FUSION RESEARCH IN THE UK 1945 - 1960

J Hendry and J D Lawson

AEA TECHNOLOGY

AHO 1

FUSION RESEARCH IN THE UK 1945 - 1960

J Hendry and J D Lawson

January 1993

TABLE OF CONTENTS

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 Tokomak 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**[1]. 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

AEA	Atomic Energy Authority.
AEC	Atomic Energy Commission (USA).
AEI	Associated Electrical Industries (Laboratory at Aldermaston).
AERE	Atomic Energy Research Establishment (Harwell).
AEX	Atomic Energy Executive.
AWRE	Atomic Weapons Research Establishment (Aldermaston), later AWE.
CAC	Churchill Archive Centre.
CERN	European Council for Nuclear Research (Geneva).
CTR	Controlled Thermonuclear Reactions.
CTRAC	CTR Advisory Committee.
Geneva Conference	See Notes on the References p. 89.
GPT	G P Thomson Archive.
HC	Harwell Council.
HMSO	Her Majesties Stationery Office (London).
HSC	Harwell Steering Committee.
ICSE	Intermediate Current Stability Experiment.
IEE Convention	See Notes on the References p. 89.
PDSC	Publication and Declassification Sub Committee.
PRO	Public Record Office.
RGMB	Research Group Management Board.
TTPC	Thermonuclear Technical Policy Committee.
TRE	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[1]. 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[2]. 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[3]. 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[4]. 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[5]. 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[6]. 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[7,8].

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[9]. 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[10]. In his paper to the 1958 Geneva Conference, Teller recalled that[10]:

> 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^3 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(10-13). (Suggestions for such an approach to fusion were apparently made in a paper by Tuck and Ulam in 1944)(14). 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[15]. 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[16]. (Toroidal geometry and the associated notation are illustrated in Fig. 1).

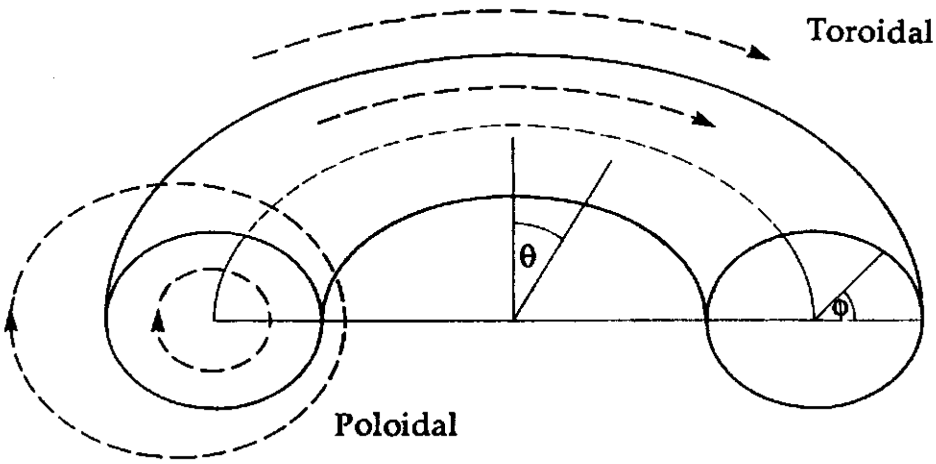

Fig. 1 Notation for toroidal geometry. Field and current components are described as "toroidal" if they are directed around the axis of symmetry, and "poloidal" if in a plane through the axis. Angles θ and φ are often used respectively for directions around the symmetry axis, and in the poloidal direction 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 φ is replaced by θ for the poloidal angle.

He proposed to ionize the gas using an external source ánd 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^3, 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[16]. 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"[17]. 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[18].

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[19]. 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[18]. Ironically, the very day that the patent agent was instructed, Peierls wrote again to Thomson, expressing strong reservations about his proposals[20]. 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[21]. 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)(22). Peierls later gave this problem to a student, J W Gardner, who confirmed his conjecture, and published his analysis in 1949(23). 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(24). 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^3 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(25). 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(26). 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(27). The theoretical study of particle confinement in the magnetic field of a current loop referred to earlier was initiated(23), 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(28). 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(29). 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(30).

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(31). 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(32) 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(32). Meanwhile, the work at English Electric on using the Wirbelrohr as an accelerator had met with no success, and was discontinued(33).

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(24). 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 8.10^{12} per cm^3/sec, a total of nearly 2.10^{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 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[34].

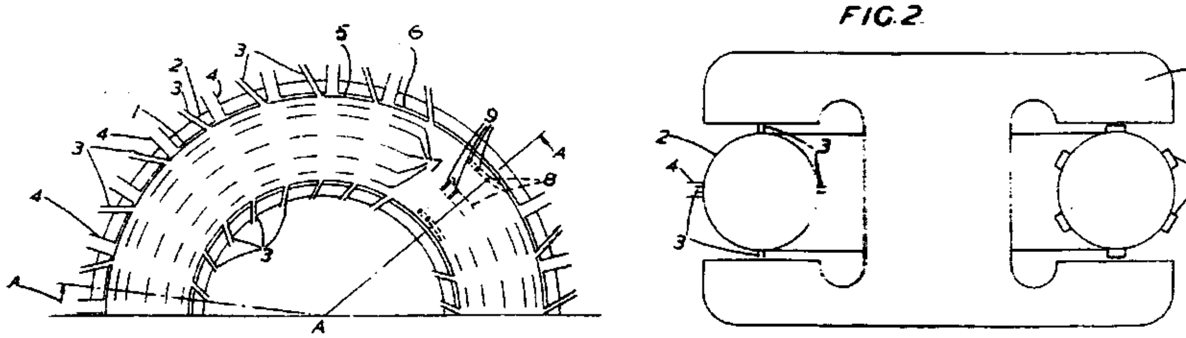

Fig. 2 Figures from the complete specification of Thomson and Blackman's original patent, ref. 24. The numbers refer to: 1. Toroidal vessel. 2. Cooling jacket. 3. Gas inlet ports. 4. Outlet ports. 5. Annular header. 6. Shell. 7. Slots to permit gas to be drawn from interior of vessel 1 into header 5, 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[35].

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[36]. 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[37]. 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[38]. In the meantime further support for Thomson's proposal had come from one of the country's leading nuclear physicists, Sir James Chadwick[39]. But despite this very strong backing, Cockcroft and his colleagues decided that it would be premature to go ahead[40].

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[41]. In the early part of 1948 regular contacts were established between Harwell, the Clarendon Laboratory and Imperial College[42]. 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[43]. 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[15]. 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[44]. 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[45]. 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[26]. 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[45,46].

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[47]. 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[48]. 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[45,46] 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"[49]. 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[50]. 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[51]. 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(52).

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(53). 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(53,54). Pentz then joined Imperial College as a research assistant at the end of the year, and was immediately assigned to the new project(55). 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(56). 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(54).

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[57]), 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[7,59]. 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^3/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^3 at a temperature of 10^6°K would provide a neutron flux of the order 10^{10} neutrons/cm^3/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[7]. 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"(60) 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(61). 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^3). 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(62). 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(63) 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(7). This apparatus is shown in Fig. 3B.

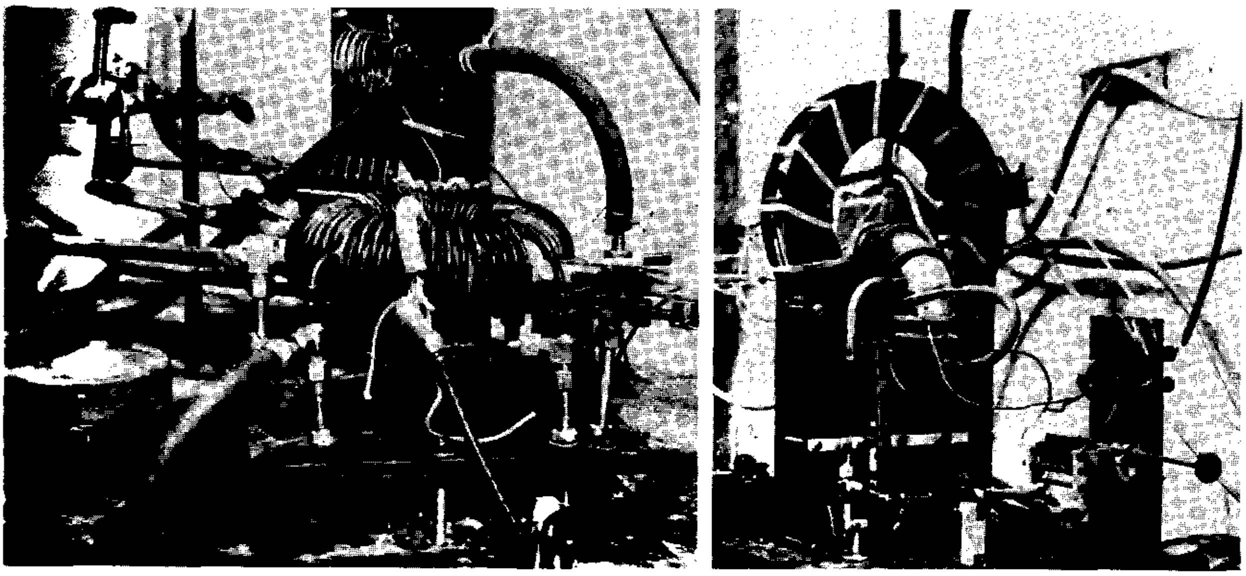

Fig. 3 Two of Thonemann's tori at the Clarendon Laboratory. (A) Pyrex torus. Water cooled coils to produce the toroidal field may be seen, together with RF coupling coils (at top), and Rogowski loop for current measurement (around torus). Below the torus on the platform is part of the ferroxcube core 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 2×10^7 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(64): "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 ϕ-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_ϕ 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[(65)]. 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[(66)]. A series of meetings followed[(49,67)], and in February 1948 Harwell took on financial responsibility for the apparatus for Thonemann's experiments[(43)]. 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[(59)]. 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[(59)].

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[(68)]. 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[(69)].) 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[(60,61)]. 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(74). 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(75).

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(59). 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(76). 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"(77,78)]. 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"(79). 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(75). 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"(75,80). 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"(81). (Much of the material in this paper was, however, published later in a Patent specification filed in 1952 but not published until 1959(82). 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(83,84). 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(35). 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(85). Thomson and Blackman had continued their theoretical researches(86). 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(7). 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[87,88].

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")[10]. 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"[89]. 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[26]. 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 α - 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 2×10^7 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[90]. 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[91]. 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[92], 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[93]. 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[93]. J E Allen, who had already contributed to the theory of the pinch effect[94], 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[95]. 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[96]. 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[97]. 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 and so little understood that the theoreticians could make little contribution at this stage. For the first half of the decade, it was the experimental side of the programme that dominated the scene.

The move from the Clarendon Laboratory to Harwell was gradual, with Thonemann moving first and Davenport joining him later. By 1952, however, Thonemann and Davenport had established themselves at Harwell and had been joined there by R (Bob) Carruthers who originally came, like many of the General Physics division, from the wartime Telecommunications Research Establishment (TRE) at Malvern. A number of TRE staff had transferred to the Harwell payroll shortly after the end of the war, but some had remained at Malvern until 1951. In particular, the small accelerator group under D W Fry had built a linear accelerator and a small synchrotron there. This group had already had some limited interaction with the fusion programme through Thomson, as described previously. For their first major experiment at Harwell the team, drawing now on Carruthers's electrical engineering experience, constructed a new 100 kW, 100 kHz power supply, making use of two spare 45kV, 5A power supplies and associated water cooling plant from the electromagnetic separator and a spare valve from the cyclotron which was located in the same wartime hangar. This was used first to power a copper torus, similar to but larger than the one used at the Clarendon Lab. Subsequently, the experimental programme was extended by using a water cooled quartz torus, on which measurements of a hydrogen discharge were made. This arrangement allowed visual observations and measurements more readily to be made. By introducing a small piece of copper sheeting close to the torus tube Carruthers and Thonemann were able to confirm and to measure the repulsion between the current channel and a metal reflector that had been the basis of Thonemann's earlier success with a metal torus. Several configurations for transferring power inductively to the discharge were tried, and screens of various designs were inserted to try to keep the discharge away from the wall[7,98].

Despite further progress, the use of a radio-frequency power supply was running into problems. Improvements resulting from the large new power supply had at first been rapid, but progress had become difficult as the power was increased. Excessive heating and interaction between plasma and the quartz wall of the torus became serious problems. Further, it was recognized that higher frequencies would be needed to prevent the plasma ions drifting to the wall at the instant when the current reversed and the pinch force was momentarily zero. Indeed, construction of a power supply for an experiment at 5 MHz (for which the quartz torus was originally intended) was started. All these problems were bypassed, however, by a radically new proposal made by Carruthers in 1953[98,99]. Drawing on his war-time experience at TRE in the development of pulsed power supplies for use in radar, he now suggested that the problem of ion drift to the walls could be avoided by using a pulsed transformer with iron core, fed by a switched capacitor bank, to give a unidirectional electric field rather than an alternating one. Borrowing a transformer core from the nearby high power klystron experiment of J B Adams and M G N Hine, Carruthers together with Davenport built a small capacitor bank and tested the idea on a new torus made up from a pair of glass U-bends of 10 cm bore and about 30 cm mean diameter. With this far from sophisticated apparatus he was able to produce unidirectional constricted discharges lasting up to 100 microseconds, and so superior were these discharges to those produced using alternating radio-frequency currents that it was decided in January 1954 to drop the latter altogether, and to work only with pulsed supplies(100). In the course of this work, moreover, Carruthers and Davenport were also able to observe the development of hydromagnetic instabilities in a toroidal plasma for the first time. Using both straight mercury vapour discharges and toroidal discharges in argon and xenon they obtained clear photographs showing the development of the so-called wriggle instability, or "kink instability" as it was later termed(100,101). The instability arises from the fact that should a small lateral displacement of part of the discharge occur, giving a small kink the self-magnetic field on the inside of the kink increases, and that on the outside decreases. This tends further to accentuate the kink. This, together with one of the original photographs is illustrated in Fig. 4. In order to distinguish the form of

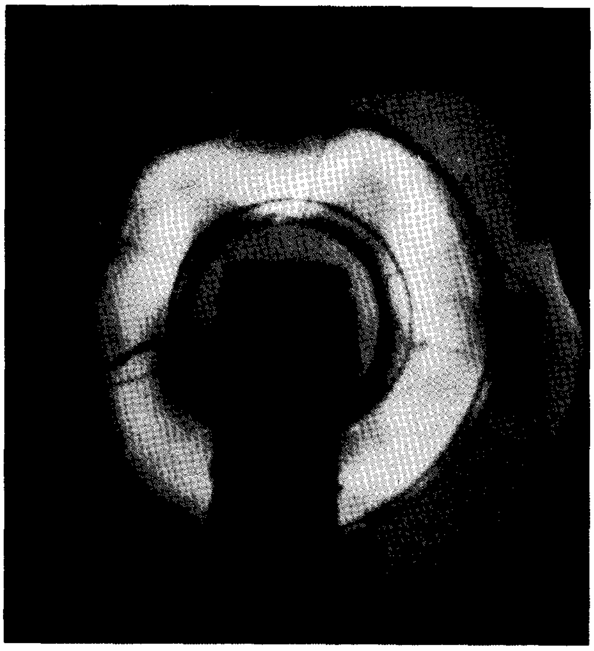
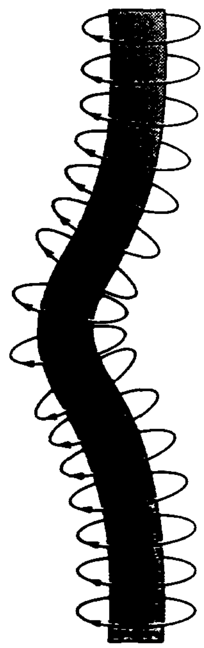

Fig. 4 Wriggling discharge in Xenon, in a glass torus of bore 10 cm and mean radius 30 cm, (from ref. 101). The gas pressure was approximately 10^{-3} torr, and the plasma current 1300 amps. The sketch illustrates how the instability arises. Once a kink is formed the magnetic forces acting on the current become stronger on the concave and larger on the convex sides, and this increases the size of the kink.

the rapidly fluctuating perturbation it was necessary to use an electronically gated image converter with gate-width of 8 μs. This had not been done in earlier experiments, so that wriggling may have been present but not observed. At the time that these observations were being made the theory to explain them was just being formulated. This was led from Princeton University in the USA, where the first branch of the American controlled fusion programme had been established under Lyman Spitzer in 1951 as "Project Matterhorn". Spitzer persuaded two of his Princeton colleagues, Martin Schwarzchild and Martin Kruskal, to collaborate on a theoretical study of plasma instabilities. In 1954 they published a classic analysis of the subject, demonstrating that a straight pinched plasma was unstable with respect to lateral distortions and laying the foundations for future work(102,103). The first work at Harwell on this type of instability was reported in November 1955, using a "simplified model - a thin extensible wire" in a curved channel(104). The Harwell theoreticians, notably W B Thompson, and R J Tayler, were also at the forefront of developments; their work will be described further in later chapters.

The successful use of the pulse transformer constituted a major step on the path to a large scale controlled fusion experiment. It had already been clear from Thonemann's earlier work and from the related experiments at Harwell that this would most probably use a metal torus, and that the current channel would receive most of its energy by acting as the secondary winding of a transformer. From the beginning of 1954 the use of pulse transformer technology was also accepted as part of the probable specification, and the suppression of the wriggle instabilities was recognised as one of the principal outstanding problems. Following the original experiments of Carruthers and Davenport a series of tori were constructed, notable amongst them were a succession of four, made of aluminium with 34 cm bore and diameter of 1 metre. In these larger tori, known as Marks I - IV, it was found that more current could be produced for a given circumferential electric field, but this field was limited (in Mark I) by arcing across the two insulated gaps separating the two halves of the torus. This problem was greatly ameliorated by inserting a metal liner inside the Mark II torus, divided into twelve sections, so that the voltage per gap was reduced by a factor of six. The current achievable before arcing occurred was about 15kA. The Mark III torus was similar to Mark II, but used more for engineering studies relevant to Zeta than physics measurements. Mark IV, built much later, was coated internally with vitreaus enamel, in an (unsuccessful) attempt to reduce arcing(105).

Enthusiasm arising from the progress being made generated pressure for a large scale experiment and, as described later, a proposal was put forward and approved in 1954, before the wriggle problem had been solved(106). Meanwhile, however, another key figure had been recruited in the experimental group from the Clarendon Laboratory. This was Roy Bickerton, who had already completed his doctoral research on the behaviour of a glow discharge in the presence of an axial magnetic field, and who was to contribute significantly to the suppression of the instabilities. In 1955, he put forward the suggestion that the discharge might be stabilized by applying externally a magnetic field parallel to the current(107). Without such a field the outward force on a displaced current channel was proportional to the displacement and to the square of the current. In a metal torus the repulsion due to the image current was also proportional to the square of the current, but this only operated at very short range. Once the channel became displaced, therefore, the instability grew as a result of the proportionality to the displacement, and was not countered until the channel got far too close to the torus walls for containment to be possible. Experimentally this was manifest as a rapid rise of the discharge impedance, requiring much more power to sustain the current.

Bickerton proposed that in addition to the windings used to induce the toroidal current in the plasma, a toroidal solenoid designed to carry a high frequency current should also be wound on the torus. This would produce an axial RF magnetic field in the direction of the discharge and would have the effect of changing the magnetic field lines at the surface of the current channel from circles into helices, along which the current would flow. If the current channel then became displaced and began to wriggle, the electromagnetic attraction between adjacent turns of the helices would provide a restoring force. To test the proposal

Bickerton constructed a new glass torus, which was then linked to Carruthers's original pulse transformer. A variety of solenoidal windings was used to produce oscillating toroidal magnetic fields at frequencies in the range 1 - 500 kHz[108]. A simple theory suggested that the applied field should have a significant stabilizing effect at frequencies upwards of 100 kilocycles if the total coil current through the torus exceeded 20% of the discharge current in the toroidal direction. In a series of experiments covering different permutations of the variables Bickerton found that the wriggling instabilities could be suppressed over a wide range of parameters. This was evident from streak camera pictures and by an increase by a factor of about two of the discharge current for a given loop voltage. It was later predicted and then found experimentally that a steady rather than an oscillating applied toroidal field could produce essentially the same effect. This was due to the high electrical conductivity of the plasma which resulted in trapping or freezing-in of the toroidal field for the entire duration of the discharge pulse. This had not been included in the original simple theory. Such a steady field is technically much easier to apply, and was henceforth a standard feature of toroidal discharge experiments. Preliminary experiments on the twelve section Mark III torus in July 1957 achieved a greatly increased current of 60 kA. This torus is shown, with windings to produce the toroidal field, in Fig. 5.

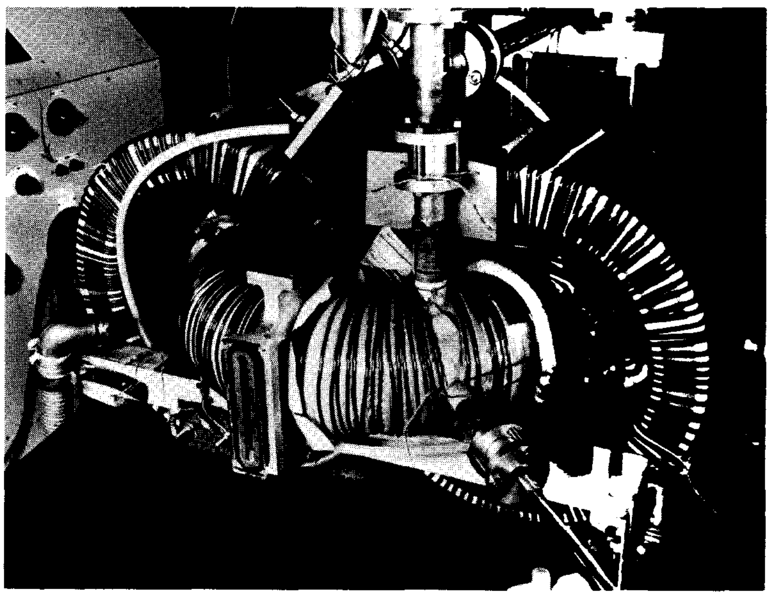

Fig. 5 The Harwell Mark III torus with windings to give toroidal magnetic field.

The work on pulsed toroidal discharges at Harwell was to lead, at the end of 1954, to the proposal for the construction of the large scale Zeta experiment there. There were, however, other events too which contributed to the growing enthusiasm for fusion research, some of these will be discussed before returning in a later chapter to details of the "Zeta story". We consider first some events outside Harwell in the first half of the decade.

At the AEI Aldermaston Court laboratories, Ware and Hemmings from Imperial College had been joined by Howard Miles, Bruce Liley and a number of assistants, and under the direction of D R Chick had pursued a programme complementary to that at Harwell. Their work is documented in detail in a series of progress reports written as a condition of the contract[109]. On the theoretical side Ware and Thomson had developed a more sophisticated theory of the pinch effect, while on the experimental side the main effort had been devoted to experiments using large transient oscillatory currents(110). These were first generated by discharging a condenser bank through a primary winding coupled to the gas, generally aided by an iron core. Many variations of torus size and material, and of mechanical configuration, were tried. Later, discharges driven by a 10 kHz oscillator with pulse length of a few seconds were studied, and by 1954 discharges with a rectangular voltage pulse applied to the gas using the pulse transformer technique introduced by Carruthers had also been generated. Detailed studies were made of power limitations, arising from evaporation of wall material in quartz and ceramic tori, and arcing across the insulated gaps in metallic ones.

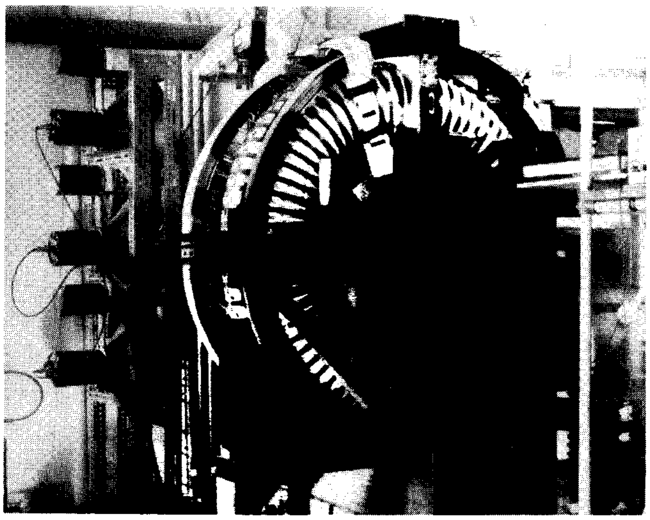

Fig. 6 The 64 sector torus, at AEI Aldermaston.

As early as 1952, working with 12 kHz transient discharges in a small aluminium torus of 20 cm major diameter, made from two semi-circular sections insulated by mica, the team had found very bad arcing between the sections, melting the metal and short-circuiting the discharge, with voltages as low as 100 volts per turn and currents of only a few thousand amps. They had then developed a series of multi-gap toruses with the aim of decreasing the voltage across any one gap and by 1955 had constructed a 64 sector torus of major and minor diameters 1 metre (mean) and 30 cm. Using a 54 kJ condenser bank they achieved transient currents of up to 80,000 amps.(109,111). Even in this apparatus, however, the problem was not solved. The complicating factor of the "unipolar arc" from the discharge to a point on the liner was not appreciated at this time(112). Furthermore, the gaps associated with the large number of sectors greatly reduced the stabilizing action of the walls. The torus is illustrated in Fig. 6.

CHAPTER 5

SHOCK WAVES, AND THE WEAPONS GROUP

The first few years of the 1950s had been years of steady progress both at Harwell and at AEI, but around the middle of the decade there was a notable diversion that had a marked effect on the scope of the controlled fusion programme. This arose in the autumn of 1954 and was centred on the suggestion that it might be possible to generate the temperature and pressure required for thermonuclear reactions by using converging shock waves. This possibility had been raised by Arthur Kantrowitz of Cornell University in the USA, an expert on aerodynamics and jet propulsion who had been studying the production of high energy cylindrical shock waves in gas filled shock tubes(113). (A speculative proposal for a "reciprocating internal combustion engine" using shock waves had been submitted to the AEC by R R Wilson in 1953(114). Nothing about this was officially known in the UK, but the possibility of a leak cannot be ruled out). Kantrowitz's work had been brought to the attention of the Harwell scientists by W B Lewis, head of the Canadian atomic energy project and an old friend and colleague of Cockcroft's, and the possibilities were thought to be worth exploring.

Because of the practical difficulty of generating shock waves of sufficient strength, the avenue seemed less promising than that of electrically driven discharges, but both Flowers and Thonemann felt that as a matter of principle the programme should not be restricted to a single approach, and the shock wave idea provided an alternative line of enquiry(115). In September 1954 they wrote a note to Cockcroft setting out their ideas for an experimental programme, suggesting that a group of four, backed by four theoreticians, be set up(116). R Hide, a newly joined Harwell fellow, together with J D Lawson and W Millar (both originally from TRE) were asked to investigate the shock wave approach and set up an experimental programme.

Further incentives to broaden the enquiry were generated by optimistic assessments by Admiral Lewis Strauss, chairman of the US Atomic Energy Commission. It was rumoured that he had told Cockcroft that within 10-15 years the East Coast of the USA would be fuelled by fusion power. There was concern that the Americans might have hit upon some new idea not yet apparent in Britain. Accordingly a part-time study group, soon to be known as the "confusion group" was set up to try to broaden the approach, and generate new ideas. In addition to those already working in the field participation by physicists working in other fields was encouraged. These included R S (Bas) Pease, H London, W D Allen and J H Tait, who had access to classified weapons information. Discussions were lively and speculative; both Thonemann and Flowers took an active part, and a course of lectures was given by Thompson. Little was written down, though Lawson's general "criterion" was formulated as an attempt to set a simple goal with emphasis especially on confinement time as well as temperature. A number of ideas that had been considered were collected together in a brief critical review(117). In retrospect, the most interesting were the use of fast fission in a U_{238} blanket to improve the energy balance(118), and the idea of a directed beam circulating in a plasma; this was shown to be possible in principle of giving a net energy gain, though the technical problems of producing a sufficiently intense beam and injecting it into the plasma were recognized as formidable.

