OXFORD'S
CLARENDON
LABORATORY

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Dedicated to, or to the memory of:

C.H. COLLIE (Christ Church)

my tutor who found me interesting jobs to do in the vacations and

RALPH BLOW (Brasenose)

who let me do them on the Teaching Course, mentioning me to his former tutor

RICHARD HULL (Brasenose)

who mentioned me to his colleague

NICHOLAS KURTI (Brasenose)

who gave me other jobs, mentioning me to his boss

F.E. SIMON (Christ Church)

who gave me a temporary job in 1947, mentioning me to

T.C. KEELEY (Wadham)

who gave me a permanent though departmental job in 1952 when acting for

LORD CHERWELL (Wadham and Christ Church)

who was not best pleased but was always very nice to me, and

BREBIS BLEANEY (St John’s)

my boss 1957 to 1977, and finally to

BILL MITCHELL (Wadham)

my boss since 1978 who has encouraged me to write this book as part of my job.

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APPENDICES

[Editorial note: This is an inferred list of Figures from references in the text.]

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PART I – PRE-HISTORY (1180-1749)

Chap. 1 Athens to Oxford

Aristotle gets a bad press from contemporary physicists. For example, Whittaker writes (1.1) "… Aristotle, whose natural philosophy was worthless and misleading from beginning to end". A pupil of Plato, his philosophical ideas still form the basis of the subject taught today as philosophy. In his eight books called Physica (from the Greek natural things) he identifies a subject which has become mathematics, physics, chemistry and astronomy dividing his subject matter into four heads. He treats 1. matter and form, 2. causality, 3. motion and 4. place and time. Aristotle undoubtedly gets a great deal wrong and this is largely attributable to the twin handicaps that he was by nature an observer and that in his time experiment had not yet been seen as an essential precursor to theoretical understanding. He could have performed Galileo's experiment on the falling of geometrically similar spheres of different mass but it was to be more than fifteen hundred years before anyone did, in spite of the continuing advocacy of the experimental method in which Oxford philosophers played so notable a part (see [Chapter 2]). Aristotle as a scientist corresponded more closely to our concept of a naturalist – he would have had much in common with Darwin but not with Newton.

But the sciences are essentially quantitative subjects and the ancient Greeks, however sophisticated their cerebration and advanced their geometry, were doomed without a workable system of computation.

The development of the physical sciences remained static in Europe for many centuries and it was not until the ninth century that the Arab civilizations were able to construct a quantitative mathematical system, drawing together Hindu arithmetic, Greek geometry and their own development of algebra, and inventing the decimal system of numerals used today. (The Arabic numerals were brought to the West by Adelard of Bath in the early twelfth century.) Meanwhile, practical chemical techniques were becoming established
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in quantitative terms – eg. A recipe for making white lead dates from the late eighth century (1.2) and there is a design for a chemical laboratory of a century later (1.3). Isolated experimentalists in physics are Alkindi (813-880), who worked in Basra and Baghdad on optics and density, and Alhazen (965-1038) also of Basra, who was the first to define the role of the eye as a receiver rather than as a transmitter and whose work on refraction by curved surfaces and reflection by spherical and parabolic mirrors was centuries ahead of its time.

The spread of scientific activity from East to West was not a gradual one over several centuries but started with a single event – the gift by Constantine VI of Byzantium to a Spanish caliph of a manuscript on pharmacology, translated into Arabic in about 950 by a Spanish Jew and translated into Anglo-Saxon in about the same year.

A scientific community thus became established in Spain which, from the mid-tenth to the mid-twelfth centuries, was to form the last link in the circuitous route from Greece and the Middle East to England, notably established by translators in Toledo of the works of

Aristotle and others from Arabic into Latin. (Toledo was at that time on the frontier between the Arabic-speaking southern part of Spain and the Europe-aligned northern Latin part.) There, in 1140, Gundissalinus was translating the works of Aristotle, Euclid et al from the Arabic into Latin and contributing to detail and arrangement. The major figure in Spain at this time was Averroes (1126-98) who wrote extensive commentaries upon Aristotle's works, including such unpopular doctrines as the infinity in time of the physical world – heretical to the Christian, Judaic and Islamic faiths. Translations into Latin from both the Arabic and Greek manuscripts were being made by scholars in Sicily and in northern Italy, some of whom had studied in Toledo.

Thus it was that the accumulated scientific learning – right or wrong – of ten centuries and embracing the Greek, Islamic and Judaic cultures was swept into a number of Latin translations assimilable by eager scholars of north-western Europe in general and in Oxford in particular.

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Nomenclature

The word physics had a rough ride between Aristotle's use of it and the late nineteenth century when its meaning became stabilized to the modern sense. Physics was not used in the intervening centuries except – usually as physic – as a synonym for medicine. But there is another more recondite exception. When Literae Humaniores was established as a school in 1747, it included in the title "…cum Mathematicus et Physicus" and this appears in translation as "Mathematics with Physics". In 1807 an advanced paper in Mathematics with Physics was set and a separate class list was published together with the Lit.Hum. class lists. The few surviving examination papers (first printed in 1828) show that physics was in reality mechanics. (For the relationship between Mathematics and Physics here and at Cambridge in the 19th century see page [239].)

Natural philosophy was until the 18th century used in the wide sense now covered by science but in the 18th century shed chemistry and embraced mathematics and physics as we now know them.

Mathematics came into use in the 17th century to include arithmetic, algebra and geometry and for a time those branches of physics which depended upon them. Until the 19th century the distinction between pure and applied was between pure and mixed.

The term experimental philosophy was introduced by Francis Bacon (1561-1626) – established in the title of a pamphlet published in 1651 by G. Thomson entitled A Vindication of Lord Bacon, the Auctor of Experimental Philosophy. This term is synonymous with physics between about 1650 and 1850 and is preserved in the title of the present-day head of the Clarendon Laboratory.

Natural Science, though ascribed by the SOED to late Middle English, was not apparently used in Oxford until 1852 when the still extant School of Natural Science was established.

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References

Chap. 1 Athens to Oxford

1.1 Sir Edmund Whitaker, A History of the Theories of Aether and Electricity, Nelson, revised 1951

1.2 Charles Singer, <u>A Short History of Scientific Ideas to 1900</u>, OUP 1959
1.3 <u>Ibid</u>.

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PART I

Chap. 2. Grosseteste and Bacon

The transformation of Oxford from one of many seats of learning to the country's first university is held to have occurred in 1167 when political unrest in our relations with France led to the expulsion of foreign scholars from the University of Paris and the banning of English scholars from studying abroad. Oxford had for long been of geographical importance as the crossing of a main North-South roadway – nearly 12 feet lower at Carfax than it is now – with the East-West river. But by the 12th century it had become a prosperous walled town, flanked by important religious foundations including St Frideswide's Priory (now embodied in Christ Church), Osney Abbey, Godstow Convent, Abingdon Priory and Beaumont Palace (on the site of the present Beaumont Street and including the church of St Mary Magdalen). The presence in the early 12th century of a scholarly establishment is for example proved, to take a document near at hand, by a short treatise by Theobald of Etampes written after 1119 in which he appends to his name "Magister Oxinefordie" (2.1).

An interest in physics as we know it becomes evident towards the end of the 12th century and the earliest name we have is that of Alexander Neckham (1153-1217), who taught in Oxford and among whose interests were the mariner's compass and the reflection of light.

The first notable luminary among the internationally-recognized school of Oxford philosophers was Robert Grosseteste (d.1253) (Fig. 1) who was to become the first Chancellor of the University. He was able to read Greek and Arabic and taught the New Learning to the Franciscans, though never joining the order himself. Grosseteste's principal contribution and one which it took several centuries to assimilate was to point to a three-stage process.

1. To formulate a theory.
2. To carry out an experiment based on the theory.
3. To refine the theory in the light of these observations.

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That Grosseteste was an experimental physicist is proved by the accounts he gives of his experiments.

The topic in physics of greatest interest to the early philosophers was optics and Grosseteste writes of rays and their reflection (the law concerning the latter being known to him) and their refraction (the law of which was unknown in his time), iridescence (ie. the production of multiple colours as in the rainbow), De Natura Locorum (a move towards the concept of focus as in the burning glass, which he knew about) and finally colour. These treatises show a clear passage towards the quantitative treatment of ideas which hitherto had been treated qualitatively and speculatively. Grosseteste made similar observations to his contemporaries upon the rainbow but, like theirs, his conclusions were unlucky.

In De Calore Solis (concerning the heat of the Sun), Grosseteste demonstrates his instinct for proposing sound doctrine in connecting heat with motion, although most of his speculations are wide of the mark.

Thus, Grosseteste was one of the founding fathers of experimental physics and in his time Oxford was an acknowledged leader in the western world.

But at the age of 60 this astonishing man left Oxford to become Bishop of Lincoln for 20 years. Lincoln was at the time the largest diocese in England, extending from the Thames to the Humber. In 1245 he travelled to Rome to argue with the Pope.

Roger Bacon (c.1219-1292) (Fig. 2)

A generation younger than Grosseteste, Roger Bacon was a Franciscan educated in Oxford and in the University of Paris, who returned to Oxford in 1249. He was very much a disciple of Grosseteste's and continued to urge the prime importance of experiment but also pressed the importance of mathematics as providing the key to the understanding of science. He also was able to read Greek and Arabic texts in the original and in his three works, Opus

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Maius, Opus Minimus, and Opus Tertium, he sets out in highly organized form the complete philosophical and scientific learning up to his time. His own contributions to physics included the results of experimental work in optics, notably with curved mirrors and the theoretical work required for the making of telescopes, microscopes and spectacles. But his contributions ranged over many other fields such as geography, cartography and calendar reform and his writings on astronomy are thought to have led Copernicus towards his discovery of the true nature of the solar system. His ideas concerning explosives he wisely left in code – unhappily decipherable. Roger Bacon was, in the words of Winthrop Woodruff (2.2) "… so far in advance of his time that he was scarcely able to influence it at all", and indeed was persecuted as a heretic.

Thus, it was that these two men working in Oxford put this country in the forefront in the development of experimental science.

There followed a number of philosophers who promoted the application of mathematics to the solution of problems in the real world by logical means rather than by experiment. One of these was William of Ockham (c.1284-1349), remembered for Ockham's razor which can be loosely translated as "minimize your assumptions".

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Chap. 2 Grosseteste and Bacon

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See also Anthony a Wood, Annals, Vol I, p140

2.2 Francis Winthrop Woodruff, Roger Bacon, London 1938

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PART I

Chap. 3 The Merton Philosophers

Merton College was founded in Surrey in 1264 by a body of scholars who had interests in common with Grosseteste's Oxford school. Ten years later the whole body moved to Oxford and the college became the headquarters of a school which gained respect among European universities. The founder of the Merton Philosophers was Thomas Bradwardine (c.1295-1349), whose contribution was to purge Aristotle's mechanics of unquantifiable entities such as "potency" and rather to insist upon the merits of restricting arguments to qualities such as velocity. Holding that mathematics was the key to the understanding of problems in mechanics – Albertus Magnus and Thomas Aquinas preferred a philosophical approach – he was one of the earliest to assign letters to quantities and stated Aristotle's law of motion in the form $v = P/r$ where P is the "power" and r the "resistance". Curiously, neither he nor his pupils proposed or performed any experiments to test this theory and it was, of course, three centuries before the true laws of motion were laid down by Newton. Nonetheless, Bradwardine had taken a decisive step forward and his Tractatus Proportionum, 1328, was also studied in Paris.

Bradwardine's successors who became known as the Calculatores included William Heytesbury (c.1313-1373), who was the first to define uniformly accelerated velocity, Richard Swineshead (floreat c. 1344-54), who left a Liber Calculationum, and John of Dumbleton (floreat c.1331-1349), who was among the first to apply graphical methods to mechanics.

When printing came in the 15th century, the work of the Oxford philosophers right back to Grosseteste (except for Dumbleton) was printed showing that it was still being studied. However, by the end of the 14th century there was among European philosophers in general and our own in particular a trend towards a return to non-mathematical lines of thought and to the study of the classical authors as opposed to the development and
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extension of their ideas.

When, in the late 15th century, the all-conquering Leonardo da Vinci (1452-1519) burst upon Europe, he ushered in a new breed of scientist – empiricists who took observation and experiment as their starting points and strove to deduce explanations from them. The heroes of renaissance physical science were, of course, Copernicus (1473-1543) and later Galileo (1564-1642), who fearlessly established a structure into which Newton could breathe mathematical life.

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PART I

Chap. 4 Sir Henry Savile and the Three New Chairs

Oxford was as able then as it is now to attract distinguished people as Heads of colleges. Sir Henry Savile (1540-1622), Warden of Merton for 37 years, was quick to see to it that the University kept abreast of the new scientific learning.

In 1619 – three years before his death – an Act was "Published and Confirmed" in Convocation as follows:

> I Henry Savile, Knight, seeing that Mathematical studies are uncultivated by our countrymen, and being desirous of supplying a remedy in a quarter almost given up in despair, and to redeem so far as in me lies, almost from destruction, sciences of the noblest kind do… found and establish for all future times in the said University, two lectureships or public professorships in the Mathematical Sciences, one in Geometry and the other in Astronomy.

(It is possible that Savile was being less than fair to Cambridge in the light of documentary evidence that a person unnamed was being paid to teach mathematics there in 1510.[1]) Savile goes on to specify the topics to be covered by the two professors and these go much further than might be expected from the titles to include the whole of mathematics and physical science known at the time, both pure and applied. For example, the Geometry Professor as well as arithmetic had to teach surveying, and the Astronomy Professor as well as teaching the up-to-date theories of Copernicus had to include optics and navigation. Further, the latter was "…utterly debarred from professing the doctrine of nativities (ie. horoscopes) and all judicial astrology without exception".

Savile was ahead of his time in several material ways. He requires the preparation of lecture notes, recommends informal instruction in English (rather than Latin) and provides for retirement due to infirmity on one-third

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salary, in which case the successor had to be content with two-thirds for the time being.

Thus, the initiative of one man, steeped in the classical tradition as he was – he taught Greek to Queen Elizabeth I, was her Latin Secretary and a translator of the Authorized Version – was responsible for the reintroduction of mathematics and science into this University, some fifty years before the founding of the Plumian and Lucasian Professorships at Cambridge.

The Savilian Professors

Until the appointment in 1699 of John Keill (see below) such teaching as there was of mathematics and physics was in the hands of the Professors of Astronomy and Geometry and these were men of widely varied and often combined talents. Over the Civil War and Protectorate periods their fortunes tended to be at the mercy of political pressures.

The first astronomer was John Bainbridge (1619)[2], a Cambridge man who combined mathematical astronomy with medicine, taking his Oxford DM in 1620 and leaving

[1] Author's thanks to Dr Elizabeth Leedham-Green, University Archives, Cambridge.
[2] Dates in this section refer to appointment

unremarkable astronomical writings. John Greaves (1643) was given to travel and collecting and, like an Oxford physicist of the present century (page [ ]), took measurements of the pyramids. Unhappily, he was ejected by the Parliamentarians in 1648. His successor was the remarkable Seth Ward who, having been relieved of a mathematical fellowship at Sidney Sussex College, Cambridge in 1644, spent five years as a private tutor and then appeared in Oxford in 1649. He advanced a theory of planetary motion, entered into controversy about education and, with John Wallis (see below) was in dialogue with Thomas Hobbes, the philosopher. Meanwhile, he had taken his DD in 1654 and left Oxford in 1661 to a rapid succession of ecclesiastical preferment culminating in his translation to the See of Salisbury in 1667 where he died in 1689.

The line of polymaths continued with the yet more astonishing Christopher Wren, who on graduating from Wadham College, became a fellow of All Souls and for four of his eight years there was Professor of Astronomy at Gresham College, London. For twelve years from 1661, Wren was Savilian Professor of
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Astronomy during which time he was heavily involved with the founding and running of the Royal Society and with learning about architecture. (Early examples of his work in Oxford are the Sheldonian Theatre (1669) and Tom Tower, Christ Church (1681-2).) He left papers on a vast number of subjects including some in medicine but it is, of course, as the architect of St Paul's Cathedral and fifty-two London churches that he is principally remembered.

Wren's successor was Edward Bernard (1673), distinguished only as tutor to two royal bastards. With the appointment of David Gregory (1691) the study of experimental philosophy as such began and, as we shall see, the teaching of it eight years later.

David Gregory (1661-1708) was a son of an Aberdeenshire doctor, who had become Professor of Mathematics in the University of Edinburgh in 1683. He was quick to see that his prosperity could only gain from an acquaintanceship with Newton and accordingly he wrote two letters to him which were not answered. He therefore engineered a personal encounter in London and by dint of well-judged flattery ingratiated himself with Newton to such effect that when in the same year (1691) Bernard died, it was Gregory whom Newton recommended for the Savilian Professorship of Astronomy rather than Halley, a colleague of great ability and a friend of many years standing. Gregory was the first author to treat gravitation.

From 1700 lectures on experimental philosophy passed out of the hands of the Savilian Professors until 1749 when an established Readership in experimental physics began. However, for the first ninety years this was held in plurality with the Savilian Professorships of Astronomy and we shall be reviewing the lives of these part-time physicists below.

The first Savilian Professor of Geometry was Henry Briggs (1620) who came from St John's College, Cambridge via Gresham College and was, apart from contributions to navigation, unremarkable*. His successor, Peter Turner (1631) was a Christ Church graduate, subsequently fellow of Merton and again served for eleven years as Professor of Geometry at Gresham College. Although also unremarkable as a mathematician, he is remembered for his help in drafting Laud's University Statutes. In 1641 he enlisted with the Royalist forces and this led to his ejection from his chair in 1648.
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The next Savilian Professor of Geometry, John Wallis (1616-1703), was remarkable in three distinct ways. Firstly, he remained in his chair for fifty-four years – even longer than the egregious Clifton whom we are to meet later. Secondly, though appointed during the Protectorate, he remained throughout the Restoration. But most notably, he was equal to telling Newton what to do. This is how he belabours him about not publishing his Opticks for thirty years.

> "… What trouble now, more than at another time. … And perhaps some other may get some scraps of ye notion and publish it as his own and then 'twill be His not yours; … I own that Modesty is a Vertue, but too much Diffidence (especially as the world now goes) is a Fault".

His major contribution to the development of mathematics was his Arithmetica Infinitorum, 1655 which paved the way for the later development of the differential calculus. Among his many activities the most remarkable is perhaps his two spells of duty as a forerunner of Bletchley Park – he decoded cyphers both during the Civil War and in 1690 for William III. Like some of the astronomers just reviewed, he served the Church though in minor roles and duly took his DD in 1654.

Surprisingly, the next Geometry Professor was none other than that famous astronomer, Edmond Halley (1703)[3] – for obscure reasons a blatant atheist such as Halley could not be the one though he could be the other – and he too was an active supporter of Newton. Halley remained in office for thirty-nine years for the last twenty-one of which he was also Astronomer Royal.

The Sedleian Professors and their Deputies

In 1618 – the year before the foundation of the Savilian Professorships – under the will of William Sedley Bart of Aylesford, a professorship in natural philosophy was established. The will specifies lectures at 8.00 am on Wednesdays and Saturdays and fines for non-attendance – ten shillings in the case of the lecturer and fourpence a head for members of his audience. The subject matter to be taught was Aristotle's Physica, de Caelo (the Heavens),
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Meteorologica, de Generatione et Corruptione, Parvia Naturalia (physiology) and de Anima. (This disastrously backward-looking step may have been one of the factors influencing Savile in the following year.)

It is not surprising to find the Sedleian chair occupied by a succession of doubtless distinguished divines, medical men* and lawyers. But one, Thomas Millington (1628-1704) a distinguished physician, had the grace to appoint a deputy to lecture on his behalf in experimental philosophy. John Keill (1671-1721), a pupil of David Gregory in Edinburgh, had been appointed to a post by Balliol College in 1694, which thus became the first college to appoint a physics tutor. (They seized a similar initiative in 1850 when they established the first chemistry laboratory in Oxford.*)

Keill gave the first official course of lectures in 1700 at Hart Hall (embodied in the original Hertford College in 1740). He had a strong instinct for authorship and in 1702 published a remarkable book originally in Latin and called Introductio ad Veram Physicam (Introduction to True Physics). The Latin text went into six editions up to 1741. In 1720 an English edition appeared and this went into five editions up to 1758 (Note – Keill died in 1721). This book was the layman's gateway to Newton's revelations in mechanics, and in the later editions, he included Huygens's work on circular motion. A Cambridge man discovered the laws of motion but a great many people saw the light through an Oxford writer.

[3] Said to be pronounced to rhyme with Crawley.

Keill had a lively interest in religious matters and as a Scot had sympathies with dissenters. Even so, it is surprising to find him leaving Oxford in 1709 to escort some [Protestant refugees] from the Palatinate to New England. He returned to Oxford in 1712 as Savilian Professor of Astronomy with the support of Newton and in preference to Halley. (See above.)

John Keill is of importance too for the major part which he played in Newton's long-standing controversy with Leibnitz about the priority of invention of the differential calculus and in the lesser engagement with Bernoulli. Although the ammunition was powerful, as will be seen, the tactics of the time demanded camouflage. Leibnitz and Bernoulli resorted to anonymity and pseudonymity while Newton for many years hid behind Keill and used him as his mouthpiece. Keill has been described (4.1) as "crude and abrasive" and
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Bernoulli disdaining mention of him by name apostrophises him as "a certain individual of Scottish race" (4.2). That a degree of intemperance suited Newton is demonstrated in a surviving manuscript (4.3) of a draft letter ostensibly from Keill to Bernoulli. Newton by no means tones down Keill's contumacious style but in his extensive amendments retains it. As the dispute raged on for some ten years – even continuing after Leibnitz's death in 1716 – nearly every mathematician on both sides of the Channel became obliged to take sides and the Oxford trio of Halley, Gregory and Keill were solidly behind Newton. This says much for the role of the Royal Society in uniting the two ancient universities.

Keill's lectures were taken over by John Theophilus Desaguliers (1683-1744), the son of a Huguenot schoolmaster who had fled to Guernsey in 1686 and had settled in Islington seven years later. Desaguliers came up to Christ Church and was lucky enough to be on the spot to succeed Keill. Although the latter showed demonstrations at his lectures, it was Desaguliers whose enthusiasm for designing experimental apparatus raised its deployment in lectures to a fine art. He left Oxford after four years when he moved to London as a private tutor, while becoming increasingly involved with arranging demonstrations for Newton and others at the Royal Society. He wrote text books on experimental physics and was the inventor of the planetarium. The Sedleian Professors thereafter did not accept responsibility for lecturing on experimental philosophy but with the appointment of Thomas Hornsby (1733-1810) in 1782 they moved into the mathematical sphere which is their realm today.

However, one of the Keepers of the Ashmolean Museum, James Whiteside (1680-1729) was a mathematician and physicist of distinction and from 1714 until 1730 kept the tradition alive.

Keill's lectures and those of his successors were given in the splendid quarters established in 1683 to house the Tradescant collections acquired by Elias Ashmole (1617-1692) together with his coins and his books. This energetic polymath had designed between the Sheldonian Theatre and Exeter College a complete institute for the study of science, which comprised a basement chemical laboratory, a lecture room, a library and a museum. It cost £4,500 and beggared the Bodleian Library. (The museum collections moved to
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the present Ashmolean Museum in 1899. The original Ashmolean Museum has been since 1935 the Museum of the History of Science.)

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PART I

Chap. 5 Boyle and Hooke

Returning temporarily to the seventeenth century, we must note the fillip to Oxford's scientific status given by the presence of Robert Boyle (1627-1681) and Robert Hooke (1635-1703).

The quickening interest in experimental philosophy had led to the formation in London in about 1645 of a society which met weekly to plan, conduct and discuss experiments in physical science. But with the Civil War in progress it was not surprising that, when one of them, John Wilkins (1614-1672), was appointed Warden of Wadham College in 1648, several of his fellow members moved to Oxford with him, including John Wallis who became Savilian Professor of Geometry (see above). As is well known, these two nuclei fused after the Restoration and became the Royal Society. But for some years, the Oxford contingent became important by the accretion of Robert Boyle and his hireling, Robert Hooke. Robert Boyle could afford to maintain a cherished independence of the University by refusing Wilkins's offer of accommodation in Wadham in favour of premises in the High Street opposite All Souls. But he took a full part in the meetings held under Rule 8 on Thursdays "before two of the clock" and noted by Wallis as "very considerable". In 1659 Wilkins became Master of Trinity College, Cambridge, whereupon the meetings were held in Boyle's premises. Boyle stayed in Oxford after the Restoration until 1668, leaving only Wallis behind.

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Important as Boyle's Law is to physics it is as the father of modern chemistry that he is remembered. Robert Hooke tends to be obscured by his employer and the contribution of this Christ Church man must not be forgotten. Hooke's Law – the mechanical analogue of Boyle's Law – is familiar, as is his development of the microscope and his superb Micrographia ([ ]) (5.1), but Hooke was the inventor of

the balance spring for watches
a brick-making method
the heliostat
the mercurial thermometer
many meteorological measuring instruments
telescopic sights for astronomical telescopes
the dividing engine for making scales
the first clock drive for telescopes
the Gregorian telescope mount
a calculating machine (though primitive)
the iris diaphragm (as used in the photographic camera)
a marine depth measuring method
the liquid diffractometer
the oil-immersion microscope objective, and
the optical telegraph.

