

**FUSION RESEARCH IN THE UK  
1945 - 1960**

**J Hendry and J D Lawson**

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## FOREWORD

By R S Pease, Director of Culham Laboratory (1967 - 1981)

When I was young, I and other children of the nineteen thirties were introduced to scientific endeavour and adventure by books such as 'Men who Found Out', where the discoveries in science are justly attributed to the outstanding genius of specific scientists. Of course the great men and women - Pasteur, Davy, Faraday, Madam Curie - had their assistants, spouses or close relatives to assist in the research, but the achievements were essentially individual. Even as we absorbed these thrilling accounts of the conquests of diseases and the mastery of electro-magnetic waves by single combat, so to speak, the pattern of individual hagiography was being undermined.

Physicists in Cambridge and California were developing the application of engineering to physics research and its concomitant techniques, of which the team method of research is pre-eminent. Men such as Cockcroft, Oliphant, Allibone and Kapitsa at Cambridge, Lawrence and McMillan at Berkeley, employed engineering industry to develop and build their apparatus, increased the laboratory budgets by several orders of magnitude (indeed openly boasting of this latter achievement) and introduced team research to nuclear physics. The age of innocence was passing.

The history of physics thenceforth becomes much more akin to military history, where the great men are the commanders and the quarter-master generals, where the views of politicians and of the tax payers have to be taken into account, and where the role of the military intelligence is played by the theoretician. Moreover the operations, or at least their results, are the subject of widespread and legitimate press comment, adding a new dimension to the rewards and hazards of research.

Just as military campaigns of dubious value or disastrous consequences have a lesson for us all and are the stuff of history, so too are the not always wholly successful research campaigns of modern physics - amongst which are some of the developments of nuclear energy. The successes and their impact on society are indeed recorded, but much of the interest lies in the inner history of human interaction in the highly technical environment. Whole armies of research workers can be marched in the wrong direction by mistaken intelligence or pigheaded commanders. Many readers will be amazed that laboratory directors, like generals, are not more perceptive, or at least more cautious, and that scientists allow the undoubted pressure of events, as well as human emotion, to affect decisions.

In both cases, the undertaking of history has another, more serious purpose, namely that of the post mortem. What actually happened? Why did the Duke of York march 10,000 men up the hill? Who is to blame? Who should take the credit? As Winston Churchill said, the purpose of recrimination is to enforce effective action in the future.

Here, at last we come to the nub and substance of this history of nuclear fusion by HENDRY and LAWSON: for fusion research is one of the most colourful of these research campaigns. It has at least one major episode, the affair of the Zeta experiment at Harwell, where the excitement of the research overrode the judgement of the

commanders, and left a lasting impression of the nature of nuclear physics research on politicians and public alike. The research has as a goal a form of energy which, if successfully developed, will rival and supplant conventional nuclear fission. Consequently the political pressure on its scientific leadership is very considerable.

Lawson and Hendry are especially well qualified to write this history of how the leaders and the workers responded to these pressures. Lawson worked at Harwell during the crucial years; he is renowned for his pioneering analysis - the Lawson criteria - encapsulating the technical objectives of the research; and yet he stood sufficiently far above the melee to preserve independence of judgement. Hendry's experience of the history of nuclear energy as a whole provides the overall background and the professional historian's discipline. Both have been thorough in their exploration of the original documentation and severe in cross-examination of the surviving actors.

How fusion research in the United Kingdom started, how it developed, got into great difficulties and how it recovered, is the essence of their story. Their lesson has a particular as well as a general significance, because fusion research is not yet successfully consummated. The present round of experiments on the Joint European Torus, JET apparatus at Culham has yet to be completed; their results, when they are available, will be used to decide, together with all other factors, whether or not to proceed with the 'International Tokamak Experimental Reactor' now being designed by a world team of engineers and scientists. This decision will be the next major milestone in magnetic fusion research.

I am sure that this history will instruct, inform and entertain the public at large about the nature of major research programmes. But it will also help those who have to lead the way forward in research on controlled nuclear fusion in the years ahead.

10 January 1993

## PREFACE

**This** history was started by the first author (JH) in 1980 as part of the official history of the United Kingdom Atomic Energy Authority, under the direction of Professor Margaret Gowing, the official historian. In 1981 a draft text was circulated (without figures and appendices) to a number of people who had worked in the field; their comments were noted, a revised draft was produced, and this was again circulated for comment. At this stage separate publication was not envisaged. However in 1987 the first part of the text, consisting mainly of the introductory chapter on the development of the underlying physics before the war and work up to about 1950, was published in *Annals of Science*<sup>(1)</sup>. Shortly afterwards the author moved on to other work. The second author (JDL), who had seen the complete draft in 1984, in 1989 agreed as a retirement activity to prepare it for issue in its present form. Having worked for a short while in the field (1954-6) and being acquainted with a number of those appearing in the history, he was able to add some material, but more particularly to enlarge on some of the physics; the basic framework of the work, and majority of the material relating to organization and policy, however, remains unchanged.

A further task was to prepare the references in a suitable form for publication; in the original these were frequently to AEA files, not then open in the Public Record Office. Most of these have since been deposited at the PRO, and they are referred to by their PRO members. An additional archive is being prepared by the second author; this will be deposited at the Churchill Archive Centre, Cambridge (CAC), and will contain unpublished material, including copies of papers in other archives. Further details are given below in the Notes on the References.

The first author conducted a number of interviews in 1981 and 1982, and references to these are given at appropriate points in the text, but transcripts are not available in the PRO.

The second author had further discussions and correspondence with many of those mentioned in the text, and would like to acknowledge their help. Only in the more important instances, however, is this specifically acknowledged in the references. Nevertheless some of the more interesting correspondence is deposited in the CAC. Several people read through a draft of the complete manuscript, and substantial comments were received from H A B Bodin, R Carruthers, G I W Llewellyn, R S Pease and P C Thonemann. In many places documentary material is sparse, recollections are not always consistent, judgements and guesses have to be made. No doubt there are errors, and credit unfairly attributed. This is particularly difficult to avoid also where the parallel work in other countries, not covered in this report, was in progress. To those concerned, we offer our apologies. Any corrections or important additional material covering the period of this report received by the authors or archivist will be welcome; an updated and corrected version will be held in the archive.

We should particularly like to acknowledge the encouragement and help given by Professor Margaret Gowing and latterly by Mrs Lorna Arnold. Our thanks are also due to Mrs M Gardiner and Mrs J Rogers of the Harwell Records Office, and Miss Anne Marshall, of AWE Archives, for allowing us access to the records, and providing information. Helpful information was also provided by Mr C A Carpenter of Culham Laboratory. We also thank the Master and Fellows of Trinity College

Cambridge for permission to see and quote as references papers in the G P Thomson archive there, and to deposit photocopies of some of these in the Churchill Archive Centre. The second author would like to acknowledge a grant from the Royal Society, and help from the Rutherford Appleton Laboratory in providing access to facilities during this work. He would also like to thank Mrs Pam Richens for typing the various drafts and help with the layout of the final manuscript. This report is produced and printed by the AEA Technology, Printing Services, whose help is gratefully acknowledged.

The photographs in Fig. 3 were kindly supplied by P C Thonemann, Figs. 4 and 8 by Dr A A Ware, Figs. 5 and 7 by the Harwell Photographic Archive, and Fig. 12 by H A Bodin. Fig. 9 is reproduced by courtesy of the Punch Archive.

J H Hendry, Cambridge  
J D Lawson, Abingdon

Correspondence and Enquiries, c/o

Authority Historians Office  
Building 77  
AEA Technology  
Harwell Laboratory  
OXON OX11 0RA

## ABBREVIATIONS USED IN TEXT AND REFERENCES

<b>AEA</b>	Atomic Energy Authority.
<b>AEC</b>	Atomic Energy Commission (USA).
<b>AEI</b>	Associated Electrical Industries (Laboratory at Aldermaston).
<b>AERE</b>	Atomic Energy Research Establishment (Harwell).
<b>AEX</b>	Atomic Energy Executive.
<b>AWRE</b>	Atomic Weapons Research Establishment (Aldermaston), later AWE.
<b>CAC</b>	Churchill Archive Centre.
<b>CERN</b>	European Council for Nuclear Research (Geneva).
<b>CTR</b>	Controlled Thermonuclear Reactions.
<b>CTRAC</b>	CTR Advisory Committee.
Geneva Conference	See Notes on the References p. 89.
<b>GPT</b>	G P Thomson Archive.
<b>HC</b>	Harwell Council.
<b>HMSO</b>	Her Majesties Stationery Office (London).
<b>HSC</b>	Harwell Steering Committee.
<b>ICSE</b>	Intermediate Current Stability Experiment.
IEE Convention	See Notes on the References p. 89.
<b>PDSC</b>	Publication and Declassification Sub Committee.
<b>PRO</b>	Public Record Office.
<b>RGMB</b>	Research Group Management Board.
<b>TTPC</b>	Thermonuclear Technical Policy Committee.
<b>TRE</b>	Telecommunications Research Establishment (Malvern), later RRE, then RSRE.

