Technical Report
RAL-TR-97-011

CLRC

Early British Synchrotrons, An Informal History

J D Lawson

February 1997

COUNCIL FOR THE CENTRAL LABORATORY OF THE RESEARCH COUNCILS

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ISSN 1358-6254

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EARLY BRITISH SYNCHROTRONS,

AN INFORMAL HISTORY

Presented as the third

PICKAVANCE MEMORIAL LECTURE,

at

Rutherford Appleton Laboratory,

15 October 1996

by

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Preface

The material in this report is an extension of that presented by the author at the Birmingham Synchrotron 40th Anniversary Reunion, held on 16 September 1993. It is published both in the proceedings (ref 56) and independently as RAL report 94-10. The original paper dealt only with electron machines, but in the present report the Birmingham machine and early work carried out at Harwell on the first proton synchrotron for CERN are included.

The subtitle 'An Informal History' implies that much of the material not drawn from published sources relies on personal reminiscences rather than written records, which are very scarce and 'patchy'. This means that some accounts may be unbalanced, important events may be omitted, and credit may not be properly assigned. (The perils of relying on personal reminiscences rather than written records are emphasized by the authors of the formal 1300 page history of CERN, ref. 74 vol. 2 section 13.7). Nevertheless, use has been made of the archives of Birmingham University, the Public Record Office (PRO), where Harwell files are deposited, and CERN. (Harwell files specifically covering the Malvern work have not, however, been located). Additional material supplied by correspondents (sometimes as photocopies) has been deposited in the archives at Birmingham, CERN, or both (one copy being a photocopy) as appropriate. The location is given in the list of references.

The earliest section, on the work at Malvern, relies more than others on published papers and personal recollection. I should, however, like to thank Herbert Watson, a member of the original team, for some useful material, including the photograph used in Fig 11. To John Carver I owe thanks for information on the fate of the 30 Mev machine that was sent to Australia. For information on the Oxford machine I am indebted to Peter Stanley for discussions, and comments on the draft manuscript, and to John Moffatt for written comments. The photograph of the Glasgow machine was supplied by Ernest Laing, who also arranged for me to borrow the theses referred to in the text. Additional material was supplied by Edward Bellamy and James Lang.

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For the Birmingham machine I relied above all on helpful correspondence from members of the original team, especially Len Hibbard, with whom I was able to have an extended discussion, and John Symonds. Both commented on the draft manuscript, as did other team members in Australia who sent helpful comments, particularly David Caro, Colin Ramm and David Robertson. This correspondence has been placed in the archive of the Birmingham University Library.

Both Hibbard and Symonds sent photographic material; this is at present held by Mrs Van der Raay at the School of Physics and Space Science, and will later be placed in the archive of the University Library. Figures 21-24 are from photographs sent by Hibbard.

I am especially grateful to Bill Burcham for help and encouragement, and for arranging access to material located at the University of Birmingham. I also acknowledge the helpful assistance of the University staff.

For the section on the British work leading to the CERN proton synchrotron I should like to thank Kjell Johnsen for material from his files and comments on the draft, also Mervyn Hine for comments. I also thank Mme Rahmy for access to the CERN archives, and for the allocation of space for interesting material, much of which is referenced in this report. Formal acknowledgements for copyright material are due to the Bodleian Library, Oxford, for permission to reproduce Fig. 18 from Standley's thesis (ref 50), to the UK Atomic Energy Authority plc for Fig. 4 from ref. 14 and to the Institute of Physics for Figs. 1 and 7 and 19 from ref. 12.

I should like also to thank the Royal Society for a research grant in the History of Science, and the School of Physics and Space Sciences in the University of Birmingham for acting as sponsor. Finally, I wish to thank Paul Williams, Chairman and Chief Executive of the Council for the Central Laboratory of the Research Councils for allowing me the use of facilities there and agreeing to the publication of this work as a CLRC report.

Introduction

During the second world war Britain's nuclear physicists were deployed in research directed towards winning the war. Many were engaged in developments associated with radar, (or 'radiolocation' as it was then called), both at universities and at government laboratories, such as TRE and ADRDE at Malvern. Others contributed to the atomic bomb programme, both in the UK, and in the USA, mainly at Los Alamos and Berkeley, and at Chalk River in Canada. Besides contributing directly to the design of the bomb itself, British physicists were actively involved in other aspects, such as the techniques required for uranium isotope separation, and instrumentation to detect blast and radiation effects.

Towards the end of the war, when victory seemed assured, the nuclear physicists began looking towards the peacetime future. The construction of new particle accelerators to achieve ever higher energies was seen as one of the more important possibilities. Those working at Berkeley on the electromagnetic separator were familiar with the accelerators there, and following the independent invention (or discovery?) there of the principle of phase-stability by Edwin McMillan in 1945(1), exciting possibilities were immediately apparent. Indeed, even before this, Marcus Oliphant, while working on the electromagnetic separators at Oak Ridge, had put forward the idea of a ring magnet with frequency increasing with magnetic field to preserve synchronism(2), though he does not mention the essential feature of phase stability needed to make a very high energy machine a practical proposition. His idea was to accelerate protons to an energy of order 1 GeV, where he guessed that 'quite new phenomena would be observed'(3).

In November 1945, shortly after the setting up of the 'Department of Atomic Energy' under the Ministry of Supply, the first meeting of a 'Panel on Apparatus for Accelerating Particles' was held at the British Thompson-Houston laboratories under the chairmanship of Oliphant(4). This was organised by the former Department of Scientific and Industrial Research, and there were representatives from the Universities of Birmingham, Cambridge, Glasgow, Liverpool, London and Oxford, from industrial firms British Thomson-Houston, English Electric, the General Electric Company and Metropolitan-Vickers, and from the Government Laboratories TRE, NPL and the Royal Arsenal and from the Medical Research Council. There was wide ranging discussion, and though the synchrotron concept and plans for an ambitious proton machine with energy 1300 MeV at Birmingham were described by Oliphant, no other suggestions for building one were mentioned. Although this was the first machine to be planned and funded, it was not completed until 1953; here it will be described after the smaller and simpler electron machines which worked much earlier. There were, however, suggestions for betatrons; Professor Philip Dee was interested in one with an energy of 200 MeV for Glasgow University. There was much discussion of different linear accelerator concepts, and Dr L H Gray 'estimated that up to six machines with voltages of the order of 30 MeV might be required for medical purposes'. Remarkably, it is stated in these minutes that 'there was at present no great interest in further cyclotrons'; clearly the possibility of frequency modulated machines was not yet widely appreciated.

After this time events moved rapidly, especially at the newly established Atomic Energy Research Establishment (AERE) where building had started on a disused wartime airfield at Harwell in April 1946(5). By the end of the year design and construction of a synchrotron of energy 30 MeV was being considered(5).

Not all the physicists returned to their Universities after the war, some remained in government service and joined the Atomic Energy programme under the direction of John Cockcroft. The AERE physicists also wanted to build and use accelerators, and they had the advantage that many of the skilled technical staff at Malvern were keen to transfer, as well as those returning with Cockcroft from the Chalk River Laboratory in Canada. The staff working on the synchrotron remained temporarily at Malvern, together with those on the linear accelerator project and those in the electronics division, under the overall direction of Denis Taylor. The 110 inch frequency modulated cyclotron, a much larger installation that could not readily be moved, was started on the new site at Harwell. The question of the extent to which AERE staff could involve themselves in 'curiosity oriented' research as well as 'mission oriented' research directly relevant to the power and weapons programmes was a vexed one, and occasioned some argument, especially between Cockcroft and James Chadwick, who had returned to Liverpool University after his wartime role as Director of the British contribution to the bomb, under the code name of the 'Tube Alloys' project(6). Accelerators were, of course, relevant to the Atomic Energy programme also for more applied tasks, such as generating neutrons by photo-disintegration from high energy X-rays produced in turn by electrons accelerated to energies between 10 and 20 MeV. The determination of cross-sections of neutron induced reactions was clearly important in the design of reactors and their shielding(7,8).

2 Early Plans at Malvern: The World's First Synchrotron

By the end of 1946 a programme for synchrotron research had been outlined, and a group was being assembled at Malvern under Donald Fry, who had been in charge of the microwave aerials group at TRE. These early discussions had naturally involved the electrical engineering industry, in particular the larger companies, Metropolitan-Vickers, English Electric and British Thomson-Houston.

Following the American groups the advantages of a synchrotron for the photo-production of mesons was now recognised, and the idea of building a 200 MeV betatron at Glasgow was abandoned in favour of a 300 MeV synchrotron. (It should be recalled that at that time only one type of meson was known, the μ and π had not been distinguished). Dee requested help from Harwell, and it was agreed to start by building a machine at 30 MeV. This was partly to explore the design and operation of synchrotrons before embarking on the larger project, but it was recognized that interesting physics could be done with 30 MeV electrons and X-rays, such as studies of γ-n reactions and nuclear photo-disintegration. Further, interest was expressed by the medical community in the potential application to cancer therapy, and it was considered appropriate that Harwell should contribute to this work. Because of the long range of X-rays and their secondary electrons at these high energies the ionization density in an irradiated solid initially increases to a depth of about 8 gm/cm^2, thereafter decaying in a distance of a few times this amount. The possibility of treating deep-seated tumours without the excessive surface damage associated with conventional X-ray energies of a few hundred kilovolts was recognised.

By the end of 1946 a further request had been made to AERE; Lord Cherwell had written to Cockcroft in November asking for help with a machine of about 150 MeV to be built at the Clarendon Laboratory in Oxford(9).

In 1945 the work on radar had moved from Malvern College down the hill to 'The Duke', previously a Naval Station. One outpost remained, however, 'The Lees', a small self-contained area outside the main College grounds where huts had been erected to house the top secret countermeasures group. It was here that the synchrotron and linear accelerator were to be built and housed, together with the electronics group under Taylor, designing equipment specifically for Harwell. Recruiting staff for these new enterprises was vigorously pursued, and three key members of the synchrotron group soon moved into the Lees to start work there. In overall charge of the synchrotron under Fry was John Gallop, an electrical engineer with industrial experience needed for large scale items such as the magnet and its power supply. John Dain, from TRE, was to take responsibility for all the electronic controls and circuitry; Frank Goward, an expert in aerials from TRE was in overall charge of the physics, and the original team contained three others from TRE: John Wilkins and Herbert Payne were responsible for the resonator and RF system, and Herbert Watson was in charge of all vacuum aspects and the injector gun. Supporting this team at Malvern was the mathematician William (Bill) Walkinshaw, a member of the Harwell Theory group under Klaus Fuchs. Although Walkinshaw's main contribution was to the linear accelerator programme, he did contribute to the synchrotron work and his role in reviewing and explaining the various theoretical papers being published in the USA was invaluable.

By the time this team had become organized an American team at the General Electric Co at Schenectady in the USA was already well on the way to building what was to be the first synchrotron. Having already built several betatrons they were well acquainted with much of the technology required. At this time there was one betatron in the UK. This had been specially commissioned by A R Greatbach of the Woolwich Armament Research Laboratory during a visit in 1942 to Donald Kerst's Laboratory in the USA. He saw the possibility of using a small machine with sealed-off vacuum chamber for inspecting unexploded bombs that needed to be defused in situ. The betatron was designed by Kerst, and constructed in the University of Illinois workshops by Ernest Englund[10]. W H Koch, then a graduate student, assisted in the construction and tested the machine in its oil filled container box in the University Electrical Engineering Laboratory towards the end of 1943. Early in the next year he took it to Woolwich and installed it there. By that time, however, conventional bombing had given way to attacks by the V1 'flying bombs' and V2 rockets, and the machine was not used for its original purpose[11].

At this point it is convenient to summarize the principle of the betatron with reference to Fig. 1. An alternating current at the supply frequency is passed through the coils; a pulse of electrons is injected from the gun at the instant that the magnetic field at the equilibrium orbit is such that the Lorentz force just balances the centrifugal force. The

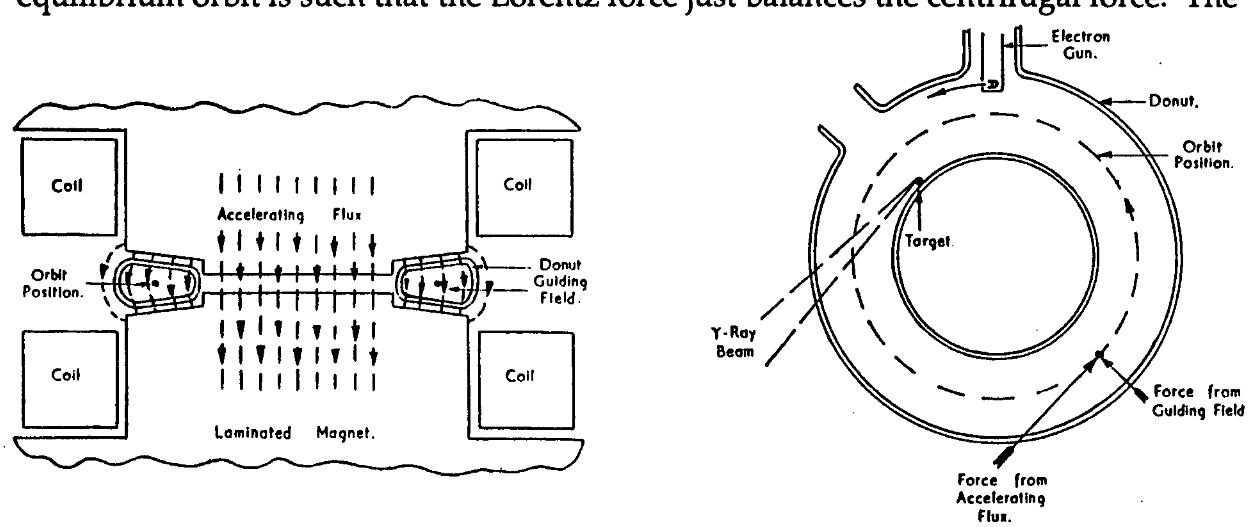


Fig 1 Schematic diagram, showing essential components of a betatron, from ref. 12.

orbit radius then remains constant as the field rises and the particle accelerates, provided that the total magnetic flux through the orbit is twice what it would be if the field were uniform at all radii, (the Wideröe 2:1 condition). Betatron oscillations about the equilibrium orbit are stable provided that the field at the orbit falls off with radius, but less rapidly than 1/r. Near the peak of the magnet field the iron within the orbit is designed to saturate, so that the orbit radius contracts and the electrons spiral inwards to strike a target and produce X-rays.