Hooke was a brisk controversialist, sometimes verging on the cantankerous and was unlucky in his dealings with Newton, who was himself not remarkable for tolerance. In 1680, Hooke had been able by intuitive means to propose the inverse square law of gravitation to Newton, not knowing that he had been anticipated by six years and his reaction when Newton published his work was inevitable. The conflict was not eased when Newton was able to prove by his superior mathematical ability the empirical laws of planetary motion proposed by Kepler which, though understanding them intuitively, Hooke had been unable to prove mathematically. Hooke was perhaps the first physicist to illustrate in his career the positive virtue of experiment alongside the negative disability of not being the master of the relevant mathematics.

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Chap 5 Boyle and Hooke

5.1 Robert Hooke, Micrographia, London 1665

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PART II – BEGINNINGS OF EXPERIMENTAL PHILOSOPHY 1749-1839

Chap. 6 The First Readers

Nathaniel, Lord Crewe, Bishop of Durham (1633-1721) has gone into history as less saintly than might be expected – the usually restrained DNB says that his career was "discreditable". Concentrating upon his last hours we must be thankful that at least then his heart was in the right place. He left no written will but dictated his wishes to his Domestic Chaplain, Richard Grey, whose evidence was accepted by the court. The first clause provided for a Reader in Experimental Philosophy who was to receive £30 per annum – a sum which even at the time represented less than one tenth of an academic salary. (The later clauses provided for the augmentation of the salaries of specific professors and for the peaches and champagne enjoyed by the great and the good at the annual festivities in late June known as Encaenia.) The whole of Lord Crewe's estate was paid over to Lincoln College and it was some years before the University's share was separated out and handed over.

The salary being what it was, the first three Readers were obliged to hold the office in plurality with the Savilian Professorship of Astronomy, although the income was augmented by £100 per annum from the Civil List by George III – an early sign that physics was held in Royal esteem. The first Reader appointed in 1749 was James Bradley (1693-1762, FRS 1718) who had held the Savilian Professorship of Astronomy since 1721 and had become Astronomer Royal in 1742. Bradley had purchased Whiteside's demonstration apparatus in 1730 and continued lecture courses, some details of which are preserved in the Bodleian Library. Thus, Bradley anticipated his appointment to the new Readership by nearly twenty years. His lectures became famous for the large audiences he attracted – up to one third of the University, according to a contemporary source.

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Bradley is famous as the discoverer of the so-called aberration of light. In the belief that the stars were nearer than they in reality are, he looked for relative movement of the brighter stars against the background of faint stars as the earth describes its orbit. He could only explain his observations by ascribing a velocity to light which was not infinite compared with the earth's orbital velocity. These observations were made on a zenith telescope at Greenwich* and he published his value for the velocity of light in 1725. Incidentally, his observations provided the first experimental proof of Copernicus's theories. Bradley went on to prove the precession of the earth.

Bradley was succeeded by another astronomer of note, Thomas Hornsby (1733-1810 FRS 1763) who, realizing that the motion peculiar to each star (proper motion) would be an important first step in the formulation of any theory of cosmology, set out to determine with accuracy the positions of as many as he could. By the time he died, he had made a hundred thousand measurements and these are still regarded as of importance. Hornsby needed an observatory and, having failed to persuade the University, turned to the Radcliffe Trustees who built him the famous Radcliffe Observatory – now incorporated into Green College –

and furnished it with telescopes for the then very considerable sum of £30,000. As well as a successful astronomer, Hornsby stands out as a champion pluralist – for his latter 27 years he was simultaneously

1. Savilian Professor of Astronomy (1763)
2. Reader in Experimental Philosophy (1763)
3. Radcliffe Observer (1772)
4. Sedleian Professor of Natural Philosophy (1782)
5. Radcliffe Librarian (1783)

It is hardly surprising that Hornsby came to need a deputy to give his lectures in experimental philosophy and around the turn of the century this was undertaken by Stephen Peter Rigaud (1774-1830, FRS 1805) (Fig. 3), who succeeded him as Reader in 1810, becoming Savilian Professor of Geometry in the same year and moving to the Astronomy Chair in 1827. Rigaud is not remembered for his experimental and observational work but is of great importance for his illuminating essay of 1838 about the first publication of Newton's <u>Principia</u> in 1687 which had been procured by Halley. He also edited the correspondence of many earlier scientists, including that of his predecessor, James Bradley. [Page 24]

The notice reproduced in Fig. 4 has been dated at 1839 from the combination of the typography and the reference to "the late Reader", who was clearly Rigaud. We note that this was not a course of lectures leading to any kind of examination but it is complete and businesslike except for the concluding single word "electricity" which leaves us wondering about the inclusion of the experimental discoveries of Oersted (1820), Arago (1820), Ampere (1822-27), Ohm (1826) and Faraday (1821-1831).

<u>Accommodation</u>

Up to 1832*, lectures in experimental philosophy were given in the (old) Ashmolean Museum (see p.[16]) but in that year the Clarendon Press moved from the stately Palladian building at the eastern end of the southern side of Broad Street, now known as the Clarendon Building (see Fig. 5), to its present quarters in Walton Street. The first floor on the eastern side was equipped for lectures in physics and it is there that Walker would have given the lecture described by Tuckwell (see p.[30]) Thus for nearly 40 years began an adventitious association of Oxford physics with the name Clarendon.

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PART II

Chap.7 The British Association

An insight into the climate in the sciences in general can be deduced from the early years of the British Association for the Advancement of Science. Far from its modern role of popular reportage, it was conceived by a company of leading scientists "… to give a stronger impulse and more systematic direction to scientific enquiry …" and to attempt to unify the various disciplines. The inaugural meeting was held in York in 1831 and the second in Oxford. This may seem surprising but one of the original sponsors of the British Association was C.G.B. Daubeny (1795-1867, FRS 1822), the great polymath of Magdalen College, who became Aldrichian Professor of Chemistry in 1822 and for 21 and 15 years respectively simultaneously professor also of botany and rural economy. But more to the point are the numbers and avocations of the members.

	Total Membership	Oxford Professors and Heads of Colls	Members with Oxford Addresses	
			Laymen	Clerics
1831 – York	353	16	6	18
1832 – Oxford	600 plus	36	143	75

Thus, Oxford was at this time no kind of scientific backwater and, far from discouraging science, the Church participated vigorously in the new venture. (The greater clerical participation in York may have been due to the fact that its leading progenitor there, the Rev. W.V. Harcourt[4], was the son of the Archbishop.) That the Oxford meeting of the British Association was taken seriously by the most eminent in scientific research is shown by the presence of Michael Faraday, who demonstrated his discovery of electro-magnetic induction – then less than a year old. Using a coil of 800
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turns which he brought with him and a "large" permanent magnet[5] in the (original) Ashmolean Museum he was able to produce a spark.

[4] Father of A.G. Vernon Harcourt, Dr Lee's Reader in Chemistry at Christ Church 1859 to 1902
[5] Since lost

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PART III - FULL-TIME PHYSICISTS 1839 AND ON

Chap.8 Walker and the University Museum* (Fig. 6)

Rigaud was succeeded by Robert Walker (1802-1865, FRS 1831) an exhibitioner of Wadham College, who had taken Class I in Mathematics and Class II in Lit. Hum. in 1822. He was Chaplain of Wadham in 1826 and, on his marriage in June 1831, was obliged to resign. He became Reader in Experimental Philosophy in 1839 and was inducted as Vicar of Culham in 1848. He was several times an Examiner in Mathematics and, after the syllabus reform of 1852, frequently Examiner in the School of Natural Science. Walker was an enthusiast for lecture-demonstrations and drew upon the fund established by Lord Leigh (1791-1850) at Oriel College, then bringing in £85 per annum. (Much of his and his predecessor's apparatus is on view in the Clarendon Laboratory, including the well-known dry pile described in Appendix A.)

Unease about the positon of the sciences in the University had been felt since an attack in the Edinburgh Review in 1810. In spite of the spirited defence put up in later articles in that journal and in the Edinburgh Quarterly, it was a glaring shortcoming as the century progressed that, in spite of various lecture courses given by able scientists, there remained but one examination in Literae Humaniores, with its optional additional paper in mathematics and mechanics.

Public expression was given to the disquiet in a lecture of 1832 by Baden Powell (sic – 1796-1860), who was Savilian Professor of Geometry from 1827 to 1860, when he urged the compulsory study of mathematics or experimental science by all candidates for degrees. Under his leadership, a new Examination Statute was debated in Convocation in 1839 but was heavily defeated.

However, some nine years later there was a fresh move from within the University to which Walker's contribution was crucial. A memorial was sent in 1848 to the Hebdomadal Board (sic) from 45 of the 64 Tutors in which it was proposed that classical studies should be completed by the end of the second
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year and the following year spent on theology, philosophy and ancient or modern history, or mathematics and physical science. Among the chief proponents of this change were Walker's usual allies, H.W. Acland[6] and C.G.B. Daubeny, but Walker would not add his signature, preferring to write (8.1) to the Vice Chancellor with the proposal that, instead of the two alternative courses, there should be three from which a candidate selected two. These were (using today's nomenclature)

1. Moral Philosophy and Ancient History
2. Mathematics
3. Physical Sciences and Physiology

To these Walker adds, curiously, philosophy for Honours candidates. He favours attendance at both college and University lectures, especially the latter and suggests the production at the examination of certificates of attendance. His common sense is displayed

[6] Henry Wentworth Acland, 1815-1900, FRS 1847, Regius Professor of Medicine 1858-1894

in his rejection of theology as "not a good instrument of education", adding "Do we want a University or a Theological School?" Again he writes, "… it is desirable … to ensure, if possible, accurate knowledge rather than superficial information upon many points". He concludes with these words: "It is to say the least discreditable that anyone should go forth from us in utter ignorance of the laws that have been impressed upon matter or that he should suppose that earth, air, fire and water are the four elements of which the world is composed and that the communications of the electric telegraph are made by pulling the wires".

In the event, the new Examination Statutes of 1852 embody one potentially disastrous lost cause: the universal obligation to study Lit. Hum. right up to the final examination. But otherwise the scheme must have been to Walker's liking, even to the point of excluding theology. The going had been hard. We find four "schools" defined, the first being compulsory with an option of one of the others, namely

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> "1st School, to be passed first, and by all, called the School of Literae Humaniores,
> 2nd School, that of Mathematics,
> 3rd School, that of Natural Science,
> 4th School, the School of Law and Modern History"

Natural Science is further divided into Mechanical Philosophy (mechanics and physics), Chemistry and Physiology.

However, there is a note in a syllabus which stifled the development of Oxford physics and which must have given great pain to Walker.

> "NB. It must be observed that the above subjects are not expected to be treated Mathematically. Mathematical symbols may be employed so far as they express laws and results with precision but Mathematical reasonings belong to the School of Mathematics." (8.2)

It is not possible to trace how long this nonsense persisted.

In 1853 a single candidate was awarded an unclassified degree in the new School of Natural Science. From 1886, when the subject was first specified, the numbers taking physics climb unsteadily from 5 to 13 in 1910. The examination papers distinguish between those for the Pass Degree and those for the Honours Degree but both show a dismal standard. For example, the Pass paper for Mechanical Philosophy in Easter Term 1858 sets seventeen questions (no choice), most of which ask simple questions which a child of twelve would find easy today. The most demanding is question 17, "Explain the methods of obtaining a spark from a natural or artificial magnet". An Honours paper (then called a "Class" paper) on Heat, of Michaelmas Term 1869, is of necessity equally phenomenological but shows a remarkable overlapping: three questions concern thermometers, including the distinction between temperature and quantity of heat, nine are about change of phase and vapours, and one about pyrometry.

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Robert Walker was not above giving popular lectures. Tuckwell (8.3), writing (by internal evidence) in the early 1840s, says of him "… lectures in the Clarendon (ie. in Broad Street) by a cheery Mr Walker, who constructed and exploded gases, laid bare the viscera of pumps and steam engines, forced mercury through wood blocks in a vacuum, manipulated galvanic batteries, magic lanterns, air guns .... A wicked wag loaded the air gun before the professor entered and, when the trigger was pulled, we saw some plaster fall from the ceiling and a clatter was heard presently upon the staircase. The Bullet had gone up into the lecture room above and put to flight another professor with his pupils".

In 1847 The British Association again met in Oxford when the secretaries were Walker and H.W. Acland. Sir Robert Inglis was President. After the meeting, Walker, Acland and two colleagues wrote a "Memorial" and circulated it among members of the University, advocating "… the erection of an edifice within the precincts of the University for the better display of materials illustrative of the facts and laws of the natural world. And … we should recommend that there should be one or more lecture rooms arranged in a manner suited to Demonstrative lectures and an apartment calculated to serve the purpose of a Library and place for Scientific meetings, as occasion may require …" They had hoped for the support of the celebrated geologist, William Buckland (1781-1856, FRS 1818) but received the reply: "Some years ago I was sanguine as you are now as to the possibility of Natural History making some progress in Oxford but I have long come to the conclusion that it is utterly hopeless. The idle part of the young men will do nothing and the studious portion will throw their attention into the channel of honours and profits which can alone be gained by the staple subjects of examinations for Degrees and Fellowships.

At present it is a detriment to a Candidate for either to have given any portion of his time and attention to objects so alien from what is thought to be the proper business of a University as Natural History in any of its branches.

I therefore return the paper, which I think it would be useless to put my name to".
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Walker's demands for his own accommodation included a separate lecture room for 250 for his exclusive use (8.4) (his bid was unsuccessful and it was to be 120 years before physics lecture audiences reached this figure), space to exhibit his collection of apparatus, which he says he believes to be the best in the kingdom, and somewhere to wash. (It is not decorous, he says, that the Reader should be compelled to wash his hands in the lecture room itself.) In the event, the University Museum (Fig. 7) was built at a cost of £85,000 and Walker had the ground floor of the south side of the west frontage. It is curious that there was little space for experimental work by undergraduates, although in 1861 a notice advertises that the Professor of Experimental Philosophy will give "practical instruction" daily. However, as we shall see, more space was found to be necessary only a few years later and, in 1870, the first Clarendon Laboratory was in use. It is especially strange that Walker, pressing in his other demands as he was, did not include adequate laboratory space in view of his colleague Acland's observation to the Royal Commission of 1851-2. "… until provisions are made by which students can work practically themselves without inconvenience, no real progress will be made by them." In their report the Commissioners "heartily concurred".

Robert Walker became (officially) Professor of Experimental Philosophy in 1860 but only three years later, after two years in his new quarters in the University Museum, he fell ill and a deputy (G. Griffiths) had to be appointed. In 1865 he died at the sadly early age of sixty-three. (There being no retiring age until 1922, Professors of the time often went on doing useful work into their eighties.)

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References

Chap. 8

8.1 Oxford Tracts 1828-1848, Bodleian G.A. Oxon 8° 77
8.2 F. Sherwood Taylor, Annals of Science (1952) 8, 82

8.3 W. Tuckwell, Reminiscences of Oxford, Cassell 1900
8.4 H.M. Vernon and K.D. Vernon, A History of the Oxford Museum, Clarendon Press 1909, p.50

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PART III – FULL-TIME PHYSICISTS 1839-1919

Chap. 9 R.B. Clifton and the Establishment of the original Clarendon Laboratory

In the year before Robert Walker died, H. von Helmholtz (1821-1894) of Heidelberg came to Oxford to stay with Professor Max Muller[7] (1823-1900), the pioneer of philology who became an important figure in the University. In a recent paper (9.1) N. Kurti has quoted a letter from Helmholtz to his wife which may be translated:

> "Yesterday I travelled to Oxford early and stayed with Max Muller. He is a young, elegant, urbane man quite unlike any professor of philology whom I have met and taking everything in extraordinarily quickly even unfamiliar scientific concepts. His wife is an English woman also very likeable, attractive, educated, pretty and forthcoming so that I spent two very delightful days there. Oxford is perhaps unique, with its fine old well-cared-for buildings, trim lawns and beautiful trees, all exceedingly grand and of extraordinary splendour. We can really have no adequate idea of it without seeing it and now I understand the love of the English for their old university. Although it is clearly admirably well-suited to the education of gentlemen, it is limited for science and it would take a strong interest in science not to sink into idleness. M.M. is perhaps at present the only man who really works here."

What happened, if anything, after Walker's death in the following year is not known but in the year after that, 1866, there is another letter from Helmholtz to his wife, the text of which appears in two versions – with and without the words in brackets:

> "I had to be back at 11 o'clock for breakfast with M. and Professor Smith[8], the mathematician from Oxford. It came up in conversation that they had wanted to ask me to come to Oxford as Professor of Physics. But they could not offer a salary of more than £700 a year, which though more than we get in Heidelberg, would not be enough to live on as comfortably in England.
>
> Thus, I find it proper for Professor Max Muller to have explained (to me) that he could definitely say that I would not come for that reason."

Particularly in view of the dismal record of Oxford physics from 1865 to 1915, it is grieving to contemplate the possibility that, for the sake of a trivial adjustment in salary, we might have attracted Helmholtz here. As the
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whole physics community knows only too well, virtually no research was undertaken by Walker's successor over these 50 years and it is tempting to lay the blame on the University authorities of the day. But we must see the position as they did at the time.

A short list of eight was drawn up. One was G. Griffiths, six have been lost track of and the other was Robert Bellamy Clifton (1836-1921, FRS 1868). He had graduated from St John's College, Cambridge in 1859 as Sixth Wrangler and second Smith's Prizeman, although his colleagues thought he deserved better in the class list. In 1860 he published a

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[7] Came to Oxford 1843. Taylorian Professor of Modern European Languages 1854. First Professor of Comparative Philology 1868.

[8] Henry Smith, Balliol College, 1826-1883*, FRS 1861, Savilian Professor of Geometry 1860.

paper entitled On the Conical Refraction of a Straight Line (9.2). In the same year he was appointed as the first Professor of Natural Philosophy at Owen's College, Manchester under Sir Henry Roscoe, who later wrote in lyrical terms of his ability and popularity. He published a paper with Roscoe on flame spectroscopy of metals and their oxides (9.3) and another by himself in which he speculates about the emission of light (9.4). His application for the Oxford Chair was supported by a galaxy of famous names: Stokes, Thomson, Adams, Whewell (Cambridge), Joule, Roscoe (Manchester), Bunsen (Heidelberg) and Kirchoff (Breslau). The latter two were acquaintances of Roscoe's. Foreshadowing his choice of priorities over the coming half-century, he laid stress in his application on the importance he attached to teaching, and in it to a strict foundation on sound mathematical principles. With his highly respected academic career, solid achievement in both teaching and research and a wealth of distinguished support, no body of electors at any time and in any place could have done otherwise than to appoint him.

However, if the Tripos papers of 1859 reflect the material taught at Cambridge at the time, the truth emerges that Clifton learnt virtually no physics at all there. He sat sixteen papers embodying 190 questions, 99 of which were of more or less pure mathematics, 51 statics and dynamics, 14 optics (mainly geometrical), 13 astronomy (mainly planetary), 9 hydrostatics and hydrodynamics, leaving only 4 on physics, and at that confined to properties of matter. It is hardly surprising that the Oxford finals papers reflect, except for mathematics, similar proportions of subject matter and it is not surprising that students had to go to London or Cambridge to study electricity.

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Robert Bellamy Clifton (Fig. 8) was the only son of Robert Clifton, a substantial landowner with properties in Gedney, Sutton, Lutton and Fleet in Lincolnshire, Newton in Cambridgeshire and West Walton in Norfolk. (At the time of R.B. Clifton's death in 1921 his estate (9.5), including his Oxford house, was valued at £158,000 – today he would rank as a millionaire – and in the event of his descendants not surviving it would have passed in trust to the Clarendon Laboratory.) He was educated at Brighton College and for a time at University College, London, but went on to St John's College, Cambridge. As a man Clifton was of immense stature – a former pupil and Demonstrator, O.F. Brown (9.6) writes of his having had "… perhaps the biggest hands and feet I have ever seen …" He is remembered for his kindness and geniality and specific evidence for this lies in his not merely giving regular Sunday afternoon parties for his undergraduates but in re-establishing them in 1895 after a gap of two years during an illness of his wife. He lived firstly in Crick Road (just to the north of the University Parks) and then in 4 Park Town, called Portland Lodge.

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Early in 1866 Clifton approached the University for the cost of alterations to his accommodation in the University Museum and for his running expenses, and his requests appear in the first column of the table below. The Hebdomadal Council set up a Committee to negotiate with the Professor and revisions were arrived at with a view to financing his immediate needs. (See second column of table.) At a meeting of Convocation on 16 March 1866 pared-down sums were passed, in three cases by vote and two <u>nem. con</u>. (see third column).

<u>Applied for</u>	<u>Revised</u>	<u>Actual</u>
ASSISTANCE		
Demonstrator £150-200	"Skilled Servant"	Skilled Assistant
"Skilled Servant" £75-100	"Servant"	Servant

"Servant" £30-40	Total of £120 p.a.	£110 p.a.
APPARATUS		
£90 p.a.		
£252 arrears of apparatus fund	ditto	£150 p.a. for 3 years
£200 p.a. for 3 years		
LECTURE EXPENSES		
"Current expenses"	£60 p.a.	£50
ACCOMMODATION		
Unknown	Unknown	Up to £110 plus up to £60

In November 1866 Clifton was joined by the Professor of Chemistry, B.C. Brodie (1817-1880) in complaining to the Delegates of the University Museum (9.7) about "… defective warming, lighting and ventilation …" in their quarters, although chemistry had more to put up with than physics. Clifton went on to ask for "sun-burners"[9] and dark shutters.

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Then in May 1867 came the breakthrough that every new professor hopes for but few have experienced in under two years – a letter came to the then Vice-Chancellor (F.K. Leighton) from the Clarendon Trustees over the signature of none other than Mr W.E. Gladstone[10], announcing the offer by the Clarendon Trustees of a building costing about £10,000 to be new quarters to house the Department of Experimental Philosophy. The story extending over two centuries of the source of these funds and the text of Mr Gladstone's letter are to be found in Appendix D.

While Clifton was in consultation with the architect, T.N. Deane and with William Thomson (1824-1907 – Lord Kelvin 1892) at Glasgow, in February 1867 an approach to Hebdomadal Council was made, in the form of a Memorial signed by 120 Members of Congregation, urging the case for a swimming bath and suggesting this as an alternative use of the Clarendon Trustees' funds, although one Senior Member wrote pointing out the risk of drowning accidents. In January 1868, two Members of Convocation circulated Members of Congregation complaining that Council had failed "… in respect of Congregation and to the Clarendon Trustees in not making them aware of the expressed wish of a majority of the Resident Members of the University". Luckily, no further action was taken.

The vote in Convocation accepting the Clarendon Trustees' offer was scheduled for Tuesday 4 February 1868 and it was therefore very much at the eleventh hour that another alternative use for the £10,000 was advanced on 30 January 1868. An unsigned circular of that date reads: "The occasion seems

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most appropriate for inviting attention to an institution of the University which has a great field of usefulness before it … and is now in need of additional means to fully meet the views stated in the Founder's Will… The generous and enlightened support of Members of Convocation is consequently called for in the hope that, at least, some portion of the Fund, now about to be appropriated by the Clarendon Trustees, may, with their consent, be devoted to extend the benefits of the Taylor Institution". Again no further action was taken and indeed none could have been taken with the finance securely in the hand of the Clarendon Trust.

Clifton set out the case for a physics laboratory and his requirements in a circular (9.8) to Members of Convocation dated 15 January 1868. Making the point that physics had

[9] A primitive form of ceiling light fitting using an array of spread-out jets of coal gas unmixed with air.

[10] MP for the University 1847-1865, Chancellor of the Exchequer at the time and Prime Minister 1868.

recently become "greatly extended" and that no knowledge of chemistry or physiology can be "sound and accurate" without training in practical physics, he obliquely implies that these subjects had their laboratories and thus physics should also. Further, he points out that in Glasgow a physics laboratory had been running for some years and that active steps to this end were under way in London and Manchester. He rather labours the point that experiments in physics laboratories may suffer from "mutual interference" and overdoes it by requiring at least nine but preferably eleven separate rooms. He has the sense to include a room for calculation of results – a recurring problem to this day. As well as a large lecture room – in the event seating about a hundred – he includes a smaller classroom for mathematical teaching. The provision of a darkroom shows that he is up-to-date in recognizing the value of the new photographic techniques. He includes large glass-fronted cupboards for storing apparatus (a few still survive and very well-designed they are) and a store for materials. The one glaring exception is the lack of any workshop.
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A significant inclusion is a pair of rooms for the Professor's "private research". He concludes with a guarantee that he will keep within the funds available from the Clarendon Trustees and will make no demands upon the University. When, by the death of the Duke of Newcastle in 1864, the number of Trustees had fallen to three (Mr Gladstone, the Marquis of Lothian and Sir William Heathcote), the Duke of Marlborough and the Earl of Caernarvon were enrolled and it was these two who signed "orders" (ie. cheques) except in August when they were no doubt otherwise occupied and Mr Gladstone, then PM, and Sir William Heathcote stood in for them. Convocation duly agreed on 4 February 1868 to accept the offer nem.con. But one voice was raised in gently mockery, for two days later a pamphlet (9.9) appeared over the pseudonym Mathematicus and this is given in full in Appendix B. It was from the pen of the Christ Church mathematician, C.L. Dodgson (1832-1898) otherwise known as Lewis Carroll.