## CHAPTER 1

### INTRODUCTION

The developments in nuclear physics, gas discharges, and astrophysics in the pre-war years which were to make possible the serious consideration of fusion energy production after the war have been described in a previous paper<sup>(1)</sup>. This may be regarded as an introduction to the present report, which covers work done in the UK from 1945-1960, before the foundation of the Culham Laboratory, and indeed duplicates part of the previous paper which contains material up to 1950. As explained in the earlier paper there was by 1945 a clear *prima facie* possibility of a new energy source from the thermonuclear fusion of deuterium ions, and the information with which to calculate the conditions necessary for this was available. Although not yet observed for a gas, the pinch effect could be predicted with some assurance and seemed a promising way of keeping a deuterium plasma away from the walls of its containing vessel for long enough for the temperatures required to be reached. In a straight discharge tube there would still be tremendous energy losses from the end electrodes. But electrodeless discharges had been familiar ever since being studied many years before by J J Thomson<sup>(2)</sup>. Thomson's experiments were in cylindrical or spherical vessels, with no conductors or magnetic cores linking the discharge, but ingenious experiments in which the high frequency circulating current could be measured in a closed tube of rather complicated shape had been devised by Knipp and Knipp in 1931<sup>(3)</sup>. Following the recent development of circular particle accelerators it was relatively easy to envisage transferring energy to such a discharge for fusion purposes.

By the late 1930s these possibilities were very much in the air, and it is impossible to say when, where or by whom the feasibility of a deuterium fusion energy producer was first seriously considered. Hans Bethe has recalled a conversation with Leo Szilard on the subject in Washington in about 1937<sup>(4)</sup>. Indeed, when G P Thomson applied for an American patent for a fusion device he found that Szilard's 1934 patent on atomic energy covered the principle of fusion<sup>(5)</sup>. It has been suggested that Houtermans, who left Germany with the coming of the Nazis and ended up in Kharkov, was working on the project experimentally before he was interned by the Russians, again in 1937<sup>(6)</sup>. And Peter Thonemann has recalled working out the basic concept of a fusion reactor using a toroidal deuterium gas discharge while he was still a student in Melbourne in 1939<sup>(7,8)</sup>.

During the war the idea of a fusion bomb, more usually called a hydrogen or "Super" bomb, was the subject of intensive study by Teller and others at Los Alamos, and also appears to have been briefly considered in Germany<sup>(9)</sup>. Then in 1946 a group of leading Los Alamos scientists, including Teller, Tuck, Fermi, von Neumann, Alvarez, Landshoff, Kerst and R R Wilson, appear to have turned their attention to an informal study of the possibility of controlled thermonuclear fusion arising much as Thonemann had envisaged, in a toroidal deuterium or deuterium and tritium discharge<sup>(10)</sup>. In his paper to the 1958 Geneva Conference, Teller recalled that<sup>(10)</sup>:

Some elementary general facts were recognised at that time: That deuterium gas could react above an ignition point of approximately 35

kilovolts; that deuterium-tritium mixtures could react at a considerably lower temperature of a few kilovolts; that the gas should be introduced at an exceedingly low density of approximately  $10^{14}$  to  $10^{15}$  particles/cm<sup>3</sup> in order to make the reaction rates sufficiently slow and in order to avoid excessively high pressures; that at these high temperatures the gas will be completely ionised (such an ionised gas is called a plasma); that with the presence of magnetic fields, the ions in this plasma follow spiral paths and that by appropriate arrangement of the magnetic fields the losses to the walls can be reduced; that the pressure of the plasma on the field leads to a thermal expansion of the plasma which tends to stabilize the reaction (containment will break down before there is any chance of an explosion); that equilibrium with radiation is not established and that the energy emitted with bremsstrahlung should be treated as a loss; that for this reason atoms other than hydrogen isotopes must be eliminated as completely as possible; and that even the equilibrium between electron and positive ion energies will be complete at the highest temperatures. Very particularly it was also noticed that in a simple closed field along a torus, the particles will not continue to spiral indefinitely around the same magnetic line of force but that they will drift in a direction perpendicular to both the magnetic field and the field gradient. This leads to smaller but nevertheless prohibitive wall losses.

Teller's statement was obviously intended as a priority claim, and there is no evidence that the details were anything like so clearly recognised as he implied. Contrary to the impression given, controlled fusion was not apparently the subject of any systematic investigation, but only of Teller's "wild ideas" seminars, and it is not even clear who the participants were. There is no doubt that the subject was considered, however, and Teller's description of the conclusions reached is compatible with the most advanced knowledge available at the time to a group of the world's most distinguished physicists.

Despite their early initiative the American physicists (if we may so call a group of whom very few were native Americans) conducted no experiments along the lines suggested, and they appear to have lost interest in controlled fusion or at least to have become fully engaged on other projects such as the design and development of the fusion bomb instead. A parallel investigation by Tuck and Ulam, who collaborated in Los Alamos the same year (1946) on a theoretical analysis of the collision of high velocity jets of deuterium, also came to nothing, despite some experiments by Tuck using metal deuterides<sup>(10-13)</sup>. (Suggestions for such an approach to fusion were apparently made in a paper by Tuck and Ulam in 1944)<sup>(14)</sup>. In Great Britain, however, there were three separate initiatives in the immediate post-war period. Only two of these, one due to Sir George Paget Thomson and the other to Peter Thonemann, led to the early establishment of a continuing, if at first limited, experimental and theoretical research programme. The third initiative, which had begun even earlier, was under the direction of J M Meek at Liverpool University, where Sir James Chadwick was Professor of Physics. This was terminated after what were regarded as some unpromising experiments, the first designed specifically to look for neutrons in a deuterium discharge.

## CHAPTER 2

### THE PROGRAMME AT IMPERIAL COLLEGE

The initiative for this programme came from G P Thomson, then professor of physics at Imperial College London. Thomson had worked before the war both on nuclear physics and, with his father J J Thomson, on discharge physics, and he was therefore well placed to see the possibilities of fusion. According to his later recollections, Thomson began to think of controlled fusion processes towards the end of 1945, concentrating on a deuterium discharge in a torus<sup>(15)</sup>. His first idea, succinctly described in an undated note of about February 1946, was to contain the deuterium gas within a toroidal solenoid in a magnetic field of 0.5 to 1 Tesla<sup>(16)</sup>. (Toroidal geometry and the associated notation are illustrated in Fig. 1).

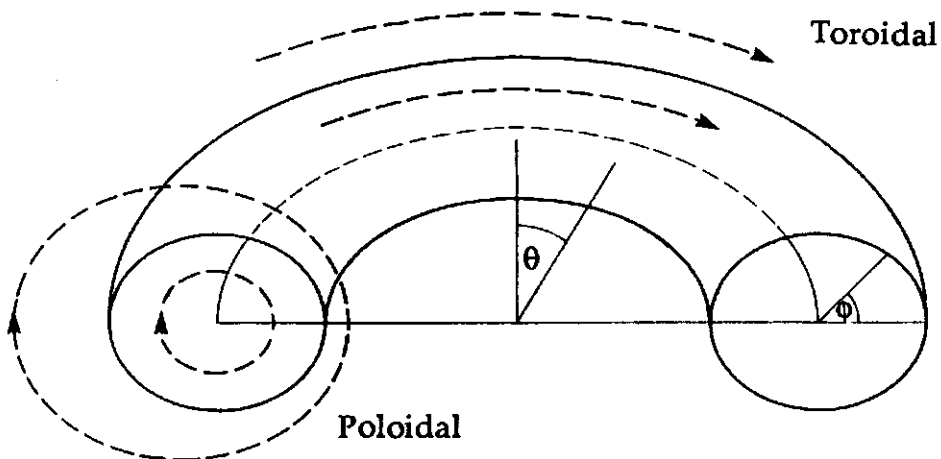


Fig. 1 Notation for toroidal geometry. Field and current components are described as "toroidal" if they are directed around the axis of symmetry, and "poloidal" if in a plane through the axis. Angles  $\theta$  and  $\phi$  are often used respectively for directions around the symmetry axis, and in the poloidal direction around the circular axis of the torus. Sometimes, especially when comparing with cylindrical systems, distances along the circular axis are denoted by  $z$ , in which case  $\phi$  is replaced by  $\theta$  for the poloidal angle.