Returning to the betatron at Woolwich, Goward realized that this could be converted to a synchrotron by increasing the magnet current, so that saturation occurred earlier in the cycle, and building a resonator around the vacuum chamber (or 'donut') in the form of a shorted quarter-wave line with a gap in the inner conductor, tuned to a frequency equal to the speed of light divided by the circumference of the orbit. At the betatron energy of 4 MeV the electron velocity was already within 1% of that of light. Then, just as the iron begins to saturate, the RF would be switched on, accelerating the particles by means of the electric field across the gap to a higher energy. This is illustrated in Fig. 2. Goward accordingly assembled an RF power supply from units

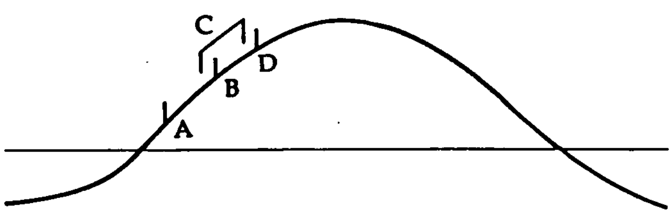


Fig 2 Magnetic field variation during positive half-cycle, showing (A) injection pulse, (B) output pulse for betatron operation, (C) radio frequency envelope and (D) output pulse for operation as synchrotron.

available at TRE, and constructed a simple resonator. The form of the resonator is indicated in Fig. 3. If the resonator were made of metal tubes, as indicated in the figure, eddy currents induced by the changing magnetic field would distort the guide field and the beam would be lost. It was therefore constructed of wires, joined only at one point by a planar ring parallel to the magnetic field. It was held together by dielectric spacers, and

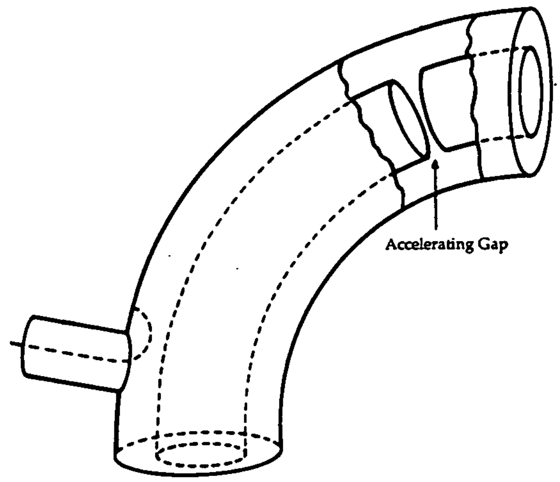


Fig 3 Schematic drawing of quarter-wave resonator. The actual resonator used was designed to fit round the vacuum chamber, and was constructed of wires to avoid eddy current loops. (Details in text).

made in two halves which clipped together around the toroidal vacuum chamber. With this very simple equipment Goward, together with D E Barnes of Woolwich Arsenal demonstrated synchrotron acceleration for the first time in August 1946, two months before the General Electric machine operated in the USA. Electrons were accelerated from the betatron energy of 4 MeV to 8 MeV(13).

The machine was moved to Malvern, and by replacing the coils, adding air cooling and providing a DC bias field it was possible further to increase the energy to 14 MeV(14). The X-ray intensity was greatly improved also by increasing the injection energy from 2 to 20 keV. A photograph of the modified machine from ref. 14 is shown in Fig. 4. With these modifications it was used both for general experiments on synchrotron operation, and for experiments on medical applications. Extensive studies were made of the distribution of ionization in materials simulating human tissue, with various filters and collimators.

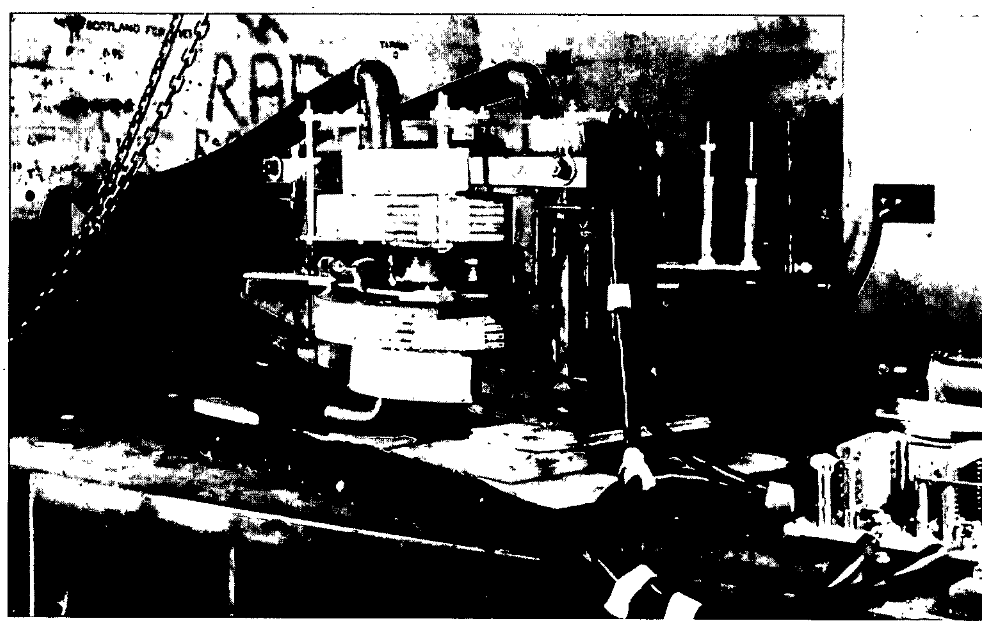


Fig 4 The world's first synchrotron, installed at Malvern. The extra cooling system and RF feed to the resonator may be clearly seen.

3 Design and Construction of the 30 MeV Machines

The practicability of synchrotron acceleration having been established by the end of 1946 by Goward and Barnes' experiment and the American General Electric machine, which first operated in October(15), what was now required to be done could clearly be seen. Construction of the first machine was well under way, and delivery of the magnet was expected during 1947. In January 1947 a fairly detailed specification of the parameters and work required had been prepared by Goward, Gallop and

Dain[16]. Some of the more important parameters of the first 30 MeV machine are tabulated below.

Energy at full excitation	30 MeV	Volts per coil	5 kV in series
X-ray output at 1 metre	10 Roentgens/min.	Current per coil	100A rms
Injection energy	10 keV	Resonant capacity	30 μF
Orbit radius	10 cm	Quality factor (Q)	50
Field at maximum energy	1T	Magnet weight	3 tons
Field index, $n = -(r/B)(dB/dr)$	0.7	Resonator frequency	477 MHz
Aperture of good field	6 cm square	Mean RF power	10 watts
Magnet coils	2 x 185 turns		

It was envisaged that several machines would ultimately be needed, and that these would be built by English Electric, who were building the magnet for the first one, to be assembled at Malvern. Two magnet designs were considered, an 'H' magnet and a more symmetrical 'C' magnet; eventually both types were constructed. The H magnet had the advantage of accessibility to the vacuum chamber but was less economical and less likely to produce a field with good azimuthal uniformity. Since access was considered very important in initial experiments, the H magnet design was chosen for the first machine. Both designs as ultimately built are shown in Fig. 5.

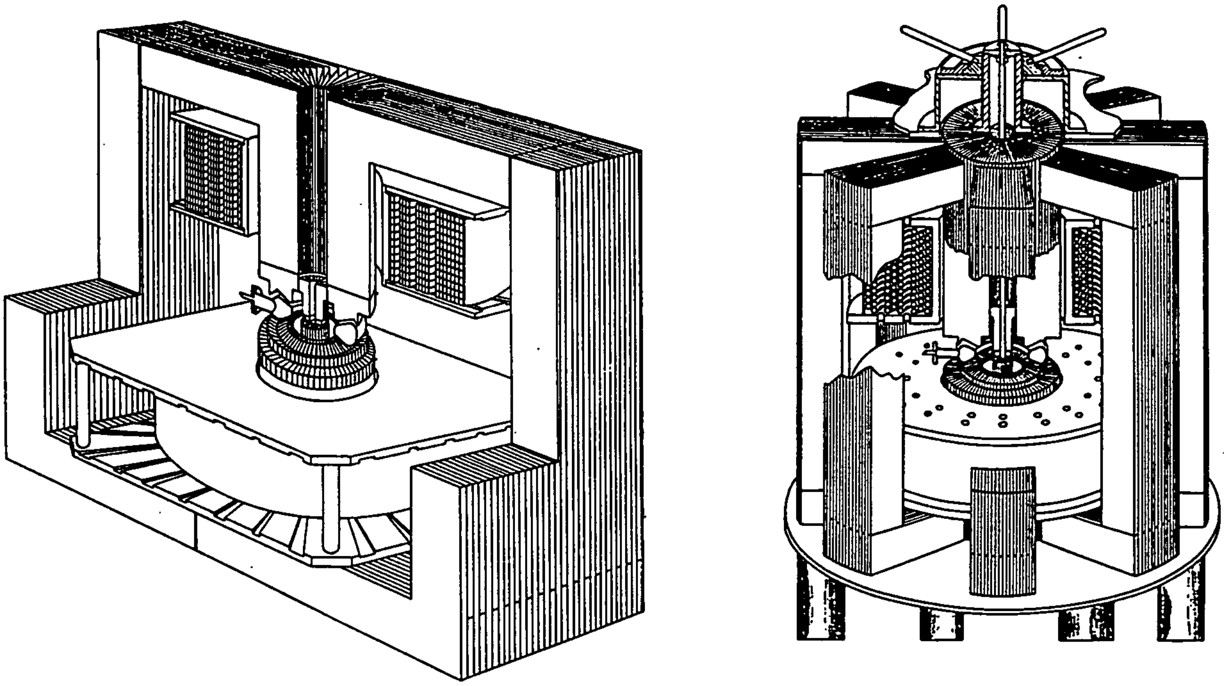


Fig 5 'H" and 'C' magnet designs for the 30 MeV synchrotrons built at Malvern [19].

The year 1947 was occupied not only with construction and commissioning the Malvern machine, but in analysing the expected performance and considering the problems of the 300 MeV machine for Glasgow. In this machine the ratio of available aperture width to radius was much less, implying tighter tolerances on field accuracy, and possible problems at the betatron-synchrotron transition, where synchrotron oscillations might cause loss by electrons striking the walls. Robert (Bob) Carruthers joined the group in this year, and started work on the RF system and magnet power supply. The magnet was to be a larger version of the 30 MeV C design, to be built by Metropolitan-Vickers, who together with the Malvern Group and Glasgow University, were to be responsible for overall design and construction. It was also agreed that the machine for Oxford would be built by English Electric, and technical help would be given by the Malvern group.

During the construction of the first 30 MeV machine there was activity analysing its expected performance, and that of the more critical larger machines. This was led by Goward, and a number of papers were published, particularly on pole face design, particle trapping at the betatron - synchrotron transition, the effects of magnetic field errors and ideas for beam extraction[17]. This problem appeared particularly difficult, and a number of suggestions had been published in the USA, some applicable to betatrons, where beams had already been rather crudely extracted. Work was also done at Oxford in preparation for the machine there by Thomas Kaiser and James Tuck, who also performed experiments on the 14 MeV converted betatron. Information from the American work, where papers on betatron operation had been published, and from the 70 MeV GE machine, which was working well, was also available. Eventually, after some constructional problems which delayed the delivery of the magnet until mid 1947, the first beam was obtained in October[18].

4 Design Features of the 30 MeV Machines

The design and operation of the 30 MeV machines, with both types of magnet, are described in two papers read before the Institution of Electrical Engineers in April 1950, and the numerous references to specialized detailed papers therein[19,20]. Design information quoted below is from these papers unless referenced otherwise. Features of the larger machines, then at an early stage, are also described, since for several items, such as the power supply and vacuum chambers, different techniques are required. Photographs of machines with H and C magnets are shown in Figs. 6 and 7, and a schematic diagram of components in Fig. 8. The greater compactness of the second design is clearly seen, but it is evident also that the vacuum chamber was less accessible for experiments, and furthermore, it was necessary to remove one of the C units in order to replace it. Another feature of this design, seen in Fig. 7, is that azimuthal magnet inhomogeneities could be more readily be corrected by 'trim' coils wound on the C's. Details of the magnets can be seen from Fig. 5. The magnet poles were designed to have a value of $n = -(r/B)\,(dB/dr)$ near to 0.7, to give a ratio of betatron oscillation frequency to rotation frequency $(1-n)^{1/2}$ of order 0.5. The necessary shape was found empirically from electrolytic tank measurements, and numerically by relaxation technique. Coils above and below the orbit carrying

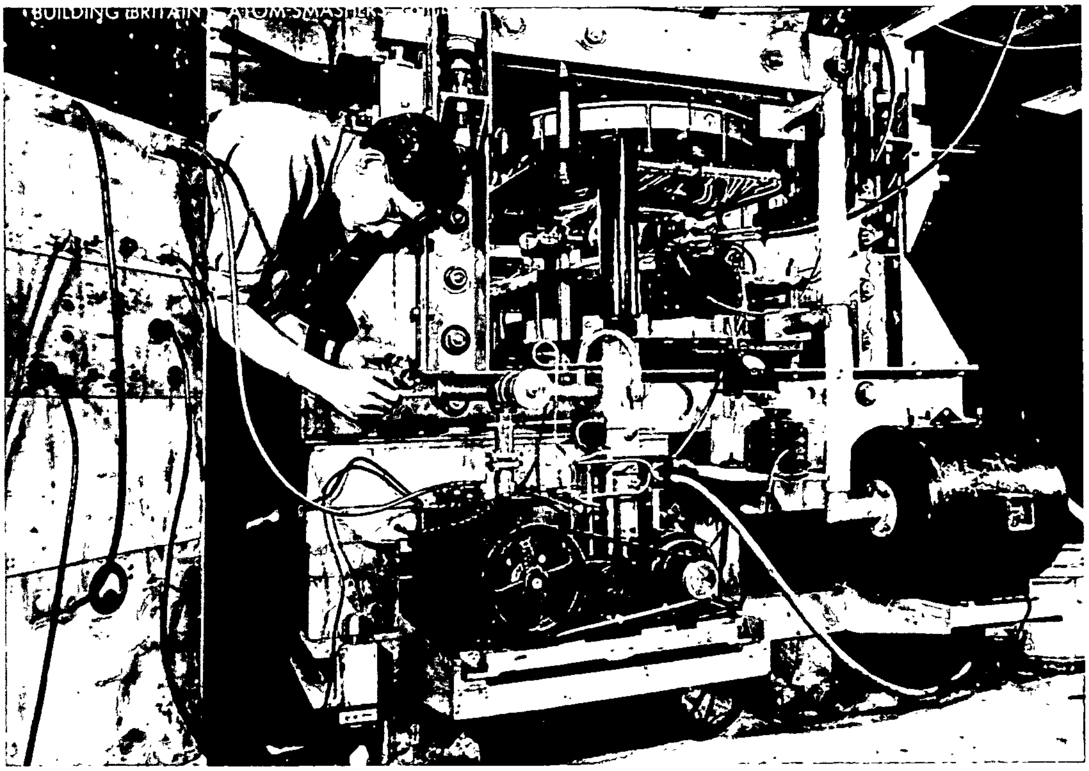


Fig 6 First 30 MeV machine at Malvern, with H magnet [21].