Curiously, there was no transfer of money from the Clarendon Trustees to the University – the administrative arrangements of the building work were handled by the Trustees and their solicitors in London, Boodle and Partington of Berkeley Square, and the University solicitors, Morrell and Hawkins in Oxford.

The eight tenders were opened on 10 July 1868 in the house of the Secretary of the Trustees, Sir William Heathcote, in the presence of Sir Thomas Deane and another anonymous person. The lowest, that of Messrs Symm – still in business in Oxford – was found to be £1,200 in excess of the available funds and Deane was asked to amend his proposals in consultation with Professor Clifton. This evidently went smoothly because by 31 August the contractors raised two objections which threatened the Trustees with "great additional expense". They were told "either give up the contract or withdraw". The progress of the building can be gauged by the transfers of cash from the deposit account at the London and Westminster Bank: £1,200 in January 1869, £1,600 in June, £1,600 in August, £1,500 in March 1870, £1,600
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in June and finally £1,800 in March 1871, leaving £600 on deposit. Each payment to the Architect, contractors and Clerk of Works involved this chain: Symm – Deane – Morrell and Hawkins – Boodle and Partington – Duke of Marlborough – Boodle and Partington – Earl of Caernarvon – Boodle and Partington – London and Westminster Bank – Morrell and Hawkins – Symm, the traditional 6s. 8d being the solicitors' fee each time. There was also much exchanging of paper when sums were moved from the deposit to the current or "drawing" account.

There was an exchange of letters between Clifton and Lord Caernarvon from early in 1871 which has perished in the purge at Boodle and Partington of 1951. These must have concerned the spending of the balance on what appear once in the Bill Book as fittings.

The new "Physical Laboratory" [(Fig. 9)] – its eponymous title does not appear in the University Gazette until October 1873 – was to have been ready for Michaelmas 1870 but then as now building projects ran late and the opening of the Professor's lecture course had to be postponed until 31 October.

A final meeting of the Clarendon Trustees was arranged for 27 July 1872 at 10 Downing Street but the other Trustees failed to turn up and arrangements were made to wind up the Trust by correspondence.

As the remaining balance dwindled the correspondence between Professor Clifton and Lord Caernarvon became lengthier and more frequent and there were journeys to Oxford by members of Boodle and Partington staff about the details of the inscription on vellum[11] by a Mr Wyatt, and of the carved lintel, part of which is preserved. Finally, the Trustees' account was closed in October 1874. (This lintel reads "1872" but it is certain that the new building came into use late in 1870.)

There happened to coincide in the year 1877 three events which were to have major effects upon Oxford physics. (In this year also there appeared Clifton's one research paper in his 50 years as Professor – a paper which banishes any regret that there were not more.)
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The effects upon physics were wholly beneficial. Up to this point, it will be remembered, there was no alternative for would-be mathematicians to taking the only possible course leading to the First Public Examination called Honour Moderations and today known as Classical Mods. The new statutes of 1877 laid out an alternative course of thoroughly up-to-date mathematics and this – later named Mathematical Moderations – was the preparatory course also for intending physicists. This somewhat rigorous curriculum persisted until 1939 when the present, though not universally popular, Science Moderations was introduced. Secondly, the number of "Honour Schools" was increased to six and – more importantly – candidates were expected only to take one, in the case of physicists the Honour School of Natural Science. Further, a candidate could now choose one out of the three subjects physics, chemistry and biology rather than the previous two out of three (p.[ ]). Thus, the obligation of even a token study of classics was lifted from the time when the initial hurdle known as Responsions was cleared.

Another introduction in 1877 was the Preliminary Examination in Natural Science which ensured an adequate background of elementary physics and chemistry, at first sandwiched between the prototypical Maths Mods and the Honour School of Natural Science. Later, Prelims became, as it still is, an alternative First Public Examination. There was from the first a practical examination in chemistry but there was not one in physics until 1891. At this point Prelims made a positive impact upon Clifton and his Clarendon Laboratory. He had the unwelcome task of being forced to find laboratory accommodation for undergraduates most of whom would be moving on to chemistry, medicine etc. In 1892 he reports (9.10) that, as well as thirteen honours men (combining those working for the Final Honours School of "Natural Science" and that of "Mathematical and Physical Sciences", being the reincarnation of the optional paper in Literae Humaniores which was in fact mathematics and
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[11] Visible on the first floor of the Lindemann Building, Clarendon Laboratory

mechanics), he has to accommodate thirteen Prelim candidates plus “three ladies”, one of whom was working for honours in physics. This obliged Clifton to limit the days for practical work by honours undergraduates to three per week. Clifton was never slow to commit his grievances to print and in some detail, so it is surprising that there is no mention of the difficulties which must have arisen when the laboratory rooms had to be cleared of advanced experimental apparatus to make room for the more elementary, and vice versa. This remains a burden in our electronics course on Wednesday afternoons.

The Universities’ Commission of 1877 arose from a meeting convened by Dean Liddell after which he wrote to the Chancellor, Lord Salisbury. The government responded and the words of the preamble to the Act, quoted by Engel (9.11) run “it is … expedient that provision be made for enabling or requiring the Colleges … to contribute more largely out of their revenues to University purposes”.

The effect upon Oxford physics in the long term was the establishment of a second professorship, to which J.S.E. Townsend (1868-1957, FRS 1903, knighted 1941, see p.[50]) was appointed in 1900 (Fig. 10) and the building of the Electrical Laboratory in 1910 (Fig. 11) and we come to their consequences to Clifton below. But our interest in the context of 1877 is the detailed snapshot provided by the verbatim evidence from the Professor, running to eight closely printed pages.

Clifton starts his evidence with a statement of the purposes of a department such as his as being unequivocally teaching <u>and research</u>. He implies that he is expected to give general lectures to non-physicists but quite reasonably rejects these as of little value – “… very little more educational effort (<u>sic</u>) is produced than by an exhibition of conjuring”. So far so good, but when it comes to quantitative detail, Clifton does somewhat stretch credulity. He says that the preparation for a single lecture takes up to 20 hours. As to his work in the laboratory, he is there for 76 hours per

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week. One is left wondering whether his pupils might not have learnt more if they had been supervised less. Even so, and even with not more than his full complement of 16 undergraduates, he goes on to say: “It seemed to me that I had to choose between teaching the whole subject very badly and teaching a portion of it fairly well and I chose the latter. The laboratory work has been almost entirely confined to three branches: the methods of weighing and measurement, which is part of mechanics, the study of heat and the study of optics”. No doubt Clifton, the professor, was able to justify to himself this exclusion of an increasingly important branch of his subject by the reduction in space forced upon him in 1868. And annually in his Reports he mentions the need for extending his building.

While including research as one of the purposes of his laboratory and mentioning the room in which “… to carry on private research”, he launches upon this statement:

> “… during the last session, for the first time since I have been in Oxford, having two very efficient demonstrators, I endeavoured to undertake original work … but I am sure that it has seriously interfered with my attention to the laboratory students and I should hardly venture to undertake such work again”.

This was, indeed, the end of Clifton’s active involvement in research but we shall see that there is a little more to be said on the subject to support the view that he had none of the positive antagonism to it that is sometimes attributed to him.

When speaking of the case for more University appointments, Clifton’s ideas again appear larger than life. He enumerates the branches of physics as he saw them – and unluckily he sees electricity and magnetism as “distinct branches” more than 40 years after the discoveries of Oersted, Faraday <u>et al</u> – and, because according to him no single person can encompass the whole range, he proposes a total of five professors. In the event, in negotiation with Council, the Commissioners proposed two additional professors and two demonstrators, including the professorship which Clifton supported in experimental mechanics and engineering. (The 23 years' delay before New College was able to fund what became the Wykeham Professorship was due to the agricultural depression recently treated in Engel's From Clergyman to Don (9.12).

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It was also in 1877 that R.B. Clifton presented a paper (9.13) to the Royal Society about the fruits of his single programme of research experiments which he began in the preceding year. The title is On the Difference of Potential Produced by the Contact of Different Substances and this is misleading because much of the paper includes work on voltaic cells composed of the same plates of different metals as he uses for his condenser (capacitor nowadays) but with an electrolyte between them. Voltage measurements are made using a quadrant electrometer and where he quotes several readings for one experiment, these show a scatter only of 1% which is commendable with this instrument. However, it is to be regretted that in quoting the voltage of various pairs of different metals relative to a Clark standard cell, he implies a precision of one part in 6,000. It is with these numerical results that the paper ends – there is no analysis of or comment upon them, let alone the drawing of any deductions from them. One way and another, Clifton's account of his one piece of research must be reckoned as of no significance[12].

For about a decade, Clifton had imposed on him an intolerable degree of overcrowding due to outside forces which he would have been wholly justified in resisting. The University was rapidly expanding its entry of chemists and pre-clinical medical students and it was not to be until around 1930 that it was made the rule rather than the exception for science to be adequately taught in the schools. The intake of 13 prelim. candidates, which we have noted above for 1892, according to Clifton's Annual Report of 1901 "… severely taxed the resources and absorbed the whole time of the teaching staff". In 1894 the number had risen to 27 and applicants were being "turned away" and by 1898, by unknown expedients, it was possible to accept 48. However, salvation was in sight – the teaching and practical accommodation of Prelim. candidates became one of the responsibilities of the new professor. (Clifton then complains that, because his fee income is reduced, he cannot afford to have the roof repaired, so as to overcome its penetration by rain.)

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Overcrowding was not only a problem in teaching Prelim. candidates, it was the problem in examining them which was to become acute in later years. Clifton reports that the invigilation of 50 examinees in six rooms by two examiners cannot be "satisfactory".

The number of honours candidates as well was still growing steadily, rising steeply from not more than 20 up to 1906 to a peak (in Clifton's time) of 38 in 1910, though falling by about 20% in the following two years. In 1910 some honours men had to be turned away and the impossibility of doing research is mentioned yet again. Although from 1901 Townsend was teaching honours candidates in his temporary accommodation (see p.[52-53]), this did nothing for the Clarendon Laboratory since Clifton claimed never to have had space to teach electricity. Further, his claims for an extension to his department fell unsurprisingly upon deaf ears in the University. There seems little doubt that there was an overwhelming case for more accommodation, however ramshackle, for teaching physics in Oxford. We do not know whether Clifton took his complaints further than to the Delegates

[12] Acknowledgement to J.H. Sanders

of the University Museum, although at the time that should have been far enough. But nothing was done.

The overcrowding makes it the more remarkable that Clifton accepted a research student, an Edinburgh graduate named Milne, for the degree of B.Sc. in 1905 on optical rotation. But before he could write a thesis, he was offered a position in his own university.

Boys's Determination of G

This classical experiment is of course the one physics measurement of importance made in the Clarendon Laboratory in its first fifty years. Boys had set himself the target of one part in 10^4 and it took only a few preliminary experiments to convince himself that he must find somewhere to do the experiment where the vibration was less than in his own premises in South Kensington. Oxford may not have been his first choice. It was true that there was likely to be available space and relative freedom from vibration but what could be expected from Professor Clifton, already famous for having done

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virtually no research over thirty years? In the event, two adjoining cellars accommodated the apparatus very conveniently and, although measurements were affected by railway and road traffic, there was sufficient time during the nights to enable measurements to be made. As for Clifton, he is said to have been most welcoming and helpful. Any lack of interest in research clearly did not apply to experiments by visitors. The work extended over three years and Boys came down to Oxford most weekends and during his holidays.

Boys was very alive to the importance of scale and by no means fell into the trap of saying "The bigger the better". In a careful analysis (9.14) of the accuracy to which each quantity can be measured, he notes that it is the measurement of the geometry of the system which is adversely sensitive to increase in scale. In discussions with colleagues, notably Poynting, his conclusion that a modest size was optimal was strenuously contested but he held to his point and, in the event, proved it. (The large spheres were 7.4 kilograms, the small ones 1.3 grams.)

His result of 6.657 (6) x 10^{-8}gm^{-1} cm^{3} sec^{-2} has stood the test of time in that P.R. Heyl in the US in 1930 (9.15) reached a result differing by only 0.2%. A major contribution to Boys's success was in his use of quartz fibres in the suspension of the oscillating beam and he regards as a major source of error disturbances due to convection currents, although he minimizes their effects by enclosing the moving parts within an octagonal shield.

Clifton's Achievement

Clifton regarded his mission in life as teaching young persons to become teachers themselves and there is every reason to believe that he lived up to this ideal all though his life. He spanned a time in which there was a great expansion in science teaching in the schools.

The design of apparatus was Clifton's main practical activity in physics and the following summarizes his achievement in this field.

Quadrant electrometer 1886

This devilish instrument was the only sensitive electrostatic voltmeter until Lindemann's electrometer of 1922 (see p.[80]). It was designed on the same mechanical principles as the galvanometer but was much more sensitive

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to disturbances, particularly dampness. Clifton's improvements were significant enough for his design to be taken up by Elliott Bros[13].

[13] These instruments were in regular use in the Cavendish Laboratory around 1900, although J.J.T. himself used an improved type designed by Kelvin. (See Sir J.J. Thomson by Lord Rayleigh, CUP 1942, p.73.)

Miner’s Lamp 1886

A Royal Commission on Accidents in Mines sat from 1879 and Clifton was a member. One of their tasks was the development of a satisfactory miner’s lamp. It is the sad truth that out of thirty-six experimental lamps submitted, Clifton’s was the only one to get no further than the experimental stage – it was a modification of an existing design known as Gray’s lamp in which the flame was shielded by its own exhaust. It was found to be too easily extinguished by tilting. (On view in the Clarendon Laboratory museum.)

Magnetarium 1892

Clifton designed a spherical model of the earth on the surface of which it was possible to reproduce and measure the two components of the earth’s magnetic field at any epoch. This pretty but limitedly useful apparatus never seems to have been taken up[14].

Optical Bench 1906

This cast-iron optical bench had inverted V and plane surfaces. It was taken up by the Cambridge Instrument Company and exhibited at the Physical Society Exhibition of 1906. (An example is preserved in the Clarendon Laboratory.)

Mineralogy 1887

Clifton’s amateur interest in mineralogy was recognized in the naming of one of the two species of <u>cliftonite</u> after him. It was a meteoric iron containing cubic crystals found at Youndagin, Western Australia (9.16).

One of Clifton’s more enlightened contributions in his evidence to the Universities’ Commission of 1877 was his advocacy of a fixed retirement age and a pension scheme, which did not come into being for nearly fifty years. He said “… it is however most undesirable that people should be allowed to

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go on when they are inefficient because it makes it extremely difficult for a department to be raised again to the proper state after it has been some years in abeyance”. In the event, Clifton remained in office until 1915 when he was 79 and it probably [was] the case that his powers such as they were were diminishing. But had he retired at 70, there would have been a strong possibility that he would have been succeeded by E. Rutherford who was languishing in isolation at McGill and had failed to get the chair at Edinburgh in 1901, although his lot had been much improved when soon after he was joined by F. Soddy (see David Wilson (9.17). The Clarendon Laboratory was put under the care of J. Walker and remained untenanted until the First War was over.

What then are we to make of Robert Bellamy Clifton? An able mathematician he certainly was and, although in his time Oxford physics was overloaded with geometrical optics, this was treated in a rigorously mathematical way. As an experimentalist we have seen him tolerably active in improving apparatus for teaching purposes. His single research paper leaves much to be desired but his failure to continue doing research was probably due more to the overcrowding which was forced upon him than lack of curiosity, or attention to his obligations. The truth of the matter is that Clifton was at Cambridge in the years when the mathematics syllabus was at its purest. From the early 1830s Whewell, for decades the great dictator of Cambridge mathematics, eagerly crammed every new discovery in physics into the mathematics syllabus but around 1850, partly due to fright on the part of the mathematicians and later to make physics a sensible subject in the Natural Sciences Tripos (see Chap. [38]), the mathematics syllabus was restored to its earlier purity. Clifton was at Cambridge in 1857-9 and was thus brought up on mathematics at its purest. Apart from this these years were in both universities a time when there seems to have been uncertainty in

[14] Details in the Clarendon Laboratory archives

many minds about the nature of physics – it was clearly not the observational natural science which it had once been but its essentially mathematical basis was not yet generally accepted.

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Chap. 9

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PART III

Chap.10 Townsend I

For its first 36 years, Townsend's Electrical Laboratory was independent. Nevertheless, it and the strange career of J.S.E. Townsend form too much of a part of that of the Clarendon Laboratory to be left out.

John Sealy Townsend was born in Galway in 1868, a son of the civil engineering professor at Queen's College, Galway – paternal civil engineering being one of the few points of similarity between his background and Lindemann's. He began his undergraduate studies at Trinity College, Dublin, as what Oxford would call a commoner but at the end of his first year became a Foundation Scholar in mathematics, and ended up with a "double Senior Moderatorship", coming top in mathematics. After taking his degree at T.C.D., he stayed on for four years teaching mathematics as a Fellowship Prizeman, although it is not clear whether he was able to do experimental work; in any event, nothing was published.

In 1895 the University of Cambridge introduced a scheme whereby graduates of other universities could get a BA after two years advanced study or research. J.J. Thomson was able to offer two such appointments – one to E. Rutherford (1871-1937, FRS 1903, Nobel Prizewinner 1908) from Wellington College, New Zealand, and the other to Townsend who must have been worried about his future in Dublin after four disappointments. Rutherford, curiously, spent two years on the propagation of radio waves, before turning to radio-activity, while Townsend started work on electrified gases and vapours. Two years later he was able to publish (10.1) a remarkable paper in which he gives the charge of the electron as 3 to 5 x 10^{-10} e.s.u. (1.0 to 1.7 x 10^{-19} coulomb), the modern accepted value being 4.80 x 10^{-10} e.s.u. (1.602 x 10^{-19} coulomb). (He uses the words "atomic charge" although the term "electron" had been coined by G. Johnstone Stoney in 1894 (10.2) as a unit – there was no suggestion that it might be a particle until J.J. Thomson's paper (10.3) of 1899). Townsend's method, which was followed by H.A. Wilson in 1903 leading
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to R.A. Millikan in 1917, was to use the mist accompanying the electrified hydrogen or oxygen evolved in the electrolysis of water, measure its rate of fall under gravity photographically and then measure its charge by passing it into a chamber connected to a quadrant electrometer. Townsend was well-versed in hydrodynamics and the propinquity in the Cavendish Laboratory of Stokes himself may have led him to apply the latter's famous theorem, enabling him to determine the charge on each droplet. This was an important experiment in several ways. Firstly, Townsend must have applied very considerable experimental skill – the quadrant electrometer was a capricious instrument even when the environment was dry and this was essentially a wet experiment. Further, he was successful in getting the correct result of an essentially microscopic quantity out of a macroscopic experiment at a time when the quantitative aspect of this distinction was not commonly understood. But most importantly, Townsend's value of e was an essential ingredient for J.J. Thomson's measurements of m, derived as they were from the value for m/e in 1897 and 1899 (10.4). This first conception of the relative scale of the masses of the proton and

the electron opened the door to the beginning of the understanding of the constitution of matter brought about by Rutherford and Bohr.

It is from this time that there comes a story which illustrates his mischievous nature. There was then new interest in what we now call ESP and some scientists of the greatest distinction thought it profitable to consider promising experiments. The topic cropped up in after-dinner conversation at Trinity and an experiment was proposed there and then of the sort in which somebody goes out while the others decide upon an object which they will think hard about on the absentee's return to see whether they can transmit their thought to him. No doubt by inconspicuous nods and winks, Townsend and a like-minded contemporary, J.A. McClelland, were able to arrange that the latter became the volunteer absentee and that the former would communicate the physical shape of the object after his return. It all worked splendidly on two occasions and the elder brethren retired for the night deeply impressed.

To Townsend's horror, two of the most distinguished of the eminent scientists present turned up in his rooms on the following morning with a draft paper which they proposed to submit to Nature. It was not until Townsend called in testimony from his fellow-conspirator that he was able to convince the would-be believers that they had been the victims of a practical joke. The joke was generally held to have been in poor taste but, as will be seen, it did not delay Townsend's FRS. [15]

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Townsend became a fellow of Trinity College in 1899 and he might have been expected to feel that he was accepted in spite of having made the fundamental blunder of not having been brought up a Cambridge man. In-breeding was very much in fashion at Cambridge at the time and a standard had been set – the first two Cavendish professors, J.C. Maxwell and W. Strutt (later Lord Rayleigh) were Senior Wranglers and J.J. Thomson was at least a Second Wrangler. Only someone of the established distinction of Rutherford could have hoped to break the succession in 1919. Twenty years earlier it must have looked to Townsend as though his expectations of getting to the top of the Cambridge tree were limited. But there was another consideration: conflict with J.J. Thomson about priority in discoveries. In his biography of J.J.T (p. 115f), Lord Rayleigh – son and heir of his more famous father – makes no bones about conflict concerning ionization in gases and J.J.T.'s son, G.P. Thomson, in his biography falls in with this view (p.103). But it was common knowledge that there was as much friction about the discovery of the electron – equably resolved though leaving the two combatants with tarnished reputations as concerned generosity to scientific colleagues. Townsend may have thought that the only routes to fame and fortune lay away from Cambridge. Time proved him wrong – his career added greatly to the applied physics subject of electrical discharges in gases and not at all to the fundamental issues of atomic structure.

Townsend's First Fourteen Years in Oxford

We have seen that the need had been recognized at Oxford for a second professorship to provide better teaching in electricity and magnetism for the physics undergraduates and for elementary teaching for the Preliminary examination in Natural Science for non-physicists such as chemists, engineers and medical students. It was therefore agreed with New College that a Wykeham Professor of Physics would be appointed in October 1900 and that they would pay the salary of about £400 p.a. plus £100 p.a. for as long as they could. It was not surprising that Townsend accepted the offer from Oxford, unsatisfactory

[15] See also Sir J.J. Thomson, Lord Rayleigh, CUP 1942

as it must have seemed as to accommodation. He was given what space could be found for him by the then Savilian Professor of Astronomy, H.H. Turner, and was also given the temporary use of two rooms in what is now the
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Old Zoology building. He lectured in the Clarendon Laboratory on electricity and magnetism and in 1903 was asked to set up practical classes for "Prelims". Following the moving of the Radcliffe Science Library into its new building along South Parks Road in 1903, he was able to expand into the first floor of the northern end of the west front of the University Museum. Soon after, he had the joint use with the Hope Professor of Zoology of the corresponding area on the south side, although in the following year we read of difficulties due to draughts through gaps where the bookcases had been. By this time he was under pressure to relinquish the space in the Old Zoology building. Electrical supplies (100-volt DC – see p.[ ]) were brought into the Museum Lecture Room in 1904 (semi-circular in layout before its conversion to the present arrangement in 1962) and Townsend was then able to lecture there. By then he had vacated the rooms used for his workshop in the Old Zoology building and his lathes were re-installed with electrical supplies in a structure abutting on the northern elevation of the University Museum which appears on early maps variously as "Iron House" and – more sinisterly – "Macerating Room" and which was then renamed "The Electrical Laboratory". Such were the quarters with which he had to be content for his first ten years and it is sad that his activities were thus limited at this time in his career – encouraging as it must have been to him to be elected FRS in 1903.

But Townsend had a powerful ally in the shape of the Rev. Henry Boyd (1831-1922), Principal of Hertford College and Master of the Drapers' Company in 1896. Principal Boyd had secured the building of the Radcliffe Science Library opened in 1901, by a donation by the Drapers' Company to the University. He writes to them again on 8 November 1907 (10.5) saying that he would have raised the matter of a second physics building when he was in correspondence about the Radcliffe Science Library. The University, he says, had not then done its part, "… but has since done very nobly …" in establishing a Professorship. The Professor was "… a very distinguished electrician (sic) and external examiner for the University of London …", the work of the department very satisfactory and the pupils increasing and they "… now need a building". Accordingly, Townsend wrote on 23 November 1907 stating his proposal for a building of three stories, including a lecture room
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with no rooms above it, rooms on the ground and first floors for research, a workshop and a room for himself, and undergraduate laboratories on the second floor. This letter was followed four months later by a reply to certain queries from the Drapers' Company in which Townsend speaks of already having "… a workshop well-provided with lathes and motors, and a fair amount of apparatus has been constructed and purchased during the last six years …" and says that as to staff he will require only one "appointment". The total sum requested for all the above was £1,000. He enumerates the existing staff as a Fellow of Queen's (F.B. Pidduck, a Cambridge mathematician of considerable ability – see for example his A Treatise on Electricity, pub. CUP 1916, 2nd ed. 1925), a Fellow of New College (himself) and one "Assistant Registrar". In April 1908 a committee recommended a grant of £22,000 for the building and £1,000 for furniture and equipment. In June 1908 the then Vice-Chancellor, the President of Magdalen College (Herbert Warren) sent a telegram of "profound thanks" to the Drapers' Company, followed in October by one from Townsend saying that the site had been passed.

There were, of course, a number of after-thoughts including the oak seating and panelling in the Lecture Room (£462.2s.) and a table for the Professor (£43.3s.). The Drapers' Company became a little restive and in 1909 they wrote to the architect saying "…. The Company is not willing to be continually incurring additional expense … a pity Professor Townsend could not have thought of it (a matter of floor/landing level) before". But when it came to the detailed arrangements for the Opening in the following year and Principal Boyd had expressed doubts about the University's being able to lay on adequate champagne, they wrote to say that they would provide champagne, brandy and cigars.