Returning to the shock-wave proposals, the subject is, of course, central to atomic weapon design, and it was evident that the Atomic Weapons Research Establishment (AWRE) at Aldermaston would have much to contribute. Accordingly, in January 1955 a joint meeting was held with senior scientists from AWRE at Aldermaston, with the idea of drawing on their explosives expertise for the work(119). There was a slight problem of classification, AWRE wanting a much higher classification than did Harwell so that they could conduct the work in close collaboration with their own weapons projects. An approach to Harwell by scientists at Armstrong Siddeley and Armstrong Whitworth who independently had ideas about using shock waves and who wanted to collaborate with Harwell also caused minor embarrassment(120). These problems were soon sorted out, however, and by mid-summer of 1955, following a preliminary report by Hide and Millar(121), a joint experimental programme on shock wave studies had been agreed(122,123). The AWRE side of this programme, devoted to pursuing some ideas of Thonemann's on the use of explosives, did not come to anything. An experiment was performed in which a hollow copper cylinder, of diameter 2 inches and thickness $\frac{1}{4}$ inch, was compressed by high explosives in a magnetic field. Initial indications were that the hoped for field of 100 Tesla had been obtained, but it turned out that this was far from the case(124). Indeed the AWRE scientists quickly dismissed the whole idea of a shock wave approach to fusion as not worth pursuing. But the project did bring the weapons scientists into the controlled fusion programme, to which they eventually devoted a substantial effort. This work, initiated in 1955, forms the subject of a later chapter. The Harwell side of the project, concerned with the effect of axial fields on Kantrowitz shock tubes, was pursued for a while. Experimentally, Hide and Millar were able to repeat and extend Kantrowitz's experiments, but not to achieve any results showing real promise for the fusion programme(108). Theoretically, however, the problem attracted the attention of several Harwell physicists including R T P Whipple, W B Thompson, and a recent recruit to the Theoretical Physics Division, Walter Marshall(125).

It was soon realized that instead of producing shocks explosively or mechanically in the presence of a static field, they could be produced by feeding energy very rapidly into plasma in a cylindrical or toroidal discharge. Indeed such shocks had already been produced in Cousins' and Ware's experiments at Imperial College(31). An approach along these lines was strongly urged in a note to Cockcroft by Flowers and Thompson in May 1955(126). In it they claim that much greater compression of the gas would be expected than in the normal pinch; the process would be a fast one in which the wriggle instability might not have time to develop. Thus, what had initially seemed to be two very different approaches now appeared to be quite closely related. The urgent programme advocated was not embarked upon, but the recognition of the role of magnetic shocks in plasma physics was an important one. Although the possibility of shock driven pinches was later extensively studied in connection with possible fusion devices, this approach was found to be not promising and the main application has been to astrophysics.

CHAPTER 6

GATHERING MOMENTUM, AND THE CONSTRUCTION OF ZETA

Meanwhile, inspired by ever increasing confidence in the transformer driven toroidal discharge, the bold decision was taken at the end of 1954, to go ahead with a large scale fusion device aimed at producing conditions under which thermonuclear reactions might take place. At about the same time Sir George Thomson renewed his own pressure for a large scale experiment[(127)], and after some months of discussion at Harwell Fry asked Cockcroft in November for approval and priority for Thonemann's proposal, for a large transformer driven discharge in a metal torus[(128)]; Thomson's proposal was not accepted. A paper was put to the newly formed Atomic Energy Authority (AEA), which had taken over responsibility for the atomic energy programme from the Ministry of Supply that year, for a 100,000 amp experiment to be sited at Harwell. The cost of the experiment was estimated as a modest £127,000 for the year 1956-7, and about £200,000 in total, and it was unanimously approved. The implications if it were successful were such as to make the risks of it not being so seem well worthwhile and Sir William Penney, then director of AWRE, also gave the project his full support as having some immediate value for weapons development work, largely through the fundamental plasma physics and mathematics involved. Although the name was not yet coined, Zeta was born[(106)].

Once the experiment had received approval in principle, however, the fusion programme quickly changed gear. New impetus was given to the experimental programme, and the aluminium Mark III torus already referred to was seen as a prototype not only for constructional and operational features of the machine, but also as a test bed on which various measurement techniques could be developed. The years prior to the operation of Zeta saw intense activity, studying the behaviour of the complex plasma configuration in both toroidal and straight systems. During this period contact with the American groups was established, and there was much exchange of information with American colleagues. (See Chapter 8 and ref. (129)). Questions arose of what to measure, and how to make the measurements. Older techniques such as spectroscopy were exploited, and new ones attempted. The feeling of excitement and challenge is well captured in the minutes of the regular meetings of the "Gas Discharge Project", attended by representatives of Harwell, AEI, and later AWRE[(130)]. The work included spectroscopic observations of radiation from the plasma, measurement of microwave radiation, and probe techniques for measuring plasma temperature and electron density. New phenomena were investigated, such as runaway electrons in the torus, detected by the X-rays which they emitted. One of the principal objects of study was the kink instability, and the further development of time resolved techniques to examine it in closer detail. Much of this experimental work was done by young physicists recruited to the project. Useful contributions to the development of spectroscopic techniques at the shorter wavelength were made by university and industrial research laboratories, through development contracts supported by Harwell.

As soon as Zeta was approved informal negotiations with potential suppliers and manufacturers were begun almost immediately, and by the summer of 1955 provisional line charts for the large experiment were being drawn up[(99,131)]. At about the same time, following a recommendation by Flowers and Thompson, Cockcroft applied to the Atomic Energy Executive for an immediate doubling of staff on the controlled fusion project to about 25, excluding the theoreticians, to be followed by a further doubling in the next couple of years(123). This was approved and Sir Edwin Plowden, the chairman of the Atomic Energy Authority, on 20 May placed on record his desire that the fusion programme should in future be given a very high priority.

In large part, the new momentum was internally generated. But there were also external factors. In May 1955 Flowers and Thompson had reported "increasingly consistent indications from the United States that the fusion of deuterium under controlled conditions is not only possible but likely to be used on an industrial scale 10 to 15 years from now", and expressed fears that Britain might be losing her initial head start on the subject and falling behind the Americans(126). At that time the barriers of classification were still up, and although it was generally recognized that the Americans had a controlled fusion programme, in 1955 virtually nothing was known about its details. The reported optimism, however, was far from universal. The only statement that had been made "on the record" was by Robert Oppenheimer, who had expressed his conviction that controlled fusion simply would not work(132). But the rumours "off the record" persisted and the resulting optimism and fear of losing out to the Americans were pervasive. In July 1955 the experiment was given its codename of Zeta (suggested by D W Fry), for "zero energy thermonuclear assembly" and assigned Harwell "station priority"(99,133).

As the year progressed, so did expectations. At the first Geneva Conference on the Peaceful Uses of Atomic Energy, held in September 1955, the conference president, the Indian nuclear physicist Homi Bhabha, joined the growing clan of optimists. Discussing the possibilities of controlled fusion energy, he concluded(134):

> I venture to predict that a method will be found for liberating fusion energy in a controlled manner within the next two decades. When that happens, the energy problems of the world will truly have been solved for ever, for the fuel will be as plentiful as the heavy hydrogen in the oceans.

Public awareness of the possibilities of controlled fusion had already been excited four years earlier, in 1951, when the Argentine dictator, Juan Peron, had claimed the successful release of fusion energy in a laboratory he had had set up for a German physicist, Ronald Richter. Although this claim had caused quite a stir, and indeed appears to have been at least partly responsible for the creation of a controlled fusion programme in the United States, it had been greeted with widespread and justified scepticism and the public interest had quickly waned(135). Now, however, Bhabha's words sparked off a sharp revival of interest, and the persistent efforts of journalists that followed led to no fewer than five countries, Britain, the USA, the USSR, France and India, claiming or admitting the existence of controlled fusion projects within their atomic energy programmes(136). The existence of the American project Sherwood and of its project branches throughout the USA was reaffirmed by Strauss in a statement on 3 October, and during the same month the fourth "Annual Conference on

Atomic Energy in Industry", in New York, provided the setting for the first ever public symposium on controlled fusion energy(137). Once more a leading physicist, this time Hans Thirring, dismissed the possibility of controlled fusion as an unrealizable speculation(138); but the following month Cockcroft reported to his Harwell colleagues that Ernest Lawrence, inventor of the cyclotron and world expert on high energy particle accelerators, shared Bhabha's optimism(139). At Harwell itself opinions varied widely. Lawson, having produced his "criterion" at the end of 1954, emphasized that a central problem was to produce a device that generates more energy than it consumes. Conditions would be stringent, and it was not yet clear whether they could ultimately be met(117). Pease, who had recently joined the Harwell fusion group when his previous work on neutron diffraction and radiation physics had been moved from General Physics to the Metallurgy Division, in May 1956 gave a simple analysis of the behaviour of a steady state pinch in which energy supplied from a longitudinal electric field was dissipated as bremsstrahlung radiation(140). Stability was not considered, and the models were not all fully self-consistent, but they gave no grounds for indicating that the conditions required for a reactor could not be achieved. The central feature of the calculation was the interesting conclusion that over a fairly wide range of assumptions equilibrium occurs at a fixed current of about a million amps whatever the line or volume density of charge in the plasma. Pease presented his work at the International Astronomical Union conference in Stockholm in 1956, where it became evident that this result was known in the USSR but had not been published. This important characteristic current is now known as the Pease-Braginskij current.

In retrospect, despite the successful demonstration of a thermonuclear explosion by American scientists in 1952, neither the conclusions of Lawson and Pease nor the many public speculations as to the possibility or otherwise of a fusion reactor gave any real grounds for optimism. But by 1956 there was something of a fusion fever, which both uninformed debate and continued official secrecy served only to fuel. Thus, for example, the very fact that the Americans were pursuing a large controlled fusion research programme was taken as indicative that there must be a real possibility of it working, even though none of their results was known. In this situation further fuel was added to the fire when the Russian Academician Kurchatov, visiting England with an official Soviet delegation in early 1956, suggested to Cockcroft that he might talk about Russian fusion work (among other things) at Harwell. This offer, which seems to have come completely out of the blue, could only be accepted, even though it was suggested that Kurchatov's main aim was to discover, through the questions following the lecture, how far the British programme had progressed. Kurchatov's lecture, delivered on 26 April 1956, was really an object lesson in the interpretation of gas discharge results. The Russian team had thought that they had created thermonuclear neutrons in a straight discharge as early as 1952, but they had subsequently realized that several features of the neutron emission were in fact inconsistent with the hypothesis of a thermonuclear origin, though at the time of the lecture they had still not been able to work out exactly how their neutrons had been produced(141). The emphasis of the lecture was upon the complexity of the situation, the difficulty of drawing unequivocal conclusions, and the dangers of jumping to equivocal ones. Most of the physicists present, who included the AWRE and AEI teams as well as those from Harwell, took note of this cautionary tale. But after the Russian atomic bomb success the fact that they were working on controlled fusion again prompted the fear of being beaten to the target, and this further acted to speed up the British programme. It was as a direct consequence of this lecture that an experimental programme on the 'Z-pinch', as this type of discharge became known, was started at AWRE.

The manifestations and repercussions of the new era of excitement were many, but the most significant by far was of course Zeta(99,142,143). It is generally recognised that Zeta was "Thonemann's baby", and certainly the details of the apparatus owed as much to his feel for the fusion problem as to formally demonstrated conclusions. Although Thonemann's was the guiding hand, however, it was far from being the only one. At the time the Zeta experiment was first approved there was really very little idea of what it would entail or achieve. There was widespread and growing pressure for a large device that, while still a net energy consumer, might at least produce a measurable quantity of thermonuclear neutrons. Thonemann had a pretty good idea, obtained from some simple basic calculations, of how big this would have to be. As Fry recalled, they went through the usual procedure of putting up a few papers saying roughly what they thought it would cost, got it approved, and only then started to work out a detailed design and costing(144). The basic specification of Zeta had been set in 1954. An aluminium torus of about 3 metres diameter and one metre bore would be filled with gas at a pressure of about 10^{-3} torr, and a discharge initiated by a pulse from a transformer generating 3 millisecond 100,000 amp pulses with a primary voltage of about 25 kilovolts(106,146). The torus wall was chosen to be 1 inch thick so that the self-magnetic field of the discharge could not penetrate through it for about 10 milliseconds. (This meant that the magnetic field of the current channel would be compressed against the wall if the current channel moved away from the axis of the torus. This would limit the increase of the large radius of the current channel, but, more importantly, would limit the amplitude of the kink instabilities).

The details of the specification took a year or more to evolve, however, and involved some changes from original ideas. In part these changes resulted from the exploratory discussions with manufacturers and suppliers that took place in the early part of 1955. In part they followed from experiments with the 34 cm bore Mark III torus in late 1955 and early 1956. The main contract for the Zeta equipment was put out in the autumn of 1955 to Metropolitan-Vickers, part of the AEI group of companies with whom Harwell were closely associated in this as in other work. The previous history of collaboration between Harwell and Metropolitan-Vickers was not altogether encouraging, as several large contracts, notably for innovative designs of particle accelerators, had severely overrun both their schedules and their budgets. But so effective was their contribution in this case that at the time the contract was considered the most successful ever let by Harwell(145). This seems to have been due largely to the role of Carruthers, who conducted initial discussions with Harold West, chief engineer of Metropolitan-Vickers, and secured an organizational arrangement which avoided the difficulties which had arisen in the past, largely due to lack of communication between the research and manufacturing sides of the firm. This was achieved by giving a special role to Charles Flurscheim, recently appointed as deputy chief engineer, who was familiar with the technology required. With his energetic participation project requirements were pushed through the production stage with exceptional speed, and several members of staff, detailed to keep in very close touch with Harwell, helped to anticipate potential snags. The final cost of the experiment was higher than at first anticipated, at about £300,000, but the increase could be put down entirely to specific design changes, all of which were agreed at a very early stage and not within the original contract. From the finalization of the design in the spring of 1956 to the completion of the installation eighteen months later, the project remained within its target costs and dates, a creditable achievement considering how demanding were the requirements[99,145]. Apart from Thonemann, Carruthers and their Metropolitan-Vickers collaborators, most notably E R Hartill, M A Bird and J B Blears, many others contributed significantly to the construction and commissioning of Zeta. Among these were John Mitchell from Harwell Engineering Division, and E P Butt who supervised the technical aspects of the instrumentation.

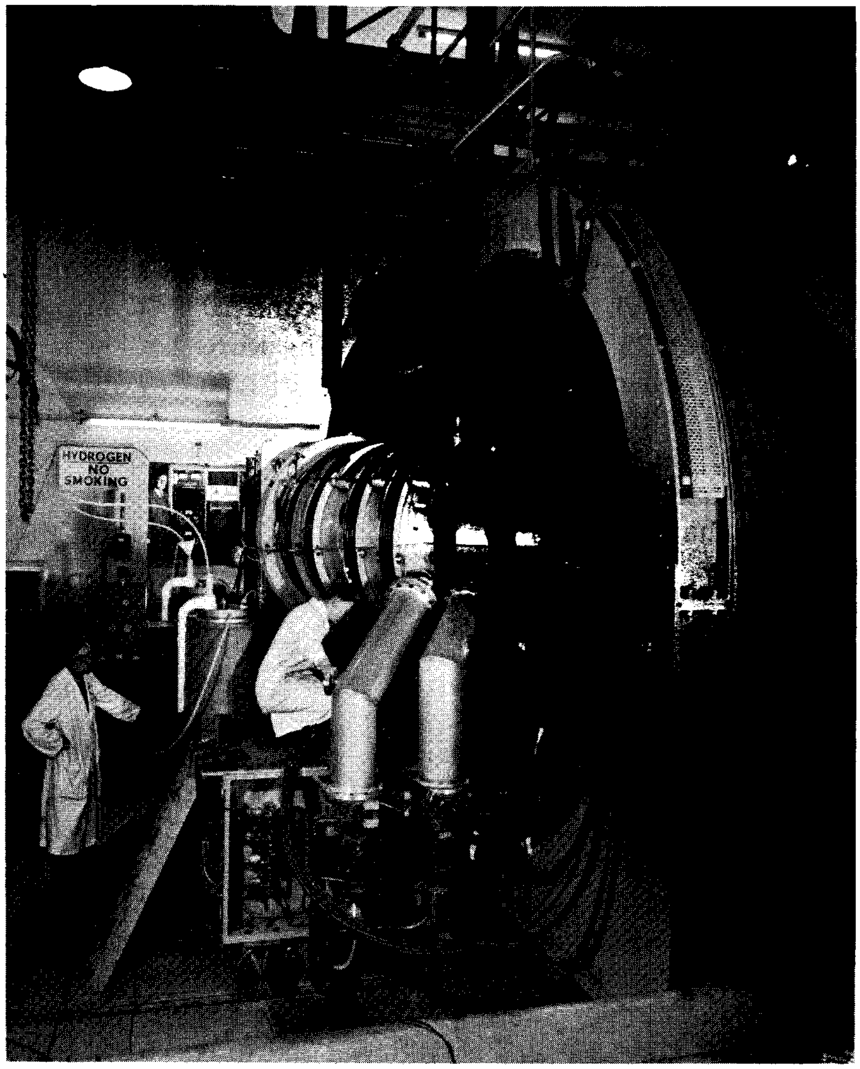

Fig. 7 The Zeta torus at Harwell, soon after its initial operation.

The Zeta project was not without its problems. Potentially the most severe was the supply of grain oriented cold-rolled steel for the 150 ton transformer, where Metropolitan-Vickers' suppliers were gloomy about obtaining sufficient material of required quality on such a short timescale. This was solved, however, when a strike in the American electrical industry produced a sudden glut of American steel of the desired quality, and other problems were solved with less need of good fortune. An important change was made after the contract had been let. This was the installation of solenoidal coils around the torus, to provide the longitudinal field prescribed by Bickerton. These were incorporated with some increase of cost, but essentially no delay to the programme. Power was supplied by a spare generator for the nearby cyclotron. Despite these changes, numerous unfamiliar problems, and the occasional need to improvise, Zeta was eventually installed on schedule and within budget, operating for the first time in August 1957[(99)]. Design details and operating parameters were later presented at the 1958 Geneva Conference by Butt *et al*[(146)], and diagnostic techniques used in Zeta and other experiments at Harwell by Harding *et al*[(147)]. Zeta is illustrated in Fig. 7.

A further improvement, not incorporated until 1959, was to replace the original liner of the torus. The liner was split into 48 segments to reduce the voltage per gap seen by the discharge, and fitted with PTFE insulation across the gaps. (The purpose of the insulation was to prevent bombardment from the plasma causing ionization across the two gaps of the torus, and thus to increase the breakdown threshold). The new liner was continuous, made of corrugated stainless steel, sufficiently thin that its resistance was considerably higher than that of the plasma[(148)]. With it the arcing problem was greatly eased, but some erosion arising from unipolar arcs still occurred[(112)]. This assembly, known as Zeta 1A was ultimately able to handle currents of 900kA, nine times greater than originally envisaged for Zeta 1[(148a)].

CHAPTER 7

SOME CONTEMPORARY DEVELOPMENTS AT AWRE, AEI AND THE UNIVERSITIES

Outside Harwell the developments of the mid-1950s also made a significant impression. Following their initial involvement in shock-wave work, AWRE soon developed their own, self-contained, controlled fusion research programme. The programme at AEI was radically revised, and the relationship between this programme and that of Harwell also underwent substantial changes. Following the growth of public interest in the possibilities of fusion energy, there was increased interest and activity too in the universities.

One of the main problems faced by AWRE during the early and middle parts of the decade was that of attracting to the establishment the first-rate physicists needed to keep up the standard of weapons work. One response to this problem was to engage in a small amount of non-military work and use this as a carrot. In 1954 K W Allen, a nuclear physicist from the University of Liverpool, was recruited on the basis that facilities would be provided for him to undertake non-military research. In 1955 S C (Sam) Curran, a senior nuclear physicist from Glasgow University with a wartime background in radar research at TRE, was recruited on a similar understanding. Meanwhile, in January 1955, Sir William Penney received permission from Sir Edwin Plowden to devote up to 10% of the establishment effort to non-military research, and this change coincided with the approach from Harwell and the suggestion that AWRE should participate in shock-wave research for the controlled fusion programme(149). The shock-wave work did not get very far, but both Allen and Curran were attracted by the possibility of engaging in controlled fusion work across a wider sphere, and in August 1955 they obtained permission to set up a small group for that purpose(150,151). From the beginning concerns of secrecy kept this group somewhat isolated from those at Harwell and AEI. When they agreed to take on the shock-wave work, for example, AWRE at first tried to insist that AEI should be kept ignorant of that part of the programme, and when it became clear that Harwell would not accept this - indeed could not accept it, since AEI were themselves pressing for work on the possibility to be done - the AWRE response was to keep their own counsel(152). They did take part in a joint progress meeting with Harwell in October 1955, but this had no sequel, and although Curran attended the joint Harwell-AEI meetings he did so at first as an observer only, with instructions to communicate nothing of the AWRE work in the presence of AEI staff(153). In the autumn of 1956 there was some concern at this attitude when it materialised that AWRE had been working on Z-pinch experiments similar to those of Kurchatov without telling anyone, for AEI had themselves been pursuing the same research with Harwell approval(130). After this the AWRE team were more forthcoming, but they continued to work quite independently of the other two groups.

At first, the AWRE team concentrated on a suggestion of Curran's that they should try to generate fusion reactions through the interaction of narrow beams of accelerated deuterium and tritium ions with plasma or with each other(151,154). The colliding beam proposal, reminiscent of early attempts using metal deuterides at Los Alamos, was criticised by the Harwell physicists as standing no chance of producing a positive energy balance(124), and was finally abandoned in December 1955 when P O Hawkins, one of the AWRE group, demonstrated it to be quite unworkable(155). The group, augmented by Hugh Bodin, A A Newton, R A Fitch and others, next turned to a hybrid fusion-fission concept, in which it was proposed to bombard a hot deuterium target with a narrow beam of accelerated tritium ions in order to create a strong neutron source. This neutron source would itself be surrounded by a blanket of uranium and lithium, and would act as a plutonium and tritium producer. As one of the earlier attempts to investigate a hybrid system this work, the idea for which dated from mid-1955, was of some interest(124,154). But following the Kurchatov lecture in the spring of 1956 it was realized that the problems lay not so much in producing and accelerating a high intensity beam (as they had thought), but in preparing the target and containing it so that the beam did not simply pass through(154). The AWRE team therefore abandoned this line too and following Kurchatov turned instead to the investigation of the simplest form of pinched gas discharge, the Z-pinch, or pinched discharge in a straight tube.

Part of the thinking behind the Z-pinch work was that by using such a simple set-up it might be possible to get far more useful information than from the more complex toroidal experiments. In this respect it tied in with the AWRE approach, intrinsic to advanced weapons work, of working within a clearly defined and well-established theoretical framework rather than speculating in an area where theory was completely lacking. There was also the thought, however, that if the gas could be heated up very quickly indeed, in principle easier in a straight tube than in a toroidal one, it might be possible for thermonuclear reactions to take place before the discharge had a chance to break down through the inevitable instabilities(154). In early February 1957 the experiment did indeed produce neutrons, and it was at first thought that these, unlike those produced in the similar experiment reported by Kurchatov, might be thermonuclear. But two years later it was shown that the neutrons were not thermonuclear, and that the current flowing through the pinched column was much smaller than the total current because much of it flowed along the walls of the tube. As a consequence the AWRE Z-pinch programme was stopped(156,157).

While AWRE kept their work very much to themselves, the developments of the mid-decade also upset the collaboration between Harwell and AEI. In the early stages of the project the AEI team had seen their relationship with Harwell as being one of equals, but with the commencement of work on the Zeta experiment it became clear that Harwell did not fully share this view. As Zeta got under way the Harwell programme was greatly expanded, but the work at AEI was not. Increasingly, the AEI team were asked to investigate specific problems of importance to the Zeta project, but without being given any major part to play in that project as a whole. Collaboration continued, and the AEI contribution continued to be a very real and important one. Although their work on arcing had been to little avail they did do valuable work on other important problems, most notably on electrode materials. They studied the possibility of using ceramics for the follow up torus to Zeta, and a group under R M Payne carried out much-needed diagnostic work on the Harwell 34 cm bore tori (Mk I-III). Ware joined in the theoretical investigation of instabilities, and an AEI "pepperpot" torus was used to study the instabilities experimentally. This was a four sector aluminium torus of about one metre mean diameter to which two 60 cm long "racetrack" sections were added. The straight sections were perforated so as to allow an external rotating camera to photograph the development of the wriggling discharge, and the whole was linked up to a large 200 cycle generator(111,130,158). While they continued to collaborate with Harwell, however, the AEI team were not happy. They were upset that Harwell were not prepared to sponsor work they wished to do on the suppression of the wriggling, and in general they wished, naturally enough, to play an active part in the direction of controlled fusion research(159).

Dissatisfaction with this situation intensified with the publicity given to controlled fusion possibilities in late 1955, and at the end of the year AEI decided to expand their own fusion effort so as to include research, for which they proposed to pay themselves, independent of the main Harwell programme. This proposal was not warmly received by the Atomic Energy Authority, and indeed ran completely counter to their established policy, but after a short deliberation they decided both to increase the value of the AEI contract and to permit them to carry out work on their own, subject to certain security safeguards and to agreement on patent rights(160,161).

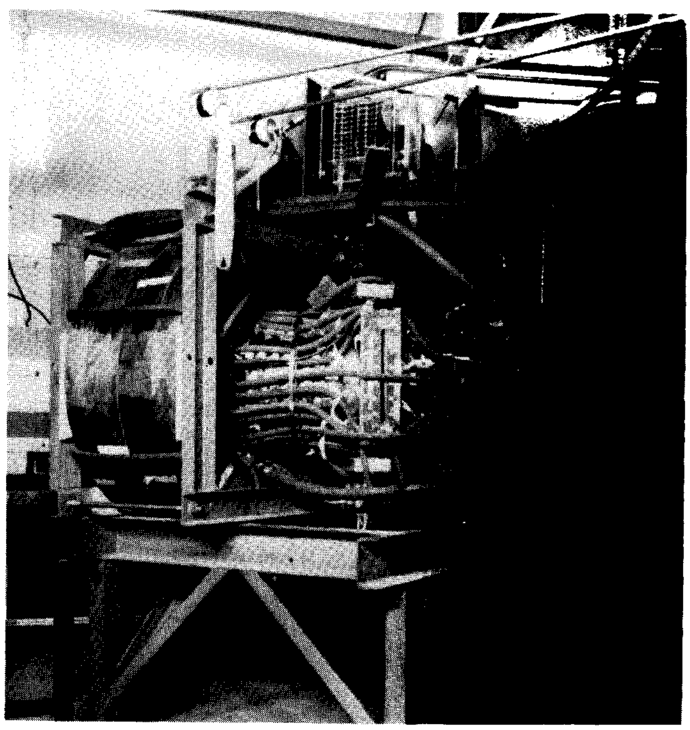

Fig. 8 The Sceptre torus at AEI, Aldermaston.

As part of their independent work AEI rebuilt their 64 sector torus and began to reconsider the possibilities of radio-frequency alternating current power sources. Then in late 1956 they decided to try to exploit some recent theoretical results of Shafranov in the USSR, Rosenbluth in the USA, and Tayler at Harwell, which provided a theoretical basis for Bickerton's proposals for creating a hydromagnetically stable discharge(111). (The details of this theoretical work and of its implications for the Harwell programme will be considered in Chapter 10). At AEI they tried to explore the possibilities for stabilization further on two small tori named Sceptre I and Sceptre II (for "stabilized controlled pinch thermonuclear reactor experiment"), using ohmic heating of the gas from the currents along and around the discharge, aided by heating from adiabatic compression. However, when in the late summer of 1957 Zeta showed a degree of stability beyond the theoretical expectation, the AEI team abandoned these experiments and, removing the straight sections from the pepperpot torus and replacing its power supply, reconstructed it as a smaller and lower powered version of Zeta under the designation Sceptre III(111). This is shown in Fig. 8.

Throughout the fifties the main fusion effort in Britain remained in the AEA and in AEI. But following the public interest in the subject during 1955 and 1956 the universities too began to get interested. In response to this interest, a gas discharge symposium was held at Harwell in June 1956 and selected representatives of the universities and industry were invited(162). At this symposium AEA and AEI staff presented reports on their research and Mark Oliphant of the Australian National University, who had recently arrived in England following a visit to Russia, reported on what he had been able to find out about the Russian fusion programme. At the end of the day the discussion showed that not all the university representatives were that keen on the subject. As with high energy particle physics they were put off by the scale and cost of the experimental apparatus. But those who had already created small programmes, most notably Oliphant and Blackett, of Imperial College London, argued passionately for an extension of university and industrial work in the field. The proceedings of this conference, and edited discussion, give a very good overall view of the subject as seen at the time(163). Following the conference a number of those present did nevertheless initiate small programmes in their departments and laboratories(164).

CHAPTER 8

TOWARDS COLLABORATION AND DECLASSIFICATION

The same events as led to the change in the size and scope of the British controlled thermonuclear fusion research programme in the mid-1950s also had considerable repercussions upon the official policy in respect of classification and collaboration, especially with the Americans.

Following the discussions on classification late in 1950 it had been agreed that the existence of a serious attempt to develop controlled thermonuclear energy should be kept secret. In 1951 the general rule was implemented that all high power gas discharge work, especially all that aimed at increasing the discharge current, should be classified(165). This policy remained unchanged for several years, and it was based on two principles both of which were reiterated when a new classification guide came to be drawn up in early 1954. The official reason for classification, incorporated in this guide, was that the programme might lead to a substantial increase in the output of fissile material(166). Concern with the possibility of the fusion process being used to generate neutrons for the production of plutonium and other fissile materials had dominated the original decision to classify the work and continued to be seen as central, especially by the London Office of the Atomic Energy Authority, and by Penney's Atomic Weapons Research Establishment at Aldermaston. At Harwell there was progressively less concern with this specifically military aspect of the project and the scientists, many of whom would have preferred the whole project to be unclassified from the beginning, grew more and more impatient with the classification, and dismissive of its military justification(167). But they were for the most part convinced of the practicability of the thermonuclear energy concept, and Cockcroft at least was convinced that the Harwell team led the world in controlled fusion research. Given his relatively limited resources Cockcroft was worried that any declassification would lead to the initiative in the field being taken over rapidly by the Americans and the Russians, and he began to stress what had originally been only a secondary principle, that the aim in deciding on questions of classification "must be to maintain any lead we have in the field"(168).