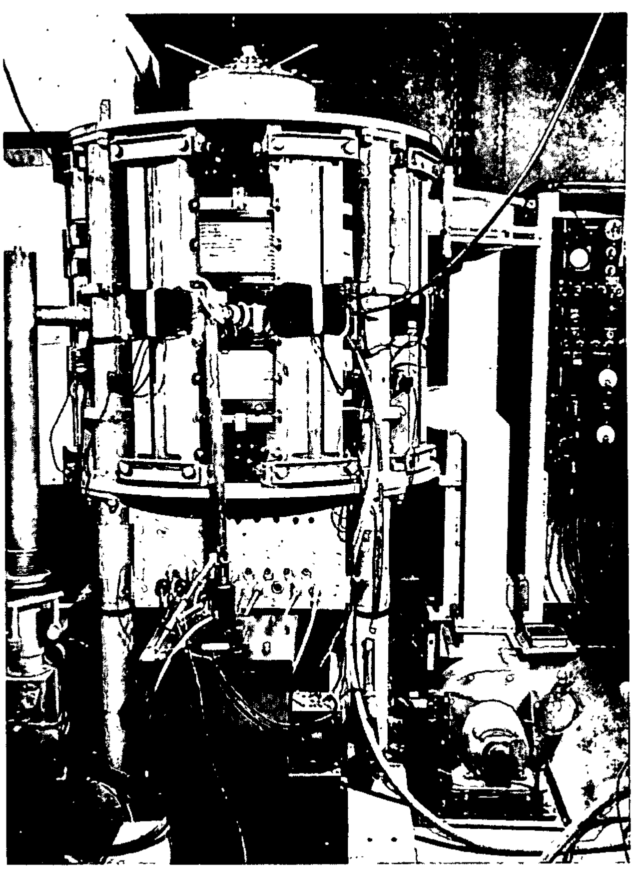


Fig 7 Second machine at Malvern, with C magnet. The greater compactness of this design, but reduced accessibility to the vacuum chamber is evident.

current proportional to the field were provided to enable the field gradient, and hence n, to be varied.

The energizing circuit in both cases was a series driven resonant circuit at the supply frequency controlled by a large manually adjusted variable ratio auto-transformer ('Variac'), (Fig. 9). 'Metrosil' was included for emergency voltage limitation, and trimming capacitors plus variable inductance were included for fine tuning. This was very necessary at the time, since the mains frequency was by no means stable; after 5 pm, when the industrial load was shed, the

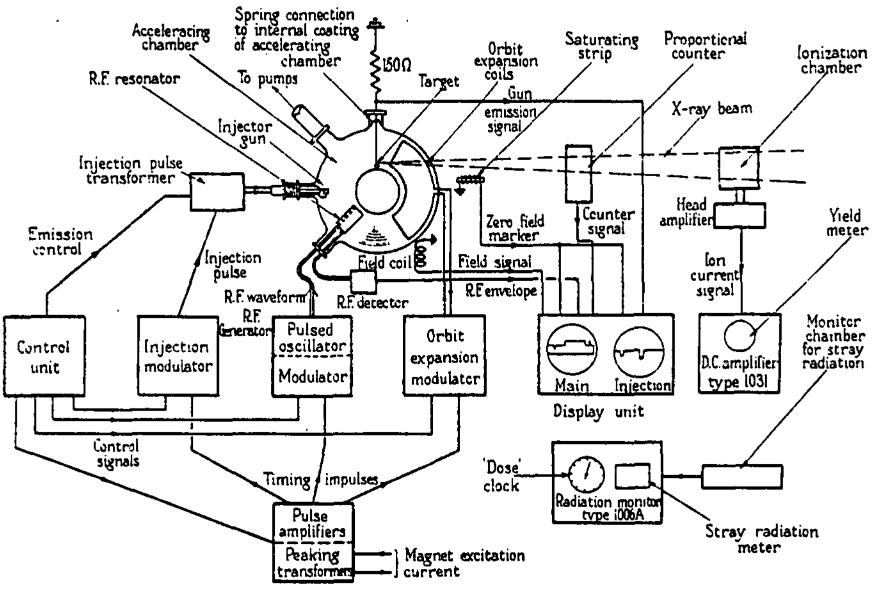


Fig 8 Schematic diagram of components of Malvern machines [19].

frequency increased; it was allowed to rise so that the total number of cycles in a 24 hour period was the same as if there had been no variation from 50 Hz.

The accelerating field was provided by a quarter-wave line resonator, made of silver plated on 'Faradex'. This is a ceramic with high dielectric constant, so that the resonator length was only 2cm enabling it to be easily inserted through the side arm. The silver coating was 20 microns thick, with a circumferential strip etched away to provide the accelerating gap. The coating was sufficiently thin that eddy currents produced negligible field perturbation of the guide field. The Q-factor was 500 at the operating frequency of 477 MHz. The resonator, shown in Fig. 10, was water cooled, and fed with a peak power of 60 watts, which provided 100 volts across the gap. The voltage gain per turn required for acceleration was about 20.

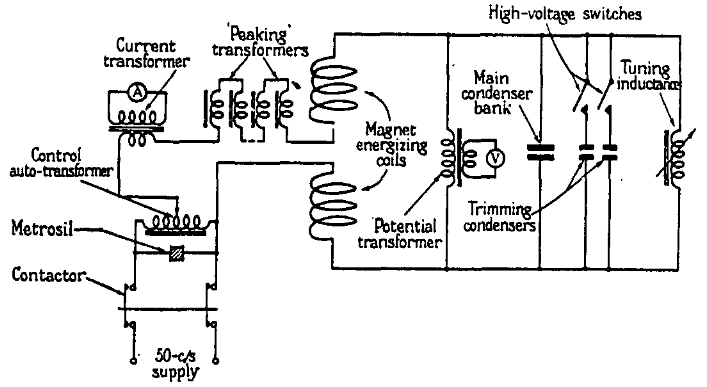


Fig 9 Magnet energizing circuit [19]

The injector gun, based on Kerst's original design, is shown in Fig. 11.[22] The cathode was a helix of 0.25 mm tungsten wire mounted within a semi-cylindrical 'Wehnelt' electrode, all of which was pulsed negatively, allowing electrons to pass through the vertical 1.8 mm gap in the surrounding earthed molybdenum shield. The gun could withstand up to 40 kV.

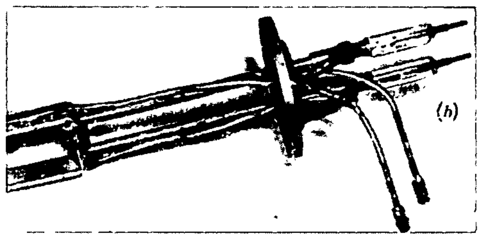


Fig 10 Resonator for Malvern machines [19]

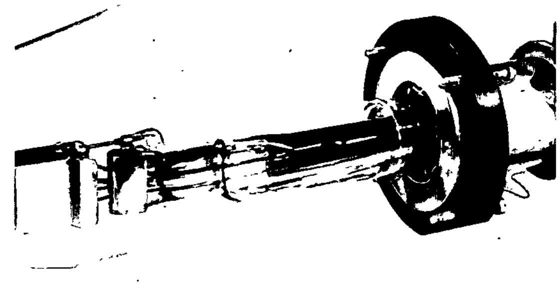


Fig 11 Injector gun for Malvern machines [14]

The original vacuum chamber was made of two flat circular pyrex plates with circular holes, joined by black vacuum wax to two cylinders, the outer of which had sidearms to accommodate gun, ionization vacuum gauge, resonator and vacuum outlet. The interior was roughened by sandblasting, and an earthed film of nichrome evaporated on to it. Lack of, or damage to, this film allows charge to accumulate which inhibits injection and capture into stable orbits. This type of chamber was soon replaced by a more satisfactory 'blown' design, ingeniously constructed by GEC from large borosilicate glass cathode ray tubes. The centre of the face, and the neck of the tube were heated to softening point and pushed together to form a 'donut' shaped tube, as shown in Fig. 12. The side-arms, which were larger than in the original design, were sealed on mid-way through this operation. Three of them were fitted with ground glass flanges for water-cooled greased vacuum joints. Platinum was fired on the inside to provide the conducting coating.

Pumping was from 2 inch Metropolitan-Vickers diffusion pumps using Apiezon B oil, with cone joints sealed by 'J-oil'. The pumping line was attached by a waxed joint and sylphon bellows to the unflanged side arm. The pumping speed of 10 litres/sec at the vacuum chamber produced an operating pressure in the range of 2 to 10×10^{-6} torr. The pressure was measured by an ionization gauge improvized from a first world war R1 army triode, and the backing pressure by a Pirani gauge initially improvised by Watson from an electric light bulb. Phosphorus pentoxide traps were used to remove water vapour, and a feature that would horrify modern safety officers was the use of liquid oxygen in the cold traps, in close proximity to the hot oil. Liquid nitrogen was not available commercially at the time.

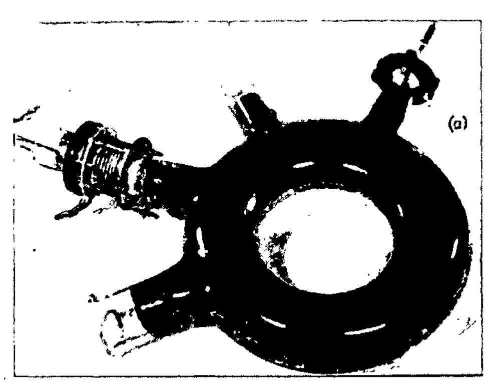


Fig 12 Vacuum chamber for Malvern machines [19]

The control circuitry used many of the features that had been developed for radar applications during the war. An additional feature, however, was the use of high permeability saturable peaking strips which could be set to respond at a pre-determined magnet current by varying the bias current. Finally, an integrator was used to provide a timebase proportional to the magnetic field, on which were displayed zero field, injection pulse, RF pulse and X-ray output. The forward sweep was during the rising field (0-90° phase) and the backward one, from 90° - 180°, was displayed below it. The negative half-cycle was not shown. This display is exhibited in Fig. 13 (from ref. 19) together with a photograph from ref. 18. The X-ray output was indicated by a Geiger counter, a quantitative measurement of the average output being provided by an ionization chamber. Two pulses may be seen; the later one is at the time expected, the origin of the earlier one will be explained later. Experiments on this machine are described in section 6, after a diversion on other activities during 1947-8.

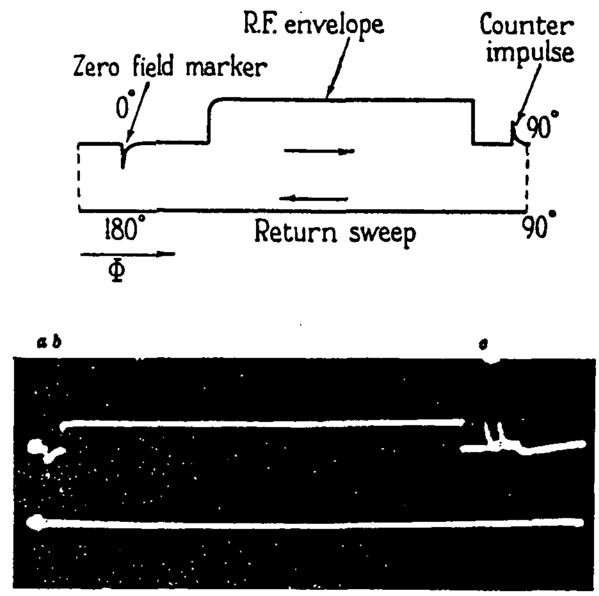


Fig 13 Display of injection pulse, RF, envelope and Geiger counter output [18,19].

5 A Failed Experiment, Links with Fusion, and an Impractical Suggestion

At this point some 'dead ends', which commonly occur, but are rarely recorded, will be described.

First, it should be mentioned that close links were kept with the Birmingham synchrotron in the early days. Discussions on theory and common problems were often held. An essential difference between the proton machine there and the electron machines was that the former required that the frequency be varied over a large range during acceleration. This problem seemed especially difficult because the change was required to be most rapid at lower energies where the frequency was low, whereas any mechanical tuning device required relatively large movement at the low frequency end.

The idea of making an electron model with frequency modulation rather than betatron acceleration, was put forward by Goward, and early in 1947 John Lawson was recruited from TRE and given the problem of making the model. This was to have the same pole shape and dimensions as the 30 MeV machines, but with a slow rise time of one second and maximum energy of 3 MeV. This would require a small magnet yoke, and radial slots in solid iron would suffice to prevent eddy currents. The gun and vacuum system would be the same as for the 30 MeV machines, and because of the low peak field and slow rate of rise the power supply would be small(23).

Unfortunately this project was embarked upon in the wrong way. Instead of an overview of the whole scheme being taken to see where the greatest problems would arise, it was tackled piecemeal. The magnet, which would take the longest time for manufacture was designed and ordered, and experiments were undertaken to make an oscillator covering the required frequency range. A butterfly oscillator with grounded grid triodes was completed which covered the range of 100 - 500 MHz, and a matched accelerating electrode designed on the (unjustified) assumption that a very small accelerating voltage would be adequate to provide the 12 mV per turn needed for the very slow rate of acceleration. After this stage unconsidered problems began to appear, such as the design of a mechanism to drive the butterfly shaft with the right frequency - time characteristic, and the need for exceptionally good vacuum to avoid gas scattering. These were found to be so severe that the project was cancelled. This was just at the time that the C-magnet and second 30 MeV machine was commissioned, and Lawson was given charge of the original H-magnet machine and asked, among other things, to extract the beam.

During work on the frequency - modulated machine an interesting proposal was made by Sir George Thomson of Imperial College, who was working on early ideas for controlled thermonuclear reactions in a toroidal tube containing hot plasma isolated from the walls by magnetic fields(24). Following suggestions of Rudolph Peierls at Birmingham he decided to investigate the possibility of confinement in the field of a very large current circulating in a torus. This would be continuously injected from a gun, and space-charge forces which normally limit the current would be neutralized by ionizing residual gas in the torus. Although the details were not yet clearly thought out, the problem of gas scattering was studied experimentally by Watson in the 30 MeV machine, and shown to disperse the beam before appreciable ionization could occur. The result of these experiments, but not the reason for doing them, was published(25). It was found that the output decreased exponentially with pressure over a wide range of parameters. Injection voltage and rate of rise of magnetic field were varied, and a general formula incorporating these parameters was found empirically. Most experiments were conducted with air as the background gas, but hydrogen was also tried and found to be roughly equivalent to air at one tenth the pressure. The scattering problem would, of course, be reduced if the acceleration were more rapid, and Thomson instigated a programme to build an ironless betatron with very rapid rate of field rise at Imperial College. Some details of the work were published, but again not its object[26,27]. He also suggested that the betatron might capture a greater current if a toroidal winding carrying constant current were would round the vacuum chamber. This appears to be the first suggestion of this scheme, now known as the 'modified betatron', which has been much studied recently as a potential high current device. The problems of injection and extraction have proved to be intractable, however, and no useful device has been built. The experiment was done on the 14 MeV converted betatron, but the current decreased in the presence of the azimuthal field. The theory was worked out for the first time by Walkinshaw, who showed that the field produces coupling between vertical and horizontal betatron oscillations, giving rise to normal modes whose projections on a plane through the vertical axis are elliptical rather than horizontal and vertical straight lines[28]. For the parameters of the experiment this would reduce the injected current.

Another early idea for a proton synchrotron avoiding the use of a continuously time varying radiofrequency system was the 'harmonic synchrotron', proposed by Kaiser and Tuck at Oxford, and independently by R B R-Shersby-Harvie at Malvern[29-31]. In this scheme acceleration is by a resonator operating at a high harmonic of the orbital rotation frequency, $\omega_g = m\omega$. As the particle velocity increases the orbit radius increases also; after a suitable time the accelerating field is switched off so that the orbit radius then contracts to its original value. This is arranged to occur when $\omega_g = (m-1)\,\omega$ after which the process is repeated, so that $\omega_g = (m-2)\,\omega$ and so on. If m is always large the radial excursion can be kept small. More than one gap can be used provided that the relative phases at which the gaps are fed are adjusted to give a rotating wave with the required phase velocity. If this is done, however, some particles are inevitably lost at each transition. The scheme is obviously complicated, and no machine of this type appears to have been designed.