Lest contemporary readers run away with the idea that 75 years ago everything went smoothly, we must turn to a top-level row which was meanwhile smouldering within the University. The then Chancellor – none other than the redoubtable Lord Curzon (1859-1925) – was in the process of launching an appeal by which it was hoped to raise £100,000 when W.W. Astor (later Lord Astor, 1886-1971) had promised a second contribution of £10,000. Curzon wrote

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to Dr Boyd suggesting that the £23,000 offered by the Drapers' Company should be counted in with his appeal fund. Boyd was furious and sent a letter to the Drapers with a note saying "Why can he not let me alone?". Lord Curzon then wrote to the Drapers saying that he hoped they would "… graciously be willing to treat me as the medium or agency …" at which Boyd pointed out that no money would go to Oxford and complained of this "… very unsatisfactory letter … behind my back … I know some object to be brought under his scheme …". Happily, no harm was done to the progress of the building and a new and commodious Electrical Laboratory was opened on 21 June 1910.

The architect, T.G. (Anglo) Jackson's[16] design incorporates a more than token gesture towards the Palladian – as Fig. [11] shows, the west frontage embodies a classical element shrunk to about two-thirds of the whole. It must send a shiver down the spine of the purist to find himself going into the building under the stylobate. The four Ionic columns support a conventional pediment. The rest of the building is executed in brick, of a quality such that brick-laying conventions used to come from far and wide to see it. The bricks, known as red rubbers, are of extraordinary smoothness and the joints very narrow. On the south front there are semi-circular brick arches to the window openings and the care with which the tapering shape of the bricks has been achieved is impressive. The window frames are of bronze.

The stone roof tiles are of extraordinary weight and are supported by closely spaced pine rafters. In 75 years the only attention required has been to the lead flashing. Inside, the stairwell is perhaps somewhat overdone in the interests of symmetry and it has been a recurring problem how one might use the redundant space. But the stairs and landings must have been one of the earliest uses of granolithic – they are hardly worn. The size of the first floor landing single in situ casting is looked upon with awe these days. The original lecture room was oriented so that the lecturer faced south. The change to the present east facing arrangement was made in 1959 and doubled the seating capacity. The original tip-up seats were of solid oak and slightly anatomically contoured. Over the original lecture room doors was a

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classical pediment with elaborate carving, somewhat in the Inigo Jones idiom. (Unfortunately, this was not preserved in 1959.) The lecture room ceiling is a plaster-finished semi-cylinder stemming from stone corbels. The same principle is followed in the

[16] Bart, 1913

large, second floor north room but there there are exposed semi-circular cast-iron supporting arches, which have provoked comparison with railway stations.

The most intriguing feature is the heating system. On the south front there is a small basement area, built to contain a coke-burning central heating boiler. A DC motor (about 5HP) drove a fan which blew air past fins on the boiler and then into a plenum behind the boiler from which led cement-lined brick ducts about one square foot each – one leading to all 12 principal rooms. The incoming air to the fan passed down a path in the chimney stack which also had a path for the flue gases of the boiler. Thus, by a crude form of heat-exchanger, the incoming air was pre-heated by the outgoing flue gases – an attempt at fuel economy which would have delighted F.E. Simon (see p.[163]). But this ingenious arrangement was destined never to be used. When the fan was switched on for the first time, it blew all the builders' dust and some of their debris into every room of the building. Professor Townsend reacted sharply, as was his wont, and condemned the ducted-air heating system outright, so a conventional hot water system was installed instead. (Luckily, several rooms including the Professor's were provided with open coal fireplaces.)

When J.J. Thomson started work on electrical discharge in gases in 1871, his experiments were exploratory in nature and led him to the discovery of the electron. Townsend continued this work while he was at Cambridge and brought it to Oxford. In the years before the First War, he was able to establish in a thorough-going way the mathematical treatment of what has become an important branch of applied physics, which is still active – Townsend's methods are still being used in the plasma physics needed for the development of fusion reactors. His early treatment of the build-up of a discharge in a gas embodied several terms covering the different processes. He soon came to see that the first of these which expressed the effect of collisions by positive ions was in most cases negligible – see A. von Engel (10.6) but in later
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years the conviction that he was mistaken led to much wasted research time. Although Townsend fully established the soundness of his mathematical methods, his experimental work was hampered by the primitive techniques of the day as concerned high vacua, gas purity, leak detection etc.

When the First War began, Townsend was aged 46 and, therefore, had no obligation to join the services. Nevertheless, on the outbreak of war in 1914, he closed down his research and volunteered for service as one of the relatively few people with a full understanding of the infant technology of wireless telegraphy and was detailed for an expedition to Russia.

> On hearing this, F.B. Pidduck called at the Townsend home and, finding the Professor out, left a message with Mrs Townsend asking her to get her husband to leave instructions that, should he not return, Pidduck was to be his successor in the Wykeham Chair.

But, in the event, the expedition to Russia did not materialize and Lieutenant Townsend, RNVR, found himself doing research work for the Royal Navy air service. Later he went to the assistance of General Ferrier, who had charge of Marconi's transmitter in the Eiffel Tower. By 1916, an instructional course for the School of Military Aeronautics had been established in the Electrical laboratory. (It was nearly forty years before the two aero-engines left behind were found a more appropriate home.)

At some point in the War not now identifiable, Townsend developed his wavemeter (see Fig. 39). It was still in regular use in the Laboratory some twenty years later.

By the end of the First War, the ranks held by the staff were as follows – the Professor himself having evidently changed services:

Lt. Colonel H.T. Tizard[17]
Major J.S.E. Townsend
Major E.W.B. Gill, Royal Signals
Capt. F.B. Pidduck
Lt. R.T. Lattey, RAF

A few years after the War, a Royal car drew up outside Townsend's Electrical Laboratory and an Equerry got out, telling someone in the front hall that HM wished to see Professor Townsend in his car for a few minutes – doubtless there would be an expression of appreciation for his work in the recent war and perhaps the offer of a knighthood. The Equerry was told that the Professor was busy, so no knighthood until 20 years later.

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[17] The same Tizard as features in Chap. [23]

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PART III

Chap. 11 H.G.J. Moseley in Oxford

Henry Gwyn Jeffreys Moseley (1887-1915) was the only son of H.N. Moseley who became Linacre Professor of Zoology and Comparative Anatomy in 1881. Moseley came up to Trinity College from Eton in 1906 to read physics and was confidently expected to get a first. In the event, he took a second but went gladly to Manchester University where "science is treated as a more serious profession". Here he worked under Rutherford, initially on the beta-activity in RaB.

At this time and for many earlier decades, the understanding of the fundamental chemical behaviour of the elements had been in a state of disorder. Since Mendeleev's proposal of a Periodic Table of the elements in 1869, people had been trying to square quantitative facts of the atomic weights with the undoubted qualitative periodicity of chemical properties, clear cases of this lack of fit being nickel-cobalt, argon-potassium and iodine-tellurium. At the same time, controversy raged about the probable constitution of the atoms themselves, the two contenders being J.J. Thomson's "plum-pudding" atom and the Bohr-Rutherford atom which we all know about today.

By turning to X-ray spectra at a time when optical spectroscopy was not yet generally understood, Moseley had been able in 1913 to show that, if the elements are arranged in order of what we now call atomic number or Z rather than atomic weight or A, they fall into the appropriate places in the Periodic Table – this was the first of his two great discoveries.

Following discussions at Manchester with Bohr in July 1913, he began measuring X-ray spectra in October. A month later he published his first paper on the X-ray spectra of the elements (11.1). It is interesting to note in passing that this work brought him into public controversy with Lindemann.

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Apart from the fundamental difficulties over the Periodic Table, inorganic chemists had been struggling for some time with the rare earth elements and the familiar names of these perpetuate some of these difficulties – eg. ytterbium, yttrium, terbium and erbium, all originally thought to be one element. The X-ray spectra technique which Moseley had developed at Manchester was obviously a perfect tool for elucidating the confusion which the rare earth chemists were in and it was this work which was carried out in Townsend's Electrical Laboratory in Oxford.

By 1913 Moseley was beginning to think that a return to Oxford might be opportune so that he could keep an eye on his chances of getting one of the very few fellowships in physics and it is clear that he had a keen eye on the Chair of Experimental Philosophy. No appointment being available and being able to afford to keep himself, he was able to accept an invitation from Townsend to work in his laboratory – a suggestion made to Townsend by Rutherford. "I will have a much better chance of a research fellowship" he wrote "if I am on the spot clamorous."

Accordingly, with the aid of a grant from the Solvay Institute and the loan of various pieces of apparatus, he set up his X-ray spectrometer with its train of little carriages on rails[18] so that many specimens could be measured without breaking the vacuum. By January 1914 he was sending Rutherford results he had obtained from elements ranging from aluminium-13 to gold-79. These results included a number of wildly improbable errors putting many familiar elements into quite the wrong places in the Periodic Table and demonstrating Moseley's extreme impetuousness and self-confidence. However, by April he had arrived at the correct and definitive results and these were reported in his famous paper (11.3) for which he drew the original diagram relating wavelength to atomic number, now framed and on view in the Clarendon Laboratory.

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Finally, in the summer of 1914 the famous French chemist, Urbain, brought his rare earth specimens to Oxford where, in Heilbron's words, Moseley "… untangled in a few days conundrums that had taken chemists six generations merely to propose".

In June 1914 Moseley left for Australia where the British Association for the Advancement of Science was holding meetings at Adelaide and elsewhere in August. On the declaration of war in August 1914, his only priority was to join up and October found him in the army. It was in Churchill's disastrous Dardanelles campaign that, with others, he was killed. Following the death, effective steps were taken to keep men of real scientific ability out of the firing line and no other leading scientists were lost in the 1914-18 war.

Thus it was that one man working on a new line in physics extending over only two years was able to sort out difficulties which had been dogging chemists for decades and to establish the basis of atomic structure.

This note has drawn heavily on <u>H.G.J. Moseley – The Life and Letters of an English Physicist 1887-1915</u> by J.L. Heilbron, pub. University of California Press, 1974.

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Chap. 11

11.1 Phil Mag <u>26</u> (1913) 1024-1034
11.2 G.W.C. Kaye, Phil.Trans.Roy.Soc.A (1909), <u>209</u>, 123
11.3 Phil.Mag. <u>27</u> (1914) 703-713

[18] An arrangement first used by Kaye ([11.2])

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PART IV – LINDEMANN 1919-1926

Chap. 12 Lindemann's Appointment

An undated draft (12.1) implies that Frederick Alexander Lindemann (1886-1957, FRS 1920) (Fig. 12) – later first Viscount Cherwell PC, CH, wrote to the Registrar of the University of Oxford as follows, sending his letter via H.T. Tizard (see p.[136]):

"Dear Sir,

I learn that the professorship of Natural Philosophy (the latter two words crossed out and the following two words substituted) Experimental Philosophy is vacant and beg to offer my services in that capacity. My experimental work has dealt mainly with the properties of solids at low temperatures, X-ray spectra, and photo-electric photometry. I have also published papers on the kinetic theory of solids, photoelectricity, the structure of the atom, the life of radioactive substances as well as a number of astro-physical problems."

He offers as referees:
Lord Rayleigh, FRS, Cavendish Professor, 1879 to 1884
Sir Ernest Rutherford, FRS
Duc de Broglie
Professor Langevin
Sir Joseph Thomson, FRS, Cavendish Professor
Professor Michelson
Professor Millikan of Chicago

The University keeps electoral boards constituted for all eventualities and it dusted down the following body who on 23 April 1919 duly appointed Lindemann.
The Vice-Chancellor (Rev. H.E.D Blakiston, DD, President of Trinity College)
The Warden of Wadham College, Dr Joseph Wells
Sir Joseph Larmor, FRS, Lucasian Professor and MP for the University of Cambridge
Arthur Schuster, FRS, Professor of Physics at Manchester 1887-1907
R.E. Baynes, Dr Lee's Reader in Physics, Christ Church
W.H. Perkin, FRS, Waynflete Professor of Chemistry
J.S.E. Townsend, FRS, Wykeham Professor of Physics

There must have been many congratulatory letters but this was one of the few that Lindemann kept:

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Corpus Christi College,
Cambridge

"April 1919
Dear Lindemann,

I feel I ought to congratulate you but I don't much want to. The high table was very gloomy when I announced the fact last night, though they asked me to congratulate you. I am afraid you will rather be in the position of a Jesuit father sent to minister to the Iroquois, though the vengeance of the unconverted is more likely to take the form of an attack on your digestion than the cruder persecutions of the American Indians. Anyhow in your efforts to wake Oxford up, you will have the most sincere good wishes of all physicists.

G.P Thomson (12.2)"

When Lindemann was appointed his income would have derived partly from the Fellowship at Wadham College, which went with the Professorship. This entitled him to rooms in college and, as a bachelor, he moved in straight away. He had a gas fire installed in his sitting room and was able to have a private bathroom built with a gas-fired geyser. He seems to have settled down, but soon there was an interesting development. The Universities' Commission of 1877 had as its main concern the boosting of the University's modest financial resources at the expense of the rich colleges. A major means to this end was the transfer of funds to provide for many professorships. Christ Church in particular had been enjoying since 1750 the bequest of Dr Matthew Lee, a former Christ Church man who had prospered as a medical practitioner in Nottingham. His will provided for the erection of an Anatomy School which still stands to the south of Wolsey's dining hall and is now used as part of the accommodation for the Senior Common Room. Dr Lee also provided for Readers in anatomy, in physics and in chemistry believing, with a faith bold at the time, in the importance to would-be physicians and surgeons of a sound basis in physical science. Implementation of the measures proposed by the 1877 Commission was delayed because of the agricultural depression of the 1890s (12.3) and it was not until after the First War that anything happened. One of the effects was that the titles of the three Professorships had "Dr Lee's" added before the word

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Professor and, in the case of Lindemann, this took effect in 1922, ie. three years after his appointment. Christ Church, in its usual open-handed way, wanted to make Lindemann a fellow or, as it calls them under the statute of King Henry VIII, a Student, and it was agreed that uniquely and for his lifetime alone he could simultaneously be Fellow of Wadham College and Student of Christ Church, though without a seat on the latter's Governing Body. In due course, Lindemann received an envelope containing much useful information about Christ Church. Because no-one thought of <u>not</u> including it, the envelope contained the usual leaflet about entitlement to rooms in college. Lindemann leapt at this – cosy as life in Wadham may have been, the grandeur offered by some parts of Christ Church would be much more to his taste. He quickly accepted (12.4) and Christ Church, who were indeed anxious to be particularly open-handed to this famous and already distinguished scientist, did not reveal the mistake. Lindemann was able to move into a set of four rooms plus bathroom at the eastern end of the first floor of Meadow Buildings. Its wide bay window commands the most breath-taking view in either of the two ancient universities. To the east and south there are the [ ] acres of Christ Church Meadow and to the north and the east, elevations of Christ Church, the south elevations of Corpus and Merton, with Magdalen Tower in the distance. Lindemann's talents did not extend to interior decoration and furnishing. His rooms, whether college or laboratory, were always spartan to the point of ugliness. When his friends teased him on this point, he turned to the view saying "I don't' have to bother".

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Chap. 12

12.1 CANC A.20

12.2 Later Sir George Thomson FRS, Chairman Maud Committee, Professor Imperial College, London 1930, Master of Corpus Christi College, Cambridge, son of J.J. Thomson. Author of Cherwell obituary in <u>Biographical Memoirs of Fellows of the Royal Society</u> (1958), <u>4</u>, 45.

12.3 A.J. Engel, <u>op.cit</u>.

12.4 MS communication, 1956, from a Student of Christ Church in office in 1922

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PART IV

Chap. 13 Lindemann's Background

The doctors have a way of telling us to take great care when it comes to choosing our parents. Lindemann could not have done better in his father. Adolphus Frederick Lindemann (1846-1931) was an engineer by profession who could bring a keenly scientific turn of mind to bear on his work. His amateur interests were carried to professional standards – there is an asteroid which bears his name.

A.F. Lindemann came from the minor nobility of Alsace-Lorraine and studied physics and astronomy at Nuremberg and instrument-making at Ertel's in Munich (13.1). He had intended going into exploration but following a bout of typhoid fever he emigrated to England and joined Siemens Bros at Woolwich.

William Siemens (1823-1883 (13.2), knighted in 1883), came to England at the age of 21, leaving three of his brothers to develop their famous electrical engineering business in Germany. Very soon after settling in England he had sold the rights of an electro-plating invention to Elkington Bros, the long-established manufacturers of silver plated cutlery. William Siemens soon became involved in the emerging submarine telegraph cable industry. His ability to inject physics-based methods of measurement and control enabled him to propose superior procedures to those currently used by the floundering specialists of the time, Newall and Gordon. This led to the building of a cable works at Woolwich in 1863 when Siemens were laying, though with difficulty, a cable between Spain and North Africa.

It was into this well-established business, but with not every practical technique conquered, that Lindemann pere came in 1866 at the age of 20. Before long he was running the cable works and certainly eight years later was responsible for making and laying submarine cable in the Atlantic.

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The heroic early days of Transatlantic telegraphy are familiar enough – the first cable of 1858 lasted for a matter of weeks and the two successors of 1866 worked well for a few months and, although they were damaged by icebergs (13.3), were successfully repaired to survive until 1872 and 1877. In 1873 a rival company was formed, the Direct United States Cable Co. and their first move was to approach Siemens Bros.

The dielectric used for submarine cables for nearly a century was gutta-percha (from Malay, <u>geta</u> meaning juice and the name of a tree similar to the rubber tree). Rubber had been tried but was found to deteriorate rapidly in sea-water. Gutta-percha had come into use in about 1845 for a variety of purposes. Where it cannot dry out it is very long-lasting and the Atlantic cables which still function in their way after more than 100 years (13.4) bear this out. Further, it is still in use by dental surgeons in root-canal treatment. But where it can dry out and crumble it was disastrous, as in the early attempts at the edge-binding of books around 1860.

Siemens Bros were, therefore, sensible in deciding to build their own gutta-percha factory around 1870 so as to keep the vital business of controlling the quality of a variable natural product in their own hands. By the meticulous attention paid to the physical and engineering details of this infant technology, Siemens Bros were able to turn the art of making and laying submarine cables from near-certain failure to unbelievable success. The individual part played in this transformation by A.F. Lindemann is now lost sight of but it is known that he was closely involved with making and laying the first long-lived transatlantic cable of 1874 and a major share in the credit for it must have been his.

A.F. Lindemann moved to other engineering fields, notably the building of water supply plant – and not merely the aqueous side but the complete financing, civil engineering and landscaping. In 1884, at the age of 38, he married a Mrs Olga Davison, an American-born widow five years his junior with three children. The family moved into a Regency house in Sidmouth (Fig. 13). There were four children of this second marriage, Charles, who became a brigadier in the British Army, Frederick Alexander and another brother and a sister who were to drop out of F.A.'s life.

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A.F. Lindemann built an observatory with a laboratory and workshop in the garden of Sidholme. Charles and F.A. received from an early age the best training possible in the practical skills required of intending scientists – the worst form of punishment in their young days was exclusion from the laboratory.

Many of F.A.'s qualities can therefore be seen to have been derived from his father whether by inheritance or environment. His mother was somewhat larger than life as instanced by her lining her siting room in Sidholme with peacocks' feathers. Her every-day treatment of at least F.A. was bizarre. Not only did she call him "Peach" in the home but allowed the fact to escape. (It is to F.A.'s credit that he signs a letter to his father thus dated as late as 1927 (13.5). An impossible standard of punctuality was exacted at meal-times – even at breakfast (13.6). The most telling anecdote illustrating the tension between mother and son is also quoted by Birkenhead. F.A. had telephoned his parents from Sidmouth railway station only to be told by his mother that, having not given more notice of his visit, he could not be accommodated at Sidholme. F.A.'s not untypical reaction was not to claim hospitality for two years – a sore deprivation when it came to scientific contact with his father.

A lifelong embarrassment to F.A. was his mother's mistake about the due date for his, her fifth child's, birth. It would have suited all concerned for this to have taken place as planned at Sidmouth but F.A. was in fact born at Baden-Baden where the family were on holiday. Just after the Second War it was thought that there might be trouble from the Americans if, on his passport, the actual place of birth were allied to the positively un-English name. The birthplace London, England, was substituted, with official sanction in his 1945 passport which survives (13.7).

When it came to education, the Lindemann parents again showed their tendency towards unconventionality – private tutors up to the age of thirteen, three years at a little-known school in Scotland, followed by the Real-Gymnasium and then the Hochschule in Darmstadt (near Frankfurt). The decisive acceptance into Nernst's laboratory in Berlin may have followed

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F.A. Lindemann's academic success at Darmstadt but possibly the acquaintance through the Siemens family may have played a part.

It was about this time that F.A. Lindemann, with his brother, made a substantial contribution to the performance of the X-ray tubes of the time by increasing the transparency of the glass envelope, when he proposed and proved the superiority of glasses in which light elements such as beryllium, boron and lithium were substituted for the usual soda and silica (13.8). Already a skilled glassblower, Lindemann was able to establish himself by taking personal responsibility for part of the process in which he included a secret ingredient. The usefulness was short-lived but the feat remains impressive, that two students should have understood the physics of this new invention and should have been able to master the highly-specialized techniques of glass-making.

According to a short note (13.9) dated 1 March 1914 signed by Nernst, Lindemann worked in his laboratory "with short interruptions" from 1908. He spent the first two years working for the Ph.D. – by no means a walkover when it came to the customary questions on other branches of physics. His work in Nernst's laboratory was published in seven papers (13.10) covering atomic heats at temperatures down to 20K. These were pioneering experiments, sometimes involving very small quantities of heat, and the use of high vacuum was itself an ambitious technique at the time. The results, many of which showed inexplicable departures from Dulong and Petit's law, were seen as being related to Planck's discovery (1898) of the quantum and Nernst and his collaborators set out to reconcile these unexpected results with the new theory.

The first attempt at a theory was by Einstein (13.11). Lindemann followed (13.12) with a slightly more sophisticated theory which also did not match the experimental data in certain respects. It was, of course, Debye in 1912 (13.13) who proposed the theory which fits the data closely.

These experiments, coming as they did early in the fitting of macroscopic physical quantities into the new quantum physics, were Lindemann's chief contribution and show him at his typical best – a skilful designer and executer of difficult experiments who was also bold enough to tackle
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pioneering theoretical interpretations, even though in this case he was not lucky enough to penetrate to the final rationale. The power of specific heat measurement as an experimental tool in solid-state physics is illustrated by the fact that R.W. Hill, a one-time pupil of F.E. Simon, is at the time of writing measuring specific heats of ultra-pure rare earth specimens down to 0.5K.

It was also during his time in Nernst's laboratory that Lindemann published his melting-point formula (13.14). This relates the atomic frequency with the temperature at the melting point. The central assumption is that melting occurs when the vibrations of the atoms causes them to touch. This is a fine example of a crude insight proving entirely realistic – a spread of frequency of a factor of 20 leads to a value for the melting point which is correct to not more than 15%.

At about this time Lindemann was asked to be joint Secretary of the 1911 Solvay Conference with L. de Broglie who was famous for his introduction of the duality concept of waves and particles. This conference brought together a galaxy of legendary names from A. Einstein and Planck onwards and no apology is needed for yet again reproducing the famous group photograph (Fig. 14). His central place in this assembly as a young man of only 25 must have contributed to his confident manner in later years.

In 1913 Lindemann was invited by R.A. Millikan to lecture in the University of Chicago on a topic of his own choice and duly gave a course in kinetic theory in June and July 1913.

According to a letter to his father dated November 1913, quoted by Birkenhead (13.15), Lindemann was expecting that Clifton would retire in September 1914 and he wound up his spell in Nernst's laboratory at the end of February 1914 (see Nernst's note quoted above). No doubt he dedicated the summer to lawn tennis – we certainly hear of his

winning a tournament at Zoppol (midway between Gdynia and Gdansk). He must have returned to England via Berlin because he picked up there his Rumkorff coil – still in regular use in lecture demonstrations – and a quantity of his own stock of precious metals etc. He travelled, it is said, on the last train to leave Germany and J.C. Masterman, who was to be his colleague at Christ Church, would have been on the next train
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but unluckily was interned for the four years of the First War.[19]

A.C.G. Egerton (1886-1959, FRS 1926, see p.[82]) had met Lindemann in Nernst's laboratory in 1913 and had been at the lawn tennis tournament in Zoppol in 1914. Together they applied to the War Office, stating their abilities and Egerton was promptly drafted into the work which made him famous, the control of "knock" in internal combustion engines. Much more in the First War than in the Second there was an hysterical distrust of foreign – and particularly German-sounding – names. The Royal house of Battenberg had to become Mountbatten, it will be remembered. A Lindemann born in Sidmouth might have been found something to do but one born in Baden-Baden …

Meanwhile, a powerful group of scientists of all kinds was being assembled by Colonel Mervyn O'Gorman. This organization was set up at Farnborough in premises built around 1905 and then known as the Royal Aircraft Factory but soon to be given its present name, Royal Aircraft Establishment. In 1955 RAE Farnborough celebrated its jubilee and Lord Cherwell was asked to speak about his time there. The following text is reprinted here by kind permission of the Director.