Backed by this combination of arguments the classification policy was executed consistently throughout the early and middle 1950s. Papers intended for open publication had to pass at first through the high level Publication and Declassification Sub-Committee, which met at Harwell, and later, when the paranoia resulting from the Fuchs case had died down, through the responsible division heads, Fry and Flowers(168,169). Scientists attending conferences or giving lectures on gas discharges and other closely related topics were supplied with explicit guides as to what they could or could not talk about(170). And those attending the Kurchatov lecture at Harwell in April 1956 were given a detailed list of subjects not to be mentioned during the ensuing discussion(171).

In one respect the classification policy created a rather strange situation. Thus, although there was no collaboration with the Americans, and although the British and American teams of scientists knew nothing officially of each other's work, and did not in theory know even of each other's existence, there was an agreement to maintain a common classification policy(172). This meant that papers considered for publication by each country were sent to the other for approval, and while the details of the two projects do seem to have been successfully kept secret, their existence could not but be inferred(172,173). There were also problems associated with the establishment of priority and with the availability of information freely disseminated before the security barrier had been erected. One example illustrating both problems arose in the summer of 1951 when E E Salpeter, from Cornell University in the USA, wished to write an article for **Nature** on the possibility of energy from fusion, and on existing research into this possibility. Knowing of Thonemann's early interest in the subject, and deducing that he must have been working on it since, he asked him to write a section of the paper. Thonemann, anxious to get some recognition for his work, wished very much to oblige, but his division head Fry insisted, and the controlling committee agreed, that this was not allowable(174). A similar problem occurred in 1955, when J G Linhart, working at the British Thomson-Houston Co in Rugby, produced a report proposing an experiment based on a toroidal system into which 10 kW of power at 3 GHz was to be fed to produce an estimated temperature of 10^7 degrees in the gas. This was submitted to Harwell by the Chief Engineer with a request for comment(175). Since it was thought that detailed comment might indicate the degree of understanding of the subject at Harwell, this was not allowed.

One final problem, to which we have already alluded in the last chapter, arose from the differing perceptions of Harwell and AWRE. In general AWRE wished to work under a higher level of classification than did Harwell, and when this level became a subject for debate in 1956 they pressed for a much higher classification for certain parts of the programme than Harwell thought fit, and than was eventually agreed upon(176). Meanwhile their reluctance to have their own work discussed in the joint Harwell-AEI progress meetings had already led, as we have seen, to some duplication of research on the linear Z-pinch(177).

Despite these small problems, the classification policy seems to have worked very successfully at least until about 1955. But in that year the first serious challenge to continued secrecy arose with the growing speculation as to the existence of fusion projects and the nearness of their successful completion, and especially with Bhabha's speech at the First Geneva Conference on the Peaceful Uses of Atomic Energy in September(134). As a result of pressure from the media following this speech a number of nations, including both Britain and the United States of America, admitted the existence of controlled thermonuclear fusion projects within their atomic energy programmes. In early October the Americans went further than this, listing publicly the locations of their project teams(136,137). So far as Britain was concerned, the location of the main project was obvious, but the American disclosure posed the rather awkward question as to whether or not the existence of the AEI and AWRE teams should be announced(178). At this time the Harwell fusion programme was beginning its rapid expansion in association with the Zeta project. Both AEI and AWRE wished to follow suit, but in order to do this they needed to recruit new staff, and so to advertise the existence of their fusion programmes publicly. Moreover, AEI's wishes in this respect were strengthened by the prospect of the immense prestige that would be reflected onto the firm through association with such a glamorous enterprise. AWRE were still faced with their chronic problem of the shortage of top quality scientists, and saw in the advertisement of their own controlled fusion programme a chance both to attract first rate physicists and more generally to improve their weapons-dominated public image(179). At first Cockcroft, backed up by Plowden, turned down these suggestions point blank. But Penney, stressing the shortage of good scientists at AWRE, insisted that advertisement of his fusion project was essential(180). Indeed, an advertisement appeared in the News Chronicle on 10 October for staff to "work in a THERMONUCLEAR PHYSICS group studying methods of utilizing energy from the fusion of light elements". This put Cockcroft in a difficult position, as he felt that he could not allow AWRE to advertise without making the same concession to AEI, and he was concerned at how other industrial concerns, who had not been included in the fusion programme, would react to what might seem to them a blatant piece of favouritism(181). He had done nothing reprehensible, since at the time of the AEI involvement it had seemed unlikely that the firm would gain any commercial benefit from it. Indeed it was still unlikely. But with popular expectations running high this might not sound convincing to outsiders, and Cockcroft's own connections with the Metropolitan-Vickers side of the AEI combine placed him in an awkward situation. The problem was further aggravated by AEI's decision, taken in the course of the discussions on the advertising issue, to expand their fusion programme beyond that required by Harwell. It was not really overcome until the spring of 1957, when a Harwell conference was convened to inform industry as a whole of the details of the entire British fusion programme(182,183). Meanwhile, Cockcroft won the day on the advertising issue, and although the announcement of the British fusion programme in the Atomic Energy Authority's second annual report, published in July 1956, referred generally to assistance from industry, the existence of the AEI and AWRE programmes remained classified knowledge for the time being(184).

There were, however, further pressures for declassification, both from within the project and from outside. The scientists had always been unhappy with the classification policy, and when Thonemann and Ware, at an international gas discharge conference late in 1955, found that their American colleagues all assumed without question that they were both working on the British controlled fusion project, it seemed to them ridiculous to keep such information secret(185). In April 1956 Kurchatov's famous Harwell lecture, containing the first significant disclosure of information by any country on their controlled fusion work, increased the pressure for declassification. In September 1956 Thonemann, returning from a conference in Stockholm, reported that the Russians were talking openly about some aspects of their own work, that for the most part they seemed to be just as advanced as the British, and that their theoretical work seemed to be several years ahead of that in Britain. These observations, he suggested, argued strongly for declassification, without which the British programme might actually be held back(186). In response to pressures such as these, Cockcroft did move in the autumn of 1955 and again in the spring of 1956 to downgrade the classification of controlled fusion work. But cold feet on the part of Fry and continuing pressure from AWRE held up the proposal, which was then effectively nullified by American reservations(187).

Of the forces acting in favour of the retention of classification, that exerted by the Americans was both the strongest and the most important in respect of the future development of policy. As in Britain there was in America increasing pressure, especially from the scientists, to declassify controlled fusion research. Within the controlling Atomic Energy Commission opinion was divided by the spring of 1956 as to whether to declassify or not[188]. But Admiral Strauss, the chairman of the AEC who effectively controlled such decisions, placed himself firmly in the way of any change from the existing policy. In January 1956 his insistence on this led to a heated public exchange, Strauss accusing those who advocated declassification of failing to understand the implications, and Senator Anderson, chairman of the Joint Committee on Atomic Energy, publishing in response a bitter and vitriolic open letter to Strauss, advocating complete declassification of the controlled fusion programme[189]. But Strauss, emphasising the potential of controlled fusion processes for the production of fissile material, held his ground. Returning from a visit to America in early June 1956 Cockcroft reported that there seemed to him to be no prospects whatsoever for declassification[190]. At the time, the relationship existing between the two countries meant that Britain could not conceive of going it alone, and was in effect tied to the American line[191]. A few weeks later the situation appeared to change somewhat with a decision by the AEC to adopt a policy of declassification by stages. Very tangible evidence of this policy was provided by the publication in July 1956 of an extensive review by R F Post of Livermore Laboratory entitled "Controlled Fusion Research - An Application of the Physics of High Temperature Plasmas"[12]. An introductory section outlined the history of "Project Sherwood", and named both the principal workers in the USA and their locations. The bulk of the paper contained a broad review covering the basic fusion reactions, and the fundamental properties of completely ionized plasma. Curves were given of reaction cross-sections as a function of energy, and reaction power densities as a function of temperature in hot gas. Topics included plasma oscillations, mean free paths, conductivity, Debye length, diffusion, compression, bremsstrahlung radiation, containment by a magnetic field, pinch effect, kink instability and discussion of diagnostic techniques. There was, however, no hint of what specific apparatus had actually been built or planned, nor what physical configurations might be considered for an actual reactor.

This paper clearly announced to the world that there was a substantial programme of work in the USA, and its optimistic tone suggested that expansion and interesting developments were to be expected. An immediate consequence was the decision to publish an account of work done by the Harwell group, again without revealing the extent or nature of the actual experimental programme. Some of the more basic physical ideas familiar to the Harwell scientists were already presented in Post's paper, and instead of a single review six much shorter and rather more specialized papers appeared some months later in January 1957[192]. Five of these were theoretical, covering reaction rates, energy balance, pinch theory and hydromagnetic instabilities, but one important experimental paper by Carruthers and Davenport was included, with photographs showing kink instability in straight and toroidal tubes, referred to earlier. Both the American and British papers caused considerable interest in the scientific world, and much speculation about what was being done and what had actually been achieved. The British contribution was perhaps more neutral and less enthusiastic in tone, but scientific journalists were frustrated by lack of information, and rumours of important breakthroughs soon arose. These escalated as they percolated to the less scientifically enlightened recesses of the media.

This American decision to reduce the classification by stages was accompanied by a readiness to consider a full exchange of information with Britain, within an agreed classified framework. The possibility of such Anglo-American collaboration had been discussed between Cockcroft and Willard Libby, a member of the American Atomic Energy Commission, in early May, and was formally proposed by Strauss to Plowden in late July 1956[193,194]. The proposal was not unanimously well received at Harwell, where it was felt that the British programme might well be several years ahead of the American one, and that while the British team would receive credit for this if everything were to be declassified the combination of secrecy and collaboration might lead to the Americans taking not only the lead, by virtue of their greater resources, but also all the glory. Following the mixed rumours of the previous year, Cockcroft had reported early in 1956 that according to Libby the Americans were putting a lot of money into controlled fusion but without yet getting anywhere[195]. In Britain, on the other hand, there was a lot of optimism surrounding the Zeta project, and there was accordingly a strong desire to get this project sufficiently well advanced before entering on a collaborative agreement with the Americans to prevent any chance of their taking its ideas and becoming the first to release them successfully. With this in mind it was decided to postpone collaboration on Zeta until some time early in 1957. But despite this reservation it was generally felt that the collaboration proposed would ultimately be in Britain's interests, and the American proposals were therefore accepted in September 1956, on the understanding that the collaboration would be accompanied by a measure of declassification[196]. In anticipation of a move towards partial declassification, a new British classification guide for controlled fusion work had been drafted in July, and this provided the basis for a meeting with senior representatives of the American programme at Harwell in November to discuss a joint classification guide and moves towards declassification[197]. As a result of this meeting a new guide was drawn up, though not yet put into force, in December, and this left some very small parts of the programme unclassified[198]. Despite all the pressures to declassify, however, anything that was considered to have the slightest chance of leading anywhere was still kept secret, and as a result of American insistence much of it, including the proposed successor to Zeta and most of the American projects, was allotted a security grading higher than that which it had already been decided was appropriate for the fusion programme as a whole by the British Atomic Energy Authority. The introduction of American concepts of classification led to an increase in the classification of parts of the British programme, this being precisely the opposite result to that which had originally been sought.

Declassification remained a problem, as the United States Atomic Energy Commission failed for some considerable time to ratify the new guide which, for all its failings (in the eyes of the scientists), did at least allow for the publication of the anticipated results from Zeta. But collaboration between the two nations got under way very quickly. In October 1956 a small British team composed of Fry, Thonemann and Thompson from Harwell and Ware from AEI visited the

American fusion laboratories at Los Alamos, Livermore and Princeton[199]. Then in February 1957 a large contingent from the British project, including Thomson and Allibone representing AEI and Fitch from AWRE as well as most of the key members of the Harwell team, attended a large American Project Sherwood controlled fusion conference at the University of California at Berkeley[200]. This was followed by visits to the laboratories at Livermore, Los Alamos, Oak Ridge and Princeton. This conference was a marked success, and the British came away impressed both by the American optimism and enthusiasm and by the aspects of their programme concerned with approaches to plasma containment quite different from that adopted in Britain. Work on the magnetic mirror machine, a concept not considered by British scientists, under the direction of Post at the Livermore laboratories made a particularly strong impact[201]. At the end of April 1957, as a first step towards bringing industry and academe into the British programme and, especially, towards preparing to make the most of whatever information might be gleaned from the Americans, a controlled fusion conference was held at Harwell for the benefit of security-cleared representatives of universities and industrial concerns[202]. Then in June, following an international gas discharge conference in Venice, Harwell acted as host to a large delegation of American fusion scientists for a three day conference, at which the British industrial and university scientists were again present, and at which both the British and the American programmes were described in detail[183]. Thereafter, the British project continued to send small teams to successive Sherwood conferences in America, these taking place about three times a year[203]. If declassification remained a distant vision, collaboration at least was secured.

CHAPTER 9

FIRST RESULTS ON ZETA - NEUTRONS AND 'WILD SURMISE'

We now return to the main line of development at Harwell, centred around the Zeta torus, and follow on from the penultimate paragraph of Chapter 6. After initial commissioning tests Zeta was first operated on 12 August 1957, using hydrogen. A few days later the first experiments were run with deuterium. The first task was to determine the optimum operating conditions, and these were soon set at 0.016 T for the axial field and about 10^{-4} torr pressure. Then on 30 August, using these conditions and a current of 120 kiloamps, counts on the neutron detectors were observed[(204,205)]. Within a few months, the production of neutrons in the smaller toroidal systems "Sceptre III" at AEI and "Perhapsatron" at Los Alamos was also confirmed.

The first run of experiments on Zeta continued for two weeks, during which the current was raised to over 180 kiloamps and the emission of large numbers of neutrons, up to 10^6 per discharge, was confirmed using both proportional counters and silver and indium-lined Geiger activation counters embedded in paraffin. But were they thermonuclear neutrons? Caution was in order; it was well known that neutrons from straight pinches in the USA and USSR were not. In their first internal report, dated 6 September, the Zeta team concluded that "at this stage, it is not possible to state whether or not the neutrons are of thermonuclear origin", and indeed it was not, as no reliable temperature measurements had yet been made[(205)]. Moreover, Pease, one of the key members of the team, had already recognized after the first deuterium runs in mid-August that even if neutrons were observed in large quantities, as they now had been, it would be very difficult and probably impossible definitely to prove them to be of thermonuclear origin[(206)]. Despite these doubts, however, the matter was immediately one for speculation, and the very high neutron yield prompted a substantial degree of optimism. On 4 September, on the eve of a trip to the United States, the deputy director of Harwell, Basil Schonland, was cautious. He stressed to Cockcroft the need to play down the results, and especially to keep the Americans away from the experiment until the team had had time to confirm that the neutrons were indeed thermonuclear and not produced as in the Kurchatov experiment, which seemed unlikely, or from wall effects, which he anticipated might be more difficult to disprove[(207)]. Hours after his arrival in America on Thursday, 5 September, he cabled Cockcroft with a further warning, mentioning that neutrons had been observed in American laboratories and urging that any press release be delayed at least until after the British and American teams had had a chance to meet and discuss the Zeta results at a forthcoming Sherwood conference in Princeton in October[(208)]. But on the very same day Cockcroft wrote a note to Plowden reporting the production of neutrons and stating that it was not yet 100% sure that they were thermonuclear in origin, but implying that the probability of their being so was nevertheless very high[(209)]. The following Monday Plowden wrote to the Prime Minister, Harold Macmillan, drawing the inference and stating that according to Cockcroft the likelihood that the neutrons were of thermonuclear origin was high[(210)].

Meanwhile the question of whether to issue a press release had been complicated by a realization that the news of the Zeta neutrons had been leaked. They were mentioned in press reports following discussions at the British Association meeting in Dublin. On the morning of Friday 6 September, just one week after the first detection of neutron counts, there was a session with the anticipatory title (supplied by the conference organisers) "Industrial applications of thermonuclear reactions". This consisted of two papers, "Nuclear fusion as a possible source of power" by Lawson and "Controlled thermonuclear reactions" by Sir George Thomson(211,212). Cockcroft had been asked to suggest the speakers. Thomson was an obvious choice; Lawson, no longer working in the field, was presumably chosen because of the studies that he had made earlier; these could form the basis of a very general lecture without any reference to experimental work. The two papers had been carefully co-ordinated, and written before the start-up of Zeta. By the time of the conference the existence of Zeta had already been announced, but no details given. The lecturers expected tough questioning, and Lawson was instructed not to give away any details, in particular the temperatures reached or the fact that neutrons had been detected. It seemed, however, to be widely expected that an important announcement of the Harwell results might be made; Professor Blackett of Imperial College was quoted in the **Irish Times** as saying as much.

The lecture room was crowded, and included among others Dr Bhabha and the Irish prime minister; the proceedings were broadcast on Irish radio. Lawson, aware of recent optimistic speculations in the press, was anxious to strike a cautious and realistic note. Emphasizing the stringent conditions for a positive energy balance he stated that "the problem of raising a gas to a sufficient temperature for thermonuclear reactions to occur, though difficult, is trivial compared with that of devising a system in which there is a net power yield". Thomson, who spoke second, ended his talk on a more optimistic note, stating that he had sufficient confidence in the ingenuity of electrical engineers to believe that since no fundamental reason had been found in ten years which made a fusion reactor impossible, this amounted to proof that it could be made. The talks were followed by questions, many of which could not be answered for reasons of security, and then by a press conference(212). Thomson, as the senior speaker, bore the brunt of the questioning, much of which was strongly critical of the secrecy surrounding the work. He gave it as his opinion that 15 years was the shortest time in which a reactor might be built, and Bhabha, who had raised the subject two years earlier at the Geneva "Atoms for Peace" conference, re-iterated his belief that controlled fusion reactors would be a source of power in less than 20 years. Lawson was more cautious, emphasizing the "enormous gap" between detecting thermonuclear reactions and building a system with net energy gain.

The subject was widely reported in the press the following day. In many cases caution was thrown to the winds, and many inaccurate, exaggerated and wildly optimistic statements were made. Among the more sober were reports in **The Times** and the **Financial Times**. The writer of the latter was aware that neutrons had been observed, though these were not mentioned openly at the meeting or the press conference. Under the headline "Harnessing H-Power for Industry. Harwell experiments successful" it was claimed that Zeta had been producing neutrons since mid-August, and that "some of these, UK scientists are confident, are due to the fusion of hydrogen atoms"(213). At this time the Zeta team had still not made any accurate temperature measurements, but the newspaper reported that:

> Although the results of the first experiments are still being analysed mathematically, UK scientists feel confident that some of the neutron atomic particles produced during the experiments must be produced by hydrogen fusion owing to the operating temperatures achieved.

The **Financial Times** did not disclose its sources, but the same morning the **Telegraph and Morning Post** reported Lawson as saying at a press conference at the Dublin meeting that the results from Zeta were "reasonably encouraging", and that the operating temperature of Zeta was apparently about 2 million degrees Centigrade - in fact a theoretical estimate rather than a result of any measurement(214). More sensational was a **News Chronicle** story under the headline "H-men are told: Don't let Zeta get too hot"(215). Here Thomson was reported as saying that large scale thermonuclear power would probably be achieved in about 15 years, and that the Zeta team would have to be careful that their machine did not run amok and turn into an H-bomb instead of a research instrument. The latter report would seem to reflect a journalist's question rather than any serious concern on Thomson's part, but the former suggests that he, perhaps even more than Cockcroft, was extremely optimistic about the Zeta results.

So far as an official press release was concerned, the Atomic Energy Authority was effectively tied to doing nothing without the approval of the United States Atomic Energy Commission, and this need for American agreement was stressed by Macmillan(216). But at Harwell Thonemann believed strongly that the newspaper reports necessitated some sort of official announcement, and Cockcroft backed him up(217). Accordingly work began at Harwell on the preparation of a draft press release, and the first draft, which was cabled to the British Embassy in Washington on the Monday, reflected a combination of caution and optimism:

> Whilst the available evidence suggests that thermonuclear reactions are occurring through the hot gas, reactions caused by deuteron collisions with the walls of the vacuum vessel, or by some internal accelerating mechanism, cannot be excluded. This is the first time (as far as is known) that ionized deuterium gas has been maintained at extreme temperatures, estimated at four million degrees C., for a time adequate for detailed scientific study.

It would appear that Cockcroft's response to suggestions of caution in claiming thermonuclear reactions was to shift the emphasis of the claim to the achievement of high temperatures, even though these were themselves as yet unproved(218). The temperature estimate was based on Doppler width measurements for oxygen and nitrogen ion lines and was quite impressive. But it was recognized that the interpretation of the measurements was rather uncertain. There was no guarantee that the temperature of the impurity (oxygen) ions was the same as that of the background gas, and there was a possibility that the Doppler widths might be due to small scale motions of the ions rather than to their thermal velocities.

The immediate American reaction to the proposed British press release was troubled. Arther Ruark, professor of physics at Alabama and in overall charge of the AEC Sherwood project, was hurt by the priority claim in respect of the operating conditions achieved, which he did not believe to be true, and sceptical of the British claims in general. The general feeling was that if the British were to release their statement then the AEC would have to publish a parallel announcement. As Willis reported from the British Embassy, a draft American press release, prepared by Tuck made the British look rather hasty, by referring to similarly high temperatures in American experiments but emphasising the difficulty of proving that thermonuclear reactions were being produced. Two days later, he reported serious doubts among the principal American fusion experts as to the supposed thermonuclear origin of the Zeta neutrons, together with a diplomatic concern that media probing following a press release might expose uncertainty behind the British claims(219). He also reported that Ruark now wanted any announcement by either country to be delayed until the Second Geneva Conference, a full year away, unless either the thermonuclear origin of neutrons could be proved beyond doubt or some success were announced by the Russians. It was by now clear that the Americans were going to do their utmost to delay any British press release(219). And although Plowden emphasised, in a personal message to Strauss, that the pressure from the British media made it difficult for him to hold back indefinitely, he agreed to the postponement of any announcement until after the Princeton Sherwood meeting(220).

In Britain, such a postponement had come to be seen as inevitable, and had also been generally recognized to be wise, in view of the need for firmer evidence. But there was still a suspicion that the Americans might have an ulterior motive. Thonemann felt, and others agreed with him, that the Americans were delaying a British announcement so as to catch up with the British team and save themselves the embarrassment of having been beaten in the fusion race. He suggested that the British should respond to this by concentrating hard on providing firm evidence for the thermonuclear origin of the Zeta neutrons, and should then publish their results, with or without American approval, in **Nature**(221). This strategy was an attractive one, for it meant that any American claims to have got as far as the British would themselves have to be substantiated in the scientific literature, which Thonemann and others believed they could not be. The new results coming out of the Zeta experiments were, moreover, promising, and it seemed that the establishment of the thermonuclear origin of the neutrons might not perhaps be too difficult. Following their initial run of experiments, the Zeta team had stripped and reassembled their apparatus, and on 17 September they were able to report their first informal estimates of the temperature achieved in a new series of runs(222). The new results showed that some of the observed neutrons were produced by a transient voltage spike, (later called an explosive instability), at the end of the current pulse. But they confirmed that 10^5 neutrons per pulse were produced at the current maximum, by then about 150 kiloamps. Estimates of ion temperature from the Doppler broadening of impurity lines suggested that this was between two and five million degrees C. Estimates based on the observed resistance and pressure balance, and electron temperature measurements using plasma probes, all seemed consistent with temperatures above 1 million degrees. The containment time could not be measured directly, but streak photographs of an equivalent helium discharge, where, unlike in deuterium, the radiation is not swamped by that of impurities, indicated a value of a few milliseconds. By making a rough estimate of the ion density it was possible to show that the temperature that would be theoretically required to produce the observed neutron flux through thermonuclear fusion was about two million degrees, consistent with the estimates of actual temperature obtained. The new results were still very provisional, especially in respect of the electron temperature, estimated from the plasma resistivity. But they did provide further cause for optimism. Cockcroft reported to the AEA on 19 September that the results were "most encouraging", and that there was now a "strong probability" that the neutron yield was thermonuclear in origin(223). Plowden took this as sufficiently final to offer his formal congratulations to the Zeta team on their achievement(224). Taking up Thonemann's suggestion, Cockcroft proposed that the stage had already been reached where a paper for a **Nature** could be written, and made arrangements with the editor of the journal for the prompt publication of such a paper as soon as it could be released, following ratification by the AEC of the new classification guide(225). The word from America, however, was that if Britain wanted ratification of this guide then both Cockcroft and Plowden would have to use all their influence and push very hard indeed. And when Cockcroft's proposal was put to the Americans their reaction was strong(226). Ruark took the view that if the British published anything, then the Americans would publish *more*. More generally the Americans continued to press for delayed and simultaneous publication, and insisted that the new guide would not be ratified until November at the earliest.

Measurements on Zeta continued, at first with the original apparatus and later with modifications introduced, including improved insulation between the sectors of the liner(204). The results continued to be very encouraging, and the Doppler measurements in particular provided sound and consistent evidence of high ion temperatures. But while all observations continued to be consistent with the hypothesis of a thermonuclear neutron source this could not be proven, and during October Fry reported to Plowden that he still had reservations about making any strong claims(227). Meanwhile, the political significance of the results was increasing dramatically. At the beginning of October the Russians launched their Sputnik satellite, well ahead of the Americans whose intended first satellite blew up on the launching pad in December. Also in early October one of the AEA's plutonium production reactors at Windscale caught fire during a Wigner energy release, with the consequent escape of substantial quantities of radioactive gases(228). The Windscale accident was the first really bad publicity the British atomic energy programme had received, and it naturally increased the pressure on the AEA to make an early announcement of their supposed great success with Zeta. For the Americans, on the other hand, the news of Sputnik was devastating. Ever since the last war, Americans had assumed almost without question that they were well ahead of the Russians in all things technological and military. Earlier in the year, however, some first doubts had been cast on this assumption by the leaking of the Gaither report, which speculated that the Russians might have the nuclear capacity for a single strike military victory over the United States as early as 1959, and by Kruschev's apparently militant "bury you" speech of June. The Soviet triumph with Sputnik opened the floodgates of fear. The father of the American H-bomb, Edward Teller, pronounced on television that the United States had lost a battle "more important and greater than Pearl Harbour"; and prominent politicians throughout the country expressed their concern that control of space could well mean control of the future, and that if America did not quickly do something dramatic the world battle for ideological supremacy would be lost. From Britain, Macmillan observed that:[229]

> The American people are no longer confident that even their great country can do everything itself, without allies, to secure its own survival. .....I say without hesitation and without excuse that this is a real turning point in history. Never has the threat of Soviet communism been so great.

The political implications of Sputnik were complex. Within the atomic energy sphere in general Macmillan's concern was to press for an end to America's refusal to collaborate with Britain on atomic weapons. So far as fusion was concerned, on which there was already collaboration, the Americans found themselves in the awkward position of needing the Zeta success as a Western response to Sputnik, but desperate to avoid this being seen solely as a British success, which would only deepen the wound to the American ego already caused by Sputnik. This last point was brought home in an article by Chapman Pincher in the **Daily Express** in mid-October, entitled "Britain wins the H-race". Pincher claimed, with nationalistic emphasis, that "the British proof that thermonuclear (H-bomb) power is controllable means that it will certainly be available for this generation"[230]. The following week the **Financial Times** asserted that many of the Zeta neutrons "have, it is now known, been created by the successful achievement of controlled thermonuclear fusion"[231]. Again it was claimed that "it now seems certain that between 1967 and the early 1970s commercial thermonuclear power stations will be supplementing today's "conventional" nuclear power stations".

In this setting of strong claims by the British media, discussions between the AEA and the AEC continued. At the Princeton meeting in mid-October a joint publication of results was agreed upon and a new draft British press release, agnostic as to the neutron production mechanism but claiming temperatures of between two and five million degrees, was put forward[232]. But by the end of the month the new classification guide had still not been ratified, and the AEC continued to insist that the British were "not on sound enough ground to publish"[233]. Midway through November they were still pursuing this line hard, and backing it up with the claim that they had similar results to the British ones, but did not consider them ready for publication[234].

It was recognised in Britain that there was some force behind the first American complaint, that the British results were not yet good enough to publish. As analysis of the experiments progressed and the difficulty of confirming the thermonuclear origin of the neutrons became ever more apparent this particular claim was no longer pursued. It was not included, for example, in the draft article for **Nature**, written in November, and the draft press release was also rewritten so as to avoid its implication[235]. It was also well known that even the temperature figures were only approximate estimates. But Cockcroft, Schonland and Thonemann all appear to have been convinced that whatever the limitations of the British results they were at least well in advance of those so far obtained in America, and that the American blustering was largely if not entirely motivated by self-interest. The British team was still prevented from publishing its results formally, but in a statement to the House of Commons on 11 November it was announced, in reply to a question, that while experiments were going on to identify the source of the Zeta neutrons, and while these could possibly be due to non-thermonuclear processes, they "probably" arose from thermonuclear reactions(236). And on 20 November the American Washington Post published a provocative statement attributed to one of Cockcroft's closest Harwell colleagues:(237).

