated 25th April 1936, Document 48, Chadwick Archive, University of Liverpool.

3.44. *Letter*, Hurst to Author, dated 1st December, 1994.

3.45. *Letter*, Burcham to Author, dated 3rd May 1996.

3.46. 'The University of Birmingham Cyclotron' *Nature*, Volume **169**, 22nd March 1952, pps. 476-7. (Unattributed article, but this author was informed in a private communication from Burcham, W. that he submitted the article to *Nature*.)

3.47. *Letter*, Chadwick to Starling, dated 24th October 1936, Document 18, File 2, Chadwick Archive, University of Liverpool.

3.48. *Letter*, Chadwick to Carter, dated 7th July 1937, Document 25, File 2, Chadwick Archive, University of Liverpool.

3.49. Original signed, *Letter*, McKerrow to Chadwick, dated 28th May 1937, Document 28, Chadwick Archive, File 2, University of Liverpool.

3.50. *Letter*, Chadwick to Starling, dated 5th August 1938, Document 48, File 2, Chadwick Archive, University of Liverpool.

3.51. *Communication*, Moore to Chadwick, dated 5th April 1938, Document 34, File 2, Chadwick Archive, University of Liverpool.

3.52. *Letter*, Chadwick to Starling, dated 30th September, Document 75, File 2, Chadwick Archive, University of Liverpool.

CHAPTER 4
Liverpool: The War Years

The author intends to set out in this chapter details of the vital research work that Chadwick conducted at Liverpool. The chapter will show that an indirect method of measuring the fission cross-section for uranium 235 (U^{235}) was applied with sufficient confidence for the atomic bomb project in the U.K. to be initiated. This work was completed on the Liverpool 37" cyclotron by June 1941 and was the basis of the Maud reports that triggered the atomic bomb project - Manhattan Engineering District Project (the Project) - in the U.S.A.

The information contained within the log books and Holt's laboratory books has enabled this author to give an indication of the research that took place in the Liverpool Physics Department on the cyclotron and the research on which Holt was primarily engaged. It is not possible, however, from the log book entries alone, to specify the detailed cyclotron research programme from early 1940 until approximately late 1942, due to the very brief or non-existent nature of the entries. By 1942 the entries were becoming more explicit although they were never complete.

The author has identified the handwriting of the writers of the log book entries over the period covered by the Second World War by enquiry to Holt, Kinsey, Martin, Rowlands, Reynolds, Randle, Sayle and Williams.

The correlation between the day-to-day log books of the cyclotron, Holt's laboratory notebooks and, from 1942 onwards, a series of monthly research reports are shown, which give a real time sequence of events. The conditions under which the researchers carried out the work and the precision of their measurements are also discussed. Also, the author has been able to draw on the personal reminiscences of a number of Chadwick's ex-students and is grateful for their permission to use extracts from private letters and conversations.

It must be remembered that while the scientific work discussed in this chapter was going on, there was a savage war raging literally around the ears of Chadwick and his staff, with frequent heavy bombing raids pounding the streets around the University. The Physics Department was situated less than one mile from the City docks and the secret bunker, from which the battle of the Atlantic was being directed. Some of this drama found its way into the log books, and is still fresh in the memory of those who lived through it. After some thought, this author included some of these stories in the text of this chapter lest they be lost to history. Hopefully, these digressions do not fragment the main scientific story too much.

Chadwick kept up to date with the progress of research at Liverpool during his absence in the U.S.A. via the monthly progress reports mentioned in the introduction. His knowledge and expertise of the atomic bomb project was shared by a very small number of the British contingent, and by only a few Americans. When President Roosevelt died on the 12th April 1945, his Vice-President (Harry S. Truman) had not been well briefed on the fission bomb. Chadwick, therefore, along with only a very few others, knew more about this aspect than did Truman. The development of the atomic bomb in the U.S.A. is detailed elsewhere, but the author will show that the weight of responsibility on Chadwick, and his diplomatic skills, were tested to the full during the critical two years of the project in America. His work was recognised by a knighthood from the U.K. establishment.

Between Chadwick's arrival at Liverpool in 1935 and 1940, he personally had not been in a position to carry out any nuclear research. His prestige however was in no way diminished and he was, in scientific circles, regarded as a leading nuclear physicist. He must have followed, along with the rest of the world's nuclear physicists, the history and development of fission.The immediate circumstances surrounding this discovery are detailed in appendix 4. Margaret Gowing, who was in the 1960's the Historian and Archivist of the U.K. Atomic Energy Authority, has in her book[1], given an account of these researches which were originated and being published from physics laboratories world-wide. She points out that none of these discoveries originated in Britain.

To set the scene for this chapter, a brief resumé of the history of fission is appropriate. In 1934[2], Ida Noddack, a German chemist, argued that the Italian scientist, Enrico Fermi, had not properly identified the 'transuranic' elements in his neutron bombardment of uranium - he had only looked for identification close to uranium and not all over, that is, much lower down, the periodic table. She also suggested that, under neutron bombardment, heavy nuclei might possibly fall into a number of large fragments which are isotopes of known elements but are not close to the irradiated elements. Although this paper was widely read, no-one, including Noddack, followed up this suggestion.

Irene Curie missed the discovery of fission in 1938 when she was completing a chemical analysis involving uranium products, and a German chemist (von Droste) examined the bombarded uranium with an ionisation chamber later that year and observed strong pulses which were

> ... undoubtedly caused by fission fragments but which he thought came from very energetic alpha particles[3].

Once the explanation and verification of Hahn and Strassmann's observations had been described in *Nature* and were published in February 1939, there followed much activity from various laboratories. Papers that were published included reports by Joliot in Paris, Bretscher and Cook at Cambridge, Abelson at Berkeley, Flugge in Germany, Fermi and his collaborators in Rome, and many others. Bohr and his former American student, J.A. Wheeler, later in 1939 published a paper which

> ... clarified and extended these ideas in what has become the classic analysis of the fission phenomenon. This paper was published two days before war broke out[4,5].

Multiple neutron emission from the fission process was recognised as an essential condition to bring about a nuclear chain reaction. If the chain reaction were to come about rapidly, there could be the possibility of an enormous sudden energy release which would cause an extensive 'nuclear' explosion. If there were not an explosion, then the released nuclear energy would cause a heating effect which could be utilised, for example, in electrical power generation. Apart from the novel aspects of research there were commercial considerations, and all of these considerations were examined in the U.K. as well as abroad. The French patented some of their findings in the hope of future benefit for France and these patents caused some difficulties later in the war.

Emitted neutrons in the fission process can be absorbed, scattered (causing an energy change), or they can be lost from the uranium surface. The actual numbers of neutrons emitted are therefore of prime importance in the consideration of a chain reaction. The volume of uranium must also be considered such that the number of neutrons which are produced within the uranium must exceed those that are lost at the surface, and F. Perrin in Paris first suggested that a 'critical' size of uranium was required to allow a chain reaction to take place. Peierls, at the University of Birmingham, extended the theories of critical size, or mass, and related it to the purity and density, and also the concentration of U^{235} in normal uranium. The probability of capture of a neutron by a nucleus also depends upon the time that the neutron is close to the nucleus, i.e., depends on its energy. The lower energies, and therefore velocities, of some neutrons mean they could be available for nuclei interaction for a longer period than very high energy, high velocity, neutrons. This concept introduces the idea of an effective interactive area of a neutron with a nucleus and is termed the nuclear cross section[6], measured in 'barns'. It is neutron energy dependent and varies from element to element.

The possibility of using slow neutrons with correspondingly high capture cross-sections was utilised by the Paris group to look for signs of a diverging reaction. A 'moderator' of neutron energy was used to slow the neutrons by first utilising water and then 'heavy' water (made from an isotope of hydrogen - deuterium - and oxygen). Von Halban et al in Paris[7] published in the middle of September 1939 their results that showed by using normal uranium a convergent chain reaction could be established, but they suggested that a divergent, self sustaining reaction was not possible.

In January 1940, L.A. Turner published a review paper[8] and in this article many of the nuclear uncertainties of cross-section, number of neutrons emitted, and even whether or not U^{235} was a fissile material, were mentioned. As 1939 ended and the 'phoney' war - for Britain - came to an end, there was an enormous amount of uncertainty in the process of fission.

But consideration of nuclear fission had come to the attention of Governments. In the U.S.A. in the summer of 1939, Leo Szilard and Eugene Wigner wrote a letter, also signed by Albert Einstein, to President Roosevelt outlining the military significance of fission. In the U.K., Professor Thomson (Imperial College, London) and Professor Bragg (Cavendish Laboratory, Cambridge) met and discussed the process of fission. They realised that Germany should be denied access to uranium which was at that time, apart from Canadian reserves, available only from the Belgium Congo. The two passed the results of their deliberations through government channels and it quickly reached Sir Henry Tizard. On the 26th April 1939, the Treasury were informed but the matter was not thought of as too pressing. The Treasury did however consult the English vice-president of the company owning the Belgian stock of uranium, Lord Stonehaven, who asked his company president to meet Tizard in London. The meeting convened in May 1939 and the president of the Union Minière du Haut Katanga, with its Head Office in Brussels, agreed to inform Britain if any abnormal demand for refined uranium were to be made. He was also able to tell Tizard that 100 tons of residues had only recently been transferred to the U.S.A. and there was not much left within Belgium. There were, in fact,

> only a few tons divided between different places[9].

Also in May 1939, Professor Tyndall (Bristol University), after writing a memorandum for the Chemical Defence Committee on the possibility of producing an atomic bomb, concluded that one or more tons of uranium would be required. As there appeared a danger of overlapping the researches, it was finally decided that all nuclear research on the possibility of a fission bomb should be directed through the Air Ministry. This was handled through Tizard's Committee on the Scientific Survey of Air Warfare (C.S.S.A.W.). When war started in September 1939, the urgency to investigate fission was further decreased and a number of University scientists were seconded for what seemed more pressing work. Cockcroft went on to radar research for example, and, in a letter to Starling on the 30th September 1939, Chadwick[10] said,

> I have lost Kempton, Kinsey and Thompson already, and some of the others may depart before long, *but I want to get the cyclotron running efficiently before we have to close down.*
> (This author has, for emphasis, italicised part of the above quotation).

Although the military significance of fission had been noted at that time, it was felt there were more pressing things which could be done that could yield practical and early positive results at that stage of the war.

But work on fission did continue, some of which turned out to be crucial. Oliphant (and Frisch) at Birmingham, although working mainly for the Admiralty on the war effort, continued the uranium and fission work at his University. Also, Thomson at Imperial College started to look for a divergent chain reaction.

The fission bomb was discussed in the War Cabinet Committee towards the end of October 1939. The Committee's Chairman, Lord Hankey, asked Dr. Appleton (the head of the Department of Scientific and Industrial Research, [D.S.I.R.]) about the feasibility of a uranium bomb. Appleton in turn sought the views of Chadwick who, in his first reply to Appleton in October, was comfortingly sceptical about whether a bomb could be produced at all. But he also told Appleton that he would think more about the subject and would write again to him.

It is crucial to realise that at this time Chadwick had his doubts about the current estimates for the uranium cross-sections. The only data Chadwick had when he wrote this first letter to Appleton was that given by Merle Tuve of the Carnegie Institute, which Bohr and Wheeler had mentioned in their paper[11] of August 1939, and Chadwick, had even then,

> ... began to think: "Well, this is too damn low altogether." I mean the total cross section must have been of the order of the nuclear diameter, and that meant a very small fraction of the collisions resulted in fission -- a very small fraction indeed[12].

The other information to hand were the papers of the French group and the more theoretical paper of Peierls.

It must have been around this time that Chadwick realised that a fission bomb might be possible, and saw a route to keep the Liverpool machine going by making more accurate measurements of the fission cross-sections. Evidence for this point of view is that in November 1939 Chadwick wrote to A.M. Tyndall at Bristol University to arrange a collaboration with C.F. Powell so that he could install his experiments, involving the use of emulsions on photographic plates for energy detection, on the Liverpool cyclotron[13].

> Powell had been studying cosmic radiation with special photographic emulsions produced by Ilford and he designed a camera using these plates to detect and measure particles emitted from a target in the cyclotron[14].

The point about this is that Chadwick would surely not have made such an offer had he thought the Liverpool machine was on course for closure as he had feared only two months before.

What Chadwick realised was that he had to convert the Liverpool cyclotron to

> ... get a beam of neutrons of definite velocity, and make measurements of the fission cross section[15].

and that Powell's technique could be used to measure the energy of the neutron beam.

By December 1939, Chadwick had come to the conclusion that a fission bomb was feasible under certain conditions using uranium, but between one and forty tons of uranium would be required. He wrote to Appleton on Boxing Day 1939 with this conclusion. But he also stressed that there was no reliable data and any results given to Appleton depended on what nuclear constants were assumed. To obtain these, Chadwick proposed in this letter to pursue some experiments with uranium oxide with the help of a Polish research worker, Dr. Rotblat. Chadwick also said he might need help 'in some matters', thinking particularly of financial help. It was at this time he intended doing research, for which he had not been funded, and he had already lost some of his staff. Appleton,

> ... wrote back and said that he would be ready to help in any way it was possible[16].

This confirmation of both financial and scientific support for the newly proposed experimental programme was a dramatic and unforeseen turn-around for the fortunes of the Liverpool cyclotron. It also offered the hope that the haemorrhaging of staff would stop, or might even be reversed.

Appleton sent the information that a bomb might be possible using very large masses of uranium to Lord Hankey who made the remark that,

> I gather that we may sleep fairly comfortably in our beds.

It is clear from the log book entries that the machine was taking a long time to commission. According to the daily entries covering this period (from July to December 1939), vacuum problems were the main difficulty. Once the vacuum was satisfactory, then the beam had to be located by judicious and empirical use of shimming which altered the magnetic field and so enabled the beam particle current to be maximised. Further maximisation was effected by tuning the oscillatory circuit feeding the transmission line 'trombones', which in turn supplied the radio frequency oscillatory power to the dees. However, by the begining of 1940, beams of a few microamps were being obtained.

The first major event in the new experimental programme was the arrival of Powell's group from Bristol. The apparatus that Powell brought was initially referred to in the log book as 'Powell's Apparatus', later it was called a 'camera', and was first connected to the target chamber on 17th January 1940. Various exposures were made on the emulsions between 17th and 24th January. This collaboration with Bristol brought Chadwick into contact with Alan Nunn May, who was, in 1946, convicted of treason. Rowlands[17], in a private communication to the author, describes the group from Bristol thus,

> Nunn May was a swarthy, gloomy lump of a fellow who accompanied Powell on his visits and was very much the assistant to extroverted Powell. ... Also accompanying Powell was a super-technician whose name I forget. But he was a superb glass blower and, like most of his trade, a superb beer drinker. He was very good company in contrast to Nunn May.

The problem the author has at this point is that the details of the precise experimental programme are not recorded in the log books, and hence it is difficult to get a clear picture of the actual experiments. A few facts can be gleaned. For example, during February 1940, selenium was irradiated by the proton beam for Rotblat, although the author has not established the reason why, and then the target chamber was filled with various gases which were also irradiated.

The only information Rotblat has given about this period is in his interview with Brown[18]. He says that Chadwick,

> ... gave me two people as assistants to help me. One was Wilson, an Australian, who came just for a year to do research. And the other one was rather an interesting character; may be you know him, Flanders, he's a physicist who did some work with Hal Gray.

Rotblat continues in the interview (about Flanders) that,

> He was a Quaker and a conscientious objector and so he did not go to the Army but chose to do some other work. ... and I had terrible qualms of conscience about getting him, Flanders, to work on something which potentially had military applications.

The above mentioned Wilson was R.S. Wilson, who is listed as a Demonstrator in the Department for 1939[19], but this author has found no further mention of Flanders.

None of this is very helpful! To try and get some information, the author sent 'Stan' Rowlands (see Bibliography: Staff; S. Rowlands), a copy of some of the log book pages including Monday, 22nd January 1940, to Thursday, 8th February 1940. In his return letter[20] Rowlands says that,

> Protons (and deuterons - I don't think we used α particles) about 4 and 8 Mev respectively entered the box at one end and to enter the emulsion edge-on; and so those travelling parallel to the plane of the plate into the emulsion would complete their range in the emulsion. Later the plates would be developed and the tracks

> measured for length, direction and grain density under a microscope (mainly by slave labour of the wives of staff members back in Bristol!)

Rowlands continues in the same letter,

> ... so we recorded elastic and inelastic scattering of particularly various gases (and foils I think) as on 20th February 1940. ... Powell had developed a range - energy relation for particles in his special emulsion.

Towards the end of February, a beryllium target was fitted (deposited onto an aluminium substrate) in the target chamber and it was irradiated with a 4μa beam of protons. It is noted that the convention in 1940 was to denote micro-amperes by μa. The 'background' had therefore been photographed and successful attempts had been made to photograph the recoil proton tracks formed by the neutrons from irradiated beryllium. The particle beam was gradually increased from approximately 3 micro-amperes (μa) in December 1939 to approximately 20 μa by the end of March 1940, by better tank alignment, improved vacuum techniques and suitable magnetic field adjustment. The cyclotron operators were also getting more familiar with the machine.

At this point we must recall that there was other work going on outside Liverpool which was to have a dramatic impact on the Liverpool research programme. As noted before, Thomson at Imperial had been attempting to produce a chain reaction. In March 1940, he wrote to Chadwick to inform him that his team had been unsuccessful in obtaining a sustained chain reaction. He also presented his results in a formal report to the C.S.S.A.W. committee in early April 1940, which showed that they had not been able to produce a chain reaction in uranium oxide by using either fast or slow neutrons. The slow (thermal) neutrons had been obtained by using paraffin wax as a moderator. Otto R. Frisch, in a report to the Chemical Society[21], came to the same conclusion as Thomson. But, the very act of writing the report made Frisch think more about the fission process and, as he said in *What Little I Remember*[22],

> But after writing that report I wondered, ... , one could use a number of such tubes (Clusius Tubes) to produce enough uranium-235 to make a truly explosive chain

> reaction possible, not dependent on slow neutrons. I used a formula by ... Francis Perrin and refined by Peierls to get an estimate.

He went on to say,

> Of course I discussed that result at once with Peierls. ... At that point we stared at each other and realised that an atomic bomb might after all be possible.

Frisch and Peierls talked over their findings with Oliphant that the U^{235} could be highly explosive and have a critical mass of only a few kilograms. Oliphant advised them to write their argument down and then send the report to Tizard. Their famous memorandum was completed in March 1940, and a copy is attached as appendix 5. It gave a summary of the mass of a possible bomb, the way it could be brought to criticality, and the devastation to land and personnel that its detonation would cause. It was a concise and cogent description of the atomic bomb. All of the criteria for producing a chain reaction could be met *if* U^{235} were to be used and its cross-section were to be of a suitable value. This step, once made, seemed obvious in hindsight. It was such a small step in the thought process, that it was assumed the German scientists would also have considered it.

On the 16th April 1940, Thomson wrote again to Chadwick to inform him that a sub-committee had been set up, with the parent committee being the Committee for the Scientific Survey of Air Warfare (C.S.S.A.W.), to consider the construction of a uranium bomb with enriched uranium; that is, by the separation of uranium 235 (U^{235}) and uranium 238 (U^{238}). Chadwick was invited and attended his first meeting of that sub-committee on the 24th April 1940. It was there that he learnt for the first time of the Frisch/Peierls Memorandum. He also learnt that the crucial measurements that were required were the nuclear cross-sections of U^{235} and U^{238}. These measurements were clearly going to be difficult as the proportion of U^{235} in natural uranium is only 0.7%, but these needs gave a clear priority to the work to be done on the Liverpool machine in the coming months.

This is a suitable break point in the history to make the first digression to see what was happening in war-time Liverpool. In the prevailing war-time conditions, all males of military age (18-45 years) were liable for National Military Service, and a National Service Committee had been set up in the University to consider whether or not students and other employees should be exempt. The author has found one of the forms that was sent to 'Mr. S.Rowlands' and dated '10 - vi - 1941' which was headed 'The University of Liverpool' and states,

> At a meeting of the National Service Committee, it was decided to exempt you for the time being from the Ministry of Labour and National Service requirement of joining the Senior Training Corps or the University Air Squadron on the grounds of Research and Univ. Fire Patrol Membership. This exemption is conditional on your continuing the above mentioned activities and is subject to revocation at the Committee's discretion.
>
> STANLEY DUMBELL
> Registrar

Rowlands, in a private letter dated 30th December 1994, to the author said,

> The whole cyclotron area was sandbagged (I think largely by us) but all the glass got broken when a land-mine fell on the Engineering building (I was fire watching on the balcony of the University Tower when that happened - we were unscathed but most of the Engineering building below us became rubble) so it was cool in there until the windows were boarded up and there was a lot of glass around.

An entry in the cyclotron log book between Tuesday, 11th March, and Tuesday, 18th March 1941 simply says 'Land Mine', but D.G. Martin has added a comment (in 1994) at the top of the page,

> Land mine on engineering building - much work needed to restore order in Geo. Holt.

Frisch, in *What Little I Remember,* also comments about the land mine (pps. 140-141), but says that it was Maurice Pryce who was fire watching. The plural indicated above in Rowlands' letter suggests there could have been two on duty that night.

When the author was in conversation with Holt discussing the damage caused by the landmine, Holt recalled that Chadwick asked him to discreetly take a Geiger counter over to the damaged building to see if there were detectable radiations! The point of this was to see if the Germans were dropping some sort of nuclear device and not to see if there had been an escape of radiation from the cyclotron area. The landmines themselves contained a large quantity of high explosive material - larger than the conventional bombs that were then being dropped. The explosion caused by the landmine on the engineering building was much bigger than the normal explosions, and Chadwick could have thought that nuclear devices were being used.

Returning to the main story, E. McMillan and P.H. Abelson, two American researchers, published a letter dated 27th May 1940[23] in the *Physical Review*, which showed that when a neutron was absorbed by a U^{238} nucleus, it formed a new isotope U^{239}, which after some decay by particle emission formed a new element 93, which further decayed to a long lived element 94. '94' was later called plutonium and '93' is now known as neptunium. According to Gowing[24], Chadwick asked that an official protest be made over this publication of McMillan and Abelson, but this author has not found substantive correspondence. Chadwick had correctly surmised - or had been informed - that plutonium was an even 'better' fissile material. Bretscher and Feather at the same time in Cambridge, had hypothesised along the same lines as McMillan and Abelson. In Birmingham, Frisch and Ernest Titterton had found examples of spontaneous fission in uranium while working on particle detection using an ionisation chamber. Due to war-time conditions of secrecy this was not published, but two Russian physicists, G.N. Flerov and K.A. Petrzhak, did publish and,

> ... are generally quoted as the discoverers of spontaneous fission[25].

In June 1940, it was clear that a coordinated attack on the fission problem was needed and the C.S.S.A.W. was disbanded, but its sub-committee was given independent status within the Ministry of Aircraft Production. This new committee called itself the M.A.U.D. Committee, later referred to as the Maud Committee, and the story of how it got its name is told by Gowing[26], Frisch[27] and others. The Maud Committee soon enlarged its membership with the addition of Professors Blackett, Ellis and Haworth (together with a representative of the Director of Scientific Research at the Ministry of Aircraft Production) and it had a civil servant secretary, Dr. B.G. Dickens; see appendix 8. Railway travel was difficult and time consuming in war-time Britain and Chadwick often had to travel to London from Liverpool as, The Maud Committee met almost invariably at the Royal Society[28].

At the June meeting, it was suggested that Professor Franz Simon (later knighted as Sir Francis Simon), a recently naturalised German and a physical chemist working at the Clarendon Laboratory, Oxford, should be asked to attempt the difficult task of U^{235} isotope separation. Chadwick was asked to co-ordinate all of the 'pure' physics work, including the work at Bristol with Powell and Nunn May, and Egon Bretscher and Feather at Cambridge, while Haworth of Birmingham University would undertake the chemical work on the production of gaseous compounds of uranium with help from I.C.I. Ltd..

So in mid-1940 Chadwick found himself with major new responsibilites on one of the most important and secret war-time committees. He quickly attempted to obtain the staff that the now crucial Liverpool fission research programme needed. One of the first to be recruited was Frisch. Chadwick recalled,

> And a little later on -- I can't remember exactly when it was, June, I should imagine -- Frisch came from Birmingham, where he had been, to work on the fission cross section and other matters connected with it, particularly on the measurements of the fission cross section[29].

Frisch joined the Liverpool team in the summer of 1940, while Peierls remained in Birmingham.

Also, about this time, Holt had finished his thesis research and he asked Chadwick if he could work on the Liverpool cyclotron. The research Holt started in the summer of 1940 is mentioned by him in a private communication to the author:

> Thus I became a junior member of the Liverpool group working on problems connected with the programme to develop an atomic bomb. Frisch was setting up a Clusius gaseous thermal diffusion separation column using uranium hexafluoride in an attempt to separate the isotopes and I helped him with this. Nothing came of it and it turned out later that the method does not work with this gas.

As noted in Chapter 2, another member of the team, Stan Rowlands, also joined at this time. Holt and Frisch worked together for almost three years and, as Holt has recalled, they soon became known as 'Frisch and chips'!

The log books shed little light on this era as log book 1a notionally covers the period March 1938 to the 12th July 1941 but has a gap in the entries between 9th May 1940 and 9th September 1940.

The workload on Chadwick was increasing. The Maud Committee meetings,

> ... built up, and we had regular meetings. At that time I had begun to make some measurements, and we found that the fission cross section -- while it was appreciably less than what had been guessed at by Frisch and Peierls -- it was also considerably more than appeared from Tuve's measurements. I should say three or four times as much[30].

Chadwick went on to tell Weiner in his interview,

> That wasn't the only thing we had to do, of course. We had to look at the inelastic scattering. We had to measure the velocities of the neutrons emitted in the fission to see what there was available, to get some idea of the number. I don't think we ever made that measurement ourselves. I'm not quite sure about that now. ... with the exception of one, everything had been done by April or May 1941. ... (the) exception was the time of emission of the neutrons -- whether there was a delay or not, which would have been quite serious. ... there was a small percentage which were called delayed neutrons -- emitted a little time afterwards. But that was the only factor, I think, that we had not measured[31].

If the timing of the information outlined in the two quotations above is factual - the interviews were made some 28 years after the events described - then the daily entries in the cyclotron log books, together with Holt's laboratory notebooks, should give a real time programme of the measurements. It also would appear unlikely that Chadwick could have been so time specific unless he had an 'aide-memoire' of some sort.

The task the author now has is to try and clarify the research programme as started in the summer of 1940. As noted above, there is a four month gap in the log books and Holt's notebooks show that he was not working on the fission cross-section programme.

The following extracts from the log books give an appreciation of the level of detail contained:

Wednesday, 9th October 1940, entry shows that,

> ... 3 Photographic plates (70 μ) fixed to holder on R.H. side 1). 0° 2). 50° 3). 90°
> (which was written by Pickavance)
>
> 3.36 p.m. Eggbox (two Al detectors +) in the eggbox at 50 cm. distance
> Angular distribution of Al detectors (0°, 20°, 40°), distance 24 cms
> (which was written by Rotblat)

The 70 μ referred to above is the emulsion thickness in microns; then the following day,

> ... Irradiation with aluminium detectors and paraffin collimator. 10mins.
> (written by Pickavance)
>
> Repeated again (0°, 20°, 40°)
> (written by Rotblat)

On the Friday, 11th October 1940, again written by Rotblat,

> Irradiation with Al detector. Angular distribution with no paraffin and paraffin collimator.
> Scattering of neutrons in PbO. 2 irradiations

and on Saturday, Pickavance wrote,

> ... 15mins run. Scattering of neutrons in PbO.

During Sunday the plates were examined and then early in the following week a further series of 'runs' were made with plates at angles to the beam on the 'L.H.S.' and the 'R.H.S.' (presumably the Left and Right Hand Side of the camera) and the plates were 'Cd covered'. The cadmium was possibly being used as a moderator to slow the neutrons by a known amount and would help show the accuracy of the photographic method of neutron energy measurement.

By the end of October 1940, not only PbO (lead oxide) had been irradiated but U_2O_5 (a uranium oxide) was bombarded in the same beam.

The cyclotron researchers were during November and December 1940, according to the log book, looking at the scattering in uranium oxide. Rotblat, obviously with a sense of frustration, wrote on Friday 22nd November 1940,

> Dobrze, psia wasza nsdza.

which, colloquially translated from the Polish means, according to Dr. M.G.Carroll, Director of Languages Centre of the University of Liverpool,

> It's a dog's life!

It is difficult from such entries to get a clear understanding of exactly what was happening. One conclusion could be that the uranium oxide experiments that Chadwick had told Appleton he planned (in the letter of 26th December 1939) were underway.

It is noted that uranium metal was not available at that time in the quantity required for irradiation, hence the oxide was the only readily available target. This author surmises that the difference between the lead oxide and uranium oxide neutron energy spectrum was found with the (approximate) presumption that the oxygen energy spectrum would cancel out.

It is as well to remember how difficult was the experimental programme. The measurement of particle energy was a problem in 1940, and W.D. Allen[32] in his book *Neutron Detectors* says,

> When one considers the problems facing the early experimenters, with low source strengths on the one hand and low detector efficiencies on the other, it is interesting to reflect that nearly all the basic properties of the neutron and of its interaction with the nucleus had been settled by about 1939, the year of the discovery of fission.

As their contribution to the fission programme, Frisch and Holt were developing a hydrogen-filled ion chamber working in the proportional mode. The principle of operation of an ionisation chamber for neutron detection is that if a neutron interacts between two plates with a voltage across them and a charged particle ensues, the charged particle dissociates the gas into free electrons and positive ions. The electrons and ions then drift to opposite plates - the positive ions normally take a much longer time to reach a plate due to their greater mass and lower velocity. The change in voltage across an external resistance connected across the plates will give a measure of the number of electrons formed by the recoiling protons. The number of electrons formed depends on the pressure and type of gas in the chamber.

Frisch and Holt commenced the manufacture of a hydrogen filled proportional counter and a uranium oxide layer ionisation chamber,

> ... which were both placed in a neutron beam from the cyclotron with the object of measuring fission cross sections[33].

An entry in the log book for Friday, 13th December 1940, concludes with,

> 5 mins run for comparing eff. of two counters.
> (written in Moore's handwriting)

On the same day there was an unexpected interruption to the experimental programme. A rocksalt crystal was irradiated and the

> Crystal painted with Glyptal to facilitate dissolving the active portion. Thin copper wire wrapped round it three times to pick up beam & indicate the correct resonance. 55 minutes. About ¾ μa.

A possible explanation for the programme interruption has been given by Rowlands in a letter to the author dated 18th January 1995, where he stated,

> The other incident was perhaps the beginning of clinical nuclear medicine. At Chadwick's behest we irradiated a beautiful crystal of rock salt, presumably to produce sodium 24; a motor-cycle Army despatch-rider was waiting and when we finished the irradiation he immediately set off for Oxford and Sir Hugh Cairns - neurosurgeon.

The author has yet to find any other explanation for this event.

Enemy action also accounted for some of the disruption to the cyclotron research activity. There are many examples cited in the log book. The Saturday, 21st December 1940 entry, indicated a broken Be target chamber window,

> This was due to enemy action, since the AC broke down for a time during last night's "blitz".

And, again on Sunday, 22nd December 1940,

> Pump again off - super blitz last night, breaking 13 windows in Geo. Holt.

During February and March 1941, there were a number of irradiations using 'thin Be target', 'thick Be target', 'irradiation of plates with scatterer', 'without scatterer', and irradiations of gases including acetylene, oxygen, hydrogen, methyl iodide, argon, helium, neon and nitrogen. Holt has said[34] that,

> The target elements under investigation were held in the body of the camera in gaseous form either as elements or compounds. Eleven different elements were exposed to the beam of protons and many excited states of the various nuclei identified through the energies of the inelastically scattered protons. The experiments were terminated when the cyclotron was pressed into service to study various problems in connection with the fission of uranium ...

But the main experiments on the cyclotron were[35],

> ... to determine the spectrum of the uranium fission neutrons by using photographic emulsions in which the tracks of the proton recoils were measured.

In the log book for Thursday, 4th March 1941, is a mention by Pickavance that,

> Uranium chamber and proportional counter
> (were installed in the cyclotron beam.)

In April 1941, there was a meeting of the Maud Technical Committee, where Chadwick reported on the results he had so far obtained and thought that a critical mass for U^{235} would be 8 kg or less. Chadwick listed his research activity in a letter to the Committee, and he informed them that,

1. Experiments on fission cross-sections were not quite finished.

2. Investigation had started on critical conditions for the bomb assembly by means of models, probably by utilising an optical analogy that had been suggested by Frisch.

3. Investigations were also proceeding on the energy spectrum of fission neutrons with some work on neutrons over 3 MeV and that there could be important investigations needed for neutrons under 3 MeV.

4. It was also necessary to measure the cross-section of separated uranium 235 (but that a sample was awaited from the U.S.A.).

5. More experiments might be necessary on the scattering of neutrons in uranium.

The Maud Committee was also told that Nier, in the U.S.A., had separated out a few micrograms of U^{235} and, therefore, some limited determination of its neutron cross-section could be made.

During May and June 1941, Frisch and Holt were continuing the improvement and calibration of the two chambers.

In Holt's laboratory notebook number 1, he has written for Thursday, 10th July 1941, the following comment,

> The increase in the U-cross-section with neutron energy is not as rapid as was expected from the results of American workers. (referring to McMillan and Abelson - reference 4.23). It is thought possible that the energy of the neutrons from the Li(p n) reaction is smaller than calculated owing to the product nucleus Be^7 being left in an excited state. To gain some idea of the neutron energy a photographic plate was exposed to the neutron beam. (Ilford special 100m, 2¾ x 1½" placed edge-on).

Both Holt and Chadwick have said that the uranium fission neutron spectrum was completed by the summer of 1941, but it has not been possible to reconstruct the actual experiments performed on the cyclotron, from the information available.

One thing is clear in this period: Chadwick was not involved in the day to day running of the experimental programme, but nevertheless carried a large administrative and lonely psychological load. For example, Holt says that the Maud Committee meetings took up a lot of Chadwick's time,

> ... Chadwick himself, due to his many other commitments was not able to take a close interest in everyday operations. In any case it was his policy, once responsibility had been delegated to someone, not to interfere but to seek an account at the end[36].

Of this period Chadwick said in 1969[37],

> I remember the spring of 1941 to this day. I realized then that a nuclear bomb was not only possible -- it was inevitable. Sooner or later these ideas could not be peculiar to us. Everybody would think about them before long, and some country would put them into action. And I had nobody to talk to. You see, the chief people in the laboratory were Frisch and Rotblat. However high my opinion of them was, they were not citizens of this country, and the others were quite young boys. And there was nobody to talk to about it. I had many sleepless nights. But I did realize how very very serious it could be. And I had then to start taking sleeping pills. It was the only remedy, I've never stopped since then. It's 28 years, and I don't think I've missed a single night in all those 28 years.

By the late spring of 1941, the work was done. Thomson had written a draft summary of the report of the Maud Committee and circulated it to the members prior to the meeting scheduled for 2nd July 1941.

> The original report was considerably redrafted, much of the work being done by Chadwick. ... the whole report was finished before the end of July[38].

The fact that the Maud Reports were completed by the end of July 1941 reinforces the Chadwick/Holt statements that the fundamental and crucial nuclear measurements necessary to make a uranium fission bomb had been completed in Liverpool by June 1941.

There were two sections comprising the Maud Reports,

> ... one on the "Use of Uranium for a Bomb" and the other on the "Use of Uranium as a Source of Power", (and) were each in the form of a general statement followed by technical evidence supporting it and by a variety of appendices[39].

The complete and final Maud Reports may be found in Gowing's book, *Britain and Atomic Energy 1939-1945*, Appendix 2, pages 394-436. The major conclusion of the reports, based on the work done at Liverpool, is to be found in the second paragraph of Part 1, where it is said,

> We have now reached the conclusion that it will be possible to make an effective uranium bomb which, containing some 25 lb of active material, would be equivalent as regards destructive effect to 1,800 tons of T.N.T. and would also release large quantities of radioactive substances, which would make places near to where the bomb exploded dangerous to human life for a long period.

To help appreciate the potential military significance of the '1,800 tons of T.N.T.' mentioned above, the anonymous author of the Maud Reports (but probably Chadwick) included as part of the Reports a graphic illustration of an incident that had occurred in 1917. An amunitions ship containing a total of 5¼ million pounds of mixed explosives (approximately 2,350 tons) blew up when in Halifax harbour, Nova Scotia:

> The zone of the explosion extended for about ¾ mile in every direction and in this zone the destruction was almost complete. Severe structural damage extended generally for a radius of 1 1/8 to 1¼ miles, and in one direction up to 1¾ miles from the origin. Missiles were projected to 3-4 miles, window glass broken up to 10 miles generally, and in one instance up to 61 miles.
> (This account is from the *History of Explosives* according to the Maud Report's Author).

During September 1941, the Maud Reports were submitted to and considered by the Defence Services Panel (D.S.P.) of the Scientific Advisory Committee. The D.S.P. members comprised Lord Hankey as Chairman, Sir Edward Appleton (knighted 1941), Professors A.V. Hill and A.C. Egerton, Sir Edward Mellanby and Sir Henry Dale (President of the Royal Society).

Their summary Report was issued in September 1941 and is reproduced in full in Appendix 7. The conclusion that affected Chadwick's team was contained in the first of five recommendations which states, in section 4, a direct measurement of the fission cross-section of U^{235} should be made as soon as possible. This statement highlights the fact that the Liverpool team had made indirect measurements only; that is, measurements on uranium oxide, or on U^{238} with 0.7% U^{235}, with theoretical calculations and assumptions extending the measurement to the isotope U^{235}. Clearly there was a worry that the result may be in error and should be checked with a pure sample of U^{235}. However, we now know that these fears were unfounded, as Chadwick commented in his Weiner interview that,

> ... the result that Frisch and I got was pretty right. I think it was only about one per cent different from the value that is accepted now after all this time. There must have been a bit of luck about that. We were very careful, but I must admit that there must have been quite a little bit of luck.[40].

To get a clear overview of the work that was done in Liverpool after the final Maud Report, and during 1942, it is convenient to look at the conclusions of Atomic Energy Report BR-47 dated 1942, which was produced at the end of the follow-on programme. This Report has been recently declassified and was written by a number of the Liverpool cyclotron group (excluding Pryce). The conclusions drawn under 'Remarks' state[41],

> We believe that we have established:-
>
> (1) that the average energy of the fission neutrons produced in uranium by thermal neutrons is abotu (sic) 2.1 MeV;
>
> (2) that there is no large proportion of slow neutrons in the spectrum of the fission neutrons;
>
> (3) that the energy spectrum follows very roughly a Maxwellian distribution.
>
> No claim to great accuracy is made for the results recorded in this report but we believe the accuracy obtained is sufficient for present purposes.
>
> One further remark must be made, that the present investigation relates to the neutrons liberated in fissions produced by thermal neutrons liberated in fissions due to the fission neutrons, the primaries and those which have suffered inelastic scattering in uranium 235. It is not likely that the energy spectrum of these latter should be sensibly different from that of those arising in thermal fissions, but we have as yet no direct evidence on this point.

This report therefore provides a useful guide to help in the understanding of the log books and other sources which are now available, and its results are also important in fission bomb design.

It is possible to correlate the research effort that enabled the Liverpool group to write BR-47 with comments in the log book number 2 (15th July 1941 - September 1944). For example, Holt wrote in the log book on Friday, 18th July 1941:

> Test of water tank method for neutron intensity using detectors of MnO

and the second of the three methods of finding the neutron energy spectrum mentioned in BR-47 is,

> (2) comparison of the distribution of the thermal neutrons in a water tank produced by the fission neutrons and by neutrons from other sources, referred to later as the detector method.

and the 'detector method' is described thus:

> In this method the absorption of the fission neutrons in water was measured and compared with the absorption of neutron beams fo (sic) different energies. A water tank, 15 by 15 cm. cross section, 24 cm. long, made out of cadmium sheet, was placed opposite the uranium box. ... A number of manganese detectors were placed at different positions in the tank to determine the distribution of thermal neutrons. The activities of the detectors (due to Mn^{56}, half-life 2.6 hours) were measured over a period of 5 hours.

Holt, with his expertise in counting and detection methods, would then do the necessary activity measurements and calculations.