6 Experiments in 'Machine Physics'

As soon as machines became operational there was intense activity in measuring their characteristics, varying the parameters to see how critical they were, and comparing with expectations from the fairly detailed theory of betatrons and synchrotrons that had already been published in the USA[32].

By the time the 30 MeV machine first operated much had already been done on the American 70 MeV machine, and furthermore, several problems such as the effect of field errors and the important and difficult question of injection efficiency had already been studied by Kerst and others in the USA on betatrons. A brief history of American work and list of references is given in the book by Livingston and Blewett[33]. Experiments on the 14 MeV machine are described in ref. 14 by the authors and by Kaiser and Tuck from Oxford[34]. Work on the later 30 MeV machine is described in refs. 19 and 20.

The precise mechanism of injection is unclear. The gun is placed outside the equilibrium orbit, and electrons are injected when the magnetic field has risen to a value such that the radius at which the centrifugal and Lorentz forces balance is that of the equilibrium orbit at the centre of the vacuum chamber. The orbits for electrons entering at various times during the injection interval can readily be calculated if the interaction between the electrons is neglected. If this is done however, it is found that the electrons invariably strike the gun structure again since this inevitably projects a few mm beyond the emitting surface. This is because of the very slow rate of damping, and the fact that part of the gun structure extends further into the vacuum chamber than the points from which the electrons are emitted. This suggests a collective mechanism of some sort, and indeed it was found that if the gun current was progressively reduced by lowering the cathode temperature, a cut-off existed below which nothing was injected. Kerst suggested a possible mechanism; since the magnetic energy associated with a current loop varies as the square of the current, injection of later electrons must reduce the energy of those already circulating[35]. Others invoked effects associated with the electrostatic space-charge. This problem was much studied, particularly (rather later) by Soviet workers. Interested readers should consult the 100 page article by Gonella, which contains over 300 references (and also a list of 43 electron betatrons and synchrotrons)[36].

Following experiments on a betatron in the USA by G D Adams[37] a further experiment on the 30 MeV machine, in which a rapidly pulsed 'orbit contraction coil' produced a rapidly increasing field in the magnet at the time of injection, showed no cut-off, but produced no increase of current at full gun emission. A similar type of device on the later Oxford machine produced a substantial increase in output[38]. It was also found (on the 30 MeV machine) that the effective vertical aperture, found by inserting a moveable horizontal wire, was greater when the orbit contraction coil was used. Numerous other experiments, described in refs. 19 and 20 unless otherwise indicated, were performed. The timing and length of the injected pulse, and the position of the gun were systematically varied; it was found, for example, that injection from inside the equilibrium orbit was equally efficient. The n value at injection was also varied by a pulsed coil attached to the poles above and below the orbit radius. Azimuthal harmonic errors in the field were deliberately introduced at

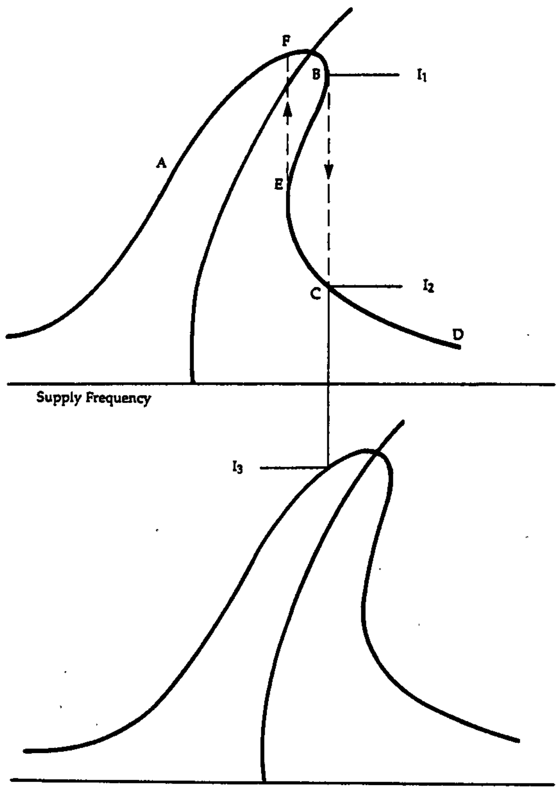


Fig 14 Non-linear resonance curve for magnet, in which inductance varies with the amplitude of the exciting current. As the excitation frequency drifts to a value f_1, the current amplitude drops suddenly from 1, to I_2. Removing condensers from the resonant circuit shifts the resonance curve to higher frequencies, with current of I_3.

injection, again by suitable windings attached to the pole faces, and the aperture constricted in various ways to find out how important these factors were. Comparison with theory was made where possible. The dependence on resonator frequency and power was also measured. A series of experiments on the effect of pulsing the RF power off for short periods was performed on the 14 MeV machine and compared with theory(34).

A suggestion as to how the puzzling double pulse illustrated in Fig. 14 might arise was made by Lawson(39). This arose by analogy from the observation that in the evening the magnet excitation would suddenly drop to a very low value. As the industrial load was shed from the supply network, the frequency, which was just below 50 Hz during the day, began to rise. Since the magnet represented a non-linear inductance which decreased with current amplitude, the resonance curve for the magnet circuit was of the form shown in Fig. 14; two states of excitation were possible over the frequency range between the dotted lines. As the frequency gradually increased the excitation followed the path ABCD. Between B and C there was a sudden drop in amplitude. (For a decreasing frequency the path DEFA would be followed, showing a hysteresis effect). During operation resonance was restored by removing the excitation, switching out a small fraction of the condenser bank and restoring the excitation, so that the resonance curve was shifted as shown.

Returning to the double pulse, the n value of the magnet is roughly 0.75, giving $Q = \sqrt{1 - n} \approx 0.5$, so that about half a cycle of betatron oscillation occurs per revolution. If now there is a perturbation at some azimuth arising from an error in the n-value, resonant build-up occurs. If, in addition the oscillation is non-linear, as in Fig. 14, and exact resonance occurs for a finite amplitude of oscillation; there will be two stable orbits, the normal one and another which closes after two turns, as shown in Fig. 15. (A 'phase-plot' is shown in Fig. 16. Such diagrams were of course unknown to us at the time of this experiment). If at injection some particles are captured into each orbit, and further, the orientation of the target is as shown, then the particles in the orbit that closes after two turns will hit the target before those in the normal orbit, giving the double pulse shown in Fig. 13. This hypothesis was tested by a simple experiment. By walking round the machine carrying a piece of iron (a small transformer) it was possible to vary the position and amplitude azimuthal perturbation and the relative amplitudes of the two pulses. Indeed, by standing in suitable positions it was possible to make either disappear completely. (Such an experiment would have taken much longer with modern regulations on radiation protection!)

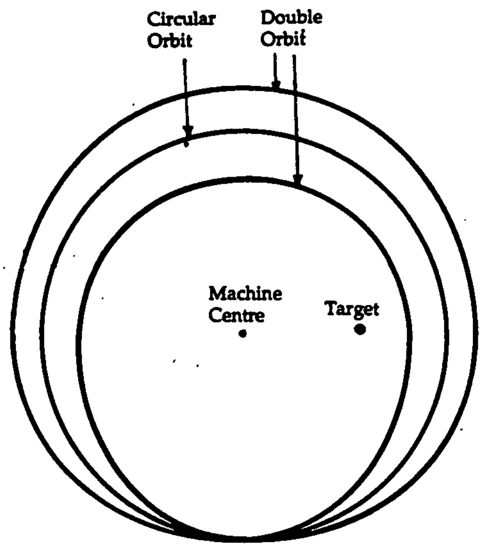


Fig 15 Two stable orbits in synchrotron with n~0.75, non-linear restoring force, and harmonic perturbation. Particles oscillating about the 'double orbit' hit the target first as the orbit contracts after the RF has been switched off.

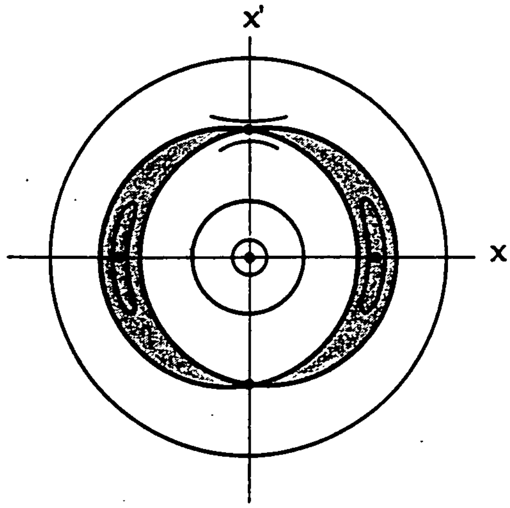


Fig 16 Schematic sketch of phase-space diagram for machine with double orbit. Coordinates x and x' are plotted at the same azimuth on successive revolutions. The shaded area represents the double orbit regime, with successive points lying on curves in the two parts, which enclose a pair of stable fixed points. There are unstable fixed points where the separatrix curves cross, and a stable fixed point at the centre.

7 Beam Extraction

Although several schemes for beam extraction were proposed and analysed, this was found to be rather difficult and met with only limited success. Indeed, extracted beams were not obtained on the Glasgow and Oxford machines, and the scheme developed at Malvern was inefficient and never used for experiments. For an extraction system, two components are necessary, an extractor channel outside the range of the normal orbits, able dramatically to reduce the curvature over a range of azimuth, and a device to displace the orbits so that particles enter the channel. The curvature of the particle orbits can be reduced magnetically, by locally reducing the guide field, or electrostatically, by producing a localized radial magnetic field with the aid of a septum. Magnetic field reduction can be achieved either by a C-shaped laminated iron shunt, or by a set of pulsed coils. A pulsed system has the advantage of not perturbing nearby orbits at injection, but large currents are needed to annul the guide field. Electrostatic schemes require the use of a septum, unless the effect of large vertical fringing fields can be tolerated. The orbit displacement can be achieved in two ways, either by a rapid sideways movement induced by a pulsed coil over a limited azimuthal range to give a first harmonic field component, (suggested for the Glasgow synchrotron but not used[40]), or by a pulsed axially symmetrical coil designed to increase the n-value above unity, so that the particles spiral out with rapidly increasing pitch towards the shunt.

Extraction from a small betatron using the second method of orbit displacement and a magnetic shunt had already been achieved in the USA in 1946[41]. Papers on this and other schemes as well as various proposals are quoted in refs. 19 and 20. Extraction from a synchrotron, using the same displacement method and an electrostatic septum, was also accomplished in the USA at 50 MeV on the GE machine in 1950 though the beam was rather broad[42]. The beam was extracted at 20 MeV from the Malvern machine in the same year, (whether earlier or later is not clear)[43]. Again the beam was caused to spiral outwards by a pulsed coil increasing the n value to exceed unity. It then entered a pulsed magnetic shunt consisting of four parallel conductors arranged in a square 2 mm apart. These carried a current of 3000 amps, in opposite directions in the inner and outer pairs; this produced a field which combined with the magnet field to produce an approximately tangential line of zero field, with stable radial (but unstable vertical) focusing. Details of the design are given in ref. 44, and operation at 20 MeV is described in ref. 43 The beam quality was rather poor, the extraction efficiency being estimated as being between 15% and 50%. Further development (including a modulator with longer life valves) was needed to make the beam usable for experiments, but owing to the closure of the programme (see below) this was not carried out.

8 The Glasgow and Oxford Synchrotrons

These two machines were in principle similar to those in Malvern though there were a number of technical and constructional differences. Both benefited from the basic research at Malvern, but the engineering designs and manufacture were the responsibility of Metropolitan-Vickers and English Electric respectively. (Sorting out just how these responsibilities were to be shared with Harwell, however was not always easy[45]). One obvious difference between these machines and the smaller ones is that the ratio of the vacuum chamber dimensions to the orbit radius is much smaller, so that some tolerances are tighter, and the relative amplitudes of both vertical and radial oscillations about the equilibrium orbit must be kept smaller.

Systems of pole-face windings were incorporated to control both field shape (and hence n-value) and harmonic errors.

Photographs of these machines are shown in Figs. 17 and 18. Both use C magnets with external yokes, but unlike the smaller machines the centre of the magnet is hollow. Instead of a central pole to provide the betatron flux for acceleration at low

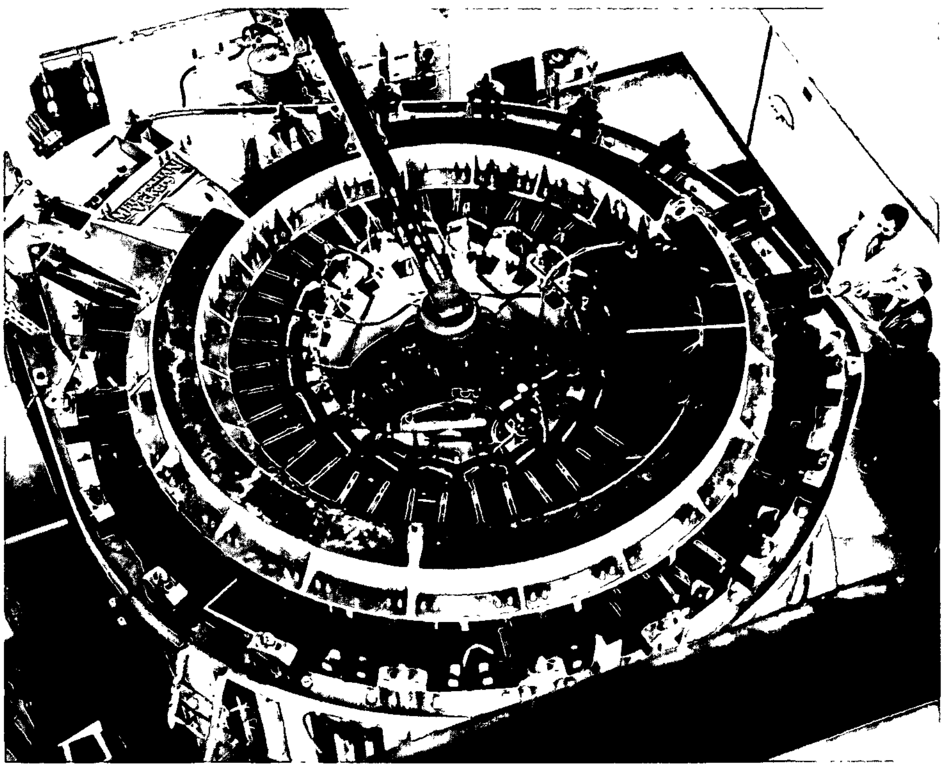


Fig 17 The Glasgow synchrotron, seen from above.

energy a number of saturable 'flux bars' were placed inside the orbit. A drawing of the Glasgow magnet is reproduced in Fig. 19. In the Glasgow machine radial arms attached to the vacuum chamber for the gun, target and vacuum pumps were on the inner rather than the outer circumference of the chamber. The Oxford donut had only one inside port, initially used for the gun.