> Britain is ahead of the United States in harnessing the colossal power of the Hydrogen bomb for peaceful purposes because it started work on the project three years earlier, Doctor J V Dunworth, head of British Reactor Research Unit, said today. Scientists at the Harwell Atomic Research Centre indicated Russia also was ahead of the United States in developing controlled thermonuclear processes.

In response to a furious telegram from Strauss, Plowden claimed that Dunworth categorically denied having made the statement, and that this was confirmed by the President of Institution of Gas Engineers, at a meeting of which the information was supposed to have been given(238). But so far as the supremacy of Britain was concerned, if not in respect of the more hurtful reference to the Russians, the view attributed to Dunworth reflected widespread feeling on both sides of the Atlantic that something politically sensitive must lie behind the non-publication of the Zeta results. Plowden agreed immediately to a request by Strauss for a joint press statement rejecting the comparison between their two countries' projects, but this carried little conviction(239). A few days later, in response to another question in the House of Commons, Lord Hailsham, the minister with responsibility for atomic energy, stated that he had "no reason to believe we are not leading the world in the experiments we are conducting"(240). Another question, tabled for 10 December and asking to what extent any British success had been denied publicity by the American policy on declassification, had to be diplomatically evaded(241).

With publicity such as this it was beginning to be in the American as well as the British interest that something should be published officially, and in late November the AEC finally decided to go ahead with the new classification guide and, before doing this, to send over one of their senior scientists, Stirling Colgate, to spend a week at Harwell to see their work for himself(242). A few days later it was decided that Colgate should be joined at the end of his visit by Ruark, Spitzer, Snell and Tuck, representing most of the main figures in the American project. On 7 December the expanded team arrived at Harwell to see the Zeta work and discuss the publication of results(243). As was to be expected, the British scientists, backed up by their politicians, pressed for immediate publication, while the Americans argued for a further delay(244). A compromise was however agreed, according to which a number of articles would be published in the scientific press as soon as was practicable, a date in mid-February being suggested, with ensuing silence until the Geneva conference in September(245). Following this agreement the new classification guide was duly ratified on 12 December(246).

Ironically, it was on this very same day that Anthony Nutting, considering the wider implications of Sputnik, wrote in the influential **New York Herald Tribune** of the "concealment of the triumph of Harwell" due to the "slavish and misguided application of security" by the Americans(247). Four days later, Chapman Pincher made the same charge in the British **Daily Express**(248). At the same time, the American broadcasting station WVNS, reporting jubilation among British fusion scientists who were now supposed to be far ahead of both America and Russia, claimed that these scientists "have recommended that Britain holds off giving any of the results to the United States until Eisenhower promises to release every shred of atomic information that Admiral Strauss is holding back" - a complete fabrication that nevertheless captured something of the political situation(249).

Articles such as those just cited continued to cause embarrassment, and had publication of the Zeta results not now been agreed upon they could have had serious consequences(250). As it was, the question of precisely what to publish and when still had to be resolved. Drafts of the proposed British press release drifted backwards and forwards, Fry's attempts to avoid any implication that the neutrons were thermonuclear being countered by Cockcroft's attempts to encourage the inference that they were so(251). Publication of the scientific papers, including four from America and one from AEI as well as that from Harwell, was to be in **Nature**. It was set first for 7 February and then, after concern at the effect of unclassified papers on the subject due to be read at the Physical Society in America on 26 January, for 25 January(252). Argument as to whose names should be attached to the Harwell paper, in what order, and with what acknowledgements to the AEI team, Metropolitan-Vickers and the Americans, was finally settled by decree from Fry(253-5). The British press release, which eventually concluded by saying that "there are good reasons to think that (the neutrons) come from thermonuclear reactions", but that this "has not yet been definitely established", was issued on 24 January following a press conference at Harwell on the 23 January, and the announcement on the 22 January of Cockcroft's retirement as director of Harwell later in the year.(254,256).

With the publication of the Zeta results, the public confusion and speculation should have ceased. Despite the general heading "Controlled Release of Thermonuclear Energy" provided by the editor, the article in **Nature** made no claim whatsoever as to the possible thermonuclear origin of the neutrons, and the Zeta scientists at the Harwell press conference resolutely refused to be drawn into any rash statements on this issue. But the press release, in striking contrast to the scientific paper, gave the clear impression that the neutrons probably were thermonuclear. And at the press conference on 23 January, Cockcroft, to the astonishment of his colleagues, responded to a question that no one else would answer by saying that he was "90% certain" that some of the neutrons at least were of thermonuclear origin(254,257).

Cockcroft had always been more optimistic about the Zeta results than his Harwell colleagues, and the Harwell press conference would have made a considerable impact even without his declaration. The tone at Harwell was openly celebratory, and American journalists were very pleased with themselves afterwards for having "discovered", without being officially told, that the British team was well ahead of the Americans(258). But Cockcroft's rash statement, inexcusable for one with his experience of the media, ensured that the Zeta results would be both widely publicised and widely misconstrued.

The British press was universally enthusiastic. There was pride that this was essentially a British achievement. There was also admiration that the work had been done by a relatively small team of young men. The British public soon had the opportunity to see Zeta and its creators in an extended television programme which well conveyed the excitement of the occasion. As indicated earlier Zeta was seen as a satisfactory "answer" to Sputnik, and there was further satisfaction that Britain appeared to be ahead of the Americans. This situation is well conveyed in a cartoon from Punch, reproduced as Fig. 9. In its enthusiasm the popular press made many statements quite unjustified by the facts of the situation. Since water, the source of deuterium, is virtually free it was concluded that one might soon expect limitless supplies of cheap, or even free, electricity.

Fig. 9 Cartoon from "Punch", 29 January 1958.

Coverage in the foreign press was also extensive; in Italy for example, the British fusion success was given even greater prominence than the Russian Sputnik had been. Overall the reports ranged from the sober and accurate, such as in the **New York Herald Tribune** and **Le Monde**, to the wildly inaccurate and spectacular. Despite the simultaneous publication of papers and press statements from both Britain and America, the press focus, encouraged by the Harwell press conference, was almost entirely on the British work. This was seen almost everywhere as a great triumph, and the charge that publication had been held up by an American attempt to save face was widely repeated. Cockcroft's "90 per cent" was quoted in most accounts, only a few of which, notably in the French press, focussed on the

10 per cent chance that Cockcroft might be wrong. Exaggerated claims proliferated(259). **The New York Times** reported Tuck as predicting that fusion reactors would be useful for spacecraft propulsion. The latter newspaper also reported some strong claims by Ware on the achievements of the AEI laboratory. Following the initial runs with Zeta, AEI had requested permission from Harwell to convert their racetrack torus into what was effectively a small scale version of Zeta. This was approved and the conversion quickly made, and by the time results were published in January, the AEI torus Sceptre III was operating and producing neutrons(111,260,261). **The New York Times** reported that:(259)

> A team of British industrial scientists reported yesterday they had achieved in a small $28,000 glass-walled tube demonstrations of controlled fusion nearly as impressive as results claimed by British and United States agencies. Furthermore, the British industrial group reported through British Information Services here that their expectations of achievements in the next year were considerably above anything voiced in official analysis from either side of the Atlantic ...
>
> Dr A A Ware, 33-year old leader of the project, working with a torus ... called Sceptre Three, said that ... "In the larger model now being designed it is hoped that thirty to forty million degrees will be reached by the end of the year. The latter reaction will be with a mixture of deuterium and tritium and will, in fact, be approximately expected to be a "break-even" temperature".

The first discussion of the Zeta results in a public scientific forum took place at a special meeting held by the Royal Society on 5 February where a wide range of topics, including the experimental results on both Zeta and Sceptre, were discussed(261). Following the reaction to his earlier statement, Cockcroft was by now more restrained. But Thomson, who had himself contributed to the high public expectations of Zeta, remained optimistic. If the neutrons were not thermonuclear, he argued, then the temperature estimates must be wrong and there must be a non-thermal neutron source giving by chance just the same neutron yield as was predicted by the observed temperature measurements. He did not believe that this could be the case. This statement was hardly justifiable, however, considering the uncertainties in the temperature measurements. Curves given in the **Nature** paper(252) show temperatures between 2.4 and 5 million degrees over the current range 80-200 kiloamps based on broadening of the Oxygen V impurity line. A similar curve using Nitrogen IV gave values less by a factor of between 2 and 4. Calculations from the neutron yield agreed with the oxygen values at 5 million degrees but were closer to the nitrogen values at lower currents. An extended table in the paper presented at Geneva indicates that "the deuteron energy is likely to be intermediate between the values corresponding to 0.7×10^6 and 5×10^6 ^oK at currents between 140 and 200 ka"(146).

Several other interesting topics surfaced at the meeting. W B Thompson and R J Tayler presented stability calculations for a pinch with longitudinal magnetic field surrounded by a conducting shell. They showed that a plasma column created in the presence of a longitudinal magnetic field and within a conducting shell would be magnetohydrodynamically stable provided that the conductivity were high enough to confine the plasma current to the surface of the column.

This would be expected in practice only if the current could be set up very rapidly indeed, so that the magnetic field had insufficient time to diffuse into the plasma, ("skin effect"). Such a current sheet would separate longitudinal fields within the plasma from circumferential fields in the space between plasma and the walls. In Zeta the conductivity was not sufficiently high, giving mixed fields. What happened in this case was not clear. (This separated field configuration was later to form the basis of ICSE, as described below).

Another interesting observation reported was a scaling law, derived by R J Bickerton and H London, relating to the three toroidal machines described in **Nature** at the time of declassification. In descending order of size there were Harwell's Zeta, AEI's Sceptre and the Perhapsatron at Los Alamos. Initially it had appeared that the very long confinement time of Zeta had put it well beyond the others in performance. Bickerton and London showed that using the density-time product as a criterion, the performance of all three was at least in this respect equivalent(262). Zeta, of course, possessed the considerable practical advantages of a longer time-scale for experimental studies. Other work reported included the fast Z-pinch studies at AWRE, described in Chapter 7.

The question of the origin of the neutrons in Zeta was shortly to be resolved in an unexpected way. Basil Rose, a nuclear physicist working on the Harwell cyclotron, located in the same wartime hangar as Zeta, managed to get an invitation to the press conference on 23 January. It became clear to him that the question of the origin of the neutrons was considered as a vital one, and after the meeting he discussed the matter with his colleague A E Taylor. They realised that what was needed was a detector that was both very sensitive, and able to measure accurately the neutron energy. Since the thermal energy of the colliding deuterons was small compared with the centre of mass energy of 2.5 MeV acquired in the reaction, accurate measurement of their energy as a function of direction would be necessary. Rose and Taylor soon realized that a suitable if unconventional detector existed; this was the diffusion cloud chamber, built for cyclotron experiments by M Snowden. After being used at Harwell this had now just been sent to University College in London. Fortunately it was not yet in use there, and was quickly returned.

Together with E Wood they set to work vigorously in conjunction with the Zeta team to set up the cloud chamber and make the necessary measurements. By the end of March they had accumulated enough evidence to provide a valuable check on the source of the neutrons, and by the end of May they had reached some conclusions(263). What they found was that the mean energy of the neutrons emitted in a direction along the axis of the torus when Zeta was operated with the current flowing in one direction was not the same as that obtained with it flowing in the opposite direction. The difference was significant, and inconsistent with a thermonuclear origin for neutrons in a stationary plasma, in which case the energy spectrum would have been unaffected by reversal of direction. Further measurements aimed at locating the neutron source suggested that these were neither produced by interactions with the wall, nor with a source localized at the centre of the discharge, but they did seem to be consistent with the hypothesis that the neutrons were produced through collisions due to a beam of accelerated ions running in the direction of the current(264). The nuclear physicists suggested how such an accelerated ion beam might be created in Zeta, and to conclude that at least

94% of the neutrons observed must arise in this or in some other non-thermal manner, and not as a result of thermonuclear fusion. Their explanation was not tenable, however, but the fact that the neutrons were mainly of non-thermonuclear origin was firmly established(265).

These results were published in **Nature** on June 14(266). A month earlier, however, on 16 May, Schonland had made a statement at a press conference held in London(267). After outlining the findings of Rose's team he stated "The fact that ... the fusion reactions cannot be described as being *thermo*-nuclear in origin does *not* make the Zeta results less significant. In fact, the acceleration process responsible may well be of value". He also announced that "a more powerful machine, a successor to Zeta, is planned". This development was widely reported by the press, both after the press conference and again following publication of the **Nature** article. The tone was one of slight bewilderment and surprise, but most papers tried to express a positive viewpoint; **the Daily Express**, however, on 14 June reported that the **Nature** report "pulls no punches in revealing that the great Zeta achievement, lauded in Parliament, the newspapers, on TV, and at the Brussels World Fair, was unfounded". It continues with the telling statement "This is a great blow to British prestige, for the Zeta 'triumph' had helped to offset Britain's backwardness in Sputnik's research".

Many opinions on this situation have been expressed, and event today there is no consensus. A perceptive comment in the **Manchester Guardian** of 17 June on how such situations can arise is worthy of note:

> This may, however, be a suitable occasion to ask whether present disappointments could have been avoided. Thermo-nuclear research has always been conducted secretly, and the Harwell team has been a small one. It would be unreasonable to expect that its members could have anticipated every possible interpretation of their first apparent successes ... In huge research projects like that revolving round Zeta the day-to-day rubbing of shoulders with scientists of other specialities is the best safeguard of sound analysis and interpretation. This, after all, is why the universities are so excellent in pure scientific research. So it will inevitably be asked whether things might not have gone differently if the members of the Zeta team had been allowed to talk freely and informally to other scientists ... The Zeta affair must therefore take its place in the list of frustrations caused by secrecy in science. It is an especially bitter one, because there is no justification, however implausible, for the present restrictions. Sir John Cockcroft has said he can see no military use for controlled thermo-nuclear fusion ... Secrecy might still spring from a wish to beat the rest of the world to an important scientific advance, but the futility of such policies should by now be clear.

In retrospect, the fact that the neutrons, though arising from fusion, were not thermonuclear perhaps seems less important than it did at the time. High temperatures, and self-constriction of the plasma channel in a toroidal vessel, had been demonstrated. Furthermore, it seemed plausible that with a larger system with higher currents, gas densities, and confinement times, conditions approaching those expected for "breakeven" might ultimately be obtained. A contemporary comment made by the Soviet physicist L A Artsimovich at the 1958 Geneva Conference is of interest[268].

> The question of whether a given neutron belongs to the noble race of descendants of thermonuclear reactions or whether it is the dubious offspring of a shady acceleration process is something that may worry the pressmen but at the present stage in the development of our problems it should not ruffle the composure of the specialists. When the number of neutrons arising in a single discharge pulse reaches a value in excess of 10^{12} then all doubt as to the origin of this effect will vanish.

What was not always properly appreciated at the time, especially by the media, was the enormous extrapolation from the Zeta experiment to a realistic power plant producing more power than it consumed. This was demonstrated by the statement sometimes made at the time that Zeta's neutrons represented a step towards fusion power corresponding to that of Fermi's original reactor with respect to fission power. This is not a true analogy, since a fission reactor needs no input power to prime it, whereas fusion requires a very large investment of organized energy to heat and confine the gas before power can be extracted.

Despite the public disappointment regarding the neutrons, and the unfortunate entanglements with the popular media (greatly exacerbated by what, in retrospect, seems a mistaken classification policy where "strip-tease" revelation in stages of what was going on quite naturally gave rise to over-optimistric speculation), Zeta remains as a bold step towards the concept of a large torus with an inductively driven stabilized plasma column. Although experiments on Zeta continued at Harwell until 1968, and despite Thonemann's urging, this line of development was not carried forward at the Culham Laboratory, and the idea of a large toroidal discharge as a candidate fusion reactor remained out of the limelight there until the success of the Russian T-3 tokamak in 1968.

Before concluding discussion of the physics of Zeta and Sceptre, it is interesting to summarize the situation as seen by the original teams at the time of the Geneva Conference, written a few months after the discovery that the neutrons were definitely not thermonuclear. For the Zeta team this is best done by quoting the summaries of their presentations at Geneva[146,147]; from the first reference[146]:

> ZETA is a fully engineered apparatus in which currents of up to 200 ka have been passed through gas in a torus. This torus has a bore of 100 cm and a mean circumference of 1160 cm. The current pulse has a duration at half height of about 2 msec. The current waveform corresponds to a capacitor discharging into an inductance and resistance in series; the resistance being about one third of the value for critical damping. The apparatus can run for long periods at a rate of one pulse every twelve seconds.
>
> A basic condition for a thermal plasma in a gas discharge is that the drift velocity of the electrons shall be much less than their thermal velocity. In ZETA, where the average values are about 10^7 cm sec^{-1} and 5×10^8 cm sec^{-1} respectively, this condition is satisfied.

X-rays of 20 kev and upwards and D-D reactions due to non-thermal processes have been observed. If these processes are a result of runaway particles they account for only a fraction of the total current; i.e. 25 amp of runaway electrons and 1 amp of 17 kev deuterons.

Both inductance and magnetic search coil measurements show that the current channel is constricted into the centre of the discharge tube, and the stabilizing field is enhanced by a factor of about ten. The stabilizing field is not trapped by currents flowing only on the surface of the plasma and the observed distribution of magnetic fields has yet to be explained.

At peak current of 140-180 ka, the resistance of the discharge and the fluctuations of the magnetic field increase markedly as the stabilizing field is reduced from about 160 gauss. At very low fields the discharge often fails to strike in clean conditions.

Streak pictures taken with the normal stabilizing field indicate that the plasma is isolated from the walls for periods of up to 1 msec in nitrogen and in contaminated helium and deuterium. Under clean conditions, the magnetic field fluctuations, and the current and voltage transients, suggest that the plasma is by no means stationary.

Impurity ions in the channel have been observed spectroscopically to have energies up to 500 ev. These energies, which are much larger than the mean electron energy, may be accounted for by this motion of the plasma, both directly and as an ion heating mechanism.

The observed power input and the estimated maximum plasma energy (3NkT/2) suggest that the energy containment time Δt_2 cannot exceed about 100 μsec. The efficiency of ZETA as a thermonuclear reactor is indicated by the Lawson product $n_i \Delta t_2$, which is thus about 10^{10} cm^{-3} sec. This may be compared with the value of 10^{16} cm^{-3} sec required for a power producing thermonuclear reactor (D-D).

And from the second reference(147).

In attempting to elucidate the mechanisms in heating and confining plasmas and to determine their physical condition, a wide variety of diagnostic techniques is being developed and used. The most important physical properties and the methods of measuring them are summarized below.

Ion temperature, defined as the mean kinetic energy of the ions, is measured from the Doppler broadening of spectral lines of highly ionized impurity atoms. The contribution of mass motion has not yet been measured, and the influence of ionic charge on the measured temperature has to be ascertained. It has not yet been possible to measure the mean deuteron energy by measuring the nuclear reaction rate, because of the occurrence of non-thermonuclear reactions.

Magnetic probes are used to measure the sum of electron and ion temperatures, subject to the limitation that the probe itself perturbs the discharge, and the result is dependent on knowing the density.

Electron temperature is found from the resistivity of the plasma, by Langmuir probes, and by measurement of microwave noise. In the conditions in ZETA, experimental difficulties have so far prevented reliable results from being obtained. Measurements of bremsstrahlung and the relative intensities of spectral lines are under investigation, but these are restricted by lack of basic data on processes taking place under conditions very far removed from those usually studied in terrestrial systems.

Electron density has been measured by microwave transmission measurements; there is at present an upper limit of about 3×10^{13} cm^{-3} due to experimental difficulties.

Ion density can be measured by Langmuir probes but for the conditions in ZETA the technique is not sufficiently developed. Confinement and stability have been studied by magnetic probes and high-speed photography.

Collision processes, ionization excitation has been studied spectroscopically.

Nuclear reactions and non-thermal processes were detected and studied by means of the particles and radiations emitted.

Two popular accounts written before the neutrons were found not to be thermonuclear are of interest. The first of these, written in early 1958, which contains Cockcroft's statement at the press conference on 23 January, is given in a 24-page pamphlet "Facts about Zeta" published by the UKAEA(256). Another popular account, by John Maddox, then Scientific Correspondent of the Manchester Guardian is his enthusiastic 16 page pamphlet "A Plain Man's Guide to Zeta", published at the beginning of 1958(269).

Both these also make brief mention of the work at AEI, and the performance of Sceptre III. This again was described at Geneva, in a more broadly based paper which summarized earlier work at AEI, including for example, the "pepperpot" experiment already described in Chapter 7(111,158). The measurements on Sceptre III in general ran parallel to those on Zeta, but one interesting exception was to the measurement of ion energies by means of photographic plates. These were shielded from soft X-rays by very thin foils, and showed proton tracks from the D-D fusion reaction of energy up to 3 MeV; by measuring the energy distribution as a function of angle it was established that at least some of the deuterons were accelerated preferentially in the direction of current flow, and could not therefore strictly be termed thermonuclear(270). The Geneva paper contains no "Summary", but the "Conclusions" section consists largely of a discussion of the problem of determining the temperature, and finding values consistent with a thermonuclear origin for the majority of the neutrons. The final part of this section is presented below:

First, it is reasonable to conclude that both the Doppler broadening and neutron yields give upper limits for the true deuterium temperature. From this it follows that in some cases all of the neutrons observed are produced by non-thermonuclear processes. For example, at 25 kv and 600 gauss, the Doppler broadening indicates a temperature of 1×10^6 °K. The deuterium ion temperature may be greater or less than the electron temperature.

Finally, the inductive and capacitive energy remaining at peak current, as estimated from the oscillograms, shows that over half of the initial condenser energy has been dissipated in the gas by that time. Since for an initial pressure of 1.4×10^{-3} mm Hg this energy is sufficient to raise all the original particles to the temperature 3×10^7 °K, it must be concluded that most of the energy is being lost from the discharge or shared with impurities.

Details of the work on Zeta may be found in a series of over 100 internal "Zeta memoranda" dating from 1958(271), and the AEI work is contained in AEI progress reports(272).

CHAPTER 10

INTERLUDE: DECLASSIFICATION, REVIEW AND CONSOLIDATION

Following the publication of the **Nature** article in January 1958, progress towards the more general declassification of controlled fusion research was relatively smooth. It had already been agreed by the British and Americans that more information would be released at the second Geneva Conference on the Peaceful Uses of Atomic Energy in September, and as the American scientists prepared a large number of papers for this conference the AEC decided to aim for complete declassification by the time it opened, and approached the British AEA with this suggestion[(273)]. In Britain, Sir William Penney still had reservations about releasing material produced by the AWRE blanket studies group. This group, which was wound up two years later, was engaged in the theoretical study of fissile blankets placed around a proposed Zeta 2 fusion reactor to multiply the neutron production, and the study had been justified partly on military grounds[(274)]. By mid-August, however, the Americans had agreed to a British request to exclude this as not being fusion research *per se*, and following a final joint meeting the two nations were able to announce the declassification of all their controlled fusion research on the eve of the Geneva conference[(275)].

Meanwhile, the Soviet Union also opted for the full publication of its achievements at Geneva, and in the course of the conference a number of invitations were issued to British fusion scientists to attend conferences in Russia and visit the Soviet controlled fusion laboratories[(276)]. By the end of the year Cockcroft, Thonemann, Bickerton, Pease, Thompson, Robson and Harding had all visited Russia, and had supplemented the knowledge of Soviet work gained at Geneva by direct acquaintance with the laboratories. Additional information was given at Geneva by many other countries who had started up fusion programmes in the past few years, and there were also further visits during the year to the United States, notably by teams from AWRE[(277)].

In Britain itself, 1958 was not a year of great experimental fusion research, despite the optimistic predictions that had accompanied the Zeta and Sceptre results. Spurred on by these results a number of new programmes were set up at the universities, including one at Imperial College London, where P M Blackett had re-injected some of Thomson's old enthusiasm for fusion[(278)]. At AEI the team continued to develop their Sceptre torus, but without reaching any significant new results[(279)]. At AWRE, there was one advance of note, due to Fitch. In the course of their fast Z-pinch work the AWRE team had had a requirement for a very low inductance capacitor bank and Fitch, noting that the lower limit of the inductance was actually determined by the switches rather than the capacitors themselves, had decided to replace the usual single switch for the whole bank by separate switches for each capacitor, all connected in parallel. The problem remained as to how all the switches were to be closed at the same instant, but here Fitch's experience of firing mechanisms for bombs came in useful, and during 1958 he was able to plan and produce the first multiple switched capacitor bank in the world[(154,280)]. A proposal to build a much larger multiple switched bank than was yet needed was, however, shelved when the Z-pinch work was abandoned.

The dominant Harwell team found themselves so pressed with invitations to write and speak about Zeta, and so bombarded by new information from abroad, that they had relatively little time left for experiment[281]. 1958 was nevertheless a fruitful year of consolidation. The accumulated results from Zeta were analyzed in detail and written up for publication; the vast quantity of information being released on results elsewhere in the world was absorbed and considered. To give some idea of the amount of information involved, two full volumes of the proceedings of the Second Geneva Conference amounting to 840 tight quarto pages, were devoted to reports of fusion research, and many of these reports were little more than short abstracts.

So far as the British scientists were concerned, the most significant aspects of the information now available concerned the theoretical analysis of Zeta-type toroidal fusion devices, and the alternative approaches to the production of controlled fusion reactions being pursued in America and the Soviet Union[282]. In one sense the performance of the rival toroidal machines to Zeta was heartening. Although the initial excitement had been over the high temperatures thought to have been achieved, it had become gradually clear that high temperature, though obviously important, was at this stage of secondary concern compared with stability. At the time of Zeta's inception little was understood about the stability problem and how it might be solved. At the time that Zeta first operated, the effect of a longitudinal magnetic field in improving stability had been demonstrated experimentally, and it was known that a conducting toroidal vacuum chamber would support image currents that repelled the current channel and greatly reduced its direct interaction with the wall. It was not clear, however, just how stable the discharge would be. Reporting on the meeting held at the Royal Society on 5 February 1958, (described and referenced in Chapter 9) Alan Gibson stated that "the most significant of the results obtained with Zeta is that stability can be obtained in a toroidal tube", but opinion in general was more tentative.

By the time of the Royal Society meeting there had already been considerable activity on stability theory at Harwell. A central figure was R J Tayler, who had arrived early in 1955 after a one year stay at Princeton where he had worked with Schwarzschild on stellar structure. Although aware of Schwarzschild's work on stability, he did not know of his then secret interest in fusion. Once at Harwell Tayler set about applying ideal magnetohydrodynamic theory (MHD) to the pinch, and studied a wide range of situations in cylindrical geometry, having soon discovered that a full toroidal treatment was analytically quite intractable. He found that if the current flowed uniformly through the plasma the system was necessarily unstable, but there were some stable configurations with axial field and conducting walls if the current flowed entirely on the surface of the plasma column[283]. This was an idealization that required infinite plasma conductivity, and manifestly did not apply to Zeta. When the Royal Society meeting was decided upon, Flowers was anxious to have something new for Harwell theorists to report, and he asked Tayler to investigate whether a small but finite surface layer for the current flow would lead to stability. This was hurriedly done, and it was found to be still possible to achieve stability provided that the current carrying layer occupied less than about 10% of the radius of the plasma column. This result was presented at the meeting, but it was unfortunately not correct. This was shown by Rosenbluth[284] and Suydam[285], making use of the new and powerful "energy principle"[286], which led to the discovery of local instabilities not predicted in the earlier theory. Further work by Rosenbluth[284], and independently at Harwell by Laing[287], showed that it was nevertheless possible to find stable distributions with the axial current not confined to a zero thickness surface layer provided that the field component B_z along the axis of the plasma column was in the direction opposite to that outside the plasma. Theoretical work in the USA and USSR was closely parallelled by work at Harwell in all these developments. The original stability criterion found by Tayler had earlier been arrived at independently in the USA and USSR before full communication on these topics had been established[288,289].

Theoretical studies of stability continued at Harwell, and subsequently became a prominent feature of the new laboratory later set up at Culham. Many effects, such as finite plasma resistivity and finite Larmor radius of the ions, conspired to make the problem ever more complex, even for an "ideal" plasma in cylindrical geometry, where real effects such as charge exchange and wall bombardment were not taken into account. One fact was clear, these theoretical results seemed to place theoretical requirements upon the field configuration necessary for a stabilised toroidal pinch that Zeta quite clearly failed to meet.