> "Reminiscences of Farnborough" by Lord Cherwell of Oxford, PC, CH, FRS Dr Lee's Professor of Experimental Philosophy, Oxford.
>
> It is some forty years ago now that Colonel O'Gorman invited me to join the Royal Aircraft Factory (the word Establishment was substituted later to avoid conflict of initials when the RAF was formed). Like other scientists with no special knowledge of aircraft, I was posted to H Department, a branch to which all sorts of odd questions were referred which did not seem to fit into any special field. What to work on was left very much to our own choice and my investigations ranged from detecting aircraft by sound, explaining why some cylinders with cast-iron fins cooled better than others, designing and making rate-of-climb meters, sun-proof doping for wings, compasses, automatic pilots, turn indicators, bombsights, accelerometers, range-finders for the Navy, a contraption to brush aside balloon cables, to occasional excursions into aerodynamics such as the cause of spinning.
>
> In those times experiments were much less carefully controlled than today. Flying was considered more an art than a science and the professional pilots were at no pains to dispel this idea and often exhibited all the allures of the prima donna or of the then fortunately non-existent temperamental film star. It was only when the first Air Board was formed that some of us at last obtained permission to learn to fly thanks, I like to think, to a somewhat journalistically phrased application concocted by myself. With George Thomson, William Farren and Keith Lucas, I went to the Central Flying School in 1916 – the last named unhappily was killed there.
>
> [Page 73]
>
> It would take far too long to describe all my various activities at Farnborough – the recording glass fibre accelerometer with which I fortunately obtained records of all known stunts before it was handed over to someone else whose zeal for perfection kept it out of use until the end of the war, the rate-of-climb meter which, since it

[19] Private communication from C.H. Collie, CLTA

included a miniature home-made vacuum flask to keep the temperature of the air constant, was persistently called the coffee-ometer by the professional pilots, and a host of other items of more or less importance.

In 1916 many pilots were killed flying our recently designed R.E.8s by spinning into the ground. Although various people had succeeded in getting out of a spin, nobody quite knew how nor indeed how or why aircraft spun at all. Anyone watching a spinning plane could see that the rate of turn did not increase on the way down. I concluded therefore that the lift on both wings must be equal and this could only be true – since the outer wing is beating against the air whereas the inner is not – if its effective angle of incidence was on the high angle side of the angle of maximum lift, whereas for the inner wing it was the opposite way round. This being so, if the speed were increased the aeroplane would no longer spin. Experiments proved that this idea was correct and the whole theory was worked out quantitatively and described in a paper by Glauert and myself published by the Aeronautical Research Committee in 1918. Thereafter the pilots were taught to push the stick forward – the very opposite of the instinctive reaction of pulling it back in order to get the nose up – and to straighten out the rudder and then pull out of the dive in the ordinary way. The only merit I can claim in carrying out these experiments is that (unlike the professional pilot, who had usually not got a very good head for figures) I was able to remember the readings of the airspeed indicator, the bubble, the angle of incidence on the two wings (measured by tapes on the struts), the height of the beginning and ending of the spin, the time taken and the number of turns, and to write them down in my notebook when I had straightened out the plane again. I am glad of this opportunity to correct some of the absurdly dramatic stories which have appeared about this investigation.

In this period Dr Dobson and I did some work on what was probably the first automatic pilot*, a gyroscopic device which operated small compressed air cylinders wired up to the rudder. Although there was a safety plug to free the gyro and another to disconnect the air pressure, my real faith was placed on a wire cutter I carried in my pocket with which I could cut the cables leading to the rudder, if all else failed. We were agreeably surprised to find that the very first of these devices flew the plane about twice as accurately as a good pilot.

I think we were also probably the first to design and test stabilized bombsights. The lateral error without such a device was, of course, gigantic for, if the sighting wire indicated that one was aiming, say, to the right of the target and one turned left, the bank of the aeroplane made it look as if one were deviating to the right, hence one had to flatten out carefully and then look again before correcting – there was seldom much time for this.

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By fixing in front of the pilot two mirrors reflecting the horizon on the left and right, it was easy to see when the aircraft was exactly horizontal. In this position a free gyro carrying a light bombsight was released and it was then possible to fly on exactly the right line, keeping the sighting wire on the target. With this device I was able from 10,000 feet to obtain an average accuracy of 60 yards – admittedly flying up wind in a relatively slow machine without being hampered by ack-ack fire. Unfortunately, stabilized sights were not developed, and we started the 1939 war with fixed bombsights with which accuracy was impossible.

Bombing experiments in the early days were carried out at Orford, a sort of sub-station of the RAE. I well remember how early one morning I dropped my stop-watch, required for calculating one's ground speed and, despite all my efforts to reach it, saw it disappear through the floor past the joy-stick. (The repercussions, incidentally, since it was laid down that a War Department watch could not be lost, though it could be destroyed experimentally, lasted a long time.) In the meanwhile, my colleague, stationed on the ground to plot the fall of the bombs, concluded that I had abandoned the test and went for a swim. As I had to jettison the bombs before landing and they unhappily straddled him in the sea, he took the whole performance somewhat amiss although they were only twenty-pounders. Nowadays, no doubt, the authorities insist on more elaborate precautions.

I can recall no serious differences in our little Mess of "pseudo-scientists and slide-rule pushers" as we were described in the "Aeroplane". We had some sad occasions such as the tragic accident which cut short the promising career of Pinsent. But on the whole we were a very cheerful group, indeed, I dare say our secretary, Farren, still recalls my flying over to Cambridge and dropping a boot attached to a small parachute over the church when he emerged from his wedding service.

These happy-go-lucky days are, I fear, over but the freedom granted to scientists by the ever-helpful O'Gorman was an example of how to get the best out of people, a lesson which in these days of highly organized research it is still wise to keep in mind."

One of Lindemann's colleagues at Farnborough was F.W. Aston (13.16), the father of mass spectroscopy. While still at Farnborough they published a joint paper about the prospects of separating isotopes (13.17). They distinguished far-sightedly between the feasibility of the methods known at the time, according to the scale required. This paper was important enough for there to have been anxious deliberation in the Tube Alloys group in the Clarendon Laboratory some twenty-two years later about whether to keep volume 37 of Phil.Mag. under lock and key. It was decided that, if the fact leaked out, it might draw undesirable attention to the contents.

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PART IV

Chap. 14 Lindemann in Oxford – the First Seven Years

Cranking up an academic machine which has been running down for years and then suffered a four-year interregnum would have been a Herculean task for anybody but still worse for Lindemann, who had not been through an English university. He was fortunate in having met at Farnborough Idris Owain Griffith (1878-1941), an Oxford mathematician with strong practical gifts, coupled with an interest in physics. He had been in Clifton's laboratory since 1903, where he spent some time doing research. In 1919 he and Lindemann were between them covering Heat and General Physics and running three six-hour practical sessions per week. Townsend was lecturing three times per week and providing four sessions of two-hour practicals. His colleagues in the Electrical Laboratory were also looking after the teaching for "Prelims" (see p.[53]) and were helping out in teaching for the Department of Engineering. To this somewhat threadbare lecturing strength Lindemann was able to add, in Trinity Term 1920, the spectroscopist, T.R. Merton (see Chap. 17). Fortunately, the mathematics tutor at Brasenose College, C.H. Sampson became Principal in 1920 and I.O. Griffith succeeded him as Tutor in Mathematics. He was still able to support Lindemann and until his death in 1941 was a valuable representative on the General Board (from 1933) and on Hebdomadal Council (from 1938)[20].

If the cover for undergraduate teaching was a little thin, the research programme at the end of the first term – ie. December 1919 – is astonishing. In his report to the Delegates of the University Museum, Lindemann can muster the following:

The Professor and Mr T.C. Keeley (a Cambridge graduate brought from Farnborough and supported by the DSIR[21]) – atomic stability and infra-red reflection-coefficient of electrolytes.

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Mr I.O. Griffith – the production and effects of high temperatures.

Mr J.H. Mackie (Senior Hulme Scholar, Brasenose College) – radiative equilibria.

Mr A.C.G. Egerton (see Thermodynamics p.[82] below) – vapour pressure of metals (Funds for equipment provided by DSIR.)

Mr S. Harcombe (Jesus College, research student for B.Sc, and advance guard of T.R. Merton – see below p.[91] under Spectroscopy) – Stark effect in absorption spectra.

Mr I.G. Evans (Jesus College, research student for B.Sc, supported by DSIR) – properties of the alkali metals at low temperatures.

Mr H.H.L.A. Brose (Christ Church, research student for B.Sc) – reflection coefficient of metals at low temperatures.

An intriguing bout of poaching on his neighbour Townsend's territory was Lindemann's acquisition of a Rhodes Scholar named R.B. Brode, who had published a paper (14.1.a) from Cal.Tech. under R.A. Millikan about the cross-section for capture of slow electrons of a variety of gases at low pressure, obtaining results comparable with those

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[20] "He was a sort of University Pooh-Bah", A.H. Cooke 1984

[21] Department of Scientific and Industrial Research, forerunner of today's SERC

of Ramsauer (14.1.b) (see p [246] Appendix C). In Oxford Brode applied the same hot electron method to zinc, cadmium and mercury and his results are published in two papers (14.1.c). No peaks were found in any of the three substances between 0 and 50V. Brode left Oxford without taking a BSc – the D.Phil. had not yet been established – and went to Göttingen where no doubt he joined Franck, famous for his collaboration with Hertz. After a short spell at Princeton he became Professor of Physics at MIT in 1931. In the Second War he worked at Los Alamos, and in his time has been a Guggenheim and Fulbright Fellow.

Lindemann had also been active in re-equipping and servicing the Clarendon Laboratory. For one thing, when he took over there was no mains electrical supply, although a 100 volt DC supply had been installed in Townsend's Electrical Laboratory a few yards away. Lindemann was quick to have it installed and to do away with the gas engine and Wilde dynamo. (Three
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years later the science departments were offered 230 volt AC on very reasonable terms but the majority of departments turned it down because AC was no good for charging batteries. A rotary converter was installed by the Oxford Electric Light Company in a brick hut put up by the University on the site of the southern wing of the Department of Pharmacology and the DC supply system kept going until the early 1950s.) A liquid air machine was presented by Townsend and four liquid air vessels were lent by the Air Ministry. The University made grants to remedy the shortcomings in scientific workshop equipment.

Lindemann was scrupulous in the matter of only including his own name on papers describing research if he had participated in the experiments. The papers covering his first seven years of experimental work in Oxford fall into two groups – those arising from his observations with G.M.B. Dobson on meteors (dealt with on p.[85] under Atmospheric Physics) and those arising from his astronomical partnership with his father in Sidmouth. Lindemann father and son had run into a fundamental difficulty in detecting faint stars. It is common knowledge in photographic work that long exposures tend to be less productive of adequate images than might be expected – a phenomenon known as reciprocity failure. The limit had been reached when stars were not producing adequate images even after a whole night's exposure. F.A. Lindemann, therefore, interested himself in the optimum performance of photo-emissive cells and he and his father undertook a considerable research project on them comparing the alkali metals, including caesium which was not easily obtainable at the time, and certainly not in a pure state. This work they published in great detail (14.1) – detail which they may have regretted some years later when Lindemann's photo-cells became a standard component in the new "talking" cinema projectors.

A photo-cell produces a charge and at the time there was no ideal method of measuring it. The Dolazelek quadrant electrometer was a difficult instrument to handle in perfect laboratory conditions and impossible on a moving telescope in an observatory tower. Further, since the quantity to be measured is inevitably a voltage, it is important to keep the capacitance of the electrometer as low as possible. Lindemann, in collaboration with T.C.
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Keeley, developed the quartz-fibre electrometer which was manufactured by the Cambridge Instrument Company (see Fig. 15). The electrometer works on the same principle as the quadrant electrometer but, in place of the moving vane suspended from a torsion head, there is a needle mounted at the centre of a gold-plated quartz fibre fixed at both ends. The breath-taking feature of this design is its dimensions – the quartz fibre is only 1.4 cm long and its diameter is 4.6 micro-metres. The ends of the quartz fibre are secured on a U-shaped

framework also made of quartz so as to minimize disturbances due to temperature fluctuations. When the electrometer is used on a microscope stage the sensitivity can be about 400 eye-piece divisions per volt, while the capacitance is 1pF (2cm). This beautiful invention was unhappily doomed to only a few years' supremacy – it was, of course, superseded by the electrometer version of the thermionic triode valve[22]. These were used by Rutherford in his work on radioactivity and, although Lindemann used them as well, having been initiated in the art by G.M.B. Dobson (see p [85]) he kept quiet about it because he was sensitive to any charge of aping Rutherford (14.2). (There had been a coolness between the two since Rutherford took exception to an innocent comment on one of Rutherford's experiments in a letter from Lindemann (14.3).)

The photo-cells produced at Sidmouth and described in the paper quoted above began to be produced in the Clarendon Laboratory by E. Bolton-King and T.C. Keeley. As word got round about their superior performance, reliability and reproducibility, more and more laboratories around the UK started wanting them. In view of mutterings about commercial activities being carried on in University premises, Lindemann bought the lease of 7 Keble Road and set up the first Oxford Instrument Company, which was soon selling photo-cells by the hundred to the new "talking" cinema industry. The enterprise was still active in 1939 when it closed down because of the Second War (14.4).

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Chap. 14

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[22] C.H. Collie found that the performance of the Lindemann electrometer proved less good than expected due to electrostatic image forces.

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PART IV

Chap. 15 Thermodynamics 1 1921-1936

Alfred Charles Glyn Egerton (1886-1959, FRS 1926 – Sir Alfred Egerton 1943) (Fig. 16) but universally known as "Jack" came from the landed gentry, being descended from a court official of Elizabeth I and Lord Chancellor in the time of James I. (He was related to the Dukes of Bridgewater, the 1st being the canal pioneer and the 2nd the instigator of the Bridgewater Treatises.) In spite of mild opposition from his baffled parents, his precocious scientific bent won through with the support of an aunt, Lucy Stanhope, who happened to be a friend of Lord Rayleigh (Cavendish Professor from 1879 to 1884). At 17, while Egerton was at Eton, Rayleigh showed him round the Cavendish laboratory and was lending him contemporary scientific books. On Rayleigh's advice, he went to University College, London where he read chemistry under Professor Sir William Ramsay (1852-1916), the first to discover terrestrial helium in 1895 – several astronomers having discovered it in the spectrum of the sun in 1868. Egerton became a friend of Ramsay's son, Willie, and used to go on holidays with the family. He took a first in chemistry in 1908 and was clearly destined for a highly successful career. It is, therefore, surprising to read that, until 1913, he worked at the Royal Military Academy, Woolwich. However, he was able – having carefully prepared the ground – to carry out experimental work in physical chemistry which resulted in three papers. In 1913 he accepted an offer to work in Nernst's laboratory in Berlin where he met Lindemann and the two naturally found much in common. When war was imminent in 1914, Egerton and his wife – they married in 1912 – were hustled back with a day to spare thanks to Nernst's efforts. It was Egerton's intention to join the Coldstream Guards but – with wisdom unluckily not deployed in the case of Moseley (see p.[61]) – he was directed into the Department of Explosives Supply of the Ministry of Munitions. Egerton soon displayed a talent as a committee member
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and rapidly achieved prominence. Immediately after the end of the First War, Egerton went on a mission to the German chemical industry led by Sir Harold Hartley, one of the most celebrated Oxford chemists, where Egerton's speciality was the investigation of the Haber process for the synthesis of ammonia.

At Lindemann's invitation, Egerton came to the Clarendon Laboratory in 1919. In 1921 he filled the Readership in Thermodynamics left vacant by Tizard's departure for London (see p.[138]). In these early years he continued his work on the vapour pressure of metals, important in determining the disputed values of their chemical constants, and published 7 papers in 1923. He also returned to the subject of combustion which proved to be the dominant topic throughout his subsequent career. In the '20s he was active in the problem of "knock" in internal combustion engines and pioneered the use of organic-metal compounds such as the familiar tetra-ethyl lead used today. He was interested in the late '20s in low-temperature techniques and also developed an optical pyrometer. His quarters were in the basement of the old Clarendon Laboratory where space and fresh air were at a premium but he seems to have thrived on such privations, saying that he was glad of relative constancy of temperature.

They say that in his lectures on thermodynamics, Egerton laid great stress on the importance to any good physicist of having Maxwell's Relations firmly committed to memory. He would then take a card from his pocket from which he would copy the four vital equations on the blackboard.

It was Egerton's ambition that the University should build a special laboratory in which thermodynamics would be studied by physicists, chemists and engineers. In a paper to the Council in 1934 he makes the far-sighted prediction that there would be a change from the compartmentalized physics of those days to a scheme in which boundaries were defined more by fundamental principles. Although the criteria of such definitions have changed, his prophecy has to a large extent held good.

In 1936 Egerton was offered the Chair of Chemical Technology at Imperial College. This threw him into an agonizing dilemma. In the end he came to see that, however less agreeable the surroundings, there was much to be said for being one's own boss and for living in London. Within a short time he had
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reorganized his new department into a teaching and research establishment in the new subject of chemical engineering. His main achievement in the latter part of his scientific career was to pioneer the use of methane – now familiar as L.N.G. (liquefied natural gas) – but up to his retirement in 1953 and afterwards he remained a much valued committee chairman or member and had the same public-spirited concern for the wise use of energy as had F.E. Simon (see p.[163]).

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PART V – NEW RESEARCH TOPICS 1926 AND ON

Chap. 16 Atmospheric Physics

Lindemann first met G.M.B. Dobson (1889-1976, FRS 1927) (Fig. 17) at RAE, Farnborough, where the latter had become Director of the Experimental Department (16.1). The son of a Lakeland medical practitioner, he had entered Gonville and Caius College as Pensioner and was later promoted to Exhibitioner. In 1910 he was placed in Class 1 of the Natural Sciences Tripos. He was offered a job at the Kew Observatory, partly on the strength of a paper in Nature (16.2) about movements of water in Lake Windermere. His work at Kew was concerned with atmospheric electricity and thus started his long career in atmospheric physics.

Within a year of his appointment Lindemann, possibly with the support of Tizard, was able to get the University to establish a lectureship in meteorology and Dobson was installed in 1920. Before long atmospheric physics was incorporated in the undergraduate syllabus – far in advance of any other university.

Until Lindemann made his famous observations on meteors, it had been thought that the temperature of the atmosphere fell continuously with altitude, but these experiments (16.3) showed that at 50 kilometres – now called the mesopause – there is a very considerable warming. The existence of ozone in the atmosphere and its absorption of solar UV radiation had been known since 1912 from the work of Fabry and Buisson (16.4) but it was not until Lindemann and Dobson (16.5) related their results to this work that the idea emerged that the heating was due to the UV absorption by ozone. Dobson set out on his study of ozone which was to last him for more than fifty years. The problems which he set himself were considerable – the design and development of a UV photospectrograph (16.6) – but by 1924 the prototype was working, and by 1926 a further five had been built and calibrated and installed in Ireland, Shetland, Sweden, Germany and Switzerland. After two years of measurements, four of

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these instruments were more widely dispersed so that by 1930 quite full data had been accumulated on diurnal, latitudinal and seasonal variations in ozone and its variation with pressure systems.

The photographic plates were all processed in Oxford and the business of handling these prompted the development of the photo-electric alternative as soon as this became available. Some six of these were built by R. and J. Beck Ltd in the later 1930s and many more for the International Geophysical Year of 1956, when photomultipliers could be used.

Dobson had thought it important to strengthen his group by the addition of a member with experience of forecasting and A.W. Brewer came as lecturer in 1952. He contributed to the work on ozone by his own electrochemical in situ radio sonde measurements. These, with Dobson's measurements, became valuable in the development of models of atmospheric circulation.

The ozone story could have reached its end in the early 1960s were it not for the worries about the long-term consequences of any weakening of the UV screening property of the ozone layer – originally by supersonic aircraft but more recently by fluorinated hydro-carbons such as those used as propellants for aerosols.

During the Second War, Dobson and some of his associates moved to instrumentation problems, a major one of which was the measurement of humidity at very high altitudes. This called for frost-point measurements down to -60°C. Dobson's direct-observation frost-point hygrometer illustrates his genius for elegant design. A black surface is cooled by a hand pump working on liquid air or solid CO_2-cooled organic fluid and the temperature measured electrically at which small ice crystals are observed neither to grow nor shrink. A method was required of illuminating these crystals very evenly with the minimum introduction of heat. Dobson's idea was to use a glass ellipse with the lamp at one focus and the ice crystals at the other – a deeply satisfying solution. This led to an important discovery soon after the War by B.M. Cwilong, who found that ice crystals do not form directly from water vapour until the temperature has fallen to less than -35°C. A.W. Brewer also developed a system for radio sonde use.

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Professor Dobson (since 1947) retired in 1956 to continue working in his private laboratory at Boars Hill and subsequently at Shotover for another twenty years. He was succeeded as Reader by A.W. Brewer who continued developing high-altitude instrumentation until 1962 when he moved to Toronto.

One of his projects there carried out with A.W. Wilson, was to measure the intensity of solar UV radiation in the wavelength region important to producing ozone. It is interesting to note that their results (16.7), discounted at the time because of disagreement with expensive but less well designed experiments are now being accepted as correct.

In 1951 J.T. Houghton, who had graduated from Jesus College, had joined the group and started the study of the information about the atmosphere which can be won by infra-red techniques. He and A.W. Brewer developed a relatively unsophisticated air-borne bolometer (16.8) and in 1956 (10.9) were measuring at up to 40,000 feet the total integrated heat radiation from below, above and in cloud. There followed two papers on the emissivity of the earth's surface. Then in 1962 A.W. Brewer moved to Toronto and Houghton found himself in the driving seat.

Since the Russians had put up a satellite into orbit with a dog on board in 1957, many atmospheric physicists were itching to get instruments into space able to take a good look at our atmosphere from the best possible place – outside. These thoughts first appeared in print in a paper by L.D. Kaplan (16.10) who put forward the 15 micro-metre CO_2 absorption band as being the most propitious and which has proved popular. In 1964 (16.11) J.T. Houghton set out in full the physical basis of the remarkable procedure by which he was able in the following decade to measure by infra-red spectrometry the vertical distribution of temperature of the atmosphere over the whole earth. The achievement of this ambitious programme by a small group in a British university is impressive not only for the physics involved but for the clear identification of the separate practical ingredients necessary for success and the manner in which they were marshalled.

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First, there is no substitute for getting to grips with the physics and letting it become known that you have done so. This was done in <u>Infra-Red Physics</u> by J.T. Houghton and S.D. Smith of Reading University (16.12), his collaborator in the first years of the satellite programme. After a study of the spectroscopic methods available, Houghton designed a system which incorporated standard up-to-date infra-red techniques carefully adapted to the requirements of use on a satellite. A spectrometer must embody a dispersive element and this may be on a space-division basis such as a prism, grating, interferometer, etc. Alternatively, time-division may be used and this can be done by using a number of interference filters, in particular, the lead-tellurium filters developed at the time in consultation with the USAF by Grubb-Parsons Ltd. The other major feature of an infra-red spectrometer – and particularly so where the detector cannot operate at low temperature – is the ability to exclude all but the range of wavelengths to be measured and to compensate out in the detector electronics as much as possible of the unwanted signal even from that limited range. The signal "window" around 15 micro-metres is provided by a wide-band filter. The compensation is done by dividing the signal into two paths and switching by a vibrating mirror (at about 240Hz) between the two, one of which passes through a cell filled with CO_2 at about atmospheric pressure and the other an identical but empty cell. The signal from the detector can, after processing, indicate only the difference at each selected wavelength between the radiation from the atmosphere as seen through the CO_2-filled cell and through the empty cell. That is, with and without CO_2 absorption bands. A signal from space is also received to provide a zero reference.

Secondly, there was the problem of deriving the vertical distribution of temperature in the atmosphere from the outputs of the six radiometers on the satellites. This was solved by C.D. Rodgers, who developed sophisticated statistical methods for the purpose. He and E.J. Williamson were also responsible for the design and operation of a PDP8 computer system to store the data arriving from the USA via the NASCOM communications network, to derive the temperatures, and to plot global temperature maps. When NIMBUS 4
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was successfully launched in April 1970, this system went into immediate and spectacular operation so that J.T. Houghton was able to demonstrate that the group could process data as it came in – a considerable advance on the techniques usual at the time which involved recording results on magnetic tape which then took months to process.

A second SCR[23] was successfully operated on the NIMBUS 5 satellite launched in December 1972.

But meanwhile, Houghton was developing in Oxford a yet more sophisticated radiometer in which he was able both to secure higher sensitivity and also to extend the measurements to higher levels in the atmosphere. The new principle was to replace the mechanical chopper system by one which varied the pressure in a single cell cyclically by a factor of about two at about 20Hz. Further, Houghton was able to cover the necessary range of operating conditions by varying the static pressure in the cell remotely. This Pressure Modulator Radiometer was carried on NIMBUS 6, launched in June 1975. It yielded an impressive volume of information on the climatology and dramatic warming effects of the stratosphere and lower mesosphere. These were of special interest in the Southern Hemisphere, where they had never been observed before. The design formed the basis of the radiometer built by the Meteorological Office for the TIROS-N operational satellites. The mechanical design was so good that the modulation arrangements worked perfectly for seven years, when they were switched off from the ground because the measurement programme had come to an end.