It was not always serious work as the researchers were still mostly 'quite young boys' and occasional comments in the log books reflect this! Powell wrote as early as 23rd October 1940 that,

> The log book was never intended for facetious comments, but as a report of progress.

and underneath, in an unrecognisable hand,

> OR LACK OF IT.

And on Monday, 30th June 1941, Moore wrote,

> ... First run made at midnight ("By the Beard of the Prophet: Old George Holt's Ghost must be about"),

(and then in Pickavance's hand)

Tues 1 July	01-35	1st run completed - air raid
	01-soon after	all clear - strange noise in building turned out to be Charlie McCarthy

Holt has also commented on the less serious side of the research team[42] and, during the seemingly endless wait for the system to be re-pumped after repair, said,

> ... boredom sometimes gained the upper hand amongst the young research students, and led to a certain amount of horse play. Chadwick treated these lapses with indulgence. I remember one occasion when a student was tied up and left helpless on the floor of a room by himself. Chadwick then happened to walk into the room, and completely ignoring his plight, carried on a normal conversation with the embarassed student for a short while before marching out. Later he confided to his secretary, Miss Lloyd-Jones, that "the boys have been up to their tricks again".

Rowlands, probably tired - and frustrated - wrote on the 1st July 1941, after attempting to adjust a number of shims on top of the vacuum tank to improve the magnetic field,

> ... (6) Piles of rubbish of all shapes & sizes on top - in arbitrary positions. Beam 3.1μa
> Home - fed up to proverbial back teeth.

The only occasion the author has found that Pryce is mentioned was on Wednesday, 25th September 1941, when Rowlands wrote,

> Pryce called to see us. [and in pencil is written] SO WHAT!

By the middle of October 1941, a log book entry states that the absorption measurements in U_2O_5 were finished off, and on Thursday, 16th October, is written by Powell,

> Started preliminary investigations for the next experiment - measurement of energy & number of fission neutrons.

and during the following week,

> ... Continued measurements to detect fission neutrons.

On the next day, Tuesday, 21st October 1941, is written,

Measurements continued.
1. Relative intensity of slow neutrons increased by placing 9 cm. paraffin in front of target.
(a) no cadmium
(b) cadmium
2. All paraffin removed
(a) No Cd
(b) Cd
(c) repeat (a)

and by Tuesday, 28th October 1941,

... Irradiations completed without incident.

But the following day, Rowlands wrote in obvious annoyance, after attempting to process the exposed photographic plates,

Woe, Woe, Woe! There is nothing on the plates. Why?? because the -------- developer is really distilled water - clearly labelled I.D.2. Blast.

That day's exposures on the plates were repeated and the 'Plates processed.'

During early December 1941, a 'toast rack' detector holder was manufactured to hold up to eight MnO detectors and, after making a test run with a Ra-Be source,

to determine length of exposure needed to standardise detectors
(Friday 19th December 1941)

further plates were exposed.

There was apparently inclement weather during January 1942 as Powell wrote on Friday, 23rd January,

Since Tuesday, work has been slowed down on account of heavy snowfall - necessary for crew to arrive late & leave early.

At the end of January 1942 is a single entry marked 'Spectrograph' and the following Tuesday (3rd February) Powell wrote that the

Components of the spectrograph cleaned & assembled.

Table 3

'42	--	--	--	--	--	--	Jul	--	Sep	Oct	Nov	Dec
'43	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
'44	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
'45	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
'46	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
'47	Jan	--	Mar	Apr	May	Jun	--	--	--	--	--	--

Monthly Progress Reports

For comparison, see list of log books which may be found in reference 2.37

Holt wrote on the Thursday that,

> Spectrograph source assembly mounted between magnet poles & pumped out.

It is therefore apparent that a spectrograph had been obtained or manufactured and had either been installed or was to be installed on the cyclotron, and which was anticipated to be capable of resolving the mass differences of the uranium isotopes U^{238} and U^{235}. The entry for Tuesday, 10th March 1942, written by Powell, states,

> Sample ~ 40% from USA, (25 mg + 30 mg).

The above indication of a sample of enriched uranium from the U.S.A. is the only information found in this log book. Professors Pegram and Urey visited the U.K. in November 1941 but there is no record of their leaving a sample of uranium at Liverpool. However, W.A. Akers, who was the Director of Tube Alloys (which had superseded the Maud Committee), led a team of British scientists to the States for a protracted visit in December 1941 and returned in early March 1942. It is possible that one of his three-man team could have brought the sample back - the timing would be right.

This author has located a series of monthly reports[43] detailing progress on all the research work within the Department between July 1942 and June 1947. See the table 3 below. Reports earlier than July 1942 are referred to in the text of the July 1942 report (for example, "see report for May 1942") which infers that other earlier reports were made, and they possibly enabled Chadwick to recall his earlier work, but these earlier reports have not been located. See Table 3 opposite.

From a survey of particular letters in words of the monthly reports, it appears that they were all typed on the same typewriter - probably by Miss Lloyd-Jones in the Liverpool Physics Department. There does appear however to be a 'style' change when Chadwick went to the U.S.A. in 1943, and it is probable that Pickavance then took over the writing of these reports to send to Chadwick to help him keep up to date with the Liverpool work. Rotblat and Holt stated that they had not seen them prior to the author showing them a copy early in 1997.

In Holt's notebook number 5[44], page151, in the experiment dated 18th June 1942 under the 'Results', is written,

> Total counts from American sample = 3406 } Ratio = 1.945 ± .05
> " " " normal sample = 6626 }
> ... hence the separated sample contains 0.00585 m.gm U^{235} ± 2%

and with 25 mgm of sample, there is 5.85/25 x 100% = 23.4%
and with 30 mgm of sample, there is 5.85/30 x 100% = 19.5%
of enriched uranium and not 40% as indicated above in the log book entry.

Frisch and Rotblat made, as occasionally did Chadwick, written entries in Holt's books, but by far the greatest number were made by Holt. He was probably not unique in having personal laboratory notebooks and the other research members of the Liverpool cyclotron group would have collated their data in similar personal books. Chadwick in his Weiner interview has mentioned his notebooks[45].

> ... it must be in this little notebook somewhere; I copied it out for certain reasons from an old notebook, and it must be in here; I've lost the old notebook.

There is a clear date and experiment correlation between Holt's laboratory notebooks and the cyclotron log books, particularly after 1940 when Holt was more actively engaged in cyclotron experiments.

The first dated entry in Holt's notebook number 5 (the cover page is annotated Notebook No. 2, 1941-1942) is 'Oct. 20. 1941' and the experiment was,

> <u>To detect the fission neutrons;</u>

and then follows a simple sketch of the H-counter in relation to the uranium oxide, (which is surrounded on three sides by paraffin blocks), and the lithium target, with the appropriate cyclotron parameters by the side, showing,

> Cyclotron frequency = 9.96 m/c
> H-counter pressure = 60 cm.
> ~8 cm. thickness of U_2O_5; counts are taken with & without Cd box around the U_2O_5. Paraffin 5½ cm. thick.

The log book entry for Monday, 20th October 1941, in Rowlands' handwriting, shows,

> Continued measurements to detect fission neutrons.

A further example is shown for Friday, 22nd May 1942. In the cyclotron log book for this date, Rowlands has written,

> Double U chamber and double amplifier installed. This is to compare fission intensity (slow n) from a separated sample with unseparated one. Both quantities known. It was first of all sensitive to R.F. but this was rectified by attention to screening. Beam reading very inaccurate owing to contact zero deflection which was not affected by soldering all connections. Beam ≈ .25 μa → 400 counts in 5 minutes. Ratio is found to be approximately Sep/Unsep = 1.14. The fast neutrons (3.72 Mv - Li threshold) were slowed down by 4 cm paraffin and the chamber was surrounded by paraffin blocks. Surrounding the chamber with cadmium → no counts showing no fast neutron fission. A bias curve was taken to check relative sensitivity of the two amplifiers.

In Holt's laboratory notebook, the entry under 22nd May 1942 is,

> A double U chamber has been made up for comparing the slow n fission intensity from a separated sample with that from a known weight of normal U. At first there was interference from the H.F. (the cyclotron is not in very good condition - one insulator [D] is discharging slightly inside) but this was much reduced by placing H.T. battery for amplifier underneath instead of on top. Voltage on chamber = 480. Amplifiers count α-particles below 21 v. so are rather too sensitive. (LiOH thick target; 3.7 Mv. H').

As can be seen from the cyclotron log book entries of 1942 and later, they became more explicit, and information with more explanation (as above) was sometimes recorded.

After the Hankey Scheme was introduced (four term year and a degree in just over two years) starting in September 1942, the Faculty of Science Minutes record that,

> ... these arrangements be conditional on the S.T.C. parades being reduced to 100 per annum.

The 'arrangements' were the introduction of the Hankey Scheme and the 'S.T.C.' was the Senior Training Corps, so that students, as well as their normal studying, had the additional workload of drills (initially more than 100 per annum) and/or fire watching duties.

According to a private communication from Professor W. Williams, who was at Liverpool University as an undergraduate in the years 1941 and 1942,

> Early in WWII it became evident that much of the success of the war effort was going to hinge on electronics: Lord Hankey was given a brief to estimate the requirement and come up with a proposal. And the Hankey Scheme was initiated some time in 1941. ... In autumn 1941 (sic) the intake on the Hankey Scheme was 60. ... In the Physics department Joe Rotblat gave the Radio lectures, two or three each week, and there was an afternoon lab class.

It is from this time - with a small number of exceptions due to missing documents - that the monthly reports give further correlation with the cyclotron's and Holt's notebooks. The reports show in detail the progress of events as they happened in the Physics Department at Liverpool University over this particular time span. It is also noted that there is up to four weeks lag as they give an account of the previous month's work but they also occasionally give the direction future research should take.

The reports are divided into numerous sections and a list of the <u>initial</u> report sections are given below as an example. A discussion on the sections and a précis of some of the experiment results will be made.

Progress Report for July 1942
University of Liverpool

1. Cyclotron work.
2. Photographic plate.
3. Fast neutron spectrograph.
4. Automatic pulse analyser.
5. Alpha-ray analysis of sample.
6. Spontaneous fission.
7. Optical analogy.

It is noted that the above initial section headings in the first monthly report change as experiments are completed and new experiments introduced.

The first two weeks in June 1942 were taken up with some essential and much needed maintenance on the cyclotron, repairing the Siemen's oscillator set - no Siemen spares were obtainable from the German manufacturers! The contactor for controlling the power was failing and this was also repaired along with other electrical and electronic apparatus. There was the ever present disruption of electrical supplies due to air-raids, and then on the 17th June,

> Dr. Roberts asked us to stop because the R.F. was interfering with practical examination. Shut down.

But by Monday, 22nd June 1942, according to the log book,

> The experiment was to find the ratio of fast neutron counts in the two samples.

The following two days were taken up with

> Thick and thin layers of U in twin chambers to find range of fission particles in U. ... Runs were made but the amplifier did not behave itself well!

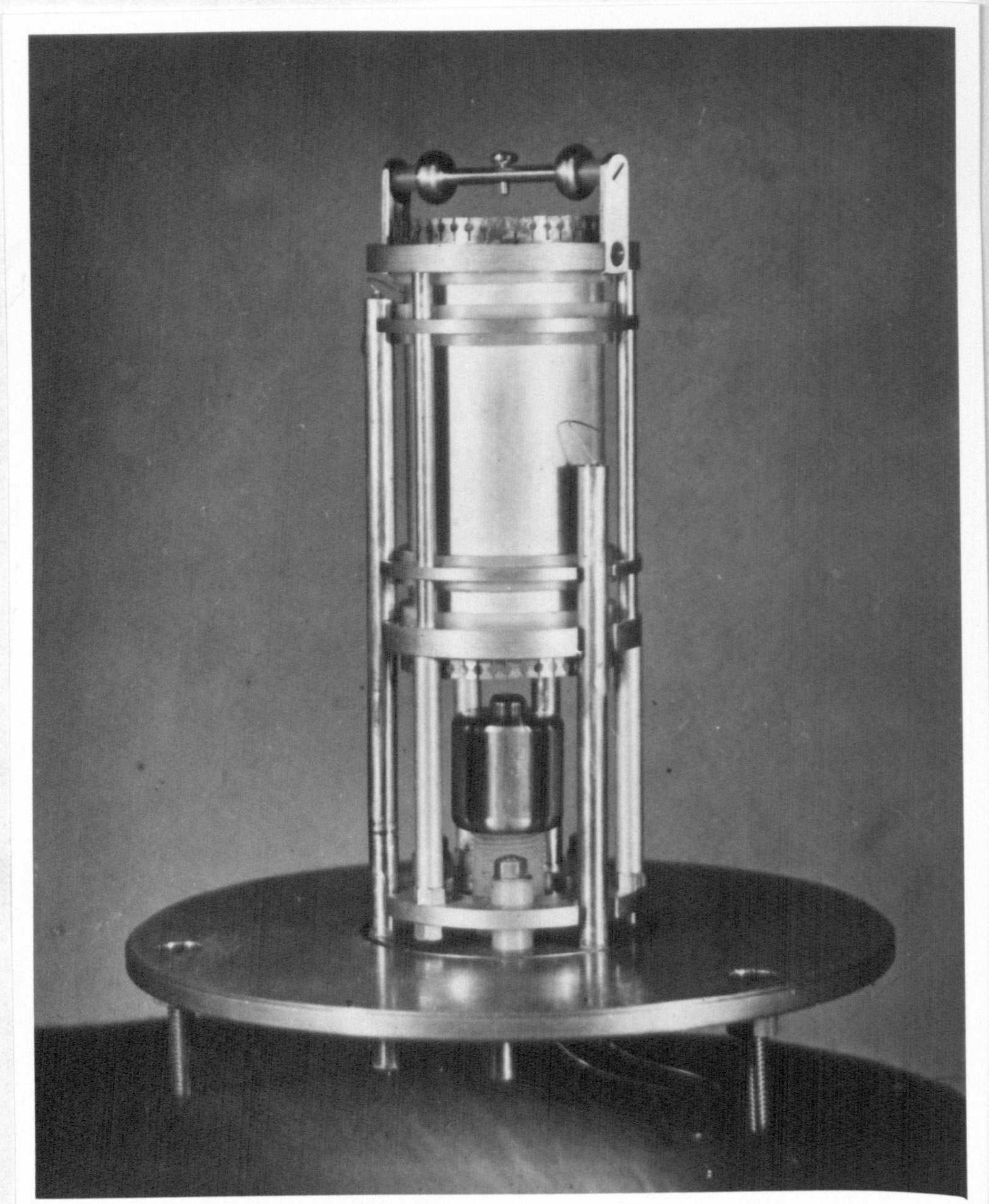
There were problems with research work undertaken in universities, as on the 30th June Rowlands writes,

> Work delayed by examiners meeting but cyclotron outgassed successfully and max. beam .35 μa, steady beam 0.24 μa.

In order to make a time sequential description of each of the various experiments taking place in the Department with effect from July 1942, the author will now discuss the experiments under section headings, but not in the order as given by the first monthly report.

Section 4.1. The Frisch Grid

This experimental device, devised by Frisch, gave a time advantage over the then current counters that had been built. The collector plate of the counter collected only pulses of ions with energies that surmounted the grid potential and so the counter



Photograph 4.4.1
Triple Counter
1942

became energy selective, and during July 1942, Frisch and Holt started experiments to test the triple coincidence counter,

> ... for use as a neutron spectrometer:

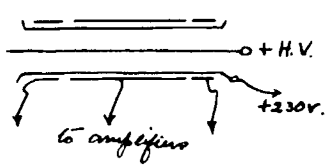
Holt wrote an explanation with experimental data and results in his notebook, together with photographs of the pulses obtained and the instrument itself. He detailed the ancillary electronic circuitry, and made various sketches.

The notebook entries of the triple counter commence between dated entries of '29 June `42' and 'July 6 `42' on page 161 and give full details of what later became known as the 'Frisch Grid'. A description of a Frisch grid is also contained in Allen's book[46], amongst others. Various refinements and modifications were added in later years to gain greater precision but Allen describes the original method as 'ingenious'.

Holt wrote in his notebook,

> The counter was made up at Oxford. The three cylinders are screened from the wire & one another by a grid of fine parallel wires of molyb. Thus pulses occur only when the +ve ions resulting from the multiplication process pass thro' the grid; they are much sharper than would be the case without this latter. Diam. of wire = 0.2 mm. Diam of grid = 1.0".

A sectionalised sketch of the triple counter is shown:-



Triple Counter

Holt continued,

> A H.V. set stabilised with midget neon lamps was built for the counter. Each lamp gives 150 v.
> A Power Pack was built to supply the amplifiers. The counter stage is fed from the unstabilised 400 v. tapping.

A photograph from his notebook of the completed counter is shown opposite.

After adjusting the hydrogen gas pressure in the counter and repairing a leak in the cylinder, Holt checked the sensitivity using a Th.B. source and then he noted that,

> A strong Ra Be (»1/3 gr.) source of neutrons was brought up to the counter. At several feet pulses several times the bkgd. due to gamma-rays could be seen from the central cyl. At a few inches these fused to form a large bkgd. above which proton pulses could be seen. Triple coincidences however were very rare (say 1 in 5 mins.)

He next found the voltage output with an induced number of ions on the pulse condensers, and equated this to the Th.B. alpha particle source (stopped down by 5 cms of air) size of pulse. A sensitivity of 240,000 ions with 1,560 volts on the chamber was obtained - the length of the main cylinder being 5 cms.

It was noted that pulses of 19 volts, only 2-3 times the valve background, were sufficient to trip the coincidence gate circuit, and on the 8th July 1942, the whole apparatus was transferred,

> to a small table & transported to the cyclotron room.

Electrical interference proved to be very troublesome in the cyclotron room due to the proximity of the powerful high frequency oscillators of the cyclotron, and numerous attempts were made to improve shielding and screening of components of the triple counter. By the 13th July 1942, most of the electrical interferences had been tracked down and either eliminated or had been reduced to a tolerable level. In the attempt to reduce some of the interference, one of the solutions had been to introduce an A.C. filter (for 50 Hertz) into the mains supply lead and it was noted that

> The A.C.filter got very hot.

Later that morning, after the counter experiment had been left for three-quarters of an hour,

> while photo-plates were being irradiated

it was found that both power packs had completely burnt out. It is probable that lack of cooling, localised high frequency heating and lack of proper layout procedures all contributed to the fire. The brief remark made in the cyclotron log book, written by Pickavance for the date Monday, 13th July 1942, was, after the irradiation of photographic plate 176:

> Fire in George's amplifier.

('George' was the name Holt was normally called by his colleagues at the time, derived from the 'George' in the name of the George Holt Physics Building!)

Reconstruction was put in hand immediately but there were the inevitable delays (for example, the transformer maker was on holiday!) The opportunity was taken to make various modifications such as incorporating

1. a sensitivity control for the coincidence pair C1
2. a negative bias (cut off) for the multivibrator valve associated with these
3. a positive bias on the filament of the diode to reduce the size of the background pulse from the coincidence pair C2 and
4. negative bias on the output valve.

The power packs were rebuilt on metal chassis mounted on top of each other and a metal lid to cover them was put on order. Also,

> All leads entering the two amplifier boxes have been fitted with filter circuits; in the case of filament leads condensers have being (sic) connected across them. Where screened leads were not used, twisted flex, one being an earth return lead, was used.

Testing of the newly shielded and repaired electronics continued, and sufficient confidence in the information they produced allowed Holt to use the instrument on the cyclotron from the end of August 1942.

Section 4.2. Automatic Pulse Analyser

Information the author has found concerning this work was obtained from the monthly reports, with effect from July 1942, and there is no indication of who was doing the necessary design and/or building of the analyser at this stage, other than Frisch. Further research manpower became available in WW2; these men were 'National Service Fellows' and were all well qualified. Instead of military service, they were directed to continue doing research in laboratories such as the one at Liverpool Physics Department. Peter Chippendale, Donald Marshall, Jim Hughes, Michael (Dickie) Deighton, Keith Baker and Keith V. Roberts were all National Service Fellows who came to the University of Liverpool, together with Eric Sayle who came from Leeds University. Sayle and Marshall both came to the Liverpool University Physics Department at Chadwick's invitation.

[One man who was doing research in the department from 1942-1946, and was listed as a Demonstrator, was H.K. Hsu. He, according to information Reynolds wrote on the back of one of his photographs (Photograph No.16, "Weekend at LLangollen", c. 1943), was,

> from China - cloud chamber

implying that Hsu was working on the cloud chamber. No further information has as yet been located about him.]

In *What Little I Remember*, Frisch said[47] he developed,

> ... a method for analysing small samples of uranium which had been enriched by an unknown amount that had to be determined ... which I proposed to Chadwick with some diffidence because it seemed an ambitious idea requiring several dozen radio valves.

After looking at the problem, Chadwick and Frisch decided to give the development of the analyser to I.C.I. Ltd., and,

> ... the instrument, which depended on having a dozen amplifiers working in parallel, each one counting alpha particles of a particular energy, turned out to be a very versatile research tool - the pulse height analyser or kicksorter.

Sayle, in a communication to the Liverpool University Archivist dated 24th August 1997, states[48],

> Liverpool, thanks to Frisch can take the credit for introducing a grid into an Ion Chamber, the conversion of Eccles Jordan cascaded binary counter to a decade counter by resetting a scale of 16 to zero at the 10th count and hence the introduction of the BCD scale and the first electronic multichannel Pulse Height analyser - christened "Kicksorter" by Frisch. In his book Frisch gives the credit to I.C.I. at Widnes, but all circuitry was first developed at Liverpool. I can remember Frisch and myself visiting Widnes and setting up a 12 channel system we had made for them.

The monthly reports give the progress, including in the November 1942 report,

> ... the main effort has been concentrated on completing a new apparatus which uses an automatic pulse analyser instead of a photographic recorder. All the parts are now ready and are being assembled and tested in Winnington.
> (Winnington was the name of a site of I.C.I. Ltd., near Northwich, Cheshire)

They show that in January 1943,

> The complete apparatus for the analysis of U samples is in working order and is now in use at Winnington.

Section 4.3. The Optical Analogy

A third major piece of research that Frisch, with Chadwick, was involved in at Liverpool was called the 'Optical analogy' - see section 7 of the July 1942 progress report previously listed.

Holt was asked in a personal interview in 1996, what was meant by the 'optical analagy' as referred to in the reports. He explained that it was the substitution of light rays and semi-spheres of plastics, that in combination could act as a simulator for neutrons and (sub-critical) semi-spheres of U^{235} respectively. It comprised a model of two hemispheres of perspex (of uranium sub-critical size) which could be brought together to simulate a critical mass of uranium. Work continued on the optical analogy until February 1944 and gave valuable details of the method of bringing together two masses in a very short time, as required in the manufacture of a uranium-235 explosive device.

The experimentation that Frisch conducted was made to show the necessary separation of the hemispheres and how accurately predictions could be calculated. Due to the mains voltage fluctuations, which varied the light output flux from the lamps simulating the neutron flux, as mentioned in the November 1942 report, accumulators were used which had much greater stability than the mains. Accumulators enabled accuracies of better than 1 in 1,000 to be achieved in calculations made from the measurements. The December 1942 report states that it appeared the critical distance between the two hemispherical models could be made appreciably smaller if the models were brought together along their axis of symmetry than if they were slid along their separating plane. This design feature was incorporated in the gun method of bringing the sub-critical masses of uranium together in the first atomic bomb that was dropped. Plastics of various colours were used to look at their absorption coefficients and this was related to the change in light flux multiplication values as the hemispheres were slid along different planes of separation. During the spring and early summer of 1943, testing and modifications were made, including making use of two tanks - one to contain the models and the other to be used as a reference tank. The reference tank contained a black semi-circular disc that could be rotated to expose any desired fraction of it. The new tanks were illuminated by four Osram 60 watt strip lights each with shades to protect the central portion from direct light and unequal heating effects. Three selenium type photocells were in each tank and, by positional changes and output mixing, the light intensity in each tank could be balanced.

To test the effect of scattering and absorption - simulating the neutrons in uranium - black perspex hemispheres were used but to be more closely analogous to uranium, semi-spheres containing suspensions in paraffin were investigated. Finally, a coarse powder of polythene and titanium oxide (prepared at I.C.I. Ltd. in Winnington) and embodied in the perspex was utilised, which combined the scattering and absorption effects of the light in the desired manner.

By the time the report of January 1943 was written, a new spherically illuminated system had been constructed and found satisfactory. Two cylinders of scattering

Photograph 5.4.2
Don Marshall seated by his equipment (optical analogy of uranium assemblies)
c. 1943

material had been received from Winnington each having a hemispherical hollow in the centre of one face into which absorbing hemispheres could be fitted, and the last report states that determination of the variations in escape probabilities with the various separations of different absorbing hemispheres were in progress.

Frisch went to the U.S.A. - he was the first of the British mission to arrive at Los Alamos with E. Titterton - on 13th December 1943; Chadwick did not arrive there until 12th January 1944[49]. The author has found no mention of this work taking place in Los Alamos. Gowing[50] has mentioned that Frisch was working on an optical analogy:

> To elucidate some of the problems of critical size Frisch at Liverpool constructed an ingenious perspex model by way of an optical analogy.

and Peierls, in Frisch's *Biographical Memoirs of Fellows of the Royal Society*[51], states,

> A very characteristic idea of his was to "mock up" the scattering of neutrons in a uranium block of complicated shape, a problem relevant to bomb design, by using the scattering of light in a partially absorbing mass of perspex of corresponding shape. This could have been used to check the theoreticians' calculations, which for general shapes were not too easy. In the end the procurement of the necessary transparent plastic with absorption took too long, and meanwhile enough confidence in the calculations had built up, so the project was abandoned.

Although the information on the 'analogy' in the reports ceased after February 1943, presumably because Frisch went to the States, there was no indication of serious hold-ups mentioned in the reports. It is therefore possible that most of the useful information obtainable from these models had been collated.

The very 'unofficial' photographer in the Physics Department at the time of the experimentation on optical analogous behaviour was Reynolds and he has allowed the author to copy a number of his photographs that he took over this period, one of which is reproduced opposite and is, as far as the author is aware, the only known photograph of the optical analogy apparatus. The description on the back of the photo is also reproduced opposite and indicates two facts. The first is the approximate size of the apparatus which, in the photograph, is on Marshall's left hand side, and the second is that Marshall was there working for Frisch.

Chadwick, in a letter marked 'Secret' to Sayle, inviting him to work at Liverpool and dated 11th May 1942[52], wrote,

> The kind of work you would have to do would be largely electrical measurements of various kinds; some of it would probably be a kind of photometry, but most of our work involves at some point electrical counting methods using valve circuits. I cannot be more precise at this stage.

Sayle, in a private communication to the author dated 4th May 1997, said that Chadwick told him he would be,

> ... involved with photometry or counting methods involving valve circuits. In the event, Don Marshall worked on photometry and myself on electronics - both initially under Frisch.

Section 4.4. Alpha-Ray Analysis of Uranium Isotopes

In order to construct an atomic bomb which would utilise U^{235}, it was necessary to find the enhancement of U^{235} due to any of the proposed enrichment processes.

Chadwick and Lawrence had kept up their correspondence and, as previously mentioned, copies of the correspondence are kept at the Bancroft Library and in the Chadwick Archive of the University of Liverpool. Lawrence must have sent to Chadwick at least one sample of uranium enriched in the 235 isotope as Chadwick wrote, in a letter to Lawrence, on 24th May 1942:

> You will have heard already that the magnesium sample you so kindly sent to me arrived safely, that it was some time on the way. Some time elapsed, too, before we could examine the sample, for we were in the middle of a set of experiments. I want now to tell you the result of our analysis of the sample. It is rather surprising and disappointing; there is far less magnesium present in the sample than there should be on the information I received through the British Central Scientific Office, Washington.

The sample that Chadwick received from Lawrence was referred in the letter as Mg Fraction 1/18/42. This author infers that the term 'Mg fraction 1/18/42' is referring to the use of an MgO crucible that was used to melt the enhanced uranium 235 'biscuits' and that

> ... the metal (U^{235}) was cast into a mold made with a steel ring inserted between magnesia top and bottom plates[53].

The '1/18/42' is probably the date of casting the sample (18th January 1942).

Chadwick was concerned that the sample Lawrence had sent to him for analysis was found to have a different composition to that which was stated in the information accompanying the sample. Chadwick said in this letter,

> The analysis was carried out in two ways. The first was a method which Frisch has developed for the analysis of U samples and which for a suitable sample gives an amount of each isotope. The ionisation pulses due to the α-particles are recorded by means of an ionisation chamber and linear amplifier.

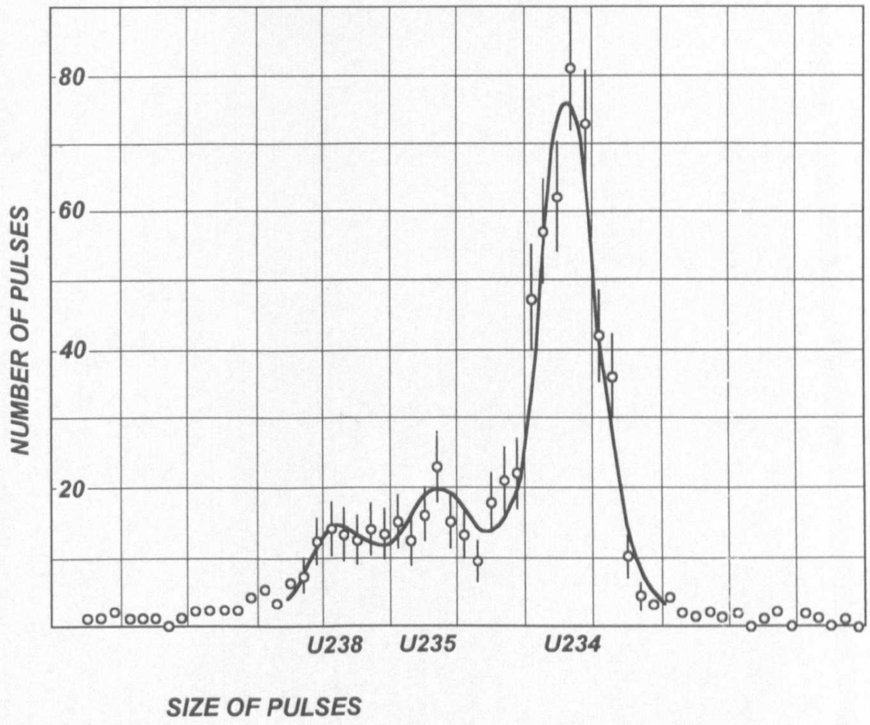
It was vitally important that verification of the enrichment could be made. One method of verification would be to record the alpha emission from the uranium isotopes and a plot of the activity of the isotopes against the alpha emission would give an indication of this enhancement. It was surmised that each uranium isotope would emit alpha particles of a particular energy.

While he was at the University of Liverpool, Frisch wrote an undated report[54] - BR-49 -which has now been declassified, on the analysis of uranium samples using α-ray particle energies. He describes in the report the method of doing so thus:

> The method depends on the fact that, in an ionisation chamber filled with nitrogen, saturation can be readily obtained; all the negative ions, which in pure nitrogen are free electrons, produced along the track of an α-particle, whatever its direction or position in the chamber, can be drawn to the collecting electrode. If this electrode is connected to a linear amplifier, the pulse will be proportional to the energy of the α-particle. Thus the analysis of the pulses according to size should give groups corresponding to the α-particle groups of the isotopes 234, 235 and 238.

The half-life of U^{234} is very much shorter than the other two isotopes of uranium and this is the reason why the activity is comparable to the activity of U^{238} although its concentration is only 0.006% by volume of normal uranium. (Activity is here defined as the number of alpha counts from the isotope divided by its life-time.)

A copy of the 'Number of Pulses' versus 'Size of Pulses', (Figure 4 in BR-49), is shown below:

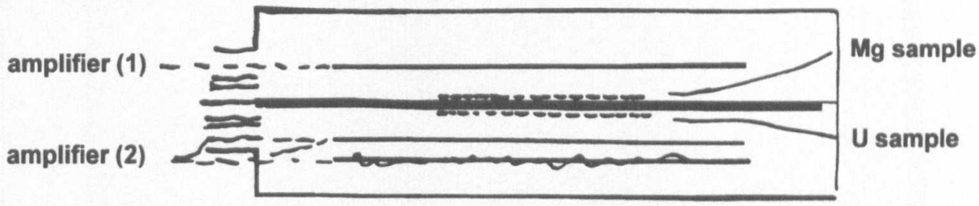


The Number of Pulses versus Size of Pulse plotted in BR-49

However, it is noted in Chadwick's letter to Lawrence of 24th May 1942 that a second method was used, and Chadwick described this method as below:

> The second method used was to compare the number of fissions produced by thermal neutrons in the sample with the number produced under the same conditions in the U sample. The magnesium sample was fixed on one side of a thin brass plate, the U sample on the other side, as in the rough sketch of Fig. 4. We thus had two fission chambers side by side, and these were buried in paraffin wax and exposed to a neutron source. The usual precautions were taken, and in addition chambers, samples, and amplifiers were interchanged.
> (The Figure 4 mentioned above is reproduced below as Sketch 2.4.2).

Sketch 2.4.2



Comparison of thermal neutrons using side by side fission chambers

This small experiment was obviously used to verify the α-particle analysis that Frisch had conducted, and comparable results were obtained with Frisch's experiment.

The verification was thus confirmed by two separate methods; firstly, by the fission fragments where the sample disc was flooded with thermal neutrons, and the subsequent fissions detected by the counters (and compared with 'normal uranium'), and the second method by the α-activity of the sample in a manner devised by Frisch and written up by him in BR-49.

From the figures in Chadwick's letter to Lawrence, the following summary may be made:

Lawrence's analysis shows	U_{234} = 0.014 μg:	Chadwick's analysis shows	U_{234} = 0.006 μg
	U_{235} = 25 μg:		U_{235} = 6 μg
	U_{238} = 38 μg:		U_{238} = 30 μg

Assuming a normal sample of uranium would contain 0.006% U_{234}, 0.7% U_{235} and 99.3% U_{238}, then it is found that the enrichment of 235 in the samples is:

Lawrence's sample ≈ 94 times (39.7% enrichment)
and Chadwick's sample ≈ 25.6 times (16.7% enrichment)

The date of the Chadwick/Lawrence letter (May 1942) shows that the α-particle analysis experiment was under way a number of months before the first monthly Progress Report (July 1942). It also gives an approximate date when the work was completed in order that BR-49 could be written; however, Holt, in his notebook number 2, on pages 166-9, dated 24th July to 1st August 1942, gives a detailed description of an experiment using a triple gridded apparatus, and commenced on the 24th July 1942 by writing,

> During Dr. Frisch's holiday am running the apparatus for testing partially separated U samples by counting the two α-particle groups. The chamber is a large one fed with N_2 freed from all trace of oxygen ...

Oxygen has a great affinity for free electrons and would capture them, giving a false electron count.

The above description (but continued in Holt's notebook) of the apparatus is similar to that in the BR-49 report. The notebook shows that Holt was able to take the data for Frisch. Part of a handwritten, signed, but undated letter from Frisch inserted in the notebook at page 166 states,

> My address for the next few days (say until Wednesday) will be c/o Dr. Hope, St. Anne's Well, Andover (Hants).
> Would you please give this address to Miss Lloyd Jones and to Mr.Welch, together with my best regards.

The correlation between Holt's notebook and Frisch's report is reinforced by a study of the monthly reports which give details of the experiment on α-ray analysis and continues from the July 1942 report to the report of February 1944. But Frisch had, according to the Chadwick/Lawrence letter, been working on α-particle analysis earlier in 1942 - prior to May 1942.

The monthly reports show that by June 1943 the original ionisation chamber apparatus had been modified and redesigned. It was passed over to the Winnington team of I.C.I. Ltd. for construction and use there. This new design had, for example, no wire grid; instead, the collecting electrode was a wire stretched axially through a tube which had an opening on one side through which the electrons produced by the α-rays were drawn into the tube. All of this work was developed in Liverpool before passing it over to I.C.I. Ltd.

The monthly report for August 1943 states that the analysis of Lawrence's latest sample was carried out in Winnington. This statement implies that Lawrence sent a number of 'samples' of enriched uranium to Liverpool. Their analysis helped to verify the percentage enrichment of uranium in its 235 isotope which could confirm the Berkeley Laboratory's findings.

Section 4.5. The Mass Spectrometer

Another way of measuring the uranium isotope ratio is by using a mass spectrometer. A mass spectrometer can also be used as in the electromagnetic method of isotope separation. If isotopes of the same velocity are introduced into a magnetic field, then they will travel in circular paths. If they are emitted from the same point, then the heavier particles will be in a group at a different position to that of a lighter group of particles. The groups of particles can then be allowed to strike a photographic plate or their charge can be collected, so that an indication of their mass and abundance may be obtained. This principle was first used by J.J. Thomson and later developed by F.W. Aston (1920), and a short time later by A.J. Dempster (1922). By 1927, Aston had further improved the instrument design and the theory behind it, and gave details in his paper published in the *Proceedings of the Royal Society*[55]. A.O. Nier, in the U.S.A., continued and improved on Aston's work and did many investigations on isotopes[56].

According to Hoddeson et al in *Critical Assembly*[57],

> At Berkeley, Lawrence used his mass spectrometer to work on the electromagnetic isotope separation method, in which gaseous uranium ions travel circular paths under a magnet, the ions of the lighter isotope following a tighter circle ending in a collection cup. In early January 1942, Lawrence produced by this method 18 μg of material enriched to 25 per cent ^{235}U. The next month, he produced three 75 μg samples containing 30 per cent ^{235}U. On the basis of these successes, he planned to use the magnet from the 184-inch cyclotron to provide fields for a number of such mass spectrometers - "Calutrons," named for the University of California (California University Cyclotrons). These devices would provide the material for the initial American and British experiments.

In order to investigate the different isotopes of uranium, Pickavance, at Liverpool University, built a mass spectrometer. Its progress has been recorded in the Monthly Progress Reports commencing in September 1942. He had two possible methods of producing the ions of uranium: the first was by heating a salt of the element and the second by bombarding a uranium gas with electrons, and the latter method was used. The uranium gas that he worked with was uranium hexafluoride (called 'hex' in the majority of the notebooks and reports), and supplies of this very corrosive and poisonous gas were obtained from I.C.I. Ltd.

Glasstone has said in his book *Atomic Energy* (page 185) that,

> In considering the possibility of separating the isotopes of uranium, it should be noted that uranium is a solid metal, and so it is first necessary to choose a compound of uranium that can be readily converted into a gas. A fairly obvious choice is uranium hexafluoride UF_6 for although it is solid at ordinary temperatures, it is easily vaporised.

Pickavance started to work analysing the isotopes of mercury - probably as these were well documented - in order to familiarise himself with the operational features of the mass spectrometer that he had built. By the end of 1942, he had finished this practice and started using hex. The nature of the hex caused a number of difficulties, including corrosion of the copper to glass seals which gave a number of vacuum problems, but by the April 1943 Report, the measurements obtained with hex were 'fairly satisfactory'. A null-method of measurement was used and consideration given to a de-mountable type of mass spectrometer by May 1943. The June 1943 Report specifically mentions the measurements taken on the Nier type spectrograph and that considerable progress had been made in the construction of an all metal spectrometer - that is, a metal demountable spectrograph. A resumé of the Report for July 1943 shows that the spectrometer resolution was still not satisfactory when hex was under examination. Improvements to the electrical circuits were undertaken and it was hoped this would increase the measurement accuracy. But, it was emphasised, the chief difficulties arose from the chemical action of hex, especially on the filament.

Between August 1943 and May 1944, work on the original spectrograph continued. This work was accelerated by the addition of people transferred from other work. (It is here surmised by the author that these people came from the cyclotron after its principal researchers transferred to the U.S.A.)

Work continued at Liverpool on the mass spectrograph with the help of Randle and Sheila Rumsby. Pickavance travelled to the U.S.A. in order to obtain information on a spectrograph that was used to determine the isotopic ratio of 'X in XF6'; that is, the ratio of the uranium 235 to 238 in the uranium of uranium hexafluoride.

Pickavance wrote up the results of his stay in the States of 'several weeks duration' in an Atomic Energy Report number BR-46 dated 14th June 1944[58]. Annexed to this report are two further reports which give the

> Requirements for the Liverpool Spectrometers.