He proposed to ionize the gas using an external source and then to heat up the plasma using an applied high-frequency (radio-frequency or 'RF') alternating current. Commencing with densities of  $10^{14}$  to  $10^{15}$  nuclei/cm<sup>3</sup>, he suggested that within a few minutes the deuterium nuclei could be heated to the order of 100 electron kilovolts (keV, one electron kilovolt being equivalent to roughly 10 million degrees Kelvin. This scale will be used throughout this report), at which point fusion energy would be generated. The electrons would be anchored by the magnetic field; the ions, less strongly held, would move towards the walls leaving a strong radial electric "space-charge" field which would contain the ions as they heated up<sup>(16)</sup>. He brought in Moses Blackman, a lecturer in the department, to assist him with some of a more difficult mathematics, and together they drew up a specification for a hypothetical device, described as a "toroidal solenoid"<sup>(17)</sup>. In this document a number of details had been worked out, including an estimated "practicable value" of the thermonuclear yield as 20 kW/litre in a torus with minor radius of 30 cms. The precise method of ionizing and heating the gas, however, was not yet specified. On the 26 March 1946 they met at Thomson's suggestion with Arthur Block of the

Ministry of Supply, then responsible for atomic energy matters, and with the Ministry's patent agent, B L Russell, who was requested to draw up a patent application<sup>(18)</sup>.

The background to Thomson's patent application was a curious one, for he was not over-concerned with establishing priority, or with any financial reward. He declared his willingness to assign the patent to the government and at this stage asked for nothing in return. What had happened was that at the beginning of March Thomson had sent a copy of his proposal to Rudolf Peierls, Professor of Theoretical Physics at Birmingham University asking for his comments. But Peierls, having learnt something of the Los Alamos discussions at first hand, was concerned about the confidentiality of this information. There was no problem in communicating it to Thomson, who had played a very prominent part in the British wartime atomic project, but Peierls suggested that if Thomson wished to work with collaborators at Imperial College then classified information arising from the American work might actually prove a handicap<sup>(19)</sup>. In this circumstance Thomson had decided that the best thing to do was to apply for a patent, thus placing on record his own thoughts and the fact that they were independent of any knowledge gained from his government work. This would then ensure that there could be no misunderstanding if he later received information from others<sup>(18)</sup>. Ironically, the very day that the patent agent was instructed, Peierls wrote again to Thomson, expressing strong reservations about his proposals<sup>(20)</sup>. He listed three particular objections, of which the second was the most fundamental. Thomson's proposal relied on a magnetic field along the torus to confine electrons, and a radial electric field to contain the deuterium fuel. Peierls pointed out that in such a "crossed field" configuration electrons would acquire a drift velocity perpendicular to both fields. This would constitute a poloidal current around the long axis of the toroidal solenoid which would neutralize the magnetic field near the centre; this would no longer confine the positive deuterons and thus the formation of the radial electric field would be inhibited. Indeed, with Thomson's proposed figures this neutralized region would extend very nearly to the wall, giving a gap smaller than the radius of curvature of the electron orbit in the magnetic field, which is clearly impossible. Peierls' first and third objections were concerned with secondary emission arising from deuteron bombardment of the walls, and the fact that an electron migrating to the wall requires a deuteron to do the same to restore the potential, resulting in a "clean up" of all the gas.

A week after Peierls' letter Thomson wrote again; he accepted the second and more fundamental objection, but was not convinced by the others. To overcome the problem he abandoned the idea of solenoidal containment, and decided instead to introduce a current round the axis of the torus, and use the magnetic field associated with this for confinement<sup>(21)</sup>. Electron drifts would now be in the toroidal direction, around the torus, and not cancel the field. It was proposed to produce the current by the radiation pressure associated with an electromagnetic wave travelling round the torus, emanating from suitably phased slots in waveguides. The precise mechanism for this pressure was not, however, described nor were any numerical estimates given. The electrons constituting this current would transfer energy to the deuterons, thus heating them to the required temperature.

Thomson met Peierls again early in May, and some of the points raised in the discussion are recorded in a letter from Peierls to Thomson on 15 May. First, Peierls had shown that even a single charged particle would not be contained in a toroidal solenoid; by now a well known result. Although anchored radially, particles moving round the torus drift vertically until they strike the walls. He conjectured, however, that a particle moving in the field of a filamentary current on the axis of the torus would be contained (provided of course that its energy were not too large)<sup>(22)</sup>. Peierls later gave this problem to a student, J W Gardner, who confirmed his conjecture, and published his analysis in 1949<sup>(23)</sup>. No reference was made in this paper to the reason for studying this particular problem. In the same letter Peierls raised the question of whether electron diffusion might be substantially higher than might be expected from simple theory, quoting the opinions of Mark Oliphant, and also H S W Massey, who during the war had worked in the same team as David Bohm on gas discharge problems in connection with the ion source for the Uranium isotope separator at Berkeley.

By this time, however, a provisional patent had already been lodged, with a secret classification, on 8 May<sup>(24)</sup>. This was based on the earlier document but modified to use confinement by the magnetic field of the current rather than an externally applied solenoid field. (Although this is essentially "pinch effect" confinement, the term does not seem to have been used by Thomson at this time). The provisional specification included various suggestions as to how the deuterium might be introduced, accelerated and removed, and noted the possible uses of the device as an energy producer and neutron or tritium source. The means of ionizing the gas were not specified and no single method of accelerating the electrons was emphasised. On the basis of some simple calculations it was suggested that with a torus of major and minor diameter of 3 meters and 60 cms respectively it should be possible to accelerate the electrons to energies of about 100 keV. At this energy, it was claimed, the pinch effect would be sufficient to contain the plasma for several minutes, long enough for the electrons to transfer their energy to the deuterons that would be carried round with them, and for thermonuclear reactions to then take place between the deuterons. The main problem foreseen was that a large part of the energy fed into the apparatus would be lost as bremsstrahlung radiation. But it was estimated that this loss could be overcome and that with an initial deuterium density of the order of  $10^{15}$  nuclei/cm<sup>3</sup> the system should be a net energy producer.

For some months after submitting the provisional specification Thomson was unable to pursue his proposals, for his role as adviser to the British delegation to the United Nations Atomic Energy Commission kept him in New York for most of the rest of 1946<sup>(25)</sup>. But his enthusiasm did not wane, and as a result of his urging a meeting was convened by John (later Sir John) Cockcroft at Harwell in January 1947 to discuss a possible programme of work on controlled fusion. Apart from Cockcroft himself, who, as director of the Atomic Energy Research Establishment at Harwell, would be responsible for any programme inaugurated, those present included Thomson and Blackman from Imperial College, Peierls, Moon and Sayers from Birmingham University, Tuck from the Clarendon Laboratory at Oxford, and Skinner, Frisch, Fuchs, French and Bretscher from Harwell<sup>(26)</sup>. At the meeting Thomson described his proposed device, including

the alternatives of "cyclotron action" and, less plausibly, radiation pressure to accelerate the electrons. Peierls responded by repeating his earlier criticisms, and suggested that there might be effects which would spoil the highly efficient containment of ions predicted by the simple theories used so far. As a first step in assessing the feasibility of Thomson's scheme, Peierls suggested that experiments on the pinch effect should be carried out by Moon in Birmingham, where work was also planned by Sayers on heavy spark discharges in deuterium. Meanwhile, Harwell were to keep in touch with developments of a new device called the "Wirbelrohr" that had been designed and built towards the end of the war by the German physicist M Steenbeck, and was to be investigated by the English Electric company. This was not a fusion device, but as a possible means of accelerating electrons in a low density toroidal gas discharge it was of obvious relevance to the subject.

The meeting helped to establish lines of communication between Thomson and the atomic energy project, and although Thomson's ideas were not exactly seized upon with vigour they were not dismissed out of hand. He was encouraged to continue with both theoretical and small scale experimental work, though the impression gained by Thomson and Blackman, that the Birmingham and Harwell theorists thought Thomson's idea a madcap one, may not have been far wide of the mark<sup>(27)</sup>. The theoretical study of particle confinement in the magnetic field of a current loop referred to earlier was initiated<sup>(23)</sup>, but the experimental work proposed for Birmingham does not appear to have been carried out there.