The vacuum chambers in both machines were made up of a number of oval sections, joined by neoprene sleeves, these were of ceramic at Glasgow and lead glass at Oxford, with resistive coating to prevent charge accumulation. The resonator was not a separate component as in the smaller machines, but was formed from a special section of the vacuum chamber, silvered appropriately to leave a gap for acceleration, and slotted azimuthally to avoid eddy current loops. The resonator design for the

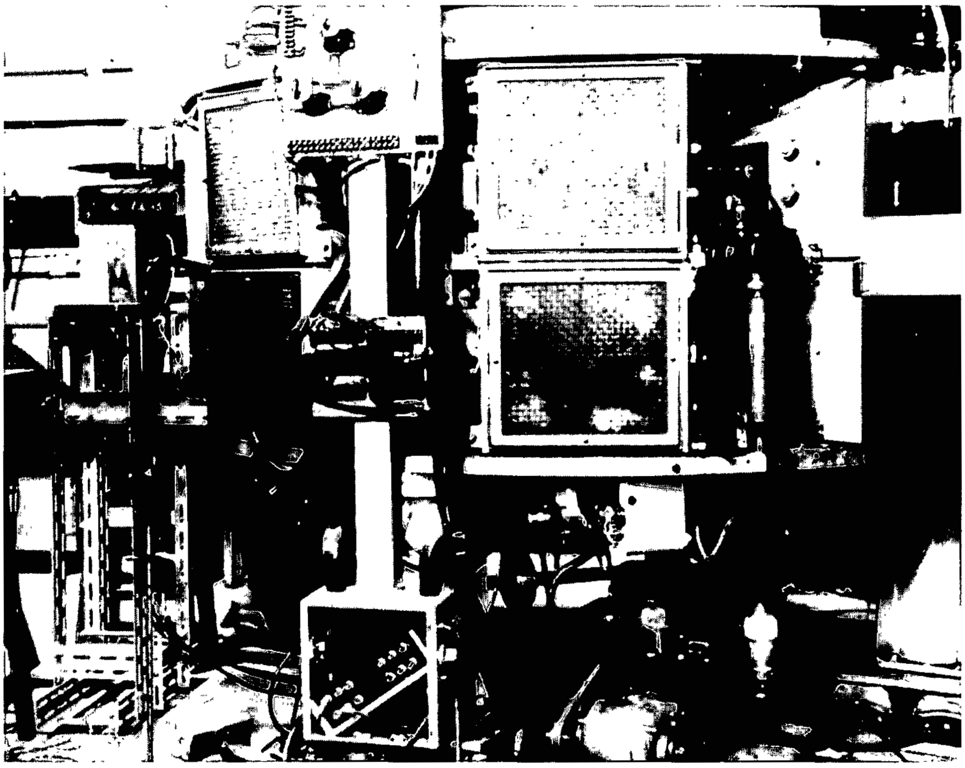


Fig 18 The Oxford synchrotron, (from ref. 30, p.25).

Oxford machine, including a description of the external feeding arrangements, is given in ref. 20, and the Glasgow resonator (of which there were two in the machine) is shown in ref. 46. In both machines the top part of the magnet could be jacked up to allow installation of and access to the vacuum chamber.

The power supply for the Oxford magnet was a variable frequency alternator of about 50 Hz, with ex-Admiralty 1μF capacitors in series-parallel to provide a parallel

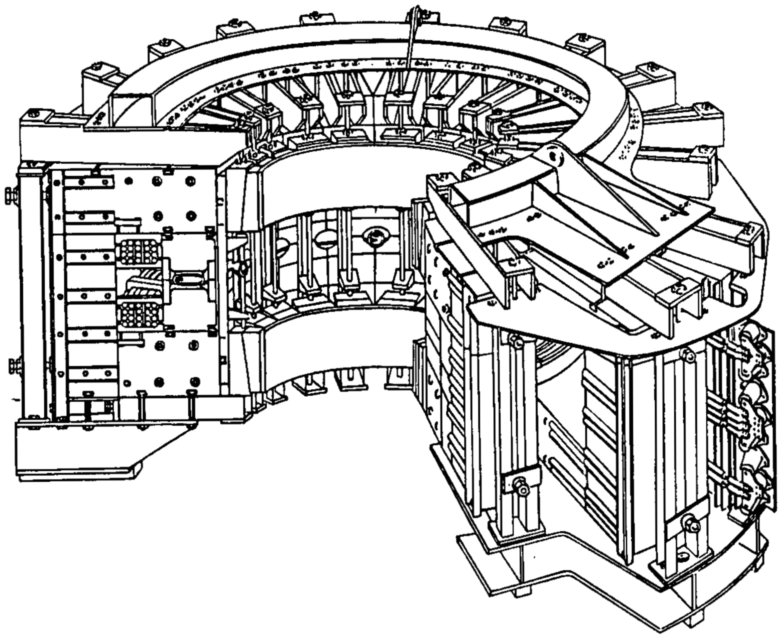


Fig 19 The magnet of the Glasgow synchrotron, with one quadrant removed, from ref. 12, p.25.

resonant circuit. This frequency was arranged not to be synchronous with the supply, so as to reduce interference with other equipment. For the larger Glasgow magnet the provision of capacitors for continuous operation would have consumed too much power, and a scheme was devised to provide 5 pulses per second, with a single oscillatory cycle per pulse. The switching was performed by BK56 ignitrons developed by BTH, four in parallel carrying each half-cycle of current. The ignitrons which conduct the first half-cycle were triggered by a pulse synchronized to a sub-multiple of the 50 Hz supply frequency, while those conducting the second half-cycle were triggered from a biased peaking transformer in such a way as to ensure a smooth transition between the two half-cycles. The average reactive power was 33 MVA, with mean power consumption of about 60 kW. The charging current for the capacitors was provided by a conventional three-phase hard valve rectifier and passed through a series pentode valve and a voltage stabilizer. Operation at 16.4 kV provided a peak field of 0.9T needed for operation at 340 MeV[46].

Power supplies for the radiofrequency and gun pulse were conventional, and the vacuum required was readily obtainable from commercial oil diffusion pumps in connection with liquid air traps. Parameters are shown in the table.

	Oxford	Glasgow
Design energy	140 MeV	375 MeV
Energy achieved	125 MeV	340 MeV
Orbit radius	46.7 cm	125 cm
Magnetic field at design energy	1 T	1 T
Vacuum chamber size (internal)	10 x 3.2 cm	18 x 6 cm
Injection energy	40 keV	70 keV
Field index n	0.55	0.7
Magnet weight	16 tons	80 tons
Magnet power	35 kW	60 kW
Resonator frequency	102 MHz	38.2 MHz
Peak volts/resonator	500 V	1.7 kV (2 resonators)

The Glasgow machine, the first to be planned, achieved an energy of 340 MeV in April 1954, with steady increase of intensity during the year. This energy is higher than the 300 MeV originally envisaged; indeed it was possible to operate as high as 450 MeV, but with greatly reduced output[47]. A major problem during construction was that the original capacitor bank failed to withstand the design operating cycle at the maximum potential of 17 kV, and this delayed the completion by almost a year. Apart from this no serious problems were encountered. Valuable experience had been obtained from a 30 MeV machine commissioned in Malvern, and then installed in Glasgow[48]. No detailed account of the design appears to have been published, apart from the short article, from which the material in this section is largely obtained.

The design energy of the Oxford synchrotron, was constrained by the basement room in which it was to be built. It was expected that the meson threshold, estimated at about 105 MeV, would be comfortably exceeded. The π-meson had not been identified at this time, and unfortunately the higher threshold of 140 MeV was not reached. This arose because of a misfortune in the final baking of the magnet to secure the mutual adhesion of the laminations. During this operation the magnet poles were supported on parallel bars; since the laminations were not yet quite solidly bonded together there was a sag, so that the pole surface was no longer plane but 'wavy'. Consequently the effective pole gap was both somewhat larger than planned, and distorted in shape. Further correcting coils were needed, and the final field was lower than planned, with field index n=0.55 instead of 0.7. The magnet was delivered to Oxford in 1949, the machine assembled in 1950 and a measurable betatron beam obtained in April 1952. An energy of 125 MeV was obtained in the following year, but with poor intensity. Apart from information on the resonator design in ref. 20, and the injection enhancer described in ref. 38, there appear to be no publications describing the design of the machine. Information in this section is largely obtained from the thesis of P Standley[49] who together with J Moffatt and M J Aitken was responsible for commissioning.

9 Experimental Programmes on the Electron Synchrotrons

A detailed description of the various experiments carried out at Malvern and on the two medical 30 MeV machines is outside the scope of this report, nevertheless a few comments (without references) will be made. The 14 MeV machine was used exclusively for the medical studies on the distribution of ionization in targets of various materials and geometrical configuration produced by the X-radiation from an internal target, yielding empirical information needed for cancer treatment. Similar work was done on the 30 MeV machine operated by the Medical Research Council in Cambridge, but abandoned after it was found to be unlikely to offer real advantages over conventional X-ray therapy.

The principal series of physics experiments on the 30 MeV machine at Malvern was on photo-disintegration of the light elements, particularly the $\gamma + C \rightarrow 3\alpha$ reaction and photo-fission of uranium, both using the nuclear emulsion technique that had been developed at Bristol for cosmic ray studies. Thresholds for γ-n reactions were measured for a number of elements, but attempts to determine the shape of the 'giant resonance' curve were not successful. It is possible to measure neutron yield as a function of peak X-ray energy, but finding the shape of the resonance curve involves the solution of an integral equation, and this requires very accurate data, especially of the shape of the distribution at the top end of the bremsstrahlung spectrum. Despite several proposals, no accurate measurements of the spectral distribution could be made, so theoretical values were used. Measurements were made of the angular distribution of the X-radiation of the target, and fair agreement was found with theory, which involves a convolution of the angular distribution from multiple scattering at various levels in the target with the angular distribution of radiation associated with a single radiative collision.

An ionization chamber with thick walls and disc shaped air volume was constructed, and the response to a theoretical bremsstrahlung spectrum as a function of energy up to 30 MeV calculated. Using also the knowledge of the angular distribution of radiation it was possible in principle to measure the current striking the target in the synchrotron.

The synchrotron programme at Malvern was terminated at the end of 1950. By this time it was realised that linear accelerators provided a more intense, reliable, and accessible beam for physics experiments and medical work for energies up to 30 MeV. Furthermore, the basic work and expertise required for the Glasgow and Oxford machines had been completed. A third reason was that the Korean war had started, and priorities returned to defence. A number of staff, including the author, were abruptly moved to defence related work.

The original H magnet 30 MeV machine, with extracted beam, was transferred to University College, London, but proved unreliable and was abandoned. The C magnet machine was moved to Harwell where photo-disintegration experiments continued for a short while. Subsequently E W Titterton arranged for the machine to be sent to the new Australian National Laboratory at Canberra, for use in Nuclear Physics Division of Oliphant's Research School in Physical Sciences. It was there used for nuclear physics experiments including studies of the giant resonance, particularly at the higher energies, and photo disintegration. Experiments continued until the mid 1960's, latterly at the University of Western Australia. At Glasgow some physics work was also done on the 30 MeV machine that had been used primarily to gain synchrotron operating experience in preparation for the 300 MeV machine. This included photo-disintegration studies using first a cloud chamber rather than emulsion techniques, and later, diffusion and bubble chambers. Isomeric transitions in heavy elements were also investigated.

The Oxford and Glasgow machines duly came into operation in 1952 and 1954, and ran for a number of years. No attempt was made to extract the beam from either machine, despite earlier plans. For both machines operation was too late for them to do pioneering work in the energy range which they covered; nevertheless some useful work was done. At Oxford, there was disappointment that the energy was just below the meson threshold. However, some studies were made of neutron multiplicities in the products of high energy photo-disintegration of heavy nuclei. Precision measurements were also made of total absorption cross-sections of 94 MeV photons in a range of elements, and the main work concentrated on electrodynamic phenomena such as Delbrück scattering and pair production in the field of atomic electrons. At Glasgow the main emphasis was on the photoproduction of pions, which proved a profitable field until the 1960's since more precise measurements were needed to settle some unresolved questions, such as the π^-/π^+ production ratio from deuterium at threshold, which work at Glasgow helped to solve. A variety of experimental techniques were employed, including nuclear emulsions, scintillation counter telescopes, and magnetic spectrometers, as well as ionization chambers, and hydrogen targets for specific requirements. Later, with new opportunities on larger machines elsewhere in view, work was done on spark chambers. Finally, studies of atomic phenomena were made using the VUV component of synchrotron radiation; for example, photo-absorption coefficients for a number of elements were measured. There was also some line classification work, and photo-electron spectroscopy of helium. The machine was closed down in 1972, and interest transferred to the higher energy machine at Daresbury.

In later sections work on the proton synchrotrons will be described. First, the Birmingham machine, conceived before any of the electron machines described so far, and second, design studies in Britain towards what was to become the 'PS' in CERN. This involved the new 'strong-focusing' principle, a discovery that we could easily have made in Malvern, but failed to do.

10 The Design and Construction of the Birmingham Proton Synchrotron

In a memoir written in 1967 for the Physics Department of the University of Birmingham Oliphant has described how the idea for this machine came to him at Oak Ridge during 1944 while he was he was on night shift tending the electromagnetic separators(50). Oliphant had worked with Rutherford at the Cavendish in the mid-thirties and had built a 200 kV accelerator for their classic experiments on the D-D reaction. Later, as Poynting Professor of Physics at Birmingham, he had initiated the construction of the Nuffield cyclotron shortly before the war. He returned to Birmingham in 1945 intent on finishing the cyclotron and building the synchrotron. An early document 'The Acceleration of Particles to Very High Energies' (post-dated September 1943, though it seems that this should be 1944) survives.(51) This clearly describes a ring-magnet accelerator, in which the frequency is varied with the magnetic field to keep the orbit radius constant. Radio-frequency electrode systems and the practical problem of frequency variation are considered,and suggestions made on methods for beam injection and extraction. No comment is made on focusing, however, either in the magnetic field or radio frequency, (phase stability). Nevertheless, in his memoir Oliphant gives the impression that he understood both the conditions for radial and phase stability at this time[50]. Iron and ironless magnets were discussed, and acceleration of both electrons and ions considered. For protons, a specific energy of 1000 MeV was quoted, with the possibility of injection from the Nuffield cyclotron at an energy of 45 MeV. At the time a machine of such high energy was a very bold proposal, illustrating Oliphant's visionary approach, and urge to explore entirely new territory. He was convinced that 'new and important phenomena would be discovered'.[51]

This document, (or a similar one) together with a further one detailing some changes,[52] were presumably presented as support for the application resulting in the award in 1946 of £140,000 by the Department of Scientific and Industrial Research (DSIR) for the construction of the machine at Birmingham. To save time and money no new building was planned, but the machine was to be put in the large room originally intended as an experimental area for the Nuffield cyclotron. This cramped location was later found to be very restrictive; space for the extracted beam and experiments turned out to be very limited. It was, of course, hoped that this would be the first machine to operate in this energy range, so that high beam intensity and precision experiments would not yet be required. The emphasis was to be on speed and ingenious improvization with as little detailed planning as possible. This approach was well suited to Oliphant's work with Rutherford, but its shortcomings in a project of this size where large scale engineering was a major component soon became apparent.