(Note the plural in 'Spectrometers' above; these were the glass machine and the all-metal machine which had been built and operated in Liverpool.)

The second annexed report was titled,

> American Equipment Required for the Mass-Spectrometer.

The 'American Equipment Required' gives at the end a very detailed component list to be obtained from America, as,

> All the foregoing are items which are very difficult or impossible to obtain in U.K.

Pickavance, in the introduction to BR-46, states,

> This report was compiled from information received during a visit, of several weeks duration, to M.G. Inghram's group in the Nash Building. Inghram and his assistants were extremely helpful at all times, and spared no effort to put me in contact with any part of the work on the spectrometer in which I was interested. The initial contact was made with Dr. Dunning, who was also very helpful.

The first annexed report shows at its end who Pickavance thought should work on the spectrometers in Liverpool and stated,

(The 'PG' referred to in the following extract is, in this author's opinion, Pure Gas; that is, pure uranium hexafluoride.)

> The following will be required in Liverpool in order to make possible the early analyses of samples of PG. This is based on experience acquired during a visit to the spectrometer group in New York.

There then is a list of personnel required including P. Reynolds, M. Deighton, T.C. Randle and J. Hughes with the proviso that

> All the above men will work full time on this branch of the work, with the exception of limited teaching duties carried out by Randle and Reynolds.

The technician effort required was then listed and the following were named:

> A. Hall, (workshop, half time), T. Norris, (workshop, two days per week), E. Rodger, (junior lab. asst., part time) and R. Sheils, (senior lab.asst., during vacations).

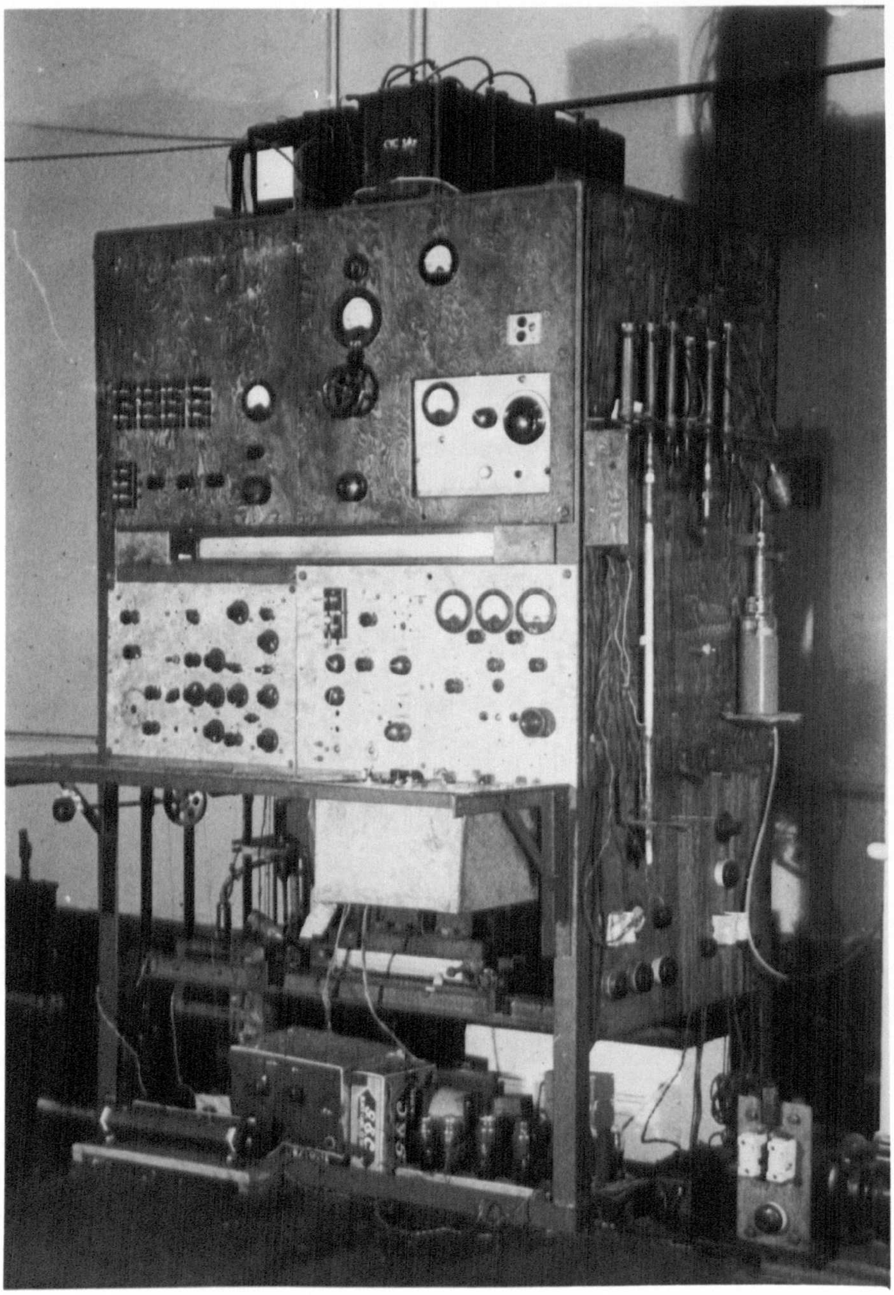
Finally, the last personnel requirement was,

> T.G. Pickavance - In charge of the above team, working on both machines. Part-time teaching.

A note is included at the end of this annexed section which is reproduced below:

> The arrangement of personnel is flexible. If at any time the work on the spectrometers does not require the attention of all members of the team, some of them may assist in the other work. This is especially important in the cases of Reynolds, Randle and Hughes, who should gain experience in all these fields. Unless there is a considerable change in his attitude, Deighton is unlikely to profit from such a scheme, and he should therefore be employed on routine work for the present.

The July and August 1944 Monthly Reports indicate that Pickavance was putting into practice the experience he had gained from his visit to the U.S.A. and that his main effort was directed to re-building the original mass spectrometer so that analysis of 'samples' could be progressed as quickly as possible. New pumping apparatus had been received, new methods of gas-handling devised and improvements to the electronic cicuitry undertaken. It is also noted in the August Report that the design details (for example, stabilising the H.T. set which would give 2.5 kV stable to one part in 15,000 against mains variations) of the 'new



Photograph 6.4.3
Mass Spectrometer Control Panels and Gas-handling System
c. 1943

instruments' had been 'largely settled' after several meetings with representatives of Met.-Vics.

Work continued on the mass spectrometers at Liverpool and the design of new machines at Met.-Vics. such that, by March 1945, the Report for that month stated that the spectrometer had operated satisfactorily for 30 working days with hex and that constructional work at Met.-Vics. had started. The ion source had now to be dismantled and cleaned as fluctuations were beginning to appear in the ion beam due to poisoning of the ion source.

General improvements and modifications were made to the machines over the next few months; for example, a comparative standard of '20 gm of normal hex' was fitted to the machine, and a series of hex mass spectrum curves were plotted against various collector slit widths to find the optimum slit width.

During September 1945, a sample of hex from Birmingham was analysed and a further six were analysed by November. At the end of 1945, the Reports state that work had been concentrated on a mass spectrometer leak detector, as no other samples for analysis had been received from Birmingham, and the efficiency of hydrogen and coal gas compared in leak detection.

The first mass spectrometer built by Met.-Vics. to the Liverpool design was installed in the Physics Laboratory at Liverpool during April 1946 and, until the Monthly Reports ceased in October 1946, they showed that further 'samples' were analysed and a machine was modified to look for low and medium mass measurements. Bromine, enriched in one isotope, was analysed according to the last Report of October 1946.

In correspondence with Reynolds, the author received not only written information concerning the Liverpool mass spectrometers but a number of photographs of the machines, one of which is shown opposite.

In a letter to the author dated 24th April 1997[59], Reynolds wrote,

> You are correct in stating that there were 2 mass spectrometers. The one depicted in the photograph is the Nier type built using a tube (60° deflection) supplied by Nier with the rest built at Liverpool using Nier's circuit diagrams. The second one was the MS1 a commercial version of the first but essentially the same design built by Metropolitan Vickers under the direction of Mr. Jack Blears of their research dept. working closely with us at Liverpool. It was a successful & happy partnership. I don't know what happened to this MS1, but several were produced with minor changes later by Met Vicks & were used at Harwell in the Mass Spec group in which I worked from 1949-54 and at Capenhurst.

Reynolds continued in the same letter,

> The original mass spectrometer lay idle for a few years but in 1946-7 I was able to revive it for Dr. J.D. Craggs (later Prof.) of the Electrical Engineering Dept at Liverpool for use in his research into gas discharge processes.

Section 4.6. The Cyclotron

In the period covered by the Monthly Reports, the cyclotron continued in service and information on, for example, the neutron energy spectrum from the Li(p,n) reaction, were studied in late May and during June 1942 but were noted in the July 1942 Report. The time lag is here noted from a correlation of work written up in the log books and then recorded in the Reports.

Accuracy in the range-energy relationship of protons in photographic emulsions was subject to continuous improvement until after the war and culminated in 1951, when El-Bedewi, at Liverpool, wrote to the Editor of the Physical Society[60], on 'The Range of Protons from the Reaction $^{14}N(n,p)^{14}C$ in Ilford C2 Photographic Emulsion'. He was also able to construct a channel consisting of a number of graphite bricks, which moderated the cyclotron neutron beam and made thermal neutrons available.

On the 2nd July 1942, Powell wrote in the cyclotron log book,

> Test run to determine, roughly, the ratio of background: target neutrons.

This ratio was found by the 6th July to be » 1/5th. The log book records show that during the 9th July the photographic

> plate soaked in 1 gm in 50cc Uranyl nitrate for ½ an hour, left for 17 hrs and developed. About 25 a particles / field of view

and Pickavance wrote on 13th July 1942 that

> During last few days there have been some experiments on uranium impregnated plates

The monthly report for July 1942 states under the section headed 'Cyclotron work':

> ... A trial irradiation was given to a photographic plate soaked in a uranyl nitrite (sic) solution (see 2).
> 2.<u>Photographic plate.</u>
> The photographic plate can be employed to investigate tracks produced by fission fragments and by fission neutrons. It is hoped that by this method one may decide whether the neutrons ... emitted at the instant of fission or later.
> Plates were soaked in various uranium solutions and the optimum conditions for the experiment were investigated.

Why was the timing of emitted neutron important? If neutrons were emitted over a relatively long period (milliseconds rather than microseconds) then, following on from the Frisch/Peierls memorandum, it would mean that two sub-critical masses of the lighter isotope of uranium when brought together would 'fizzle' rather than explode, and it was therefore important to confirm over what time neutron emission took place. 'Delayed' neutrons - those which are emitted in a group a number of milliseconds after fission takes place - are also important. (Delayed neutrons were later utilised as stabilising devices in nuclear reactor piles and elsewhere.)

A reference in the log book of Monday, 20th July 1942, after completing the experiment on the impregnated photographic plates, in Powell's hand, states,

> Working on deuterium generator. 10 cc taken from stock

showing that there must have been at that time an amount of heavy water held in the department, from which the deuterium was obtained.

The following day he wrote,

> Deuterium generator working - reservoir filled.
> Volts on - small proton beam obtained.
> No deuteron beam obtainable (using microammeter).
> N.B. deuterium apparatus now works perfectly - no sucking back, etc. - can be left running indefinitely with DC mains & 400 W in series, when it makes about 500 cc. per hour.

The team then began searching for a deuterium beam; it was finally found the following day after the deflector was adjusted and the tank outgassed. It wasn't until the Wednesday that Powell was able to write:

> (7) Deflector pushed in another ½ mm. Optimum at 31 on Variac. Running very nicely.

There were a number of difficulties optimising the deuterium beam. The progress report for September 1942 states under its first heading that:

> The cyclotron has been changed over for deuteron bombardment, and a source of radio-sodium has been prepared for a measurement of the fission cross-section of 235.
> The irradiation of heavy water by the (gamma)-rays of radiosodium provides a source of neutrons of energy 0.4 MeV, which, except for the effects of scattering in the heavy water, should be homogeneous. The effect of scattering can be estimated by using different amounts of heavy water.

The method used was to dissolve some of the irradiated radiosodium in heavy water, and the emitted gamma rays from the radiosodium plus a deuteron would give a proton plus a neutron of the above mentioned 0.4 MeV energy. The technique of 'the nuclear photo-effect' had been previously found by Chadwick and Goldhaber in 1934[61] while they were at Cambridge and it was one way in which mono-energetic neutrons could be obtained.

This same Progress Report gives values to the fission cross-section for U^{235} and for 'ordinary' uranium (assumed to be U^{238} with $0.7\%U^{235}$):

> Fission cross section of ordinary U is 1.5×10^{-26} cm^2, giving 2×10^{-24} cm^2 if the fission is due entirely to the light isotope. This is in good agreement with values previously reported.

If one now, according to information on page 482 of Kaplan's book *Nuclear Physics*, makes the assumption that

> If the absorption cross section of a nuclide is known at 0.025 ev (2200 m/sec) and there are no resonances in δ_t at low energies, it is usually safe to assume that δ_a follows the 1/v-law for absorption so that the absorption cross section can be calculated for other energy values.

By making a simple ratio calculation and using the normally accepted thermal energy cross-section for U^{235} of 580 barns (580 x 10^{-24} cm^2), it appears that the energy of the neutrons used were 3.36 MeV, which approximates to the cyclotron proton energy of 3.7 MeV. Unfortunately, in 1942 there were no standard text books with this available data and the measurements obtained had to be considered very carefully. Chadwick was unaware of the 'resonances' that occur in the cross-sections of U^{235} although he was very probably aware that they could occur. No one knew at that time the accuracy of Powell's range/energy expression in photographic emulsions. The energy and therefore velocity of neutrons was the important 'unknown' factor which Chadwick had recognised, as at about this time he had written to P.B. Moon at Birmingham to ask him to complete 'time of flight' measurements in order to obtain a more accurate estimation of neutron velocities. It was not until Fermi's first graphite moderated pile in Chicago was successfully tested (on 2nd December 1942) that a constant neutron beam, albeit with a Maxwellian energy distribution, was obtainable. The reactor pile led on to the neutron beam 'chopper' experiments of post-WW2 which allowed mono-energetic beams of neutrons to be produced.

On the 20th August (1942), the tank was 'outgassed', and the supply circuitry and magnetic field adjusted. In Holt's notebook is recorded between 24th and 29th August 1942,

> Helping Pickavance with Cyclotron. Deuteron beam is being built up by adjustment of deflector position (very critical), of filament position & by shimming. Irradiation with LiOH + H^2 neutrons of CCL_4 for radio sulphur (80 day).

At the end of August, on Sunday the 30th, Powell wrote, Beam of 1.3 μa - demonstration for Polish engineers.

Frisch has written in *What Little I Remember*, pages138-9, in his chapter titled "Liverpool 1940-1943",

> That I had a private life outside the laboratory I largely owed to Joseph Rotblat. ... I remember on one occasion when he arranged for me to play the piano to a group of Polish soldiers stationed in Liverpool. When I got there I found a classroom with something like a hundred people crammed into it, and an old upright piano with half a dozen keys not working.

This author surmises that the Polish engineers who saw the 37" cyclotron could have been part of the group that Frisch entertained.

During September of 1942 the cyclotron was re-adjusted for use with protons, for further experiments on fission problems and also a new filament assembly was constructed. It was hoped that this, with other adjustments, would lead to a considerable increase in the proton beam. These adjustments were obviously successful and a substantial increase in the proton beam was obtained. The increased ion beam was mentioned in the Monthly Reports for December 1942 as being 40-50 μa. Allowing for the time lag of about four weeks, the log book shows that the shims adjusting the magnetic field and the tuning of the oscillator feed to the dees, together with the improved filament, allowed the generation of a proton beam current of 53 μa. This was an occasion of celebration as Chadwick had previously agreed to supply a bottle of champagne when the cyclotroneers were able to produce a beam of 50 μa or more. According to the log book, this momentous event was achieved on Wednesday, 11th November 1942, and Rowlands has written,

> Under the conditions of shims etc. the RF was brought up to -8, the emission to 200 μa and the Hydrogen leak voltage 132½ & the maximum beam was 53 μa.
> Witnessed - TGP J.R. (Pickavance's and Rotblat's initials!)
> With reasonable conditions it seems possible to run continuously at over 40 μa.

Moderation of the neutron beam was effected normally by the use of paraffin blocks but war-time conditions of stringency meant that sufficient paraffin in solid form was not readily available. The author understands, in conversation with Holt, that a donation was made to the Department of a number of cases of whale fat which could be used in place of the paraffin blocks. Unfortunately, the whale fat had a lower melting point than the paraffin and when inadvertently it was placed too near the room heating radiator, Holt recalled the disgusting smell and mess that had to be cleaned up. He also recalled that Mrs. Chadwick even attempted to use whale fat in her cooking but found it rancid and unacceptable!

With the increased cyclotron proton beam, the energy spectrum of the fission neutrons (where the fission was caused by a thermal neutron) enabled photographic plates which were exposed to the beam to be measured much more satisfactorily. They were examined both at Liverpool and at Bristol. The Monthly Reports during the early part of 1943 showed that photographic plate recordings were made of the fission neutrons from 235 due to fast neutrons - but with energies below the threshold of 238. Experiments were started to try to modulate the proton beam whereby it was hoped to investigate any delayed neutron emission. The way this was first attempted was to apply a modulated accelerating voltage to the filament ion source. A 'cathode ray oscilloscope' recording was then used, showing that with long pulses the modulated proton beam followed that of the input filament voltage. 'Difficulty was found', however, when pulses of a few microseconds were applied because of the time taken for the ions to spiral out to the deflector. The team were able to measure the time lag between filament pulse and beam appearance.

N.B.The resonant high frequency applied to the dees was approximately 10 MHz and the time, therefore, for one revolution of an ion was approximately one tenth of a microsecond.

Experiments were also started to modulate the neutron counter amplifier so that it went on only when the ion beam was off. In order to test the performance of the modulation apparatus, the life-time of thermal neutrons in paraffin was examined and found to be 260 microseconds. This figure for mean life-time, without correction for diffusion, was in fair agreement with their expected value and showed that the modulation apparatus was operating satisfactorily.

During the summer of 1943 the cyclotron team attempted to modulate the actual beam instead of the ion source. Consultations took place between electronics staff at T.R.E. Malvern and also Titterton of Birmingham, with regard to the design of a suitable beam modulator. While the modulator was under consideration, a simpler deflector system for beam modulation was constructed and assembled in the target chamber, and a general overhaul of the cyclotron and its circuitry was undertaken. The beam deflector system for the modulator included a rotary spark gap and a power supply for it, and this arrived from Birmingham University during August 1943. By October, building of a 1,000 cycle rotary spark gap modulator had been completed and tested on the cyclotron, and modulation of the proton beam seemed satisfactory. The Monthly Reports then indicated that it was hoped to start the experiments on fission recoils to determine the short delays in the emission of fission neutrons. A design of a hard valve modulator, from 1-100 kHz and with various pulse widths, had been completed and construction had started at T.R.E. Malvern.

By December 1943, most of the interference problems caused by the rotary spark modulator in the various electronic circuits had been eliminated. Counting the pulses, from background neutrons emitted from various parts of the cyclotron in the half cycle in which the beam was deflected from the target, were reduced to 0.05% of the total number of pulses counted in the absence of modulation. This background, scattered from various parts of the cyclotron, was found to be caused, according to the Monthly Reports, due to the reaction Cu^{65}(p,n) which was found to take place with a proton beam of only 2.5 MeV. After all of the target chamber was covered with aluminium (except for the lithium target), the background was reduced by a further factor of three. It is noted that the dees were manufactured mostly from copper.

The first three months of 1944 gave the cyclotron team the time to set up the conditions necessary to prepare the cyclotron for experiments concerning the detection of delayed neutrons. Holt and Rowlands, with occasional comments from Rotblat, according to the handwriting evidence in the log book and Holt's laboratory notebook, started these experiments by determining the thermal neutron flux with various arrangements of paraffin wax and whale fat arranged around the lithium target. Later, a uranium target was substituted for the lithium and numerous dimensioned sketches are depicted in the log book starting from the end of November 1942, showing some of the arrangement and positions of both lithium and uranium targets.

The rather unpleasant task of arranging blocks of paraffin wax or whale fat were not commented upon in the log book other than in the occasional remark, as on the 14th December 1942, in this case by Holt,

> Carve out hole in whale fat block so that U can be turned round to have the axis in the direction of the counter.

These preliminary tests were completed before the arrival from Malvern of the 100 kHz beam modulator in early January 1944. This modulator was assembled in Liverpool and tested away from the cyclotron. It was found on test to need modification as the pulse transformers were not suitable for use with the longer pulses at the lower end of the frequency range. This was probably due to unacceptably long time constants of the transformers which would seriously affect the shape of the square wave output of the unit.

The final installation of the modulator on the cyclotron had to wait until measurements of fission neutrons from a target of 'thick' uranium had been completed, by which time a modified drive unit that eliminated the requirement of pulse transformers had been constructed. The experiment on a thick layer of uranium was necessary so that the measurements that could be expected from a suitable thin layer of uranium could be found.

The Monthly Report for March 1944, showed that tests were completed on the thick layer of uranium to detect neutrons from the fission process. They indicated that the quantity to be measured would be 0.3 impulses per minute from the present sample of enriched uranium which was stated as 3.5 milligrams of uranium enriched to contain 15% U^{235}. This impulse rate was also stated to be smaller than the background counting rate and that the experiment would be postponed until such time as a larger sample could be obtained. The May Report section on the cyclotron shows that,

> The apparatus to detect delayed emission of neutrons from the fission process has been reassembled in preparation for the completion of this experiment, following the acquisition of a 100 milligram enriched sample of uranium.

It is recorded in Brown's book[62] that Chadwick had, in the middle of April 1943,

> ... not completely forgotton the Liverpool laboratory, and in the middle of April had formally asked Groves for 100 mg of enriched uranium to be sent there. He justified his request by saying the Liverpool workers would provide "independent confirmation of work being done at Y [Los Alamos], so that we can feel more confident of some fundamental constants."

Holt wrote in the cyclotron notebook during May 1944,

> May 15-30
> Cyclotron in operation for a few days; beam ≈ 5 μa.
> 100 mgm. separated sample (13.5% 235) has arrived.

In June 1944 is stated, without further explanation, in the Monthly Reports that,

> The experiment to detect delayed fission neutrons has been discontinued.

The September 1944 Monthly Report states under the section heading, Cyclotron,

> In accordance with the scheme to concentrate the nuclear work in Cambridge for the time being, work on the Liverpool cyclotron has been suspended. In the mean time, the cyclotron is being kept in working order by the remaining members of the T.A. team.
> (N.B. It is noted that T.A. is the abbreviation for the U.K. Tube Alloys Atomic Bomb Project.)

A last dated entry in cyclotron log book number 2, of 'Sept. '44' shows,

> Prof's. visit. Cyclotron work suspended in Liverpool. People transferred to Cambridge. (Holt, Allen, Livesey, Deighton). Hughes to U.S.A.

The author will consider the implications of the above statements later in this chapter.

The course of this chapter has shown how Chadwick was willingly drawn into the research and then the politics of the atomic bomb project in England. Building on his thoughts on the feasibility of an atomic bomb, he devised a series of experiments within the capabilities of existing apparatus and state of knowledge at not only Liverpool University but Bristol, Birmingham, London and Cambridge. He gave responsibility for the running of the various experiments necessary to produce the results he required to others but he led the teams involved. As Holt has said[63] concerning Chadwick;

> ... he was often to be found at tea time when we would all sit around the long table in the small library outside his office on the first floor. This was a time for general discussion of many topics, scientific or otherwise; but it was not a time for loose talk.

Holt continued in the same lecture,

> The role of leader in the convential sense did not come naturally to him. He organised the Department as he wanted it to be, assigned people their roles, and then left them to play their part without interference. If there were serious problems or difficulties, of course he was there to advise and help, but one was made to feel responsible for one's own particular job, and such was the natural respect that one felt for Chadwick that one strove to do well and to earn his approval at the end.

There was little choice but for Chadwick to do this as his presence was required in many places at the same time! That he attracted the calibre of research worker to Liverpool was because of his ability to recognise that talent, and time showed the correctness of his choices. He was able from 1940 to 1943 to lead at Liverpool University and elsewhere the various lines of research that have been discussed in the foregoing sections in this chapter. His other University involvements included 'normal' professorial duties such as being an internal examiner at Liverpool, interviewing students, lecturing to undergraduates, external examiner in Sheffield and Leeds (from June 1940) and his active involvement on the Faculty of Medicine and Faculty of Science Committees. He had also to arrange his time-table schedule for the increasing participation on work for the Maud Committee. He made time to meet representatives of commercial organisations and led the way for his staff to take over those responsibilities.

Examples of some of Chadwick's work load in the three years which built up to the U.K. participation in the Manhattan Project are given below.

In May 1941, Chadwick wrote a report to G.P. Thomson on the experimental work of Halban and Kowarski in the Cavendish Laboratory, showing their claim that the fission chain reaction was potentially divergent with their set-up of ordinary uranium (and moderated by heavy water), with slow neutrons, was justified. It was on the strength of this report that the Americans commenced building their heavy water manufacturing plant.

In July 1941, Chadwick re-wrote a resumé of the work of the Maud Committee. A number of copies of this report were sent to the United States, one of which went to Lyman Briggs, but he locked it in his safe and did not show it to the American Uranium Committee. Another copy was sent to Vannever Bush, the ex-head of the National Defence Research Committee (N.D.R.C.) and the Director of the Office of Scientific Research and Development (O.S.R.D.).

During the late summer of 1941, Chadwick was able to get *Movement Restriction Orders* lifted from Rotblat and Frisch.

Chadwick was also asked in late 1941 about the possible, and probable, involvement of German scientists in atomic bomb work and, on the strength of this information, secret agents on the Continent were given the task of locating the list of scientists that Chadwick had provided, to find the extent of their atomic bomb research. By early January 1942, Brigadier S.G. Menzies, the Head of the Secret Intelligence Service, wrote to Lord Hankey suggesting that there was strong circumstantial evidence of atomic bomb work being carried out by those German scientists that Chadwick had named.

The Scientific Advisory Committee, chaired by Lord Hankey, interviewed Chadwick over the 'efficiency' of an atomic bomb and Chadwick was able to answer questions on neutron radiation and radioactive contamination that would be found after an atomic explosion.

After initially appearing reluctant to suggest full cooperation with the U.S.A. on atomic matters, Chadwick changed his mind and told the S.A.C. that the Maud Committee was in favour of American involvement.

The S.A.C. produced its report on the Maud Committee at the end of September 1941 and attached the highest priority to the bomb manufacture. The full summary of the Maud Report produced by the S.A.C. is given in Appendix 7. The Maud Committee had completed its work and was now disbanded. Shortly after the S.A.C. Summary Report was completed, Wallace Akers, Research Director of I.C.I. Ltd., was asked to head the group of scientists that had comprised the Maud Committee. Akers and Lord Hankey named this new organisation 'Tube Alloys' and set up a Technical Committee of Tube Alloys to which Chadwick was invited as a member, together with Halban, Peierls, Simon and Dr. Slade, an I.C.I. Ltd. scientist.

By the end of October 1941, on the strength of the Maud Committee's findings, Pegram and Urey from the U.S.A. visited Liverpool to meet Chadwick. On their return to the States, they wrote a report which confirmed the positive tone of the Maud Committee's Report that had been sent to Bush. Bush sent their report to President Roosevelt on the 27th November 1941.

On the 7th December 1941, the Japanese bombed Pearl Harbour and America entered the war. This event galvanised the American research into action and, from this time, the industrial and scientific might of the U.S.A. concentrated on the production of atomic weapons.

Early in 1942, Akers led a team consisting of Simon, Halban and Peierls to the U.S.A. as official members of the Technical Committee of Tube Alloys, but Chadwick chose not to go. The team were given full access to all parts and aspects of the American atomic bomb effort. A short time after their visit, Bush, realising the enormous work necessary to commence the manufacture of a bomb, approached the

U.S. Army to ask them to provide the necessary engineering support. In June 1942, the U.S. Army became responsible for all aspects of the atomic energy programme and the Manhattan Engineering District Project came into being.

The bombing of U.K. cities and industrial complexes made John Anderson realise that a production plant within the U.K. was out of the question as, apart from its size, there were insufficient suitably qualified people available to run it. Anderson sent a memo to Churchill to suggest moving all the work to the U.S.A. Meanwhile, in the U.S.A. in the last week of September, Bush met with the newly-promoted Brigadier General Leslie R. Groves (U.S. Army Corps of Engineers) in Secretary Stimson's office at the War Department, the others present being General George Marshall (Army Chief of Staff), two other senior Generals, Conant and Harvey Bundy (Stimson's special assistant). They made a decision that there should be no collaboration with Britain until Stimson had conferred with the President. The U.K's original lead in information on atomic bomb matters was rapidly eroded due to the advancement of knowledge in the U.S.A., and the chance for the British to have contributed to that knowledge had now gone. The chance for full partnership with the U.S.A. was over and it was now the turn of the British to attempt to repair a damaged relationship and to join in the Manhattan Project in the States. In order to participate in a U.K. bomb project, Akers and Perrin reviewed the cost involved to construct suitable U^{235} separation plants, etc., in the U.K. and the review revealed that it would be impossible to do this within the U.K.'s war-time budget. It was imperative to join forces with the Americans to get the work done there. In May 1942, Churchill and Roosevelt met in an abortive attempt to reconcile the two countries over the exchange of information in the atomic area. During the summer of 1942, Bush and Stimson went to London and Churchill attempted to restore the American/U.K. relationship. This took almost a year and collaboration was nearly non-existent during this time.

Churchill's diplomacy was eventually successful as, on the 19th August 1943, Churchill and Roosevelt signed what became known as the Quebec Agreement, the document titled, Articles of Agreement Governing Collaboration Between the Authorities of the U.S.A. and the U.K. in the matter Tube Alloys

Chadwick, Simon, Oliphant and Peierls were summoned by telegram to arrive in North America on that date after aides had signalled that agreement between the two countries was imminent.

Chadwick and the U.K. team attended on the 8th September 1943 a combined policy committee meeting held in the Pentagon, where it was agreed that a tripartite (American, Canadian and British) technical sub-committee be established with General Styer as Chairman, Chadwick to be the British member, and C.D. Howe to represent Canada, (Howe was nominated by Mackenzie King, the Canadian Prime Minister). Colonel J.J. Llewellin, the Minister in Washington, decided that, as the Americans had objected to Akers attending these highly secret and sensitive meetings - mainly due to his commercial interests in I.C.I. Ltd. - Chadwick should take Akers's place. This decision put Chadwick at the head of the British mission to the States.

On Monday, 13th September 1943, Chadwick, with Oliphant, first met Groves and his scientific adviser Richard C. Tolman at the Pentagon, where the British learned of the Los Alamos site (site "Y") with Oppenheimer as its Director. They also learned of the security arrangements that Groves required in the Manhattan Project. At this meeting with Groves, three working parties were set up. The first was a Fast Neutron Physics Group; secondly, the Diffusion Project Group; and thirdly, the Plutonium Production Group. It was at a further meeting with Groves in his office that Lawrence, with Chadwick's and Oliphant's support, recommended that between three and ten bombs should be made and tested before the design of a weapon could be established. At the end of September, Chadwick returned to Liverpool.

In October, Bohr, and one week later his son Aage, came to England after flying to Scotland. Chadwick and his wife met them in London and took them both to Liverpool. On the 15th October, Anderson in a letter to Churchill informed him that Chadwick would continue to act as the Scientific Adviser to the British members of the Combined Policy Committee, and at the end of November Chadwick left for the States after Frisch had been made a British citizen; and Rotblat, who wished to remain a Polish citizen, was kept in charge of the Tube Alloy work in Liverpool.

When Chadwick returned to the U.K. in September 1943, he had two months to reorganise the work in Britain in which he had been involved, including the university and academic aspects, as well as the research programmes that he had led.

The foregoing discussions on the work taking place at Liverpool has shown that Chadwick was not often actually with his teams of researchers on a daily basis. He was available for consultation and advice while at his office in the Department, but was frequently absent from the University attending meetings. The extensive correspondence, held in the Churchill Archive Centre at Cambridge, with letters from 1940 onwards, shows his enormous work load dealing with many aspects of the work in progress in Liverpool, Bristol, Birmingham and other universities.

The next chapters will outline the work that Chadwick did in the new, more diplomatic, role as the Head of the British Mission in the States over the next two years, culminating in the successful completion of the atomic bomb project.

References:

4.1. Gowing, M., *Britain and Atomic Energy, 1939 - 1945*, London, Macmillan & Co. Ltd., 1964.

4.2. Noddack, I., *Angew. Chem.* **47**, p. 653, 1934.

4.3. *ibid.* 4.1., p. 26.

4.4. *ibid.* 4.1., p. 28.

4.5. Bohr, N., and Wheeler, J.A., *Phys. Rev.*, **56**, p. 426, 1939.

4.6. A 'cross-section' is an area. The atomic cross sections are approximately 10^{-16} sq.cm. and are of the order of the square of the atomic radius (10^{-8} sq.cm.) but can vary considerably depending on the type of reaction and the energy of the interacting particles.

Nuclear cross sections, approximately 10^{-24} sq.cm., are similarly of the order of the square of the nuclear radius (10^{-12}sq.cm.) and also vary considerably with the type of reaction and the energy of the interacting particles.

In Jerrard and McNeil's book, *A Dictionary of Scientific Units including Dimensionless Numbers and Scales*, (2nd ed.), London: Chapman & Hall, 1964, p. 35, is the following proposed unit,

> **Chad**
> The chad is the name proposed in 1960 to represent a neutron flux of one neutron per square centimetre per second. The name is derived from that of Sir James Chadwick (1891 - --), a pioneer in nuclear physics. Some people consider that if the chad be taken as 10^{12} neutrons per square centimetre per second, more convenient numbers would be obtained for the neutron fluxes usually met with in practice. This would make the flux of a power reactor, which is of the order of 10^{12} neutron cm^{-2} $sec.^{-1}$, equal to one chad, and that of a sub critical assembly (10^{6} neutrons cm^{-2} $sec.^{-1}$) equivalent to one micro-chad.

The unit of area normally used in atomic or nuclear physics is called the barn (b) and is defined as 10^{-24} sq.cm.*

The total cross-section is a measure of the probability that an incident neutron will interact with the nucleus. The probability of each of the separate events, of elastic and inelastic scattering, radiative capture with gamma emission, fission, etc., can be represented by 'partial cross-sections', with the sum of the partial cross-sections being the probability of the event as a whole.

It can be shown that in a gas or solid, the average kinetic energy for each of the three degrees of freedom carried by a molecule is

Kinetic Energy = ½ kT where k = Boltzman's constant
= 1.38×10^{-23} Joules per^{0}Kelvin
and let T = 293°Kelvin (room temperature)

Now,

$$1 \text{ electron volt (eV)} = 1.6 \times 10^{-19} \text{ Joules}$$

and as,

$$\tfrac{1}{2} mv^2 = 3 \times \tfrac{1}{2} kT$$ where m and v are mass and velocity,

it can be seen that at room temperature, a neutron (i.e. a 'thermal' neutron), has an energy of about 1/40th ev, with a velocity of approximately 2,200 metres per second.

* According to Glasstone, *Sourcebook on Atomic Energy*, p. 371:-

> The term "barn" was proposed in 1942 by the American physicists M.G. Holloway and C.P. Baker, as the result of a broadly humourous association of ideas. It served the purpose of a code word, which was desirable at the time, and seemed appropriate because "a cross section of 10^{-24} sq.cm. for nuclear processes was really as big as a barn" (Los Alamos Report, LAMS 523).

4.7. von Halban, H., Joliot, F., Kowarski, L., Perrin, F., *Phys. Radium* **10**, p. 428, 1939 .

4.8. Turner, L.A., *Rev. Mod. Phys.* **12**, p. 1, 1940.

4.9. *ibid.* 4.1., p. 36.

4.10. Chadwick Archive, File 2, Document 75, University of Liverpool.

4.11. *ibid.* 4.5.

4.12. Tape recorded interview, Chadwick and Charles Weiner, 15th-21st April 1969, Neils Bohr Library, Center for History of Physics, American Institute of Physics, Page Chadwick-102.

4.13. *Biographical Memoirs of Fellows of the Royal Society*, 1971, Volume 17, C.F. Powell, p. 546.

4.14. *Biographical Memoirs of Fellows of the Royal Society*, 1994, T.G. Pickavance, p. 308.

4.15. *ibid.* 4.12.

4.16. *ibid.* 4.12, Page Chadwick-103.

4.17. *Private Communication*, S. Rowlands to Author, dated 25th February 1995.

4.18. Rotblat, J., Brown, A.P., *Interview*, 7th January 1994, London, transcribed by Brown.

4.19. Edwards, D.N., *Liverpool Who's Who in Physics (1881-1996).*

4.20. *Private Communication*, S. Rowlands to Author, dated 18th January 1995.

4.21. Frisch, O.R., *What Little I Remember*, Canto Edition, 1991, Cambridge University Press, 1979, p. 124.

4.22. *ibid.* 4.21, p. 126.

4.23. McMillan, E. and Abelson, P.H., Letter, 27th May 1940, *Physical Review*, **57**, pps. 1185-6, 1940.

4.24. *ibid.* 4.1, p. 60_n.

4.25. *ibid.* 4.21, p. 129.

4.26. *ibid.* 4.1, p. 45.

4.27. *ibid.* 4.21, p. 131.

4.28. *ibid.* 4.1, p. 45.

4.29. *ibid.* 4.12, page Chadwick-103.

4.30. *ibid.* 4.12, page Chadwick-104.

4.31. *ibid.* 4.12, page Chadwick-104.

4.32. Allen, W.D., *Neutron Detection*, London, George Newnes Ltd., 1960, p. 5.

4.33. *Private Communication*, J.R. Holt to Author, u.d.

4.34. J.R. Holt, *Biographical Memoirs of Fellows of the Royal Society*, Thomas Gerald Pickavance, pps. 305-323, 1993 (particularly p. 308).

4.35. *ibid.* 4.34, p. 309.

4.36. *ibid.* 4.34, p. 308.

4.37. *ibid.* 4.12, page Chadwick-105.

4.38. *ibid.* 4.1, p. 76.

4.39. *ibid.* 4.1, p. 76.

4.40. *ibid.* 4.12, page Chadwick-106.

4.41. Rotblat, Pickavance, Rowlands, Holt and Chadwick, 'The Energy Spectrum of the Fission Neutrons produced in Uranium by Thermal Neutrons', *Atomic Energy Report; BR-47*, 1942.

4.42. Holt, J.R., 'Sir James Chadwick at Liverpool; Personal Reminiscences of the Nobel Laureate with Archive Film', *History of Science and Technology Seminar*, 8th February 1988, University of Liverpool.

4.43. An incomplete set of typewritten monthly reports covering the period July 1942 to June 1947. The 'originals' are held in the Churchill College Archive Centre, Cambridge, and a copy is held in the Chadwick Archive, University of Liverpool, with permission from the Master and Fellows of Churchill College, Cambridge.

4.44. J.R. Holt's laboratory notebooks:

Book No. 1. Miscellaneous Notebook, 1939-51
2. Ph.D notes, etc.
3. Experiments in Radioactivity, 1940
4. Experiments with Linear Amplifier and Cyclotron, 1941
5. Notebook No. 2, 1941-42
6. Notebook No. 3, 1944-45
7. Notebook No. 4, 1945-50

4.45. *ibid.* 4.12, page Chadwick-102.

4.46. Allen, W.D., *Neutron Detection*, London, George Newnes Ltd. (1960), pps. 51-53.

4.47. Frisch, O.R., *What Little I Remember*, Cambridge, Cambridge University Press (Canto Edition, 1991), p. 144.

4.48. *Letter*, Sayle to Allan, 24th August 1997, Chadwick Archive, University of Liverpool.

4.49. Szasz, F.M., *British Scientists and the Manhattan Project. The Los Alamos Years*, New York, St. Martin's Press, 1992, Appendix iii, pps. 148-9.

4.50. *ibid.* 4.1, p. 228.

4.51. Otto Robert Frisch, *Biographical Memoirs of Fellows of the Royal Society* by Sir Rudolph Peierls, Volume 27, 1981, p. 283.