One particular outcome of the meeting was that Thomson became intrigued by the concept of the Wirbelrohr. This device had become known to the British in 1946 through the activities of the Control Commission in Germany. The mode of operation and historical background are related in a report by Wasserab<sup>(28)</sup>. It was proposed as a novel form of electron accelerator by Steenbeck, and had been built at the Siemens Schukert laboratories in Berlin; it is described in detail in a report by Steenbeck and Hoffmann<sup>(29)</sup>. A gas discharge was struck in a toroidal glass tube. This was achieved by metallizing the outside of the tube, with the exception of a small azimuthal "gap"; when the two sides of this gap were connected to a charged condenser, an oscillatory discharge was set up, which induced an oscillatory electric field around the axis of the torus. This caused breakdown of the gas, forming an oscillatory gas discharge. Steenbeck postulated that by having the gas pressure low enough, some of the electrons would "run away", forming a directed current round the torus. This can occur because for electrons of sufficient energy, the scattering cross-section decreases rapidly with energy, so that they are continuously accelerated rather than being thermalized by collisions. It was expected that these electrons would make many circuits of the tube, being confined to the axis by the self-magnetic field of the discharge current. No accelerated electrons were found, however, though these are a well-known (and unwanted) feature of more recent devices such as the Tokamak. Thomson decided to investigate the Wirbelrohr further at Imperial College. Two students, Alan Ware and Stanley Cousins, had just returned from military service in 1947 to start their Ph.D. research in his department and he immediately put Ware onto building a Wirbelrohr, and Cousins onto a related study of the diffusion of a plasma across magnetic fields<sup>(30)</sup>.

In the course of the next three years Ware built a Wirbelrohr of external diameter 25 cm and 3 cm bore, and he made extensive studies of the discharge with various gases over a wide pressure range and currents up to 13,000 amps. Voltage-current characteristics were measured, photographs were taken and spectroscopic measurements made through an inspection window. Spectroscopic evidence of a pinch, the first to be observed in a toroidal discharge, was found, but the pinch was not observed directly. The question of accelerated electrons had assumed secondary importance, but none was found<sup>(31)</sup>. Indeed, because of field perturbations near the feed point at the gap they were not to be expected. An ingenious feeding arrangement that removed with these distortions was incorporated in a second series of experiments with Cousins<sup>(32)</sup> using a slightly larger torus. Accelerated electrons were again not found, but by using a gauze covered window and a rotating mirror camera Ware and Cousins, working together, did succeed in 1949 in attaining currents of 27,000 amps in a 40 cm torus and in making the first ever recorded direct observations of the pinch effect in a toroidal gas discharge<sup>(32)</sup>. Meanwhile, the work at English Electric on using the Wirbelrohr as an accelerator had met with no success, and was discontinued<sup>(33)</sup>.

Returning to events in 1947, Thomson himself had continued to pursue his original idea. In April, four months after the Harwell meeting, he submitted the complete specification of his patent application, which made no mention of the Wirbelrohr mechanism<sup>(24)</sup>. Compared with the provisional specification the overall diameter of the proposed torus was increased to 4 metres, and specific proposals were made for electron acceleration and gas input and extraction, though not for the initial ionization of the gas. The electrons were to be accelerated as in his original conception, through the application of a radio-frequency current to pairs of slots a quarter wavelength apart in waveguides set into one sector of the torus. To keep the electron beam thus generated in a circular path the whole torus was to be placed in a vertical magnetic field, adjustable in line with the energy of the electron beam up to a maximum of 0.15 Tesla. The metal torus was to be built in sectors, and apart from the one incorporating the wave guides these were each to contain provision for the input and extraction of gas. Thomson also noted in the new specification the existence of the important secondary reaction between the deuterons and the tritons, or tritium nuclei, formed in the primary reactions between deuterons. He also included more detailed calculations of the operation of the device, suggesting that with a power input of 1,900 kW and deuterium consumption of  $7\frac{1}{2}$  grams a day it should generate currents of about half a million amps and produce a total of 9,000 kW output power, 1,900 kW of it in the form of neutrons. The neutron flux was estimated to be of the order of  $8 \cdot 10^{12}$  per  $\text{cm}^3/\text{sec}$ , a total of nearly  $2 \cdot 10^{19}$  neutrons/sec. This prompted him to suggest that the device could be used not only as a power source but also as a substantial source of neutrons, and in particular, if surrounded by  $\text{U}_{238}$ , the heavy isotope of uranium, as a plutonium producer. The diagrams from the complete specification of his patent are reproduced as Fig. 2.

Thomson's proposals contain many unjustified assumptions and assertions, backed up neither by calculation nor by careful enough consideration of the relevant physics. With many of his ideas tucked away in a secret patent there was no opportunity for external discussion or criticism. Nevertheless, allowing for the

primitive state of knowledge at the time, and the need to describe a plausible complete system for patent purposes it represents an interesting and creditable endeavour. Certainly Thomson appears to have had faith in it; on 29 May 1947 he wrote to Lord Portal, Controller of Atomic Energy, suggesting that theoretical work on the patent had now gone as far as was useful, and that if the idea were to be fully tested it would soon be necessary to start work on a larger scale than was possible in the Imperial College laboratory. Noting also that the question of security would arise, he suggested that the work might be placed at the brand new AEI laboratories for fundamental research at Aldermaston Court in Berkshire<sup>(34)</sup>.

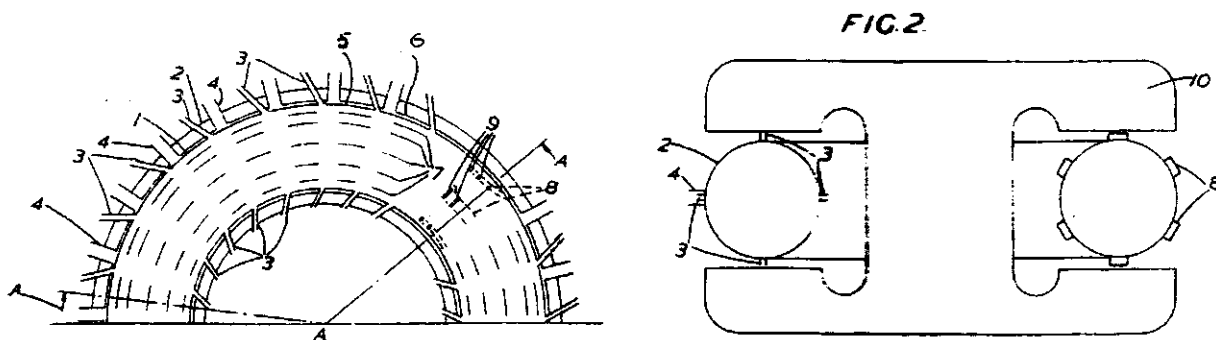


Fig. 2 Figures from the complete specification of Thomson and Blackman's original patent, ref. 24. The numbers refer to: 1. Toroidal vessel. 2. Cooling jacket. 3. Gas inlet ports. 4. Outlet ports. 5. Annular header. 6. Shell. 7. Slots to permit gas to be drawn from interior of vessel 1 into header 5, and thence through outlet ports 4. 8. Adaptors to be fed from high frequency generators and terminating in spaced slots as in vessel 1. 10. Electromagnet.

The question of security arose not because of any supposed similarity between a controlled fusion reactor and a fusion bomb, nor at this stage from any commercial considerations, but from the possible application of the device as a plutonium producer, and in particular from the specific mention of this in the patent specification. Thomson was unwilling to conduct classified work in his university department, and Harwell were indeed unwilling that he should do so. In this circumstance the Aldermaston Court laboratories were a natural choice. They had just been set up by the AEI group (British Thomson Houston and Metropolitan-Vickers) specifically for long term fundamental research for which there was no room at their Rugby and Trafford Park works, and they were opened the very month of Thomson's proposal. The first section to be set up was a nuclear physics section under D R Chick, and the director of the laboratory was T E Allibone, a friend and colleague of Thomson's and a close friend and erstwhile colleague of Cockcroft's. AEI were accustomed to conducting classified work, and the Metropolitan-Vickers side had close contacts with Harwell; Cockcroft himself and several of his senior colleagues had in the past been on the Metropolitan-Vickers research staff<sup>(35)</sup>.

Allibone had already been consulted about the possibilities of fusion devices by the Harwell head of theoretical physics, Klaus Fuchs, and he soon declared his support for the project<sup>(36)</sup>. AEI were in fact so keen to build up links with the atomic energy programme that they seem to have been willing to pay for Thomson's work themselves. But when Portal referred Thomson's request to Cockcroft, Cockcroft's response was that Harwell would have to keep full control over the work, that they would therefore have to bear the entire cost of it, and that

**further** discussions would be needed before they could do this<sup>(37)</sup>. After several **delays** these discussions, at which Cockcroft, Thomson and Allibone were joined **by** Fuchs and by H W B Skinner, head of general physics at Harwell, were finally **held** in early October 1947<sup>(38)</sup>. In the meantime further support for Thomson's **proposal** had come from one of the country's leading nuclear physicists, **Sir James Chadwick**<sup>(39)</sup>. But despite this very strong backing, Cockcroft and his **colleagues** decided that it would be premature to go ahead<sup>(40)</sup>.