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PART V

Chap.17 Optical Spectroscopy

We have seen that Professor Clifton had always had a love of optics and towards the end of his fifty years an almost exclusive one. He purchased optical instruments with enthusiasm and his teaching of undergraduates in this subject was probably near to being adequate. Further, we have seen that his mind was at least open to the possibility of research in this field.

Naturally, spectroscopy was very much part of the University Observatory's solar research under H.H. Turner but many decades were to pass before astronomers and physicists pulled together under the banner of astrophysics.

T.R. Merton (1888-1969, FRS 1920) (Fig. 18) had come up to Balliol College in 1906 as a scholar and showed such ability that by 1919, at the age of 31, he was appointed Reader in Spectroscopy and a Fellow of Balliol College although, owing to supposed ill health, he had not taken a degree course. Lindemann gave him space in his laboratory which Merton, coming from a wealthy family, was able to equip. By the end of 1923 he had published 14 papers, some with J.W. Nicholson, mostly on the Balmer series and on the contribution to the hyperfine structure of the different isotopes of lead and of chlorine. In 1923 he moved his laboratory to his estate, Winforton in Herefordshire, nominally keeping his University appointment until 1936.

In 1927 a curious change overtook Merton. He ceased publishing papers and appeared to be giving himself over entirely to the pursuits of a country gentleman, for example catching the second largest salmon ever taken from the Wye – 63 pounds. But the reality was that he turned to that now suspect trade of inventor. Of the greatest significance in physics is his work on the generation and reproduction of diffraction gratings, which spans some 30 years. In particular, his name is celebrated in the Merton Nut – soft, compliant transmission between a lead-screw and the workpiece, enabling the latter to achieve a higher degree of precision than the former. His greatest
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achievements as an inventor were during the 1939-1945 war when he contributed four inventions of considerable significance – the two-layer long-persistence screen for cathode ray tubes which was in use by 1939 (although subsequently re-invented by A.C. Cossor Ltd), a black paint of greatly decreased reflectivity for night bomber aircraft, the use of N_2O as an oxidant for aero engines and a beautifully simple diffraction range-finder for fighters.

Merton was knighted in 1944 and died in 1969 aged 81, leaving five sons.

Before the advent of the quantum theory when the rationale of spectral lines became spectacularly clear, the classical spectroscopists drew a distinction between two families of satellites of the "normal" spectral lines. The relatively gross sets of lines shown up by the application of magnetic fields in the Zeeman and Passchen-Back effects were termed fine structure lines, leaving the fainter closer lines to be described as hyperfine structure. The Zeeman Effect had been predicted by the classical theory of Lorenz. But when the differences in spectral lines due to the presence of different isotopes were first observed, eg. By Merton (17.1), it was already suspected that this effect could not account for the whole

phenomenon. However, in 1924 Pauli (17.2) postulated the possibility that, if the nucleus proved to have a magnetic moment, its interaction with electronic levels might result in hyperfine structure and this was proved by D.A. Jackson (1906-1982, FRS 1947) in 1928. Nowadays the term hyperfine structure is reserved exclusively for the latter effect and the term isotope shift is reserved for lines resulting from the contribution of different isotopes.
D.A. Jackson (Fig. 19)

In 1923 a remarkable pair of identical twins came up – Derek Ainslie Jackson to Trinity College, Cambridge and Charles Vivian Jackson to Oriel College, Oxford. In the easy informal atmosphere of physics in those days, it was natural that C.V. Jackson should mention his more brilliant brother at Cambridge to Lindemann and, in due course, there was an introduction. D.A. Jackson could have taken up an offer of Rutherford's to work in the Cavendish Laboratory on a topic of the professor's choice but Jackson already knew what he wanted to work on. It is not every day that
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somebody comes along fresh from his final examination, tells the professor at another university what he wants to do and gets taken on. It was, of course, in D.A. Jackson's favour that, also coming from a wealthy background, he too could provide his own very expensive equipment. Within a year he had published his results on caesium (17.3) and Kuhn (17.4) recognizes this as the first attempt to measure nuclear magnetic moment. Jackson went on to make measurements on Rb, Ga, Tl and In. He used an RF method for excitation and an etalon spectrometer.

Derek Jackson enjoyed a substantial income from a trust established by his adoptive father*, Sir Charles Jackson, a barrister and landowner and art collector. He was able to run a modest stable and typically, rode in the Grand National. This fact of history throws up an illuminating trait of character. It was known by his few intimates in Oxford that he had ridden in three Grand Nationals but the official line was one – that of 1949 on Tulyra when he stayed on but didn't finish because his horse refused the last fence when he was lying second. The truth was, of course, that on the other two occasions he fell but for him "rode" was synonymous with not falling.

But with all his dash and gusto went occasional glimpses of ungovernably violent temper. There was a time when he was near the end of building a complicated quartz gas-purifying apparatus embodying several liquid-air traps. Much too late he realized that he had forgotten the clearance required to allow the liquid air dewars to be put in place round the traps. Oaths would have sufficed for most of us but for Jackson it was a matter of seizing a handy hammer and beating the quartz system into fragments. He would have paid for it so he was entitled to pulverize it.

Less disturbing was his response to Lindemann when gently chided for letting it be supposed that he was charging his car battery via a piece of twin flex through a window – "People notice these things, you know … Vanishingly small cost, of course, but …" "There you go Prof. – jumping to conclusions again. None of the batteries in your threadbare lab. would give a steady enough current for what I was trying to do, so I've had to use my own."
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A quiet private life was not to be expected for so tempestuous a character. Derek Jackson had five wives – one more than he owned up to in "Who's Who". But his last marriage was ideally happy for his last 14 years.

In 1933 he was joined by H.G. Kuhn from Göttingen (Fig. [20], already the author of an advanced book on modern spectroscopy, who had come to England with Simon and his colleagues. Kuhn was about the same age as Jackson but far ahead of him in theoretical learning. Their personalities couldn't have been more different and it says much for HGK's natural patience that collaboration was possible at all.

Kuhn and Jackson set out to eliminate from their measurements the broadening of spectral lines due to the Doppler effect. They moved to atomic beam methods observed from the side and by this means were the first to be able to see much smaller nuclear magnetic moments which had been thought not to be present, eg. 41K, Li, Mg, Ag and Al. They also observed the change from the weak to strong field Zeeman effect in Na, finding good agreement with the quantum mechanical predictions.

D.A. Jackson returned to spectroscopic research with H.G. Kuhn after the Second War but the fiscal enthusiasms of the Attlee Government bore so heavily on beneficiaries of trusts that flight was the only recourse. He tried living in the Irish Republic but the pressures there were little better so he accepted an appointment at Bellevue Laboratory Aimé Cotton in Paris, where he remained until his death.

After the departure of D.A. Jackson, H.G. Kuhn and a succession of research students continued with their quest for narrower line-profiles, notably of helium at liquid helium temperature, excited by a minimum of microwave energy (17.5). They also continued their work on isotope shifts.

G.W. Series (Fig. 21) came back from the war to take Finals in 1947 after one year. H.G. Kuhn invited him to join his group which during the war had dwindled to one research student, later to become Father Ronald Bright S.J., who had written a D.Phil thesis on matters of spectroscopic technique.

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When it came to discussing the topic for his thesis with H.G. Kuhn, Series was a little put out that the fine structure (Zeeman effect) in hydrogen was suggested – surely a thoroughly worked-out field. But he was soon to find out that there was still important work to be done, and one understands that even now, thirty years later, work is still going on. Since Dirac's predictions of the early 1930s, spectroscopists had been trying to get experimental confirmation of Dirac's work. The first positive experimental evidence that Dirac's treatment was wrong came in experiments using microwaves by W.E. Lamb[24] (17.6). In his subsequent theoretical treatment he added to Dirac's treatment the radiation by the electron, and by the recoil. This produced a different position for the satellite line between two D lines which, after constructing a Fabry-Perot spectrometer of greatly improved performance, Series was able exactly to confirm. He followed this up with a series of similar experiments on deuterium (he was able to extend his measurements from N=2 to N=3) and, after his thesis was finished, with the more difficult singly ionised helium.

At this point Series was casting about for a field different from classical spectroscopy. He happened to be involved in a discussion with M.H.L. Pryce and H.G. Kuhn of a recent paper which was causing no little interest to spectroscopists and others. F. Bitter and J. Brossel had reported in 1952 (17.7) an experiment at MIT in which they were able to detect resonance of RF power with an assemblage of free atoms which were themselves excited by resonant light. The cell containing mercury atoms was irradiated by mercury vapour light at 184.9 NM. The cell was placed in a steady magnetic field of about 100 G provided by two Helmholtz coils and also in a radio-frequency field variable between 50 and 150 MHz. Electron multiplier photocells picked up the resonant light and their output was read from a galvanometer. This was the first optical double resonance experiment and it opened the door to magnetic resonance experiments by increasing the sensitivity by many orders of magnitude.

[24] Wykeham Professor of Theoretical Physics, 1956-1962

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With G.J. Ritter, Series designed a double resonance apparatus similar in principle to that of Brossel and Bitter but differing from it in significant ways. They were aiming at the h.f.s. measurements so that higher sensitivity was required and, to this end, they fitted to their photomultiplier detectors electronic amplifiers and a cathode ray tube display. This required modulation of the steady magnetic field, as in the experiments of B. Bleaney et al.

In the middle 50s Series had been looking for a way to establish the conditions under which coherence in optical radiation might be expected. In an experiment (17.8) with the same apparatus Series et al were able to detect beats at frequencies of the order 100 MHz between neighbouring atomic states, demonstrating unambiguously that the radiation which was beating was coherent. (The laser was discovered shortly thereafter, when the phenomenon of coherence became of even greater significance.) This tour de force led to many related experiments and in 1968 Series's departure for Reading.

He was awarded the FRS in 1975 and the Meggers Medal of the American Optical Society in 1984. A. Corney[25] continued working in the field of atomic physics and also laser spectroscopy.

After the retirement of H.G. Kuhn (1971) and the move to the University of Reading of G.W. Series (1968), D.N. Stacey pursued the study of spectral line profiles using such new techniques as digitized read-outs and laser radiation sources. With Doppler broadening virtually eliminated, he was able to get satisfactory agreement with theory of the inherent radiation broadening. In recent times he has joined forces with P.G.H. Sandars's group – see p.[218]

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PART V

Chap. 18 Nuclear Physics up to 1958-9

Nuclear physics came to Oxford in 1919 when the subject was not yet so named but was called Radioactivity and was the province of the inorganic chemists. Alexander Smith Russell (1888-1972) (Fig. 22) graduated from the University of Glasgow in 1908 and stayed on to work with F. Soddy (1877-1956, FRS 1910) (18.1), who was investigating gamma-radiation. In 1910 he went to Berlin for a year where he worked under Nernst on specific heats and under Marckwald on the radio-chemistry of thorium. In the following year he joined Rutherford's team at Manchester to continue working on radio-chemistry. It was over several years around this time that the concept of isotopes emerged and, although it was Soddy who later coined the word, it has become accepted that the idea was jointly arrived at between him, Russell and Fajans. Russell distinguished himself in the First War and was decorated with the Military Cross. In 1919 he was appointed Dr Lee's Reader in Chemistry at Christ Church, where he occupied the laboratory in the former Anatomy School and there he worked on radioactivity, and was to do pioneer work on intermetallic compounds. (N.V. Sidgwick, the famous authority on valency, would have liked to have moved to Christ Church himself and A.S. Russell's failure to be made an FRS was generally attributed to Sidgwick's weighty influence.) It was in Russell's laboratory that C.H. Collie (Fig. 23) worked for his B.Sc., having graduated in chemistry from New College. In 1925 Collie joined Lindemann in the Clarendon Laboratory, where he became responsible for mainstream nuclear physics for the next thirty years. Meanwhile, research in the subject had already made a curious start in the Clarendon Laboratory.

A certain A. Miethe, working in Charlottenburg in 1924, had claimed to have produced 0.1 mg of gold by subjecting mercury to a direct current of 12.5A at 170V for up to 200 hours (18.2). Lindemann, always a brisk controversialist, put M.W. Garrett of Exeter College on to the problem with
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a view to a D.Phil. thesis. In a decisively negative experiment, Garrett reports (18.3) finding no gold, where he could have detected 10^{-8}gm. Further claims of the same sort were made by other authors and in a succeeding paper (18.4) Garrett reports yet more valiant efforts to transmute lead into thallium and mercury, titanium into scandium and tin into indium. He left Oxford soon after for an appointment at Swarthmore University, New Jersey.

The same underestimation of the energies required to penetrate the nucleus led Lindemann to propose an experiment for Collie, in which the effect on the branching ratio of uranium Y and Z of high energy electron bombardment was to be studied but the result was similarly negative and thereafter Collie was able to pursue more rewarding work on the uranium radioactive series (18.5).

The first of the new generation of Oxford-educated physicists who were to pursue successful careers during the Second War and for many years after was J.H.E. Griffiths[26] (1908-1981) (Fig. 24), who was to discover ferro-magnetic resonance and who, in 1968,

[26] Fellow of Magdalen College 1934-1968, OBE 1946

became President of Magdalen College. His D.Phil. thesis (1933) had been on the lifetimes of excited states in neon, which he had measured by means of a Kerr cell containing highly purified carbon disulphide.

Leo Szilard (1898-1964) had left Budapest for Berlin in 1919, where he had worked under Einstein, Planck and von Laue. Fleeing the Nazis in March 1933 he spent eighteen months at first in Vienna and then in London, where he helped the work of the Academic Assistance Council. He worked for a while at St Bartholomew's Hospital, where he played his part in the discovery of the Szilard-Chalmers reactions. For financial reasons he went to New York for nine months until June 1935, when Lindemann invited him to Oxford on a three-year fellowship endowed by ICI Limited. The plentiful supply of Czech radium (see below) influenced Szilard's decision to accept the invitation.

Szilard had been thinking about the possibilities of neutron chain reactions and had been unable for nearly two years to do more than plan a programme of experiments. He was interested in patenting the chain reaction idea but anxious to ensure confidentiality. He offered it to the Army, who totally failed to see the point of secrecy but the Admiralty agreed, possibly on the strength of a recommendation from Lindemann. Later, without telling anyone but Lindemann what the real purpose of the work was, he and
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Griffiths (18.6) investigated eleven elements from chlorine to mercury for possible neutron chain reactions, publishing their results as "Gamma rays excited by the capture of neutrons". Szilard considered that the only remaining candidates were indium and uranium. He looked at indium, fruitlessly from his point of view, though discovering the narrow neutron resonance absorption, but for various reasons never got round to uranium. G. Dannen* believes that, had he thought to discuss the matter with C.H. Collie, the discovery of fission in uranium might have been made earlier.

In 1937 J.L. Tuck came to Wadham College as a Salter Research Fellow. He and Szilard, among whose early patents was a species of cyclotron, began working on a design for the more advanced form of circular electron accelerator in which relativistic increase in mass with velocity is allowed for, and the energy is derived from the changing magnetic field, usually called a betatron. Kerst in a review (18.7) of such machines written in 1946 says "Tuck's (and Szilard's) seems to have been most promising and most complete in technical detail … and would surely have succeeded were it not for the War in Europe". On p.[104] we shall see that in Kerst's hands it did work. Szilard left for the US in December 1937 and Lindemann's subsequent attempts to lure him back were unsuccessful.

Meanwhile, a high voltage proton accelerator was constructed by C.H. Collie and C. Hurst (Fig. 25) using a 400 KV Cockcroft-Walton set which was put in the lecture room for experimental work in the basement. (The source of finance has never been clear and as in similar cases one is left with the suspicion that Lindemann paid for it himself and that his security arrangements were leak-tight.) This equipment was successfully used to generate neutrons by the deuteron-deuteron reaction and, although no publications resulted from it, much useful experience was gained. Its usefulness was limited by the inability at the time to have the ion source at high voltage but E.T. Booth was able to produce a beam of 2.5 MeV neutrons which he used to measure the capture cross-section of silver and to determine the neutron-proton scattering cross-section in paraffin wax. These experiments, completed in 1937, represent the best work done on this modest system.
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Until the late 1930s when Lindemann was able to instal the 400KV HT set, most experimental work in nuclear physics needed a radium source. The Royal Society had lent

him 0.2-0.5 gramme of radium in solution but his colleagues were sufficiently short of radioactive sources to collect decayed radium "needles" from the Radcliffe Infirmary.

It came to Lindemann's mind around 1933 that radium was separated from the uranium ores mined in Czechoslovakia and that he knew Jan Masaryk well[27]. He was told that there had been more than 1.5g lodged in a London bank since 1919. The offer of a loan of this was warmly accepted and the Royal Society radium returned. The transfer of the radium to the Christ Church laboratory, Oxford, in 1934 was relatively simple. After the usual negotiations about security, it was finally agreed at the beginning of the Second War that the radium could be stored in a basement room in the Clarendon Laboratory, which was fitted with a Bramah lock and insured for an annual premium of £20.1.0.

Late in 1938, by which time Germany had invaded Czechoslovakia, Lindemann was successful in getting the loan which had expired in November 1937 extended by three or five years, it is not clear which. It must therefore have been painful to Jan Masaryk to have to write to Lindemann on 30 December 1938 saying that his government "having lost the radium mines by appeasement, insists on having the Oxford radium returned at once, as it is the only large stock they have". Lindemann replied, expressing the University's thanks with the greatest cordiality (18.8). However, he then wrote to the Registrar, bringing up the question of how much radium is to be returned – "… after all, we all know that in 1800 years half of it will have turned into an isotope of lead" and saying "… to measure the amount accurately is a difficult and complicated business … Bona fide misunderstandings might easily arise, as it will be difficult to prove that the right amount was returned".
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As could be expected, it was the length of the Second War plus three years before we hear of the Czechoslovak radium again. Mr R. Hankey, of the Foreign Office, with a history of appointments in Eastern Europe, wrote to Lord Cherwell saying that a Mr Koblic would be coming to Oxford to recover the radium. He never turned up and it was another four years, 1952, before the Czech embassy wrote to the then Foreign Secretary, Mr Anthony Eden, in these brisk terms: "The Czechoslovak Ambassador desires to emphasise the totally unacceptable situation which has arisen owing to the continued failure of the Clarendon Laboratory to carry out the obligation of returning the radium to the Czechoslovak mines, under the terms of the protocol of 23 November 1934. In order to avoid the necessity for the Czechoslovak mines to secure the return of the radium by other means, Monsieur Ulrich asks Mr Eden to cause Lord Cherwell to inform this Embassy of the date upon which an authorized representative … will be able to take over the radium in question". This communication was sent to Lord Cherwell, who at the time was Paymaster-General, by Sir Roger Makins, KCMG, the then Deputy Under-Secretary of State at the Foreign Office. Lord Cherwell asked Mr Keeley – his deputy for the years 1951-3 – to write to the Czech Embassy, pointing out, one that the radium had been ready for collection for nearly four years, two that the original quantity of radium was short by 63.5 mg, the balance being impure, three that it would have lost 20 mg by radio-active decay, four that we had to pay £500 or £600 for having it sealed into capsules and £475 for a radio-chemical assay and, finally, we had had to pay £260 insurance over the 13 years since 1939 when the radium was last used.

It was with no little relief that we waved goodbye to the radium in 1952.

Oxford Nuclear Physicists in the Second War.

[27] Jan Garrigue Masaryk (1886-1948), Anglophile Czech Minister to Great Britain, 1925-38, Minister for Foreign Affairs, Czechoslovak government 1940, Deputy Prime Minister 1941-45.

The nuclear physics group in the Clarendon Laboratory switched to research on radar on the outbreak of war in 1939 – see chap.24 p.[145] – and Professor F.E. Simon's work on the separation of the uranium isotopes is well known. Here it is appropriate to note the central involvement of several former Clarendon Laboratory figures. Following the key paper of Hahn and Strassman (18.9), there remained the final experimental establishment of the fission by
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slow neutron capture of the light uranium isotope U^{235} as distinguished from the other isotopes. E.T. Booth, just mentioned above, at Colombia since 1937 was set up with an appropriate source of neutrons and A.O. Nier (18.10) working at the University of Minnesota and H.C. Pollock[28] and others (18.11) working at G.E. Schenectady had produced small samples of the separated isotopes, typically 21 pico-grams of U^{235} taking 11 hours to accumulate in Nier's spectrometer. L.Szilard was at home with chain reactions and Oxford and Westinghouse were probably the only laboratories in the world owning specimens of uranium of high purity. Finally, the enigmatic J.L. Tuck was present at Los Alamos when the design of the first fission bomb was being worked out and it was his idea of a hollow sphere for the fissile core that won the day.

Lindemann was thus able, when such questions became pressing, to call upon authorities with first-hand knowledge of uranium. Further, the country was lucky to have in the Cabinet Sir John Anderson[29], a man of great ability who had received a scientific education.

For slightly over a decade between the end of the Second War and the coming of Wilkinson, nuclear physics in the Clarendon Laboratory pursued a course which was in some respects unlucky but not undistinguished.

<u>The Machines</u>

The Metropolitan Vickers 400KV set was brought over from the old laboratory, uprated to 600KV and installed in the new HT room. The 1.0MV set, later to be uprated to 1.2MV, ordered in 1939 from Philips was also installed (see Fig. [26]). (Philips were said to have buried the components of our HT set on the outbreak of war to stop the Germans from capturing them.) Meanwhile, a great deal of work went ahead on developing ion sources for use at high voltages and many forms of detecting and measuring equipment. This work was organized by C.H. Collie, F.V. Price and D.Roaf. The paper which has best stood the test of time was the measurement of the capture cross section for
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slow neutrons in water, published by R.E. Meads, C.J. England, C.H. Collie and G.C. Weeks (18.12). Their result, which is accurate to 2%, has been the accepted value for twenty-five years and only when a higher accuracy is needed will anybody want to repeat the experiment.

In a paper (18.13) in <u>Nature</u> in 1948, D.W. Kerst spells out the early history of the synchrotron, first applied to the acceleration of electrons. (Kerst started life as the expert at G.E. Schenectady on the design of X-ray transformers and it is not surprising that he moved from accelerating electrons in copper wires to accelerating them in a toroidal tube.) In this paper Kerst mentions a design by J.L. Tuck and L.Szilard in which it was proposed to run the AC magnetic field at radio frequency, say a few mega-hertz. Tuck, with T.R. Kaiser, returned to the air-cored betatron after the Second War in a paper (18.14) of 1948, but no air-cored betatron was ever built in Oxford and probably nowhere else either.

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[28] The research student in Oxford who had worked on the ultra-centrifuge as a means of isotope separation (see p.[ ]).
[29] 1882-1958, later 1st Viscount Waverley cr 1952, FRS 1945, GCB 1923

What did happen was that, funded by DSIR, a 16MeV betatron was built by British Thomson-Houston Ltd and its arrival was notified in a paper (18.15) by J.L. Tuck, R.S. Rettie and K.J.R. Wilkinson (the latter being the designer from BTH). The peak electron energy was 16.3MeV. A similar betatron was designed by D.W. Fry of the infant Atomic Energy Research Establishment, then at Malvern, which was built by Metropolitan Vickers. J.L. Tuck's research student, T.R. Kaiser, was able to gain useful experience on this machine. However, the Clarendon Laboratory betatron was found to suffer from a slow instability of the electron orbit. This was analysed by Rettie and corrected by adding an out-of-phase component to the magnetic field by R.W. Parsons, who went on to measure photo-disintegrations of heavy elements (18.16). This betatron is now in the Science Museum, London.

As part of a national programme mounted by DSIR, the Clarendon Laboratory was to have a synchrotron planned to accelerate electrons to 140MeV. When it became known in 1947 that the threshold energy for the production of pi-mesons (later pions) was 140MeV, there was room to hope that the actual energy achieved would exceed this figure. But, unhappily, largely because of
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distortion in the fifteen tons of magnet material, the maximum energy was 125MeV and the Oxford synchrotron was limited in its ability to advance the frontiers of nuclear physics.

Under the direction of J. Moffatt[30], the machine was installed in the HT room basement at the far (east) end. An extension was built northwards into the Parks but hidden behind a beech hedge. This provided for experimental work, for the capacitor bank and for a workshop. The orbit tube which had to be sectioned into segments presented problems but, by early 1953, a beam of 120MeV electrons was produced. A number of interesting papers were published (18.17).

Hans Halban (1908 Leipzig – 1964 Paris) (Fig. 27) was very much a key figure in nuclear fission circles in the late 30s and his name has gone down into history linked with those of Kowarski and Joliot-Curie. It was he who brought the best part of the world's stock of heavy water from Paris to London in milk churns by public transport in June 1940. During the Second War he worked for two years at the Cavendish Laboratory where he was joined by Kowarski. In 1942 he went to Montreal to take charge of the Joint British-Canadian project which included the building of a heavy water moderated reactor. But administrative life did not suit him and when, in 1946, Lord Cherwell invited him to the Clarendon Laboratory, he accepted with alacrity, although the offer cannot have been very attractive when it came to accommodation, financial support etc. The ground floor north-west part of what is now the Townsend Building was cleared of the strange assortment of rotating electrical machinery, once thought essential for the education of physicists, and naturally became Halbania. With his personal technocrat, Vic Round – French by birth and a sergeant major in the British Army in the Second War – Halban rapidly integrated himself with the Clarendon Laboratory. Sometimes he worked with the established nuclear physicists but also independently, using for example isotopes from AERE Harwell.