4.52. *Letter*, Chadwick to Sayle, 11th May 1942, Chadwick Archive, University of Liverpool.

4.53. Hoddeson et al, *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945* (Cambridge University Press, 1993), p. 264.

4.54. Frisch, O.R., 'Isotope Analysis of Uranium Samples by means of their α-Ray Groups', u.d., BR-49.

4.55. Aston, F.W., 'A New Mass Spectrograph and the Whole Number Rule', P*roceedings of the Royal Society (London)*, **A115**, (1927), p. 487.

4.56. Nier, A.O., 'Mass Spectrograph Studies of the Isotopes of Various Elements', *Physics Review*, **50**, 1936, p. 1041; and **52**, 1937, p. 933.

4.57. *ibid*, 4.53, p. 28.

4.58. Pickavance, T.G., 'Report on the Mass-Spectrometer Method of Determination of the Isotopic Ratio of X in XF6', *Atomic Energy Report*, BR-46, 14th June 1944; annexed to BR-46, 'Requirements for the Liverpool Spectrometers' dated 16th June 1944, and 'American Equipment Required for the Liverpool Mass-Spectrometer' dated 22nd June 1944.

4.59. *Private Communication*, Reynolds to Author, 24th April 1997.

4.60. El-Bedewi, F.A., 'The Range of Protons from the Reaction $^{14}N(n,p)^{14}C$ in Ilford C2 Photographic Emulsion', *Proceedings of the Physical Society*, **64** A, 1951.

4.61. Chadwick, J., Goldhaber, M., 'A "Nuclear Photo-Effect": disintegration of the diplon by gamma-rays', *Nature*, London, **134**, 1934, pps. 237-8.

4.62. Brown, A.P., *The Neutron and the Bomb: A Biography of Sir James Chadwick*, Oxford, Oxford University Press, 1997, p. 267; Brown, (his reference number 22, p. 277), quotes a letter from Chadwick to Groves dated 15th April 1944 containing the request.

4.63. *ibid.* 4.42.

Photograph 7.5.1
Professor Chadwick (left) and Major General Groves
(re-printed from Plate 8, *Britain and Atomic Energy, 1939-1945*, Margaret Gowing, 1964)
1945

CHAPTER 5
The Manhattan Project

In this chapter, the author will show that during the course of the Manhattan Project, Chadwick's career changed from physicist to diplomat and that he single-mindedly put his country and the successful completion of the Manhattan Project above all else. Within the two and a half years of his stay in the U.S.A. he led the British Mission to the U.S.A. and Canada, and was instrumental in obtaining and operating a good working relationship with Groves, the U.S. Engineer and effective leader of the Project. This cooperation with Groves was crucial and ensured that Britain, although in 1943 very much the junior partner in the Manhattan Project, was able to increasingly participate in the majority of its various aspects. See photograph opposite.

Chadwick realised that if cooperation and friendly relations with Groves and the U.S.A. were to fail, then not only would Britain be an even weaker participant in the atomic bomb work but would not have the expertise to become an Atomic Power as quickly as it might be possible after hostilities ended. Chadwick's diplomatic skills would be fully tested in this period. His analytical abilities, possibly gained from his scientific background, would enable him to see the results of actions that were sometimes contrary to the smooth running of the Project.

Chadwick helped to produce policies which laid the structure to regenerate the nuclear research programmes in post WW2 Britain. He was able to attract sufficient money and high calibre researchers to enable the Physics Department at Liverpool University to become a leading Nuclear Laboratory in Europe.

One major aspect of Chadwick's role in the U.S.A. was to act as security adviser to Groves on the integrity of the British contingent. This author will discuss some aspects of his security role in this chapter but it will be developed further in the following chapter. Security was one of Groves's continuous and contentious war-time problems, and from the start of Chadwick's time in the U.S.A. in 1943, he would receive and then pass on to Groves the security clearance information necessary for the British scientists to work on the secret Manhattan Project. Chadwick received the security information directly from the British Intelligence Service.

This author will also discuss the German and Japanese progress in nuclear bomb research that occurred during WW2. In particular, it will be shown that in the case of the Germans, they did not appreciate that it was practical or possible to make an atomic weapon and did not start the necessary research. They were, however, investigating a 'boiler' in 1945 in order to obtain electrical power. The Japanese, on the other hand, this author will show, were experimenting with the production of uranium 235 and had measured the necessary cross-sections on one or more of their three cyclotrons. When the Japanese surrendered, they were at about the same stage in nuclear bomb development as the Allies were in 1943. They did not however have access to the necessary resources.

In the Weiner interview of 1969[1], Chadwick stated that,

> One thing that pleased me very much was not merely the discovery of the neutron essentially, but the feeling then that the work that had been put in for years was paying off. .... Another thing was, I suppose, making the physics department at Liverpool into a really active place, as it was becoming when I left it, and it became even more so afterwards. It was I suppose at that time the best equipped department in Europe in nuclear physics. I still feel a certain amount of satisfaction. I've only got to think at this moment there are four professors of physics in Liverpool as against one in my time, and all four are Fellows of the Royal Society.

Chadwick has said[2],

> ... I practically left physics during the war; that is, after the Maud report, there was very little more that I could do. We had done all the measurements.

In reply to further questions from Weiner concerning his stay in the U.S.A., Chadwick continued in their interview by saying,

> When I got over there, it so happened that the matter of arranging cooperation fell upon me. I was supposed to go to Los Alamos, and a little house was arranged for me and my wife. But when I got there I found that really there was very little for me to do, although there were very large numbers of people very busy. Practically all the physical factors that were important had been measured. They were not only

known but had been measured. I was wrong, but I felt I was much more needed in Washington to keep in touch with our people there and to see what was happening in different places.

The cooperation between the U.S.A. and the U.K., as outlined in the Quebec Agreement, was initially not viewed favourably by Groves, as Chadwick recalled in his interview with Weiner. He said, in fact, that initially Groves only wanted two people and that he was not really interested in anyone else.

> He wanted Mark Oliphant and me over there. But, of course, that was not cooperation. He gradually found that some of our people were useful and he would ask me to get other people over to help ... And, of course, we had some people in Los Alamos doing various things -- one or two mainly working on the electronic side of the equipment necessary for the bomb and some on the explosive side. That was how Bill Penney came over. He was not originally connected with the Maud Committee in any way[3].

Chadwick continued in the interview that he thought his function was merely to help in any way he could by supplying people and discussing things with Groves.

These rather self-deprecating statements by Chadwick give examples of his inate modesty and are at variance with the facts. He was an eminent scientist, a Nobel Laureate in Physics gained by his discovery of the neutron, and a Fellow of the Royal Society. He had moved in high scientific circles and, more recently, had held office in prestigious committees. His scientific acumen was trusted by all who had dealings with him. These were among the reasons why he was appointed to lead the British Mission to the U.S.A. in the Atomic Bomb Project.

During this initial period, Chadwick also had the task of directing the research work that was taking place at Liverpool University and at the other involved universities in the U.K. In the Physics Department at Liverpool, for example, Roberts was left in charge of the teaching side and Rotblat looked after the research. The situation, however, was fluid and Rotblat, who retained his Polish citizenship, was asked by Chadwick to join the Manhattan Project in early 1944. The research side should have then been looked after by Kinsey, who had returned to Cambridge in 1942 from his radar work to continue work on their cyclotron.

According to Gowing[4], the teams Chadwick brought over from the British Isles that he and Groves thought necessary for the expediency of the Project consisted in total of 140 people. These people were among the foremost engineers, chemists and physicists in the U.K., and are listed in appendix 8. Many of them went on to distinguished post-war careers. The calibre of the personnel may be judged by the positions given to the British participants. For example, Cockcroft was asked, initially by Chadwick, to be the Director of the Canadian Research Establishments with effect from the middle of April 1944, and, as Szasz has said[5], concerning the 19 British at Los Alamos,

> The overall calibre of the British Mission to Los Alamos may be estimated by the fact that six men - Frisch, Peierls, Bretscher, Moon and Placzek - became heads of Los Alamos groups. Gregory Marley headed a section. The Mission included seven experimental physicists, two electronics experts, and five specialists on explosives. It was quite a team.

Chadwick was the leader of these men, and Peierls, for example, sent Chadwick a series of reports on the work he and his team were able to do there. The summaries that Peierls produced were so good and Chadwick therefore so well informed that Groves asked Chadwick to send him copies! Brown in his Chadwick Biography[6] states, concerning Chadwick, that,

> He also asked Peierls to keep him abreast of scientific progress, which Peierls did by writing a series of detailed letters, sometimes in conjunction with Moon. ... The letters continued through May and June (1945) and apart from including reports on contributions made by British scientists, they also gave details on the new methods available for analysing implosions (the technique developed to detonate the plutonium bomb). As a result Chadwick was conspicuously well-informed of all developments - to such an extent that Groves started to feel inadequately briefed by comparison.

The problems that Chadwick had to grapple with ranged from housing and accommodation difficulties of the incoming British people and their salary adjustments, to the assimilation of the scientific reports from each of the Canadian and American Establishments. He had regular scientific and policy meetings with Groves and politicians and also visited the various research laboratories as often as he was able. He kept the British politicians abreast of developments and advised them on policies concerning the harmonious relationship with the U.S.A.

Chadwick's priority when he first went to the U.S.A. was to gain an overview of the whole Manhattan Project. He therefore spent the first few weeks of his stay, during November and December 1943, in criss-crossing the U.S.A. to visit all the laboratories, but with one exception. Groves would not let Chadwick tour the plutonium production plant at the Hanford Engineering Works in Washington State; Groves thought access to this plant was not necessary to the successful completion of the Project, and he did not want the British to have this knowledge.

During December 1943, with Groves's approval, Chadwick wrote to Anderson (the recently appointed Chancellor of the Exchequer), to arrange the transfer of more Tube Alloys U.K. research staff to the U.S.A.

Halban, the rather volatile Frenchman, who with Kowarski had brought the heavy water to the U.K. from France, had been appointed to the Directorship of the Canadian heavy water programme at the end of 1942. Due to his weak heart and lack of management skills, he was replaced by Cockcroft in April 1944, who was able to push the production of heavy water plant, and eventually the research, into plutonium production, along in a more structured and agreeable manner. Chadwick was pleased that Cockcroft had accepted his invitation to take the Directorship of the Canadian plants and Groves was satisfied with this arrangement as it meant that the French were removed further from positions of authority and knowledge of the Project.

After Chadwick completed his review of the Project, Aileen Chadwick and he took up residençe in the Los Alamos Ranch School site early in 1944, but not before they had made a detour to Halifax to see their daughters. It is stated by both Brown[7] and Szasz[8] that Mrs. Chadwick did not settle down at Los Alamos but there is no strong evidence for this. Her reserved English upbringing may have given that impression to the more outgoing of the British contingent, but her warm nature and the kindnesses shown to those who knew her belie this impression.

As part of the security arrangements for senior scientists working in or connected with the Manhattan Project at Los Alamos, they were each given code names. According to Hoddeson et al, in *Critical Assembly*[9], the code names for some of these scientists were as listed below,

Hans Bethe	became known as	Howard Battle
Aage Bohr		James Baker
Neils Bohr		Nicolas Baker
JAMES CHADWICK		JAMES CHAFFEE
J.B. Conant		Uncle Jim
Enrico Fermi		Henry Farmer
Ernest Lawrence		Earl Lawson
J.R. Oppenheimer		James Oberhelm
G.T. Seaborg		G.T. Sutton
Emilio Segré		Earl Seamen
Leo Szilard		Leo Samuel
Edward Teller		Ed Tilden
R.C. Tolman		Uncle Richard
John A. Wheeler		John Woolley
Eugene Wigner		Eugene Winston

Early examples of Chadwick's increasingly diplomatic role are shown by the following events that started with a January 1944 meeting - prior to Cockcroft's arrival. Groves and Chadwick jointly chaired a meeting with a ten strong group from Canada, led by Halban, also attended by Compton and Fermi from Chicago. At this meeting Groves eventually decided that there would be no collaboration over either the chemistry or production details of plutonium with the U.S.A. (Groves's decision was finally agreed by Conant but he was told that political decisions of that nature were for others to decide in the Combined Policy Committee.)

The decision on non-collaboration over plutonium production was so important that Chadwick wrote to Sir Ronald Campbell - Llewellin's replacement in Washington. He told Campbell that, due to the importance of plutonium as a fissile material, the Canadian question (of plutonium production) should be raised before the C.P.C. At the next C.P.C. meeting on the 17th February 1944, Chadwick, after helping to draft a tripartite agreement on the control of world uranium stocks, was asked together with Mackenzie and Groves to prepare a report on the resources needed and the completion time scale of a Canadian/U.K. plutonium plant.

During the rest of February 1944, Groves's staff prepared the report in which it became obvious to Chadwick and the others that it was beyond the U.K. war-time budget. Groves pointed out that the Hanford complex could produce sufficient plutonium for the war and that there was no military advantage in duplicating effort in Canada. But, Chadwick countered this by suggesting that a moderate size, pilot heavy water reactor plant could be constructed for research purposes in Canada. Groves acquiesced, and the report's conclusions were modified in this respect. By March, just prior to Cockcroft's arrival to take over from Halban, it was agreed that such a Canadian research establishment be set up. The decision was unanimously ratified at the C.P.C. Meeting of 13th April 1944. Construction commenced in August 1944, under Cockcroft, at Chalk River after the Canadian Government's approval. With Chadwick's initial insistence and through the goodwill attending Cockcroft's arrival, arrangements were made for not only heavy water to be transferred to Canada from the U.S.A., but some sample uranium slugs were obtained from Chicago. These enriched uranium rods, negotiated for at a meeting between Chadwick, Groves and Mackenzie on the 8th June 1944 at Chicago, enabled limited U.K./Canadian research into plutonium processes to be pursued. Chadwick's diplomatic success in this instance gave invaluable aid to the post war U.K. atomic bomb work.

At the end of February 1944, Chadwick was able to persuade Groves that Rotblat, a Polish citizen, would be an asset to the Manhattan Project at Los Alamos and was a loyal ally. Consequently, without a change of citizenship, Rotblat was asked to go to site Y, as the only foreigner without either American or British citizenship.

Oliphant, who had been on a visit to the U.K., passed information to Kinsey from Chadwick directing Kinsey to take over the Liverpool cyclotron during Rotblat's absence. Kinsey, who was at this time working on the Cambridge cyclotron, commenced after Rotblat's departure working half-time on the Cambridge and half-time on the Liverpool cyclotrons.

Rotblat stayed for a few months with the Chadwicks at Los Alamos until the Chadwick daughters arrived there from Halifax in the early summer. It was a great comfort for mother and daughters to be together again as Mrs. Chadwick must have been lonely during Chadwick's long absences.

Kinsey sent a letter to Chadwick dated 11th April 1944, giving him the latest information on the work of the Liverpool cyclotron and of the future use to which the cyclotron should be put. He wrote again on the 2nd June giving an updated review of the work[10] at Liverpool.

Following the 6th June 1944 D-Day landings by the Allies in Normandy, Chadwick returned to the U.K. to brief Anderson on the Canadian and other Project developments. Prior to his return, he had written to Cockcroft offering to recruit people for the new Canadian work at Chalk River. While in London, he attended a Tube Alloys Consultative Council Meeting on the 7th September 1944 and at the meeting it was agreed that atomic bombs should be made in the U.K. as soon as possible. It was also there agreed that a Gaseous Diffusion Plant be constructed at once in the U.K. which would have an output of 600 grams per day of two-fold enriched uranium.

On the 5th October 1944, Sir Henry Dale who was the President of the Royal Society, Niels Bohr (coincidentally also in London) and Chadwick had a discussion over their deep anxieties on the future uses to which possession of atomic bombs might be put.

General de Gaulle had visited Montreal during July 1944 and was secretly briefed on the development of the atomic bomb work by Guéron, the only one of the five French scientists not on the British pay roll. In October, Guéron visited Paris and this began to alarm the American security people. Shortly after his visit, Joliot asked to visit Montreal - he had returned to his Laboratory as soon as possible after Paris had been retaken by the Allies on 25th August 1944. Chadwick and Groves, at a meeting held on the 23rd October 1944, decided that Joliot should not visit Canada and that nuclear technology should not be shared with France even though France was the third member of the countries initially involved with the Project. The difficulty of honouring the patents that had been taken out by Halban and Kowarski in 1939 did not help to alleviate American/British anxieties and this meeting brought to the fore the underlying tensions that existed between the French, and the Americans and British. After Joliot's request to visit Montreal had been refused, Halban asked Chadwick if he might visit Paris over the Christmas period of 1944. Chadwick, knowing the sensitivity of the French issue and in particular the friendship of Joliot and Halban, refused, as he thought that Halban could not help but to give information to Joliot if Joliot were to request it.

Halban, however, was determined to go and sought and obtained Anderson's permission over-ruling Chadwick's decision. The Americans (and probably Chadwick!) were very upset by Halban's Paris visit and relations between the British and Americans were strained and at a very low ebb. Eventually, Anderson apologised over the affair, and Campbell and Chadwick were able to sort it out with Groves. Anderson suffered a humiliation and his influence with both Churchill and the Americans was diminished. But, as Anderson's political stature decreased, Chadwick's increased.

After Chadwick's return to the U.S.A. in early November 1944, he held a meeting at Los Alamos with Oliphant and Peierls covering the 11th and 13th November. They discussed the Tube Alloys meeting of 11th September 1944 held in London concerning the building of a gaseous diffussion plant in the U.K., the main purpose of which, they assumed without question, would be to manufacture a U.K. atomic bomb.

Rotblat, after realising that the Manhattan Project atomic bombs would not be directed against Germany, decided to return to England. He resigned from Los Alamos and arrived back in Liverpool just before Christmas 1944.

Chadwick, 'for services rendered to his country' was awarded a Knighthood in the New Year's Honours List of January 1945. Amongst many congratulations he received a telegram from 'his boys' at Liverpool (Holt, Pickavance, et al) which stated, *Congratulations Dear Sir*. Telegrams were charged per word, (including the address), and the obviously warm greeting using a minimum number of words must have been a double delight.

This author will now give a brief account of Chadwick's involvement in security matters that shocked and coloured subsequent political actions.

It was shortly before December 1945 that Chadwick was made aware of a serious breach of security in the Manhattan Project. Groves was informed that Nunn May (one of Chadwick's previous scientific collaborators at Liverpool) who had been based and in a senior position at Montreal (and later at Chalk River) from January 1944, had visited Chicago frequently enough to arouse American security suspicions. Nunn May had been security checked, as was Klaus Fuchs, prior to their appointments in Canada and Los Alamos.

> ... Nunn May was suspected of having handed over to the Russians microscopic samples of uranium-233 and uranium-235[11].

Nunn May, with Chadwick's and Groves's blessing, had worked under Cockcroft at Montreal and then Chalk River on the "plutonium question". (According to Brown, Groves previously discussed Nunn May and his security clearance with Chadwick on the 20th January 1945[12], after Chadwick had interviewed Nunn May and found him "exceptionally reliable".) In an effort to find his London-based Russian contacts, it was decided not to arrest Nunn May until after his return to London. Nunn May was finally arrested on 4th March 1946, after taking up his position as a Reader at King's College[13,14]. He had been under suspicion since the previous September. A more detailed account of events surrounding the discovery and conviction of Nunn May and Fuchs may be found in Moorehead's book[15].

This author has corresponded with the archivists of both King's College and Bristol University. Michael Richardson, the Bristol archivist, informed this author in a private communication that,

> All of Nunn May's papers were collected from Bristol after his arrest in 1946, by security officers. The security people also remained closeted in Powell's office for 2 or 3 days immediately after Powell's sudden death in August 1969, and removed most of his papers including information on Nunn May.

In a letter dated 2nd May 1995, the University of London (King's College) Archivist's office, informed this author that,

> **Dr Alan Nunn MAY**
> ... I have checked through the College calendars, Registry slip books and departmental student record cards for the period covering 1939-1946 and have found no trace of an Alan Nunn May. I have also checked with the Central Registry at Senate House, University of London, and they have also drawn a blank.

Nunn May was eventually convicted of treason. A number of senior scientists, led by Professor Nicholas Kemmer from Imperial College (and including Cockcroft who later regretted his action), were dismayed at his ten-year sentence of penal servitude. They wrote a letter to Chuter Ede, the Home Secretary, asking for clemency on the grounds that the sentence was out of proportion to the offence that had been committed. The appeal was not successful. Chadwick's opinion of Nunn May plummeted. (Chadwick later limited his association with him by removing the titles of papers on which they had collaborated from the list in Chadwick's Royal Society Biography. As a Fellow, Chadwick must have specifically arranged this.)

The Association of Atomic Scientists was formed early in 1946 and Chadwick, in a cautionary letter dated 2nd April 1946 to its Chairman, H. Massey, wrote,

> I was a little uneasy to read that you had *volunteered* to act as an adviser for the defense. I may be unduly cautious but to me this is a very different matter from consenting to act as an adviser on the request of his counsel.
> I expect you know the Canadian affair in general and the case of May in particular, has produced a very strong effect on this side. Whatever the result of May's trial may be, there will remain a deep feeling of uneasiness. In every way this is a most unfortunate affair[16].

Groves was very upset about the breach of security and, again, it was Chadwick who helped to smooth over the inevitable tightening up of all security and other procedures. The senior politicians, after Nunn May's arrest, became more wary of their dealings with the Soviet Union and it caused disquiet to Attlee. There was a 'Declaration' by the three Heads of Government in Washington in November 1946 between the U.S.A., U.K. and Canada which,

> ... recognised and supported the free exchange of basic scientific knowledge, ... ; the sharing of detailed information on the practical applications of atomic energy would not be permitted until "effective enforceable safeguards against its use for destructive purposes can be devised"[17].

The strain of secrecy that Chadwick had been under from 1941 is shown to be all the more intense when, for the first time, in January 1945, Churchill informed his own Chiefs of Staff about the Manhattan District Engineering Project.

Returning now to the other problems that were part of Chadwick's remit and continuing from early 1945, Groves asked Chadwick to contact Appleton and request Geoffrey Taylor, an explosion theory expert, and William Penney, a mathematician, both at Imperial College London, to come to Los Alamos. Chadwick did so in a letter dated 21st March 1945 and the two went to Site Y soon after.

Chadwick sent a report to Anderson at the end of March 1945 in which he outlined, as he saw it, the future Tube Alloys' policies and programmes. Anderson was also told that the U.S. programme was expected to have a very powerful military weapon ready by August 1945. A further important piece of information sent to Anderson was that the Americans would not take kindly to being told of an independent British nuclear programme and to delay any announcement of such a meeting or programme.

In April 1945, the Chadwicks rented a house in Washington where he had spent, and continued to spend, most of his time, and his family joined him there. This was the time when Peierls took over as head of the British contingent at Los Alamos and continued the informative letters to Chadwick (which had started in the February), and copies of which Groves eventually received.

During April, the Americans and the Allies were saddened by the death of President Roosevelt, and the Vice-President, Harry S. Truman, took over the Presidency.

Truman had not been briefed on the Manhattan Project prior to Roosevelt's death and was hastily informed of progress in the Project and its military and political implications.

On the 7th May 1945, in order to check on the calibration of instruments and the distribution of radioactive contamination, a 100 ton conventional explosive seeded with an active contaminant was detonated at the northern limit of the Almogordo Army Air Field (at the Jornado del Muerto valley, Southern New Mexico)[18]. Taylor and Penney were actively engaged in the setting up, analysis and instrumentation needed for this and subsequent tests.

Germany surrendered on the 8th May 1945, and a number of German nuclear scientists - Heisenburg among them - who had been captured by the Allies were interned at Farm Hall in Godmanchester, near Cambridge. The whole of the accommodation had been secretly wired for sound recordings to be made of their conversations. The conversations and discussions the German scientists had when they heard the news of the successful atomic explosion over Hiroshima will be discussed later.

Germany's surrender released tremendous military resources, particularly American, to direct against Japan. The Allied Military Commanders were preparing and planning an invasion of Japan that would take place later in 1945. Although the Allied Chiefs of Staff had been informed about the atomic bomb Project, they had not been informed of its state of readiness so could not plan their campaign with the assumption of using an atomic bomb. In Hamby's biography of Truman[19], it is stated that the casualties the Pentagon planners projected would be sustained in an invasion of Japan at Kyushu and then at Honshu 'no later than November 1st, 1945', would be in total approximately 220,000 of which one quarter would be fatalities. They assumed at least a comparable number of Japanese casualties. It was armed with this knowledge that Truman was able to give the order for an atomic bomb to be dropped firstly on Hiroshima and later on Nagasaki.

At the end of May 1945, Chadwick wrote to Anderson giving his detailed views on the proposed establishment of a U.K. post-war nuclear research facility and its possible programme. This letter was followed on the 2nd June 1945 by a letter to Appleton [Director of the D.S.I.R.] endorsing the choice of Cockcroft as the first chief of a U.K. atomic energy research establishment.

James Franck, with five other Chicago scientists, sent to Washington a report voicing their concerns over the moral issues of using an atomic bomb on Japan without prior warning. This report - later referred to as the Franck Report - was considered by the U.S. War Office. A scientific panel was set up comprising Oppenheimer, Fermi, Lawrence and Compton (head of the Chicago Metallurgical Laboratory) and, after considering the report, wrote a secret statement to the effect that an atomic bomb was a necessary weapon to be used, and its use would save Allied lives.

The design of the uranium bomb - the 'gadget' as it was referred to by the scientists at Los Alamos - was frozen in February 1945[20] and production of enriched uranium (in the 235 isotope) was stepped up. This was to ensure that there would be sufficient U^{235} available for the 'gun gadget' and that the production laboratory would not be 'holding up the military application of the weapon'[21]. The uranium gadget became known amongst the scientists as 'Little Boy' and the plutonium implosion bomb became known as 'Fat Man'. 'Thin Man' was the name given to a plutonium bomb in which the detonation mechanism was to fire a sub-critical mass of plutonium into another sub-critical mass of plutonium, as in a uranium 235 bomb. But due to the fact that spontaneous fission occurs in plutonium, a 'fizzle' would take place if one part were fired into another. This was because of the time taken for the two masses to come together which would give sufficient time for a number of 'spontaneous fissions' to occur and prematurely start fission within the two approaching sub-critical masses.

It was only by the 24th July 1945 that there was sufficient moulded uranium 235 for one complete bomb, which comprised the target U^{235} and the projectile U^{235} (the two sub-critical masses respectively). The components were, on that date, ready for shipment to Tinian Island in the Pacific Ocean by the U.S. warship *Indianapolis*.

It may be noted here that there was no test of a U^{235} bomb as there was of the plutonium atomic bomb. There were two reasons for this. Firstly, there was insufficient U^{235} to manufacture more than one bomb at the time the bomb was required for use, and secondly, Oppenheimer and Groves were convinced a U^{235} bomb would explode. The reason they were sure the U^{235} bomb would explode was due to a series of dangerous experiments that had been completed. These experiments were later called the 'Dragon Experiments' as Richard Feynman, at a Los Alamos Coordinating Council Meeting, compared them to 'tickling the dragon's tail'. The experiment was proposed by Frisch - who else? - and emerged from suggestions that he had sent to Oppenheimer in two memos dated 17th and 24th October 1944. In these memos, Frisch proposed dropping a slug of U^{235} through a just sub-critical assembly,

> ... making it supercritical for prompt neutrons for an extremely short time, ...[22]

Hoddeson's book, *Critical Assembly*, states that Rotblat and Hughes both worked on the detection equipment for the dragon experiments. After the final dragon experiments - that probably took place in late January or early February 1945, after Rotblat's return to England - the gadget design was frozen (see above), as it was found that,

> the burst of neutrons and dramatic temperature rise gave "very direct evidence of a nuclear explosion nipped in the bud" and a much-needed confidence boost to the scientists, for the results of the assembly experiments agreed with their theoretical predictions[23].

There were many problems to be solved in transporting the necessary bomb components to a suitable airfield within range of Japan and not least in the design of a bomb casing which was suitable for dropping from an aeroplane. There were a series of parallel investigations in 1944 and 1945, all designed to make the atomic bombs as effective as possible, including the crucial height at which the bombs should be detonated for maximising blast effects. Taylor's and Penney's expertise was here invaluable.

The Fat Man implosion plutonium bomb had much more complex detonating and firing mechanisms than the uranium bomb. For example, in order to make the plutonium critical, the necessary amount of plutonium had to be reduced to the size of a walnut (thereby increasing its density many times) in a very short time. This was achieved by surrounding the plutonium with a number of explosives which on detonation compressed the plutonium to a critical mass in a few microseconds. It meant that the compressing explosives must all detonate at the same time (plus or minus two microseconds) and a homogeneous spherically shaped blast wave would then suitably compress the plutonium to achieve criticality.

The involved mathematics on waves and fluid mechanics was rapidly developed and was led in many cases by Peierls, who had became head of the Implosion Hydrodynamics Section of the Theoretical Division at Los Alamos, and by Feynman[24] and others. It is also noted that Frisch became head of the Critical Assemblies and Nuclear Specifications Section, and Placzek was in charge of the Composite Weapon Group. (See the comments previously mentioned in this chapter that may be found in Szasz's book *British Scientists and the Manhattan Project* for the other British Group and Section Leaders.)

The area around the Los Alamos site was surveyed for a suitable site for testing an implosion plutonium bomb. A convenient place was found to be the area known as the Jornada del Muerto Valley in the Alamogordo Bombing Range in New Mexico. In September 1944 permission was obtained to use this area. The 'Trinity' test site was chosen to be in the north west corner of the bombing range. The design for the implosion plutonium bomb was frozen on the 1st March 1945 and all of what the Los Alamos Cowpuncher Committee - set up to 'ride herd' over the programme - (consisting of Oppenheimer, Groves, Conant, Tolman, Kistiakowsky, Bethe and Lauritsen) thought of as unnecessary research was halted. Effort was then shifted to concentrate on completing the work. Plutonium was also now, it was thought, being manufactured in sufficient quantity for there to be enough for a test firing and a combat weapon.

The plutonium bomb started to be assembled at the top of a 100 foot high tower at the Trinity site on the 3rd July 1945. As the date for the scheduled test firing neared, weather forecasters were approached to forecast suitable weather slots which had maximum clarity and still, dry conditions, with any wind blowing away from human habitations. The initial test date had been the 4th July 1945[25], but all of the necessary components were not ready - the earliest completion would be 16th July 1945. This information was conveyed to Washington and in their reply, the President's advisers informed Bainbridge (who was coordinating the testing) that the President was meeting with Stalin and Churchill on 16th July 1945 in Potsdam and wanted the result of the test by that date. In the event, the weather was good enough for the test to take place. A successful detonation of the implosion plutonium bomb took place at 05.30 hours on 16th July 1945. Chadwick was among the senior scientist witnesses of the event. He later described the explosion to a reporter of the *Liverpool Daily Post*, and the article was published on the 4th March 1946. The observation point was a hill called Compañia about 20 miles northwest of ground zero.

Chadwick said in the newspaper article mentioned above,

> Although I had lived through this moment in my imagination many times during the last few years and everything happened almost as I had pictured it, the reality was shattering. What had begun in a few simple ideas had at last been put to the test of experiment, and the awe-inspiring nature of the outcome quite overwhelmed me.

The successful outcome of the Trinity test was immediately conveyed to Truman in Potsdam and an extensive description of the test was sent to him by Groves on the 21st July 1945. On the 26th July 1945, Churchill was succeeded as Prime Minister by Clement Attlee (after the general election on the 5th July 1945), and Attlee found himself in a similar position to that which Truman had been a short time before - not well informed on atomic energy matters! On that same day, the 26th July, a proclamation was issued from Potsdam by the U.S.A., Great Britain and China, demanding Japan's unconditional surrender. This was rejected by Japan in their reply dated 28th July 1945. As a consequence of their non surrender,

> ... at 8.11 am on August 6th, a B-29 (bomber type aeroplane), the *Enola Gay*, piloted by Colonel Paul Tibbets, dropped an atomic bomb over the city of Hiroshima from an altitude of 31,000 feet. The explosion occurred at 2000 feet. ... almost nothing remained standing within a one-mile radius of 'ground zero'. Perhaps 75,000 people, mostly civilians, were killed at once; more tens of thousands would eventually die from the effects of radiation[26].

On the 8th August 1945, Truman announced that the U.S.S.R. had declared war on Japan. On 9th August 1945, at 11 am, a plutonium atomic bomb exploded over Nagasaki. The fatalities and deaths caused by this bomb were about half of the Hiroshima uranium bomb casualties. The Japanese surrendered on 14th August 1945, but not until after a 1,000-plane raid on Tokyo that took place on the 13th August.

It was previously mentioned that captured German scientists were held at Farm Hall and that the accommodation had been wired for sound. The prisoners were allowed access to B.B.C. radio broadcasts and their oral reaction to, and conversations about, the news of the Hiroshima bomb were recorded.

The ten interned scientists were, at that time, the leading German nuclear scientists, and are named below,

> Erich Bagge; Kurt Diebner; Walther Gerlach; Otto Hahn; Paul Harteck; Werner Heisenberg; Horst Korsching; Max von Laue; Carl Friedrich, Freiherr von Weizsäcker; and Karl Wirtz.

In the August 1995 *Physics Today*[27], Bernstein and Cassidy reviewed the recently declassified tape recordings of the internees and showed that the German nuclear physicists, although aware of the theoretical possibility of using U^{235} and plutonium in an atomic bomb, did not however embark on a programme to make one. In an effort to distance themselves from the horror of the Hiroshima and Nagasaki bombs, the Germans decided that future versions of accounts of their war-time atomic research would show they had decided not to make an atomic bomb and wanted only to use atomic power for peaceful purposes. As the article's authors state,

> On hearing the news from Hiroshima, the incredulous internees came up with a self-serving story to explain their failures in nuclear research: To keep Hitler from winning, they had deliberately not developed the atomic bomb.

Groves was sent transcribed copies of the tape recordings on a regular basis and showed them to Chadwick. The transcriptions confirmed intelligence data that the senior nuclear physicists in Germany had continued their university lecturing programme during the war and could not have devoted the necessary time to atomic bomb manufacture. There had been no large scale construction work commenced in Germany that the Allies thought could have housed the apparatus for isotope separation. Since the middle of 1944, and possibly some months before that, it was apparent to the Allies that there was no equivalent German project to the Manhattan Project.

It is noted that Groves, Field-Marshal H.M.Wilson and Chadwick met on the 14th September 1945 in Washington and discussed the futures of the interned German scientists held at Farm Hall. Groves stated categorically that they would not be made welcome in the U.S.A. and it was Chadwick who suggested that they should be allowed a modest living back at their respective universities. This course of action was followed and, early in 1946, the German scientists were released to the British controlled zone of West Berlin.

On the 9th July 1995, on pages 12 and 13, the *Independent on Sunday* published a report by Brian Cathcart titled 'One Man and his Bomb'. The article described the life of, and two momentous occasions recalled by, Sir Rudolph Peierls. The first occasion was the realisation in 1940 that an atomic bomb was a possibility, and,

> The other important moment for Peierls came when the fear that the Germans were developing the bomb was demonstrated to be unfounded. This was only established with certainty as the Third Reich collapsed, but Peierls satisfied himself that there was no threat in mid-1944, through a study of some current German academic publications which came his way. These showed that those scientists who would have been essential for any German atomic weapon project were still at their universities, lecturing more or less as usual.

Cathcart continued,

> This meant that the original rationale for developing the bomb was gone; Germany was not making a nuclear weapon and was all but beaten. There was, moreover, no possibility that the Japanese might have a serious atomic bomb programme.

It is highly likely that Peierls's intuition and understanding of the current German atomic energy programme would have been passed on to Chadwick, and if he did so it would have reinforced Chadwick's and Groves's intelligence concerning Germany's programme. Rotblat meanwhile had, in his own mind, decided that Germany would not be the first to have an atomic bomb and the reason for him to continue working on the Project had therefore been removed, and this was the reason he gave for his resignation from site Y.

Cathcart, in the newspaper article above, stated that he thought the Japanese did not have 'a serious atomic bomb programme', and in order to find more information on this topic, this author has been in correspondence with a number of Japanese historians who have an interest in early atomic bomb and cyclotron projects.

Chadwick's connection with the Japanese started in 1936, as far as this author has been able to ascertain, when he received a letter from 'Mr. Sagane'. Chadwick wrote to Kinsey on the 28th July 1936, who was at the time with Lawrence at Berkeley, and said[28],

> I have received a letter from Mr. Sagane, whom you apparently know, asking to come here in October and work on the cyclotron. I have written to accept him. It seems that he wanted to go to Cambridge but the prospect of helping to build the cyclotron attracted him here.

No record has been found of Sagane's presence in Liverpool.

Ryokichi Sagane was listed as a 'Sabbatarian' and member of the academic staff at Lawrence's Berkeley Laboratory for the years 1936/7 and 1937/8[29]. This author made enquiries to Cambridge University, and through the good offices of their Archivist, O. Green, was told that Sagane was listed as being at the Cavendish Laboratory for two terms between January and June 1937, inclusive.

Sagane would have gained an overall picture of nuclear physics and, in particular, the state of the art of building cyclotrons in the U.S.A. and at the Cavendish, and indirectly, the progress at Liverpool University. Sagane was therefore up to date at that time of the state of progress in nuclear physics by both the U.S.A. and the U.K. In Heilbron and Seidal's book mentioned above is shown information on 'Foreign Cyclotrons by Size, 1940[30]. The table indicates, concerning Japanese cyclotrons, the following,

Tokyo-1:-	Energy 3 MeV, 26" pole diameter, 23 ton magnet Planned September 1935, First beam April 1937 Builders Yasaki, Sagane and Watanabe
Osaka:-	Energy 12 MeV, 40" pole diameter, First beam 1939, Builder Kikuchi
Tokyo-2:-	Energy 16 MeV, 60" pole diameter, 196 ton magnet Planned 1936, First beam > 1941 Builders Yasaki and Sagane

Shizue Hinokawa, a Japanese historian of science, has given this author a book[31] which contains, amongst other data, information gathered and papers written by Japanese scientists immediately after the atomic bomb was dropped over Hiroshima on 6th August 1945. According to the 'Personal Record of Editor of this Volume, Dr. Sakae Shimizu', his 'experience', that is, his research activity during 1943 - 1945, is stated as

1943-1945	Photonuclear reactions. Lecturer of Physics 1943. Construction of 12" cyclotron, *Separation of ^{235}U by the ultra-centrifugal method (this was a part of the War-time Research).*

(This author has italicised part of the above quotation for emphasis)

One of the papers contained in this book edited by Shimizu, and also separately published by him[32], contains the following statements:

> In the final stage of the war, separation of ^{235}U by applying the centrifugal method was studied and only a few months before the end of the war a basic design of a high-speed rotating apparatus of magnetic suspension and magnetic driving for this

> purpose was completed. But, it was never fabricated owing to destruction of nearly all industry by the air raids.

Shimizu continued later in the same paper:

> These wartime works concerned with uranium under the supervision of Professor Arakatsu in Kyoto were aimed at confirming the possibility of a chain reaction of uranium fission, of which realization could result into the nuclear bomb by the Allied Powers. Before the last day of the war, the 15th August, 1945 (sic) however, we had carried out no promising experimental project to get a positive clue for this possibility. Here, it is noted that during the whole period of the war we of Japanese scientists did not get any information on large activities of Allied Nations, the Manhattan Project, excepting a news in 1943 that a British military unit destroyed thoroughly the Norsk Hydro plant in Norway in 1943 where production of heavy water was being conducted on a large scale by German forces.

It is therefore apparent that Cathcart's statement that the Japanese did not have 'a serious nuclear programme' was factual, but they were certainly further advanced than the German scientists and could have developed a uranium fission bomb given sufficient time. If the Japanese had not been defeated later in August 1945, it is probable that a nuclear bomb programme could have commenced. Manpower and other resource problems would have extended the time scale of completion of a uranium bomb beyond that taken by the Manhattan Project, so that a minimum of a further two years would have been required for the Japanese to possess a nuclear atomic bomb.

It is noted that, following the atomic explosion over Hiroshima, a team of Japanese scientists did a field survey of radiation and medical effects in and around Hiroshima. They were able to confirm that the bomb comprised U^{235} from the intense neutron radiation effects. A measure of the half-lives of a number of irradiated items, including the elements found in the soil, the sulphur in the ceramic of insulators, iron found in a watt-meter movement, the potassium from the skull bone of a horse and other items, enabled the scientists to calculate that a uranium bomb had been exploded. In order to arrive at an estimate of the quantity of uranium 235 that could cause the irradiations to give the half-lives found, they calculated the total number of neutrons emitted of the uranium used. This number was 10^{+24}.