**Although** it may not have seemed that way to Thomson, the Harwell physicists **were** not against controlled thermonuclear fusion research as such. In the course **of** the next few months, during which they came to know of other work being **done** in the field by Thonemann, then at the Clarendon Laboratory in Oxford, they **instigated** a small theoretical programme at Harwell. Oscar Buneman, who had **earlier** made important contributions to the theory of the magnetron oscillator, **was** encouraged to see whether similar techniques could be applied to the study of **instabilities** in a pinched gas discharge<sup>(41)</sup>. In the early part of 1948 regular contacts **were** established between Harwell, the Clarendon Laboratory and Imperial **College**<sup>(42)</sup>. Arrangements were made for the finance of the Clarendon work by **Harwell**, and W T Cowhig from the outstation at Malvern (staffed mainly by staff **originally** at TRE), was recruited on the Harwell payroll to work with Thonemann **in** Oxford<sup>(43)</sup>. Some small assistance, in the form of some condensers, was also **given** to Ware at Imperial College, and Buneman's effort was tied in with **Thomson's**<sup>(15)</sup>. Thomson's proposals for a large experiment seemed grossly **premature** though. Michael Perrin, Portal's second in command, later noted that **the** Imperial College work was never classified, in part because it was never **thought** that anything would come from it<sup>(44)</sup>. Even a more modest proposal of **Thomson's** put forward in February 1948, fell for the time being upon deaf ears.

Following the rejection of his proposal for large-scale work at Aldermaston Court, **and** following discussions with Skinner and Fuchs, Thomson himself reassessed the chances of his original proposal in the winter of 1947-1948. Thonemann recalls explaining his ideas to G P Thomson, who was visiting the Clarendon **Laboratory** during this period, without realizing who he was. He did not yet know of Thomson's work nor his secret patent. During this time Thomson set down his latest ideas in a note entitled "Atomic Energy from Deuterium", written **apparently** for Harwell<sup>(45)</sup>. Discussions had led to the realization that the method of acceleration originally proposed was not practicable, and the problems are set out in detail in this paper. As an alternative he was now thinking of "running the torus intermittently like a betatron", following the suggestion of Skinner<sup>(26)</sup>. He still foresaw problems, however, and did "not suggest for a moment that it would be possible to design a machine straight away that would work", nevertheless he was convinced that "the difficulties are not fundamental and can be overcome", but believed that further progress could only come from **experimental** work<sup>(45,46)</sup>.

About this time he also visited D W Fry, who was in charge of the electron accelerator development at Malvern. In a "Note on the Torus Project" he discussed the possibility of "cyclotron" acceleration using a configuration similar to that which he had seen at Malvern, but following an unsuccessful experiment by Hemmings in 1951 this rather impractical method of acceleration was

abandoned<sup>(47)</sup>. Ideas gained during his Malvern visit, together with Skinner's suggestion did, however, lead to the construction of a "plasma betatron". In conventional betatrons the current is limited by mutual repulsion of the electrons to a value of a few amps at most. Thomson's idea was to introduce gas, which would become ionized, and provide positive ions to neutralize the space-charge repulsion, thus removing the limitation to the current. Experiments at Malvern, however, showed that in a conventional betatron introduction of the gas would introduce scattering severe enough to disperse the beam before a high current could be accumulated<sup>(48)</sup>. To avoid this a much more rapid rate of beam injection and acceleration would be required. As detailed below an experiment along these lines, for an air-cored betatron with very rapid rate of rise of magnetic field was started at Imperial College in late 1948.

The notes by Thomson referred to above<sup>(45,46)</sup> are undated, but on 8 April 1948 Skinner presented to the Atomic Energy Technical Committee a document "Thermonuclear Reactions by Electrical Means" which "covers briefly the ground of discussions between Sir G Thomson, members of the Clarendon Laboratory and AERE staff during the last six months"<sup>(49)</sup>. This is a wide ranging and highly perceptive review. Skinner had a good appreciation of the slender basis for the whole concept, and pointed out clearly where the uncertainties lay, and what was required to resolve them. He was sceptical of Thomson's approach in which gas is introduced into a betatron or synchrotron in which a beam was already circulating, and preferred the idea of establishing a plasma with radio frequency fields and then accelerating the plasma electrons by betatron action. It is worthy of note that at this time, especially in the papers of Thomson, there is often confusion about the relative roles of directed and thermal velocities of the electrons in toroidal systems. (It was, indeed, to be several years before this was to be clarified). The question of plasma containment was recognized by Skinner as a central problem, and the possibility of destructive oscillations, being investigated by Buneman, is noted as an important area of study. Skinner comments that it would be "useless to do much further planning" before the resolution of this problem. Work at the Clarendon Laboratory (described in Chapter 3) was also considered, and Tuck's suggestion of a neutron source as an intermediate stage towards a power producing reactor noted.

It was beginning to look as if Thomson's ideas might be by-passed altogether and work continue only at Harwell and the Clarendon, but Thomson had still not assigned the rights to his original patent, and in the spring and summer of 1948 he used this as a bargaining point in negotiations with the Ministry of Supply. In February he was asked by the Ministry to fulfil his stated intention of assigning the rights, but Blackman had been taken ill with malaria in South Africa and had not yet returned from sick leave, so the question had to be put off<sup>(50)</sup>. It was raised again in May, and after some confusion had arisen over precisely what was required, a meeting was arranged for mid-July to sort everything out<sup>(51)</sup>. At this meeting Thomson complained about the Harwell refusal to support his proposals and about the general lack of development by them of his ideas, and he expressed his reluctance to assign any rights until something could be done about this. In response to this position it was finally agreed that Harwell should place a development contract with Imperial College for work on the air-cored betatron experiment, and that in return Thomson and Blackman should assign their

patent rights to the Ministry of Supply. Since the experiment would not be a neutron producer it was decided that the work could remain unclassified and so be conducted at Imperial College itself. It was eventually approved by the Technical Committee in November, and the patents were assigned by the end of the year<sup>(52)</sup>.

The work on the betatron, which was conducted by R Latham and M J Pentz, was never completed and never found its way into the mainstream literature on fusion. It was, however, one of the first experimental projects to be directed explicitly towards the production of fusion energy. Although not approved until later, it was effectively set in motion early in 1948 after Latham, then a demonstrator at the Cavendish Laboratory in Cambridge, had expressed a wish to move to London for personal reasons. Thomson was consulted, and an appointment at Imperial College was quickly arranged to commence in the summer<sup>(53)</sup>. Meanwhile Latham was introduced to Thomson's ideas and by the time Harwell support was promised he was ready to start work on a torus using betatron rather than synchrotron action (his own suggestion), but basically similar to that conceived of by Thomson<sup>(53,54)</sup>. Pentz then joined Imperial College as a research assistant at the end of the year, and was immediately assigned to the new project<sup>(55)</sup>. Over the next two years Latham and Pentz built an air-cored betatron of 30 cm diameter and 5 cm bore to which they applied a 50 cycle current to provide an alternating betatron field of 0.14 Tesla. On this was superposed a pulse of opposite polarity to provide the rapidly changing field rising from zero that was required. No ionization was produced at the design pressure, though a discharge could be produced at a pressure so high that no accelerated electrons could be expected. Attempts to accelerate electrons from a gun in the absence of plasma also failed. After getting further assistance from Harwell in the autumn of 1950 they began to make progress in diagnosing the problems. By the end of the year they were able to specify a redesign of the accelerating and containing coils which they thought should be successful<sup>(56)</sup>. This was probably the first attempt to build a "plasma betatron". In fact, there are many more problems to be overcome than was realized at the time. Forty years later, after many attempts, no satisfactory device of this type has been built. Shortly after this time the work at Imperial College was, as we shall see, wound up; and although Ware moved to Aldermaston Court to continue his investigations, the air-cored betatron project was dropped<sup>(54)</sup>.

## CHAPTER 3

### WORK AT OXFORD, AND OTHER DEVELOPMENTS TO 1950

We return now to 1946, to a contemporary initiative at the Clarendon Laboratory Oxford, following the arrival there of Peter Thonemann. Having completed a Master's degree at Sydney University (where the pinch effect was first identified in 1905<sup>(57)</sup>), Thonemann arrived in Oxford in October 1946 on an ICI research fellowship, proposing to carry out research on ideas for controlled fusion for his doctorate. He had not yet done any experimental work on the problem, but according to his later recollections he had thought out the theoretical possibilities in some detail over the previous few years. Thonemann's appointed supervisor, Douglas Roaf, was not apparently taken with the idea, and suggested that Thonemann should continue his previous research on ion sources. But the two topics were closely related, and he proceeded to work on both of them. In January 1947 he wrote to the director of the laboratory Lord Cherwell, requesting apparatus for an experiment directed towards fusion, and in a laboratory seminar of the same month at which Cherwell was present he set out the basic requirements of a power producing fusion reactor. A series of short notes written early in 1947, covering, amongst other topics, material at the seminar, constitute the first written record of Thonemann's ideas. These are listed in ref. 58. Rather little contemporary documentation exists of Thonemann's first years at the Clarendon, and the following description relies also on later recollections<sup>(7,59)</sup>. The first of the Clarendon notes, entitled "Atomic Energy Sources Using the Light Elements" and dated 13 January 1947, is a short statement of aims, together with parameters required to obtain a yield of  $10^{10}$  neutrons/cm<sup>3</sup>/sec. The text of this note is here reproduced in its entirety:

Attempts are being made to devise an apparatus dependant on the thermonuclear disintegration of the light elements as a power source. The main problem is to devise an "electromagnetic wall" which will take up the pressure of the high temperature gas ( $10^6 - 10^7$ °K). It is estimated that deuterium gas of density  $10^{16}$  ions/cm<sup>3</sup> at a temperature of  $10^6$ °K would provide a neutron flux of the order  $10^{10}$  neutrons/cm<sup>3</sup>/sec. The total pressure exerted by the gas amounts to about 2 atms.