These theoretical results could be viewed in either of two ways. It could be argued that the fact that Zeta possessed a degree of stability that could not be theoretically accounted for demonstrated simply that the theory was deficient. This was the viewpoint taken by Thonemann and some of the other experimental physicists and engineers in the Harwell fusion programme. It highlighted the fact that even the refined theory of the plasma was still naive in its assumptions, and it was backed up by reports from America where they were also unable to find any clear correspondence between theoretical predictions and experimental results[290]. It led to the suggestion that future progress should be sought along the experimentally proven lines of Zeta, and should not be guided by suspect theoretical considerations. The alternative viewpoint, at first adopted principally by the more theoretically minded physicists, was that while the difference between the theoretical predictions and the actual Zeta performance did indeed indicate a weakness in the theory in absolute terms, there was no reason to believe that this reflected upon the relative theoretical predictions, and no reason to assume that the instability predicted for the Zeta configuration would not become mainfest at higher energies. This viewpoint led to the suggestion that the next step should be based not upon the straightforward Zeta configuration, but upon that which was theoretically most promising.

By the Autumn of 1958 the first of these two options, a bigger and better version of Zeta, had already been studied to some extent through the design project for a proposed Zeta 2, initiated two years earlier. In preparation for a decision, attention therefore centred on the second option, and, as a focus, on the lecture series being developed at Harwell by R J Tayler, and considering in depth the theoretical developments (already outlined) of the past year or two[291].

So far as the Harwell team was concerned, alternatives to the toroidal geometry itself were never seriously considered. It was felt, quite reasonably, that this was the only line of attack through which they stood any real chance of achieving results ahead of, or even in conjunction with, the much more heavily funded American and Soviet teams; the alternative designs were nevertheless of interest, especially in so far as they incorporated innovations that might be applicable to toroidal work. The AWRE team, without any commitment to or accumulated experience of toroidal devices, and seeking an alternative line of development to complement the Harwell one, were particularly interested in reviewing the possibilities that were being explored.

Of the fusion devices discussed at Geneva, those not considered and therefore unfamiliar in the UK naturally excited a great deal of interest. The most interesting seem to have been the American Stellarator, the DCX and Pyrotron Mirror Machines, and the Soviet Ogra. The Stellarator, developed at Princeton by Lyman Spitzer, shared with Zeta the idea of an endless tube from which the contained plasma could not escape. There were, however, notable differences; whereas in Zeta the main magnetic field was produced by the current channel itself, with a weak axial magnetic field superposed externally to improve stability, the Stellarator concept relied upon a strong externally applied axial magnetic field, produced through a toroidal solenoid, as the primary containment. This had the advantage that it allowed in principle for continuous operation, but a disadvantage was that the magnetic field produced fell off in strength away from the central axis of the torus, with a resulting tendency for the ions to drift vertically to the wall. To counteract this effect the Princeton group introduced further modifications to twist the field lines so that they were no longer simply closed, and a current along them could counter the ion drift. In the first Stellarator this was done by twisting the torus itself into a figure of eight. But further investigation had shown that to get a stable discharge it was necessary for the amount of the twist, or rotational transform angle, to be varied in a way that could not be achieved by purely geometric means. The design had therefore reverted to a simple torus, but with additional helical windings carrying small currents in alternate directions and thus producing transverse magnetic fields and satisfying the theoretical requirements.

Compared with their own Zeta the Stellarator concept seemed unnecessarily complex to the British, with few apparent compensating advantages. But it did employ one interesting device with possible applications elsewhere. This was the "diverter", in which the outer lines of the magnetic field were forced, by subsidiary coils, to loop out of the torus altogether and through a baffle and heavily pumped side chamber. This was devised to prevent impurity atoms emitted from the walls of the torus reaching the hot plasma in the centre of the tube. The idea was that such atoms would quickly be ionised by collisions with electrons and would then be constrained to move along the outer lines of force, eventually striking the baffle, becoming neutralized, and being pumped away. According to the evidence available this device, potentially applicable to all toroidal machines, seemed to work quite effectively at least at low particle energy and low density.

Another machine concept of major interest was quite different from either Zeta or the Stellarator, in that it did not entail a closed system of magnetic field lines. Instead the aim was to confine the plasma in a linear device by using the "magnetic mirror" effect, in which a particle is reflected when passing from a region of weak to strong magnetic field. This may be explained as follows. In a uniform magnetic field, particles move in helical orbits. If the field now increases in the direction represented by the axis of the helix, then for adiabatic changes it is readily shown that the perpendicular kinetic energy increases, and the ratio of perpendicular kinetic energy to total kinetic energy is proportional to the magnetic field. As the particle moves into a higher field, therefore, its forward energy must decrease; when the field becomes high enough the forward velocity becomes zero and the particle is reflected. The simplest form of mirror machine consisted of two mirrors facing one another. Such a configuration can be produced by two coils, as shown in Fig. 10. Once trapped, the particles bounce back and forth and collide with one another. Such machines, however, have two rather basic problems. The first is how to inject particles in the first place, and the second is how to stop the particles near the axis which are scattered in directions almost parallel to it from escaping; clearly a particle moving actually along the axis will escape, and there is a range of position and direction near this which is not contained.

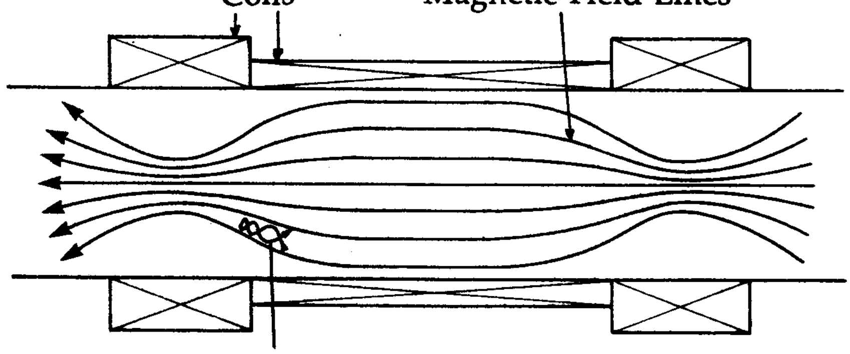

Fig. 10 Schematic diagram of coil configuration, magnetic field lines, and trajectory of a reflected particle in a magnetic mirror (adapted from ref. 292).

Two groups in America and one in Russia were studying such machines. One group was at Livermore under the direction of R F Post, who had experimented with the injection of plasma at low temperatures and through a low magnetic field, then very quickly raising the field in order to compress, heat and contain the plasma. As yet this procedure could be carried out only on a very small scale, with the volume and density of the plasma orders of magnitude too low for useful thermonuclear fusion to be possible. Nevertheless, at very low density, temperatures and containment times far in excess of any achieved elsewhere were demonstrated(292). Two other devices incorporating magnetic mirrors, the DCX at Oak Ridge in the United States and the Soviet Ogra, used an alternative technique for injection and containment(293,268). In these experiments, beams of high energy molecular ions (D_2^+) produced in particle accelerators were injected into the machine and then dissociated, in Ogra through collisions with the residual gas and in the DCX by being passed through the column of a carbon arc. The charge-to-mass ratio of the injected molecular ions was such as to allow them to penetrate the magnetic fields; that of the dissociated ions was reduced, giving rise to more tightly coiled particle orbits and containment within the machine. This approach still had a very long way to go, and an order of magnitude improvement in both the injection energy and the background gas pressure was thought to be necessary before a plasma could even be formed, let alone heated to ignition point. But P R Bell, in charge of the experiments at Oak Ridge, had an infectious enthusiasm and optimism and managed to create a very favourable impression, especially on a visiting AWRE team.

A further concept, relying on a fast field rise and compression of the plasma to a high density, was the theta pinch, typified by the Scylla programme at Los Alamos[295]. In this device the current flows around the axis of the tube (which may be straight or toroidal) rather than along it. This current is induced by a rapidly rising pulse of current in single turn coils around an insulating tube containing pre-ionized plasma. This "coil" can indeed consist of a single copper tube with a longitudinal slot, fed by a pulse generator shown schematically in Fig. 11. Currents in the coil and the plasma are in opposite directions, so that they repel and the plasma is compressed and heated.

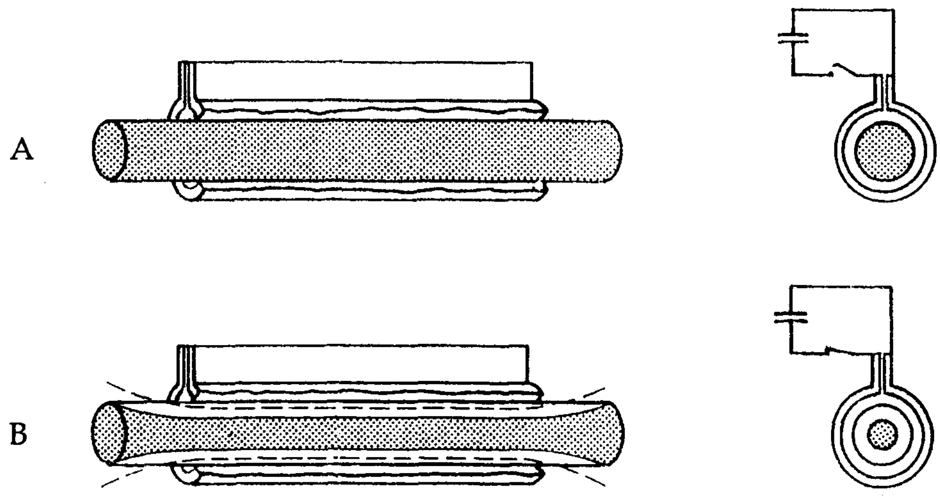

Fig. 11 Schematic diagram of thetatron, adapted from ref. 156. A single turn coil surrounds an insulating tube full of gas, (A). When the circuit is closed (B) a current flows through the coil; this induces an oppositely directed current on the surface of the plasma column, which drives the plasma radially inward. The magnetic field is shown dotted.

The description of the various devices given here is very sketchy. For more details of the situation as perceived at the time, the reader is referred to the 1958 Geneva Conference Proceedings, and the excellent text published in 1960 by Glasstone and Lovberg which sets out very clearly the basic concepts of fusion, and describes the various devices and theoretical ideas revealed in Geneva[296]. Many contemporary accounts of the ideas revealed at Geneva may be found in the literature; some of these are listed in ref. 282.

CHAPTER 11

A TIME FOR DECISIONS: FUSION AT CERN? PLANS AT AEI AND AWRE

The concepts, theories and possibilities outlined in the previous chapter combined with the British experience, predominantly with Zeta, to provide the foundations for a number of decisions that had to be taken around the end of 1958. So far as the main British fusion project was concerned, decisions were required as to what the Harwell team should do as a follow up to Zeta and, as this team threatened to outgrow its accommodation, where it should do it. With the abandonment of the fast Z-pinch project a decision was also needed as to what path the growing AWRE team were to pursue. And as the scale of operations increased the future role of the AEI project also had to be determined. As things stood this was still being funded by Harwell, but its importance in respect of the overall Harwell programme seemed to be diminishing. Finally, as a background to all these specifically national issues, developments over the past two years had in addition left the British scientists involved in a European initiative, the future course of which had to be resolved(297).

The first suggestion of a joint European controlled fusion project reached Britain in March 1957, when Cockcroft heard informally of the possibility that Euratom, the atomic agency of the European Economic Community, might request CERN, the European particle accelerator centre at Geneva, to undertake unclassified fusion research on their behalf(298). This threatened to place the British, who were members of CERN but not of Euratom, in an awkward position. Discouraging noises were relayed back to Europe, and nothing further happened for the time being(299). But once raised, the possibility of fusion research at CERN gradually caught on. Towards the end of 1957, in the wake of the first Zeta publicity, the view was put forward in Geneva that once the existing accelerator projects had been completed CERN would be left with a first rate electrical engineering design team with little to do, and that a move into the fusion research field, seen at the time as closely related technically to that of accelerators, would be a natural response to this situation(300). Then in March 1958 the Euratom proposal was revived formally, at a meeting of the CERN Scientific Policy Committee(301).

At the scientists' level, Thonemann had already expressed his personal support for some kind of European fusion centre, and had discussed his views with John Adams, who was in charge of the largest CERN project group, during a visit to Geneva timed to coincide with the March scientific policy meeting(302). Adams himself, anxious to retain the engineering team he had built up in Geneva, also supported the idea(303). He encouraged his staff to take an interest, and even organised informal discussion groups on fundamentals in his own home. Politically, however, the British response was less encouraging. First indications were that the proposal would double the size of the CERN establishment, and there was general agreement that Britain would not wish to contribute financially to such an increase, or to its administrative consequences(304). Cockcroft also doubted whether the current CERN administration, under Bakker, could cope with the expansion. Although he believed that Adams could manage this he was also anxious for Adams, who had originally been seconded from Harwell to go to

CERN, to return and take over the British fusion programme as it entered a large scale engineering stage[305]. There were also doubts in Britain as to whether a fusion programme would fall under the CERN terms of reference of fundamental nuclear research, especially in view of the openly industrial motives of the Euratom initiative[306]. Even Thonemann, while arguing that Britain should participate wholeheartedly in and if possible lead any proposed international collaboration, agreed that this should not be done at CERN[307].

In May, the Atomic Energy Authority opted for neutrality on the Euratom proposal, provided that it was restricted to fundamental research and financed entirely by the Euratom countries. But the issue was soon complicated by an agreement at CERN to begin by setting up a study group, and by a Foreign Office request to the AEA to go along with this, and with the Euratom proposals in general, as far as was possible[308]. It was clear that Britain would have to take part in the study group at least, and indeed that the British scientists with their advanced knowledge would be expected to play the central role in it. There followed a period of hectic activity within the CERN administration as the Euratom countries sought to extend the size and terms of reference of the study group while Britain, concerned about the time it would take up, and backed up by other non-Euratom countries, tried to limit its scope[309]. In the end neither the original Euratom proposals for a wide-reaching study group preliminary to an actual programme, nor the British alternative proposals for a more limited group independent of Euratom funding and without any commitment to an actual programme, were put to the vote. But the agreed compromise effectively accepted the British line of a limited study group for the time being, and such a group was duly set up[310].

The CERN study group held its first meeting in Geneva in late September 1958, and continued to meet regularly throughout 1959[311]. But despite continued pressure for a full scale collaborative programme from the Euratom scientists, and especially from the French scientists led by Kowarski, Adams's report as chairman of the study group in May 1959 proposed only a continuation of general and informal cooperation and collaboration, and ruled out the possibility of any actual programme at CERN[312]. The first attempt at a joint European fusion project, a concept finally realized in the JET project of the 1970s and 1980s, thus fizzled out.

Decisions were also required on the future programme at AEI. The tension between the Harwell and AEI teams had been growing throughout the preparation of the Zeta experiment, and in the early part of 1958, as AEI sacrificed their independent line of attack in favour of the Zeta-type tori Sceptre III and IV, the value of their contribution to the main project was called more and more into question. When their contract came up for renewal in the spring, approval of this by the AEA was first deferred and then given only reluctantly and subject to the reservation that financial support might be withdrawn at any time after March 1960[313]. Then in May 1959, Allibone wrote to Schonland proposing a new programme of work, and charging the Harwell team with inconsistency in having approved AEI work in the past, in particular on Sceptre III, and in having then gone on to ignore this work and duplicate the research themselves[314]. These charges appear to have had some substance, but they reflect a division rather than a conspiracy within the Harwell team, part of whom saw the AEI work as duplicating their own rather than the other way around. In January 1958, when the AEI future programme had been revised in the light of the Zeta results, agreement had been reached between the AEI team and Fry on a programme close to that being planned at Harwell. But others, including Flowers, Thonemann, Pease and Sir George Thomson, had disagreed with this and had made their disagreement known; and although the issue was for the moment resolved in AEI's favour, the resolution was not strong enough to safeguard AEI from future criticism[315].

Now, in response to Allibone's charges, Pease reacted with a strong attack on the AEI programme, which seemed to him more and more like a waste of effort. He had no criticism to make of the standard of work at AEI, which he thought was generally high. But he criticised the way in which decisions were reached on their programme, without consulting the Harwell scientists, and he suggested that their recent proposal was a thorough waste of time. If they were to continue to be funded by Harwell, he now suggested, it should be under direct Harwell control[315,316]. This attack left Fry very awkwardly placed, for the organisational factors criticised were effectively his responsibility. His response was to argue that whether Pease liked it or not the AEI work done so far had been effectively if not explicitly approved and supported, and that no criticism of AEI in this respect could fairly be made[314]. But recognising the strength of feeling behind Pease's argument he suggested that the time had come to reconsider the future role of the AEI team in the Harwell programme and perhaps to redirect their activity over the next couple of years to an area of greater potential benefit.

Fry's compromise seems to have been accepted for the time being, and in July a formal AEI proposal for a future programme was put forward. But then Sir William Penney, who took over from the retiring Cockcroft as AEA Member for Research at the beginning of July, turned his attention to the problem and apparently accepted the anti-AEI view. He declared himself prepared to support the completion of existing work but not to support any new work at AEI, and he wrote to Allibone to the effect that financial support from Harwell would cease altogether in just over a year's time, at the end of September 1960[317]. In general, this decision met with the approval of the main fusion group, but in one sense it went too far. Pease and others had attacked the programme organisation of the AEI project, but they had not asked for its prompt termination and did not feel that this would be either fair or justifiable. Pease himself now complained to Schonland that if the project were to be terminated there must at least be concessions, such as the continued rent-free use of AEA equipment, and a gradual run-down of the contract rather than the sudden cut-off proposed by Penney[318]. In response to such arguments Penney duly eased his position, and in October he agreed with Allibone to a continuation of the contract, but at a lower level and in respect of work aimed in a new direction[319]. The following year the contract was therefore renewed, and rather than contracting the AEI programme actually expanded over the next few years, mainly in respect of interferometer measurement techniques and a project on a toroidal "levitron" device, a straight tube version of which had been devised and explored by Colgate and Furth in the USA[320]. Just as the project was getting into its stride, however, internal problems in the AEI organisation led to the closure of the entire Aldermaston Court laboratory, and with it the AEI fusion project, in 1963[321].

There was need, too, to take stock of the Weapons Group programme. Up until mid-1958 the main AWRE effort on controlled fusion research had been devoted to straight Z-pinch investigations, but when the neutrons emitted from these were found to arise from currents near the wall rather than thermonuclear reactions in the main plasma the team looked around for an alternative line of investigation able to utilise the condenser bank, "Maggie", developed by R A Fitch(321a). Earlier in the year they had already pressed hard for permission to build a second Zeta, with the idea of supplementing the existing electromagnetic heating by shock heating(322). In April they had also argued that they should build the successor to Zeta, Zeta 2, leaving Harwell to concentrate on the fundamental physics(323). Both requests had been refused, however, for although it was considered that the establishment might need a largish civil project to hold the weapons team together in the event of a moratorium on weapons development, it was clear that the main thrust of the important controlled fusion project could not be entrusted to a team that might never exist at all, and might at any moment have to drop the project altogether in order to return to weapons work(323).

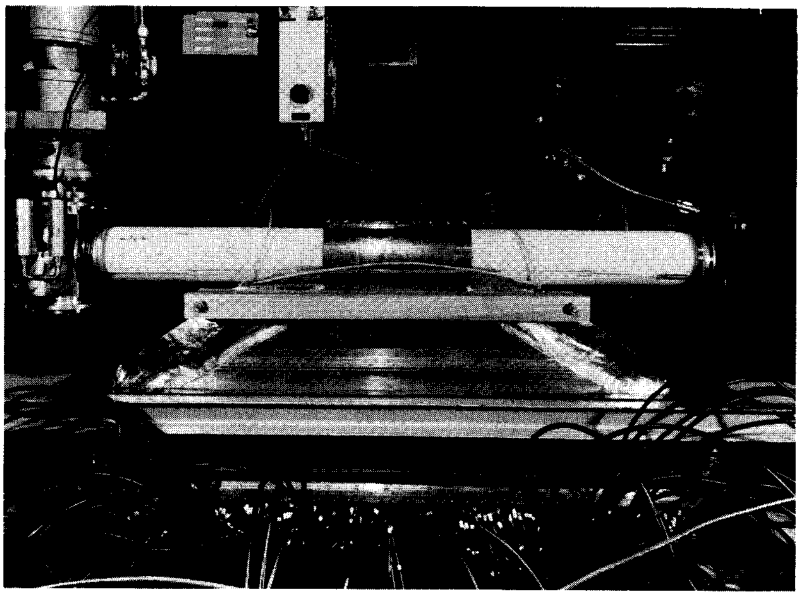

Fig. 12 The Oswald condenser bank and early theta pinch experiment at AWRE. The single turn copper coil, which encircles the industrial quartz tube, can be seen.

The AWRE decision on the project to follow their linear Z-pinch followed from their involvement in the Sherwood conferences and their other experiences of the American programme in the course of 1958. Unlike the Harwell team, that from AWRE looked naturally to the United States for a lead, and visiting the Oak Ridge laboratory they were much impressed by Bell's enthusiasm for the DCX machine. They also found the performance of the Scylla theta-pinch at Los Alamos very encouraging(156), and in July 1958 they decided in principle to investigate this approach(324). The theta pinch requires high voltage fast capacitor banks, and so was particularly suited to the AWRE experience gained earlier on their Z-pinch. Whereas the Harwell emphasis was on low density quasi-continuous systems, there was a developing world-wide view that very short time high density approaches should be investigated, and the experience of the AWRE scientists was such that this path was clearly the most attractive for them to follow. Later in the year they decided to go ahead with an experiment on these lines, which they called a Thetatron. There was some consideration also of a toroidal system, such a system is described in a proposal by G B H Niblett in August 1958(325), and also more generally in refs. 156 and 296. The decision was to build a linear system, however, and by the end of the year small scale experiments were under way. The situation at this time is summarized by Penney in a paper presented to the CTR advisory committee on 18 December(326). A proposal is made for expanding the experimental programme, with theoretical backing, using initially an existing 50 kJ condenser bank together with a new 25 kJ bank to be ready in a new building in 1959. This was accepted by the committee, and the programme went ahead as planned, under the direction of Fitch and Niblett(327). The condenser bank "Oswald" used in these early experiments is shown in Fig. 12 together with an early theta pinch tube.

A second proposal, also endorsed by the advisory committee, was for a mirror machine with high energy injection of neutral hydrogen atoms. These ions would be trapped by stripping with an internal arc or on background gas. Further work, particularly on determining the various relevant cross-sections, was required before a realistic experiment could be designed. By 1960 a wide range of cross-section measurements had been made, and "Phoenix 1", under the direction of D R Sweetman, was in operation using injection of about 1 mA (equivalent) neutral hydrogen in the range 20-80 KeV. Stripping was by collision with the residual gas background and trapped plasma, this was enhanced by "Lorentz stripping", in which the electric field experienced by charges moving in a strong magnetic field is able to remove an electron from an excited hydrogen ion(328).

CHAPTER 12

THE SUCCESSOR TO ZETA AND ITS LOCATION

Important as the decisions on the AEI, AWRE and proposed CERN projects were, they were secondary in comparison with that on the successor to Zeta. For it was on this decision that the British controlled fusion programme as a whole rested. To understand the situation prevailing when it came to be taken, in the winter of 1958-1959, we have to go back over two years to the summer of 1956.

Discussions on a possible successor to Zeta, known as Zeta 2 or by its Metropolitan-Vickers project number 972, had been initiated by Fry in July 1956. A series of meetings between the Harwell and Metropolitan-Vickers engineers had been arranged and an outline specification had been drawn up by Carruthers(329). The idea was to use the same basic design as Zeta, but to modify and enlarge it to take a toroidal current of 2 million amps and to aim at a pulse duration of one hundredth of a second. The stored energy was estimated at 35 MJ, but the torus dimensions were similar to Zeta. Shortly afterwards a fuller specification was produced jointly with Metropolitan-Vickers(330).

The organisation for the new project was to be based on that already in existence for Zeta, and preliminary discussions and investigations progressed smoothly throughout 1957. In September, in the wake of the first Zeta results, approval was sought to go ahead with the successor project at an assumed cost of £1.5 million, and later in the year a formal design committee was set up(331). By November 1957 Zeta 2 was seen as being "within striking range of a power producing reactor using D-T-Li^6 cycle with U_{238} blanket"(332). (Cockcroft shortly afterwards stated that Zeta's successor "will aim at achieving the break-even point")(333). Energy storage of 50 MJ was now envisaged, with mean power consumption of 7 MW. The toroidal current flowing in the plasma was unchanged at 2MA, with both ion and electron temperatures 5×10^8 degrees, and containment $n\tau = 4 \times 10^{13}$ sec/cm^3. The toroidal field B_z would be 0.5 Tesla and the circumference of the torus 20m. A construction time of 3 - 4 years was envisaged at a cost of £3.5M(332). Ambitious parameters were also given for Zeta 1', an upgrade of Zeta expected to reach an electron temperature of 10^8 degrees and containment $n\tau = 10^{13}$ sec/cm^3 at a current of 1 MA. The target date for this was January 1959. These optimistic figures reflect expectations before the measurements in mid-1958 which showed that the neutrons were not thermonuclear, and before serious concerns about stability had arisen.

At this stage, just after Zeta had started operating, there were no doubts about the project itself. Zeta looked to be remarkably successful and a bigger and better version of it seemed to be the obvious next step. But a problem had arisen over where to site the new experiment. The Harwell fusion team was already expanding rapidly to cope with Zeta, and the proposed new experiment would have been on a very large scale, requiring a staff complement of perhaps several hundred professional scientists and engineers together with an even larger supporting force. Arguments raged about the actual numbers required, and the division of work between the AEA and industry(334). There was already serious concern in the London Office of the Atomic Energy Authority that Harwell was becoming too big for effective management. There was also a feeling among the AEA administrators that a project such as the one proposed, on an effectively industrial scale, should not be sited at a fundamental research establishment such as Harwell. Indeed it had always been understood, unofficially, that this was one of the projects for which the Winfrith site, just then being opened up near the Dorset coast, was intended(335). In mid-December 1957 the siting of Zeta 2 was considered by the Harwell Council, and Cockcroft put forward the suggestion, which he attributed to Schonland, that the project should go to Winfrith. Fry, who had already accepted the prospective post of director of Winfrith, replied that it would be better for the project if it were to stay at Harwell, but he accepted that the move was necessary and it was finally agreed upon(336). Schonland, meanwhile, had already anticipated this decision and had arranged with the AEA Industrial Group at Risley that they should be responsible for the actual building of Zeta 2, at Winfrith(337).

No sooner had the decision to move the fusion work to Winfrith been taken, than strong opposition to this course was voiced on behalf of the scientists by Flowers(338). He argued that the whole success of the original Zeta had depended upon the active involvement of people originally outside the Controlled Thermonuclear Research (CTR) Division itself, that Winfrith was "not a place to which the best research physicists will want to go, or should go, or are likely to go". He argued that contrary to the impression held by senior management Zeta 2 would not be an industrial engineering enterprise but rather a physics experiment, albeit on a very large scale, and that if the fusion project were to be removed from Harwell then that establishment, which had already largely lost its role in the AEA reactor research programme, and from which large chunks were about to be moved to Winfrith, the Wantage Radiation Laboratory and the National Institute for Research in Nuclear Science, would be left without any role whatsoever. Flowers's arguments touched on a topic that was already a matter of serious concern, and to the discussion of which he had already contributed significantly. Cockcroft, who had been sympathetic to Flowers's position in the course of this wider debate, now responded by agreeing to have the question of fusion siting discussed on the Harwell Steering Committee, where the scientists were well represented(339). When this discussion took place in mid-January 1958, the scientists present, now supported fully by Fry, argued that the fusion team must be kept together and that Harwell would be by far the more suitable location for them, despite the drastic change in the balance of the establishment's research that might result(340). In a note written a week later, Flowers suggested that the changes to Harwell might in fact be rather a good thing, and that the site might be reorganized, preferably without any security fence, around fusion and solid state materials research. This, he suggested, would capitalize on its strengths and lead to its growth in stature as one of the world's leading centres for fundamental research(341). The senior Harwell engineers, however, Dolphin and Grout, still saw Zeta 2 as an industrial project with short term commercial implications. In their view there could be no question of its being sited at Harwell, and no consensus was therefore reached.

When the Zeta 2 proposals came to be considered by the Atomic Energy Executive in April 1958 the question of siting, though much discussed, was again left open(342). But at this meeting the AEA chairman, Plowden, insisted that whatever decision be made the existing Harwell staff ceiling must be retained. At a meeting with Cockcroft and Schonland in early May he again stressed that while recognizing the strength of the case for keeping Zeta 2 at Harwell he could not accept any increase whatsoever in the Harwell staff numbers(343). Cockcroft and Donald Perrott, the AEA member for finance, were asked to consider the problem, and when they reported back at the beginning of August it was to the effect that given this inflexibility on the staff ceiling there was no real option but to move Zeta 2, and with it the whole fusion project, to Winfrith. A decision to this effect was announced in a press release early in September(344), and the following month Thonemann was instructed to start making the necessary arrangements for the move.