An initial grant from the Nuffield Foundation, before the DSIR money was available, enabled a team to be assembled and exploratory work to begin. Practical aspects of the various components were considered in more detail, and the synchrotron theory already published in the USA [32] was extended and adapted to the specific problems of a machine in which the frequency varied over a wide range during acceleration. Trade-offs and tolerances were considered, and a somewhat over-elaborate phase equation derived. This initial work is described in two papers published 1947[53,54].

After rejecting a resonant ironless magnet on account of the very high cost of the capacitors that would be required, a steel ring magnet of orbit radius 450 cms weighing about 800 tons was chosen; this was to be constructed of 1/2 inch radial plates of low carbon steel thin enough to avoid eddy current field distortion of the magnetic field, for a rise time from zero to 1.5 Tesla in about one second. A coil winding with 22 turns carrying 11 kA driven at 1.1 kV by twin-coupled motor generator sets was envisaged. The magnet current would be driven back to zero by reversing the generator field current, the speed being kept sensibly constant by means of a 36 ton flywheel. There would be 1 pulse every 10 seconds. Injection of protons was planned at 0.3 MeV implying a frequency change during acceleration from about 0.27 to 9.3 MHz, a factor of 34. (Oliphant's original idea of using the Nuffield cyclotron as injector was soon seen to be quite impractical). Single turn extraction using electrostatic deflection was envisaged, though never built, as explained later. An eight section ceramic vacuum chamber was planned, though a 60 section system was ultimately used. No mention was made in these early papers of what was to be one of the more challenging problems, the provision of a radio-frequency system with the required 34:1 frequency range.

Formal progress meetings had already begun in 1946. At the first of these, held on 17 September, eight members of staff were present, including Professors Oliphant and Moon, and John Gooden, who had been appointed project leader. He was one of the many Australians besides Oliphant who was to make an important contribution to the project. These meetings are meticulously documented in the minute book by the secretary D F Bracher, whose early reminiscences are documented in the Proceedings of the 1993 Anniversary Meeting[(55)]. The project moved ahead, but the sheer amount of effort that would be required was beginning to be apparent. The rather small scale engineering and technical support meant that many of the physicists participated in detailed design decisions, and spent time supervising and taking part in actual construction and installation work. This was particularly so in the early days of the magnet installation. Oliphant always believed that conventional engineers were too conservative, and was ready to flout conventional practice to save time; this gave rise to tensions, particularly with H H Taylour, the engineer brought in to design and oversee construction of the magnet. The local workshop staff, however, were very flexible and contributed enthusiastically without undue formality. Most of the team were swept along by Oliphant's and Gooden's infectious enthusiasm, and despite occasional opinionated disagreements, worked well together. Oliphant had originally hoped for completion in 1950, but as time passed it was soon appreciated that this was unrealistic.

During 1947-8 the synchrotron passed from design to construction, and the main magnet steel work was erected. A major challenge here was the accurate alignment and stabilization of the large magnet plates. During the following year the copper coils were wound, and tested with the newly installed generator. Overall responsibility for the magnet system during this period, and associated problems such as field measurement rested with John Gooden. Meanwhile work was proceeding on other aspects of the machine, described in more detail later, David Robertson and

Walter Stiles contributed to early work on the RF system, for which L U (Len) Hibbard, assisted by David Caro later took responsibility. John Symonds contributed in various ways, applying the theory of ref. 55 to injection studies, calculating gas scattering and vacuum requirements, and building the pulsed ion source. This was fitted to the 500 kV Philips HT set which had originally been used for nuclear physics experiments[56]. Len Riddiford arrived to take charge of the vacuum system; after heated arguments between him and Hibbard on the one side and Oliphant on the other, ceramic was chosen rather than corrugated stainless steel. Several test sections were ordered in relatively inexpensive chemical stoneware; this was found to be much too porous, and electrical porcelain was chosen for what was, at the time, a very large vacuum system for such low pressures.

The year 1950 was a disheartening one. First came the untimely illness and death of John Gooden, to be followed shortly by the departure of Oliphant. He felt that his loyalty was primarily to Australia, and left for Canberra in July to set up the physics department at the new Australian National University and there embark upon his ill-fated 10 GeV machine. The background to these events is presented in the biography of Oliphant by Cockburn and Ellyard, where the personal and organizational factors involved are discussed in some detail[57]. Further comment may be found in the history of the Birmingham Physics Department by Moon and Ibbs[58].

In this year Hibbard wrote a paper giving an overall description of the machine, including many diagrams and a table of the main parameters[59]. This is the most complete overall description that exists, though of course it is not up to date in some

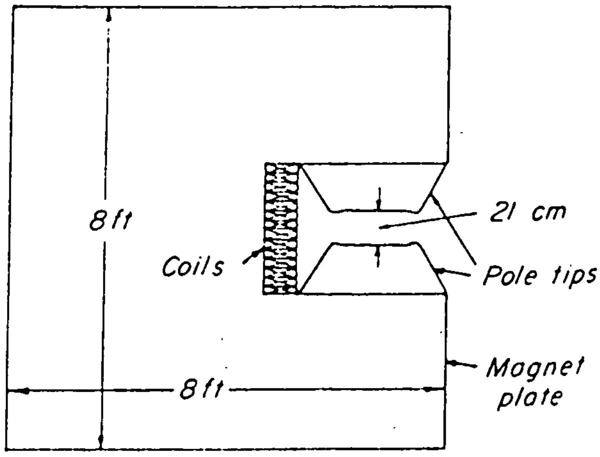


Fig 20 A Cross section of Birmingham synchrotron magnet plate [59].

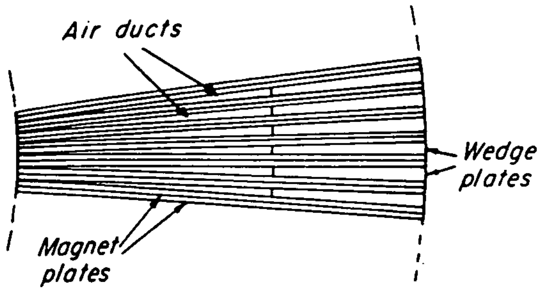


Fig 20 B Plan view of a section of the magnet showing pairs of flat 1/2" magnet plates spaced by short wedge plates [59].

details, particularly of the radiofrequency, vacuum chamber, and extraction system.

In it parameters foreseen at the time (but not all achieved) are tabulated. Sketches of the magnet from ref. 59 are shown in Fig. 20. Details of the magnet cycle, and the power supply and triggering circuits are also given in ref. 59 essential features being an almost linear rise of magnetic field from zero to a maximum of 1.5 Tesla in 1 second, triggered every 10 seconds by a signal from the variable frequency r.f. generator at the appropriate time to an accuracy of 1 μsec.

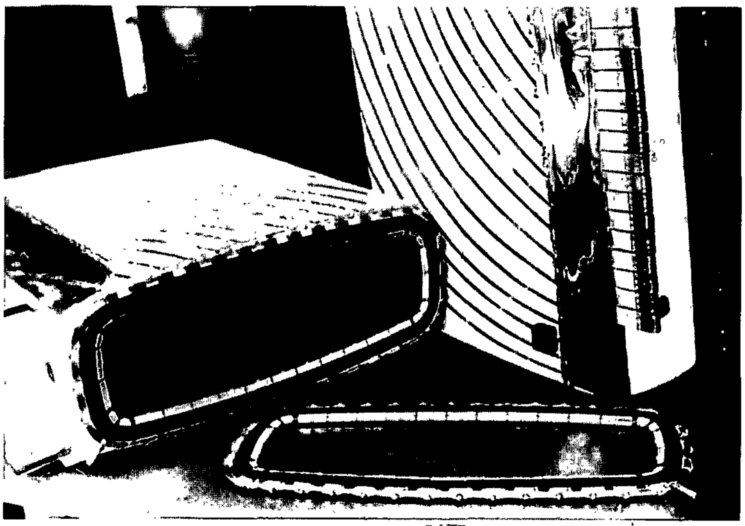


Fig 21 Porcelain vacuum sections at the centre of the Cee, showing the laminated silver coating leading out to the spring contacts of the sliding joint. Brass gasket plates with moulded rubber rings used for vacuum sealing can be seen. Electrical connection between the porcelain sections is effected by the two sets of spring contacts carried by each gasket [63].

The 60 sections of the porcelain vacuum chamber were coated internally to prevent charge accumulation, and joined together with double rubber gaskets. The accelerating electrode was in the form of a centre-fed 'cee' extending over an angle of 96°, in which circumferential strips of copper were sprayed on the outside of the vacuum chamber, (Fig. 21). This, together with thin copper foil glued to plastic and mounted on the magnet pole face produced a 5 ohm transmission line, and was fed through a wide-band transformer with core of very thin wound mu-metal[60].

With Oliphant's departure at the beginning of July, responsibility for completing the synchrotron fell on Moon, soon to be appointed Poynting Professor. Neither particle accelerators nor high energy physics were close to his current interests, and although he was not happy to be 'landed' with the project, he tackled it conscientiously and with vigour. It was a difficult year, and some of the problems were proving less tractable than anticipated. Furthermore, lack of technical support was causing some of the installation work to move more slowly than planned. Indeed, the original hope for completion by 1950 could clearly not now be realized.

Moon felt that additional technical support was very desirable, and, at Professor Blackett's suggestion, approached Cockcroft to see whether Harwell could help. At one point it was suggested that Lawson, who had studied in particular the problem of frequency variation in connection with the electron model (section 5), should be seconded to Birmingham but in the end nobody moved there. The detailed problems at Birmingham turned out to be rather different from those familiar to the Harwell staff. Harwell had, to some extent, already been involved in contractual matters concerning steel for the magnet and the generators supplied by Parsons. A file is preserved in the Public Record Office which well indicates some of the special problems imposed by post-war shortages(61). For example steel was rationed and allocations had to be obtained through the DSIR. Relations between Harwell and Birmingham though generally cordial, on contractual matters were not always so. W W Abson, after a visit to Birmingham where he had been sent to 'sort things out' records in January 1949 that he does 'not think our efforts are welcomed on technical matters at Birmingham'(61).

One of the more challenging problems was provision and synchronization with the magnet field of the variable accelerating frequency. Nothing quite like it had been tackled before; the initial low energy stage when the frequency is low and changing rapidly is particularly difficult. Tolerances are tight, and the resonant tuning of the capacitive cee requires a rapid and large change of inductance by a factor of 1000 in a coil in parallel with the cee. This was accomplished by plunging a very non-uniformly wound cylindrical coil into a pot of mercury at high speed, where splashing and scum formation presented problems that needed much ingenuity to solve. (62) This is shown in Fig. 22. In addition, the frequency had to be generated

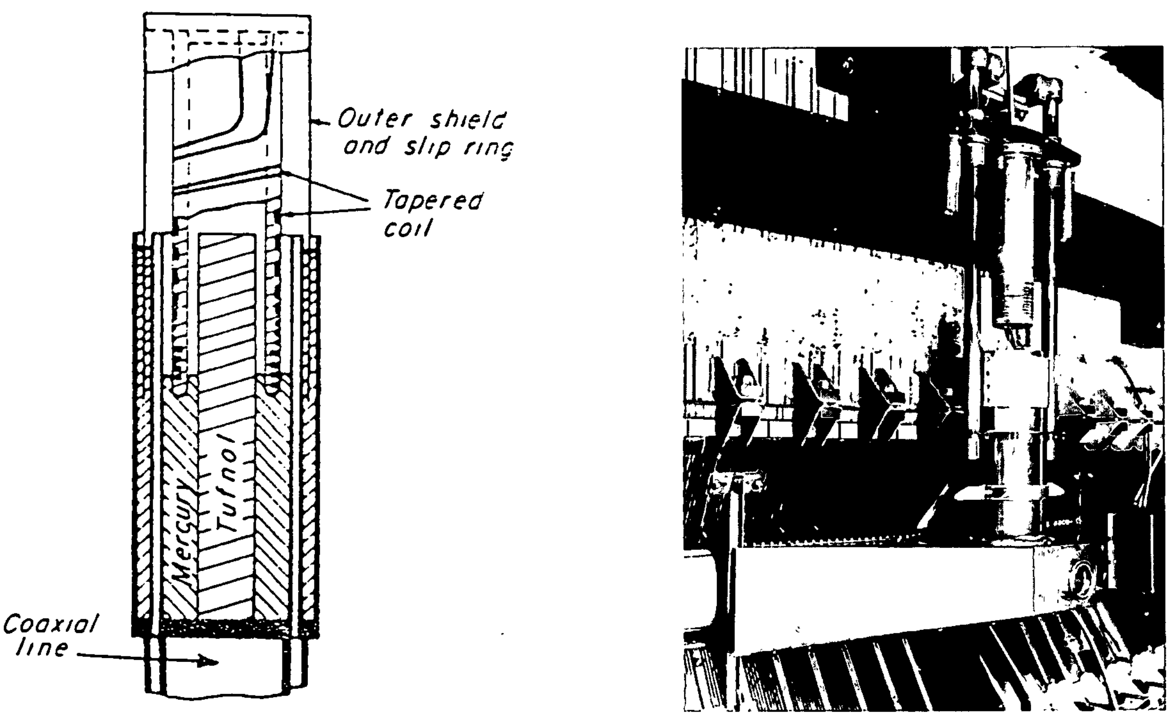
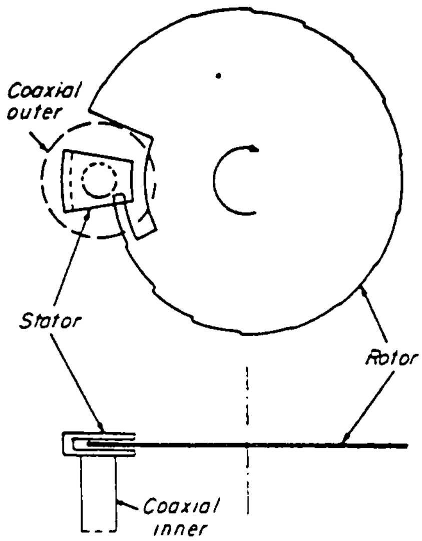


Fig 22 Variable inductance for tuning the accelerating electrode. A tapered coil is plunged into mercury and the inductance varied by a ratio of 1,000 to 1. (From refs 59,63).

accurately at low power and then amplified. The tolerance at low frequency was ± 0.1%, and the variation had to follow the rising magnetic field. This implied that the field variation with time had to be known accurately, and the initial synchronization needed to be good. The required frequency was generated by beating together two oscillators, one with fixed frequency, and one with frequency controlled by a variable condenser, one plate of which was a very carefully machined rotating disc.