> ... assuming from the estimated value of the amount of U^{235} enabling full fission, it seemed that approximately 10 kg of the specimen enriched to a ratio of about 1 to 10 of U235 to 238 was utilized, having approximately 50 kg of mixed fluids, consisting of water and heavy water, to be the main constituents of the bomb[33].

The above quotation was taken from the final report of the series of reports published on consecutive days of 14th, 15th, 16th and 17th September 1945, and is part of the English translation that is also published in Shimizu's book.

It shows that the Japanese scientists had a good understanding of the mechanism by which a nuclear fission explosion could be achieved.

Robert Jungk's book, *Brighter than a Thousand Suns*, states that three of the Manhattan scientists, Luis W. Alvarez, Phillip Morrison and Robert Serber, wrote to Sagane in a letter dated 9th August 1945 - three days after the Hiroshima bomb and on the same day as the Nagasaki bomb exploded. They asked him to find the appropriate authorities in Japan and ask them to surrender in order to prevent further loss of life. The above letter is also mentioned in *Sin-itiro Tomonaga - Life of a Japanese Physicist*[34] stating,

> A message directed to the late Professor Sagane disclosed that the powerful bomb dropped several days before had been an atomic bomb, and urged him to advise the high-ranking officials of Japan to stop the futile war. One of those that signed the message was ... Morrison. He had been a colleague of Professor Sagane when the latter was in the United States studying under Professor Ernest Orlando Lawrence.

Acting on orders which had originated from Groves's office, the U.S. Army then occupying Japan confiscated the three Japanese cyclotrons and had them dumped in the Pacific Ocean. Most of the U.K. and U.S.A. scientific communities were "profoundly shocked"[35] over this affair and Chadwick could sense the increasingly strained relationship between the U.S. and U.K. in scientific and other collaborative matters.

The 'heavily vetted' Smyth Report (which was published in book form two years later[36]), was finally reviewed by Groves and Chadwick in Secretary Stimson's office on the 2nd August 1945. It gave some disquiet to Chadwick as the report would have been of some help to a possible future nuclear power and also does not single out or give specific credit to the British contingent.

Chadwick began to wind down from his enormous work load in Washington, but, with Groves's full permission, he gave an interview to the New York press. An article was published in the *New York Times* on 13th August 1945, in which Chadwick paid tribute to Groves and the British scientists in the Manhattan Project both at home and on the American continent. He outlined the future of atomic research and,

> He thought the industrial applications of atomic energy would be realised in about a decade.

Churchill, just prior to his election defeat of 5th July 1945, had written a statement to be issued in the event of the atomic bombs being used. This statement not only drew attention to the part played by the British contingent but Chadwick was singled out for a special mention. Attlee published it in a White Paper which is now out of print, but Gowing and Arnold published the text in its entirety in their book as referenced below.

> The smoothness with which the arrangements for cooperation which were made in 1943 have been carried into effect is a happy augury for our future relations and reflects great credit on all concerned - on the members of the Combined Policy Committee which we set up; on the enthusiasm with which our scientists and technicians gave of their best - particularly Sir James Chadwick who gave up his work at Liverpool to serve as technical adviser to the United Kingdom members of the Policy Committee and spared no effort; and not least, on the generous spirit with which the whole United States organisation welcomed our men and made it possible for them to make their contribution[37].

It is fitting that Chadwick should have been recognised in this way, as he could not, according to his colleagues in Britain and America, be like a

> ... soldier ... (and) ... simply unload his wartime responsibilities on returning to civilian life[38].

There were pressing problems in the University of Liverpool as the war ended, not least in the retention and acquisition of staff. There is a large collection of correspondence held in the Churchill Archive Centre of the University of Cambridge, between the Officers of the University of Liverpool, senior members of University staff as well as communications to leading political figures.

The state of nuclear physics equipment in the universities of the U.K. was about the same as would have been found pre-war, except for the Liverpool 37" cyclotron. As Gowing and Arnold state in their book[39], Chadwick told Anderson's Scientific Advisory Committee that,

> ... even before the war Britain had fallen behind the United States, for example in the number of her cyclotrons. During the war no new equipment was provided and the three old cyclotrons (at Birmingham, Cambridge and Liverpool) had limped along doing their best for Tube Alloys until almost past repair; Chadwick pointed out indignantly that the Liverpool cyclotron was not a 'lame duck' but a 'war casualty', which had done more true physical work than any other cyclotron except the one in Los Alamos.

A technical sub-committee of the Advisory Committee had been set up in 1944 and Chadwick had been asked to Chair the group, known as the Nuclear Physics Sub-Committee, and while he was in America, Blackett deputised for him. This committee had as its remit

> to make recommendations regarding the programme of nuclear physics to be pursued in this country as a whole.

The Advisory Committee was abolished at the end of the war and the Nuclear Sub-Committee became a full committee reporting to the Ministry of Supply through Lord Portal. Chadwick was here leading the formulation of post war nuclear physics research policies in the U.K.

Chadwick and his family returned to Liverpool University in August 1946 after extricating himself from his post in Washington, crossing the Atlantic on the liner *Queen Mary*. One of his colleagues at the University, the new Vice-Chancellor James Mountford, was shocked to see the effects that the war-time strain and worry had had on Chadwick and noted that he had never seen a man 'so physically, mentally and spiritually tired' as Chadwick was at that time[40].

This chapter has shown how Chadwick became in 1943 the head of the British Mission to the U.S.A, and how he developed his role as scientific adviser to the British Government. It shows how he acted as security adviser to Groves and that, without the cooperation he achieved in his dealings with him, the U.K. would not have participated so fully in the Manhattan Project or have been so well informed on atomic bomb matters. Chadwick chaired the Nuclear Physics Sub-Committee that considered and advised on the nuclear research and development that post-war Britain should pursue. This author has also attempted to put in perspective the state of nuclear weapon progress of the Germans and Japanese during the war.

References:

5.1. Tape recorded interview, Chadwick and Charles Weiner, 15th-21st April 1969, Neils Bohr Library, Center for History of Physics, American Institute of Physics, page Chadwick - 120.

5.2. *ibid.* 5.1, page Chadwick - 106.

5.3. *ibid.* 5.1, page Chadwick - 107.

5.4. Gowing M., *Britain and Atomic Energy, 1939-1945*, 1964, Macmillan & Co. Ltd., The list of Maud workers are on pps. 48_n, 53_n, and the list of Manhattan Project workers on pps.258_n, 262_n, 443_n.

5.5. Szasz F.M., *British Scientists and the Manhattan Project; The Los Alamos Years*, 1992, St. Martin's Press (New York), p. 18.

5.6. Brown A.P., *The Neutron and the Bomb; A Biography of Sir James Chadwick*, 1997, Oxford U.P., p. 286.

5.7. *ibid.* 5.6., p. 261.

5.8. *ibid.* 5.5., pps. 39 and 40.

5.9. Hoddeson, L.; Henrikson, P.W.; Meade, R.; Westfall, C.L., *Critical Assembley: A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945*, Cambridge University Press, 1993, p. 95.

5.10. *Letters*, Kinsey to Chadwick, dated 11th April 1944 and 2nd June 1944, held in the Churchill Archive Centre, Cambridge.

5.11. Hartcup, G., Allibone, T.E., *Cockcroft and the Atom*, Bristol, Adam Hilger Ltd., 1984, p. 133.

5.12. *ibid.* 5.6., p. 308.

5.13. *ibid.* 5.11., p. 133.

5.14. Article titled 'Atomic Secrets Charge', *The Times*, Wednesday, 20th March 1946, p. 2, column 1: see also,
article titled 'Canadian M.P.'s Trial. Toronto March 22nd', *The Times*, Saturday, 23rd March 1946, p. 4, column 3.

5.15. Moorehead, A., *The Traitors*, Hamish Hamilton, 1952.

5.16. *ibid.* 5.6, p. 315

5.17. *ibid.* 5.6., p. 310.

5.18. *ibid.* 5.9., 'The 100 - Ton Test', pps. 360-2.

5.19. Hamby, A.L., *Man of the People: A Life of Harry S. Truman*, Oxford University Press, 1995.

5.20. *ibid.* 5.9., p. 249.

5.21. *ibid.* 5.9., p. 255.

5.22. *ibid.* 5.9., 'The Dragon Experiment', pps. 346-8.

5.23. *ibid.* 5.9., p. 348.

5.24. Feynman, Richard P., *Surely You're Joking, Mr. Feynman!*, London, Unwin Hyman (Paperbacks) Ltd., 1986.

5.25. *ibid.* 5.6, p. 288.

5.26. In a review of A. Hamby's book, *ibid.* 5.19, in *History Today*, Volume 45 (8), August 1995, p. 24.

5.27. Bernstein, J.; Cassidy, D., 'Bomb Apologetics: Farm Hall, August 1945', *Physics Today*, August 1995, pps. 32-36.

5.28. *Lettter*, Chadwick to Kinsey, dated 28th July 1936, Document 66, Chadwick Archive, University of Liverpool.

5.29. Heilbron, J.L.; Seidal, R.W., *Lawrence and his Laboratory: A history of the Lawrence Berkeley Laboratory, Volume 1*, University of California Press, 1989, Table 5.4b, pps. 230-1.

5.30. *ibid.* 5.29, Table 7.1, facing p. 320.

5.31. Shimizu, Sakae, (Editor), *Hiroshima Atomic Bomb, August 1945, and Super-Hydrogen Bomb Test at Bikini Atoll in the mid-Pacific, March 1954*, Investigation by Scientists of Kyoto University, Katsuhiko Yazaki, Kyoto Forum, July 1995.

5.32. Shimizu, Sakae, 'Historical Sketch of the Scientific Field Survey in Hiroshima Several Days after the Atomic Bombing', *Bull. Inst. Chem. Res.*, Kyoto University, Volume 60, No. 2, 1982.

5.33. *ibid.* 5.31, 'A Report on the Atomic Bombing - Nuclear Investigation on Field Surveys in Hiroshima' by Professor Bunsaku Arakatsu, 17th September (1945), pps.15-24.

5.34. Matsui, Makinosuke, Editor, *Sin-itiro Tomonaga - Life of a Japanese Physicist*, MYU, Tokyo, 1995, translated by Cheryl Fujimoyo and Takako Sano, English version edited and annotated by Hiroshi Ezawa, p.109.

5.35. *ibid.* 5.6., p. 311.

5.36. Smyth, H. D., *Atomic Energy for Military Purposes*, Princeton University Press, 1948.

5.37. Gowing, M.; Arnold, L., *Independence and Deterrence, Britain and Atomic Energy, 1945-1952*, Volume 1, 'Policy Making', Macmillan, 1974, *Statements by the Prime Minister and Mr. Churchill on the Atomic Bomb, 6 August 1945*, p. 17.

5.38. *ibid.* 5.6, p. 323.

5.39. Gowing, M.; Arnold, L., *Independence and Deterrence: Britain and Atomic Energy 1945-1953*, Volume 2, p. 220.

5.40. Mountford, James, 'Obituary of Sir James Chadwick', *University of Liverpool Recorder* **66**, 1974, pps. 14-15, University of Liverpool Archives.

CHAPTER 6
Post Second World War

It is intended to show in this closing chapter how Chadwick transformed the Department of Physics at the University of Liverpool and how he laid the groundwork and policies for the Department to become a leading European Nuclear Research Facility. It will also be shown that Chadwick was influential in obtaining U.K. participation in the C.E.R.N. collaboration, and in the formation of policies that set out the basis for the U.K.'s nuclear policies of the 1950s and 60s.

By the time Chadwick resigned his Chair at Liverpool in 1948, he was a Pro-Vice Chancellor and the Department was building up its academic and technical staff. He had obtained new land for purpose-built laboratories and had nearly completed the designs for what would become one of the biggest cyclotrons in Europe. When the 156" synchrocyclotron was completed in 1954, the new research opportunities it opened up attracted an ever increasing number of researchers to Liverpool.

However, in 1948, at the age of 56 years, Chadwick was invited to become Master of his old Cambridge College. With some reluctance and out of a sense of duty to the College, he accepted the position. This author will show it was not a completely happy time for him. He resigned from the Mastership of Gonville and Caius College in 1958 and retired to North Wales. The Chadwicks returned to Cambridge in 1968 when he was 76 years old.

On the 17th August 1945, he received a letter from Prime Minister Attlee asking that he serve on the Advisory Committee on Atomic Energy (A.C.A.E.), with Anderson at its head. The full committee comprised:

Sir John	Anderson	Chairman and an Independent Member of Parliament
Sir Edward	Appleton	Secretary to the D.S.I.R.
Sir Alan	Barlow	Second Secretary at the Treasury
Sir Patrick	Blackett	Controller of Atomic Energy (within the Ministry of Supply
Sir Alan	Brooke	Chief of the Imperial General Staff
Sir Alexander	Cadogan	Under-Secretary of State at the Foreign Office
Sir James	Chadwick	Chief Technical Adviser to the U.K. Members of the Policy Committee

Sir Henry	Dale	President of the Royal Society
Sir George P.	Thomson	ex-Chairman of the Maud Committee
(Dr. D.H.F.	Rickett	as Secretary)

It was a committee with an impressive membership! As Chadwick was in the U.S.A., Rickett sent him the Committee minutes, and in the first set Chadwick received he was asked for an assessment of the damage caused by the bombs dropped on Japan. Penney, who had a seat on the observation plane that flew over Nagasaki after the bomb had been dropped on the 9th August 1945, made a detailed report[1] of his findings concerning the explosive power of the two atomic bombs. The power of the Nagasaki bomb, according to Penney in his report, was calculated from his observations in the devastated city. It was about four times that of the Hiroshima bomb, namely 20,000 tons of T.N.T. This impressive assessment of the blast damage, made without the benefit of other than simple instrumentation, was gleaned from Penney's observations after he had toured both cities, and also,

> By studying "small things such as squashed tin cans, dished metal plates, (and) bent or snapped poles"[2].

Penney's report was crucial in determining Chadwick's subsequent recommendation to the A.C.A.E. that the more powerful and sophisticated plutonium bomb, rather than the uranium bomb, be developed in Britain.

On the 7th November 1945, Churchill, as Leader of the Opposition in a House of Commons debate, stated:

> This I take it is already agreed, we should make atomic bombs.

It is noted that the official decision to produce atomic bombs in the U.K. was taken in an ad hoc committee by Attlee in January 1947, but not announced in parliament until May 1948.

In the late autumn of 1945, Chadwick came back to London primarily for his investiture at Buckingham Palace. Aileen and James were formally received by the King on the 12th November 1945.

While Chadwick was in London during November, he lunched at the Athenaeum with Sir John Lennard-Jones (the retiring Chief Superintendent of Armament Research {the C.S.A.R.} at the Ministry of Supply) and suggested William Penney as the next holder of the post of C.S.A.R.

According to Cathcart[3], in his book on Britain's post-war quest for an atom bomb, it was,

> ... probably in November 1945, (that) Penney was approached by C.P. Snow, the novelist, who was a Civil Service Commissioner and an important influence on government scientific policy. Snow informed Penney that the post of Chief Superintendent of Armament Research (C.S.A.R.) was about to fall vacant and that if he applied he was certain to be appointed.
>
> Snow was not acting alone. Chadwick had already invited the out-going CSAR, Sir John Lennard-Jones, to lunch at the Athenaeum to put forward the case that Penney should be his successor. Lennard-Jones had known Penney at Cambridge, and gave his support. It is likely that Lord Cherwell, who was scientific adviser to Churchill, was also involved. These were some of the most influential voices in the country on matters of scientific policy, and they wanted Penney on hand, in a government post with research and development resources, ready to take part in the design and manufacture of the British bomb they thought inevitable.

Why was Penney selected? Cathcart continued, saying of the senior suitably experienced physicists:

> Chadwick was too exhausted by his wartime efforts and Taylor was probably thought too eminent for anything other than consultancy work. That left Rudolph Peierls, William Penney, Otto Frisch and Klaus Fuchs.

In January 1946, Cockcroft was formally appointed as the first director of the atomic energy research establishment (A.E.R.E.) situated on the site of an airfield, near Harwell, Berkshire. It was agreed that Cockcroft should assume his responsibilities under Lord Portal (who had been the war-time Chief of the Air Staff) who in turn reported directly to Attlee. Christopher Hinton, a senior I.C.I. engineer was at the same time appointed to design, and lead the team, to construct the atomic pile at Risley. (This had been discussed and decided at a cabinet meeting in December 1945[4].) Portal, meanwhile, set up the controlling headquarters complex in the Shell-Mex Buildings in The Strand, London, at the ex-home of Tube Alloys.

Michael Perrin, who was also from I.C.I. Ltd., was appointed by Portal to be his technical adviser.

As Cathcart has also written[5],

> One further figure had an important role at the outset: Sir James Chadwick. ... Portal, in the early months, leant heavily on him for advice.

The Radiochemical Centre was set up as part of Thorium Limited in October 1946 on a suitable site initially in Chilcote House at Amersham in Buckinhamshire, with Patrick Grove as the Chief Chemist. The Radiochemical Centre has issued *The Amersham Story*, a brief history of its development to 1980 in four parts, and in part one - *The Forties* - is written,

> ... the war years saw Chilcote producing other radioactive material for several of the remaining academic laboratories and neutron sources for the crucial determination of the neutron yield in the fission of uranium. Valuable contacts were made with eminent scientists and doctors, among them Professor James Chadwick, discoverer of the neutron and Nobel prize winner; they first met on the plane when Dr. Grove flew across the Atlantic in September 1944 to join the Anglo-Canadian Nuclear Project ... .
> Professor Chadwick gave powerful support to the idea that a national centre for radioactive materials production should be set up at Amersham;

(It is noted that, according to Gowing, P. Grove is not listed as part of the Manhattan Project personnel.)

The Chadwicks returned to Washington at the end of November 1945, and diplomatic relations with the Americans further declined - although the two events were unrelated! The three heads of government, Truman, Mackenzie King (the Canadian Prime Minister) and Attlee, had met in Washington on the 9th November 1945. On that date a compromise declaration was issued on U.S.A., U.K. and Canadian collaboration - see the previous chapter. But a parallel confidential meeting between Groves and (Sir John) Anderson,

> ... suggested that the still secret 1943 Quebec Agreement should be superseded by new arrangements agreed by the Combined Policy Committee[6].

Truman, (who was trying to prise control of the American atomic energy programme from the U.S. military, that is, away from Major General Groves), and Vannevar Bush, who was

> ... the most influential, nuclear scientist-politician in Washington[7]

did not however, in Chadwick's view, support the tripartite agreement reached. Secretary of State Byrnes raised further stumbling blocks to full collaboration with Canada and the U.K. after prompting by Groves. It was a condition of the new United Nations Charter that had been signed at San Francisco on the 26th June 1946 that all Treaties would be published. This action would reveal the secret dealings of the U.S.A., Canada and the U.K. during the war. Publishing details, for example, of the Quebec Agreement (as Groves had pointed out in the Groves/Anderson Memorandum mentioned above) and the (1944) Declaration of Trust would,

> ... provoke the wrath of the Soviets, and probably outrage the French[8].

Relations deteriorated and the flow of assistance on technical matters and collaboration with the U.K. almost ceased from the end of 1945.

Uneasiness amongst the scientists over moral issues concerned with the atomic bombs had already begun to surface, as in the Chadwick/Bohr/Dale discussion of 5th October 1944, and the Franck report of June 1945, previously mentioned. Chadwick forwarded a memorandum early in 1946, signed by all the British scientists from Los Alamos and Berkeley, to Prime Minister Attlee, among the objectives of which were complete international co-operation and inspection of nuclear stockpiles. The Association of Atomic Scientists, with H. Massey as Chairman, had been formed, as noted in Peierls's letter[9] to Chadwick dated 12th March 1946. There was a background of disquiet over the tremendous powers that were becoming available and much thought was given to the consequences of non-Allied Powers gaining atomic bomb information and technology. Some scientists, such as Nunn May and Fuchs, thought that all the Allied Powers should have the nuclear technology and it was this idealistic notion that allowed them to pass secret nuclear information to Soviet Russia. In Nunn May's case, there was no hint of financial gain.

It was announced on the 11th March 1946 that Chadwick was to be the U.K.'s Scientific Adviser to the United Nations' Atomic Energy Commission, and the alternate representative (on the Commission) to Sir Alexander Cadogan, the permanent British delegate.

The Americans, meanwhile, formed a strategic policy group for the forthcoming United Nations debate on Atomic Energy to be included at the Peace Conference, Paris, on the 1st August 1946, and at the General Assembly of the United Nations, which opened in New York on the 23 October 1946. Under-Secretary of State Dean Acheson was appointed Chairman of the Group, with Robert Oppenheimer as Consultant and David Lilienthal (head of the Tennessee Valley Authority Energy Consortium), together with other members including Tolman, Compton, Urey and Bac[illegible]. This powerful and authoritative Committee became known as the *Acheson-Lilienthal* Committee and backed the United States representative to the U.N. Atomic Energy Commission, Bernard Baruch, whom Truman had appointed on the 16th March 1946. Cadogan - with Chadwick - represented the U.K.

Chadwick received a communication from Sir Lawrence Bragg of the Cavendish Laboratory in early March asking him if he would accept the Jacksonian Chair at Cambridge. This Chair had been occupied by C.T.R. Wilson during Chadwick's time at the Cavendish and by Cockcroft until 1946 (although Cockcroft never actually took it up). As indicated in Chadwick's return letter dated 11th March 1946, he refused the offer, but he took just over two months to finally make up his mind.

April 1946 was a significant month for Chadwick. Bush had not supported Britain in atomic energy matters at the Combined Policy Committee as he had promised. At its April meeting, it was left to Chadwick and Groves to allocate and devise a method of sharing the uranium ore from the Congo so that Britain could start its own uranium programme. The British allocation of refined uranium and the subsequent delivery of refined ore that was negotiated by Chadwick (much to Groves's annoyance!) allowed Britain to make an early start on its nuclear programme. Chadwick also wrote to Bretscher on the 27th April 1946 at Los Alamos. In this letter he asked Bretscher, who was working on the hydrogen bomb (the 'super'), to bring back to the U.K. details of anything of interest that he could get. In the event, details of deuterium-tritium reactions that Bretscher had been working on were personally delivered by Klaus Fuchs two months later to Chadwick in Washington! Fuchs, in contravention of strict American security procedures, had been asked by the British to smuggle out from Los Alamos highly secret information. This, with hindsight, and in the later knowledge of his treachery, was an irony that was probably not lost on Chadwick. A lot of information came to Britain from Los Alamos. As the British team left site Y over the period 1945 to 1947, most of them returned with considerable details of their personal work and a general overview of the whole programme.

The information on Rotblat's contribution to the Manhattan Project which was contained in letters and documents were packed for despatch to the U.K. just prior to his departure in December 1944. The box never reached the U.K. and, according to Rotblat in his interview with Brown[10], it was intercepted by the U.S. security forces.

Meanwhile, Chadwick had made a request to the University of Liverpool Senate to enable a Chair of Theoretical Physics to be established. He received a cable from the Registrar, Stanley Dumbell, to say that this request had been approved by the University Grants Committee during May 1946. In the cable he was asked to approve the choice of Erwin Schrödinger (of the Institute for Advanced Studies in Dublin) for the Chair. But, Rotblat was able to collate a number of reports about Schrödinger which suggested that he would not be an ideal choice to set up a sub-department of Theoretical Physics[11]. Chadwick accepted this advice and offered the Chair in Theoretical Physics to Herbert Fröhlich from Bristol University. Fröhlich accepted Chadwick's offer with the proviso that the sub-department became a separate entity under his control. This was agreed and Fröhlich built up the Theoretical Physics Department where he remained Head until his retirement. Shortly after his arrival in Liverpool, Fröhlich's interests turned from nuclear to solid state physics.

Chadwick returned to Liverpool in the summer of 1946 when he was 55 years old. He was exhausted by his war-time efforts.

He had kept in touch with university business during his absence through correspondence with his friend Sir Arnold McNair, the University Vice-Chancellor, and also with the Registrar, Stanley Dumbell. He had received progress reports from the Physics Department on a regular basis as well as reports from other U.K. universities involved with Tube Alloys work. He was very much aware of the state of nuclear physics in British universities as WW2 ended.

It was an opportune time for Chadwick to return to the U.K. In August 1946, the final McMahon bill was passed, which indicated the hardening of Anglo/American cooperation in atomic matters and, also, that the Quebec Agreement was being largely ignored by the Americans. As previously mentioned, Chadwick was the Chairman of the Nuclear Physics Sub-Committee (N.P.C.) and he attended a meeting on the 12th November 1946, the purpose of which was to co-ordinate research work in the universities and to oversee the relationship between them and the Atomic Energy Research Establishments. (The members of this committee, a sub-committee of the Advisory Committee on Atomic Energy, comprised Blackett, Cockcroft, Thomson, Oliphant, Dee, Peierls, Feather and C. Darwin.)

One result of this N.P.C. meeting was the decision that, provided funding was approved, Liverpool University should, along with A.E.R.E. Harwell, construct large cyclotrons for research purposes. In the middle of November, Rotblat took the recommended Liverpool request for funding for a proposed 120" cyclotron to the D.S.I.R. in London. This was approved by them and they authorised a grant of £200,000 over five years. A location plan of the area of the original Liverpool Roman Catholic Cathedral, and a dotted outline of the proposed new Physics building to house the 120" cyclotron, has been located in the Physics Department and dated December 1946[12].

However, Rotblat was later to ascertain that steel slabs of up to 13 feet (156") in length could be machined in the U.K. and, as this would enable the magnet pole diameter to be increased from 120" to 156" (and therefore correspondingly increase the proton energy output to 400 MeV), he successfully applied again to the N.P.C. for an increased building grant. The N.P.C. approval was given on 1st December 1948 at the N.P.C. number 17.

On the 6th November 1946, Chadwick formally opened a travelling exhibition, at the Central Railway Station, Liverpool, called the Atomic Train. Rotblat organised this exhibition under the auspices of the Atomic Scientists Association, and it contained a number of demonstrations with general information on the peaceful uses to which atomic energy could be put. A handbook[13] was available to all who visited the exhibition.

The American Atomic Energy Commission came into being on the 1st January 1947 and Chadwick was recalled by Cadogan, the permanent U.K. delegate to the United Nations. Chadwick sailed on the *Queen Elizabeth* to New York. While he was there, he learnt that the problem of the patents taken out by the French at the start of WW2 was again being raised by the Americans. He suggested in a memorandum to the Government Cabinet in London that caution in British relations with France over this issue should be exercised in order not to upset the Americans. He returned to England in February 1947.

Chadwick was awarded an Honorary Doctor of Laws from the University of Liverpool in July 1947, and work started on the buildings for the synchrocyclotron in the area of land indicated on the above-mentioned location map.

Early in 1948, Chadwick was asked to become the Vice-President of the Royal Society and he accepted the position without appreciating the attendance requirements for meetings. He found he was unable to complete the two-year Vice-Presidency as he could not fulfil the attendance requirements, and he resigned the following year.

At the end of 1994, this author located a file[14] which throws new light on the design and construction of the Liverpool University 156" synchrocyclotron and its associated equipment. Four design discussion documents are contained within the file and are dated between the 10th February and the 13th July 1948 for the 156" magnet, equipment and auxiliary apparatus. It also contains the first 30 sets of minutes of the University of Liverpool Nuclear Physics Research Laboratory Committee spanning the time period from 13th July 1948 to 16th December 1952.

In April 1948, F.J.M. (Chubby) Stratton, the President of the Council of Fellows of Gonville and Caius College, Cambridge University, was asked to contact Chadwick and to find out whether he would accept the Mastership of Caius. The retiring Master was John Cameron, Master from 1928, succeeding Sir Hugh Anderson. Chadwick, in an effort to repay what he felt was his debt of gratitude to the College, and after discussions with Mountford, the Liverpool University Vice-Chancellor agreed to accept the Mastership. On the 18th May 1948, the Fellows of Gonville and Caius College formally elected Chadwick to the Mastership of the College. Chadwick resigned from the University of Liverpool and took up his new duties in Cambridge at the start of the first term of the 1948/9 academic year. Although Chadwick had left Liverpool, he did not sever all ties and retained a keen interest in the development of the new Physics Laboratories.

The members of the Nuclear Physics Committee, on hearing of Chadwick's departure from Liverpool to take the Caius Mastership, voiced their concerns over the building of such a state-of-the-art cyclotron machine in Liverpool. The minutes show that[15],

> If the instrument (the cyclotron) is erected at Liverpool, it may fetter that University in its choice of Professor for generations to come....
> The Committee agreed, ... ,
> (iii) that plans to erect it at Liverpool should proceed until the appointment of the new professor there is announced, when the location of the machine should be re-considered.

H.W.B. Skinner, who had been a member of the Manhattan Project and was then a Chief Scientist at A.E.R.E. Harwell (see Appendix 8), was invited to take the Lyon Jones Chair at Liverpool University. Skinner accepted the offer and, as the minutes of the N.P.C. show[16],

3. The appointment of Dr. H.W.B. Skinner to the Chair of Physics at Liverpool has now been announced, and the Committee should therefore proceed, in accordance with its earlier decision to reconsider the location of the machine.
4. It is thought rightly that this matter can be settled by correspondence, without a meeting of the Committee, ...

Skinner was appointed from the 1st October 1949[17] but did not arrive at Liverpool until January 1950. Rotblat accepted the Physics Chair at St.Bartholomew's Hospital with effect from October 1949 but he, too, delayed taking up his Chair until early 1950.

In the event, the synchrocyclotron was built at Liverpool University[18] under Chadwick (to October 1948), Rotblat (just over one year from October 1948) and then Skinner; the machine first having beam in April 1954. The experimental physics programme commenced in February 1955.

Chadwick, as Master at Gonville and Caius, continued his scientific administrative role in committees, and sub-committees (or Panels of the N.P.C. as they were known), such as the Photographic Emulsion Panel, Cyclotron Panel, Isotope Panel and Photo-Multiplier Panel, and which reported directly to the N.P.C.

Russia exploded its first atomic bomb in August 1949 making the Western world realise that there were no atomic secrets and that the concept of a nuclear explosive was common knowledge - it was the technological and engineering aspects that were so difficult to complete.

Klaus Fuchs was unmasked as a nuclear spy in February 1950 and it was eventually discovered the enormous help that Fuchs had given to the Russians. This help had considerably shortened the time that the Soviets had taken for them to construct an atomic bomb.

Britain became an independent nuclear power on the 3rd October 1952 when her first atomic bomb was exploded at Monte Bello Island.

European countries were beginning to cast envious eyes towards the U.K. and were desirous of recommencing nuclear research. Due to the ever increasing costs of building large machines such as the Liverpool synchrocyclotron, collaborative discussions were started on the Continent. To Chadwick it appeared there was no advantage for the U.K. in joining a European Consortium, but European scientists were made welcome at Liverpool.

The United Nations Organisation in October 1946 first discussed the idea of European atomic co-operation after the French Delegation had submitted a draft resolution. In Chapter 2 of the *History of CERN*, Pestre writes[19],

> In October that year the French delegation to the UN Economic and Social Council submitted a draft resolution inviting the "Secretary General to consult UNESCO and the other specialized Agencies concerned and to submit to the Economic and Social Council if possible during the next session, a general report on the problem of establishing UNITED NATIONS RESEARCH LABORATORIES."

In the following few years after the Second World War, Lew Kowarski, Jules Guéron, Denis de Rougemont - an authoritive Swiss writer - Pierre Auger and Raoul Dautry, amongst many others who had returned to their pre-war homes and laboratories in leading nuclear scientific roles, thought that a major laboratory could be established in which all European countries could participate. This was part of an attempt to unite Europe, by inter-country collaboration, and by the organisation of a general European Movement. There was set up for example, the Organisation for European Economic Co-operation (O.E.E.C.), formed early in 1948, and one year later the Council of Europe. It transpired that by 1951 there were two views on a European Nuclear research Laboratory.

The first viewpoint was spearheaded by Pierre Auger who, at a Geneva meeting in 1950, suggested an ambitious new research programme. He was supported by Isadore Rabi, an American Nobel prize winner. The second viewpoint was put forward by Hendrik A. Kramers, a Dutchman, while visiting Bohr in Copenhagen. Chadwick was also visiting Bohr that August of 1951. Their concensus was that a new European Laboratory be annexed to an existing Institute, and it was thought that the Bohr Institute would be suitable.

A Paris meeting of Government scientists to consider a European Laboratory was scheduled for December 1951 and the British representative should have been Chadwick. He was unable to attend and Sir George Thomson went instead. A compromise was reached at this meeting. The result was that the Liverpool University synchrocyclotron would be available (as soon as it was completed) for Europeans wishing to make use of a high energy machine - provided the University authorities agreed - and a European effort would be based in Copenhagen. At the same time, plans would be made for an International Nuclear Centre.

Bohr went to Cambridge and spent a few days with Chadwick and there they discussed their European policies, prior to a meeting with the Liverpool authorities.

> A meeting with Mountford, Skinner, and other university authorities was arranged for 9 and 10 January (1952). 'Perfect agreement' - to quote Skinner - was reached between the parties, in what Bohr called 'very pleasant and I think fruitful discussions'[20].

Later in January 1952, Chadwick was asked to chair a new committee set up under the auspices of the Royal Society, and titled the Advisory Committee on the Nuclear Research Centre (A.C.N.R.C.). Its remit was to provide recommendations and advice to the British U.N.E.S.C.O. Committee on developments around the European Project. The membership comprised Thomson, Cockcroft, Ben Lockspeiser (for the D.S.I.R.) and Skinner (for Liverpool University), and met for the first time on the 23rd January 1952.

It is noted that the offer of the use of the Liverpool synchrocyclotron to European scientific collaborators was an offer in kind and not a financial contribution, as the A.C.N.R.C. stated in their report[21]:

> Consideration was given to the terms on which this country could become a member of the Council of representatives by making a contribution in kind, the form of contribution being the provision of facilities on an international basis for work with the synchro-cyclotron at the University of Liverpool.

The Committee also resolved to request funding, through the Ministry of Education, for help in the running costs of the Liverpool machine and maintenance of foreign scientists at Liverpool or British scientists at Copenhagen. It appears that the willingness to participate in European scientific collaboration stems from this time.

The political committees that finally decided U.K. policies in Europe were the D.S.I.R.'s Committee on Overseas Scientific Relations (O.S.R.), and the Cabinet Steering Committee on International Organisations (I.O.C.). This latter was 'a high-level interdepartmental body advising ministers'.

On the 15th February, the Agreement constituting the Council of Representatives was set up in Geneva and signed by nine delegations excluding the U.K. The Advisory Committee, chaired by Chadwick, met again on 27th May 1952 and it recommended that Chadwick be invited to put forward the scientists' view to the I.O.C. On the 11th June at the Steering Committee, Chadwick's views were taken on board and a recommendation to sign the Geneva Agreement was made. This recommendation was finally turned down by the Chancellor of the Exchequer, R. Butler.

Cockcroft arranged a conference at Buckland House near Harwell that was held on the 7th June 1952, and the consensus of the eminent and generally more youthful scientists, (James M. Cassels, Pickavance, John Fry, for example) who attended and presented papers, were strongly in favour of European collaboration in a high energy laboratory project. Chadwick, although invited, did not attend. However,

> On the 11 June, so (presumably 'some') four days *after* the Buckland House meeting, Chadwick told the Cabinet Steering Committee that Britain's physicists were against the building of large cyclotrons "because during the next four or five years our available resources should be of sufficient range for adequate research"[22].

The general opinion at Buckland House had been just the opposite!

A meeting at the Cavendish was convened on the 1st November 1952 and it was the fourth and last meeting of the Chadwick Advisory Committee. Fourteen others were present including the other three scientific members, Cockcroft, Skinner and

Thomson, and eleven more by invitation. These invited scientists included Blackett, Cassels, Fry, Goward and Pickavance. The minutes state,

> i) that it was the unanimous view of all present that it was necessary for the progress of British nuclear physics that British nuclear physicists should have access to a proton synchrotron of the size and character projected by the European Council for Nuclear Research;
> ii) that it was the unanimous view of all present that i) could best be achieved by the United Kingdom cooperating in the European scheme and signing the agreement with the European Council for nuclear research; and
> iii) that it be a recommendation to Council that the Royal Society propose to the Treasury that, in view of the contributions in kind to be made by this country to the European scheme, an annual commitment of £250,000 for eight years be undertaken as the British contribution to the European Council[23].

In the event, and after further Governmental considerations, the Chancellor endorsed the U.K. application towards the end of 1952. Full ratification of the Convention establishing C.E.R.N. was signed on 1st July 1953 by Sir Ben Lockspeiser on behalf of Her Majesty's Government, although Cockcroft and Lockspeiser, representing H.M.G., were the first U.K. official observers at the fourth session of the provisional C.E.R.N. Council held in Brussels from 12th to 14th January 1953.

After the scientists' unanimous consensus minuted at the fourth Chadwick Advisory Committee meeting, Krige, writing in the first volume of the *History of CERN* concerning Chadwick's initial reluctance to support the high energy European accelerator project, says,

> what is striking about Chadwick, for example, is how constructive he immediately became[24].

Chadwick, until that time, could be seen as a somewhat aloof father figure and it appears that, in this instance, he had misjudged the mood of his fellow scientists. But, in a number of later incidents, he was able to give guidance to his colleagues and senior politicians.

After the political intrigues of the start up of C.E.R.N., Chadwick and Aileen made a relaxing tour of Canada and, while at the University of Toronto, he gave a lecture[25] outlining the prospects of electrical power from atomic energy.

Chadwick, after taking up the Mastership in 1948 had. amongst many other things, been quietly and diligently putting the Gonville and Caius's investment and financial affairs in a more healthy state. He had taken over the Bursarship when the Bursar was seriously ill, as well as the office of Master. The steady improvement in the financial position was not lost on the College Fellows and the younger, more radical, Fellows thought that they should share in the improving situation. The influx of the more worldly wise and ex-service under- and post-graduates probably exacerbated this feeling and Chadwick's introverted manner did not help the situation. He did what he did to the best of his considerable ability. But he did not always communicate to others in the College the actions that he had taken.

There were a number of instances of the Fellows going directly against his wishes. An early incident, for example, was in a Council of Fellows meeting on the 6th October 1950. The election of Fellows for the Council was due to take place and Chadwick had hoped to have H.E. Tunnicliffe elected - a senior and well respected Fellow. However, the Council fielded two younger candidates, Michael Swann (later Lord Swann) and Peter Bauer - 'farmer' in German but with the implication of a peasant farmer - (later Lord Bauer). The latter two were elected. This started what became known as the "peasant's revolt".

Later in October 1950, Chadwick was proposed as the Vice-Chancellor of Cambridge University. The term of office would be for a two year period from 1951 to 1952. He accepted the office but, due to personal worries over his health, he travelled back to Liverpool to consult Lord Cohen, his friend and physician. Cohen advised him not to take 'a high-profile position of enormous responsibility'[26], and on his return to Cambridge he asked to withdraw from the office.

Opposition and criticism of the manner - most definitely not of the actions that Chadwick took - and the way in which decisions affecting the College were taken, climaxed in July 1958 over a number of relatively small matters. As Chadwick said in his interview with Weiner[27],

> I resigned; I didn't retire. That was a sad business, but I didn't like some things that were happening. I don't want to say more than is absolutely necessary. What it amounted to was that there seemed to be a desire to limit the powers and responsibilities of the Master. They were laid down by the statutes, but they could be interfered with in some ways, and I didn't want to make any trouble, and so I resigned, an almost unheard of thing.

He vacated the Master's Lodge in December 1958 and the Chadwicks retired to Wynne's Parc, near Denbigh, North Wales. Nevill Mott, succeeded Chadwick as Master of Gonville and Caius College, taking up his duties in February 1959.

Chadwick's retirement enabled him to indulge in some of the outdoor pusuits that he enjoyed, namely, a little gardening and fly fishing for trout. But he had his retirement interrupted when the University of Liverpool asked him to open the new physics teaching laboratory, comprising laboratories and lecture theatres, named the Chadwick Laboratory in his honour. This he did in October 1959 and the University published a commemorative booklet describing the event[28].