Several schemes such as a heavy condensed spark in deuterium, the high current ring discharge and the electron space charge disintegrator have been considered. Although these methods can undoubtedly be made to give small neutron yields, their extension to a large power source does not appear practicable at present.

It is believed that further investigations on the interaction of strong electric and magnetic fields, particularly inhomogeneous magnetic fields, with highly ionised gases must be made before it is possible to say if the objective is attainable. Simple experiments to test out the theoretical predictions are already planned and will be under way by March 1947.

**This note** needs no further explanation, except perhaps for the "space charge disintegrator"; this hypothetical system consists of a spherical vessel into which

ion beams are injected radially. At the centre they are neutralized by a cloud of electrons, and reactions occur between the colliding ion beams. This idea was soon found to be impracticable. In the second note estimates of yield and bremsstrahlung radiation loss are presented, and the essential features of the confinement problem indicated. "Three main subjects" are identified for immediate investigation, (1) recalculation of the energy and neutron yield of light element reactions using the latest data, (2) experimental verification of the radiation loss formula and (3) investigation of the interaction of inhomogeneous electric and magnetic fields with a highly ionized gas. In a further paper dated March 1947 he describes three suggestions for containment in a high frequency ring discharge in a toroidal vessel, but only two, the "transformer method" (essentially the theta-pinch described in Chapter 10) and an RF driven azimuthal pinch in a steady solenoidal magnetic field are considered promising. Thonemann clearly recognized that the forces tending to contain the gas would not be continuous, for parts of the RF cycle they would disappear or reverse, and that for continuous containment it was necessary to rely on inertia (implying high confining frequency) or a subsidiary magnetic field. Plans for specific experiments are also discussed.

Thonemann began his investigations, as his means indeed dictated, with a series of modest experiments<sup>(7)</sup>. In the course of 1947 the Clarendon glass blowers, two brothers called Saxton, provided him with his first glass torus, and he began his research by analysing the way in which a discharge could be produced in a gas-filled torus by electromagnetic induction. The idea, which was already familiar, was to use the gas as the secondary winding of an alternating current transformer, the primary windings of which were placed outside and in the plane of the torus. This may, nevertheless, be the first time that this particular configuration was used. Thonemann's concern stemmed from a doubt as to the relative roles of the electromagnetic and electrostatic fields in the creation of the discharge; by using a 300 watt 5 MHz radio transmitter as a power source, and a Faraday screen to filter out the electrostatic field, he showed that this field was, in fact, necessary to initiate the discharge. Only once a conducting current channel had been established could the electromagnetic induction take over to perpetuate and increase the current through the gas. In these experiments the current was too low for pinching to be expected.

The following year Thonemann constructed a 7 kW 100 kHz oscillator to replace the radio transmitter, and acquired a direct current generator with which to produce magnetic fields. He then set out to confirm experimentally several aspects of the theoretically predicted motion of charged particles in inhomogeneous magnetic fields. At the same time he also confirmed that the conductivity of the current channel in an externally applied longitudinal magnetic field increased, as predicted by theory. Using mercury vapour and gas discharges in straight tubes he measured the diamagnetic susceptibility of a plasma column. This was done by measuring the transient voltage induced in a solenoid around a straight tube when a discharge was struck within it. And in 1949, using a pyrex torus to which a magnetic field was applied along the axis of the tube through coils of water cooled copper tubing, he demonstrated experimentally an important consequence of this susceptibility. The discharge in the torus was maintained by transformer action using Ferroxcube iron cores. The ring current was about 10 amperes. As the toroidal magnetic field was increased in intensity

from zero the discharge first increased in intensity and then moved towards the outer wall of the torus. A further increase in the strength of the applied magnetic field caused the discharge to extinguish, much to the surprise of the onlookers. The diamagnetic susceptibility of the plasma had been demonstrated in a dramatic fashion. The torus used in this experiment is shown in Fig. 3A. No further attempts were made to confine the plasma by an externally generated magnetic field on its own.

Before proceeding to the next stage of his work, Thonemann had acquired two assistants; these were W T Cowhig, technically on secondment from Harwell (though he never worked there), and Philip Davenport, a fellow research student at the Clarendon. There now remained two outstanding questions to be answered before Thonemann would seriously consider the construction of a fusion device. The first one concerned the current and gas pressure necessary for deuterons in a plasma to reach temperatures at which thermonuclear fusion reactions might take place. Both Thomson and the Los Alamos group had made rough estimates of these, and in retrospect they were very accurate, considering the paucity of information. But they had little experimental basis. Thonemann and Cowhig therefore calculated the "Rate of Thermal Disintegration of Deuterium"<sup>(60)</sup> using published cross section measurements. They then reworked the theory of the pinch effect for low gas pressures, and confirmed their theoretical prediction within an accessible range by experiments on a high current mercury vapour arc discharge in a straight tube<sup>(61)</sup>. On the basis of this theory, it was predicted that provided kinetic equilibrium between electrons and deuterons could be achieved, and provided of course that no instabilities arose, thermonuclear fusion should be detectable at currents of 200 kiloamps (Thomson's patent proposal aimed at 500), and initial gas pressures of about  $10^{-3}$  torr (i.e., within the familiar range  $10^{14} - 10^{15}$  nuclei/cm<sup>3</sup>). It is interesting that Thonemann and Cowhig's pinch theory turned out to be essentially the same as that of Bennett, derived in 1934 in a somewhat different context<sup>(62)</sup>. Bennett's theory, unknown to Thonemann and Cowhig until their work was completed, applied to a neutralized particle beam, in which the directed velocity of the electrons or ions greatly exceeded the transverse velocity. No such condition, however, was assumed by Thonemann and Cowhig. Meanwhile Blackman at Imperial College had also produced a theory, which was published at about the same time. These theories were essentially in agreement, and differed from Tonks' calculation in 1939<sup>(63)</sup> in that they predicted at what current a constricted discharge could be expected.

The second outstanding problem that could be treated without progressing to large scale work was how to contain the tendency of the toroidal current channel to expand outwards from the centre of the torus under the influence of both the applied and the self-magnetic fields. The solution proposed was to use a copper torus, containing four uniformly spaced conductors near the walls carrying a current oppositely directed to the plasma current. The plasma and coil currents would be mutually repelling, stabilizing the plasma current channel, and keeping it away from the walls of the torus. (The effect of image currents in the conducting wall of the torus in producing a similar effect was not yet appreciated). In order to test this idea, a copper torus was built by the Harwell workshops. This was constructed in two halves, which were separated by short glass cylinders for observation, and argon and helium discharges were initiated by the application of

a spark coil to the connecting tube to the vacuum pump. The 7 KW oscillator then provided the primary current to promote the discharge by induction. The result was that secondary currents of up to 2,000 amps were produced in argon at a pressure  $10^{-3}$  torr. Through the glass windows a clearly defined bright current channel could be seen, apparently perfectly stable, and in the centre of the tube<sup>(7)</sup>. This apparatus is shown in Fig. 3B.