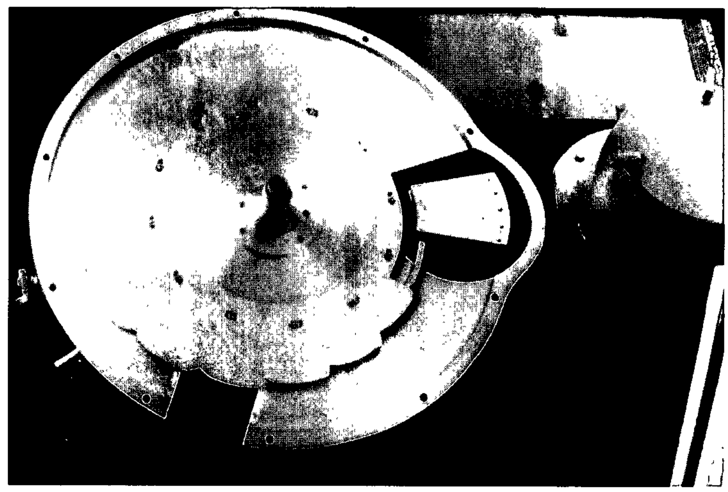


Fig 23 Variable capacitor for generating the accelerating radio frequency. The capacitor is attached to the open end of a high-Q coaxial line. The stepped rotor is coupled capacitively to the coaxial outer. (From refs 59, 63).

In order to cope with slight variations between magnet cycles the disc was driven by a servo motor. Information on the position of the disc was obtained from 120 strip mirrors placed with extreme accuracy around the circumference of the disc. The time between successive pulses of light reflected from these mirrors gave a measure of the angular velocity, and the servo ensured that the angular velocity corresponded to the correct magnet field. This was determined by integration of the e.m.f. across a coil in the magnet gap. Very tight tolerances, both mechanical and electrical, were required on all aspects of this system, which in a sense was the 'heart' of the machine. Full details of this very elegant and ingenious solution to a difficult and quite novel problem are given by its designer, Len Hibbard, in a paper which contains full references to earlier contributors(63). A photograph of the disc from this paper is shown in Fig. 23, together with a diagram from ref. 59.

In the three years between the departure of Oliphant and the first operation of the machine the team worked hard, facing, and overcoming, a number of unexpected problems. Detailed progress is recorded in the departmental reports(64), and details of the design may be found in departmental theses(65) and a number of detailed publications[66]. Some aspects went smoothly, the vacuum design by Len Riddiford[67] and the 500 kV injector by Colin Ramm[68] proceeded as planned. (The 500 kV set had earlier been used for nuclear physics experiments). In other areas there were problems; the most notorious of these was the 'pole-face' disaster. When the magnet was activated the pole-faces, specially shaped and made of 1/8" thick soft iron plates, broke away from their relatively light securing brackets and crashed against one another. The reason for this surprising effect was discovered after 'a few hours hard thought'. In a more accurately constructed machine the pole tips would all hold firmly to the yoke and no clamps would be needed. The yoke plates were not of the same length, however, giving rise to an irregular gap between yoke and poles. Flux concentrated where the gap was small, leaving a weaker field in the large 'accidental' gaps than in the main gap, and this forced the pole plates away from the

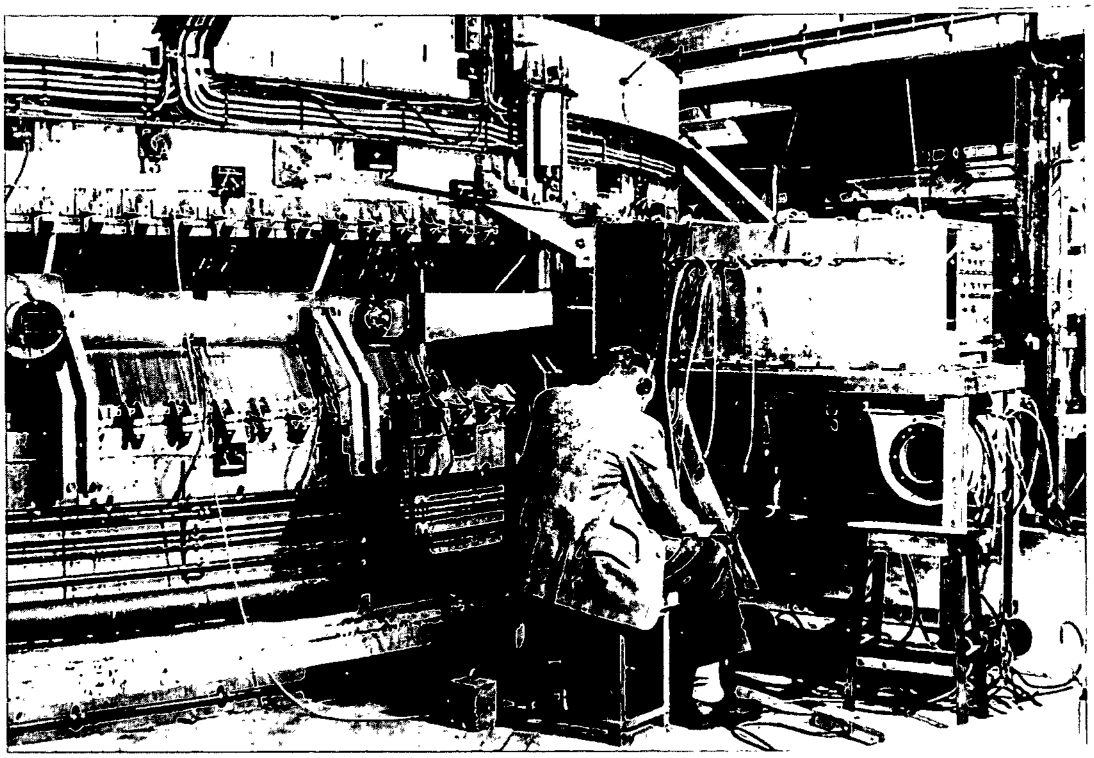


Fig 24 The completed Birmingham synchrotron. The case containing the variable inductance for tuning the RF can be seen bolted to the top of the magnet.

yoke. The cure was simple in principle, the insertion of a few millimetres of plastic between poles and yoke to reduce the degree of irregularity. Its execution, however, turned out to be very time consuming and resulted in a delay of many months to the project. Details may be found in ref. 58. One consequence of this delay was that with the magnet unavailable, it was not possible to test the motor-generator set to peak current. When ultimately this was tested bearing problems were found in the generator which had to be remedied by the manufacturer (Parsons), causing further delay. After these problems were finally remedied and the 'log jam' had been cleared, Moon enlisted the work of the whole department, and progress was rapid.

At last, in July 1953, an internal beam of about 10^9 particles/pulse was accelerated to full energy just short of 1 GeV. This was a notable achievement after seven years of hard work by an indefatigable team, though one whose members were often changing. Indeed, few members were there during the whole period. A photograph of the completed machine is shown in Fig. 24, and a list of parameters issued when the machine started is reproduced in the table. A short description of the machine at the time of its start-up was published in 'Nature' (69) and further details and background information can be found in ref. 70.

DATA ON BIRMINGHAM PROTON SYNCHROTRON

General Particle Properties	
Estimated maximum energy	1,000 MeV
Period of acceleration	1 sec
Repetition rate	6 per min
Energy per rev. (mean)	200 eV
Total number of revolutions	5×10^6
Number of particles accelerated	3×10^9
Final velocity	0.88 c
Distance travelled	100,000 miles
Magnet	
Max, usable radial space in magnet gap	33 cm
Value of n at mean orbit	0.68
Total weight	810 tons
Maximum field strength	12,500 gauss
Magnet gap	21 cm
Radius of magnet	16 feet
Radius of stable orbit	450 cm
Thickness of laminations	1/2 in
Number of 1/2" laminations	1200
Number of taper plates	600
Thickness of pole tip laminations	1/8 in
Number of pole tip laminations	15,000
Number of exciting turns	24
Peak current	12,500 amps
Peak voltage	1,100 volts
R.M.S. current	3,500 amps
Average rate of rise of magnetic field	15,000 gauss/sec
Time constant of magnet and generators	10 sec
Duty cycle	10 sec
Dissipation in coils	110 KW (mean)
Dissipation in magnet	20 KW (mean)
Energy stored	7,000 K.W.S.
Accuracy obtained in gap survace	± 0.003 in
Air cooling rate	20,000 cu.ft/min
Maximum magnet coil temperature rise	40°C
Motor Generator Set - Pulsed DC	
Excitation : Separate pilot and main exciter	
Peak voltage	1,100 volts
Peak current	12,500 amps
Speed in unloaded condition	500 rpm
Power of driving motor	1,500 HP
Weight of flywheel	36 tons
Energy stored	55,000 K.W.S
% loss in speed at peak current	5%
Time constants : Pilot exciter : tp = 0.09 sec. Main exciter : tm = 0.34 sec.	Main gen. field : tg = 0.96 sec
RF System	
Initial frequency	330 Kc/s
Final frequency	9.3 mc/s
Voltage on Cee	240 R.M.S
Angular length of Cee	96°
Peak anode dissipation of amplifier	10 KW
Reactive power in Cee	5 K.V.A

Accuracy of relation to magnet field	1/4%
Mean phase angle (with 240 V RMS)	22°
Injection - Cockcroft - Walton Set	
Injection energy	460 KeV
Injection beam current	~ 0.7 ma
Injection radius	~ 465 cm
Overall length of injection guide electrodes (straight)	3'6"
Aperture to guide electrodes	0.32" x 0.37"
Magnetic field at injection	217.5 gauss
Time for radius of injection orbit to shrink to mean orbit	~ 100 μ sec
Vacuum System	
Available aperture for particles	10 cm x 33 cm
Number of 15" oil diffusion pumps	5
Average pressure in donut	8×10^{-7}mm
Total volume of donut and manifolds	4000 litres
Pumping speed at manifolds	10,000 1/s
Total leak rate	0.7 micron - 1/s
Cost	About £250,000
Date of first operation at full energy	16 July 1953

Of course there was disappointment too that for more than a year already the Brookhaven Cosmotron had been operating at twice the energy and much higher intensity(71). Furthermore, it was now realized that space would not permit an electrostatic extractor as originally anticipated, and only a relatively feeble scattered external beam appeared to be possible. Both injection and extraction on the Cosmotron had been aided by the incorporation of four straight sections, a possibility not appreciated at the start of the Birmingham machine. A further feature which caused much embarassment was the very large fringing field which extended a long way outside the magnet. This again was not anticipated at the time the magnet design. It could have been greatly reduced by providing reversed current windings on the outside of the magnet, as was done on the Cosmotron, and is indeed now general practice.

During the 14 years of operation of the machine a number of improvements were made which greatly improved its reliability, and the increased current available for experiments. A completely new ion source was built and a much more efficient extractor provided, in which a coil was plunged into the magnet after the beam size had contracted, and then energized to reduce the guide field locally and thus eject the particles. A 'flat top' to the magnetic field time profile was added to lengthen the extracted pulse. The rotating condenser in the RF system was replaced by a flexible function generator that enabled deuterons also to be accelerated, and the coil that dipped into mercury was replaced by a system using ferrites. A detailed description of these later developments is outside the scope of this history, but information on them may be found in ref. 55 and departmental theses. A paper written in 1955 for experimentalists to present the capabilities of the machine contains a list of acknowledgements to those who contributed to its design and construction, including a number who are not specifically acknowledged in this report or by earlier references[72].

Reference 55 includes some information on the physics programme on the synchrotron which was under the supervision at first of P B Moon, later of G W Hutchinson and finally of W E Burcham. It was determined by the maximum (external) proton energy of 970 MeV, which was below the strange particle threshold, and by the beam intensity, which was not high enough to provide useful secondary pion fluxes. Under these circumstances the main field of work had to be the nucleon-nucleon interaction. The earliest experiments with scattered-out protons and emulsion detectors were poor statistically but yielded total (and some differential) cross-sections for the elastic and inelastic proton-proton interactions and succeeded in demonstrating spin-polarization of the scattered beam. The proton-neutron interaction was investigated using a deuterium target. Improved statistics came from the use of diffusion, bubble and spark chambers, from the development and use of fast counting systems but above all from use of the plunging coil extractor. Both double scattering and triple scattering studies were made. Results were analysed by optical model techniques and information was obtained on such topics as charge independence of forces, Coulomb-nuclear interference in scattering, validity of dispersion theory predictions, the $\Delta(1232)$ nucleon resonance and the possibility of a pion-pion resonance. Deuterons of energy 650 MeV were used to test stripping theories and to investigate isospin selection rules.

From 1963 onwards the Birmingham Synchrotron programme began to transfer to 'Nimrod' at the Rutherford Laboratory and then to CERN. In the preceding decade the machine had made possible a useful though not spectacular contribution in a specific field, and its existence had led to the emergence of a strong and experienced research group with potential for future work.

The project has, of course, been criticized on the grounds that the style of working was inappropriate to an installation of this size. The more conventional and thorough approach to the Cosmotron, with organized engineering support, is more likely to be successful in reaching its targets in time. Though this is no doubt true, it would hardly have been possible for Oliphant to set up such a costly organization in post-war Britain in a University setting. The enterprise can be seen then as a bold and courageous attempt to be first with a 1 GeV machine. Though at times irritatingly stubborn Oliphant was an inspiring leader, with great faith both in 'fire in the belly' as a receipt for getting things done quickly, and in the rapid emergence of good ideas to circumvent difficulties as they are encountered. He was fortunate to have colleagues able to select from his flood of ideas those which were worth pursuing, and strongminded enough firmly to reject the others. Hibbard was outstanding in the respect, and contributed to all aspects of the design. After Oliphant's departure Moon, though not participating in the detailed design and not enthusiastic about Oliphant's style, provided continuous encouragement and gave high priority to providing resources and support.

11 Work at Harwell for CERN, 1951-3

The events leading up to the formation of CERN in 1954 are set down in detail in the official 'History of CERN'[73]. Before this date there was not only extensive international discussion and diplomacy by senior European scientists, with advice from the USA, but also considerable technical activity towards the design of both the 600 MeV synchrocyclotron, and the proton synchrotron now known as the PS. British scientists were involved in the early negotiations, and opinions on whether or not to participate were divided. Here, however, the emphasis is on the technical issues involved, and the organizational background will be only briefly sketched. (A fuller summary, based on the official history, may be found in the biography of John Adams[74]). The account here extends to October 1953, when the first British members of the 'provisional' CERN team left for Geneva.

The first British technical commitment was in response to a letter in March 1951 from Pierre Auger, who, in his capacity of Director of UNESCO's 'Department of Exact and Natural Sciences', had written to ask whether it would be possible for Cockcroft 'to give permission to one of [his] young men, especially competent perhaps in the high-frequency part of the accelerator to help with our Planning Bureau'[75]. Cockcroft agreed, and appointed Goward to help. In May 1951 a meeting was organized in Paris by Pierre Auger to discuss the proposed European Laboratory; representatives from a number of countries were present, and it was decided to plan for a high energy synchrotron, with an energy between 3 and 6 GeV[76]. Goward attended this meeting, and also a larger meeting held in October also in Paris, at which it was proposed that the new laboratory should contain both a synchrotron of energy 5 GeV and a synchrocyclotron of 500 MeV. Estimates, more detailed than those of the May meeting, were made of costs and staff requirements. The names of people who might be asked to participate in the study groups were put forward. Discussions continued, and by May 1952 the first meeting of the Council of the group shortly to be known as 'Provisional CERN' was held in Paris[77]. Four study groups were set up. The Norwegian Odd Dahl was nominated 'Head of the study group in charge of studies and investigations regarding accelerators of particles for energies greater than 1 GeV'. His deputy was Goward, with other group members Hannes Alfvèn (Sweden), Wolfgang Gentner (Germany), Edouard Regenstreif (France) and Rolf

Wideröe (Norway) (78,79). Their remit was to study the design of a machine similar to the Cosmotron, but with higher energy.