In 1961, Chadwick attended two major functions. The first, in September, was a conference celebrating the 50th anniversary of Rutherford's hypothesis on the structure of the atom. This was held in Manchester and was also attended by Niels (and Aage) Bohr, E.N. Da Costa Andrade and C. Darwin amongst others.

The second occasion was a celebration of his 70th birthday, held at the Cavendish Laboratory in October, and where Sir James and Lady Aileen were Guests of Honour.

Chadwick's health did not improve as he grew older and when, in 1962, he was invited to a Symposium at Cornell University, Ithaca, New York, on the thirtieth anniversary of the discovery of the neutron, he was unable to attend due to ill health. He did, however, prepare a text titled *Some personal notes on the search for the neutron*[29].

Chadwick's lifelong friend and colleague, Niels Bohr, died in 1962. Aage Bohr wrote to Chadwick in December 1962, telling him[30] 'how highly his father had valued Chadwick's friendship'.

This author has read, in the course of research for this thesis, a number of books peripheral to the subject, amongst which was *The Griffin* by Arnold Kramish[31] . This book throws up an intriguing and previously un-noticed link between Chadwick and a spy! It gives a speculative insight into the espionage in WW2 by a Nazi agent, working for the Allies, known as the Griffin. Kramish set out to prove that the Griffin was, in fact, one Paul Rosbaud.

Chadwick, who edited three volumes of Rutherford's collected papers[33], mentioned in the preface to Volume 1 that 'Paul Rosbaud' had initially suggested to him the compilation of the papers. It is a fact that the Rosbaud mentioned in both books are one and the same person, and Chadwick would therefore have known him quite well. How long Chadwick had known Rosbaud, if at all, prior to their collaboration in the editing of Rutherford's papers is not known and is open to question as is the reliability of all parts of *The Griffin.*

However, *The Griffin* was reviewed in *Nature*[32] by R.V. Jones and he states in his review that Rosbaud was not the author of the 'Oslo Report' (a highly informative document smuggled out of Norway and concerned partly with nuclear information), nevertheless, that

> Kramish has performed a welcome service in ensuring a wider appreciation of those genuine and important contributions that Rosbaud so courageously made.

(It is noted that Jones was the Member of the Air Staff responsible for scientific intelligence during the Second World War.)

Related to this is the whole question of Chadwick's involvement in security matters during World War Two and later; it is difficult to assess due to the secret nature of the processes involved. There are still a number of retained and classified files by the U.K. and American Security Services which may eventually throw light on some of these questions.

The Chadwicks returned to Cambridge in 1968 after 10 years at Wynne's Park. In

V. Bowden A. Merrison J.D. Cockcroft P.I. Dee J.M. Cassels
E.T.S. Walton J. Chadwick Lady Chadwick B. Flowers E. Bretscher

L.F. Bates O.R. Frisch

J. Rotblat
F. Jackson Mrs. Jackson R.W. Ditchburn D. Shoenberg
W.A. Wooster W.E. Burcham K.G. Emelius R.G.P. Voss
M. Crowley Milling A.J. Egginton Miss Lloyd Jones J.R. Holt
B. Zacharov W.T. Davies C.W. Gilbert H. Rothwell
M.J. Moore
T.G. Pickavance D. Wilkinson

<u>Photograph 8.6.1</u>
Chadwick's 75th Birthday Celebration at Liverpool University
20th October 1966

the 1970 New Year's Honours List, it was announced that Chadwick would be awarded the Companion of Honour, and he attended his investiture in the spring of the same year.

The University of Liverpool gave a celebration dinner for Chadwick's 75th and for his 80th birthdays. A photograph is shown opposite. It was thought by a number of the persons attending how much he enjoyed meeting again with his colleagues and friends at Liverpool[34]. At the celebration marking his 75th birthday, Chadwick was presented with a scrapbook containing signed photographs and compliments by many of his friends and colleagues at home and abroad whom he had known during his career. In an interview at Cambridge on the 9th September 1996, Chadwick's daughters Joanna and Judith kindly showed the author the scrapbook which is now a treasured family possession.

Sir James Chadwick died on the 24th July 1974 and Lady Aileen Chadwick passed away on the 25th October 1986.

References:

6.1. Penney, W., *Report on the Atomic Bomb Damage to Hiroshima and Nagasaki* (1945), Chadwick Archive, Churchill Archives Centre, Churchill College, Cambridge.

6.2. Brown, A.P., *The Neutron and the Bomb: A biography of Sir James Chadwick*, Oxford University Press, 1997, p. 305.

6.3. Cathcart, Brian, *Test of Greatness; Britain's Struggle for the Atom Bomb*, Murray, 1994, p. 39-40.

6.4. Public Record Office, *Cabinet Papers*, Number GEN.75, 1945.

6.5. *ibid.* 6.3., p. 16.

6.6. *ibid.* 6.2., p. 310.

6.7. *ibid.* 6.2., p. 311.

6.8. *ibid.* 6.2., p. 312.

6.9. *Letter*, Peierls to Chadwick, 12th March 1946, Chadwick Archive, Churchill Archive Centre, Churchill College, Cambridge University.

6.10. Rotblat, J.; Brown, A.P., *Interview*, 7th January 1994, London, transcribed by Brown.

6.11. *ibid.* 6.10.

6.12. King, C.D., *The Design and Construction of the Liverpool 156 inch Synchrocyclotron (1945-54)*, M.Sc. Thesis, University of Liverpool, 1993, pps. 15 and 16 (including photocopies opposite).

6.13. *Atom Train - Guide to the Travelling Exhibition on Atomic Energy*, admission price 6d, (2½p), Chadwick Archive, University of Liverpool.

6.14. *Minutes of the University of Liverpool Nuclear Physics Research Laboratory Building Progress Committee, 10th February 1948 to 16th December 1952*, Chadwick Archive, University of Liverpool.

6.15. N.P.C. 18, *Minutes of the Nuclear Physics Committee*, 27th January 1949, Chadwick Archive, University of Liverpool.

6.16. N.P.C. 26, *Minutes of the Nuclear Physics Committee*, 4th August 1949, Chadwick Archive, University of Liverpool.

6.17. Edwards, D.N., *Liverpool Who's Who in Physics (1881-1996).*

6.18. *ibid.* 6.12.

6.19. Hermann, Armin; Krige, John; Mersits, Ulrike; Pestre, Dominique; *History of CERN*, Volume 1, Oxford, North Holland Publishing Company, 1987, p. 64.

6.20. *ibid.* 6.19, p. 454.

6.21. *ibid.* 6.19, p. 455.

6.22. *ibid.* 6.19, p. 479.

6.23. *ibid.* 6.19, p. 493.

6.24. *ibid.* 6.19, p. 500.

6.25. Chadwick, James, *Prospects for Atomic Power*, published as a pamphlet, University of Toronto Press, 1953.

6.26. *ibid.* 6.2, p. 348.

6.27. Tape recorded interview, Chadwick and Charles Weiner, 15th-21st April 1969, Neils Bohr Library, Center for History of Physics, American Institute of Physics, Page Chadwick-118.

6.28. Chadwick Laboratory, Department of Physics, University of Liverpool, *Open Day Booklet*, October 1959.

6.29. Chadwick, James, handwritten text, *Some personal notes on the search for the neutron.* u.d., Chadwick Archive, University of Liverpool.

6.30. *ibid.* 6.2, p. 358.

6.31. Kramish, Arnold, *The Griffin*, Houghton Mifflin Co., 1986.

6.32. Jones, R.V., 'A Merchant of Light', Book Review, *Nature*, **325**, 15th January 1987, p. 203-4.

6.33. *The Collected Papers of Lord Rutherford*, edited by James Chadwick: Volume 1, 1962; Volume 2, 1964; Volume 3, 1965.

6.34. Holt, J.R., "Sir James Chadwick at Liverpool; Personal Reminiscences of the Nobel Laureate with Archive Film", *History of Science and Technology Seminar*, 8th February 1988, University of Liverpool.

CHAPTER 7
Conclusions, Discussion and Analysis

This author has found many new facts concerning Chadwick's involvement in obtaining the necessary information required to build an atomic bomb.

It has now been established in this thesis that Chadwick and his team obtained the nuclear fission cross-sections for uranium on the Liverpool 37" cyclotron. There is corroboration from three sources, namely,

i) the Monthly Reports

ii) the 37" Cyclotron Log Books

iii) the recollections of and publications by staff who were involved in making the fission cross-section measurements at Liverpool and elsewhere.

The information obtained from the experimental results taken on the 37" cyclotron was a major contributing factor to the final Maud Report which was written almost entirely by Chadwick. This Maud Report, a feasibility study on the possibility of manufacturing atomic bombs, was sent to the U.S.A. in 1941. It reinforced American scientific opinion and was instrumental in securing their involvement in nuclear bomb research before the attack on Pearl Harbour. The Maud Report helped persuade them to devote their industrial power to develop an atomic bomb as a matter of urgency.

It has been shown that just prior to the start of the Second World War there had been open scientific and popular press coverage of the amount of energy, both explosive and controlled, that could be obtained from nuclear fission. All states including America, Germany, Japan and Russia had access to this information. There was in 1939/40, therefore, a 'level playing field' in the knowledge of the power available from fission to all the countries involved in the hostilities, and this knowledge compounded the sense of urgency in development of the Allied atomic bomb.

The German scientific community between 1939 and 1945 did not attempt to make an atomic bomb, believing that it could not be developed in their forseeable future, whereas it is shown that the Japanese were, compared to the Germans, at a more advanced level in atomic bomb research.

It has also been shown that secrecy and security were stricly applied to atomic bomb development and manufacture, particularly in the U.S.A., but not applied so rigidly to the British laboratories. Photographic evidence of various research projects concerned with atomic bomb research at Liverpool University, taken by young staff members, is strong evidence of a more relaxed approach in the U.K.

Chadwick was involved in the security surrounding the British personnel who joined the Manhattan Engineering District Project, but to what extent this involvement went has not been ascertained. It is known that he had a close collaboration with Rosbaud in his later years and who has now been exposed as an important spy for the Allied cause in Nazi Germany during World War Two. Chadwick gave evidence to the British Security Service about the trustworthiness of the 'enemy and foreign aliens' who worked for him at Liverpool University and vouchsafed for the many British personnel working on the Manhattan Project. He advised the American Leader of the Manhattan Project, Major General Groves, on the selection, expertise and suitability of the British staff taken on for research in Canada and the American nuclear research laboratories.

In the research necessary for writing this thesis, it was found that there is much evidence, as portrayed in letters and his dealings with people, that Chadwick was an extremely shy and retiring person. He would not seek to gain, in his scientific and his later administrative career, the prominence that his position could have given him. It appears that he was the right man, in the right place and at the right time (but see later). Churchill did a superb job of leading the British people during the Second World War, and Chadwick, in this author's opinion, also did a superb job of leading the scientific community in the same time period. Chadwick was instrumental in keeping the collaboration between the American and British during the War on a harmonious footing and, through his diplomatic skills, was able to put Britain in a more knowledgeable and better resourced position than might otherwise have been the case.

Chadwick, as the War was ending, also played a major role in the selection of staff and formulation of policies that enabled Britain to re-commence nuclear research. Evidence of his initial involvement in setting up the Harwell research facility, the Risley Reactor and the Amersham Radiochemical Establishment has been given. His judgement and advice were sought in initial senior staff selection and there has also been shown the significant part played by Chadwick in the European scientific collaboration that is now so significant in the 'big science' of today's major research laboratories.

The willingness of the Liverpool University Authorities to support both the partial funding of the original cyclotron and Chadwick's research team, together with the University's post-war support of the synchrocyclotron and laboratory buildings for it, was of immense help to Chadwick and the Physics Department Heads who succeeded him. The Physics Department that had started out in 1935 as a 'provincial university' department became, under Chadwick's initial leadership, a major European Nuclear Research Laboratory. It became a facility of such importance that it was used for barter by the U.K. Government in order not to spend money in European scientific participation.

From the mid 1950s, many European and American scientists were able to do research on the high energy synchrocyclotron that had been built at Liverpool.

It has been stated above that Chadwick was 'the right man, at the right time and the right place'. The justification for this statement may be shown in three instances:

The first instance was when he found the neutral particle, the neutron. It so happened that his radioactive source was of the necessary strength and that the apparatus he had to hand was suitable for the verification experiments. He had previously thought deeply about the neutron and how it might be recognised. The practical things plus, of course, the mental acumen to see the implication of published works which others did not, allowed Chadwick to make his discovery.

The second instance has been described at length in this thesis, in that Chadwick was able to build the 37" cyclotron, with Lawrence's full support, and have it ready at the outbreak of World War Two. Chadwick was the only person at that time in the U.K. with the necessary apparatus that could be exploited to verify the figures given in the Frisch-Peierls Memorandum. Chadwick, therefore, was once again in a position where he could play an important scientific role. He had the ability, the apparatus, the measuring devices and the staff that enabled the figures to be obtained.

The third example occurred when Chadwick was in the U.S.A.; Akers was not thought tc be acceptable to the Americans. It was Chadwick who was available in Washington, known to the Americans and who was thought capable of taking Akers's place at least on a temporary basis. Chadwick was later established as the Chief British Adviser to the Combined Policy Committee and Head of the British Mission to the U.S.A.

This author must stress that it is not intended to down-play any of Chadwick's great achievements, but rather to emphasise his ability to take the opportunities that came to him. (It is interesting to speculate on whether it was Chadwick's overriding desire to keep his hard-won cyclotron running, or his belief that the fission cross-sections could be measured, which was uppermost in his mind when he wrote to Appleton on Boxing Day 1939!)

Chadwick's period at Cambridge after leaving Liverpool, was not a completely happy time for him. What he did at Cambridge, again in a quiet and dignified manner, was to re-establish the finances of the College to such an extent that a significant number of his College's members wanted to enjoy the benefit of the newly accumulated finances. It is a very creditable achievement to turn around the financial status of a large organisation such as a Cambridge College and put it on a viable financial footing.

It has been shown how, in the first three chapters, Chadwick's early career advanced under Rutherford until he took advantage of the vacated Liverpool University Chair to build a state-of-the-art cyclotron. Chapter 4 has shown the various experiments that were undertaken during the War on the machine which gave a better understanding of the nuclear parameters required to consider the manufacture of an atomic bomb. Chapter 6 has shown how Chadwick's career, through the circumstances of the time, changed from experimental physicist to that of a scientific adviser.

A number of the staff that Chadwick selected to work for him went on to distinguished post-war careers. The way in which Chadwick trusted the 25-year-old Pickavance to go to the U.S.A. to obtain information on building mass spectrometers is a prime example of selection and trust. Pickavance went on to lead the team at Liverpool and his career developed from there - see the Staff Biography.

A final note may be written about Chadwick's character. It appears that he was a hypochondriac and a very private person. His daughters say he was an excellent, loving and considerate father. During their teens, he became pre-occupied with the work on which he was involved but he was very much a family man. He was introverted in the sense that he was at ease only in company with the few friends that he had come to know well, and was not comfortable in public speaking or debate. He was a conservative, with a small 'c', and did not project himself as a leader. He became a leader prior to and during the War because of the thoughtful, honest and wise information he was able to give. He did not waste words, and each word or phrase he spoke or wrote was the most suitable for the event. This author regrets that he did not have the opportunity to meet him.

Appendix 1
THE CYCLOTRON, OR MAGNETIC RESONANCE ACCELERATOR

If a charged particle, such an ion or electron, is travelling perpendicular to a uniform magnetic field, the particle will be constrained and tend to move in a circular orbit. If the particle were to come under the influence of an electric field, i.e., a voltage difference between two electrodes, the positive ion is attracted to the negative electrode and repulsed from the positive electrode. Similarly, an electron is attracted to the positive electrode and repulsed from the negative electrode.

E. Lawrence and N. Edlefson, in *Science*, **76**, 1930, p. 376, made use of the above properties of charged particles in, firstly, proposing, and then building, a device in which particles, ions for example, were injected into a magnetic field constraining them to move in circular orbits between the poles of the magnet. In a divided cylindrical chamber positioned between the poles, a voltage was applied first to one side of the chamber and then the other side, alternating at a very high frequency. Provided the frequency of the applied voltage and the time that the ion took to travel one half an orbit was known, it was possible to give the ion an accelerating electrical 'kick' each time it passed between the halves of the divided chamber. As the ion was made to traverse that electric field many times, the ion's velocity was increased many times, thus giving to it more and more energy. The higher energy which the ion now had could then be utilised in allowing it to collide with a 'target' and the results of the collision analysed. The above device, which enabled particles of high energy to be used experimentally, was called a cyclotron,schematic diagrams of which are shown below.

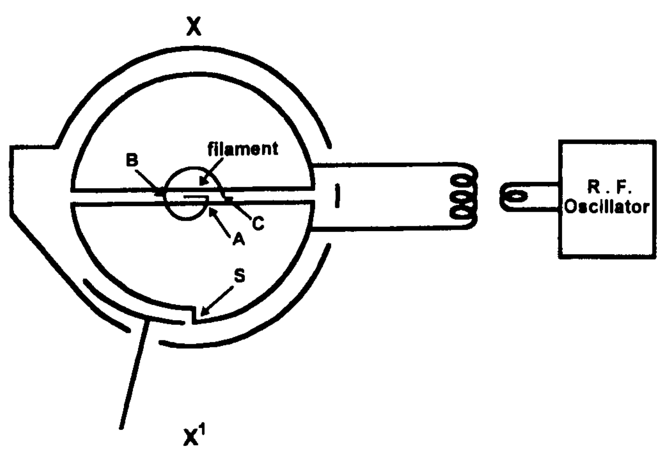


Fig. 1: Plan View

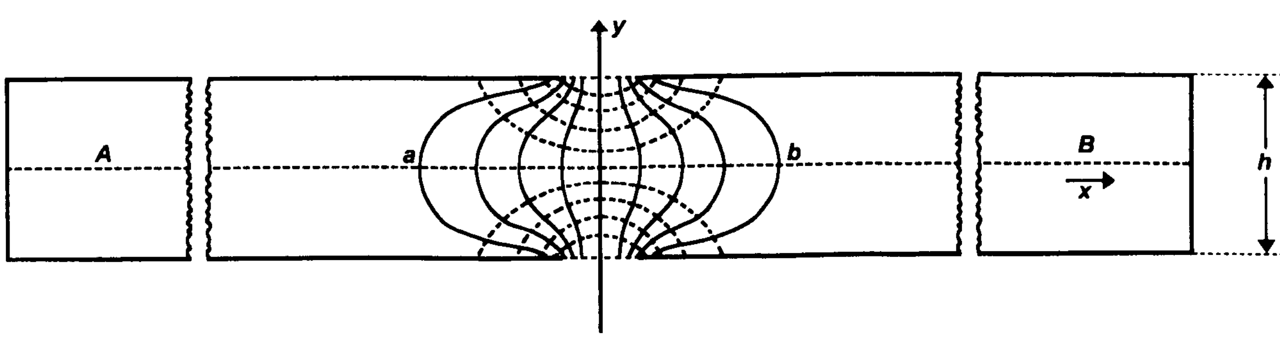


Fig. 2: Section of dees through A parallel to XX (Fig. 2).
[Full lines, including dee sections, represent equipotentials, broken lines represent electric lines of force.]

In the plan view of Figure 1 above, the two halves of the chamber, commonly called 'dees' because of their shape, have voltage applied from a radio frequency (RF) oscillator, the two dees forming part of a resonant electrical circuit. The RF electric field is concentrated in the gap between the dees, and the magnetic field is normal to the plane of the paper.

Now, if a positively charged ion is in the gap between the two dees (position A2), it will be accelerated to a dee and will pass into the electric field free region within the dees (being unaffected by changes in the electric field in the dee gaps). However, the ion will, within the dees, be influenced by the magnetic field and will trace out a semi-circular path emerging at the gap to come once more under the influence of the accelerating electric field. Provided the time the ion spends within the dee is equal to half the period of the oscillating electrical field, then the ion will again receive an accelerating voltage - region B in Fig. 2. The ion is still constrained to move in the semi-circular path by the magnetic field and will emerge into the gap at C with the electrical field then ready to accelerate the ion once more. This procedure is repeated many times.

At first sight it would seem that very high velocities could be imparted to the ions, but Einstein's special theory of relativity shows that the velocity of the ion does not increase *ad infinitum* because its mass increases as the velocity increases. Thus, there is a self-limiting relativistic effect.

There is a mathematical expression for the frequency of revolution of a particle and is given by

$f = eB/2pm$ where m = mass of particle
v = velocity of particle
B = magnetic field strength
p = numerical constant

which gives a linear relationship of frequency versus magnetic field provided m is constant.

The radii of successive semi-circular trajectories of particles is given by

$r = 1/B(2Vm/e)^{½} N^{½}$ where V = accelerating voltage across the dee gap
N = number of accelerations the particle receives

from which it can be seen that the radius of successive orbits increases by the square root of N and gets closer together as N increases. As the radius increases, the beam of particles is accelerated each time it receives the resonance impulse so that there is a defined beam. The separation between successive orbits can be calculated and the beam can be extracted by placing a channel, at the appropriate radius, in which the magnetic field is reduced as much as possible. The particles then travel in an orbit of much larger radius and can be directed to suitable targets.

In cyclotron design studies it is useful to express the radial rate of decrease in magnetic field in terms of an index 'n' defined by

$B = B_o\ (r_o/r)^n$ where B_o = the magnetic field at a fixed radius r_o
and B = the magnetic field at any radius r

This n value can be determined to be

$$n = -(r/B)(dB/dr)$$

which shows mathematically that n is proportional to the rate of decrease of magnetic field with radius.

In practice, the majority of particles do not quite follow these idealised orbits and there is spread of the beam forming a loose bunch of particles with only the centre of the bunch following the theoretical track. Eventually, as the number of orbits increase, these bunches overlap and merge into a continuous radial distribution, but 'focusing' of the beam by magnetic and electrical means can help.

Consider firstly focusing by a magnetic field. If the magnetic field between the poles of a magnet decreases with increasing radius, the magnetic lines of force would become concave inwards increasing at the periphery of the cylindrical poles (the fringing effect). This deviant field causes a restoring force to be exerted on the particles tending to return them to their orbit. The field decrease can be influenced by shimming the magnets' poles, i.e., altering the pole shape by adding suitably shaped pieces of iron, whereby it is found that the radial component of the magnetic field - the restoring force - is proportional to the displacement from the theoretical orbit. For the ions to reach maximum energy there is then a compromise between keeping the magnetic field constant and giving it a decrease to restore the beam to its orbit to allow 'resonance' (i.e., the frequencies of electrical kicks and orbits to be similar) maintained over a sufficient number of accelerations.

An expression can be obtained mathematically which shows that radial and amplitude oscillations of the beam occur, one effect of the oscillations being to cause the beam orbits to precess as if the centre of the magnetic field had moved, which can cause an apparent partial discontinuity in the field and a knowledge of the location of the magnetic field centre is extremely useful.

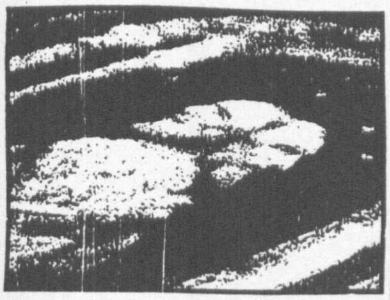
If one next considers electrical focusing, it can be seen that the radio frequency electrical field will also produce a focusing or restoring effect - in a similar manner to an optical lens - as there are divergent forces acting on a particle when it leaves the dee gap and convergent forces on entering the gap. Provided there is a net voltage displacement towards the median plane, then focusing occurs. This can be seen to be achieved by looking at a particle which is at a higher velocity when leaving the gap, i.e., it has been accelerated within the gap. The effect of electric field focusing is felt only for the first few accelerations and hence the magnetic field is the overriding factor in focusing effects.

In order to obtain maximum beam energy, the fringing magnetic field must be as far from the pole centre as possible so that the deflector which allows the particles to be extracted can be at the maximum orbit radius consistent with beam intensity.

With a fixed radio frequency accelerating voltage applied to the dees, there is a relativistic limitation on particle energy which restricts cyclotron sizes to about magnet pole diameters of 60" and, therefore, proton energies of about 25 MeV (with 18 kG magnetic fields).

Scientists Make An Amazing Discovery

Stumble On A Power "Too Great To Trust Humanity With"



This is the two-headed mosaic portrait of Mr. Norman in the Bank.

YOU CAN TREAD ON THE FACE OF MONTAGU NORMAN

Sunday Express Special Representative

IF you visit the Bank of England, using the new entrance in Princes-street, E.C., you will be in danger of treading on the features in mosaic of the governor, Mr. Montagu Norman.

His face has been put on the two heads of the gods Janus and Jupiter by the Russian artist Mr. Boris Anrep.

No one looking at the mosaic work could fail to recognise Mr. Norman's features.

"What a remarkable likeness to the governor," I said to the beadle who showed me the rebuilt offices of the Bank.

"I suppose it is because Janus and Jupiter are given brown beards that every one thinks the heads are intended to represent Mr. Norman," he replied.

But Mr. Boris Anrep, who designed the floors, frankly admitted to me that the two heads were portraits of Mr. Montagu Norman.

"I deliberately fashioned the likenesses of Mr Norman as a compliment to him," he said. "They are rather good, don't you think?"

"Did Mr. Norman raise any objection?" I asked.

"Well," laughed the artist, "I'm afraid he did not have much opportunity. He did not know of my intention until the work was completed. He was certainly surprised when he saw the finished job.

"I explained to him that I had used his features as a compliment to him, and I think he accepted the compliment which I intended, for he declared that it was a very good picture and well done."

Janus, most ancient King of Italy, said to have been the son of Apollo, according to the ancients, presided over all gates and avenues, and through him prayers reached the gods.

A Whole Country Might Be Wiped Out In One Second

By C. A. Lyon

Somewhere In England—Saturday.

A DISCOVERY OF SUCH TERRIFIC POTENTIALITIES THAT IT MAY TRANSFORM THE WORLD BEYOND MAN'S WILDEST DREAMS IS BEING INVESTIGATED IN THE LABORATORIES OF BRITAIN AND CERTAIN OTHER COUNTRIES.

This is the first news of it that has been published. The experiments concern a new way of producing energy in inconceivable quantities by splitting the atom of a rare metal, called uranium.

If the experiments succeed 1lb. of the metal will produce as much power as twenty million tons of coal do now. The new potential power was described to me by one of the scientists who are investigating it as "too great to trust humanity with."

All the other scientific discoveries ever made would be dwarfed by this discovery. In Britain it is being studied at Liverpool, Birmingham and Cambridge.

In America the implications are considered enormous for humanity but the American scientists who are engaged in the work have asked the others not to disclose the details of the work lest the new power should be misused for making war.

One of the English scientists engaged in the work gave me the following outline of what is happening. It reads like H. G. Wells, but it is strictly authentic.

The new power is derived from the release of the energy in the atom. This energy has long been known to exist in enormous quantities, but it has never been possible until this year to release it except to a very small extent at prohibitive expenditure.

Then a new way of doing it was discovered.

Released Energy

The older method, it is necessary to explain, was that devised by the celebrated scientist Lord Rutherford in 1919. His method depended partly on the fact that the nucleus of every atom of any substance in the world consisted of two kinds of particles, first, the protons, which are electrically charged particles; second, the neutrons or neutral particles which have no electric charge.

Lord Rutherford released the energy of the atom by splitting it with a bombardment of what are called Alpha particles. These particles are composed of two neutrons and two protons.

When the atom of the bombarded substance broke up it would form two new substances. But in addition it would set free a fabulous amount of energy.

The average for a single atom, an object only one two-hundred millionth of an inch in diameter, was to release four to five million electron volts of energy.

The only thing was that in order to produce this amount of energy other energy thousands of times greater still was necessary.

Alchemist's Dream

The reason was that only one atom in a great many would be hit during a bombardment, so as a motive power atom-splitting was impracticable, though it was of great scientific importance.

In all the years since the war interesting experiments in atom splitting have been going on in laboratories.

The alchemist's dream has come true. Gold has been produced from base metal, though it costs more than gold from the earth. It is possible to make artificial radium that is better than "real" radium. But all along it seemed an impossibility to use atomic energy in a practical way

Last Christmas time a revolutionary discovery was made by a German worker called Hahn.

It has been discovered since Lord Rutherford's time that it was possible to split the atom by either the proton or the neutron alone.

But Hahn noticed that there was one substance which, when bombarded with neutrons, did not behave like the others.

This substance was uranium, a rare metal, and the heaviest of all substances. It looks like iron, and it is considerably heavier than lead.

When Bombarded

All other substances except uranium divide up into a heavy substance and light-weight particles when bombarded with neutrons. But Hahn found that uranium divided up into two heavy substances.

Workers in all countries seized on his discovery. It was found simultaneously in Copenhagen, New York and England that the breaking up of uranium produces forty times as much energy as the breaking up of other substances.

But even this was not the most important thing. For although uranium produces forty times more energy in breaking up than other substances, it still takes a force of 10,000,000,000,000 volts to produce only 200,000,000 volts of energy from the atom of uranium.

The discovery beyond all these things which was destined to shake the world was the following:—

Both the broken-up halves of the uranium went on firing out neutrons themselves. Each neutron fired out from the "halves" attacked other atoms of uranium. These atoms in turn attacked others

In other words, the atom, once broken up, goes on breaking up more and more atoms. Each atom releases 200,000,000 volts. The energy is thus released in ever-growing quantities, just as a bit of rock will produce an ever-growing avalanche of snow.

As yet the process has been seen in only a very small way, because impurities in the uranium have made the "chain reaction" process fizzle out.

But if it could continue—and there does not seem any reason why it should not—the outpouring of energy would exceed anything ever known in the world.

If the splitting up of a single atom of uranium would result in more and more splitting up, and if each gives off 200,000,000 volts, anything in the world may result.

It Costs 32s.

It is at this stage that things are now. Science stands on the threshold of a release of power so vast that no one knows what will happen.

In the laboratory I saw today they are making the plans for the first big experiment. That will consist of the breaking up of one pound of uranium.

The uranium will cost 32s. to buy. Theoretically it will produce the energy of 20,000,000 tons of coal.

The first thing which will be done is to purify it in the laboratory electric furnaces.

After the purification by smelting the uranium will be powdered and mixed with liquid hydrogen at unimaginably low temperatures—minus 253 degrees centigrade. It will then be bombarded with neutrons from a radio-active substance and then—no one knows what will happen.

The potential energy which might be released in this experiment would be capable of blowing up the whole of England.

But although theoretically there is such great energy available in a pound of uranium, it is certain that the whole of this cannot be released at once.

Experimenting on this scale is therefore perfectly safe.

Driving Engines

But the uses to which it might be put are appalling. Science, with its explosives, its electricity, its wireless, has been growing ever more powerful. This is the climax.

A nation at war might be able to wipe another nation right off the face of the earth in a second.

It might also blow itself off the earth in the attempt.

The experiment with 1lb. of uranium is intended only as a beginning. It is held that it is probably only possible to determine whether the uranium will release its energy in such a way that it could be used to drive engines by experimenting with a ball of uranium about a yard in diameter. Such a ball would weigh a ton.

The possibilities of experimenting with it are being seriously considered. It would cost about £3,500 to buy the uranium ore and perhaps another £1,000 to purify it.

The university in which I write this hopes to raise the money for such an experiment.

A professor at the university whose judgment is as good on the subject as any one's in the world told me in utter seriousness that if the experiment with the one-ton ball went wrong the energy released would not only blow up this country but the whole world.

Their Secret

The professors of Liverpool, Birmingham, Cambridge, America and France have kept their amazing experiments secret.

The first hint of what was going on was in a letter which has just been published in a British scientific paper. It was written by M. Curie-Joliot, son-in-law of the celebrated Mme. Curie. He and Mme. Curie's daughter are among those working on the project.

It is curious to think that these experiments, undertaken by men out of the great world in isolated laboratories, may perhaps change the world more than all the conferences, the kings, and the wars that there have ever been.

Army Council Discuss Conscription All Day

The Army Council held its longest continuous meeting for many years yesterday, hammering out final details of the conscription scheme. Members sat continuously from 11 a.m. until 6 p.m., with only a brief break for a sandwich luncheon.

The meeting was made necessary because the Cabinet had rushed its decisions only last Tuesday, leaving many details to be settled. The text of the Conscription Bill will be before the Cabinet tomorrow and published tomorrow night.

TEST BY-ELECTION

Conscription will be the main issue in the Hallam (Sheffield) by-election, the candidates for which (nominated yesterday) are Mr. Roland Jennings, chartered accountant (Conservative) and Mr. C. Darvill, school teacher (Socialist). Polling day is May 10.

—Cheer, Boys, Cheer!—

THE greatest cheer in the Cup Final at Wembley yesterda[y] went up when Portsmouth scored their fourth goal. Th[is] was the climax of the match. Wolverhampton Wanderers ha[d] no further chance and the crowd shouted its relief.

A Marconi-Ekco sound recording instrument "listened to the whole match" and registered every noise in phons—units of sound.

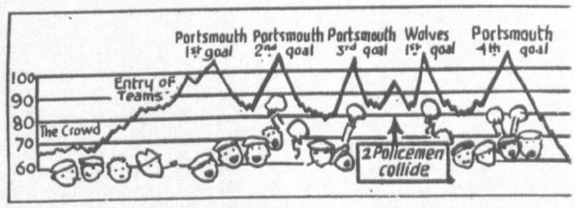
Portsmouth's fourth goal raised 110 phons. Next came Portsmouth's second and Wolverhampton's only one, with 107 each. The first and third registered 105 phons.

Most of the game was played out between 85 and 95, which shows that there was considerable excitement as the general crowd noise, measured before the match started, wa[s] sixty-five phons.

A policeman who tripp[ed] over another policeman in run[]ning after the ball raise[d] ninety-six phons of laughte[r] which was one phon mo[re] noise than greeted the Kin[g].

All the exciting raids an[d] "near things" were shown [by] jumps.

The noise was fairly stead[y] around 85. Then there was [a] raid on goal and a shot. Th[e] needle jumped to 95.



Woman Learns To Swim At 72

SEVENTY-SIX-YEAR-OLD Mrs. Wheelwright, of Norton-close, Enfield Highway, Middlesex, was one of the few to brave the chill wind for a swim at the summer opening yesterday of the Enfield open-air swimming pool.

She learned to swim at the age of seventy-two.

NEARLY CRASHED ON CUP FINAL

WHILE 100,000 people watched the Wembley Cup-tie drama yesterday, another drama, featuring a woman pilot, was taking place over their heads.

The engine of her airplane partly failed. The aircraft began to sink down towards the crowd, which was too absorbed in the game to notice.

But by cool flying she was able to prevent a crash and land on Sudbury Golf Course uninjured.

The pilot was Mrs. Winifred Crossley, of Thurleigh, Bedfordshire.

My Goodness McGuinness

NIALL McGUINNESS is [a] waiter. But he does[n't] like having to wait when he[']s off duty.

At 5 a.m. yesterday the mana[ger] of the Criterion Restaurant, P[ic]cadilly-circus, gave him in cha[rge] for smashing a mirror at [the] restaurant.

At Bow-street Police Court [Mr.] McGuinness said that he had [had] to wait half an hour for service [and] then found there was nothing [for] him. So he lost his temper.

Now he has to pay 10s. fine[,] costs, and £2 10s. for repair[ing] the mirror.

Confidential.

THE ROYAL SOCIETY.

GOVERNMENT GRANT

"FOR SCIENTIFIC INVESTIGATIONS UNDERTAKEN WITH THE SANCTION OF A COMMITTEE APPOINTED FOR THE PURPOSE."

1936.

CLASSIFICATION OF APPLICATIONS.

BOARD A.

Mathematics, Mathematical Physics, and Mathematical Astronomy.

Chairman, Prof. C. G. Darwin, 4, *Church Hill, Edinburgh.*

Prof. C. G. Darwin.	Prof. E. A. Milne.
Prof. L. N. G. Filon.	Prof. H. C. Plummer.
Prof. G. H. Hardy.	Prof. J. Proudman.
Prof. D. R. Hartree.	Prof. G. N. Watson.

No applications received this year.

Council Minutes Printed
Vol. 14

1936.
June 18.

At a Meeting of the Council of the Royal Society held on June 18, 1936.

Present:

SIR WILLIAM BRAGG (*President*) in the Chair.

PROF. E. D. ADRIAN.
MR. D. L. CHAPMAN.
PROF. A. W. CONWAY.
DR. W. H. ECCLES.
PROF. L. N. G. FILON.
DR. J. GRAY.
SIR DANIEL HALL.
PROF. A. V. HILL (*Secretary*).
SIR PATRICK LAIDLAW.
SIR GERALD LENOX-CONYNGHAM.
SIR HENRY LYONS (*Treasurer*).
PROF. G. T. MORGAN.
PROF. R. ROBISON.
PROF. A. C. SEWARD (*Foreign Secretary*).
SIR FRANK SMITH (*Secretary*).
PROF. W. STILES.
MR. W, TROTTER.

The ASSISTANT SECRETARY attended.

Minutes. 1. The Minutes of the previous meeting (May 21) were read and signed, by the President, as correct.

Death. 2. The Secretaries reported the death of Sir George Hadcock.

Resolved—That Sir Eustace d'Eyncourt be requested to write an obituary notice.

Foreign Members. 3. Council proceeded to the nomination of Foreign Members. A ballot having been taken, it was agreed to nominate the following for election into the Society :—

Sigmund Freud, Vienna.
Ludwig Jost, Heidelberg.
Felix Andries Vening Meinesz, Utrecht.
Hermann Weyl, Princeton.

4. The Secretaries reported that the old epidiascope lantern and the transformer connected with it were of no further use to the Society. *Epidiascope.*

Resolved—That members of Council be invited to inquire whether any suitable institution could make use of it; and that the Secretaries be authorized to dispose of the epidiascope lantern and transformer in due course at their discretion as a gift or for payment.

5. Read the following Report of the Government Grant for Scientific Investigations Committee :— *Government Grant.*

The Committee submit their report as follows :—

(i) That grants amounting to £4,690 be made in accordance with the recommendations of the Boards (as appended).

(ii) That, on the recommendation of Board B, a grant of £200 be made to Professor H. Dingle for an investigation of molecular spectra in the extreme ultra-violet.

(iii) That the application from Professor J. Chadwick for an apparatus grant of £2,000 be referred to the Council.

(iv) That the sum of £780 be placed in the hands of the Council under Regulation 24 to meet urgent demands upon the Grant which may be made before the next meeting of the Committee.

(v) That the sum of £1,000, representing the increase in the amount voted by Parliament for the current year, be placed in the Reserve Fund.

Resolved—That the report be received and that it be ordered accordingly.

Resolved further—That, in the event of a decision being required on Professor Chadwick's application between the Council meetings of July and October, the Warren Committee be authorized to pledge the Council to generous support from the Warren Bequest. *Warren Committee.*

6. Read an application from Dr. G. Stella for a grant of £60 towards research work on respiratory and circulatory reflexes from alterations of the pulmonary circulation. *Medical Research Fund.*

Resolved—That a grant of £30 be made to Dr. Stella for this purpose from the Medical Research Fund.

7. Council considered the report of the Committee appointed by Minute 24 of April 30, 1936, which had been circulated. In consequence of criticisms made, it was *Ordnance Survey.*

Resolved—That the Physical Secretary be requested to discuss the report in detail with Dr. B. Smith and to submit a revised report to Council.

5. J. Chadwick, F.R.S. ....£2,000.

March, 28, 1936.