> The generosity of HH and his wife, a member of the Rothschild family* and now Lady Berlin, knew no bounds. To mark the Coronation of 1953 he invited the entire Clarendon Laboratory to an ox-roast on his Bayswater Farm. He had had a brick hearth built and three butchers from Oxford market were in attendance.

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[30] Fellow of Queen's College, 1950, Provost 1987

But it is for the part which his group played in the cooperative work of the nuclear physics and low temperature sides of the Clarendon Laboratory in the early nuclear orientation experiments of 1952 that he will be remembered the longest (see p.[163-164]). In 1954 the University gave him the title of Professor. In 1956 he decided to return to Paris where he became a professor at the Sorbonne.

One of the saddest descents down the brain drain was the loss to Harvard University in 1956 of Dick Wilson. A pupil of C.H. Collie he took his D.Phil. in 1949, subsequently spending a year in Rochester University, N.Y. He returned to be a five-year Research Student (ie. Fellow) at Christ Church and, over this period, published 25 papers from the Clarendon Laboratory. Some of them concerned work on the HT sets but most of his work was done with the cyclotron at AERE Harwell.

Radiochemistry

P.F.D. Shaw[31], the UF_6 expert in the Tube Alloys group during the Second War, became involved in nuclear physics after it – partly working on the many problems in which physicists are dependent upon chemists, but also, as the Prof put it, to "erode the bastions of physics". (This may well have incorporated a reference to an occasion many years earlier when, with the greatest geniality, the Prof had drawn Shaw's attention to brown staining on the brickwork below the third floor chemistry rooms – stains which took on the brilliant white of $CaSO_4$ until further applied chemistry had brought them down to background.) His first work in nuclear physics was a quantitative study of Szilard's method of detecting neutrons using large volumes of iodide. Notably, he and E.Eliot (18.18) explored the difference in the angular distribution of neutrons between the two reactions $D(d,n)He^3$ and $D(d,p)H^3$.

Before the establishment under G. Garton (p.[189]) of his chemical preparation group, P.F.D. Shaw helped the groups working on paramagnetic materials and the like to grow crystals of an increasingly wide range of complex salts. Shaw was obliged to retire early and was unable to see through his planned move to the new Nuclear Physics Laboratory.

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> Peter Shaw draws attention to (18.19) the vigorous social life which flourished in the first fifteen years of the new Clarendon Laboratory and which was particularly fostered by F.E. Simon, H. Halban and M.H.L. Pryce and many members of their groups – including Shaw himself. The implication is, of course, that subsequently social life became less united and it is undeniable that this is true, though attributable mainly to the rapid increase in numbers.

As everyone knows, Professor N.F. Mott[32], when he went to the Cavendish Laboratory in 1954, decided that nuclear physics at Cambridge must go and, in consequence, the versatile D.H. Wilkinson[33] (Fig. 28) came to Oxford to take up a new professorship of experimental physics. In those days it was possible and, indeed, customary for new professors to be able to command a new building and the left-hand part of the model shown in Fig. [29] houses the department which he was able to establish.

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[31] Fellow of St Catherine's College 1954 to 1970

[32] Cavendish Professor of Physics, 1954-1971, FRS 1936, knighted 1962

[33] Fellow of Jesus College, Cambridge 1944-1959, FRS 1956, Professor of Experimental Physics, Oxford 1959-1976, knighted 1974, Vice Chancellor University of Brighton 1976

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18.19 Private communication to AJC

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PART V

Chap. 19 Four Exports

Four Oxford physicists of the inter-war years* must be noted here because of their subsequent distinction. R. d'E. Atkinson (1898-1980) was a pioneer in the field of nuclear astrophysics (19.1) and his papers written later with F.G. Houtermans in the thirties laid the foundation for the more detailed work of Gamow later, when the necessary data had become available. The basis of the carbon-nitrogen cycle for energy release by the fusion of hydrogen nuclei to form helium in the sun and the stars, and the conditions for cooking the heavier elements in stellar interiors was thought out by them many years ahead of anyone else.

Atkinson came up from Manchester Grammar School in 1919 proposing to read classics, having won a scholarship in 1916 but, after service in the Royal Field Artillery, he was able to persuade Hertford College to let him read physics, if he passed mathematical moderations. This he did with a Class 2. During his time as an undergraduate in Lindemann's laboratory, he was working on theoretical and practical problems in astrophysics but nonetheless achieved Class 1 in 1922, having published his first paper as an undergraduate. He was at times a sharp critic of Lindemann who never seemed to resent it. Hertford made him a research fellow in 1922 but, after two years as a Rockefeller Travelling Fellow starting in 1926, it became clear to him that the life of an astronomer fitted his talents best. From 1937 to 1964 he was Chief Assistant at the Royal Greenwich Observatory with a gap in the Second War when he became a leading expert in degaussing. Among his gifts was practical excellence – in his fifties he designed an astronomical clock for York Minster and in his eightieth year personally engraved a special sundial for Indiana University.

M. Ryle (1918-1984, FRS 1952, Kt. 1966) came up to Christ Church in 1936. At the outbreak of war he went to TRE Malvern, where he served with
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distinction and was one of many contemporaries in the field of radar who, after the war, applied the techniques they had learnt to physical problems. Ryle's insight as a physicist led him to demonstrate that one does not need the whole circle of a dish aerial to give any particular aperture – the effective aperture can be synthesized using at the minimum one fixed and one movable aerial of small size. Thus, latterly he was using four fixed and four movable 40-foot paraboloids, the latter on a 5-kilometre length of railway line near Cambridge and at a wavelength of six centimetres. This system had a resolving power of two seconds of arc. Ryle's group, using such equipment, were able to build up a very detailed map of the radio sources deep into the universe and the "Big Bang" theory of its evolution has become accepted by the combination of his and other methods of observation. Recognition came satisfactorily soon so that in 1972 it was possible for Ryle, as the first physicist astronomer, to become Astronomer Royal. He and Anthony Hewish were joint Nobel Prizewinners in 1974.

R.V. Jones come up to Wadham College in 1929 where he was a pupil of T.C. Keeley. After taking a D.Phil. on infra-red in 1934 – as it seems one had to if nobody else was – he stayed on for two years doing further infra-red work as Skynner Senior Student in Astronomy at Balliol College. His spectacular wartime career is covered in his fascinating book <u>Most Secret War</u> (19.2) and here it only needs to be said that, if Winston Churchill would have been lost without Lindemann, Lindemann would have been lost without R.V. Jones. There is also the unbelievable truth that he managed to work simultaneously for both Lindemann and Tizard.

Stephen Hawking (born 1942) read physics at University College when R. Berman was Tutor there. On graduating in 1962 he moved to Cambridge where he became a research student in the Department of Applied Mathematics and Theoretical Physics under D. W. Sciama[34]. (Sciama has been in Oxford for the years 1976-86.) His work has covered mainly the gravitational collapse of giant stars, i.e. those of around ten solar masses. In 110 books and papers in the last twenty-five years he has established the behaviour of these condensations termed by J.A. Wheeler 'black holes' (19.3). Elected FRS in 1974
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appointed CBE in 1982 and awarded six honorary degrees, Hawking has been Lucasian Professor of Mathematics in the University of Cambridge since 1979. Unhappily Hawking has been the victim of one [of] the motor-neurone conditions for the last twenty-five years.

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<u>References</u>

Chap. 19

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19.2 R.V. Jones, <u>Most Secret War</u> (1978), Hamish Hamilton
19.3 J. A. Wheeler, American Scientist (1968), <u>56</u>, 9

[34] FRS 1983

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PART V

Chap. 20 New Blood – 1933

Lindemann had been interested in work at low temperatures since his days in Berlin (1908-1914) but his efforts to get it started in Oxford had not met with much success – the hydrogen liquefier which he had bought from Nernst had suffered from that scourge of cryogenics, blockages due to impurities in the hydrogen supply. Lindemann had kept up his contacts and in 1931 he bought a hydrogen liquefier designed by F.E. Simon and made by the reigning cryogenic technocrat of the day, Hoenow in Berlin. This was set up by T.C. Keeley (Fig. [30]) and, using hydrogen from a Knowles electrolysis plant, he was able to produce 2.5 litres per hour. Lindemann was then ready to procure a helium liquefier and again Simon was able to sell him one of his expansion liquefiers – for £30. K. Mendelssohn brought it over in January 1933 and, within a week, it could produce in one shot 20cc of liquid helium, which typically lasted one and a half hours. (There was no sort of race in question, of course, but P. Kapitza at Cambridge was not pleased when he heard that the first helium to be liquefied in the UK was not to be his after all.)

Thus the pursuit of low-temperature physics at Oxford was inaugurated at a time when the future was still obscure. Hitler's rise to power early in 1933 came unexpectedly but within five months F.E. Simon and his colleagues (Fig. 31) were established in Oxford. Lindemann was quick to see that prompt action on his part might secure for Oxford a group of physicists of exactly the specialities he most wanted, and others for elsewhere. He got into touch with the chairman of ICI, Sir Harry McGowan, who was able to persuade his directors to offer contracts to many would-be emigres from Germany and some of these started from 1st May 1933. Later that month the Academic Assistance Council was formed under the Chairmanship of Lord Rutherford. This body over the succeeding years found permanent jobs in the UK for 500 university teachers from the continent and had 2,600 on its books.

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F.E. Simon could have stayed in Germany having been a <u>Frontkampfer</u> in the First War, particularly as he had been decorated with the Iron Cross, First Class. (In later years he was to speculate about how many other bearers of that decoration also had the CBE.) But conditions were rapidly getting worse and more sinister and, with Lindemann behind him, the prospects in Oxford were very attractive. So, with his wife and two little girls he settled into 10 Belbroughton Road where his widow still lives. K.A.G. Mendelssohn had come in April and N. Kurti (Fig. [32])[35], H.G. Kuhn[36], the spectroscopist, and H. and F. London came later to the Clarendon Laboratory, although the London brothers were to move on. Security was very much what they didn't have – the rather meagre income from ICI could not be counted on for more than five years. In the case of Simon, A.C.G. Egerton, Reader in Thermodynamics, moved in 1936 to become Professor of Chemical Technology at Imperial College and Simon inherited his Readership. N. Kurti was financed by the Tube Alloys project from 1940 and after the war by university and college

[35] Fellow of Brasenose College 1947-75

[36] Fellow of Balliol College 1950-71

appointments and H.G. Kuhn held a college appointment from 1938 and university appointments from 1945. K. Mendelssohn was slightly less lucky in that he did not hold a permanent appointment until 1947. But scientific success was certainly theirs – all four were FRS by 1956.

When F.E. Simon made his move to Oxford, it was at an ideal time in his career – he was still just under forty, he had fifty papers to his credit and his two associates, K. Mendelssohn and N. Kurti, were in their twenties with established reputations in low temperature physics. Simon himself was, of course, a totally dedicated physicist and his interests ran wide and deep, from the establishing of a third law of thermodynamics to getting as much heat out of a domestic fire as possible, and letting little of it escape up the chimney. For a very short time the locals in Oxford must have thought they had acquired a typical German, thorough, didactic and perhaps even obsessive. But they would have soon discovered that this one had a rich sense of humour and was given to self-mockery.
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> C.H. Collie told me that Simon had said that the refugees from Germany in the '30s had to decide whether to speak English better than the natives or concentrate on being a Funny German. Simon was one of the latter to the length of appointing himself Vice-President of the Broken-English Speaking Union – not the President because no-one could compete with F. London.

But, though F.E. Simon was a fully paid-up physicist, he was also very much a family man – his schoolgirl daughters had to be brought down to gaze at the hole in the ground which was to receive the 2MW generator. The family, however, extended to include his whole group and their welfare, whether a matter of their immediate research and income, their future job, their prospects of matrimony were kept an eye on and, no doubt, entered up in the ever-present pocket book. (People used to say that Simon only picked good-looking secretaries so as to provide wives for his research students and, if this had been his policy, it certainly worked. However, he protested that those who take care over their appearance also take care about their work. We knew we were not expected to take this seriously.)

> H. Meyer looked in to see him when the pocket book happened to be out. Simon said, "On Saturday 'Lotte and I give a party. But you are not invited". This story illustrates the complete candour which reigned whether with a colleague of long-standing or a new research student.
>
> I was struck by this when I was first interviewed in 1947. The Great Man said, "You are not expected to get a good degree, I think". I said that that was so. "I do not regard performance in examinations as important – I did not do well myself in Berlin." I was staggered. Here was a university professor, not merely dismissing the be-all and end-all of the whole system, but volunteering the information that, although a patently distinguished academic scientist himself, he had not done it by the usual route.
>
> This candour went with a remarkable depth of imagination. In my first year, my bicycle let me down (literally) and I lost some front teeth. Simon came round to see me – not unremarkable in itself – and said he supposed that I would have to have a plate. Indistinctly I said yes, I would. Simon said, "For the first twenty-four hours you will wonder how you are going to live with a thing like this in your mouth. And for the rest of your life you won't know whether it's in or out". What he said was, of course, a comfort during the twenty-four hours but how many people relate their own discomfort of decades earlier to the transitory condition of someone else today?

It was a strength of F.E. Simon that he was enormously persuasive and he had no scruples in getting down to business after the coffee in Christ Church Common Room. "Now you know we are very poor in the Clarendon …" And then later "… perhaps it is easier if you make the cheque out to Solid State Research Group". Out of such resources came the small quarterly cheques known
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only to the Vice-Chancellor, which gave tangible evidence of what was perfectly clear in any case – that one was valued.

> Not that it was smiles all the way. Simon liked parts of a cryogenic system between which heat flow must be minimized to be physically close. I had designed the helium liquefier of 1956 with two such parts, separated by a metre or so. But I had minimized and calculated the heat leak. Simon came and saw the parts in question under construction in the workshop and disapproved, as I knew he would. As he moved off, he said "If it works, yours will be the glory: if not …". And the saurian look I got over his shoulder was frightening. As it happened, I telephoned him to say that the liquefier was working on the day before his death. Nonetheless, he could mock the English and their outlandish units with "And how many gahllons per hour does it produce?" Luckily, I could say more than one.

F. E. Simon's principal interests in experimental physics were as follows.

1. The Third Law of Thermodynamics

Nernst was very much a chemist and his "heat theorem" made possible the calculation of the kinetics of chemical reactions in previously insoluble cases. To F.E. Simon it had the status of a fundamental physical law. N. Kurti in his obituary (20.1) quotes Simon's preferred enunciation of it as follows:

> The contribution to the entropy due to each factor within the system which is in internal equilibrium becomes zero at absolute zero".

It was therefore one of Simon's chief preoccupations to show that all the apparent contradictions of the Third Law arose either from cases when "internal equilibrium" did not apply or in which extrapolation of specific heats was unsound. Familiar cases of the first kind are the glasses and of the second kind the specific heat of solid ortho-hydrogen which he showed to have the anomaly at around 8K (20.2, 3, 4)

2. Liquid and Solid Helium

It had long been puzzling that liquid helium-4 at or below atmospheric pressure shows no sign of becoming solid as the absolute zero is approached. Simon therefore measured the melting curve of solid helium and was able to deduce the rationale of its properties which he put into words as "Liquid helium is blown up by its own zero point energy". As we shall see (p.[207]) he and B.V Rollin (20.5) discovered the film properties of liquid helium II, later extended by Daunt and Mendelssohn (20.6).
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Simon pursued the melting curve of helium up to 5,000 atm. in Berlin and up to 7,300 atm. in Oxford, enabling him to confirm Lindemann's melting formula. He was also able to confirm the validity of the law of corresponding states and applied it, for example, to cases of geophysical interest.

3. Magnetic Cooling

Simon and Kurti had carried out experiments in Breslau on the magnetic properties and specific heat of gadolinium sulphate (20.7). The first demagnetization experiments were carried out by Giauque and MacDougall at Berkeley (20.8). When Simon had settled in Oxford, he applied similar thermodynamic procedures to those used ten years earlier in experiments with Lange (20.9) on solid hydrogen. Further, he applied his brilliant idea of using gamma-rays as a source of thermal energy. These experiments are described below (p.[118]).

4. Nuclear Effects

At about the time that Simon came to Oxford the possibility was being adumbrated of exploiting the magnetic moments possessed by some nuclei for exploring the temperature range at which they could be expected to interact – and a much lower temperature because the magnetic moments were some three orders of magnitude smaller (20.10, 20.11). Further, following a meeting with H. Halban in 1937-8 he was able to plan for the successful experiments in nuclear orientation of the early 1950s.

A scientific interest which did not come to fruition until after the Second War was the mechanism behind the variation of thermal conductivity with temperature in dielectrics. In a programme of experiments with R. Berman[37] (20.12) and others he was able to show the relative contributions of two effects, one predicted by R.E. Peierls and the other discovered at Leiden in the 1930s. It was also with Berman that Simon was able to determine the equilibrium conditions for the synthesis of industrial diamonds (20.13).

As well as bringing these lines of investigation in low temperature physics with him, F.E. Simon brought a lively interest in applied thermodynamics in a variety of temperature ranges. His famous expansion helium liquefier gave him great satisfaction because of the special low

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temperature phenomena which account for its remarkably high performance – that is the vanishing specific heat of the metal pressure vessel and the uniquely high density of helium gas at modest pressure (eg 100 atm) and temperature (~11K).

The experimental work which chiefly engaged F.E. Simon in his early years in Oxford was the classical experiments which he did with N. Kurti on the establishment of absolute thermodynamic temperature scales below 1K.

> In an idle moment I once asked Simon what his role in these experiments was. He replied "I was the magnet's chauffeur". Dexterity with deliberation was always his strong suit.

Kurti and Simon published a number of papers between 1933 and 1936 in which they describe the techniques of adiabatic demagnetization and the use of gamma rays as a controlled source of heat energy. However, they were seriously limited by the magnetic field available at the time – only 1.0 tesla for 20 minutes or 1.2 tesla for 5 minutes. (Simon introduced a useful method of keeping abreast of the field in use at any particular time – by smell.) Fortunately, Simon was offered the use of the enormous magnet at the Bellevue Laboratory of the Academie des Sciences in Paris. With the polegap at five centimetres as required for a cryostat, a field of 4 tesla was available: (with the gap reduced to a few millimetres, 7.5 tesla was possible). The magnet weighed a hundred tons and required only 100 kW.

Using iron ammonium alum, Kurti and Simon were able to establish the absolute thermodynamic temperature scale between c. 0.05K and 1K by the famous procedure of taking the working substance round a Carnot cycle. They were also able to establish that the iron ammonium alum has a Curie point at 0.03 K and to plot the specific heat curve with its anomalies at the Curie point as well as a Schottkey anomaly between 0.05K and 0.1K (20.14).

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[37] Fellow of University College 1955

K.A.G. Mendelssohn (1906-1980, FRS 1951), a first cousin of F.E. Simon, came to Oxford with his D.Phil. from Berlin which had included the highly important discovery of the increase with falling temperature of the specific
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heat of ortho-hydrogen. There had been an agreement in Berlin that Simon's group would not compete with W. Meissner's work on superconductors. But during their brief stay at Breslau and when they had reached Oxford, no such ban applied and moreover they were free to work on the fascinating new effect in superconductors discovered by Meissner – the expulsion of magnetic lines of force at the onset of superconductivity.

Mendelssohn gathered a substantial group round him. J.D. Babbit, R.B. Pontius (research students who left Oxford), Judith R. Moore who became Mrs R.A. Hull, and J.G. Daunt, who went into radar during the Second War, returned to low temperatures after, but most of whose career was in the US. (T.C. Keeley participated in much of this work.) This group published 35 papers over the six years 1933-39. Their experiments reinforced by similar experiments at Leiden and at Kharkov gave the first clues to the difference in kind between superconductivity in pure metals and in alloys as well as in the way they presented the Meissner effect.

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Chap. 20

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PART V

Chap.21 Building the New Clarendon Laboratory

No doubt Lindemann set about formulating plans for a new laboratory very soon after his research programme had got under way in 1919. One problem was solved for him – under pressure to find a site for the School of Pathology endowed by Sir William Dunn, the University agreed in 1924 to the use of land on the southern border of the University Parks for new science buildings and nine acres would be left unallocated when the Dunn School was complete in the south-east corner (21.1). On 24th May 1934 the Committee on Sites requested the Professor of Engineering Professor R.V. Southwell and Lindemann's factotum, I.O. Griffith (see p.[77]), who had recently been coopted to the committee, to prepare a report on the possible sites for science buildings. (Engineering already occupied its original site at the apex of the Keble triangle and eventual occupation of the remainder was in prospect, although some leases had, at that time, still thirty years to run.) A great deal of time was devoted to the conflicting claims of Professors Lindemann, Townsend and Goodrich (Zoology) over a small patch of ground which all three had their eye on for extensions. However the committee felt that the time had come for the broad brush and, on 2 November 1935, an architect, T.A. Lodge, of Lanchester and Lodge, Bedford Square, was appointed to prepare a scheme for the redevelopment of the science buildings and the clearing away of many huts, sheds and outhouses. The report singled out the Clarendon Laboratory as being in "an advanced state of obsolescence" and, subject only to minor work for Zoology, the Clarendon Laboratory's new building became top priority in the scheme drafted by Lanchester and Lodge (see Fig. 34). (The cost of the building fell well below the estimate, as did the cost of the conversion of the east wing by the same architects in 1964,)

Much of this saw the light of day, viz. Clarendon Laboratory, plus post-war extension, Zoology Department extension, Physical Chemistry Laboratory, Dyson Perrins Laboratory extension, Inorganic Chemistry Laboratory extension, Botany/Forestry Laboratory, Geology Department extension,
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Physiology Laboratory and roof extensions to several of their earlier buildings.

Lindemann's requirements for his new laboratory are set out in some detail in an undated draft letter (21.2) probably early in 1937. He makes the point that the building must be designed not just for today's physics but for tomorrow's and, therefore, flexibility must be incorporated to allow variation in the sizes of rooms. Vibration must not travel far so brick is needed, not steel and concrete. The workshops and compressor rooms must be isolated from the main building as to vibration. High power DC electrical supplies must be run into both low temperature and nuclear physics rooms so that a large magnet of the future can be used anywhere. A large double-cube screened room for the high-tension set that the writer seems very uncertain of getting is to be built at the back and four screened rooms provided. Two ground-floor rooms were to be built with vibration-free floors for spectroscopy. (In this letter this discipline is looked upon very much as a hand-maiden to nuclear physics.)

The astonishing point about the letter is not so much its far-sightedness – although much of what it says would still apply – but that everything Lindemann asked for he got.

A row broke out between Lindemann and the University Chest about the heating of the building. The lay finance lobby proclaimed that electricity was the fuel of the future and was bound to get cheaper. Lindemann – backed up by Simon, no doubt – explained that thermodynamic principles showed a difference in kind between thermal energy such as that offered by fossil fuels and electrical energy claiming, quite rightly, that in the long term the former was certain to become cheaper. In the end, it was decreed that electrical heating was to be provided but that the cost of this heating, which would be separately metered, would not fall upon the laboratory. And this position ruled for the next thirty-five years or so.

An interesting footnote to this ancient controversy has been that in recent years rising heating costs in the University as a whole brought the annual cost per square foot in the Science Area increasingly close to the Lindemann Building's electrical heating annual cost per square foot and, for a
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time, actually exceeded it. The explanation is, of course, that it is much easier to control electrical heating and now that the Department pays for its own heat, it is much easier to maintain a spartan standard.

The heating panels themselves are of polished Portland stone, many fossiliferous. Unlike hot-water heating, maintenance costs over 45 years have been virtually zero.

A Decree of Congregation on 16 June 1937 allocated the estimated sum required to build the new Clarendon Laboratory in accordance with legislation in force at the time:
"Decree 11. That the Curators of the University Chest be empowered to submit to the Ministry of Agriculture and Fisheries a proposal under the University and Colleges Estates Act of 1925 for a loan of a sum not exceeding £77,000 to be raised by the sale of securities held by the Ministry on account of the University and to be expended on a new building for the Department of Experimental Philosophy[38]".

Then, at the last moment there was a fuss. Lindemann had pointed out that, if his new building were moved six feet eastwards, the splendid chestnut tree between it and Parks Road could remain and be the great feature of the place that it has been for nearly fifty years. This sensible change was made. But some eagle-eyed enemy of the sciences pointed out that Lindemann had grabbed six feet more of the Parks at the back of his new building. The position was thought to have been regularized by this remarkably opaque passage in the University Gazette of 13 December 1939:
"Decree 16. That subject to the consent of the City Council, Decree (8) of 12 June 1934 be amended by the insertion of 'more or less (so however that the total area so excepted shall not exceed the total area which would have been provided by a depth of 300 feet precisely)' after '300 feet'."

What we now call the Lindemann Building consists of a hollow square with two floors of research rooms and a second floor of undergraduate teaching rooms with a central lightwell, with an additional third floor added in 1960*. On the eastern side of this square was the room 30' x 70' and two and a half storeys high for the 1.2MeV accelerator. This
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room had a de-mountable floor with a basement underneath where the experimental work was to be carried out. The screening arrangements were thorough and, no doubt, very expensive. There was a generous single storey workshop block to the south, including glassblowing and the hydrogen liquefaction facilities.

[38] This nomenclature was wholly irregular and, after many years, has been successfully suppressed.

On the SE corner there had to be excavation to provide for the overrun chamber for the substantial goods lift. When the virgin soil was first pegged out, someone with a taste for symmetry provided for excavation on the NE corner as well and, before anyone had noticed, the basement had become larger by some 400 sq. ft.