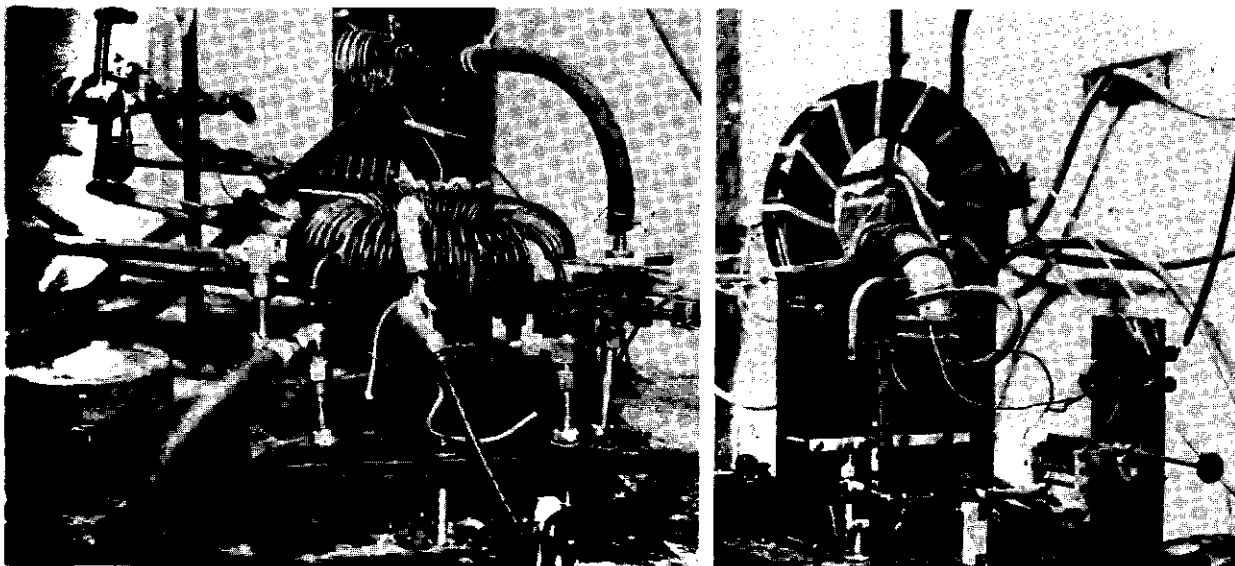


Fig. 3 Two of Thonemann's tori at the Clarendon Laboratory. (A) Pyrex torus. Water cooled coils to produce the toroidal field may be seen, together with RF coupling coils (at top), and Rogowski loop for current measurement (around torus). Below the torus on the platform is part of the ferroxcube core to provide linkage between transformer primary and discharge secondary, (most of the core has been removed for clarity). (B) Copper torus. The primary winding can be seen round the rho metal continuous strip transformer core. Water cooling is provided for the four concentric stabilizing coils inside the torus, and also externally on the torus itself. This torus is now on display at the Museum of The History of Science at Oxford.

The copper torus was completed and working by the summer of 1949, and although the work on the pinch effect was substantially completed soon after, refinement of both theory and experiment continued throughout 1950.

In 1949 a new arrangement for providing a steady pinch current was suggested and built. A solenoidal winding round the torus was fed with a high frequency current in such a way that a travelling wave was propagated round the torus with a phase velocity of about  $2 \times 10^7$  cm/sec. This could be arranged with a multiphase RF system, or by loading the solenoidal winding with condensers, and feeding power in one end and providing a matched terminating resistor at the other. This arrangement, which would now be called "current drive", produces a d.c. component of current round the torus. The mechanism is well described in a paper describing an experiment on such a system<sup>(64)</sup>: "The origin of the force driving electrons around the torus is readily understandable from a macroscopic point of view. In cylindrical co-ordinates, the changing z-component of the magnetic field induces electron currents circulating in the  $\phi$ -direction. These currents interact with the radial component of the magnetic field and therefore experience a force in the direction of wave propagation. If the electron currents

are in phase with the electric field, the force is always in the direction of propagation. If they are  $\pi/2$  out of phase with  $E_\phi$  the net force over a cycle vanishes. .... It is interesting to note that radiofrequency power can be converted into direct current power without the use of a commutator or a non-linear element". Direct currents of order 100 amps were observed in mercury at a frequency of 1.36 MHz, supplied by a generator capable of delivering a few kW.

Although the concept is an interesting one, the current is too small to form the basis of a fusion system. Nevertheless, it was incorporated, together with ideas from the copper torus referred to above, in a patent for "Gas Discharge Apparatus" capable of producing thermonuclear neutrons applied for in August 1950<sup>(65)</sup>. The scheme described in this patent is hardly realistic, and the motive for filing it was to stake some sort of claim for Harwell in this field. During this time also Thonemann and his colleagues continued to use the straight tube mercury discharge to obtain more accurate measurements than were possible with a torus, to explore wall effects, and to study the technical problems surrounding the setting up of a current channel. Then, in November 1950, the project began to move gradually over to Harwell.

Harwell interest in the Clarendon work appears to date from December 1947, when Cockcroft asked Cherwell's permission to speak to Thonemann<sup>(66)</sup>. A series of meetings followed<sup>(49,67)</sup>, and in February 1948 Harwell took on financial responsibility for the apparatus for Thonemann's experiments<sup>(43)</sup>. Eighteen months later, in the autumn of 1949, the Harwell involvement increased. When Thonemann had succeeded in demonstrating a visually stable pinched discharge in argon his copper torus, Cherwell and Cockcroft were invited to see it, and they seem to have been strongly impressed<sup>(59)</sup>. On 1 October 1949, when Thonemann's research grant ran out, he and Davenport, though still working at the Clarendon, were taken onto the Harwell payroll. Cockcroft and Cherwell began to pay regular visits to the laboratory on Saturday mornings to see how the work was progressing<sup>(59)</sup>.

During the winter of 1949-1950 the question of security came to the fore in a dramatic way with the investigation and subsequent arrest for spying of Klaus Fuchs, who had been fully aware of the fusion programme to date<sup>(68)</sup>. The grounds for classifying the work, strongly urged by Cockcroft, were that fusion might provide a copious neutron source capable of breeding plutonium from  $U_{238}$ . (Classified work with the aim of using neutrons from targets bombarded by accelerated deuteron beams was already underway in the United States at this time<sup>(69)</sup>.) Thonemann and his colleagues found themselves being closely questioned about the possible implications of their work, and although Perrin could write on 10 May 1950 that this work was non-secret, reports on the rate of thermal disintegration of deuterium by Cowhig and Thonemann and experiments on the pinch effect in straight tube mercury discharges written by Thonemann and Cowhig a week or two later were promptly classified<sup>(60,61)</sup>. Thonemann himself objected to the work being classified on any grounds other than commercial ones, and later that summer the second of these reports was declassified and subsequently published. Papers by Latham, Pentz and Blackman on the betatron design, by Buneman on a toroidal magnetron, by Blackman on the theory of the pinch effect, by Ware on the Wirbelrohr, and by Cousins and Ware

on their pinch effect observations were also published between 1949 and 1951(31,32,70-73). But as a general principle it was decided not to release for publication anything that gave any open indication that there was an active programme aimed at the design of a thermonuclear reactor. In particular, all work on high temperature discharges in toroidal tubes was to be treated as classified<sup>(74)</sup>. In November 1950 a special meeting of the Publication and Declassification Sub-Committee (PDSC) was convened at Harwell in order to establish and formalize rules for the future classification of thermonuclear fusion research<sup>(75)</sup>.

To keep secret the fact that Harwell were interested in the possibilities of controlled fusion would already have been impossible. Since the work at Imperial College and the Clarendon had not originally been classified it had been openly discussed, and Thonemann in particular had described his work and his aims freely<sup>(59)</sup>. He recalls discussing the subject without restraint, and in particular giving a lecture at Ernest Lawrence's request, during his visit to the United States in 1951<sup>(76)</sup>. The Americans knew of the possibilities as well as the British; since Tuck had recently returned to Los Alamos, and Teller had talked at length to Thonemann in 1949 they must also have known of the British effort. [J L (Jim) Tuck, who as part of the British team at Los Alamos during the war had worked on the design of the explosive lens for the bomb, had returned from Oxford to the USA in 1949, and to Los Alamos in 1950. He initiated the thermonuclear research programme there in 1952, starting with an inductively driven discharge in a toroidal glass tube. This apparatus was known as the "Perhapsatron"<sup>(77,78)</sup>]. There could be little doubt either that the Russians knew of the British work through Fuchs. In this respect the attempt at classification seems in retrospect rather strange. But given the near-hysterical attitude to security that was the natural consequence of the Fuchs case it was inevitable that some sort of classification should be imposed, and if the work itself could not be kept entirely secret it was reasonable to suppose that its extent could. Since Cockcroft and his Harwell colleagues were now coming to accept the need for a massive increase in the programme, this last point was important. It had become increasingly clear during the year that what could be done on the small laboratory scale had been done, and that if the idea were to be pursued it would have to be outside the universities and on a larger scale than they could manage. This change of scale and location and the imposition of classification went naturally hand in hand.