"1. I wish to build a magnetic resonance accelerator of the type devised by Professor E. O. Lawrence of Berkeley, California. This is an apparatus in which protons and deuterons can be accelerated by means of successive applications of a relatively low potential until their energies are such as would be derived from a fall through a potential of some millions of volts. In this way Lawrence has obtained beams of protons and deuterons of energies as high as 6 million volts, and he has been able to cause transmutations of heavy elements such as bismuth as well as of the lighter elements. This method of accelerating ions has certain pronounced advantages over any other method which has yet been used. The difficulties inherent in any direct method of accelerating particles to these high energies do not occur. This cyclotron apparatus of Lawrence is a physical instrument of very great power. Several are under construction in the United States, and it is highly desirable that at least one should be built in this country. I propose to build a cyclotron to accelerate protons and deuterons to a maximum energy of about 10 million volts. With this apparatus I propose to investigate the transmutations of the elements which are provoked by bombardment by protons and deuterons. (i) In some of the transmutations produced by deuterons, neutrons are emitted, and in this way very intense sources of neutrons can be obtained. The properties of neutrons, their behaviour in collisions with atomic nuclei, and the transmutations produced by them will be investigated. (ii) Unstable nuclei—artificial radio-active elements—are formed in many cases. It is intended to examine the radiations of these unstable nuclei and to search for possible series disintegrations. (iii) This is a very brief outline of the physical side of the proposed work; the chemical aspects are also important. The natural radio-active substances have been of great use as indicators in chemical and physical processes but their application is very restricted. Many types of elements can be obtained in artificially active modifications, *e.g.*, carbon, nitrogen, etc. It is proposed to use the unstable elements as indicators in biochemical processes as well as in the more usual chemical processes. The biochemical applications may well turn out to be as important as the physical applications. In this section of the programme I shall have the collaboration of Professor H. J. Channon, Professor of Biochemistry in this University. (iv) A further application may be mentioned. The action of neutrons in their passage through matter differs markedly from that of X- or γ-rays, and their biological effect may prove very interesting. In addition, the biological effects of some of the artificial radio-active elements will be examined.

"2. The cost of building this magnetic resonance accelerator or cyclotron is difficult to estimate closely. Professor Lawrence has given me a figure of 12,000 dollars, based on the estimates for an apparatus under construction at Princeton. I have discussed the matter with Mr. A. P. M. Fleming of Metropolitan Vickers and others, and it seems that I must be prepared for a total expenditure between £4,000 and £5,000. The greater part of this is due to the magnet. A uniform magnetic field of about 17,000 gauss is required over a circular area of about 35 inches diameter, and a gap of about 2 inches between the pole faces is necessary to accommodate the acceleration chamber. The magnet will contain roughly about 50 tons of iron and 8 tons of copper, so that the greater part of the cost of the magnet is the cost of the material. A considerable sum might thus be saved if the iron and copper could be obtained below market prices. I am very hopeful that this will be possible, but at present I must face the prospect that the complete apparatus, magnet and oscillator, will cost £4,000–£5,000. I appeal to the Society for a grant of £2,000, with a possibility that the whole of this will not be spent

3. "Towards this apparatus I have been granted £2,000 by the University of Liverpool.

"5. Professor of Physics in the University of Liverpool.

"6. By far the greater part of the apparatus has a permanent value. The magnet might be so constructed that it could be adapted for use in other lines of work.

"7. Physical Laboratory of the University of Liverpool."

This application is also relevant to Board C.

6. C. H. Collie ....£85.

Christ Church,
Oxford.
March 30, 1936.

"1. The object of the research is to measure the velocity of slow neutrons by a direct method. Hitherto it has only been possible to measure the velocity of the slowest neutrons and it is highly desirable to be able to extend the range of the measurement. The following is an outline of the proposed method. (i) The neutrons are produced artificially by the action of D on D, using the existing 400 k.v. plant: (ii) These neutrons are produced over a very small time interval by deflecting the incident diplon beam in an alternating field: (iii) The neutrons having been slowed down by collision in a small block of paraffin wax are detected by means of a detector with a very small time constant synchronised with the swinging diplon beam. Such a detector has not yet been developed, but the success of secondary emission amplifiers in the field of television suggests that it will

Appendix 4
NUCLEAR FISSION

A rigorous treatment of nuclear fission is complex and a description of events leading to the discovery is given below.

Lise Meitner's two German colleagues, Hahn and Strassmann, were both chemists and were perplexed over the results obtained when they had irradiated uranium with neutrons in late 1938. Meitner and her German colleagues had been working together in Berlin until she had been forced to flee from the Nazis and the persecution of all Jewish people there. Meitner went to Stockholm, and the experimental difficulties experienced by Hahn and Strassmann were outlined in Hahn's correspondence with her. Hahn submitted a report, published in *Naturwissenschaften*[1], of November 1938, in which he listed three radium and three actinium 'isomerics'[2] found in his results, but also noted the similarity to an experiment with thorium which, it was thought in both cases, produced the radium with a neutron-induced expulsion of an alpha particle.

Meitner and Hahn met up with each other in Copenhagen over Meitner's 60th birthday (5th November 1938) and, as Sime says in her book[3], after Hahn returned to Berlin:

> Otto Hahn and Fritz Strassmann were racing to verify their earlier findings: by early December they thoroughly characterised what appeared to be three radium and three actinium species, determined their half-lives, and eliminated neighboring elements from consideration. ... Then, spurred by the objections Lise Meitner had voiced in Copenhagen, they began a quite different test: an attempt to verify the presence of radium by partially separating it from its barium carrier.

It was confirmed in the final test by the two chemists that, surprisingly, the "radium" appeared to be barium. A fuller picture of these experimental events is given in Glasstone's *Sourcebook on Atomic Energy*[4].

Hahn quickly wrote back to Meitner with the results of his findings. Before receiving a reply from her, he submitted the results to *Naturwissenschaften* on 22nd December 1938. Meitner replied to Hahn's letter on the 21st December and then went to spend the Christmas holiday with friends in Kungälv, on the west coast of Sweden, and it was there that Otto Robert Frisch, her nephew, joined them. Meitner and Frisch discussed the Hahn letter the day after Frisch had arrived (probably on the 24th December) and, instead of the nucleus being chipped or cracked away when bombarded, they considered that the results might be explained if the nucleus could be thought of - as George Gamow (in 1928) had described it - as a liquid droplet. (See R.H. Stuewer's analysis[5]). Frisch, in his memoirs[6], recalls that:

> ... such a drop might elongate and divide itself.

and he also described the fission process in *Physics Today* [7].

The aunt and nephew considered the surface tension holding the nuclear 'droplet' together and thought that it was possible for the rather unstable nuclei of uranium to divide itself if it received some additional energy as in the absorption of a neutron. When the two droplets separated, they would be driven apart by their mutual electric repulsion and would acquire a very large energy - about 200 MeV. But from where would that energy come? Meitner and Frisch 'did the sums' and found that the energy could come from the two lighter nuclei so formed. The two nuclei were together lighter by about 1/5th of the (rest) mass of a proton, and from $E = mc^2$, the 1/5th proton mass was just equivalent to 200 MeV, and, as Frisch said, "it all fitted."

Glasstone[8], attributed the name 'fission' in the following manner, which was:

> ... suggested by the American biologist W.M. Arnold who was in Copenhagen at the time working with G. Hevesy. The same word has long been used to describe the division of cells in living organisms.

and, as Frisch explains in *What Little I Remember*:

> I asked an American biologist who was working with Hevesy what they call the process by which single cells divide in two; "fission", he said, so I used the term "nuclear fission" in that paper.

'That paper' was the *Disintegration of Uranium by Neutrons: A New Type of Nuclear Reaction*, published jointly by Frisch and Meitner in *Nature*[9], February 1939, the letter actually dated 16th January 1939. Frisch published experimental confirmation, in *Nature*[10], later on in the same month, the letter again dated 16th January 1939, which showed that he had found the fission fragments by looking at the recoil energies and so had proved their hypotheses.

Frisch met Neils Bohr in Copenhagen just prior to Bohr's visit to Princeton, New Jersey, in early January 1939 and, according to Frisch, after only a few minutes discussion, Bohr realised the importance of Meitner and Frisch's explanation of the fission process. As soon as Bohr arrived in Princeton, he wrote a letter to the Editor of *Nature*[11] dated 20th January 1939, which was published towards the end of February, in which he put his scientific backing to the process of fission.

The next few months was a period of great excitement for the nuclear physicists and chemists working on the nuclear disintegration of uranium, and it was soon realised that there could possibly be danger in the fission process because, if more than one neutron is emitted, the emitted neutrons could in turn cause more fission of the nucleus and an explosive "chain" reaction could occur. The control of fission was therefore considered and, for example, Adler and von Halban in Paris, published a letter[12] which suggested the use of cadium as an absorber to prevent explosion.

The idea of fission and possible use of atomic energies in a super bomb had not gone unnoticed by the Press. A report in the *Sunday Express*, reproduced in appendix 2, of 30th April 1939, written by a C.A. Lyon[13] from 'somewhere in England - Saturday', had the headline:

Scientists Make An Amazing Discovery
Stumble On A Power "Too Great to Trust Humanity With"
A Whole Country Might Be Wiped Out In One Second

The newspaper article outlined the

> new way of producing energy in inconceivable quantities by splitting the atom of a rare metal, called uranium.

It mentioned that Hahn - a German worker (the author's underline!) - noticed that 'uranium divided up into two heavy substances', and it would release 'a fabulous amount of energy:

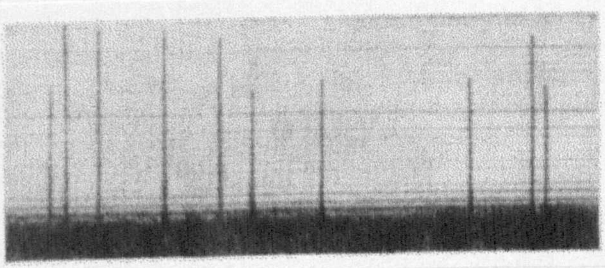
each neutron fired out from the "halves", attacked other atoms of uranium. These atoms in turn attacked others. The energy is thus released in ever increasing quantities, just as a bit of rock will produce an ever-growing avalanche of snow.

(a description of the chain reaction!), and towards the end of the report is the following:

Their Secret
The professors of Liverpool, Birmingham, Cambridge, America and France have kept their amazing experiments secret.

This author considers it important that in the freely available scientific journals, and the free press, all the information was there to start considering the potential military applications in the form of a super-bomb.

The fission process is extremely complex but, by the middle of February 1939, researchers at Columbia University, among others, had confirmed the ionisation bursts caused by the fission products of Uranium. This is shown in the photograph reproduced below. In addition to the ionisation effects, Meitner and Frisch (and others such as McMillan in the U.S.A. and Joliot in Paris), found that fission fragments were ejected with such high velocity they could be deposited on a surface a short distance - about 2 cms - away. These ejected nuclei were found to possess the radioactive properties which had previously been attributed to the transuranium elements.



Photograph of Ionisation Bursts caused by Fission Products (u.d.)
(reproduced from Glasstone, *Sourcebook on Atomic Energy*, p.477)

A number of researchers, including N. Feather and E. Bretscher at Cambridge, as well as Frisch, continued the work on clarifying the fission processes and examined the products of the radioactive decay of the elements formed - of the middle range of atomic number. These included Bromine, Krypton, Strontium, Molybdenum, Rubidium, Antimony, Tellurium, Iodine, Xenon and Cesium. Most of this work had been undertaken within three months of Frisch and Meitner's January paper to *Nature*!

On 27th May 1939, Feather, working at the Cavendish Laboratory, published a paper[14] in *Nature* and in it gives an account, including the names of some of the scientists involved, of the events leading to the discovery of the fission process.

Uranium can be split by neutrons of all energies, that is by thermal, slow and fast neutrons. Fission of the more abundant U^{238} requires neutrons of energy about 1 MeV but fission cross- sections are generally higher for slower neutrons. It was not until well after the

Second World War, in 1948, that published data began to show that fission was possible even down to nuclei of tantulum with atomic number of 73, and below.

Although uranium^{235}, for example, can split in a number of different ways, the most probable nuclei yield is in producing nuclei of mass numbers of about 95 and 139. These two radioactive products undergo a number of radioactive decays and, after the emission of seven beta-particles, the stable end products of molybdenum-95 and lanthanum-139 are formed.

$$U^{235} + n \rightarrow Mo^{95} + La^{139} + 7e + 2n$$

If one now checks the atomic masses of the two sides of the above equation, there is an imbalance of about 204 MeV, which appears as the fission energy.

According to Brown (Chadwick's biographer), in an interview with Rotblat on the 7th January 1994[15], when Chadwick returned to Liverpool from the U.S.A. in about 1947, Chadwick

> was "conned" by Chapman Pincher, the journalist who contacted him and said he was a graduate of Liverpool University and wanted to talk to Chadwick about the bomb. Chadwick agreed to meet him and show him the department, etc., and to discuss the Manhattan project and bomb in private conversation. Pincher, who had not said he was a journalist, recorded their conversation on a hidden tape recorder and then published a sensational scoop in the *Daily Express*.

The Express group of newspapers under Lord Beaverbrook had, in the space of just six years, published the story of the beginning of the age of atomic power, right from its outset.

References:

App.4.1. Hahn, *Naturwissenschaften* **26**, 18th November 1938.

App.4.2. Cassell, *Concise English Dictionary* 1995, which defines 'isomeric' as:

> (*Chem.*) having identical elements, molecular weight and proportions, with difference in physical characteristics or chemical properties owing to different grouping; of atomic nuclei, having the same numbers of protons and neutrons but different energy states.
> **isomer**, n. a compound, chemical group, atom etc. isomeric with one or more other compounds, etc.

App.4.3. Sime, R., *Lise Meitner: A Life in Physics*, Berkeley, University of California Press, 1996, p. 231.

App.4.4. Glasstone, *Sourcebook on Atomic Energy*, Princeton, D. Van Nostrand Company, Inc., (Third Edition) (1967), pps. 473-6.

App.4.5. Stuewer, R.H., 'The Origin of the Liquid-Drop Model and the Interpretation of Nuclear Fission', *Perspectives on Science* **2** (1994), pps. 76-129.

App.4.6. Frisch, O.R., *What Little I Remember* (Cambridge), Cambridge University Press, (1979), Canto Edition, 1991.

App.4.7. Frisch, O.R., 'The Discovery of Fission: How it all began' *Physics Today* **20**, No.11, November 1967,

App.4.8. *ibid.* App.4.4., p. 476

App.4.9. Frisch, O.R.; Meitner, L. 'Disintegration of Uranium by Neutrons: A new Type of Nuclear Reaction' *Nature* **143**, 11th February 1939, pps. 239-40.

App.4.10. Frisch, O.R., *Nature* **143**, 18th February 1939.

App.4.11. Bohr, N., 'Disintegration of Heavy Nuclei', *Nature* **143**, 25th February 1939, p. 330.

App.4.12. Adler, von Halban, 'Control of the Chain Reaction involved in Fission of the Uranium Nucleus', *Nature Supplement* **143**, No. 3628, 13th May 1939.

App.4.13. Brigadier Cyril Arthur Lyon, Retd., 1937, an Artillery Officer (1880-1955).

App.4.14. Feather N., 'Fission of Heavy Nuclei: a New Type of Nuclear Disintegration', *Nature*, No. 3630, pps. 877-9.

App.4.15. Transcript of Rotblat/Brown Interview, 7th January 1994, Joseph Rotblat Archive, University of Liverpool.

Appendix 5
THE FRISCH-PEIERLS MEMORANDUM

On the Construction of a 'Super-bomb'; based on a Nuclear Chain Reaction in Uranium

The possible construction of 'super-bombs' based on a nuclear chain reaction in uranium has been discussed a great deal and arguments have been brought forward which seemed to exclude this possibility. We wish here to point out and discuss a possibility which seems to have been overlooked in these earlier discussions.

Uranium consists essentially of two isotopes, ^{238}U (99.3%) and ^{235}U (0.7%). If a uranium nucleus is hit by a neutron, three processes are possible: (1) scattering, whereby the neutron changes direction and, if its energy is above about 0.1 MeV, loses energy; (2) capture, when the neutron is taken up by the nucleus; and (3) fission, i.e. the nucleus breaks up into two nuclei of comparable size, with the liberation of an energy of about 200 MeV.

The possibility of a chain reaction is given by the fact that neutrons are emitted in the fission and that the number of these neutrons per fission is greater than 1. The most probable value for this figure seems to be 2.3, from two independent determinations.

However, it has been shown that even in a large block of ordinary uranium no chain reaction would take place since too many neutrons would be slowed down by inelastic scattering into the energy region where they are strongly absorbed by ^{238}U.

Several people have tried to make chain reaction possible by mixing the uranium with water, which reduces the energy of the neutrons still further and thereby increases their efficiency again. It seems fairly certain, however, that even then it is impossible to sustain a chain reaction.

In any case, no arrangement containing hydrogen and based on the action of slow neutrons could act as an effective super-bomb, because the reaction would be too slow. The time required to slow down a neutron is about 10^{-5} sec and the average time lost before a neutron hits a uranium nucleus is even 10^{-4} sec. In the reaction, the number of neutrons would increase exponentially, like e^{t}/τ where τ would be at least 10^{-4} sec. When the temperature reaches several thousand degrees the container of the bomb will break and within 10^{-4} sec the uranium would have expanded sufficiently to let the neutrons escape and so to stop the reaction. The energy liberated would, therefore, be only a few times the energy required to break the container, i.e. of the same order of magnitude as with ordinary high explosives.

Bohr has put forward strong arguments for the suggestion that the fission observed with slow neutrons is to be ascribed to the rare isotope ^{235}U, and that this isotope has, on the whole, a much greater fission probability than the common isotope ^{238}U. Effective methods for the separation of isotopes have been developed recently, of which the method of thermal diffusion is simple enough to permit separation on a fairly large scale.

This permits, in principle, the use of nearly pure ^{235}U in such a bomb, a possibility which apparently has not so far been seriously considered. We have discussed this possibility and come to the conclusion that a moderate amount of ^{235}U would indeed constitute an extremely efficient explosive.

The behaviour of ^{235}U under bombardment with fast neutrons is not known experimentally, but from rather simple theoretical arguments it can be concluded that almost every collision produces fission and that neutrons of any energy are effective. Therefore it is not necessary to add hydrogen, and the reaction, depending on the action of fast neutrons, develops with very great rapidity so that a considerable part of the total energy is liberated before the reaction gets stopped on account of the expansion of the material.

The critical radius r_o - i.e. the radius of a sphere in which the surplus of neutrons created by the fission is just equal to the loss of neutrons by escape through the surface - is, for a material with a given composition, in a fixed ratio to the mean free path of the neutrons, and this in turn is inversely proportional to the density. It therefore pays to bring the material into the densest possible form, i.e., the metallic state, probably sintered or hammered. If we assume, for ^{235}U, no appreciable scattering, and 2.3 neutrons emitted per fission, then the critical radius is found to be 0.8 times the mean free path. In the metallic state (density 15), and assuming a fission cross-section of 10^{-23} cm^2, the mean free path would be 2.6 cm and r_o would be 2.1 cm, corresponding to a mass of 600 grams. A sphere of metallic ^{235}U of a radius greater than r_o would be explosive, and one might think of about 1 kg as a suitable size for the bomb.

The speed of the reaction is easy to estimate. The neutrons emitted in the fission have velocities of about 10^9 cm/sec and they have to travel 2.6 cm before hitting a uranium nucleus. For a sphere well above the critical size the loss through neutron escape would be small, so we may assume that each neutron, after a life of 2.6 x 10^{9} sec, produces fission, giving birth to two neutrons. In the expression e^{t}/τ for the increase of neutron density with time, it would be about 4 x 10^{-9} sec, very much shorter than in the case of a chain reaction depending on slow neutrons.

If the reaction proceeds until most of the uranium is used up, temperatures of the order of 10^{10} degrees and pressures of about 10^{13} atmospheres are produced. It is difficult to predict accurately the behaviour of matter under these extreme conditions, and the mathematical difficulties of the problem are considerable. By a rough calculation we get the following expression for the energy liberated before the mass expands so much that the reaction is interrupted:

$$E = 0.2M(r^2/\tau^2)(\sqrt{(r/r_o)} - 1) \qquad (1)$$

(M, total mass of uranium; r, radius of sphere; r_o, critical radius; τ, time required for neutron density to multiply by a factor e). For a sphere of diameter 4.2 cm (r = 2.1 cm), M = 4700 grams, τ = 4 x 10^{-9} sec, we find E = 4 x 10^{22} ergs, which is about one-tenth of the total fission energy. For a radius of about 8 cm (M = 32 kg) the whole fission energy is liberated according to formula (1). For small radii the efficiency falls off even faster than indicated by formula (1) because τ goes up as r approaches r_o. The energy liberated by a 5 kg bomb would be equivalent to that of several thousand tons of dynamite, while that of a 1 kg bomb, though about 500 times less, would still be formidable.

It is necessary that such a sphere should be made in two (or more) parts which are brought together first when the explosion is wanted. Once assembled, the bomb would explode within a second or less, since one neutron is sufficient to start the reaction and there are several neutrons passing through the bomb in every second, from the cosmic radiation. (Neutrons originating from the action of uranium alpha rays on light-element impurities would be negligible provided the uranium is reasonably pure.) A sphere with a radius of less than about 3 cm could be made up in two hemispheres, which are pulled together by springs and kept separated by a suitable structure which is removed at the desired moment. A larger sphere would have to be composed of more than two parts, if the parts, taken separately, are to be stable.

It is important that the assembling of the parts should be done as rapidly as possible, in order to minimise the chance of a reaction getting started at a moment when the critical conditions have only just been reached. If this happened, the reaction rate would be much slower and the energy liberation would be considerably reduced; it would, however, always be sufficient to destroy the bomb.

It may be well to emphasise that a sphere only slightly below the critical size is entirely safe and harmless. By experimenting with spheres of gradually increasing size and measuring the number of neutrons emerging from them under a known neutron bombardment, one could accurately determine the critical size, without any danger of a premature explosion.

For the separation of the ^{235}U, the method of thermal diffusion, developed by Clusius and others, seems to be the only one which can cope with the large amounts required. A gaseous uranium compound, for example uranium hexafluoride, is placed between two vertical surfaces which are kept at a different temperature. The light isotope tends to get more concentrated near the hot surface, where it is carried upwards by the convection current. Exchange with the curre moving downwards along the cold surface produces a fractionating effect, and after some time a state of equilibrium is reached when the gas near the upper end contains markedly more of the light isotope than near the lower end.

For example, a system of two concentric tubes, of 2 mm separation and 3 cm diameter, 150 cm long, would produce a difference of about 40% in the concentration of the rare isotope between its ends, and about 1 gram per day could be drawn from the upper end without unduly upsetting the equilibrium.

In order to produce large amounts of highly concentrated ^{235}U, a great number of these separating units will have to be used, being arranged in parallel as well as in series. For a daily production of 100 grams of ^{235}U of 90% purity, we estimate that about 100,000 of these tubes would be required. This seems a large number, but it would undoubtedly be possible to design some kind of a system which would have the same effective area in a more compact and less expensive form.

In addition to the destructive effect of the explosion itself, the whole material of the bomb would be transformed into a highly radioactive state. The energy radiated by these active substances will amount to about 20% of the energy liberated in the explosion, and the radiations would be fatal to living beings even a long time after the explosion.

The fission of uranium results in the formation of a great number of active bodies with periods between, roughly speaking, a second and a year. The resulting radiation is found to decay in such a way that the intensity is about inversely proportional to the time. Even one day after the explosion the radiation will correspond to a power expenditure of the order of 1000 kW, or to the radiation of a hundred tons of radium.

Any estimates of the effects of this radiation on human beings must be rather uncertain because it is difficult to tell what will happen to the radioactive material after the explosion. Most of it will probably be blown into the air and carried away by the wind. This cloud of radioactive material will kill everybody within a strip estimated to be several miles long. If it rained the danger would be even worse because active material would be carried down to the ground and stick to it, and persons entering the contaminated area would be subjected to dangerous radiations even after days. If 1% of the active material sticks to the debris in the vicinity of the explosion and if the debris is spread over an area of, say, a square mile, any person entering this area would be in serious danger, even several days after the explosion.

In these estimates, the lethal dose of penetrating radiation was assumed to be 1,000 Roentgen; consultation of a medical specialist on X-ray treatment and perhaps further biological research may enable one to fix the danger limit more accurately. The main source of uncertainty is our lack of knowledge as to the behaviour of materials in such a super-explosion, and an expert on high explosives may be able to clarify some of these problems.

Effective protection is hardly possible. Houses would offer protection only at the margins of the danger zone. Deep cellars or tunnels may be comparatively safe from the effects of radiation, provided air can be supplied from an uncontaminated area (some of the active substances would be noble gases which are not stopped by ordinary filters).

The irradiation is not felt until hours later when it may be too late. Therefore it would be very important to have an organisation which determines the exact extent of the danger area, by means of ionisation measurements, so that people can be warned from entering it.

O.R. FRISCH
R. PEIERLS

The University, Birmingham

Part II

The attached detailed report concerns the possibility of constructing a 'super-bomb' which utilises the energy stored in atomic nuclei as a source of energy. The energy liberated in the explosion of such a super-bomb is about the same as that produced by the explosion of 1,000 tons of dynamite. This energy is liberated in a small volume, in which it will, for an instant, produce a temperature comparable to that in the interior of the sun. The blast from such an explosion would destroy life in a wide area. The size of this area is difficult to estimate, but it will probably cover the centre of a big city.

In addition, some part of the energy set free by the bomb goes to produce radioactive substances, and these will emit very powerful and dangerous radiations. The effect of these radiations is greatest immediately after the explosion, but it decays only gradually and even for days after the explosion any person entering the affected area will be killed.

Some of this radioactivity will be carried along with the wind and will spread the contamination; several miles downwind this may kill people.

In order to produce such a bomb it is necessary to treat a few cwt. of uranium by a process which will separate from the uranium its light isotope (U_{235}) of which it contains about 0.7%. Methods for the separation of isotopes have recently been developed. They are slow and they have not until now been applied to uranium, whose chemical properties give rise to technical difficulties. But these difficulties are by no means insuperable. We have not sufficient experience with large-scale chemical plant to give a reliable estimate of the cost, but it is certainly not prohibitive.

It is a property of these super-bombs that there exists a 'critical size' of about one pound. A quantity of the separated uranium isotope that exceeds the critical amount is explosive; yet a quantity less than the critical amount is absolutely safe. The bomb would therefore be manufactured in two (or more) parts, each being less than the critical size, and in transport all danger of a premature explosion would be avoided if these parts were kept at a distance of a few inches from each other. The bomb would be provided with a mechanism that brings the two parts together when the bomb is intended to go off. Once the parts are joined to form a block which exceeds the critical amount, the effect of the penetrating radiation always present in the atmosphere will initiate the explosion within a second or so.

The mechanism which brings the parts of the bomb together must be arranged to work fairly rapidly because of the possibility of the bomb exploding when the critical conditions have just only been reached. In this case the explosion will be far less powerful. It is never possible to exclude this altogether, but one can easily ensure that only, say, one bomb out of 100 will fall in this way, and since in any case the explosion is strong enough to destroy the bomb itself, this point is not serious.

We do not feel competent to discuss the strategic value of such a bomb, but the following conclusions seem certain:

1. As a weapon, the super-bomb would be practically irresistible. There is no material or structure that could be expected to resist the force of the explosion. If one thinks of using the bomb for breaking through a line of fortifications, it should be kept in mind that the radioactive radiations will prevent anyone from approaching the affected territory for several days; they will equally prevent defenders from reoccupying the affected positions. The advantage would lie with the side which can determine most accurately just when it is safe to re-enter the area; this is likely to be the aggressor, who knows the location of the bomb in advance.

2. Owing to the spreading of radioactive substances with the wind, the bomb could probably not be used without killing large numbers of civilians, and this may make it unsuitable as a weapon for use by this country. (Use as a depth charge near a naval base suggests itself, but even there it is likely that it would cause great loss of civilian life by flooding and by the radioactive radiations.)

3. We have no information that the same idea has also occurred to other scientists but since all the theoretical data bearing on this problem are published, it is quite conceivable that Germany is, in fact, developing this weapon. Whether this is the case is difficult to find out, since the plant for the separation of isotopes need not be of such a size as to attract attention. Information that could be helpful in this respect would be data about the exploitation of the uranium mines under German control (mainly in Czechoslovakia) and about any recent German purchases of uranium abroad. It is likely that the plant would be controlled by Dr. K. Clusius (Professor of Physical Chemistry in Munich University), the inventor of the best method for separating isotopes, and therefore information as to his whereabouts and status might also give an important clue.

At the same time it is quite possible that nobody in Germany has yet realised that the separation of the uranium isotopes would make the construction of a superbomb possible.

Hence it is of extreme importance to keep this report secret since any rumour about the connection between uranium separation and a super-bomb may set a German scientist thinking along the right lines.

4. If one works on the assumption that Germany is, or will be, in the possession of this weapon, it must be realised that no shelters are available that would be effective and could be used on a large scale. The most effective reply would be a counter-threat with a similar bomb. Therefore it seems to us important to start production as soon and as rapidly as possible, even if it is not intended to use the bomb as a means of attack. Since the separation of the necessary amount of uranium is, in the most favourable circumstances, a matter of several months, it would obviously be too late to start production when such a bomb is known to be in the hands of Germany, and the matter seems, therefore, very urgent.

5. As a measure of precaution, it is important to have detection squads available in order to deal with the radioactive effects of such a bomb. Their task would be to approach the danger zone with measuring instruments, to determine the extent and probable duration of the danger and to prevent people from entering the danger zone. This is vital since the radiations kill instantly only in very strong doses whereas weaker doses produce delayed effects and hence near the edges of the danger zone people would have no warning until it were too late.

For their own protection, the detection squads would enter the danger zone in motor-cars or aeroplanes which are armoured with lead plates, which absorb most of the dangerous radiation. The cabin would have to be hermetically sealed and oxygen carried in cylinders because of the danger from contaminated air.

The detection staff would have to know exactly the greatest dose of radiation to which a human being can be exposed safely for a short time. This safety limit is not at present known with sufficient accuracy and further biological research for this purpose is urgently required.

As regards the reliability of the conclusions outlined above, it may be said that they are not based on direct experiments, since nobody has ever yet built a super-bomb, but they are mostly based on facts which, by recent research in nuclear physics, have been very safely established. The only uncertainty concerns the critical size for the bomb. We are fairly confident that the critical size is roughly a pound or so, but for this estimate we have to rely on certain theoretical ideas which have not been positively confirmed. If the critical size were appreciably larger than we believe it to be, the technical difficulties in the way of constructing the bomb would be enhanced. The point can be definitely settled as soon as a small amount of uranium has been separated, and we think that in view of the importance of the matter immediate steps should be taken to reach at least this stage; meanwhile it is also possible to carry out certain experiments which, while they cannot settle the question with absolute finality, could, if their result were positive, give strong support to our conclusions.

March 1940

Appendix 6
THE UNIVERSITY OF BIRMINGHAM CYCLOTRON

UNIVERSITY OF BIRMINGHAM
DEPARTMENT OF PHYSICS

The Nuffield Cyclotron
by
Professor Sir Marcus Oliphant

When I joined the University of Birmingham in 1937 the Department of Physics was housed in the original Poynting Building and in some wooden huts, erected after the First World War, between the Poynting Building and the Library (Harding). With the exception of an L-shaped laboratory set aside for the professor's use, a roomy but cluttered basement, and a small room opening off a cloakroom, there was no space whatever which could be used exclusively for research purposes. The sum available for all purchases and maintenance of equipment for both teaching and research was £2,000 each year. The small workshop was quite inadequately equipped for research purposes, and the technical staff were very small in number and occupied, almost exclusively, with the teaching laboratories and lecture theatres.

This was a grim period for science in Britain. The country was recovering slowly from the great depression of the nineteen twenties, largely as a result of the rearmament imposed by the threat of Hitler's war in Germany, but real momentum had not yet been generated, industrial activity was far below normal, and money for any purpose was desperately short. Professor Moss, who was Dean of the Faculty of Science, had persuaded me to leave the Cavendish Laboratory, where I had spent ten happy and profitable years, in order to come to Birmingham to establish greater research activities in a Department where such opportunities had been frustrated through lack of funds and a heavy load of teaching carried by members of its staff. In my appointment interviews with the Faculty of Science, and Council, I had indicated that research in the newer area of physics required expenditures large compared with what the University had contemplated previously, a building in which to house complex apparatus and research workers, and some additions to the existing staff. The Faculty of Science, led by the Dean and Professor W.N. Haworth, endorsed my proposals, but was not hopeful of finding the money. The Council listened with respect to what I had to say, but received my request for money in stony silence. It was clearly necessary to seek the money required for ourselves.

It was my hope that we could build a larger cyclotron than those in Cambridge and Liverpool, so that we could study the nucleus with bombarding particles of greater energy than those available elsewhere in England and Europe. Professor E.O. Lawrence, the inventor of the cyclotron, was designing a large machine in Berkeley, and he kindly provided me with a provisional specification and estimates of what it would cost in the U.S.A. It was clear that about £40,000 was required for the cyclotron and a modest building in which to house it.

It seemed likely that industrial firms in the Midlands would be willing to help, either with donations in cash, or more easily perhaps, by making parts of the apparatus free of cost. I called upon the Chairman or Managing Director of every large firm in the area, was often entertained royally, and after spending hours looking round a works, returned with hope that a contribution would follow. But I was invariably disappointed. Forges in the area were prepared to make the steel parts, but only as a business proposition, even when the Chairman was a member of Council. Electrical industries would design what equipment they could supply, but would manufacture only if they were paid normal prices. The coppersmiths were interested and helpful, but business was so bad that they could not afford to do anything for nothing.

I had met Lord Austin (then Sir Herbert Austin) in Cambridge, to which University he had given a large sum to enlarge the Cavendish Laboratory. He invited me to take my wife to afternoon tea on a Sunday at his home in the country near Bromsgrove. I had an ancient Morris car, and realised as we approached the gates to the Austin estate, that it was scarcely diplomatic to arrive on a begging mission in a car produced by a rival manufacturer. So we left the car in the road and walked up the long winding drive to the large house. There we were received kindly and had an interesting chat over a pleasant tea. Before I could introduce the subject of my visit, Lady Austin spiked my guns most effectively. She was a formidable woman of humble origin, with an abrupt but disarming frankness in conversation. The conversation went like this:

"Nobody calls on us unless they want something. I suppose, like all the rest, you are after the sugar?" Recovering as rapidly as I could from such a shrewd blow, I replied, "Well, eh! I know that your husband is interested in science, and I was hoping ......" She interrupted me firmly, saying, "We have given away all that we can afford. Would you like another cup of tea?"

After an hour or two we were ushered to the door by the Austins. As we shook hands he asked me how we were getting home. I've forgotten my answer, but I did not feel as hopeless as Lady Austin's remarks indicated I should be, so I did not reveal our Morris car. He insisted upon sending us home in his chauffeur-driven car. I had then to cycle back to the Austin entrance, tie my bicycle onto the luggage carried at the back of the car, and return home. There was no donation.

These fruitless attempts to obtain the funds we needed in the Birmingham area led to exploration further afield. Professor Moss and I approached the then Prime Minister, Mr. Neville Chamberlain, a Birmingham man whose family had virtually founded the University. He was not of a warm disposition and did not give the impression that he could give much help, but he did indicate that he would look into the question. Sometime later, we were very pleased indeed to hear from Lord Nuffield that he was prepared to make a gift of £60,000 for our purposes, and he wrote to the University accordingly.

Our jubilation was short-lived. Professor Moss came to me shortly afterwards to tell me that the Pro-Chancellor had written to Lord Nuffield, thanking him for his generous donation, but requesting that the University be permitted to use this money for general purposes, rather than for Physics alone. Our anger was matched only by that of the Faculty of Science as a whole. There were some tense moments and some plain speaking, but the donation came to Physics.

It seemed wise to seek the fullest possible information about the design of the large cyclotron at Berkeley before embarking on our own. I was authorised by the Vice-Chancellor to visit California during the Christmas and New Year break at the end of 1938. Lawrence was most helpful, providing me with a full set of drawings and encouraging me to discuss details with all concerned. I returned with a much better appreciation of all parts of the cyclotron, including the tricky problems of the high frequency system. It was necessary, however, to modify the drawings extensively in order to adapt them to our needs. The Crocker cyclotron in Berkeley was intended primarily for research into the medical applications of the radiations produced, whereas we required our instrument for nuclear physics. The magnet of the American machine used a minimum of copper in its windings, and these were oil-cooled by a method which we were informed would be considered highly dangerous in a confined space in Birmingham. Investigation showed that a closed loop oil cooling system, with external heat exchanger, would be expensive, and that hollow conductors of suitable dimensions, for water cooling, could not be obtained in long lengths in England. So it was decided to use forced air cooling, despite the inconvenience of the large air-ducts required.

Fortune began to smile upon us. We were able to acquire from the Manchester Corporation a 600 k.W., D.C. generator driven by an 11,000 volt A.C. motor, at no cost beyond that of installation. Dr. McCance, of the Glasgow steel firm, Colvilles, agreed to supply the steel for the magnet, ready machined by Harland and Woolf, at a nominal cost. Plans for a very simple laboratory block were produced rapidly and the building commenced. The local branch of I.C.I. Metals undertook to manufacture the copper strip required for the magnet coils for the cost of the raw copper required. The Birmingham Battery Co. made seamless tubes of copper for the dees and resonant vacuum lines.

Dr. R.R. Nimmo accepted the offer of the first Nuffield Research Fellowship for a year in order to help with the design and construction of the cyclotron. Dr. P.B. Moon had already joined the Department in October 1938. It was clear, therefore, that when the cyclotron was in operation we should be able to use it to advantage for work in nuclear physics.

At that time the only electrical supply in the Department of Physics was direct current obtained from the ancient power station operated in connection with the Department of Mechanical Engineering. It was necessary, therefore, to obtain a supply of alternating current at both 11 kV. and 230 - 400 volts, and this necessitated the provision of a transformer and switching station. Unfortunately, the Department of Physics had to meet the cost of this extra building, and of the provision of mains between it and the new laboratory.

Winding of the coils for the magnet was carried out in part of the wooden sheds. Our improvised winding rig was made with a second hand gear train located in the neighbourhood by S.R. Cornick. It was arranged that the pancake coils of 1" x 1/8" copper strip should be wound on this rig in a vertical position, on a wooden core, with paper insulation between turns, the whole being made rigid by applying a grey varnish, obtained from a leading electrical manufacturer, to the paper during winding. The copper strip was in lengths weighing about 100 lbs, so that many joints had to be made in each pancake. To avoid having to make joints in the side of the coils during assembly on the cyclotron, the pancakes were wound in pairs. When finished, each double pancake was slid along a steel shaft, supported at intervals, to the further side of the room. This was a long and tedious process. The varnish produced a nasty form of exzcema on the hands and arms of the skilled technician, Mr. D. Stanley, who did the winding, so that he had to take time off to recover. However, he completed the task excellently. The only real problem was that the wooden floor gave way under the weight of some tons of copper, so that the shaft on which the coils were mounted was bent into a series of curves, making subsequent handling rather difficult.

The steel from Glasgow was delivered by road in 1939 and stacked, as it was received, at the opposite end of the 15' deep pit from the location of the cyclotron itself, with the aid of a 5 ton crane running the length of the pit. A technique for sorting it and lifting it into position had been worked out. In a moment of enthusiasm, to see the magnet erected, Dr. Nimmo, with a young technician, Mr. D. Hudston, worked overtime on this process, though it was properly a task for four men. Unfortunately, one of the big steel plates, about 2" in thickness, 2'6" wide and 18' in length, and weighing more than a ton, proved unstable when stood on its edge and fell over, breaking badly both legs of both men. This accident delayed erection of the steel, not only because they were out of action for many weeks, but because it was necessary to introduce far more stringent rules of safety in handling the material. However, the suppliers had made an excellent job of both rolling and making the steel, and in the end it went together perfectly, to make a magnet frame and poles with all the precision we had requested, although its total weight was about 200 tons.

The copper coils were brought across from the shed, pancake by pancake, on an improvised hand-truck, held in a nearly vertical position. They were lifted with a 'spider' attached to the crane at the centre, slid into position through the gap between the poles, and lowered on to blocks of dry ice. This was necessary because the gap between coils, about ¼" did not allow removal of the lower carrying supports if the pancakes were lowered onto the radial insulating spacers between pancakes. The dry ice evaporated, placing the pancakes gently and accurately in position. The top coil was assembled in this way round the lower pole-piece, on the large brass plate which was to hold it in position. The whole was then lifted up to location round the upper pole-piece and bolted in position. Then, the lower coil was assembled in situ.

The copper dees were made for us by an old-fashioned firm of coppersmiths in Birmingham. The work was done entirely by hand, but when finished they were beautiful examples of the work of true artisans, accurately to dimensions and with a mirror finish.