The public announcement of the move to Winfrith was intended to close the siting debate, but even before it took place there were clear signs that the problems raised by Flowers and the other scientists might not be so easily removed. In mid-August Thonemann had written to Fry giving two years as an estimate of the time that would be lost by the move, stating that he personally would be "most reluctant" to join a project at Winfrith, and suggesting that other key members of the team would also drop out(345). Cockcroft's view, as given to the AEA the following week, was that a delay of one year might ensue from the move but no longer, and that the fusion scientists would have to be forced to realize that given the staff ceiling imposed on Harwell there was simply no alternative to the move to the South Coast. As Fry pointed out to him, however, the whole project rested upon the scientists, and it simply could not exist without their support(346). A month later, after the press release, Franz Mandl, who was about to leave Harwell for the United States, wrote to Cockcroft in much the same vein(347). He suggested that it would be very difficult to persuade the key people to move, and that a Winfrith-based project would also suffer severely from the lack of immediate contact with people on the fringes of but not actually in the fusion programme, such as Lawson, Marshall, Hubbard and London. This letter brought only a sharp retort from Schonland that the decision had been taken and was final, but a scientists' rebellion was already effectively launched. In late October Thompson argued on behalf of the theoreticians that working in Winfrith would be practically impossible. Ralph and Johns, both of whom were involved on the administrative side of the fusion programme, wrote a memorandum condemning the travel and accommodation arrangements at Winfrith, which was sent with a covering note detailing further objections to the move by Thonemann to Schonland(348). Meanwhile at a meeting of the Research Group Management Board, also in late October, at which the provision of new fusion project buildings at Winfrith was approved, Spence and Finniston, heads of the Harwell Chemistry Division and Metallurgy Division respectively, voiced their support for the fusion scientists(349). A special meeting of the Harwell Council held on 5 November revealed unanimous opposition among the Harwell scientists to the proposed move, with the previous dissenters being joined by all the leading fusion scientists and by other heads of divisions, including Bretscher of Nuclear Physics(350). A new crucial argument came from Marley, the head of the Health Physics Division. Pointing out that Winfrith had been designed as a remote site subject to stringent safety regulations he drew attention to the fact that, even though the fusion project would itself be quite safe, it and its large staff would have to be subject to all the restrictions and regulations of the site, thus producing a considerable and on the face of it quite unnecessary administrative burden.

Schonland's response to this onslaught was one of considerable confusion. Not convinced of the case for the move he yet felt obliged to defend it, and so ended up in an awful muddle. Fortunately, however, two developments offered a chance of a way out. One was a suggestion raised by Dolphin in a letter to Schonland of 4 November and put forward more forcibly a week later, to the effect that the fusion programme should be sited neither at Harwell nor at Winfrith but at a new site close to Harwell and to the National Institute for Research in Nuclear Science (Rutherford Laboratory)(351). This possibility had originally been neglected, largely because one of the reasons put forward by the London Office for the staff ceiling on Harwell had been that the local facilities could not accommodate any further growth. It had since become clear, however, that this was not a major obstacle, and that the real grounds for containing the size of Harwell were those relating to management efficiency. Dolphin still had doubts as to whether the Treasury would accept yet another new site, but the possibility did seem to offer a way out of the impasse that had been created between the needs of the scientists and the Harwell staff ceiling. Moreover, after Marley's intervention, it seemed that it might in fact be no more expensive to build a new site than to accommodate the fusion team at Winfrith(351). While all this argument and negotiation had been taking place the design of Zeta II was becoming more complex and the cost was escalating. In a paper to the AEA by Cockcroft entitled "Proposals for Zeta II" the temperature and containment product $n\tau$ were unchanged, but the diameter was now 6 metres, the pulse length 0.5-1 second, the stored energy 50 MJ, and the toroidal B_z field $\frac{1}{2}$ T. The cost was now £5M, and staff estimated at about 100 professionals and the same number of ancillaries(352).

A more radical change in design was to follow; to avoid excessive bombardment in the early stages of the discharge a very fast current rise was proposed by Bickerton, necessitating a field E_z of 500 V/cm, an inductive energy store of 200 MJ, and a current of 7MA(353). A new type of power supply, based on a concept of Allen and Bickerton, was suggested(354). This very fast heating was also advocated as producing a more stable configuration. At the same time the theoretical analysis of the stabilized pinch and the assessments of alternative geometries had emphasised the lack of certain knowledge and had led to the conclusion that it might perhaps be premature to go ahead with Zeta 2 as planned. Instead, as explained later, it was proposed to concentrate attention first on a large power supply incorporating 25 MJ of fast capacitors to give a fast collapse, together with 200 MJ of inductive energy storage to raise a current for a few milliseconds, and a battery store to hold it for up to a tenth of a second. The specification was in line with the new requirements for Zeta 2, but it was intended to be sufficiently flexible to be coupled to any large fusion device, the choice of which was to be postponed pending further investigation. It represented a very substantial technology development programme. Technical details of the Zeta 2 design may be found in ref. 355.

The changing concept of Zeta 2 gave Schonland the opportunity to reconsider the move to Winfrith, and at a meeting of the Research Group Management Board on 18 November it was argued that in view of the uncertainty of the new concept, and of the fundamental nature of the research need to clarify the situation, the project could no longer be viewed as being essentially an engineering one. Thus the Winfrith site could no longer be viewed as being appropriate for it(356). What should be done instead was yet to be decided, but the argument of the scientists, that the future of the programme rested on close contacts with the Harwell divisions and with university departments, was now accepted. At the Atomic Energy Executive later in the month Plowden reluctantly accepted the changed situation, and the move to Winfrith was finally abandoned.

Plowden's insistence on the preservation of the Harwell staff ceiling did not change, and nor did his concern at the overall growth of the Research Group. But with the proviso that Harwell, Winfrith and any new site should be put under separate managerial control he accepted Dolphin's suggestion that a new site in the Harwell area should be sought. Such a suggestion had by this time already been put forward, to make use of Culham airfield, then owned by the Admiralty, just 6 miles from Harwell. Approaches to the Admiralty revealed that they would be prepared to transfer it(357), and despite a natural concern that the site might be slightly too near to Harwell and would thus stretch the local amenities (the Harwell secretary, Le Cren, was of the opinion that the choice was based on the desire of the scientists to avoiding moving house)(358), and despite pressure from the local council to look at alternatives to the North of Oxford(359), it was eventually agreed that the Culham site would be best(360). The decision to buy it was finally taken in May 1959(361).

Meanwhile, an explanation of the change of plan had to be prepared for presentation to the public. This was no great problem, as the loss of confidence and retreat into fundamental physics were not peculiarly British phenomena. The American teams were also going through a pessimistic phase and apart from continuing work on the large Model C stellarator were beginning to concentrate their own attack on fundamental physics rather than on the immediate construction of bigger machines(362). What had to be explained was thus the change from the earlier optimism rather than the current realism itself. To this end the results of the Geneva conference were presented as absolutely crucial to the new decision, and as having indicated that all the countries engaged on fusion research had unexpectedly reached a barrier, the penetration of which was assured in due course but could not be achieved without further fundamental study. The need to build up a team of the highest possible calibre to undertake this fundamental research and keep the British effort effective in a world context, and the difficulty of assembling such a team in Dorset, were then, quite properly, emphasized(363).

With the question of siting settled, the question as to what to put in the site remained. In order to find an answer to this it was decided to reconstitute the CTR Advisory Committee, a body that had so far met only sporadically and to little effect, with a new membership(364). This was duly done, and guided by their deliberations it was agreed by the end of February that Zeta 2 should be officially abandoned, and that the flexible large power supply, named (for obvious reasons) Pandora, should go ahead together with an Intermediate Current Stability Experiment (ICSE), which was to be a toroidal experiment based on the configuration identified by Rosenbluth as being theoretically stable(365).

Initially envisaged as a modest sized experiment, it rapidly grew in size, and the decision to undertake a large scale experiment was approved by the AEA in May(361,366), and by the Treasury in July. ICSE was considered to be the theoretically most promising development of Zeta, and in political terms it provided an identifiable *raison d'être* and centre-piece for the large new Culham site. It was presented as an obvious step in fusion research, the results of which should settle one way or the other the prospects of eventual success, at least for a toroidal type of device. The opinions of those working on the project, however, was divided concerning the wisdom of such a large commitment made with inadequate physical understanding and incomplete engineering assessment. The background and consequences of this decision will be the subject of the next chapter.

Despite the uncertainty surrounding the future location of the fusion work, and the concern with Zeta's successor, it should be appreciated that both development of Zeta and a steady background programme of basic research continued during 1958 and 1959. On 1 March 1958 a new "Controlled Thermonuclear Reactions" division had been set up at Harwell under Thonemann to contain all the fusion activities except the theoretical work which remained in the Theory Division under W B Thompson. The experimental work had been, since its inception, in the General Physics Division, which now ceased to exist; D W Fry, its leader, was left free to concentrate on his new responsibilities at Winfrith, though as Deputy Director he still retained some responsibility for the fusion programme. The new division was soon to be taken over by Pease in an acting capacity on Thonemann's departure to the USA for a year's leave at Princeton in April 1959, but in a note to Schonland in January 1959 Thonemann set out his ideas for the organization of the division(367). Carruthers was to take charge of technical developments, Bickerton to supervise experimental plasma physics, and Pease to lead the work on Zeta 1 and Zeta 2. The larger machines feature in this history, but since the Geneva conference and declassification many smaller plasma physics experiments on new configurations were started, and experimental studies of fundamental topics such as plasma waves of various types and shock waves. Furthermore, there was continuing activity in diagnostic techniques. These are not detailed here, but a comprehensive chart dated April 1960, shortly before the move to Culham shows the extent of the programme at Harwell(368). The more important results were presented at the now more frequent open conferences and in the published literature.

CHAPTER 13

THE ICSE AFFAIR

To understand the affair of ICSE, the fusion machine that was sanctioned but never built, it is necessary to look at the general context of the original decision to go ahead. We have already referred to the uncertainty at Harwell following the removal of key aspects of its research to Winfrith and to the development laboratories of the Production Group, and this effect had been magnified in 1958 due to changes in the organisation of the Atomic Energy Authority. Following Cockcroft's announcement of his impending retirement the day before the Zeta press conference, the AEA decided, for a combination of reasons, to divorce the roles of Authority board membership and executive directorship of the groups. In the Research Group, covering Harwell and the new southern sites, this meant that while Cockcroft remained the Authority Member with responsibility for scientific research until his retirement, (which did not take place until 1 July 1959), executive directorship of the group was lodged in Basil Schonland, previously deputy director of Harwell. Sir William Penney continued for the time being to combine Authority board membership and executive functions for AWRE, where the promotion of William Cook to take charge of the Production Group had left a gap. Later in 1958, however, Nyman Levin was appointed director of AWRE and Penney was relieved of his executive duties in order to lead the British delegation to the Geneva talks which led, at the end of the year, to the three power moratorium on nuclear tests. Following these rather exhausting negotiations, crucial to Anglo-American relations and to the future of nuclear weapons research in Britain, Penney spent the first half of 1959 tying up loose ends before becoming Authority member for scientific research, with executive responsibility now restored, on Cockcroft's retirement[369]. During this period, in anticipation of his forthcoming responsibilities, he began to take an increasing interest in the fusion programme as a whole and he set up the Thermonuclear Technical Policy Committee (TTPC) as described later. Meanwhile, Keith Roberts, one of the leading AWRE theoreticians, had moved over to join the Harwell Theory group[370] under W B Thompson; D W Fry, who had been division head of the experimental team throughout the Zeta programme, had been appointed director designate of the new establishment at Winfrith. Thonemann, the driving force behind Zeta, had requested and been granted a year's leave (1959-1960) at Princeton University in America in order to catch up with developments there in plasma physics, and generally take stock of where fusion research was going[371].

Schonland was an old friend of Cockcroft's from his Cambridge days. He had returned to England from an important position in South Africa in October 1954 to take the position of deputy to Cockcroft; it was widely believed that he had been chosen by Cockcroft to succeed him, and this he did on Cockcroft's retirement as director in the summer of 1958. Cockcroft's successor was bound to have a difficult task, and Schonland was not popular in his role of director; this arose partly at least from the need for some retrenchment after Cockcroft's generosity and extravagance. His scientific experience was not directly relevant to Harwell's main programme, though as a world expert on atmospheric electricity, including especially thunder and lightning, he had a knowledge of plasma physics and took a lively interest in the Zeta programme. Although formally responsible for the fusion programme he was still operating in the shadow of Cockcroft, and knew that he would soon have to hand over responsibility to Penney. Cockcroft himself was still Member for Scientific Research and still a strong advocate of controlled fusion, but with one eye on his new Cambridge position he was no longer as interested in the details of the programme as he had once been[371,372]. Penney had not so far taken much interest in the fusion programme outside AWRE for which he had as yet no direct responsibility. But he knew that he would be responsible for the carrying out of whatever programme should be devised, and he naturally wished to be involved in its choice[373].

Lower down the hierarchy, the presence of three senior members of staff, Cockcroft, Penney and Schonland, each with an interest but only a limited interest in the fusion programme, compounded the confusion arising from other changes taking place. Fry, now deputy director of Harwell, still retained some responsibity for the Harwell fusion work, but knew that this would be so only for a short time. With the arrival of K V (Keith) Roberts at Harwell and the impending translation of Penney the views of the AWRE team competed with those of the Harwell team, who were themselves divided. The situation was still further confused by the fact that while it was believed that John Adams would almost certainly be recalled from CERN to take over the fusion programme from Fry and head the new establishment, Adams was not actually offered the appointment until April 1959 and did not accept until June[374]. Despite a feeling in some quarters that the post should have been offered formally to Thonemann, he had no wish to devote himself to administration, and joined in the general approval of Adams's appointment. Thus the views of yet another senior figure with a strong interest but as yet only partial responsibility had to be taken into account. Then there was Thonemann, looking forward to a year's break affording him the time to consider the options for the future, but expected to play a leading part in making a choice between these options before he went away. Fully aware of the general lack of understanding of the basic plasma physics, he was reluctant to recommend any large scale experiment at this stage. A further complicating factor was that whatever options were chosen Thonemann himself wished to remain with a small group at Harwell rather than move to the new establishment at Culham, a course that had been agreed to by Cockcroft but was strongly opposed by Schonland[375].

Into this confused context were placed the differing attitudes already referred to in Chapter 11. The original Harwell team were still committed, on the whole, to the continuation of toroidal pinch research, but at AWRE the preference was strongly for the mirror machine. This was currently a strongly advocated option in America, since at least at low particle densities it seemed to be the most amenable to theoretical analysis[376]. The latter point was significant for the AWRE scientists; accustomed to the small tolerances of bomb design work, were used to programs being led and dictated by the theoreticians and were accustomed to being able to predict precisely how something would behave before actually building it[377]. Thus, at the end of 1958 Penney wrote, in a paper on the CTR work in the weapons group "The design of successful uncontrolled thermonuclear experiment - a megaton bomb - required detailed analysis to be made before the 'experiment'. The same general approach is necessary in CTR work"[326]. The AWRE scientists constituted a relatively small group who had arrived late on the fusion scene, but with the impending appointments of Penney and Adams, who was also used to working on near-certainties at CERN, they could exert an influence disproportionate to their numbers and experience, and directly opposed to the more experimental philosophy of the Harwell team. The main research on the H-bomb having been completed both Roberts and J B Taylor, two of the leading British weapons physicists, had transferred to controlled fusion[378], and their opinions, backed by an impressive track record, carried considerable weight with Penney. Given the history of fusion research in Britain, the AWRE preference for a mirror machine was unlikely to be reflected in the choice of the top priority project to succeed Zeta. Their influence was felt, however, on the debate within the Research Group upon the choice of configuration for the new machine, assumed to be a toroidal one. Here the AWRE scientists' instinctive reaction was to prefer a configuration which appeared to have at least some theoretical foundation, rather than one based upon Zeta, the complexity of which was well beyond the scope of the theoretical analysis of the time.

In retrospect, the choice between the available alternatives, none of which had yet been adequately explored, needed an extended period of thought and discussion. But if there was one thing on which the senior people, Cockcroft, Penney, Schonland, Fry and Adams, were more or less agreed it was that a decision was required quickly. The controlled fusion programme had acquired considerable momentum and international status. A new establishment was being set up to house it. It seemed important both for general morale and in order to justify the new establishment that a large scale experiment, promised earlier by Schonland at the press conference of 16 May 1958[267], be sanctioned and embarked upon quickly. Moreover, despite the lessons of the past year as to the inadequacy of theoretical knowledge, there was still a tendency to assume that the pursuit of any given line of development would be essentially non-problematic. This view was not shared by those scientists with direct experience of the intractable nature of plasma physics; well aware of the uncertainties they were reluctant to accept the responsibilities associated with the promotion of a large project which they knew well would be in the public eye, and which would be a disaster if it fell short of expectations.

We now take up the discussion of the progress towards the ICSE project from the previous chapter, at the end of 1958. By this time the lack of stability in Zeta had been recognized as a major problem and studies begun on how to modify Zeta 2 to include the conditions stipulated by Rosenbluth, Tayler and others. Bickerton, in particular, was urging what, since the Geneva Conference, had become known as the "Rosenbluth distribution"[284]. This requires first, that the current should flow on the surface of the plasma column, and second, that the toroidal magnetic field should be of opposite sign inside and outside the plasma. To ensure that the current flows on the plasma surface and does not diffuse to the interior advantage must be taken of the "skin effect"; this requires that the current be set up very rapidly. Programmed reversal of the direction of the toroidal field must also be accomplished, with the aid of pulsed coils around and linked with the toroidal vacuum chamber. This requires a vacuum chamber of insulating material to allow penetration by the rapidly varying magnetic fields. This configuration, and a schematic diagram of the apparatus planned to generate it, are shown in Fig. 13.

The decision on how to proceed now lay with Cockcroft and, especially, Schonland. In order to provide general guidance, as noted in the last chapter, they

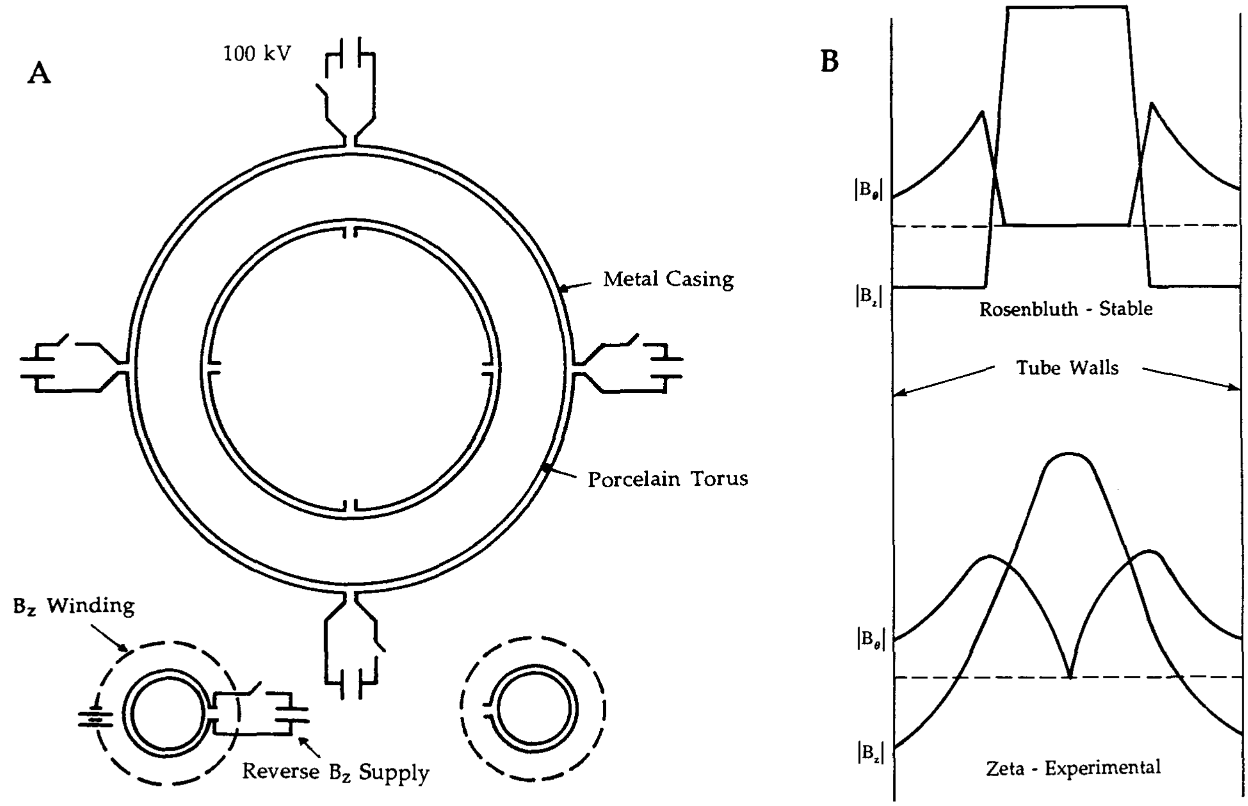

Fig. 13 (A) Schematic diagram of the arrangement envisaged for ICSE, re-drawn from ref. 379. The large and small diameters of the torus, which is made of porcelain, are 6m and 1m. 100 kV is applied across each of the gaps in the metal casing shown in the top view when the switches are closed, and this drives the toroidal plasma current. The dotted lines represent coils to produce a steady B_Z field; the power supply across the slot around the toroidal casing is to provide the time dependent field reversal. (B) Sketch of the distribution of toroidal magnetic field and poloidal current density across the torus, compared with the corresponding distributions in Zeta, from ref. 421. This diagram is only schematic; it shows a skin depth of about 30% of the plasma radius, which is far too large for stability.

decided to reconstitute the CTR Advisory Committee with a new and enlarged membership to include several senior distinguished scientists not directly concerned with fusion(365,380). At its first meeting on 18 December 1958 papers were presented by staff of AERE, AWRE and AEI, reviewing their programmes and making recommendations for the future(381). Reviews of the international scene, especially details of the various American projects, were also presented. In plenary session following the presentations a number of recommendations were made; it was agreed that the main AERE effort should be "concentrated on the Zeta approach until it could be seen clearly whether or not this was likely to succeed". Nevertheless "a decision to build a major pinch device such as Zeta 2 should be postponed", and in its place "the technical design and development work on large-scale energy storing and switching and the procurement of the necessary power supply be undertaken as rapidly as possible". Finally, about 20% of the staff should work on "basic plasma physics not immediately relevant to major projects"(382). These were very much in line with the recommendations in the paper presented by Thonemann(383).

In the context of these recommendations "the Zeta approach" presumably covers any system using a transformer driven toroidal discharge, whether as in the original Zeta experiment or modified in accordance with Bickerton's suggestions to produce a "theoretically" stable configuration. Ultimately, it was clear, a large scale experiment would be required, but it was as yet by no means clear just what this experiment should be. This became a topic of discussion and debate. As a forum for discussion Penney decided to set up the "Thermonuclear Technical Policy Committee" (TTPC) to decide and co-ordinate programmes both at Harwell and Aldermaston, where current thinking was in favour of a large mirror machine similar to the American DCX at Oak Ridge(293).

The first of these meetings was held on 21 March 1959. Penney was in the chair; Cockcroft, Schonland and Fry were present, with AERE representatives Thonemann, Thompson, Pease and Bickerton with Allen and Hulme (with apologies from Curran) to represent AWRE. However, one month before this an informal meeting was held to have a preliminary discussion of the main issues. The first item on the agenda was ICSE, which here first enters the written record. Discussion was based on a paper of Thonemann dated 12 February entitled "Proposals for Zeta 1, Zeta 2 and Pandora (Garbo)"(385). In it he states that the ICSE experiment is designed to answer the questions: "is the toroidal pinch discharge stable" and "is the energy loss to the walls calculated from binary collision theory?". An experiment to be done in the next two years at Harwell is proposed, with a current of 1.5 MA in a porcelain torus, at a cost of about £500,000. Plans for proceeding with the power supply Pandora and postponing Zeta 2 are endorsed. "The ICSE experiment replaces the Zeta 2 experiment, and it is designed to give the relevant information in a shorter time and at lower cost". If ICSE were to prove successful, then plans for a modified Zeta 2 would be considered, making use of Pandora. After some supportive discussion at the meeting, Penney asked for a fuller paper to be submitted to the first formal meeting.

Shortly before the first formal meeting of the TTPC, Thonemann (with Johns) at Fry's request wrote a paper (dated 12 March) for the Research Group Management Board setting out the options. In this the sum of £420,000 was requested for the ICSE experiment(386). (This excludes the cost of the capacitors, £780,000, which would form part of Pandora(387)). Problems had arisen with siting the experiment at Harwell, however, and it was now suggested that it should be in an existing aircraft hanger on the Culham airfield, (an option found to be impractical on closer inspection of the condition of the hangars). Speed was considered essential, so as not to lose the lead.

By the time of the TTPC meeting, ideas about ICSE had changed, as explained in a paper by Thonemann and Johns presented to the meeting(388). It was now to be split into two parts ICSE(a) and ICSE(b). The former was to replace the earlier proposal for a fast experiment, but would have an aluminium torus (originally intended for Zeta) rather than a porcelain one. It was intended to have a fast current rise to confine the current flow to the plasma surface, but not field reversal, at the lower current of $\frac{1}{2}$ MA. It was conjectured that even without field reversal the plasma might, with luck, be stable, since the theory was incomplete. ICSE(b) was now to have the ceramic torus and field reversal, and was now seen as a "bigger device", carrying a current of $1\frac{1}{2}$ MA, as proposed for the earlier small version of ICSE. (It would certainly need to be physically larger because of the difficulty of making a low aspect ratio torus from ceramic). The capital cost of both experiments was stated to be £1¼m (without contingency).

Although the discussion on ICSE was introduced by Thonemann, he does not seem to have been happy with the idea of an expensive large scale experiment with so little real theoretical support, believing that, if indeed a large machine was to be built, the original Zeta 2 option might have been a wiser choice[389].

The original request to the RGMB was updated in April, in line with the paper presented to the TTPC. They were asked to approve an approach to the Atomic Energy Authority and the Treasury for the construction of ICSE(a) and ICSE(b)[390]. Approval was given, and a paper presented to the AEA in May by Schonland[391]. This was based on the paper to the RGMB, emphasising the endorsement of the TTPC. The view of the TTPC at its meeting on 21 March "that a major uncertainty in this field would best be resolved by the ICSE project" is quoted, and with regard to siting Schonland writes "It is the unanimous opinion of Sir John Cockcroft, Sir William Penney and myself that the right policy is to plan on putting ICSE at Culham Airfield as soon as possible". (No action was taken with regard to the plans for a large DCX type mirror machine put forward at the TTPC meeting by K W Allen of AWRE, and opposed by Thonemann).

The plans for ICSE were approved by the Authority in May[392] and by the Treasury in July[393]. By this time detailed consideration of the physics and engineering were under way by the design group set up some three months earlier, this was now chaired by Pease with G Francis responsible for the physics, and D L Smart (reporting to Carruthers) for the engineering design[394]. A realistic appreciation of all the problems of such a large and novel project, despite the experience of Zeta 1 and the Zeta 2 studies were, however, hardly to be expected in such a short time.

Once ICSE had been approved, every effort was made to create a project as tightly and efficiently controlled as the Zeta project had been. Wherever industry was brought in senior representatives of the firms concerned were involved and the prestige of the project and the need for tight control were emphasized. In view of the large cost that would be entailed every precaution was taken against the dangers of going over budget. Careful costings were done, and the estimates of outside firms were checked and modified so as to arrive at a firm estimate for the project, to which the team felt they could commit themselves with confidence[395]. By the spring of 1960 such an estimate had been reached. The cost of the project would be £2.5 million. A contract had been put out for the large capacitor bank, the expenditure on which was sanctioned ahead of the rest of the project on the grounds that it would still be needed even if something went wrong with the toroidal part of the experiment. And the team were ready to go ahead with the ordering of the rest of the equipment[396]. At this point, however, the project began to run into difficulties for a number of different if connected reasons.

The cost figure of £2.5 million was, so far as the ICSE engineering team was concerned, both fair and final. It was substantially higher than the £1.5 million that had originally been approved, but since the original figure had been no more than a estimate made on the basis of rather general scaling rules, the difference was considered to be reasonably small, and the cost of the same order of magnitude as that originally envisaged. To Penney and Schonland, however, who seem to have underestimated the technical complexity of ICSE and to have treated it from the beginning as a relatively straightforward engineering task, the cost increase of 67% was highly significant and was seen, almost certainly incorrectly, as a harbinger of things to come. This impression had, moreover, been reinforced by Schonland's insistence, as feasibility studies had progressed, on repeatedly asking for the latest "guesstimate" price, which had of course steadily risen(397). When Pease had replied to one such request in October 1959 that the estimate stood at £2.1 million, the first significant increase over the original sanctioned amount, Schonland had exclaimed to Penney that "I am used to shocks but this is a ------- and presumably not the end"(398). As the studies continued the price slowly rose from £2.1 million to £2.2 million and then to £2.5 million. Denis Willson, who was to be secretary of the soon to be opened Culham establishment, asked Schonland not to indulge in any "sod-cutting nonsense" by way of opening celebrations, on the grounds that he did not want any attention to be drawn to ICSE(399).