As a basis for the PDSC meeting a note summarising what had been achieved to date and recommending how future work should be treated was prepared by Thonemann and D W Fry, who had succeeded Skinner as head of the General Physics division at Harwell, and to whom Thonemann was responsible. The achievements to date were thought to be promising, and it had already been agreed that Thonemann's team would begin moving to Harwell at the end of the year in order to conduct experiments with much higher currents than had been possible at the Clarendon. Regarding classification the paper concluded that "it would be wise for the new work now starting to be graded Secret at least until the stage has been reached where a high mean power reactor is no longer considered to be practical. If this isn't done, a stage may easily be reached in the development where the fundamentals of the scheme are established and sufficiently widely known for others to take advantage of them"<sup>(79)</sup>. This seems to have represented

Fry's view accepted only reluctantly by Thonemann, and at the meeting itself a similar balance of opinion was manifested. Cockcroft, claiming that research had been kept unclassified as long as was possible, suggested that with the increased effort now to be devoted to it "we ought to go rather carefully on publication in case it turned out that the new work resulted in the production of neutrons". Thomson accepted this, but only "with some reluctance", since the work was not directed at the production of a weapon as such. Peierls, sharing this reluctance, stressed that the proposed move was setting a precedent in declassification policy<sup>(75)</sup>. The point was also made that the Americans knew all about the work anyway, but Skinner and the Department of Atomic Energy (Ministry of Supply) representatives backed up the hard line and Cockcroft concluded that there was "general agreement that high power work", which he distinguished from the low power work that had been done at the universities, "should be classified until we knew where it was going"<sup>(75,80)</sup>. The meeting as a whole concluded that:

The general objective should be to keep secret the likelihood that the gas discharge may lead to a method of obtaining a thermonuclear reaction and that we are trying to realize this in the atomic energy project.

The immediate impact of the new policy was on the Imperial College work. Thomson agreed at the meeting to withdraw a substantial paper he had already submitted for publication on "Thermonuclear Reactions"<sup>(81)</sup>. (Much of the material in this paper was, however, published later in a Patent specification filed in 1952 but not published until 1959<sup>(82)</sup>. The theory was too simple to describe the complex phenomena which actually occur, and it contributed little if anything to the development of the field). Unwilling to do classified work at Imperial College Thomson also raised again the possibility of shifting the work there to AEI, and this time both Allibone and Cockcroft agreed<sup>(83,84)</sup>. Ware had been joined at Imperial College by R F Hemmings and they began to make arrangements for the move early in 1951, finally moving to Aldermaston Court in August 1951<sup>(35)</sup>. Pentz and Latham were unhappy about participating in classified work and did not wish to move. Both transferred to other work, and the betatron project came to an end.

By the time this move took place, Ware and Hemmings had built a new torus of quartz and had achieved peak currents of 72,000 amps, albeit at very high gas pressures and with accompanying vaporization of the quartz<sup>(85)</sup>. Thomson and Blackman had continued their theoretical researches<sup>(86)</sup>. At Harwell Thonemann and his colleagues had been investigating a number of topics in gas discharge physics. In addition to work on toroidal systems already described, further study of mercury arcs in the straight tubes used for the pinch experiments yielded information on the interaction between a low pressure gas and the wall of a discharge tube, and the "outgassing" procedures necessary before a high temperature discharge could be set up<sup>(7)</sup>. One conclusion of this latter work was that quartz, or indeed any other chemically compound material, was unlikely to be suitable for the containing vessel under the extreme conditions required for a thermonuclear reactor. Bombardment and radiation would cause dissociation, and impurity atoms would enter and adversely affect the discharge. The facilities available at Harwell and Aldermaston Court placed experimental fusion research on the brink of a new and less tentative phase. Elsewhere recent work on the

theoretical physics of plasmas, most notably that of Bohm and Gross, and of Hannes Alfvén in his classic text *Cosmical Electrodynamics*, prepared the way for the establishment of plasma physics as a discipline in its own right<sup>(87,88)</sup>.

In addition to the work at Harwell and Imperial College some studies on the possibility of thermonuclear neutron production had been made at Liverpool. Indeed, this was probably the earliest serious discussion of the subject in Britain. British scientists who worked on the bomb project at Los Alamos may well have considered the possibility of controlled fusion towards the end of the war, at the time of early speculative discussions on the possibility of a fusion bomb (the "Super")<sup>(10)</sup>. Several of them attended the lecture course on thermonuclear reactions and plasma physics given by Fermi in 1945. Some notes on this course material were sent by Philip Moon of Birmingham University to Sir James Chadwick at Liverpool, together with a note dated 11 October 1945 entitled "On the possibility of igniting deuterium by an electric discharge"<sup>(89)</sup>. In this note, the substance of which was reported by Moon at the Harwell meeting in February 1947, some brief calculations were presented which gave some indication of the conditions required to obtain fusion in a discharge in deuterium at atmospheric pressure<sup>(26)</sup>. Typical figures suggested were a discharge of length 10 cm and radius 1 cm, carrying 3 million amps, to give sufficient magnetic field to contain  $\alpha$  - particles. It was recognized that the discharge would have to be built up very rapidly to avoid excessive radiation loss during heating (of order  $10^{11}$  watts) and a total energy of about  $2 \times 10^7$  joules would be required to reach ignition temperature. As a result of this note Chadwick then arranged that additional money should be made available to support J M Meek's work on heavy current discharges at the Metropolitan-Vickers Research Laboratory at Trafford Park without letting him know why<sup>(90)</sup>. It appears, however, that Meek, together with J D Craggs, later both at Liverpool University, had independently considered the possibility though their calculations indicated that it was unlikely that neutrons would be observed<sup>(91)</sup>. The idea had been suggested to them by Professor Kendall of Edinburgh University as early as 1943. They did not expect to be able to detect thermonuclear neutrons, but nevertheless felt that it was worth while making a search. In 1949 Reynolds and Craggs, using a high current generator built by Durnford and Reynolds<sup>(92)</sup>, passed 100 sparks each of 300 kA through deuterium at atmospheric pressure. A few neutrons were found, but these were later found to be background, since the same number were observed also in 100 sparks in hydrogen<sup>(93)</sup>. After this experiment there was no further work specifically directed towards fusion.

## CHAPTER 4

### TRANSFORMER DRIVEN TORI AT HARWELL AND AEI

The pattern of research established at the beginning of the decade continued substantially unchanged for several years. There continued to be a small programme at Liverpool University studying the pinch effect in high current spark channels, which was financed by Harwell, but there was no great expectation of detecting thermonuclear neutrons, especially after the experiments of Reynolds and Craggs<sup>(93)</sup>. J E Allen, who had already contributed to the theory of the pinch effect<sup>(94)</sup>, moved from Liverpool to Harwell in 1952, and Reynolds, who had moved there earlier to work in another field, re-joined the fusion programme soon afterwards. Under Thonemann's general direction they studied fast pinches in toroidal tubes, using their Liverpool experience on pulsed circuits to broaden the "Wirbelrohr" approach of Ware at Imperial College, and improved high-speed photography led to a shock-wave theory of the transient pinched discharge<sup>(95)</sup>. The AEI programme at Aldermaston Court flourished under the guidance of T E Allibone and Sir George Thomson, but this too was kept on a Harwell contract and subsidiary to the dominant and growing Harwell programme.

Once Thonemann's group had moved to Harwell, the commitment of the establishment to a controlled thermonuclear fusion research programme increased markedly, and this could be seen on the theoretical as well as on the experimental side. Buneman soon left the atomic energy project altogether, but his place in the fusion programme was taken by W B (Bill) Thompson, a Canadian who had joined the Harwell Theoretical Physics Division soon after completing his doctorate in 1950. Thompson devoted the bulk of his time at Harwell to theoretical plasma physics, and quickly became a leading authority on the subject. His earliest work was aimed at providing a model of an equilibrium constricted discharge in a straight tube. He wrote a series of reports studying various aspects of such a discharge in various gases, some of these in conjunction with existing members of the theory group<sup>(96)</sup>. While not participating in the fusion project as such, several other senior theoretical physicists both at Harwell and elsewhere also kept in close touch and offered help and advice. At Harwell Heinz London, who had earlier made substantial contributions to the field of superconductivity, was able to provide useful insights, and Brian Flowers began to take a close interest. H S W Massey, Professor of Physics at University College London was enlisted to help with the special problems of atomic physics, important in the understanding of collision and radiation processes in the plasma<sup>(97)</sup>. There was also interaction with the AEI team, both through personal contacts and through the joint Gas Discharge Committee, and especially with Sir George Thomson who continued his work first at Imperial College London and then from the autumn of 1952, as Master of Corpus Christi College Cambridge.

Both at Harwell and at AEI the theoretical and experimental parts of the fusion programme were kept in close contact. At AEI in particular, Alan Ware was equally involved on either side. At Harwell, however, the two parts were administratively separate, the one coming under the Theoretical Physics division, at first without a division head in the wake of Fuchs's departure from the scene and then under Flowers, and the other under Fry's General Physics division. In practice, moreover, the conditions prevailing in a real plasma were so complex







































































































