The vacuum chamber was machined by Tangye from a rather poor bronze casting which was somewhat porous. The head of this firm was married to a sister of Hevesy, the originator of the use of radioactive isotopes as tracers in chemistry and biology. It had been hit hard by the depression and was in the hands of receivers. When returned to the laboratory the machining looked excellent, but when measured the locating faces for the pole tips proved to be more than a millimetre out of parallel. This was most disappointing, but the day was saved by Mr. M.P. Edwards, who gallantly undertook to scrape the surfaces by hand to make them parallel. Though an arduous task the result was better than we could have achieved by having the box remachined.

The coned casting for the outer dee-lines, and the large copper tubes, were threaded in our own small workshop. We had no machines which could handle such large pieces of metal. However, through the ingenuity of S.R. Cornick, and the enthusiasm of Mr. W.A. Holland, these were successfully and accurately machined by overhanging the tubes between the beds of two lathes, rotating them on improvised bearings, and by up-ending a small lathe so that it operated vertically, bolted to one of the stanchions in the workshop. This was a real triumph of which Holland was very proud.

War was looming. Some of us were introduced to the mysteries of radar during the summer of 1939 under a scheme designed to ensure that, if war did come, there would be a body of scientists able to contribute rapidly and effectively to the development of the air defences of Britain. Work on the cyclotron ceased abruptly when war broke out and, until 1943, the laboratory was engaged upon the production of new devices for the generation of radio waves of very short wavelength. By 1943 we felt that we had contributed all we could to radar, and the laboratory returned to work on nuclear energy. The cyclotron magnet was completed with the object of using it for experiments upon the electromagnetic separation of the isotopes of uranium. Only preliminary investigations had been made when the decision was made that the team should move to America.

The cyclotron was completed after the war. I was able to persuade a war-time propaganda department of the Government to let us have, on loan, a mercury arc rectifier system to replace the unit made for us in 1939 by G.E.C., which never worked. A continuously exhausted oscillator was designed and assembled by Mr. S.M. Duke, and the circuitry by R. Wilson, R.R. Nimmo, G. Fertel and Mrs. Skyrme (then miss D. Millest). Magnet field stabilization was the work of J. Blamey. Although I played an active part in this work I was more closely concerned with the proton synchrotron. Thus others are more competent to record the subsequent history of this piece of equipment.

Appendix 7
THE SCIENTIFIC ADVISORY COMMITTEE PANEL SUMMARY OF THE MAUD REPORT

The Power Project

1. We regard this as a long term project. The United Kingdom Government should however maintain a close control over research and development. The matter should not be allowed to fall into the hands of private interests but should be pursued in close collaboration with the Canadian and United States Governments. The responsibility for developments in this country might appropriately be entrusted to the Department of Scientific and Industrial Research.

The Bomb Project

2. On the evidence before us we are satisfied that the development of the uranium bomb should be regarded as a project of the very highest importance.

3. The estimates before us of the time required to bring it to fruition varied from two to five years. We expect that the lower estimate will be found to be too short.

4. All steps should therefore be taken to press on with the work as rapidly as possible. At the present stage the work to be done falls under the following heads:

(i) Direct measurement of the fission cross-section of U235.
(ii) Design of fusing mechanism.
(iii) Further laboratory work to complete the design of the pilot separation plant.
(iv) Construction of pilot plant consisting of two ten-stage units.
(v) Development work on chemical processes.
(vi) All necessary preliminary steps to enable the construction and installation of the full-scale separation plant to be begun as rapidly as possible when the final requirements are known.

5. The Medical Research Council should be invited to nominate experts to co-operate with the M.A.U.D. Committee and with the Experimental Station at Porton in a fuller study of the range and extent of the radioactive effects of the bomb explosion and the feasibility of obtaining such effects by a more gradual release of the energy.

6. No final decision to proceed with the construction of the full-scale plant should be taken until the results of the work referred to in (4) and (5) above are available and have been subjected to careful and independent review.

7. We recommend that one pilot plant should be constructed in this country provided that the necessary priority can be assigned to it.

8. There are strong technical arguments for considering that one pilot plant and the full-scale separation plant should be assembled in Canada, the necessary components being manufactured in the United States.

9. Adequate arrangements should be made to secure control of uranium supplies for the use of the Governments concerned.

Appendix 8
THE BRITISH TEAMS IN THE MANHATTAN PROJECT

Margaret Gowing, in *Britain and Atomic Energy: 1939-1945*, 1964, Macmillan & Co., Ltd., London, lists the people engaged on Maud Committee work and the three teams of British people engaged in the Manhattan Project and related research. The lists in the above book names 44 people engaged in Maud work and 140 people who participated in the Manhattan Project - there being some duplication as researchers went on from Maud work to the Manhattan Project.

The members of the Maud Policy Committee were as follows:

Chairman: Professor Thomson
Members: Professors Blackett, Chadwick, Cockcroft, Ellis, Haworth and Simon

The members of the Maud Technical Committee were as follows:

Chairman: Professor Thomson
Members: Professors Mott, Oliphant and Peierls
Doctors Blackman, Bretscher, Feather, Frisch, Halban, Johnson, Kowarski, Mann and Moon
Representates of I.C.I. and Professor Lindemann
Secretary: Dr. Dickens (of the Ministry of Aircraft Production)

The teams engaged in the universities in 1940-1 on Maud work (though not necessarily paid out of H.M.G. money) were:

Birmingham: Professors Peierls and Haworth
Doctors Johnson and Fuchs (from 28th May 1941)

Cambridge: Cavendish Laboratory
Professor Peacock
Doctors Bretscher, Feather, Halban, Kowarski, Freundlich and Kemmer
Mr. Fenning, Cook, Birtwhistle, Manning and Miss Murrell

Liverpool: Professor Chadwick
Doctors Frisch, Rotblat, Pickavance and Pryce
Mr. Holt, Rowlands and Moore

Bristol: Doctors Nunn May and Powell
(Doctors Heitler, Fröhlich and London were sometimes consulted on specific problems)

Oxford: Professor Simon
Doctors Kurti and Kuhn
Mr. Arms and Llewellyn
(Professor Dirac was consulted on theoretical points)

The British team at Los Alamos (site 'Y') was as follows:

E.	Bretscher	J.	Hughes*	P.B.	Moon	J.	Rotblat*
N.	Bohr	D.J.	Littler	R.E.	Peierls	H.	Sheard
J.	CHADWICK	C.	Mark	W.G.	Penney	T.H.R.	Skyrme
A..P.	French	W.G.	Marley	G.	Placzek	E.W.	Titterton

O.R.	Frisch*	D.G..	Marshall*	M.J.	Poole	J.L.	Tuck
K.	Fuchs						

The British team at Berkeley and Tennessee that worked on the electromagnetic separation of the fission isotopes were:

W.D.	Allen	S.M.	Duke	M.J.	Moore*	F.	Smith
T.E.	Allibone	M.P.	Edwards	H.J.	Morris	D.F.	Stanley
J.P.	Baxter	H.J.	Emeléus	R.R.	Nimmo	P.P.	Starling
O.	Bunemann	H.E.	Evans	M.L.	Oliphant	H.S.	Tomlinson
E.H.	Burhop	M.E.	Haine	G.	Page	C.S.	Watt
J.D.	Craggs*	A.G.	Jones	S.	Rowlands*	M.H.F.	Wilkins
S.C.	Curran	J.P..	Keene	J.	Sayers	K.J.R.	Wilkinson
Mrs.	Curran	H.S.W.	Massey	H.	Skinner	R.M.	Williams
R.H.	Dawton	C.J.	Milner	A.A.	Smales		

List of British-paid Scientific Staff employed at the Montreal Laboratory up to the end of World War Two:

J.D. Cockcroft
H. Halban

F.T.	Adler	J.	Diamond	J.V.	Jelley	D.T.	Roberts
A.H.	Allen	J.V.	Dunworth	K.D.B.	Johnson	H.	Seligman
C.B.	Amphlett	A.C.	English	N.	Kemmer	B.S.	Smith
G.S.	Anderson	F.W.	Fenning	B.B.	Kinsey*	K.F.	Smith
H.S.	Arms	G.J.	Fergusson	L.	Kowarski	R.	Spence
W.J.	Arrol	B.H.	Flowers	N.Q.	Lawrence	J.F.	Steljes
P	Auger	H.F.	Freundlich	J.R.	Leicester	F.	Sterry
A.F.	Barr	K.D.	George	A.G.	Maddock	J.	Sutton
S.G.	Bauer	C.W.	Gilbert	P.M.	Milner	J.	Thewlis
D.V.	Booker	A.H.	Gillison	J.S.	Mitchell	H.	Tongue
W.E.	Burcham	D.W.	Ginns	F.	Morgan	N.	Veall
R.	Callow	B.L.	Goldschmidt	W.K.R.	Musgrove	A.C.	Ward
H.	Carmichael	H.	Greenwood	R.E.	Newell	J.B.	Warren
P.E.	Cavanagh	J.W.G.	Gregory	A.	Nunn May	C.N.	Watson-Munro
K.F.	Chackett	J.	Guéron	F.A.	Paneth	D.	West
S.G.	Cohen	E.A.	Guggenheim	H.R.	Paneth	C.H.	Westcott
H.C.	Cole	R.G.	Hanna	C.O.	Peabody	W.J.	Whitehouse
G.B.	Cook	B.G.	Harvey	G.	Placzek	G.R.	Wilkinson
P.	Gorey	H.G.	Heal	B.	Pontecorvo	R.	Wilkinson
E.	Courant	H.G.	Hereward	H.	Preston-Thomas	W.W.	Young
T.E.	Cranshaw	R.P.	Hudson	M.H.L.	Pryce*		
B.	Davison	J.F.	Jackson	C.	Reid		

* Liverpool University based

LIST of SOURCES and LOCATION

Section 1. Primary un-Published Sources and Location.

1.1. Liverpool University 37" Cyclotron Log Books; — Chadwick Archive, University of Liverpool

Book Number	Date
1a	21st March 1938 - 12th July 1941
1b	20th February 1939 - 13th June 1939
2	14th July 1941 - September 1944
3	1st November 1945 - 21st February 1946
4	12th March 1945 - 2nd April 1948
5	3rd April 1948 - 28th October 1950
6	30th October 1950 - 7th July 1954
7	8th July 1954 - 30th December 1958

1.2. Liverpool University Monthly Progress Reports, July 1942 to June 1947 (with the exception of Reports for August 1942 and February 1947); — Chadwick Archive, Churchill Archive Centre, Churchill College, University of Cambridge and Chadwick Archive, University of Liverpool

1.3. J.R. Holt's Laboratory Notebooks; — Chadwick Archive, University of Liverpool

Book Number	Title and Date
1	Miscellaneous Notebook, 1939 - 1951
2	Notes and Colloquium Talks to 1945
3	Experiments in Radioactivity, 1940
4	Experiments with Linear Amplifier and Cyclotron, 1941
5	Notebook No. 2, 1941-42
6	Notebook No. 3, 1944-45
7	Notebook No. 4, 1945-50

1.4. Correspondence; — Chadwick Archive, University of Liverpool

Chadwick and T. Beaumont
Chadwick and E. Bretscher
Chadwick and J.M. Cassels
Chadwick and J.D. Cockcroft
Chadwick and W.H. Hatfield
Chadwick and M.C. Henderson
Chadwick and B.B. Kinsey
Chadwick and E.W. Marchant
Chadwick and Metropolitan-Vickers Staff including
- T.E. Allibone
- G. Burrows
- H.S. Carter
- A.P.M. Fleming
- L.W.M. Husbands
- P. Lloyd
- J. McKerrow
- L.H.J. Phillips
- P.P. Starling

Chadwick and G.H. Nisbett
Chadwick and E. Sayle

Cockcroft and T. Beaumont

Kinsey and G.W.G. Coyle
Kinsey and E.O. Lawrence

Moore and J.M. Dodds

Rutherford and A.P.M. Fleming

1.5. D. Cooksey's Memorandum to Professor Pegram; — Chadwick Archive, University of Liverpool

1.6. Transcribed oral taped interviews:

Chadwick and A. Merrison; — Chadwick Archive, University of Liverpool

Rotblat and A.P. Brown; — Rotblat Archive, University of Liverpool

Rotblat, D.N. Edwards and C.D. King; — Rotblat Archive, University of Liverpool

1.7. University of Liverpool Nuclear Physics Building Committee Minutes; — Chadwick Archive, University of Liverpool

1.8. Location Maps and Building Plans; — Chadwick Archive, University of Liverpool

1.9. Numerous Photographs, by ex students and by commercial organisations; — Chadwick Archive, University of Liverpool

1.10. Personal Correspondence:-

King and A. Amery
King and A.P. Brown
King and W. Burcham
King and Miss J. Chadwick
King and P. Gavin
King and L.L. Green
King and Miss S. Hinokawa
King and J.R. Holt
King and D.G. Hurst
King and B.B. Kinsey
King and A. Kramish
King and M. Okamoto
King and M.L.E. Oliphant/Mrs. Vivian Wilson (daughter)
King and T.C. Randle
King and P. Reynolds
King and J.R. Rotblat
King and S. Rowlands
King and E. Sayle
King and Miss N. Welch
King and W. Williams

1.11. History of Science and Technology Seminar, 8th February 1988, Lecture by J.R. Holt, 'Sir James Chadwick at Liverpool; Personal Reminiscences of the Nobel Laureate with Archive Film.'; — Chadwick Archive, University of Liverpool

Chadwick's Centenary Lectures, Liverpool University, October 1991, including contributions by Holt, Oliphant and Peierls:
[see also Holt, J.R., 'Reminiscences and Discoveries; James Chadwick at Liverpool', *Notes Rec. R. Soc. Lond.*, **48**(2), p.299-308, (1994)].

1.12. Physics Department Files, University of Liverpool, 1935-1952, (Restricted), — Chadwick Archive, University of Liverpool

1.13. Minutes, *Nuclear Physics Committee* (NPC): (Restricted) — Rotblat Archive, University of Liverpool
: NPC(M)3, 27th January 1949;
: NPC 26, 4th August 1949;
Minute supplement; Skinner, H.W.B., 'Estimate of Running Costs of Liverpool Nuclear Physics Equipment':
16th February 1950
: NPC(M)4, 23rd February 1950;
: NPC(M)5, 21st November 1950;
: NPC(M)6, 12th July 1951;

1.14. Institute of Electrical Engineers, Film; 'James Chadwick': — Chadwick Archive, University of Liverpool

Section 2. Primary Sources and Location

2.1. Atomic Energy Report; — Public Records Office, Kew, Richmond, Surrey, TW9 4DU
Br-46; Public Records Office (P.R.O.) No. AB4/497:
Br-47; P.R.O., No.AB4/481:
Br-49; P.R.O., No.AB4/502:

2.2. Holt J.R., Mountford, J., 'Sir James Chadwick', *University of Liverpool Recorder*, October 1974: — Sydney Jones Library, University of Liverpool (ref; SJL LF379.5.A1.R1(66)

2.3. Transcribed oral taped interview: Chadwick and C.Weiner; — Neils Bohr Library, Center for History of Physics, American Institute of Physics

2.4. *Biographical Memoirs of Fellows of the Royal Society*:
'James Chadwick', by Sir Harrie Massey and Norman Feather, 1976, Volume 22, p.11-70;
'Otto Robert Frisch', by Sir Rudolph Peierls, 1981, Volume 27, p.283-306
'T.G. Pickavance', by John R. Holt, 1993, p.305-323

2.5. Thesis presented for the award of Doctor of Philosophy, University of Liverpool: J.R. Holt, 1942; S. Rowlands, 1942; P. Reynolds, 1944;	Thesis Library Department of Physics University of Liverpool
2.6. Chadwick, J., *Some personal notes on the search for the neutron*, u.d.,	Chadwick Archive University of Liverpool (ref. D.97/4)
Chadwick, J., *Rutherford: The Pioneer*, u.d.	Chadwick Archive University of Liverpool
2.7. The Chadwick/Lawrence Correspondence, November 1935 to January 1947:	The Bancroft Library University of California Berkeley California 94720 and Chadwick Archive University of Liverpool
2.8. Tape recording of radio broadcast interview, Sir Marcus Oliphant and Radio Merseyside, October 1991:	Chadwick Archive University of Liverpool
2.9. Edwards, D.N. *Liverpool Who's Who in Physics (1881-1996)*:	Chadwick Archive University of Liverpool
2.10. Edwards, D.N. 'Sir James Chadwick (1891-1974), *Lecture to the British Association*, Plymouth 1991.	Chadwick Archive University of Liverpool
2.11. *The Amersham Story, The Forties;* *The Fifties;* *The Sixties;* *The Seventies*	Radio Chemical Centre Amersham Bucks

Section 3. Secondary Sources

3.1. Chadwick's published papers:

1. 1912 (with E. RUTHERFORD) A balance method for comparison of quantities of radium and some of its applications. *Proc. phys. Soc.* **24**, 141-151; also *Le Radium* **9**, 195-200.

2. The absorption of γ-rays by gases and light substances. *Proc. phys. Soc.* **24**, 152-156.

3. The γ-rays excited by the β-rays of radium. *Phil. Mag.* **24**, 594-600.

4. (With A.S. RUSSELL) Excitation of γ-rays by α-rays, *Nature, Lond.* **90**, 463.

5. 1913 The excitation of γ-rays by α-rays. *Phil. Mag.* **25**, 193-197.

6. (With A.S. RUSSELL) The excitation of γ-rays by the α-rays of ionium and radiothorium. *Proc. R. Soc. Lond.* A **88**, 217-229.

7. 1914 (With A.S. RUSSELL) The γ-rays of polonium, radium and radioactinium. *Phil. Mag.* **27**, 112-125.

8. Intensitätsverteilung im magnetischen Spektrum der β-Strahlen von Radium B + C. *Dt. Phys. Gesell. Verh.* **16**, 383-391.

9. 1920 The charge on the atomic nucleus and the law of force, *Phil. Mag.* **40**, 734-746.
10. 1921 (With Sir E. RUTHERFORD) The disintegration of elements by α-particles. *Nature, Lond.* **107**, 41.
11. (With Sir E. RUTHERFORD) The artificial disintegration of light elements. *Phil. Mag.* **42**, 809-825.
12. (With E.S. BIELER) The collisions of α-particles with hydrogen nuclei. *Phil. Mag.* **42**, 923-940.
13. 1922 (With Sir E. RUTHERFORD) The disintegration of elements by α-particles. *Phil. Mag.* **44**, 417-432.
14. ... distribution in the β-ray spectra of radium B and C. *Proc. Camb. Phil. Soc.* **21**, 274-280.
15. 1924 (With Sir E. RUTHERFORD) The bombardment of elements by α-particles. *Nature, Lond.* **113**, 457.
16. (With Sir E. RUTHERFORD) Further experiments on the artificial disintegration of elements. *Proc. phys. Soc.* **36**, 417-422.
17. (With Sir E. RUTHERFORD) On the origin and nature of the long-range particles observed with sources of radium C. *Phil. Mag.* **48**, 509-526.
18. 1925 (With P.H. MERCIER) The scattering of β-rays. *Phil. Mag.* **50**, 208-224.
19. (With Sir E. RUTHERFORD) Scattering of α-particles of atomic nuclei and the law of force. *Phil. Mag.* **50**, 889-913.
20. 1926 (With K.G. EMELÉUS) On the γ-rays produced by α-particles in different gases. *Phil. Mag.* **1**, 1-12.
21. Observations concerning the artificial disintegration of elements. *Phil. Mag.* **2**, 1056-1075.
22. 1927 (With Sir E. RUTHERFORD) The scattering of α-particles by helium. *Phil. Mag.* **4**, 605-620.
23. 1929 (With Sir E. RUTHERFORD) Energy relations in artificial disintegration. *Proc. Camb. Phil. Soc.* **25**, 186-192.
24. 1930 The scattering of α-particles in helium. *Proc. R. Soc. Lond.* A. **128**, 114-122.
25. (With G. GAMOW) Artificial disintegration by α-particles. *Nature, Lond.* **126**, 54-55.
26. 1931 (With J.E.R. CONSTABLE & E.C. POLLARD) Artificial disintegration by α-particles. *Proc. R. Soc. Lond.* A **130**, 463-489.
27. 1932 Possible existence of a neutron. *Nature, Lond.* **129**, 312.
28. (With J.E.R. CONSTABLE) Artificial disintegration by α-particles. Part II - Fluorine and aluminium. *Proc. R. Soc. Lond.* A. **135**, 48-68.
29. The existence of a neutron. *Proc. R. Soc. Lond.* A. **136**, 692-708.
30. 1933 The neutron and its properties. *Br. J. Radiol.* **6**, 24-32.
31. (With P.M.S. BLACKETT & G.P.S. OCCHIALINI) New evidence for the positive electron. *Nature, Lond.* **131**, 473.
32. The neutron (Bakerian lecture). *Proc. R. Soc. Lond.* A. **142**, 1-25.
33. 1934 (With D.E. LEA) An attempt to detect a neutral particle of small mass. *Proc. Camb. Phil. Soc.* **30**, 59-61.
34. (With P.M.S. BLACKETT & G.P.S. OCCHIALINI) Some experiments on the production of positive electrons. *Proc. R. Soc. Lond.* A **144**, 235-249.
35. (With N. FEATHER & W.T. DAVIES) Evidence for a new type of disintegration produced by neutrons. *Proc. Camb. Phil. Soc.* **30**, 357-364.
36. (With M. GOLDHABER) A 'nuclear photo-effect': disintegration of the diplon by γ-rays. *Nature, Lond.* **134**, 237-238.
37. (With N. FEATHER) Nuclear transformations produced by α-particles and neutrons. *Int. Conf. Phys., London 1934*, **1**, 95-111.
38. 1935 (With M. GOLDHABER) The nuclear photoelectric effect. *Proc. R. Soc. Lond.* A **151**, 479-493.
39. (With M. GOLDHABER) Disintegration by slow neutrons. *Nature, Lond.* **135**, 65.
40. (With M. GOLDHABER) Disintegration by slow neutrons. *Proc. Camb. Phil. Soc.* **31**, 612-616.
41. 1937 The neutron and its properties (Nobel lecture). *Les prix Nobel en 1935.*

42. (With N. FEATHER & E. BRETSCHER) Measurements of range and angle of projection for the protons produced in the photo-disintegration of deuterium. *Proc. R. Soc. Lond.* A **163**, 366-375.
43. (With A.S. EVE) Lord Rutherford. *Obit. Not. Fell. R. Soc. Lond.* **2**, 395-423.
44. The cyclotron and its applications. *Nature, Lond.* **142**, 630-634; also *Proc. R. Inst.* **30**, 398-399.
* 45. 1940 New applications of Physics to Medicine. *Mem. Proc. Manch. Lit. & Phil. Soc.* **84** (1931-41), 9-22.
* 46. (With A. NUNN MAY, T.G. PICKAVANCE & C. POWELL) Excited states of stable nuclei. *Nature*, Lond. **145**, 893-4.
* 47. 1944 (With A. NUNN MAY, T.G. PICKAVANCE & C. POWELL) An investigation of the scattering of high energy particles from the cyclotron by the photographic method. *Proc. R. Soc. Lond.* A **183**, 1-25.
* 48. 1947 Atomic Energy (Lloyd Roberts lecture). *The Lancet*, **CCLII** (1), 315-320.
49. 1954 The Rutherford Memorial Lecture, 1953 (Montreal). *Proc. R. Soc. Lond.* A **224**, 435-447.
50. 1964 Some personal notes on the search for the neutron. *Proc. X Int. Cong. Hist. Sci., Ithaca 1962*, **1**, 159-162.
51. 1971 With Rutherford at Manchester. (Contribution to Ernest Marsden. C.A. Fleming, *Biogr. Mem. Fellows R. Soc. Lond.* **17**, 463-496).

* Not found in Chadwick's list in his *Biographical Memoirs of Fellows of the Royal Society.*

Section 4. Books by Chadwick

1921 *Radioactivity and radioactive substances.* 4th ed. (revised J. Rotblat). 1953. London: Sir Isaac Pitman and Sons.

1930 (With Sir E. RUTHERFORD & C.D. ELLIS) *Radiations from radioactive substances.* Cambridge University Press.

1962-5 (editor) *Collected papers of Lord Rutherford of Nelson.* Vol. 1, 1962; vol. 2, 1963; vol. 3, 1965. London: George Allen and Unwin.

Section 5. Published Secondary Sources containing Quotations used in Thesis

Allen, W.D., *Neutron Detection* (George Newnes Ltd., 1960)

Arnold, Lorna, *A Very Special Relationship: British Atomic Weapons Trials in Australia* (London, 1987)

Brown, Andrew, *The Neutron and the Bomb: A biography of Sir James Chadwick* (Oxford University Press, 1997)

Cathcart, Brian, *Test of Greatness: Britain's Struggle for the Atomic Bomb* (Murray, 1994)

Feynman, Richard P., *'Surely You're Joking, Mr. Feynman!'*, London, Unwin Hyman (Paperback) Ltd., 1986

Frisch, Otto R., *What Little I Remember* (Cambridge University Press, 1979)

Gowing, Margaret, *Britain and Atomic Energy 1939-1945* (London, Macmillan, 1964)

Gowing, Margaret; Arnold, Lorna, *Independence and Deterrence, Britain and Atomic Energy 1945-1952*:- Volume 1, *Policy Making* (Macmillan, 1974)
Volume 2, *Policy Execution* (Macmillan, 1974)

Groves, Leslie M., *Now it can be told. The Story of the Manhattan Project* (Da Capo Press, reprinted 1983)

Hamby, Alonzo, L., *Man of the People: A Life of Harry S. Truman*, Oxford University Press, 1995 [Reviewed in *History Today*, Volume 45 (8), August 1995, pps. 18-25]

Hartcup, Guy; Allibone, T.E., *Cockcroft and the Atom* (Adam Hilger Ltd., 1984)

Heilbron, J.L.; Seidel, Robert W., *Lawrence and his Laboratory: A history of the Lawrence Berkeley Laboratory, Volume 1* (University of California Press, 1989)

Hermann, A.; Krige, J.; Mersits, U.; Pestre, D., *History of CERN, Volume 1; Launching the European Organization for Nuclear Research* (North-Holland Publishing Co., 1987)

Hoddeson, L.; Henriksen, P.W.; Meade, R.; Westfall, C.L., *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945* (Cambridge University Press, 1993)

Ketchum, J.D., *Ruhleben: a prison camp society* (University of Toronto Press, 1965)

Kramish, Arnold, *The Griffin* (Houghton Mifflin Co., 1986)

Matsui, Makinosuke (English version edited by Ezawa, Hiroshi), *Sin-itiro Tomonaga - the Life of a Japanese Physicist* (MYU K.K., 1995)

Shimizu, Sakae (editor), *Hiroshima Atomic Bomb, August 1945 and Super-Hydrogen Bomb Test at Bikini Atoll in the Mid-Pacific, March 1954* (Katsuhiko Yazaki, Kyoto Forum, July 1995)

Smyth, H.D., *Atomic Energy for Military Purposes* (Princeton University Press, 1948)

Szasz, Terence Morton, *British Scientists and the Manhattan Project: The Los Alamos Years* (Macmillan, 1992)

Section 6. Newspaper Articles

Liverpool Daily Post, 7th February 1938
4th March 1946

New York Times, 29th February 1932
13th August 1945

Sunday Express, London, 30th April 1939

The Times, 14th August 1925
20th March 1946
23rd March 1946

STAFF BIOGRAPHIES

Biographies of the following ex-staff members may be found in *Biographical Memoirs of Fellows of the Royal Society*:

J. Chadwick, **22**, 1976; O.R. Frisch, **27**, 1981; T.G. Pickavance, **39**, 1993; C.F. Powell, **17**, 1971:

Selected Brief Biographies:

1. John Riley Holt, F.R.S., (1918 - date)

Schools: Local elementary school and then the Runcorn County Secondary School
1934, started 4 year (Physics) course at Liverpool University.
1938, gained a First in Physics, and registered at Liverpool University for a Ph.D.
1940, had completed measurements for Ph.D. and started work on the 37" cyclotron.
1942, awarded Ph.D. and made a Demonstrator in the Department of Physics.
1944, October, transferred to the Cavendish Laboratory, University of Cambridge.
1945, September, returned to Liverpool University.
1963 (May)-1965 (May), seconded to Daresbury Laboratory as Head of Magnet Design Group.
1966, Awarded Personal Chair in the Department of Physics.
1983, retired as Professor Emeritus from Liverpool University.

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2. Bernard B. Kinsey, (approx.1912 - November 1995)
(A.P. French is to publish Kinsey's biography in the Journal of the American Institute of Physics.)

1933, Post-Doctoral appointment at Berkeley Laboratory with E.O. Lawrence.
1936, October, awarded Oliver Lodge Fellowship at Liverpool University.
1938, October - 1942, October, Leverhulme Fellow and Lecturer, in Department of Physics at University of Liverpool.
1942 - 1944, seconded to radar development at T.R.E. Malvern and Swanage.
1944, (July), Member of Tube Alloys Team, working on Cambridge and then Cambridge and Liverpool 37" cyclotrons.
1945, Transferred to Montreal, becoming member of staff there.
Awarded Chair in Physics.
Died November 1995.

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3. Michael John Moore. (1911 - 1987)

1911, Born in Cork, Eire.
1937, 4th January, commenced working at the University of Liverpool, Department of Physics, as a `Mechanic' after completing a mechanical engineering apprenticeship at Metropolitan-Vickers Ltd., of Old Trafford, Manchester.
1943, 1st February, transferred to salaried staff.

1944, Transferred as team member to Manhattan District Engineering Project at Berkeley Laboratory, California.
1946, 1st October, on return to the U.K., promoted to `Research Assistant and Special Lecturer in Technical physics'.
1951, Promoted to Lecturer in Physics.
1954, Awarded `ex-officio' Master of Science, presented at the Degree Ceremony held in the Philharmonic Hall, Liverpool, at 11.30 a.m. on 15th December.
1955, Promoted to Senior Lecturer.
1963, Resigned from the University of Liverpool to become Head of the Engineering Division, Daresbury Nuclear Laboratory, Warrington.
1967, November, elected to the Lancashire County Aldermanic Bench.
Appointed Justice of the Peace.
1968, Awarded O.B.E. in New Years Honours List.
1974, October, Appointed Deputy Chairman of the Merseyside and North Wales Electricity Consultative Council,(M.N.W.E.C.C.).
1974 - 1979, Appointed as a member of the Government Committee of Inquiry into the Management and Government of Schools.
Made Knight of the Order of St. Gregory the Great, by his Holiness, Pope Paul V1.
1977, Appointed Chairman of MNWECC.
1979, Retired from Daresbury.
1987, Died.

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4. Joseph Rotblat, (1909 - date)

1909, born Warsaw, Poland.
Attended Warsaw University.
As a post Doctoral Researcher at Warsaw University, worked under Professor Wertenstein.
1939, awarded Polish Research Fellowship.
March, arrived at Liverpool University.
August, returned to Poland to bring wife to Liverpool, but she had appendicitis and could not travel. Returned without her.
1st September, Poland overrun by German troops.
October, awarded Oliver Lodge Fellowship and Lecturship.
1st November, started lecturing on nuclear physics.
1944, member of Manhattan Engineering District Project at Los Alamos.
December, returned to Liverpool University.
1945, Appointed joint acting Head of Department (Research) with Roberts (Teaching).
1948, Accepted Chair in Biophysics at St. Bartolomew's Hospital, London.
1996, Awarded joint Nobel Peace Prize (with Pugwash).

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5. Harold J. Walke. (1911 - 1939)

Educated at Plymouth Corporation Grammar School and University College, Exeter.
1935, Awarded a Commonwealth Fund Fellowship, working under E.O. Lawrence for two years at the Radiation Laboratory, Berkeley, California.
1937, October, Awarded 1851 Exhibition Senior Studentship.

Joined the Department of Physics, University of Liverpool.

939, 21st December, Electrocuted and killed in a cubicle which housed cyclotron electrical apparatus. (Refer to letter, Chadwick to Lawrence, dated 24th December 1939, held in the Bancroft Library Archive).

Obituary Notice published in *Nature*, Number 3665, 27th Jan 1940, p.133.)

BIBLIOGRAPHY

Allen, W.D., *Neutron Detection* (George Newnes Ltd., 1960)

Alvarez, Luis W., *Adventures of a Physicist* (New York, 1987)

Andrade, E.N. da C., *Rutherford and the Nature of the Atom* (Heineman, 1965)

Archdiocese of Liverpool: Directory and Guide to Quarant' Ore, Volumes, 1947-51
1954-55
1957-59
1960-63

Arnold, Lorna, *A Very Special Relationship: British Atomic Weapons Trials in Australia* (London, 1987)

Arya, Atam P., *Fundamentals of Nuclear Physics* (Allyn and Bacon, Inc., 1966)

Bickel, Lennard, *The Deadly Element: The Story of Uranium* (New York, Stein and Day, 1980)

Birks, J.B., *Rutherford at Manchester* (Heywood and Co., 1962)

Boag, Rubinin and Schoenberg [editors], *Kapitza in Cambridge and Moscow* (Elsevier Science Publishers B.V., Amsterdam)

Brooke, *A History of the University of Cambridge* (Cambridge University Press, 1964)

Brown, Andrew, *The Neutron and the Bomb: A biography of Sir James Chadwick* (Oxford University Press, 1997)

Burrows, E.H., *Pioneers and Early Years. A History of British Radiology*, Channel Islands, Colophon, 1986

Cathcart, Brian, *Test of Greatness: Britain's Struggle for the Atomic Bomb* (Murray, 1994)

Catholic Directory, 1962

Cockburn, Stewart; Ellyard, David, *Oliphant. The life and times of Sir Mark Oliphant* (Axiom Books, 1981)

Cockcroft, Sir John D., *Neils Henrik David Bohr* (Biographical Memoirs; The Royal Society, Volume 9, London, 1963)

Dalitz, R.H., Stinchcombe, R.B., *A breadth of Physics: the proceedings of the Peierls 80th birthday Symposium* (London, World Scientific Publishing Co., Ltd., 1988)

Dampier, Sir William Cecil, *A Shorter History of Science* (Cambridge University Press, 1945)

Duncan, T., *Advanced Physics: Fields, Waves and Atoms,* 2nd edition (John Murray, 1981)

Eijkelhof, Harrie M.C.; Boeker, Egbert; Raat, Jan H.; Wijnbeek, Niko J. [Translated by Eijkelhof-Booth, Jenny; Jones, Stephen B.; Williams, Bill], *Physics in Society* (Vu Boekhandel/Uitgeverij, Holland, July, 1981)

Fearnside, K.; Jones, E.W.; Shaw, E.N., *Applied Atomic Energy* (Temple Press Ltd., 1951)

Feynman, Richard P., *'Surely You're Joking, Mr. Feynman!'*, London, Unwin Hyman (Paperback) Ltd., 1986

Frisch, Otto R., *What Little I Remember* (Cambridge University Press, 1979)

Gamow, George, *The Birth and Death of the Sun* (Viking Press, 1940)

Gamow, George, *Thirty Years that shook Physics, the Story of Quantum Theory* (Heinemann, 1966 [Printed in Great Britain, 1972])

Glasstone, Samuel, *Sourcebook on Atomic Energy*, 3rd edition (D. van Nostrand Co., Inc., 1967)

Gowing, Margaret; Arnold, Lorna, *The Atomic Bomb* [Siscon Project], Butterworths, 1979

Gowing, Margaret, *Britain and Atomic Energy 1939-1945* (London, Macmillan, 1964)

Gowing, Margaret; Arnold, Lorna, *Independence and Deterrence, Britain and Atomic Energy 1945-1952*:- Volume 1, *Policy Making* (Macmillan, 1974)
:- Volume 2, *Policy Execution* (Macmillan, 1974)

Groves, Leslie M., *Now it can be told. The Story of the Manhattan Project* (Da Capo Press, reprinted 1983)

Hall, Tony, *Nuclear Politics, The History of Nuclear Power in Britain* (Penguin Books Ltd., 1986)

Hamby, Alonzo, L., *Man of the People: A Life of Harry S. Truman*, Oxford University Press, 1995 [Reviewed in *History Today*, Volume 45 (8), August 1995, pps. 18-25]

Hanson, Norwood Russell, *The Concept of the Positron, a Philosophical Analysis* (Cambridge University Press, 1963)

Harrup, Sylvia A., *Decade of Change, the University of Liverpool, 1981-1991* (Liverpool University Press, 1994)

Hartcup, Guy; Allibone, T.E., *Cockcroft and the Atom* (Adam Hilger Ltd., 1984)

Heilbron, J.L.; Seidel, Robert W., *Lawrence and his Laboratory: A history of the Lawrence Berkeley Laboratory, Volume 1* (University of California Press, 1989)

Hendry, John, *Cambridge Physics in the Thirties* (Hilger Ltd., 1984)

Hermann, A.; Krige, J.; Mersits, U.; Pestre, D., *History of CERN, Volume 1; Launching the European Organization for Nuclear Research* (North-Holland Publishing Co., 1987)

Hobsbawm, Eric, *Age of Extremes, the short twentieth century, 1914-1991* (Michael Joseph Ltd., 1994)

Hoddeson, L.; Henriksen, P.W.; Meade, R.; Westfall, C.L., *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945* (Cambridge University Press, 1993)

Holloway, David, *Stalin and the Bomb: The Soviet Union and Atomic Energy 1939-1956* (Yale University Press)

Hutton, Robert Salmon, *Recollections of a Technologist* (Sir Isaac Pitman and Sons, London, 1964).

Jungk, Robert, *Brighter than a Thousand Suns*, translated by James Cleugh (Penguin Books, 1958)

Kaplan, Irving, *Nuclear Physics* (Addison-Wesley Publishing Co., Inc., 1956)

Ketchum, J.D., *Ruhleben: a prison camp society* (University of Toronto Press, 1965)

Kramish, Arnold, *The Griffin* (Houghton Mifflin Co., 1986)

Küntzel, Matthias, *Bonn and the Bomb*, Ph.D. Thesis (Pluto, 1995)

Livingston, Stanley M.; Blewitt, John P., International Series in Pure and Applied Physics, *Particle Accelerators* (McGraw-Hill Book Co., Inc., 1962)

Matsui, Makinosuke (English version edited by Ezawa, Hiroshi), *Sin-itiro Tomonaga - the Life of a Japanese Physicist* (MYU K.K., 1995)

Moore, W., *Schrödinger: Life and Thought* (Cambridge University Press, 1992)

Moorehead, Alan, *The Traitors* (Hamish Hamilton, 1952)

Neilson, J.R., *Neils Bohr Collected Works*, Volume 1, Early Work (1905-1911) (North Holland Publishing Co., 1972)

Pais, Abraham, *Inward Bound: of matter and forces in the physical world* (Clarendon Press, Oxford, Oxford University Press, 1986)

Pais, Abraham, *Neils Bohr's times* (Oxford University Press, 1991)

Pincher, Chapman, *Into the Atomic Age* (London, 1947)

Secular Clergy in Order of Ordination, 1903 to 1962

Shimizu, Sakae (editor), *Hiroshima Atomic Bomb, August 1945 and Super-Hydrogen Bomb Test at Bikini Atoll in the Mid-Pacific, March 1954* (Katsuhiko Yazaki, Kyoto Forum, July 1995)

Sime, Ruth Lewin, *Lise Meitner: A Life in Physics* (University of California Press, 1996)

Smith, C.J., *Intermediate Physics* (Edward Arnold and Co., 1947)

Smyth, H.D., *Atomic Energy for Military Purposes* (Princeton University Press, 1948)

Sproull, Robert L.; Phillips, W. Andrew, *Modern Physics, The Quantum Physics of Atoms, Solids and Nuclei* [3rd edition] (John Wiley and Sons, 1980)

Szasz, Terence Morton, *British Scientists and the Manhattan Project: The Los Alamos Years* (Macmillan, 1992)

Taylor, A.J.P., *Beaverbrook* (Penguin Books, 1972)

Teller, Edward, with Brown, Allen, *The Legacy of Hiroshima* (New York, Doubleday and Co., Inc., Garden City, New York, 1962)

Waloschek, Pedro (English translation by Karen Waloschek), *The Infancy of Particle Accelerators: Life and Work of Rolf Wideröe* (Friedr. Vieweg and Sohn, 1994)

West, Nigel, *A Matter of Trust, MI5 1945-72* (Weidenfeld and Nicolson, 1982)

Williamson, Rajkumari, *The Making of Physicists* (Adam Hilger, 1987)

Wilson, R.S., *Investigations in Nuclear Physics* (Ph.D Thesis, University of Liverpool, 1947)