At the second Council meeting, which took place some six weeks later in Copenhagen, the machine energy was fixed at 10 GeV, and further members with specific expertise in accelerators were added to the team(80). These included D W Fry from Britain, who was head of General Physics division at Harwell which included the Accelerator groups. Also chosen were Kjell Johnsen an accelerator theorist from Norway who had already assisted Dahl in his planning for the proposed laboratory, and Chris Schmelzer a German with experience of radio-frequency applications. Fry responded by asking John Adams, who had made a major contribution to the design and operation of the 175 MeV Harwell synchrocyclotron, to look at the magnet design for the proposed European synchrotron. At this time Adams was engaged on designing a high power klystron, based on the design at Stanford, for a proposed high energy electron linear accelerator; the accelerator itself was the responsibility of Goward. (This accelerator and the klystron project were later abandoned, after the realization that the use of quadrupole focusing would make a proton linear accelerator feasible, and in the belief that this would be a more interesting option).

A very important development occurred in the middle of 1952; Dahl, accompanied by Goward and Wideröe, made a visit Brookhaven in August to see the Cosmotron. When they arrived they learned of a new concept just discovered at Brookhaven to be known as 'strong focusing' or the 'alternating-gradient' principle. By greatly strengthening the gradient of the magnetic guide field and also alternating it around the circumference a much greater net focusing force in both horizontal and vertical planes is generated, so that a much smaller space for the orbits, and hence a smaller magnet, is required. The improvement was dramatic; the basic orbit dynamics and speculative parameters for a machine of energy 30 GeV with internal aperture of the vacuum chamber only 2.5 x 5 cm had been worked out and presented in a paper submitted to the Physical Review on 21 August by Courant, Livingston and Snyder(81). There were two features of the new machine that later gave grounds for concern. First, the very strong focusing implied that the number of betatron oscillations per circuit of the machine greatly exceeded unity, and decreased as the magnet saturated and the field gradient decreased. Second, because of the very small amplitude of the betatron oscillations the phase-focusing corresponded to that in a linear accelerator, where the stable phase occurred when the accelerating field in the accelerating cavity was decreasing in time. At extreme relativistic energies, higher than that of the proposed machine with the original parameters, there would be a 'transition energy' at which normal synchrotron phase-focusing on a rising field would occur.

Dahl returned to Europe full of enthusiasm for the new concept and eager to explore its feasibility for the new machine. By October he was ready to put his proposals to the Council, who sanctioned his proposal for a 30 GeV machine and entrusted the design to his team. This immediately changed the balance of the work that was required to be done, implying a much larger component of 'machine physics' as compared with engineering design. What was needed was far more than the simple scaling up of a machine already working, and built on well understood principles. European accelerator physicists were keen to study and explore the new idea.

At Harwell Goward quickly aroused interest in the new principle(82), and this was enhanced by a visit by Courant early in November. Meanwhile the first indication of future complications had occurred. Lawson, though no longer working on accelerators, had earlier studied forced oscillations on the Malvern machine and he quickly realized that as the number of betatron oscillations per revolution passed through an integral value small errors in the magnet alignment or field value would cause resonant build up of the oscillation and the beam would strike the vacuum chamber. After discussion with various colleagues a brief note was written(83). One suggestion that had been made in discussion was that the focusing field should be non-linear, so that the effect of a resonance would be limited. In his note Lawson assumed that this would give a random build up of amplitude, and that even this would be unacceptable. This hypothesis was not generally accepted; indeed, what would happen was not clear, and this gave rise to some intensive study of non-linear oscillations. Many proposals were explored for overcoming or mitigating the difficulty.

Now that the design of the machine was seen to involve new and unknown features the study group was extended, and contained a number of part-time participants. It was clearly necessary to proceed to quantitative studies so that a set of parameters could be chosen, and to assess the full significance of the resonances. Adams, who was concerned with the magnet was clearly deeply involved, and he was joined early in 1953 by Mervyn Hine another ex-radar scientist who had been working on the abandoned 600 MHz electron accelerator at Cambridge. Niels Bohr, head of the CERN theory study group arranged for Gerhard Lüders from Göttingen and T Sigurgeirsson from Iceland, to work in Copenhagen on orbit dynamics. At Harwell John Bell also contributed to the orbit theory, and in January 1953 wrote a report on the algebra of strong focusing, which contained a derivation of what is now known as the Courant-Snyder invariant(84).

During 1953 the design team was in several locations. Dahl remained in Norway at the Chr. Michelsen Institute; he had reacted enthusiastically to the idea of building a strong-focusing machine, and was keen to pursue the engineering design. Johnsen remained there also. The theoretical group was based in Copenhagen at Niels Bohr's Institute, and the British team remained at Harwell. Regenstreif continued to work in Paris in Pierre Grivet's laboratory at the Sorbonne studying orbits, magnets and profiles. Work on radio-frequency problems was centred at the University of Heidelberg under Ch. Schmelzer. Close touch was maintained with Brookhaven, and it was agreed in March that John and Hildred Blewett, both major contributors to the Cosmotron, would help directly in the European project, and would come first to Bergen in April and then move to Geneva later in the year when the other teams assembled there.

The year 1953 was a busy and stimulating one. There were two achievements of the British study group. First, new features of the orbit dynamics were discovered and investigated, and second, the theory was used to calculate actual parameters for a realistic design of a machine for 30 GeV, including tolerances and engineering constraints. During the year a number of meetings were arranged and numerous informal reports were written. It is not clear how complete a record these provide. On the theoretical side Lüders and Sigurgeirsson (who introduced the concept of 'admittance')(85) together produced a formal theory of orbits in periodic structures, incorporating effects of misalignments responsible for the integral resonances, and also errors in gradient which also gave rise to half-integral resonances(86,87). These were at the same time identified by Hine using more intuitive arguments; he also raised the question of higher order subharmonic resonances. Hine working closely with Adams embarked on a study of non-linear effects, making for the first time the extensive use of computation on ACE, the 'Automatic Computing Engine' at the National Physical Laboratory. This work is preserved among a series of papers, all jointly by Adams and Hine, in which a large number of effects, such as vertical-horizontal coupling were investigated(88,89). These studies were accompanied by parameter surveys and analysis of tolerance analysis appropriate to an actual machine. Over the year the value of n assumed neglecting the resonances was considerably reduced, leading to an aperture of 7 x 15 cm rather than 2.5 x 5 cm in the original proposal. Nevertheless, this represented a very substantial improvement compared with what would be required in a weak focusing machine. By the time of the move to Geneva in October, the parameters of the PS had essentially been fixed.

Elsewhere other factors were being considered, such as the design of the radio-frequency system. One new problem that arose was that the 'transition energy', where the stable phase changes over from being on a falling field in the resonator to a rising one occurred at 6.7 GeV, now less than the machine energy. The question arose as to whether this could be crossed without loss of the beam, and detailed analysis was required to produce a reassuring result. This problem was addressed particularly by Kjell Johnson in Bergen, who also investigated other aspects of the dynamics in the radio-frequency field, such as the behaviour at injection(90).

Goward, as well as his general duties as Dahl's deputy, studied the possibilities of aligning the magnets using the beam as a monitor. Engineering topics such as details of magnet design and power supply requirements were studied by Dahl, and trial machine layouts sketched. Regenstreif continued with non-linear orbit dynamics and model magnet studies.

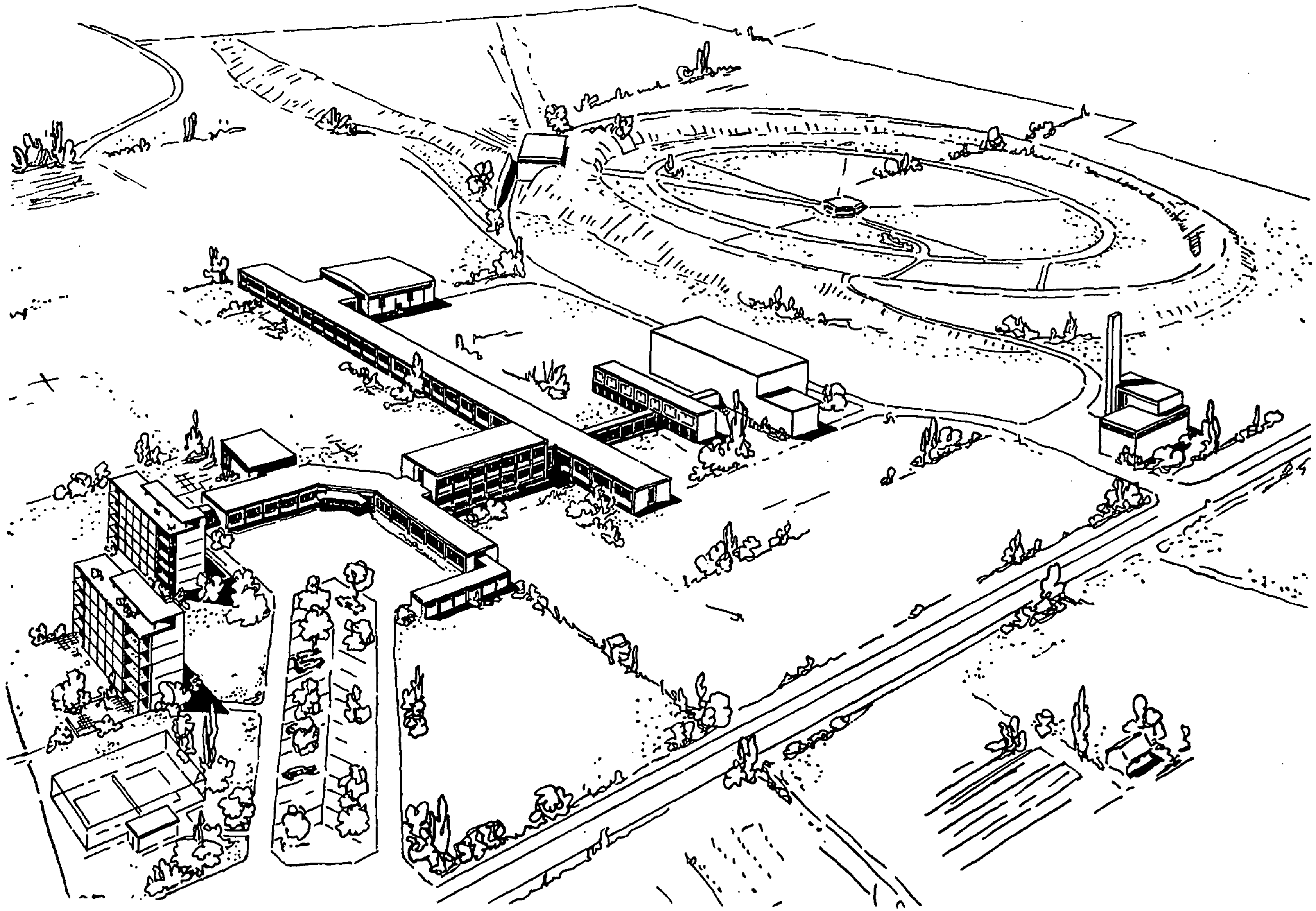
During 1953 meetings between the sub-groups had been held, at least two of these being in the UK. Records survive, and the agenda and minutes give a good impression of what the various participants were doing(91). One such meeting, was held at Harwell by the orbits sub-group on 1 March. In addition to Harwell staff Johnsen and Regenstreif were present, and three members of the theoretical group, Jacobsen, Lüders and Sigurgeirsson. Several conclusions are reached; first, the prospects for making a strong-focusing synchrotron are good; second, because of alignment difficulties, n should be reduced by 4 to 900; third the magnetic field could be non-linear, but if so it must be closely controlled; fourth the frequency and phase need to be carefully controlled in passing through transition energy and finally, the field inhomogeneties at injection will require an injection energy of 50 MeV rather than 4 MeV as previously assumed.

Just six months later, in September, there was a further discussion but with no member of the theory group present. It was attended by the Blewetts, who had been working with Dahl and Johnsen in Bergen since July. The neatly handwritten summary by Adams begins: 'It is becoming possible to choose some of the critical parameters of the CERN proton synchrotron by scientific arguments. In view of the coming presentation of our progress to the CERN council the above group members met to agree on a set of parameters that could be used to illustrate the theoretical work completed to date'(92). A summary of proposed parameters, essentially those of the final machine, is appended. The meetings mentioned here were held at Harwell; others were held elsewhere, dealing with other aspects of the machine, for example its layout and shielding requirements, and the design of the radio-frequency system. Some details may be found in the CERN archive.

It is difficult at this time to chronicle the details of this very eventful year, and to apportion credit in an authoritative way. One factor to be remembered is that the alternating-gradient idea came from America, and the staff of Brookhaven and elsewhere were generous with their information and help. Nevertheless, it is generally accepted that the British contribution of Adams and Hine, who worked together as a very powerful combination, was an important one in defining a set of realistic parameters for the machine. They insisted on deep understanding and cautious realism in practical matters; this extreme caution did not always endear itself to the Americans, who had been encouraged by the successful operation of the Cosmotron, which had also faced many unknown factors at its inception. This gave rise to Hildred Blewett's famous remark about the 'miserable English'[(93)]. Adams himself confesses to 'Jeremiah-like prognostications' concerning inhomogeneities, together with Hine and Lawson[(94)]. (Lawson, whose single contribution had been a negative one, was no doubt influenced by his earlier disastrous entry to the field of accelerators, described in section 5).

In October 1953 the team that was to design the machine assembled in Geneva. This did not include all who had been working in the study group, notably Odd Dahl, who resigned his appointment shortly after, nor the theoreticians who had been working in Copenhagen. It did include, however, a number of others who had so far not been deeply involved. In a list provided at the time, 17 technical staff are listed, together with seven consultants. Their accomplishments, however, are well exhibited in the series of lectures presented at the Conference held in Geneva at the end of October[(95)]. Included is a historical review of the project by Dahl. Many of the speakers had no previous experience in accelerator design, furthermore the team consisted of a number of sub-groups in different locations; communication was not so easy as it is today. Despite some tensions, noted in the Official History, the team had worked well together, and laid the foundations for a remarkably successful outcome. In conclusion, Fig. 25 shows how the laboratory was envisaged at the time[(96)].

Fig 25



LE LABORATOIRE DE MEYRIN

1. A l'extrême-droite : le Synchrotron à protons sous son remblai circulaire. — 2. A droite, en bordure de la route : la centrale électrique. — 3. Au centre : le synchrocyclotron, les laboratoires et les ateliers. — 4. A gauche : le bâtiment de l'administration et les buildings d'habitations, cantine, etc...

REFERENCES AND NOTES

The following abbreviations are used in the references:
HMSO, Her Majesty's Stationery Office.
AERE, Atomic Energy Research Establishment, Harwell, now AEA Technology, plc.
CERN/ARCH/-, CERN Archive. Papers of interest referred to in this report have been placed in box JDL0001. Copies of some may be elsewhere also. Any new interesting material found by the author after this report is written will also be placed here.
PRO, Public Record Office, Kew.
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