Since the £2.5 million was a final figure it could probably have been approved had other circumstances been favourable. But in July 1960, following a considerable overspend on some fissile material production plant in the North (an overspend quite unrelated to fusion or to any other Research Group activities), Sir Roger Makins, who had taken over from Plowden as chairman of the Atomic Energy Authority in January, asked Penney for a complete review of capital expenditure in the Research Group, and in particular of the dominant item of that expenditure, that relating to ICSE(400). Penney and the financial administrators at Harwell then reviewed the ICSE estimates, and taking into account the change between the original sanction and the current estimate, but not taking advice on the technical circumstances underlying that change, they predicted a further cost increase, proposing first a final figure of £3.2 million and then, in mid-August, one of £4 million(401).

It is impossible to say what ICSE would have cost if built. On one hand all large and complex projects do have a tendency to escalate in cost. On the other hand, this danger was probably adequately accounted for in the £2.5 million estimate. Project control was tight and all the indications are that the experiment stood a fair chance of being completed to specification, to time, and to cost. The estimate included all the large one-off costs that had resulted from the feasibility study investigations, such as those for the construction and assembly of the large transformer, and for a new factory being built in France especially to manufacture the ceramic torus. Moreover, it was items such as this, rather than the general creeping inflation of which Penney and Schonland were afraid, that had accounted for the increases in estimate to date. Whether or not the further predicted increases were justified, however, they clearly constituted a serious threat to the ICSE project, even when set against a substantial cancellation cost in the region of £0.4 million(402).

Another threat to the ICSE project, closely linked to its rising estimates, was posed by a decision of the AEA to curtail expansion of the fusion programme(403).

Thonemann, in his original plans for the Culham site, had stressed that the need for a fundamental physics programme meant above all that it should not be dominated by one or even two experiments[404]. The same criterion, however, reflecting as it did the longer term context in which controlled fusion was now seen, also seemed to the administrators to imply a programme of restricted size. They felt that if fusion research were allowed to grow unfettered it would simply acquire its own internal momentum, generating further research projects rather than concrete results. Since this tended to happen anyway, even with a restricted programme, they were probably right. Thonemann's response, made from his sojourn at Princeton, was that if the programme were to be limited, then no large scale experiments at all should be undertaken until more was known about their basis. At Harwell, however, the response to the restricted funding was initially to try and keep ICSE going by cutting other projects, most notably the flexible large power supply Pandora[405]. This policy became increasingly difficult to support. In the summer of 1960, when Adams reviewed the existing projects before taking over as director of Culham, he came to the conclusion that there was no hope of achieving a balanced research programme within the expected financial constraints if ICSE were to be continued, whichever estimate were adopted for it. Even on the lower estimate ICSE would take up such a large portion of the Culham budget that he as director would have no room at all for manoeuvre, and he was therefore ready to back up Penney's inclination to cancel on cost grounds[406].

The combination of the factors discussed above was quite sufficient to ensure the demise of ICSE, but there were other pressures too, acting in the same direction, and coming to fruition at the same time. So far as Penney was concerned, the greatest influence, and one that made itself felt even before the ICSE cost rises and may have affected his judgement of these, was that exerted by the theoretical physicists. In general terms it had been the theoreticians who had provided the greatest impetus in support of ICSE. But once this became a concrete project they began to have doubts. Would it in fact prove possible to set up experimentally the theoretically inspired Rosenbluth configuration? And would this indeed be stable? There had been no problem working out how the required fields might initially be set up, using the sudden application of critically timed magnetic fields. But no detailed calculations had been done on the reaction of the plasma to the imposition of these fields[407]. It was simply not known, either experimentally or theoretically, whether the plasma configuration would in fact develop as required, and in this sense the whole ICSE project was purely speculative. At first the theoreticians kept their doubts on this issue to themselves. But at the end of January 1960 Flowers, who had been visiting the Harwell Theoretical Physics division from Manchester where he was now Professor of Physics, relayed these doubts to Cockcroft, who promptly got in touch with Penney[408]. When Penney went back to Flowers' successor as head of Theoretical Physics Division, W M Lomer, Lomer reported that he personally had no doubts about ICSE but that many members of his staff did[409]. Having also consulted Thompson, Bickerton and Pease, of whom he had particular trust in Pease, Penney reported back to Cockcroft in early February that he was happy with the project[410]. Nevertheless, those senior physicists who might have had responsibility for leading the project were well aware of the uncertainties in the physics; the project had a strong public profile, and Penney appeared to promise great things in his Press Conference held on 22 July 1959[411]. It was to be a success to restore the public faith lost after the Zeta episode. Under these circumstances it is not surprising that their support was tempered by reluctance. When Penney came to review the ICSE estimates later in the year he took the precaution of consulting others, including Sir James Chadwick, from outside the AEA, as well as K V Roberts, in whom he had particular faith following his role in the bomb project, from inside. Chadwick expressed himself as agnostic in respect of the particular problem raised, but as generally opposed to the direction of research represented by ICSE[412]. Roberts, who was working on the computer modelling of plasma conditions, expessed himself as being astonished by the pursuit of ICSE on the flimsy theoretical basis available. He was, he said, convinced that the ICSE configuration could not in fact set itself up; and he presented Penney with a theoretical analysis in support of this position[413].

Although in spirit still an AWRE man, Roberts was now working, albeit more or less on his own, at Harwell. But meanwhile, as plans for the combination of the existing AWRE and Harwell fusion teams at Culham took shape, the representatives of the AWRE team had also begun to make their presence, and their opposition to ICSE, felt. At first this was not achieved by any great increase in pressure from the AWRE team themselves, though as early as December 1959 Curran did press hard to get a mirror machine programme established as a Culham priority[414]. Rather it resulted from disarray in the Harwell camp.

Up until early 1960, the strong Harwell commitment to toroidal pinch machines had dominated British fusion thinking, and the dominant role of the Harwell team had ensured that the alternative approaches favoured by the AWRE scientists were kept in the background. A change in this situation came, however, when Thonemann returned from sabbatical in April 1960. From America, he had watched the progress of ICSE and its domination of the British programme and, most seriously, of the best talent, which was thereby removed from fundamental research, with dismay. Before leaving in 1959 he had already had talks about keeping a small research group of his own, preferably at Harwell, rather than getting involved in the administration of Culham and its big projects, and while in Princeton he had set about securing this. Schonland was not and never had been keen on the idea, but it had had Cockcroft's backing, and Thonemann remained too important to lose. So after a period of four way negotiations between Thonemann, Schonland, Adams and Penney, Thonemann had been offered by Penney a position as a "senior distinguished physicist with a small team of about a dozen people, free to work as you please on a line of your own choice". But Penney had insisted that Thonemann's group should in due course move to Culham, and that Thonemann must be responsible to Adams as Culham director.

Thonemann's future position was thus agreed, but the problem remained as to who should take charge of the Harwell fusion programme between his return in April 1960 and Adams's arrival and the beginning of the move to Culham in October. While Thonemann had been away his deputy Pease had taken charge of the CTR Division. Now, since he was no longer to work within the main thrust of the programme, it seemed natural that Pease should remain in charge of the Harwell team until Adams arrived to take over. However Pease, like Thonemann, was more interested in doing research than in administration. He had already tried to escape being lumbered with the administration of ICSE by following Thonemann's example and, at Alfven's pressing invitation, requesting a sabbatical in Sweden but this had been refused[415]. Repeating this request he now expressed his own wish that Thonemann, not he, should be in charge during the interim period. Thonemann himself, seeing an opportunity of making sweeping changes in the fusion programme, and so countering the current tendencies, before getting down to his own research, also seems to have wished to do this. On the other hand Adams, who naturally did not want any large changes made in the months before he took over, wanted control to be vested in a committee, with Penney in the chair. Technically, the situation was resolved when Penney decreed that Schonland should take charge of the whole programme, including Thonemann's group, for the interim period. But this was the one solution that neither Thonemann nor Pease nor Adams had wanted. Under his direction, the Harwell ICSE team effectively lost their dominating role and their power to defend ICSE[416].

The combination of pressures against ICSE, from all the sources mentioned, was immense. In late August 1960 the project was cancelled[417]. A month later this cancellation was announced in a press release[418]. The design for ICSE was never published, but detailed reports with set of drawings was prepared for both ICSE (a) and ICSE (b). These are deposited at the PRO[419].

CHAPTER 14

CONCLUSION

Within the Atomic Energy Authority generally the cancellation of ICSE was greeted with relief and, in some quarters, positive satisfaction. The AWRE scientists quickly seized the initiative. As the team's enthusiasm for their programme made itself felt, Adams's early plans for Culham accordingly reflected a strong AWRE bias[(420)]. These plans are clearly set out by him in the first planning report for the new laboratory dated January 1961[(421)]. Work on both the thetatron and the magnetic mirror "Phoenix" started at AWRE were expanded, and larger versions of both these experiments were built at Culham under Niblett and Sweetman respectively. Using their pioneering fast switched condenser banks the AWRE team moved forward to a leading position in thetatron research, which became a fashionable field world-wide in the next decade. Kilovolt temperatures and true thermonuclear neutrons were regularly observed, albeit in short pulses. Other containment methods such as "cusp" geometry were tried, and there was increased interest in shocks and shock heating. The emphasis was at first on smaller scale work to establish basic principles, with a strong theoretical group first under W B Thompson and later J B Taylor, and further development of computational methods by K V Roberts. Both Roberts, and Taylor a senior member of Thompson's group, had been major contributors to the weapons programme at AWRE. Steady work on Zeta continued until 1968. One of the more important results obtained from Zeta was the unexpected observation that during the establishment of the discharge the toroidal field changed sign in the outer regions of the plasma. Although not understood at the time it later formed the starting point of the Reversed Field Pinch programme at Culham. This was another concept that became of interest internationally, producing for a while at least an important contender as an alternative to the Tokamak.

Within the ICSE project design team itself, however, there was naturally disappointment at the cancellation of a project into which so much effort had been put, for reasons that they could not themselves accept as valid. Moreover, the cancellation was also received badly outside the AEA, by parties who felt with some reason that they might have been consulted or at least warned in advance. Immediately following the issue of the press release reports came back of deep concern at the United States Atomic Energy Commission, then in the middle of a budget review. Ruark had heard of the cancellation only by accident, during a visit to Russia, and the AEC had no firm knowledge of it until it was made public in the press release. They were naturally disturbed by the thought of possible repercussions of the British axe on their own programme, and upset at not having been warned. As Martin Fishenden, the scientific secretary of Harwell, noted, "evidently all concerned forgot about the US angle"[(422)].

In Britain, Lord Hailsham, then Minister for Science, also found himself in an embarassing position. Having already gone back on a previously published decision to move fusion to Winfrith in order to approve the AEA's request for a large new establishment at Culham, he now found that what was to have been its central feature was no longer to be built. Furthermore, the site had originally been bought by the Admiralty using a compulsory purchase order, and its purchase by the AEA had therefore required an enabling act in parliament. In order to secure the services of the director of Culham, John Adams, the Government had also had to veto his prospective appointment to the prestigious position of director general of CERN, pleading an urgent need for him in this country(423).

The problems blew over, and Adams concentrated on the difficult problem of integrating the Harwell and AWRE teams, and on the almost impossible one of pursuing the original intention of keeping Culham as an open site, without security restrictions, in the face of persistent pressures to do otherwise(424).

Looking back over thirty years later at the ICSE affair, opinions remain as deeply divided as ever on whether it should or should not have gone ahead, though with some changes of stance. Some of its supporters argue that it would have led to the Tokamak concept and advanced controlled fusion research by a decade or more compared with the programme that has been followed since. Others maintain that it would have retarded progress in this country by an equal amount; apart from the uncertain physics the advanced high speed high power pulsed technology required was not yet sufficiently developed and this too would have led to problems and disillusionment. The prestigious reputation for excellence in research that Culham was later to enjoy would not have been achieved. Carruthers, who would have been largely responsible for carrying out the project, maintained that it should have been built, on the grounds that it would have brought forward by at least a decade serious consideration of the real problem of fusion devices, namely that of whether successful or not as physics experiments, they can be developed as economic reactors(425).

Whatever its rights or wrongs, the cancellation of ICSE, followed swiftly by the closing down of the AEI Aldermaston laboratories, marked the end of an era in British controlled fusion research. Throughout the 1960s the prevailing atmosphere in Culham as throughout the world was more cautious. In contrast with its earlier expansion, the British CTR programme was actually cut back quite severely during the latter part of the decade, and it was only with the Russian tokamak success in 1969 that things began to look up again. Even with this later revival, moreover, the fifteen years from the first proposals by Thomson and Thonemann to the cancellation of ICSE remain something of a heroic though at times confused age, driven through scientifically unknown territory by the enthusiasm and tenacity of the early pioneers.

NOTES ON THE REFERENCES

An attempt has been made to include references to as much source material as possible. With published material this is straightforward. AERE and AEA reports which were originally unclassified are held at the Document Supply Centre of the British Library at Boston Spa. Those which were originally classified are deposited at the Public Record Office (PRO) at Kew, and the reference numbers (explained below) are given. Here are deposited also numerous official files and documents which originated at Harwell. These references all begin with the group letter AB, followed by the class number and after a slash the piece number, (for example AB12/131). The class number is generally 6 is for general files, 12 for committee papers, and 15 for reports, but may be different for more recent items. A few of the PRO files listed may not yet be available for public inspection.

Copies of many of the more interesting PRO papers, together with others not at the PRO are at the Churchill Archive Centre, Churchill College, Cambridge, (file HIFU). These are identified by the initials CAC. Many other papers not referred to in the report are also in the archive, including a bibliography. At the time of writing compilation of this archive has not been completed, but a key related to reference numbers in this report will be included.

A problem arises in that some of the material, though generally not classified, is in files or papers that are not yet released for public inspection. In this case references are omitted, though information will be available at the CAC archive. It is believed that few, if any, of these are of major importance. Further, some of the files inspected (by JH) in the early stages in this work were destroyed rather than being sent to the PRO. Again, information will be available at the CAC archive.

Another source of information is from discussion with individuals who participated in the work. Much of this was from a series of interviews with JH in 1981-2; though no transcripts are available. Further information was gleaned from comments made by those who read and commented on all or parts of the manuscript. These are not in general referenced, though specific letters dealing with points of importance are referenced and in the CAC archive.

Abbreviations to be found in the references are listed separately. Much of the later work referred to was presented at the Second United Nations International Conference on the Peaceful Uses of Atomic Energy, held in Geneva 1 - 13 September 1958, published by the United Nations. This is referred to simply as "Geneva Conference". Papers presented at the Convention on Thermonuclear Processes held at the Institution of Electrical Engineers in London in April 1959 were published in the Proceedings of the Institution Vol. 106, Supplement No 2. This is referred to as "IEE Convention".

REFERENCES

1 Hendry J, Annals of Science **44** 143 (1987).

2 Thomson J J, Phil. Mag. **4** 1128 (1927), and Proc. Phys. Soc. **40** 79 (1928).

3 Knipp C T and Knipp J K, Phys. Rev. **38** 948 (1931).

4 Stuewer R (ed.) "Nuclear Physics in Retrospect", (Minneapolis, 1979) p.78.

5 Szilard L, US Patent 7840/34, (1934).

6 Interview Pease-Hendry 1981. Letter Atkinson - Pease, 8.12.1973, CAC.

7 Thonemann P C "Principal CTR Experiments", Manuscripts for Hendry and Lawson, 10.9.1981, and 10.1.1989 CAC.

8 Attestation of Thonemann's interest before the war in Australia is given by L G Alexander, 17.2.82, CAC.

9 Hewlett R G and Anderson O E, "History of the USAEC, Vol 1: The New World 1939-1946", (Philadelphia, 1962) and Irving D, "The German Atomic Bomb" (New York, 1967).

10 Teller E, Geneva Conference **31** 27 (1958).

11 Tuck J, *ibid* **32** 3.

12 Post R F, Rev. Mod. Phys. **28** 338 (1956).

13 Bromberg J L, "Fusion, Science, Politics and the Invention of a New Energy Source", (The MIT Press, Cambridge, Mass. 1982) p.18.

14 Ulam S M, "Adventures of a Mathematician", (Charles Scribner's Sons, NY, 1976) p.228.

15 Recollections of G P Thomson from an unpublished history of the Department of Physics, Imperial College, London, CAC.

16 "Method of Using the Nuclear Energy of the D-D Reactions", GPT-E72 (2'), CAC.

17 "Draft Provisional Specification", GPT-E74 (1') and (2), CAC.

18 GPT-E88, CAC.

19 Peierls to Thomson 12.3.1946, GPT-J89, CAC.

20 Peierls to Thomson 26.3.1946, GPT-J89, CAC.

21 Thomson to Peierls 2.4.1946, GPT-J89, CAC.

22 Peierls to Thomson 15.4.1946, GPT-J89, CAC.

23 Gardner J W, Proc. Phys. Soc. B62 300 (1949).

24 Thomson G P and Blackman M, Patent No. 817681, Application date 8.5.1946, Complete Specification 28.4.1947, Published 6.8.1959.

25 GPT-E50.

26 AERE-TC27, 23.1.47, AB16/727, CAC.

27 Interview Blackman-Hendry, 1981.

28 Brief note on the "Wirbelrohr", taken from report by Wasserab T, (Wasserass?), CAC.

29 Steenbeck M and Hoffman K, "The Acceleration of Electrons to very High Velocities in a Gas Filled Induction Tube (Wirbelrohr)", Translation of Siemens Technical Report HW/P1, No. 27, 13.12.1943, CAC.

30 Ware to Haines 1.12.1976, CAC; Allibone T E, Lecture on Thomson CAC.

31 Ware A A, Phil. Trans. Roy. Soc. **A243** 197 (1951) and University of London Ph.D. Thesis, 1949.

32 Cousins S W and Ware A A, Proc. Phys. Soc. B64 159 (1951).

33 Thomson to Brown 21.11.1947 and Brown to Thomson 8.12.1947, GPT J-9.

34 Thomson to Portal 29.5.1947, GPT-E89 and AB6/350.

35 Allibone T E, "The AEI Long-term Research Laboratory: an Industrial Experiment" Proc. IEE **134A**, 610 (1987); Niblett C A, "Images of Progress; Three Episodes in the Development of Research Policy in the UK Electrical Engineering Industry", University of Manchester Ph.D. Thesis 1980.

36 Allibone to Cockcroft 28.8.1947, AB6/350.

37 Portal to Cockcroft 2.6.1947 and Cockcroft to Thomson 10.6.1947, *ibid.*

38 *Ibid, passim.*

39 Allibone to Cockcroft 28.8.1947, *ibid.*

40 Allibone to Cherwell 27.10.1947, *ibid.*

41 Cockcroft to Cherwell 9.12.1947 and Fuchs to Cockcroft 19.12.1947, *ibid*, and Cockcroft to Thomson 29.12.1947, GPT-E89.

42 AB6/350 *passim* and GP-E89 *passim.*

43 Cherwell to Cockcroft 24.1.1948, AB6/350; AEA-TC66, 22.1.1948, AB16/727, CAC; Interview Davenport-Hendry 1981.

44 Perrin to Collard 10.5.1950, AB6/350.

45 Thomson G P, "Atomic Energy from Deuterium", winter 1947-8, GPT-E3(2'), CAC.

46 Thomson to Cockcroft 25.2.48, AB6/350.

47 Thomson G P, "Note on the Torus Project" (Feb 1948?), AB6/350, CAC; AERE report XPR/2142, CAC.

48 Watson H H H, Proc. Phys. Soc. B62 206 (1949).

49 Skinner H W B, AEA T/C 54, 8.4.1948, AB16/728, CAC.

50 Moss to Thomson, 9.2.1948 and 16.2.1948; Thomson to Moss 13.2.1968, GPT-E89.

51 GPT-E89, *passim*; meeting of 13.7.1948, GPT-E88 and AB6/350.

52 *Ibid* and Cockcroft to Goodway 20.7.1948, GPT-E88; Thomson - Cockcroft 9.9.1948 and 15.12.1948, AB6/350; AEA-TC61, TC66, AB16/728.

53 GPT-J53.

54 Latham to Hendry 2.4.1982, CAC.

55 GPT-J56.

56 Latham R and Pentz M J, AERE report XM/68, Dec. 1950, AB15/1520.

57 Pollock J A and Barraclough S H, Jnl. and Proc. Roy. Soc. NSW 31 131 1905.

58 Early typescript notes of P C Thonemann, all in CAC.

Atomic Energy Sources using the Light Elements	13.1.47	1p
Nuclear Energy from the Light Elements (Clarendon Laboratory Lecture)	19.1.47	3pp
The Interaction of Electromagnetic Fields and Ionized Gases	19.1.47	3pp
Nuclear Power from the Light Elements	12.3.47	2pp
Thermonuclear Reactions for the Production of Power (in AB6/350)	9.1.48	4pp
Thermonucleonics. Proposed Experiments	11.2.48	2pp
Description of Apparatus	ND	2pp
Thermal Rate of Disintegration of Deuterium	ND	2pp

59 Interview Thonemann - Hendry, 1981 and letters Thonemann to Hendry and Lawson, CAC.

60 Cowhig W T and Thonemann P C, AERE Report G/R 531, (May 1950) AB15/1313.

61 Thonemann P C and Cowhig WT, AERE Report G/R 532, May 1950, AB15/1314 and Proc. Phys. Soc. B64, 345, 618 (1951).

62 Bennett W H, Phys. Rev. 45, 89, (1934).

63 Tonks L, Phys. Rev. 56 30 (1939).

64 Thonemann P C, Cowhig W T and Davenport P A, Nature 169 34 (1952).

65 Thonemann P C, Cowhig W T and Davenport P A, Patent No. 839,248, Application Date 15 August 1950, Published 29.6.1960.

66 Cockcroft to Cherwell 9.12.1947, AB6/350.

67 AB6/350 *passim*, GPT-E89.

68 Gowing M M, "Independence and Deterrence" (Macmillan, London 1974), Ch. 16.

69 Van Atta C M, "A Brief History of the MTA Project", Livermore Laboratory report UCRL 79151, Jan 1972.

70 Latham R and Pentz M J, Nature 164 485 (1949).

71 Latham R, Pentz M J and Blackman M, Proc. Phys. Soc. B65, 79 (1951).

72 Buneman O, Nature 165, 474 (1950).

73 Blackman M, Proc. Phys. Soc. B64, 1039 (1951).

74 Fry to Cockcroft 28.8.1950, AB6/857.

75 AERE-PDSC (M)18, 3.11.1950, AB6/785.

76 Interview Thonemann-Hendry 1981.

77 Bromberg, as ref. 13, p.25.

78 Burkhardt L C, Los Alamos History report LA-5922-H, June 1975.

79 Thonemann P, and Fry D W, "Note for the PDSC, A Thermonuclear Reactor", AB6/857.

80 *Ibid*, and Skinner to Fry 28.10.1950, AB6/857.

81 GPT-E12 (1) and E15.

82 Thomson G P and Blackman M, Patent No. 822,462. Application date 14 January 1952, complete specification filed 14 January 1953, published 28 October 1959.

83 Thomson to Cockcroft 8.11.1950, AB6/350.

84 Cockcroft to Thomson 10.11.1950, AB6/350, Allibone ref. 30.

85 Ware A A, Hemmings R F and Miles H, AERE report X/M 164, (11.7.1956), AB15/5111.

86 Various notes in GPT archive, E16 to E29, some in CAC.

87 Bohm D and Gross E P, Phys. Rev. 75 1851 and 1864 (1949) and 79 992 (1950).

88 Alfvén H, "Cosmical Electrodynamics", (Oxford, 1950).

89 Moon P B, CAC.

90 Moon to Pease 17.4.1989, CAC.

91 Craggs to Hendry 1.3.82, CAC.

92 Durnford J and Reynolds P, IEE Monograph No. 70 (1953).

93 Reynolds P and Craggs J D, Phil. Mag. 43 258 (1952).

94 Proc. Phys. Soc. **A64** 587 (1951).

95 Allen J E, Proc. Phys. Soc. B70 24 (1957).

96 Sabel C S, "UKAEA and associated British Work on Controlled Thermonuclear Reactions; A List of Unclassified Documents and Published Articles", AERE-Bib 124, 1960, CAC.

97 AERE-GD M4, 24.10.52, AB12/131.

98 AERE-GD M7, 19.10.53, AB12/131.

99 Interview Carruthers - Hendry 1982.

100 AERE-GD M8, 12.1.1954, AB12/131.

101 Carruthers R and Davenport P A, Proc. Phys. Soc. **B70**, 49 (1957).

102 Kruskal M and Schwarzchild M, Proc. Roy. Soc. **A223**, 348 (1954). See also Shafranov V D. Atomnaya Energiya **1, No. 5 38 (1956)**.

103 Bromberg, ref. 13, p23.

104 Roberts S J, Sturrock P A, Thompson W B and Whipple R T P, "The Wriggling Discharge in Free Space", AERE report T/R 1792, (Nov. 1955).

105 CTRAC meeting 4.2.1959, AB73/3.

106 "A Proposal for an Experimental Fusion Reactor", AEA(54)44, 16.12.1954, AB6/1359. Part in CAC.

107 Bickerton R J, "Gas Discharge Anti-Wriggle Means", British Patent 830,254, Dec 12 1956, and Third Int. Conf. on Ionized Gases, Venice May 1957, Pub. Italian Physical Society (1957) p.101.

108 Turnbull A H, and Dellis A N, (eds.), AERE report GP/R 1789, AB15/4617; AERE-GD, AB12/131 *passim*.

109 See ref. 35. Details of the AEI work may be found in the quarterly report series AERE report X/PR 2150 (1-35), with a summary of experimental work to 1956 in AERE report X/M 164, AB15/511 and CAC. Most of the X/PR 2150 series that were originally classified are deposited at the PRO, see bibliography in CAC.

110 Thomson G P, AERE reports X/M 122, AB15/511; X/M 119, AB15/3291 and X/R 1831, AB15/4655; Ware A A, X/M145, AB15/486.

111 Allibone T E, Chick D R, Thomson G P, and Ware A A, "Review of Controlled Thermonuclear Research at AEI Research Laboratory". Geneva Conference 32, 169 (1958).

112 Craston J L, Hancox R, Robson A E , Kaufman S, Miles H T, Ware A A and Wesson J A, Geneva Conference 32 414 (1958). Robson A E and Hancox R, IEE Convention 47.

113 Resler E L , Shao-Chi Lin, and Kantrowitz A, J. Appl. Phys. 23 1390 (1952).

114 Wilson to Lawson 19.4.1991, CAC.

115 Flowers and Thonemann "Fusion Reactors" about September 1954, AB6/1359 CAC.

116 Flowers and Thonemann to Cockcroft 13.9.54, AB6/1359, CAC.

117 Lawson J D, AERE P17/P3, September 1955, AB15/4779; AERE reports GP/M185, AB15/4368 and GP/R 1807, AB15/4634; Proc. Phys. Soc. B70, 6 (1957).

118 Lawson J D, Poole M J and Tait J H, AERE P17/P1, July 1955 CAC; Thonemann P C, Poole M J, Lawson J D and Tait J H, British Patent 830,255, Application 18.1.1957, Complete Specification published 16.3.1960.

119 AERE-TSC, 13.9.54, AB12/170; Fry to Flowers 25.1.55, AB6/1176.

120 Fry to Cockcroft 1.6.55, AB6/1176.

121 AERE report GP/M 176, April 1955, AB15/4361.

122 Fry to Cockcroft, 1.6.55, Cockcroft to Flowers, 13.6.55, Flowers to Cockcroft 11.7.55, AB6/1176; meeting of 18.5.55, AB6/350.

123 Cockcroft "Nuclear Power from the Light Elements" AEX(55) 61, CAC.

124 AERE-GD M15, 7.10.55, AB12/131, CAC.

125 Whipple R T P, AERE report T/R 1601; Marshall W, Proc. Roy. Soc. A233, 367 (1955).

126 Flowers B H and Thompson W B "Development of Fusion Reactors in the UK" 9.5.55, AB6/350, CAC.

127 AERE-GD M10, 23.4.54, AB12/131.

128 Fry to Cockcroft, 10.11.54, AB6/1176.

129 AB6/1842 and 2139, passim.

130 AERE-GD, which covers the period 1952-60, AB12/131.

131 Cockcroft to Fry 29.6.55, Fry to Cockcroft 14.7.55, AB6/1176, Fry to Cockcroft 8.6.55, Cockcroft to Fry 13.6.55, AB6/1359.

132 Oppenheimer R, Nucleonics 13, May 1955 p.10.

133 Fry to Cockcroft 26.6.55 and 21.7.55, AB6/1434.

134 Atomic Scientists' Journal 5 74 (1955); Engineering 180 242 (1955).

135 Bromberg, ref. 13, p13; "The Argentina Episode" AB6/909.

136 Nucleonics 13, September 1955 p10; Atomic Scientists' Journal 5 124 (1955).

137 Nucleonics 13, October 1955 p.9.

138 Atomic Scientists' Journal 5, 227 (1956).

139 Cockcroft memo 4.11.55, AB6/1434. Other speculations of the period include Vollsath R E and Samson J A R, Bull. Am. Phys. Soc. 30, 917 (1955) and Nucleonics 14, February 1956 p.10.

140 Pease R S, AERE report GP/R 1932, (May 1956), and Proc. Phys. Soc. B70 11 (1957).

141 Kurchatov I V, "On the Possibility of Producing Thermonuclear Reactions in a Gas Discharge" Moscow 1956. CAC. See also AB6/1847 and AB6/1894 for transcript and discussion.

142 AEX 3.5.56, AB6/1896.

143 AERE-GD M14, 29.9.55, AB12/131.

144 Interview Fry - Hendry 1981.

145 Interview Tozer - Hendry, 1981.

146 Butt E P, Carruthers R, Mitchell J T D, Pease R S, Thonemann P C, Bord M A, Blears J, and Hartill E R, Geneva Conference 32 42, also in Proc. IEE 106A Suppt. 2 12 (1959).

147 Harding G N, Dellis A N, Gibson A, Jones B, Lees D J, McWhirter R W P, Ramsden S A, and Ward S, Geneva Conference 32, 365.

148 Mitchell J T D, Whittle H R, Jackson E M and Clarke P B, IEE Convention 74.

148a Burton W M *et al.*, Proc. 1961 Salzburg Conf. on Nuclear Fusion, Nucl. Fusion Suppt. III 889, 909 (1962).

149 Penney to Plowden 14.1.55, CAC.

150 AERE-GD, M-15, 14.1.1955, AB12/131.

151 Allen K W, "Fusion Reactors" June 1955 and "The Fusion Programme" 25.8.55, CAC.

152 Fry to Cockcroft, 1.6.55, AB6/1434.

153 Cockcroft to Plowden, 18.9.56, *ibid*; AERE-GD M18 8.11.56, AB12/131; AERE report X/PR 2150 (19).

154 Interview Bodin - Hendry, 1981.

155 Hawkins P O, "Some Thoughts on Fusion/Fission Reactors and Ordered Beams" in AERE GP/R 2073, AB15/5342.

156 Bodin H A B, Atomkernenergie 19 175 (1972).

157 Curran S C, Allen K W, Bodin H A B, Fitch R A, Peacock N J and Reynolds J A, Geneva Conference 31 365.

158 AERE reports XPR/2150 *passim*.

159 AERE-GD contract meeting 10.2.1956, AB12/131.

160 Cockcroft to Plowden 30.12.1955, AB6/1434.

161 AERE report X/PR 2150 (18).

162 AERE-GD M16 and 17, AB12/131.

163 "The Production of Controlled Thermonuclear Energy", AERE report GP/R 2073, 1956; eds., Allen J E, Hide R, and Reynolds P, AB15/5342. The following papers were presented:

Cockcroft, Sir John, Introduction
Thomson, Sir George, The Nuclear Physics of the Problem
Lawson J D, Energy Balance
Thonemann P C, The Gas Discharge Method, (a) Quasi Stationary Discharges
Ware A A, The Transient Type of Discharge
Thomson W B, Theoretical Aspects of the Gas Discharge Project
Blacket P M S, Research at Imperial College (A)
Latham R and Wheeler C B, Research at Imperial College (B)
Oliphant M L, Resumée of Recent Discussions in Moscow
Hawkins P O, Possibilities of Ordered Beams
Millar W, Shock Waves
Pease R S, Problems Requiring Further Study
Fry D W, Summary and Concluding Remarks.

164 AERE-GD M17 31.7.1956 *ibid*; transcript of discussions in AERE-GD, AB12/131; Fry to Cockcroft, 9.11.56, AB6/1434.

165 AERE-PDSC (M)18, 3.11.1950, AB12/103; Fry to Chick 3.3.1952, AB6/350.

166 AERE-PDSC March 1954, AB12/206.

167 Interviews Flowers - Hendry, Thonemann - Hendry and Davenport - Hendry 1981.

168 AERE-PDSC 154, 10.3.54, AB12/55.

169 Flowers to Cockcroft 25.1.54, AB6/785.

170 Fry to Flowers, memo 26.1.1955, AB6/785; Cockcroft to Plowden 11.9.56, AB6/1434.

171 Memo 24.5.1956, AB6/785.

172 AERE-PDSC (M)21, 7.8.51, AB12/103.

173 Beckerley to Longair 25.8.52 et seq. AB6/785.

174 Fry - Cockcroft 27.6.1951, AB6/857 and 11.9.51, AB6/785; AERE-PDSC (M)21, 7.8.51 and AERE-PDSC 103, AB12/103.

175 Davies to Dunworth 27.1.1955, AB6/1176.

176 Penney to Cockcroft 20.6.1956 and Cockcroft to Penney 21.6.1956, AB6/785.

177 Fry to Cockcroft 1.6.1955, AB6/1176 and Cockcroft to Plowden 11.9.1956 AB6/1434.

178 Fry to Cockcroft 7.10.1955, AB6/1434.

179 Penney to Cockcroft 2.1.1956 and Allibone to Cockcroft, 29.11.1955, *ibid*.

180 Cockcroft to Plowden, 4.10.1955, Cockcroft to Penney 29.12.1955, and Penney to Cockcroft 2.1.1956, *ibid.*

181 Cockcroft to Penney 4.1.1956, *ibid.*

182 Cockcroft to Plowden 30.12.1956, *ibid*; report of meeting 30.4.1957, AB6/2139.

183 Proceedings of the Conference on Controlled Thermonuclear Reactions, Harwell, June 1957, AERE Report GP/R2371, eds. Burton W M, Davenport P A and Paul J W M, AB15/5385-6.

184 UKAEA, 2nd Annual Report, para. 112, HMSO 1955-56.

185 Allibone - Cockcroft 29.11.1955, AB6/1434.

186 Cockcroft to Plowden 11.9.1956, *ibid;* Thonemann memo., 13.9.1956, AB6/1842.

187 Schonland to Plowden, 18.10.55; Cockcroft, memo., 30.4.1956; Cockcroft to Willis 15.6.1956; Penney to Cockcroft 20.6.1956; Cockcroft to Penney 21.6.1956, AB6/785.

188 Cockcroft, memo 30.4.1956, *ibid;* Cockcroft to Plowden 21.6.1956, AB6/1842.

189 UKAEA press release, 24.1.1956, AB6/1434.

190 Cockcroft to Willis 15.6.1956, AB6/785; 8.6.1956, AB12/343.

191 The Anglo-American nuclear relationship is fully examined in ref. 69 and by Simpson in "The Independent Nuclear State", (Macmillan, 1986).

192 These papers were published in Proc. Phys. Soc. B 1957. The authors and titles are:

Thompson W B, Thermonuclear Reaction Rates, p.1.
Lawson J D, Some Criteria for a Power Producing Thermonuclear Reactor, p.6.
Pease R S, Equilibrium Characteristics of a Pinched Gas Discharge Cooled by Bremsstrahlung Radiation, p.11.
Allen J E, An Elementary Theory of the Transient Pinched Discharge p.24.
Tayler R J, Hydromagnetic Instabilities of an Ideally Conducting Fluid p.31.
Carruthers R and Davenport P A, Observations of the Instability of Constricted Gaseous Discharges, p.49.

193 Cockcroft to Plowden, 21.6.1956, AB6/1842.

194 Cockcroft to Plowden, 2.5.1956, AB6/785; Strauss to Plowden 25.7.1956, AB6/1842.

195 Cockcroft, memo, 30.4.1956, AB6/785.

196 AERE-HC 9.10.1956, AB12/343; Willis to Perrin 6.9.1956; Cockcroft to Plowden 12.9.1956 and Plowden to Willis, 28.9.1956 AB6/1842.

197 AERE-PDSC 213, 18.7.1956, AB12/206; memo 20.11.1956, AB6/785.

198 AERE-PDSC 219, Dec. 1956, AB12/206.

199 Bromberg, ref. 13, p.73.

200 Memos 14.11.1956 and 11.12.1956 and Thomson to Cockcroft 19.1.1957, AB6/2139.

201 Thomson to Cockcroft, 8.3.1957, AB6/1842; Fry to Cockcroft 16.2.1957 AB6/2139.

202 Memo 30.4.1957, AB6/2139.

203 AB6/2139 *passim*.

204 Gibson A, Nature, **181** 803 (1958).

205 Carruthers, Harding, Pease and Thonemann, memo 6.9.1957, AB6/1434.

206 Fishenden to Schonland 22.8.1957, AB6/1980.

207 Schonland to Cockcroft 4.9.1957, AB6/1994.

208 Schonland to Cockcroft 5.9.1957, *ibid*; Schonland to Cockcroft 6.9.1957, and 8.9.1957 AB6/1847.

209 Cockcroft to Plowden 5.9.1957, AB6/1994.

210 Plowden to Macmillan 9.9.1957, *ibid*.

211 Manuscripts of talks to British Association, Dublin, CAC.

212 Nature, **180**, 780 (1957).

213 Financial Times 7.9.1957.

214 Daily Telegraph and Morning Post, 7.9.1957.

215 News Chronicle 7.9.1957.

216 Macmillan to Plowden 9.9.1957, AB6/1434.

217 Fishenden to Schonland 7.9.1957 and Thonemann to Fishenden 10.9.1957, *ibid*.

218 Cockcroft to Willis 17.9.1957, AB6/1980.

219 Willis to Cockcroft 11.9.1957, AB6/1980.

220 Allen to Willis 10.9.1957 and Cockcroft to Willis 17.9.1957, *ibid.*

221 Thonemann to Cockcroft 17.9.1957, *ibid.*

222 Carruthers, Harding, Pease and Thonemann, memo 17.9.1957, AB6/1994.

223 AEA, 19.9.1957, CAC.

224 Plowden to Fry 25.9.1957, AB6/1994.

225 Cockcroft to Willis 20.9.1957, Cockcroft to Thonemann 23.9.1957, AERE to "Nature" 27.9.1957, "Nature" to AERE 30.9.1957, all in AB6/1980.

226 Gaunt to Cockcroft 26.9.1957, AB6/1980.

227 Fry to Plowden, AB6/1994.

228 Arnold L, "Windscale 1957: Anatomy of a Nuclear Accident", (Macmillan, London, 1992).

229 Killian J K, "Sputnik, Scientists and Eisenhower" (MIT press, Cambridge, Mass. 1977).

230 Daily Express 16.10.1957.

231 Financial Times 25.10.1957.

232 Fishenden to Gaunt 18.10.1957, Gaunt to Peirson, 18.10.1957 and draft press release 19.10.1957, AB6/1980.

233 Gaunt to Fishenden 31.10.1957 and Caccia to Plowden 31.10.1957 *ibid.*

234 Fishenden to Gaunt 14.11.1957 *ibid.*

235 Cockcroft to Gaunt 28.11.1957 and Fishenden to Fry 11.12.1957 *ibid.*

236 Statement in House of Commons 11.11.1957, "Hansard" and AB6/1980.

237 Strauss to Plowden, 20.11.1957, AB6/1980 and Washington Post 20.11.1957. See also News Chronicle 26.11.1957 and Allen to Gaunt 26.11.1957, AB6/1980.

238 Plowden to Strauss 21.11.1957, AB6/1980.

239 Press Statement 22.11.1957, *ibid.*

240 Statement to House of Commons 27.11.1957, "Hansard" and AB6/1980.

241 Draft Statement 5.12.1957, AB6/1980.

242 Gaunt to Cockcroft 27.11.1957 and 29.11.1957, *ibid.*

243 Gaunt to Cockcroft 2.12.1957, and meeting of 7.12.1957, *ibid*. See also Thonemann to Jackson 3.12.1957, *ibid*.

244 Fishenden to Gaunt 10.12.1957, and Allen to Pye 6.12.1957, *ibid*.

245 Meeting of 7.12.1957, *ibid*.

246 Gaunt to Allen 12.12.1957, *ibid*.

247 New York Herald Tribune 12.12.1957.

248 Daily Express 16.12.1957.

249 WVNS 15.12.1957, AB6/1980.

250 See for example Cockcroft to Plowden 18.12.1957, and Dean to Plowden 23.12.1957, *ibid*.

251 AB6/1980 *passim*.

252 Six papers were published in the Jan 25 1958 issue of "Nature" (vol. 181) under the title "Controlled Release of Thermonuclear Energy". The first of these was "Production of High Temperatures and Nuclear Reactions in a Gas Discharge" by Thonemann P C, Butt E P, Carruthers R, Dellis A N, Fry D W, Harding G N, Lees D J, McWhirter R W P, Pease R S, Ramsden S A, and Ward S, p.217. The third paper, which followed a short but important cautionary discussion on heating entitled "Co-operative Phenomena in Hot Gases" by Lyman Spitzer of Princeton, described the AEI work under the title "A Stabilized High-Current Toroidal Discharge Producing High Temperatures" by Allen N L, Allibone T E, Chick D R, Hemmings R F, Hughes T P, Kaufman S, Liley B S, Mack J G, Miles T H, Payne R M, Read J E, Ware A A, Wesson J A, and Williams R V, p.222. Then follow four experimental papers describing work at Los Alamos on linear and toroidal pinches.

253 Fry to Cockcroft 17.1.1958, AB6/1980.

254 AB6/1980 and AB6/2127 *passim*.

255 Fry to Chick 3.2.1958, AB6/2127.

256 "Facts about Zeta", UKAEA 1958, CAC.

257 Interviews Thonemann - Hendry, Pease - Hendry and Davenport - Hendry 1981; A file of papers for delegates to the Press Conference is in CAC.

258 Saxton to White, 24.1.1958, AB6/1980.

259 Press cuttings 25.1.1958 - 27.1.1958, AB6/1847, AB6/1980 and AB6/2127.

260 AERE-GD M21, 7.11.1957, AB12/131; AERE report X/PR 2150 (25 ff).

261 Gibson A, Nature 181 803 (1958). Agenda of this meeting in CAC.

262 Bickerton R J and London H, Proc. Phys. Soc. 72A, 116 (1958).

263 Fishenden to Schonland 5.5.1958, AB6/2127.

264 Thonemann to Furth 9.4.1958, AB6/2194.

265 Gibson A, Nature 183, 101 (1959).

266 Rose B and Taylor A E, Nature 181 1630 (1958).

267 Atom, No. 21, July 1958, p.2, CAC.

268 Artsimovich L A, Geneva Conference 31 6.

269 Maddox J, "A Plain Man's Guide to Zeta", Manchester Guardian 1958, CAC.

270 Ware A A, "Sceptre III A", Proc. IEE 106A Suppt. 2 30 (1959).

271 PRO number not yet allocated. List of titles in CAC.

272 See ref 111. The first mention of Sceptre III occurs in AERE 2150 (22), Nov. 1956 - Feb. 1957.

273 Memo of 8.8.1958, and Plowden to Cockcroft, 11.8.1958, AB6/2143; AEX 15.8.1958, CAC.

274 AEX *ibid*; Levin to Penney, 9.6.1960, AB6/1994; see also J M Weale et al., Jnl. Nuclear Energy A/B 14 91 (1961).

275 Gaunt to Plowden, 14.8.1958 and AEA press notice 38/58, 30.8.1958, AB6/1994; AEX *ibid*.

276 AB6/2194 *passim*.

277 Carruthers to Thonemann, 14.12.1958, and Allen to Thonemann 16.3.1959, AB6/2194.

278 Blackett to Cockcroft 7.3.1958, AB6/428.

279 AERE-GD 22-25, AB12/131.

280 SNR Fusion Meeting, AWRE Aldermaston 20.3.1958, CAC; Fitch R A and McCormick N R, IEE Convention 117; Bodin to Lawson 27.10.1992, CAC.

281 Thonemann to Spitzer, 22.1.1958, AB6/2194; AB6/1894 *passim*.

282 The information given below is taken primarily from the proceedings of the Second Geneva Conference, 31, 32. These volumes are summarized, with emphasis on experiments, in a book "Nuclear Fusion" by Allis W P, (Van Nostrand, 1960). However, the American work is conveniently summarised in A S Bishop, "Project Sherwood" (New York, 1958), and the Soviet work in I V Kurchatov, Jnl. of Nuclear Energy, 8 168 (1958), and in M A Leontovich, ed., "Plasma Physics and the Problems of Controlled Thermonuclear Reactions", 3 volumes in translation. (Pergamon Press, Oxford 1959). Useful introductory summaries may be found in papers by Ware A A, Engineering 196 796 (1958); Bickerton R J, *ibid*. p.823; Robson A E, New Scientist 4 657 (1958).

283 Tayler R J, Proc. Phys. Soc. **B70** 1049 (1957).

284 Rosenbluth M N, Geneva Conference **31** 85.

285 Suydam B R, *ibid* p.157.

286 Bernstein I B, Freeman E A, Kruskal M D, and Kulsrud R M, Proc. Roy. Soc. **A244** 17 (1958).

287 Laing E W, AERE report T/M161 (May 1958).

288 Rosenbluth M N, Los Alamos report LA-2030, (1956).

289 Shafranov V D, Jnl. Nuclear Energy **2** 86 (1957).

290 AB6/2139 *passim*.

291 Tayler R J, AERE lecture series L/102-8, CAC.

292 Post R F, Geneva Conference **32** 245.

293 Barnett C F, Bell P R, Luce J S, Shipley E D and Simon A, Geneva Conference **31** 298.

294 Artsimovich L A, Geneva Conference **31** 6.

295 Elmore W C, Little E M and Quinn W E, Geneva Conference **32** 337.

296 Glasstone S and Lovberg R G, "Controlled Thermonuclear Reactions", Van Nostrand 1960.

297 For the later evolution of the UK and European fusion strategy see D Willson, "A European Experiment" (Adam Hilger, Bristol, 1981).

298 Cockcroft to Plowden 19.3.1957, and Cockcroft to Lockspeiser, 19.3.1957, AB6/1982.

299 Cockcroft to Peirson 29.11.1957 *ibid*.

300 Peirson to Cockcroft 28.11.1957 *ibid.*

301 Cockcroft to Melville 12.3.1958 *ibid.*

302 Thonemann to Adams 24.2.1958 and Adams to Thonemann, 27.2.1958, AB6/1894; Cockcroft to Plowden 25.3.1958, AB6/1982.

303 Cockcroft to Melville 3.4.1958, AB6/1982.

304 Cockcroft to Melville 12.3.1958 and Melville to Cockcroft with note to Verry 31.3.1958, *ibid.*

305 Cockcroft to Melville 3.4.1958 *ibid.*

306 Verry to Cockcroft 29.4.1958 *ibid.*

307 Thonemann to Cockcroft 2.5.1958, AB6/1982.

308 Paper CERN/269 and Fry to Cockcroft June 1958, AB6/1982.

309 Fry to Blackett 24.6.1958, Cockcroft to Peirson 26.6.1958, Peirson to Cockcroft 25.6.1958, and CERN/278 rev., AB6/1982.

310 Fry to Cockcroft 1.7.1958, AB6/1982.

311 AB6/1982 *passim.*

312 CERN 59-16, AB6/1982.

313 AEA (59)61; AEA 20.3.1958 and 1.5.1958, CAC.

314 Fry to Schonland undated, AB6/1846 (covering Allibone to Schonland, 11.5.1959).

315 AERE-GD 22, 14.1.1958, AB12/131.

316 Pease to Fry 15.5.1959, AB6/1846.

317 Penney to Allibone 4.7.1959, CAC and 15.9.1959, AB6/1846.

318 Pease to Schonland 6.10.1957, AB6/1846.

319 Penney, note of 14.10.1959, *ibid.*

320 AERE-RGMB (60)73, AB12/483 and (62)34 AB12/540; AERE-RGMB 16.8.1960, AB12/483 and 15.5.1962, AB12/513.

321 AERE-RGMB (63)10; AERE-RGMB, 19.2.1963. See also Niblett, ref. 35.

322 AWRE-SNR fusion meetings, 19.12.1957, 16.1.1958, CAC; AERE-HSC, 13.1.1958, AB12/350.

323 AEX 17.4.1958, CAC.

324 AWRE-SNR fusion meeting 10.7.1958, CAC.

325 Niblett G B, AWRE report NR/P-958, AB12/329.

326 CTRAC (58) 10, AB12/329, CAC.

327 CTRAC (58) 8, AB12/329, CAC; Niblet G B F, "Rapid Compression of a Plasma with Azimuthal Currents", IEE Convention 152.

328 Sweetman D R, Nucl. Fusion Suppt. Pt. 1 279 (1962).

329 Fry to Flurscheim, 6.7.1956, Flurscheim to Fry, 20.7.1956, and Carruthers, memo 31.8.1956, AB6/1896.

330 Carruthers R C and Hartill E R, "Zeta II, Some Design Parameters for a Thermonuclear Reactor", MV972/3, 25.10.1956, AB12/315.

331 Fry to Cockcroft, 18.9.1957, AB6/1994; Fry to Schonland, 25.10.1957, AB6/1978.

332 AERE-CTRAC, 26.11.1957, Appendix, AB12/329.

333 Cockcroft J D, "The Next Stages with Zeta", New Scientist, Jan 30 1958.

334 AB12/384 *passim*.

335 Interviews Carruthers - Hendry 1982, Thonemann - Hendry 1981 and Fry - Hendry 1981.

336 AERE-HSC, 17.12.1957, AB12/350.

337 Schonland to Owen 9.12.1957, AB6/1896.

338 Flowers to Cockcroft 18.12.1957, AB6/1994; CTRAC (58) 9, AB73/1.

339 Cockcroft to Flowers 23.12.1957, AB6/1994.

340 AERE-HSC, 13.1.1958, AB12/350.

341 Flowers, memo of 21.1.1958, AB6/1994.

342 AEX 17.4.1958 and AEA(58)28, AB6/1994.

343 *Ibid*; AEX 15.5.1958 and AEA(58)28 rev, note of meeting *circa* 12.5.1958, all in AB6/1994.

344 AEA press release, 8.9.1958, AB6/1994.

345 Thonemann to Fry, 11.8.1958, AB6/2144.

346 Fry to Cockcroft, 18.8.1958, AB6/2144.

347 Mandl to Cockcroft, 17.9.1958, *ibid.*

348 Thompson, memo of 24.10.1958, Ralph and Johns memo of 24.10.1958, and Thonemann to Schonland, 3.11.1958, *ibid.*

349 AERE-RGMB, 21.10.1958, AB12/409.

350 AERE-HC(58)96, Thonemann, memo of 3.11.1958, and Bretscher, memo of 5.11.1958, AB6/2144.

351 Dolphin to Schonland, 10.11.1958, *ibid.*

352 AEA (58) 28, AB6/1994.

353 AERE-GD M24, 5.11.1958, AB12/131; Carruthers R, Progress Report for Zeta 2, 12.8.58, AB12/384; Smart D L, AERE memo Z/M213, in part, CAC.

354 Allen J E and Bickerton R J, Patent 832, 270. Application date 20.6.57, complete specification filed 19.6.1958.

355 Technical details of Zeta II may be found in AB12/315, AB12/384, AB77/10, 13 and 17.

356 AERE-RGMB, 18.11.1958, AB12/409.

357 Perrott to Schonland, 5.12.1958, AB6/2144.

358 Le Cren to Schonland, 3.12.1958 and 11.12.1958, *ibid.*

359 Dolphin to Schonland, 20.2.1959, Perrott to Dean, 25.2.1959, and Perrott to Penney, 5.3.1959, *ibid.*

360 Dolphin to Schonland, 3.4.1959, AB6/2144; AERE-RGMB (59)55 and 28.4.1959, AB12/447.

361 AEA, 14.5.1959 and AEX, 14.5.1959, AB6/1994.

362 AB6/2139, *passim*; AERE-TTPC(59)13, AB12/461.

363 AERE-HC(59)1 and 29.12.1958, AB12/343; AEX(59)2 and 2.1.1959, both in AB6/1994.

364 Membership and terms of reference of this committee may be found in the 5th Annual Report of the AEA (1958-9), paragraph 120, and in AB12/139.

365 Thonemann to Le Cren 27.2.1959, AB6/2151.

366 AEA(59)39, AB6/2292.

367 Thonemann to Schonland 3.1.58, AB6/2151.

368 Chart dated 12 April 1960, AB6/2151.

369 AEA, Annual Reports Nos. 4 and 5, HMSO, London.

370 Interview Roberts - Hendry 1981.

371 Interview Thonemann - Hendry 1981.

372 Interviews, Fry and Pease - Hendry 1981.

373 Interview Penney - Hendry 1982.

374 Schonland to Penney, 1.4.1959, and Schonland to Thonemann, 15.6.1959, AB6/1994.

375 Schonland to Penney, 10.2.1959, CAC, and 1.4.1959, AB6/1994.

376 AERE-11/Conf/153 (4)-(8), AB6/2139.

377 Interviews Pease, Roberts - Hendry.

378 See, for example, various AWRE Theoretical Physics Notes on pinches, Thetatron, and magnetic traps, 1958-9; titles in CAC.

379 Francis G, "A Stability Experiment at Intermediate Currents", TTPC(59) 20, AB12/462.

380 AERE-CTRAC (58) 8, AB12/329, CAC.

381 AERE-CTRAC (58) 12, AB12/329, CAC.

382 *Ibid* CTRAC (58) 13, CAC.

383 *Ibid* CTRAC (58) 7, CAC.

385 AB6/1994, CAC.

386 AERE-RGMB (59) 34, AB6/2292, CAC.

387 RGMB (59) 35, AB12/461.

388 AERE-TTPC M1 and TTPC/1, AB12/461, CAC.

389 Thonemann - Hendry 24.6.81, CAC.

390 AERE-RGMB (59) 54, AB12/461, CAC.

391 *Ibid* RGMB (59) 39, CAC.

392 AEX 15.5.59, AB6/1994.

393 Willson - Pease 16.7.59, AB6/2292.

394 Note by Thonemann, 12.3.59, AB6/2347.

395 Interview Carruthers - Hendry 1982.

396 Schonland to Penney 31.5.1960 and Le Cren to Schonland, 6.4.1960, AB6/2292; interview Carruthers - Hendry 1982.

397 AB6/2292 *passim*.

398 Pease to Schonland and annotations 21.10.1959, *ibid*.

399 Willson to Schonland 1.2.1960, AB6/2444.

400 Schonland to Perrott, *ibid*.

401 Note of 29.7.1960, but see also Carruthers to Penney 10.8.1960, *ibid;* AERE-TTPC, 24.8.1960, AB6/2292 and AB12/451.

402 Carruthers, memo 10.8.1960, *ibid;* interviews Carruthers and Penney - Hendry 1982.

403 AEA(59)61, AB6/2292; Schonland, memo 17.8.1960, AB6/2292.

404 "Proposals for an Establishment for Controlled Thermonuclear Research", P C Thonemann 12.3.1959, CAC.

405 Note of 27.8.1959 and Schonland memo, 9.11.1959, AB/2151. Adams to Schonland 14.1.1960, AB6/1994.

406 AERE-TTPC, 24.8.1960, AB12/461.

407 Interviews Thonemann - Hendry 1981, Roberts - Hendry 1981.

408 Cockcroft to Penney 29.1.1960, AB6/2292.

409 Lomer to Penney 2.2.1960, *ibid*.

410 Penney to Cockcroft 5.2.1960, *ibid*.

411 Transcript of Press Conference, *ibid*.

412 Penney to Chadwick 9.8.1960, and Chadwick to Penney 10.8.1960, *ibid;* Chadwick and Thomson had already been consulted on the role of ICSE generally; Penney to Chadwick, 12.7.1960 and Penney to Thomson, 12.7.1960, *ibid*.

413 Roberts to Penney 10.8.1960 and 11.8.1960 CAC, *ibid;* interview Roberts - Hendry 1981.

414 Curran to Schonland 14.12.1959 et seq., AB6/1994.

415 Pease to Schonland 22.12.1959 and Schonland to Pease, 30.12.1959, AB6/2292.

416 See for example AB6/2151 *passim.*

417 AEA(60)86 and AEA, 1.9.1960, AB6/2292.

418 Press notice, 28.9.1960, AB6/2292 and "Atom" Nov. 1960, p.4.

419 AB88/18, AB77/20. Other files, for example containing a complete set of ICSE technical memoranda, have not yet been deposited in the PRO.

420 Allen to Penney, 30.8.1960 *et seq.* AB6/2292; Adams to Penney, 9.9.1960 and 21.6.1960, AB6/2444; AEX(60) 135, CAC.

421 Adams J B, Culham Laboratory First Planning Report, CLM-M1, January 1961. To be deposited in PRO; CAC.

422 Cable of 28.9.1960, Washington - Harwell and annotations, AB6/2292.

423 Note of letter Makins to Hailsham 7.9.60 *ibid,* AB6/2131.

424 AB6/2444 *passim.*

425 Carruthers R, Interdisciplinary Science Review 6 127 (1981).

HMSO publications are available from:

HMSO Publications Centre
(Mail and telephone orders only)
PO Box 276, London, SW8 5DT
General enquiries 071-873 0011
Enquiries about previously placed orders 071-873 0022
(queuing system in operation for all numbers)

HMSO Bookshops
49 High Holborn, London WC1V 7HB 071-873 0011 (Counter service only)
258 Broad Street, Birmingham B1 2HE 021-643 3740
Southey House, 33 Wine Street, Bristol BS1 2BQ 0272-264306
9-21 Princess Street, Manchester M60 8AS 061-834 7201
80 Chichester Street, Belfast BT1 4JY 0232-238451
71 Lothian Road, Edinburgh EH3 9AZ 031-228 4181

HMSO's Accrediting Agents

and through good booksellers

PRICE £7.95

ISBN 0-7058-1664-8

Printed in England