The internal furnishings were on what might have been regarded as extravagant lines – genuine teak everywhere and bronze door furniture. It is, of course, easy to poke fun at architects when small but essential details get left out and the Clarendon was no exception – a letter box had to be extemporized into a small window and 45 inches of solid masonry had to be drilled through to provide a front door bell. The major asset in the design is the very generous provision of service ducts – both for wired services which go from the inside of the building and radiate outwards into the various rooms and for piped services which run in vertical ducts and radiate inwards from the outside walls.

However, the provision of electrical services within the research rooms was a matter of prolonged haggling between the consultants and the physicists. Unfortunately, Derek Jackson had a highly eccentric, not to say dangerous, preference for live conductors which one could clip crocodile-clips to. He made it an issue for rebellion in a way which only he knew how. If he had confined himself to the provisions for his own rooms, it might have been all right 50 years ago. But his influence was allowed free rein all over the laboratory. The final compromise was to fit specially made chambers (see Fig. 35) and in these to run bus-bars for 100V DC and for 240V AC. These bus-bars were well up to taking 100 amps, although backed by fuses limited to 30 amps.

Special plugs were provided for AC and DC which, though not inter-changeable, were not polarized. Worst of all, no provision was made for an earthing conductor and that at a time when 3-pin plugs and sockets had been standard for many years. The bus-bars and sockets were virtually unused and
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the bus-bar chambers functioned merely as ducting through which 3-pin plugs and sockets mounted under were supplied.

It has been possible recently to replace the main electrical services in ducts which had been provided but, in the meantime, used as cupboards, 45 years before. Elsewhere, the generous provision of supplies from a total of six local generators were added to in the 25 years after the Second War and have been stripped out to accommodate new and heavier main supplies.

The only shortcomings in the original design were the woefully small area for stores – both technical and scientific – and the total absence of research offices. Lindemann held that people only needed a small table in the corner of their research room. He did not allow for the insupportable clatter of perhaps three rotary vacuum pumps nor for the heat often generated nor for the proportion of 230 sq. ft. space that the apparatus filled up. This problem was not resolved until 1966 when twelve minute offices were built in the central lightwell.

Only two structure defects gave serious trouble – one was the universal tendency of flat roofs to develop rain water leaks. This was effectively cured in 1960 by the simple expedient of clapping on a new storey with pitched copper-clad roofs. The other fault was uneven settling on the asymmetrical front staircase – a frequent occurrence, we were told, on clay. The total restoration of the terrazzo staircase and landing was done for us in about 1970 in a period of relative affluence.

University Appointments

Remarkable as Lindemann's achievements were between the wars in attracting outstanding academics and in getting a new laboratory built on a prime site, it was the shameful truth that, apart from himself, Dobson, Jackson and Simon, there was but one University-paid post: there were five college fellows (C.H. Collie – Christ Church, J.H.E. Griffiths – Magdalen (until 1940), C. Hurst – Jesus, T.C. Keeley – Wadham and H.G. Kuhn – University (until 1939)[39].
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Lindemann writes in February 1939: "It is, of course, difficult to ascertain how much is contributed by University and Colleges to the Chemistry School. But there is no doubt that the Chest pays 14 University Demonstrators, whilst the Colleges provide 16 Fellowships.

As stated above, one must recognize that the Clarendon Laboratory comprises only half of the Physics School at Oxford but the fact that I have got only one full-time demonstrator, one Reader and three (permanent) College Fellows working in the Department, does seem to indicate that my application for a large increase in grant in order to provide stipends for demonstrators and researchers is not only inevitable but justified.

… Literally three quarters of my activities are concerned with finding and maintaining stipends for the research workers in my Department. Apart from this waste of time – for it is not for this that the University has actually engaged me – the uncertainty of their position and the worry of the men about the future is bound to impair their work."

In the event, the Second War started seven months later and the Admiralty and the DSIR between them paid many salaries. After the War, C.H. Collie made a demarche on the Professor on behalf of his colleagues. By 1947 there were eleven University appointments and in the following two years fourteen.

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Chap. 21

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[39] In Townsend's department, the Electrical Laboratory, there were four University appointments, three of which were college fellows (E.W.B. Gill – Merton, P. Johnson – Magdalen and S.P. McCallum – New College).

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PART V

Chap. 22 Townsend II

After the First War, Townsend published two papers on radio topics, arising from his time in the services but he soon got back to electrical discharge in gases. The Rutherford-Bohr atom had been launched in 1911 and physicists were agog to perform experiments which might confirm its essential rightness or otherwise. Among the most directly relevant experiments were those pioneered by J. Franck and G. Hertz (22.1) of Göttingen. They used a free-electron apparatus not unlike the three-electrode thermionic valve known as a triode but in which a vapour, for example that of mercury, was included. Electrons from a hot filament were accelerated by a grid which could be set at a known voltage and the current measured. When the voltage of the grid V was such that the electrons excited radiation from one of the mercury lines of frequency ν, when hν = eV, a discontinuity in the grid current was seen. This experiment, involving as it did a beam of mono-energetic electrons, was generally regarded as the most direct and decisive that could be performed and confirmatory experiments of the same sort were carried out in many laboratories.

Franck and Hertz and their experiments became a King Charles's head to Townsend for a number of reasons.

1. In their 1913 paper they speak of his results as "ausserst verschieden" (widely different) from the accepted results from other laboratories.
2. Their electrons were not as mono-energetic as they supposed - they had the Maxwellian spread of energies appropriate to the temperature of the filament.
3. The variation of voltage along the length of the filament contributed to the spread in energy.
4. Although he never published anything on the subject, Townsend had tried a Franck and Hertz experiment and had found that he could get current discontinuities wherever he liked.

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5. Franck and Hertz were German. (It was not unusual in and after the First War for attitudes entirely appropriate to an enemy in wartime to spill over in irrelevant directions and balance in such matters was not to be expected of Townsend.)

He, therefore, set out to refine his measurement of ionization potential in the rare gases, starting with helium, in a number of papers with V.A. Bailey. (Bailey, who was Egyptian by birth, was for most of his career Professor of Physics in the University of Sydney. He became famous for his application of Townsend's concepts to the explanation (22.2) of the cause of the so-called Luxembourg effect (22.3) – a modulation of the ionosphere caused by the powerful Luxembourg transmitter (1190m.) which caused reflected radio signals to be similarly modulated.)

Townsend knew that the Rutherford-Bohr atom was not <u>right</u> – he had been experimenting with atoms for years and knew that they behaved like little billiard balls and not at all like solar systems. (This wholly correct prejudice has been mis-interpreted as geriatric distaste for the quantum theory but later Townsend was to welcome the more spherically symmetrical atom of Schrödinger of 1926 (22.4).

Townsend's experiments and their acceptability by the world of physics were bedevilled by three difficulties, all of which had obscure effects at the time.

1. Purity of samples and their contamination by mercury, high-vacuum oils and greases, etc.
2. Statistical treatment.
3. Properties of the rare gases, arising from wave mechanics.

1. Townsend and his early collaborators undoubtedly had a Cavalier attitude to purity – in a paper of 1912 they write: The gas was "not quite pure … about 2% impure". No mention of how he knew it, nor how accurately.

Charges that some or all of Townsend's anomalous results were due to his high-vacuum technique drove him to resort to pumps of the molecular drag type invented by Gaede in 1912 (22.5) and improved by Holweck in 1926 (22.6), in place of diffusion pumps using mercury or oil as the working substance. How he raised the considerable sum necessary has been lost sight of but we still

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have the mechanical parts of the pump. It consists of an aluminium alloy rotor driven at 5000 r.p.m. by a six-phase set of coils through part of the external housing. The rotor and starter are grooved and the clearance between them varies from 0.02 to 0.05 mm. Unhappily, there was no change in the results and Townsend was quick to defend his earlier work. But the oral tradition has it that Townsend had contaminating items such as the mercury-sealed valves in use at the time (see for example Strong (22.7)) on the high-vacuum side of the Holweck pump.

It became clear that the necessary gas purities varied from one part in 10^6 to one part in 10^8 and only when S.P. McCallum and F. Llewellyn Jones joined Townsend's team in the late 1920s were such purities and low contamination achieved by quartz systems baked out to 700°C, liquid air and charcoal traps, calcium gettering and RF clean-up.

2. The Maxwellian energy distribution is, of course, the pattern but clearly in a gas through which a stream of electrons is passing several phenomena occur which change the situation utterly: two species are present, differing in mass by at least a factor of 2,000, one of these (the electrons) is accelerating rapidly, the other more slowly in the opposite direction; inelastic collisions will be occurring between the more energetic electrons and the atoms. Townsend's original treatment (22.5) made approximations which were not justifiable and it was not until Druyvesteyn in 1930 that an exact theory became known. (See Appendix C contributed by Prof. F. Llewellyn Jones.) In the meantime, a remarkable controversy had sprung up in the pages of the Proceedings of the Royal Society. R. d'E. Atkinson (see p.[109]) who had left Hertford College two years before, attacked Townsend's work in his characteristically trenchant way. (Lindemann, of course, had to be the channel and it is good to read a letter from Atkinson from New Jersey dated 5th January 1930 (22.6) in which he chides the former for delay in passing his paper on.)

3. Of the three lighter Group 0 gases, neon behaves most like a billiard ball. Helium at high purity can form the short-lived (1 m.sec.) metastable state and is then liable to form molecules which produce band spectra. These are likely to be interpreted as evidence of the presence of molecules such as

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hydrogen, oxygen, nitrogen etc. Argon shows an effect explicable only by wave-mechanical treatment which was discovered by Townsend in its low voltage manifestation. This is that its acceptability of an electron is very sharply "tuned" at c.12eV. The consequence is that a swarm of electrons in the presence of argon atoms soon reaches the state of being mono-energetic.

The same phenomenon was independently discovered by C. Ramsauer for energies above 20eV and subjected by him to very detailed practical and theoretical treatment[40]. Today the term "Townsend-Ramsauer effect" is used though Townsend himself might not have welcomed the union.

The one collaborator who stayed with Townsend until his premature death in 1941 was S.P. McCallum[41], who suffered greatly from the compromises which he was forced to make. However, one thing was certain: Townsend could be trusted never to read Nature. When McCallum and his research student, M.S. Wills, wanted to publish a paper in 1938 about the band spectrum of helium, it could safely be sent to that journal (22.7).

R. J. Van der Graaff (1901-1967) is the best-known of Townsend's research students. He was brought up as an engineer in the cotton-growing state of Alabama, but his ambitions led him to study physics at the Sorbonne, where he basked in the society of de Broglie. A Rhodes Scholarship to Queen's College, Oxford in 1925 gave him the chance of working with atomic particles. He was already thinking about high-voltage machines but had to content himself with thinking until, having taken his D.Phil. in 1928, he returned to K.T. Compton's laboratory at Princeton. There he built his first van der Graaff accelerator, working up to 600 kV. After the war HVEC was formed with him as Chief Scientist, with the object of making modestly-sized machines up to 2MV for scientific, medical and industrial uses.[42]

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The Final Controversy

Townsend had two sons, Edward and John, the latter having come up to New College in 1931 to read physics. Edward – also a New College man – went into the Royal Corps of Signals and rose to the rank of full Colonel. At the outbreak of the Second War he was Adjutant to the Brigadier Commandant at Catterick. Having become impressed with the superior performance in the Signals of recruits who had learnt some science at school, he arranged for an official approach to be made to his father whereby six-month courses in elementary electricity and magnetism, including the fundamentals of radio, for about 30 "Probationers" would be held in the Electrical Laboratory. Hebdomadal Council papers record that the first course started in October 1940 and was attended by 31 "Probationers" working to a scheme drawn up by Professor Townsend, who had "… carefully worked out the financial details with the Secretary of the Chest …" The second course (loc.cit.) was attended by 140 which must have given rise to accommodation difficulties.

As will appear in Part VI, the Clarendon Laboratory had meanwhile become a centre of research in radar (known at the time as RDF or radiolocation) and not unnaturally Oxford was an obvious place for the training of radar personnel. (Both the Clarendon Laboratory and the Electrical Laboratory were, of course, still nominally teaching the full three-year physics course, although physicists fit for service did a special course called Shortened Finals.) Townsend's laboratory and his staff were essential for the training of Service personnel and for what were called special radio courses – naturally for security reasons nothing could be said about their involvement with radar. Townsend himself thought that the work was concerned with sonar and that the new HT room basement had been filled

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[40] Similar experiments were carried out in Lindemann's laboratory by R.B. Brode on the three elements Hg, Cd* and Zn at voltages between 0.4 and 150V but without finding anything anomalous (22.10).

[41] Research Fellow of Hertford College, later Fellow of New College

[42] The author is grateful to Prof. F. Llewellyn Jones from whose lecture to the IEE on van der Graaff in 1985 this paragraph is drawn.

with water to that end. On 5 April 1941 the then Vice Chancellor[43] chaired a conference at which the Services' needs were discussed. A solution was arrived at which involved the Electrical Laboratory and Professor Sir John Townsend (he had been knighted in the New Year's Honours 1941) seemed to accept the proposals. However, on 10
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June 1941 two senior members of the Clarendon Laboratory and two of the Electrical Laboratory wrote to the Vice Chancellor saying (22.8) "We need not give you all the details (as you already know most of them) of how Professor Townsend has impeded these classes. It suffices to say that every obstacle has been placed in the way of the most important class – the special radio: the tuition of the RAF course has likewise suffered and we are compelled to state … that we have ample reason for believing that the teaching of the signals course, so far as it is given by the Professor himself, is almost entirely useless". (Confirmation as to the latter was available to one of the signatories by a son on one of the courses.) The letter concludes by mentioning that Oxford was uniquely able to make a great contribution to this particular side of the War effort and was making it, and says "… we shall regard it as a calamity if the University allows one man to render all this impossible".

Although the functions of the Visitatorial Board have changed over the years, it was then, and still is, responsible for dealing with recalcitrant professors and it was to them that Council sent the papers. Accordingly, on 20 June 1941 the Vice Chancellor, five Heads of Houses and the Proctors held the first of seven meetings, the last of which on 5 August went on from 5.00 pm until 9.15 pm. Nine witnesses were examined, the four senior members who had signed the original letter plus another, and four Departmental Demonstrators from the Electrical Laboratory; one technician and three now unidentifiable witnesses were called by Townsend. In spite of an initiative by the New College Law Fellow which might have led to a compromise, the Visitatorial Board found themselves obliged to pronounce "sentence of deprivation of Professorship". Townsend exercised his right of appeal to the Chancellor[44] but, before the result was known, Townsend resigned on 24 August 1942 with effect from 5 September 1941.

What poor Sir John thought about all this will probably never be known but it is pleasant to record that Lord Cherwell was a visitor to 55 Banbury Road on most Saturday afternoons and that in 1946 Townsend gave a lecture-
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demonstration in the Electrical Laboratory in which he repeated Hertz's experiments with modern radar equipment. He was 78 at the time and survived in active life for another 10 years.

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Chap. 22

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PART VI – THE SECOND WAR

Chap. 23 Tizard

The early years of radar in the UK were the close concern of the Oxford physical chemist, H.T. Tizard, whose later clashes with Lindemann are well known. Because his career is of interest and importance in itself and because of his complicatedly varying relationship with Lindemann, we shall treat his career in full.

Henry Thomas Tizard (Fig. 36)[45] (1885-1959, FRS 1926, KCB 1937, GCB 1949) was a notable son of Oxford science who had associations with Lindemann which veered from close friendship up to about 1920 to increasing dislike up to the middle '30s and acute hostility in the early war years. The importance of this relationship in the present context is that it contains the seeds of the spectacular part which the Clarendon Laboratory played in the development of microwave radar. We shall see that this was of decisive importance in the Battle of the Atlantic, though less so that is often supposed in the war in the air.

Tizard was born the middle child and only son of the five children of a Captain in the Royal Navy who became Assistant Hydrographer, with an engineering tradition on his mother's side. Westminster School, where he was a Queen's Scholar, curiously advised against entry for a closed scholarship at Trinity College, Cambridge, on the ground that there was a bias against the sciences. The young Tizard, therefore, applied to Magdalen College, Oxford, to read chemistry where he duly became a Demy (Scholar). There being no chemist on the strength at the time, Tizard was farmed out to N.V. Sidgwick of Lincoln College, famous for uniting the chemical concept of valency with the early ideas about the electronic constitution of atoms. (Sidgwick had added to his first in Natural Science in 1895 another two years later in Greats – classical and philosophical studies. This rare combination of ability
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reappeared a generation later in C.N. Hinshelwood OM, who was simultaneously President of the Royal Society and of the Classical Association.) In spite of illness during the week-long practical examinations, Tizard won his first and is next to be found in Nernst's laboratory in Berlin where he met Lindemann. His two years there were unfruitful in that he was unlucky in the topics assigned to him by Nernst but he was happy in his relationship with Lindemann – there was little to indicate the possibility of major friction in the future. After a year at the Royal Institution, he was offered a Fellowship by Oriel College, which together with some demonstrating in the Electrical Laboratory for Townsend, gave him a comfortable total salary. In the three years before the First War, Tizard published three papers on positive ions, (23.1, 23.2, 23.3) one with R.T. Lattey and two with Townsend and appears to have been none the worse for it.

On the outbreak of war, Tizard joined the army but a year later found him in the same ambience as Lindemann – work in the aeronautical field, involving instrumentation and including actual flying. Towards the end of the First War traditional hydrocarbon fuels

[45] As H.T.T. hissed to a Palace official when collecting his GCB in 1949, the highest award in the Civil Service, "It rhymes with gizzard".

were becoming scarce and, with D.R. Pye[46], he was an obvious choice to head a research team investigating alternative fuels with particular emphasis on their anti-knock properties. This work extended over 8 years and was Tizard's chief contribution to chemical engineering. His work lives on in the octane rating – the concept having started as the toluene rating was later converted by the US fuel technologists. (See also A.C.G. Egerton, p.[83].)

Although his work on aircraft fuels continued at Shoreham, Tizard was able to return to Oriel College and to chemistry lecturing early in 1919. Now with consultancies from the oil companies, he was prospering and was able to set up house with his wife and growing family. In February 1919 he was appointed Reader in Thermodynamics with effect from 1 October 1920.

Lindemann's application for the professorship was sent by him to Tizard for his scrutiny and transmission to the Registrar (23.4). They were thus still on the best of terms.
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Most people would feel that, as a Reader at the age of 35, they were doing well and could settle down for the next ten years, if not the next thirty. But Tizard was a restless character and when a tempting letter came two months later (April 1920) from a Civil Service friend, he was quick to pull up his roots and move to London. Much later he was able to ascribe this volte face to doubts about his long-term ability in pure science, lack of financial security, much of his income coming from consultancies, hankering for the applied science of his war-time years and – less familiarly today – the reputedly unhealthy Oxford climate. These notes encapsulate Tizard's nature – worries about such issues clouded his early life and restlessness persisted right through it.

The contribution which Tizard was almost uniquely able to make in the recently (1916) constituted Department of Scientific and Industrial Research was that of a first-rate scientist who had, during the war, learnt the art of communicating with service officers and he turned it to good effect here. By 1927 he had become Permanent Secretary – a remarkable feat at the age of forty three and with only eight years in the Civil Service. But once again anxiety about money overtook him and, when he was offered 36% more salary by Imperial College to become its Rector, he accepted – after all, he had by now three sons to educate. This second volte face was again not without its misgivings but Tizard came to see that to him an organization was at its most rewarding in its "nursery" stage. With bold practicality he pulled three separate bodies into the unity which we respect today, while increasing its prosperity and adding industrial economics to its curricula. In 1920 Tizard had become a useful, if unspectacular, member of the Aeronautical Research Committee of the Air Ministry and had himself become Chairman soon after. He was, therefore, an obvious candidate in 1934 to chair a new committee, officially the Committee for the Scientific Survey of Air Defence but more often known as the Tizard Committee, answerable to the Air Defence Research Sub-Committee (Chairman Lord Swinton) of the Committee of Imperial Defence.
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Before looking at the Tizard Committee which was, of course, the battleground for the first conflict between Tizard and Lindemann, we must note its place in the structure of the committees concerned with defence. First, it had two masters, one the Air Ministry and the other the more senior and wider, the C.I.D. Secondly, the parent bodies were mixtures of service chiefs, politicians and civil servants. Thirdly, the Tizard Committee were all

[46] Later Sir David Pye, FRS (1937) and finally Provost of University College, London

scientists. It will be seen later that it was this body that launched radar and – more to the point – got it into effective use in time to save us from losing the Battle of Britain in 1940.

At the time of its first meeting in January 1936 its members were H.T. Tizard – Chairman, P.M.S. Blackett – then Professor of Physics at Birkbeck College, London, renowned for his cosmic ray research, A.V. Hill – Foulerton Royal Society Professor of Physiology in University College, London and famous for his work in the biophysical field, H.E. Wimperis – a Cambridge engineering graduate with eleven years in the Civil Service and then Director of Scientific Research, Air Ministry and A.P. Roe, who at the time served as secretary of this and other science-orientated committees but later became Director of Bawdsey Research Station, the forerunner of TRE Malvern. (Finally, he was Vice-Chancellor of Adelaide University.) This body had held ten meetings in an atmosphere of electrifying excitement and had settled down into a harmonious group of mutually trusting people. Into this cosy nest a cuckoo descended from a great height. Lindemann had declared his interest in air defence in a letter to the Times in August 1934 and this interest naturally figured in many a discussion with Churchill. Lindemann, with Churchill's support, had been pressing in the last months of 1934 for a body of scientists not allied to any government department, with direct responsibility to the Cabinet. Only after the maladroit Prime Minister, Ramsay Macdonald, had become involved did it become clear that two processes were working towards nearly the same end and the obvious solution was reached – to put Lindemann on the Tizard Committee and he duly took his seat. (For the fascinating detail see Tizard by Ronald W. Clark (23.5).) This arrangement was doomed from the start – Lindemann wanted to serve on a committee with no
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allegiance to a department and the Tizard Committee was responsible to the Air Ministry. On the other hand, Lindemann, however gifted as a scientist, was known to have the ear of Winston Churchill who, at the time, had the reputation of a wayward troublemaker. When it came to items of agenda, Lindemann had his hobby-horses and rode them hard. The most notorious non-starter was aerial mines – an idea which must have seemed as wildly impracticable then as it does now, if only from the point of view of scale. How could one hope to concentrate enough of the suspension wires at a sufficient height at the appropriate moment? Lindemann undoubtedly wasted valuable time with this idea. More promising was his proposal to locate enemy bombers at night by picking up infra-red radiation if not from their engines then at least from the exhaust gases. This idea was tried out by R.V. Jones at Farnborough and found not to work. After a short period of further work, infra-red research was dropped.

It has been said that Lindemann was cool about or even antagonistic to radar. This is manifestly untrue and R.A. Watson-Watt confirms this explicitly and, as an instance, records Lindemann's having taken him to tea with Churchill to tell him about radar in June 1936. (This was very much the kind of activity which gave rise to friction on the Tizard Committee.) But Lindemann certainly voiced some uneasiness about radar, notably its susceptibility to jamming by the enemy. In the event, this was fortunately not much of a problem, although its use to cover the escape of the Scharnhorst et al in March 1942 was a sharp reminder of the essential correctness of Lindemann's caveat. A further cause for concern to him was what he saw as the slowness of the development of radar once the first rapid phase of 1935-6 was over. Any criticism of the Tizard Committee's doings was anathema to its Chairman and major trouble was bound to blow up sooner or later. The incandescent atmosphere was in no way lessened by Lindemann's committee technique. He might have taken the view that with the nation's security at stake he should close the

dampers and concentrate upon moderation, persuasiveness, toleration, patience, but he was unable to restrain his worst personal faults and before long his colleagues were united against him. In a
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letter to Lord Swinton of 12 June 1936 Tizard writes "I am really sorry to say that the gain would be greater than the loss. His querulousness when anybody differs from him, his inability to accept the views of the Committee as a whole and his consequent insistence on talking about matters which we think are relatively unimportant, and hence preventing us getting on with more important matters, make him an impossible colleague". The breaking point came when he suggested submitting a draft minority report in July 1936 to the (ADRC) Swinton Committee, in which he summarized his misgivings about the Tizard Committee's effectiveness. The result is well-known: after a tense meeting of 15 July, Tizard, Blackett and Hill resigned. The Secretary of State, Lord Swinton, dissolved the Committee and immediately reconstituted it without Lindemann.

The Tizard Committee struggled on until Churchill came to power in 1940. But it never recovered the vitality of its first months.

Lindemann spent two years in the wilderness so far as defence matters were concerned, making himself unpopular with a barrage of letters. It was a welcome surprise that, in November 1938, he was invited by the then Secretary of State for Air, Kingsley Wood, at Churchill's suggestion to sit on the Air Defence Research Committee. Tizard was still a member and the Secretary, Sir Thomas Inskip, thought it wise to let him know that Churchill had assured Kingsley Wood that Lindemann had given him "… an assurance … of what I may call good behaviour" (23.6). At the first meeting he attended an announcement was made which Lindemann backed up with an offer of help by the Clarendon Laboratory, leading to the development of centimetre wave radar, as will appear in the next chapter.

The chronology of the falling out of Tizard and Lindemann seems to have been as follows. According to R.V. Jones (23.7), Lindemann was exhibiting impatience with Tizard in the early 1920s. In 1936 Tizard invited Lindemann on to the DSIR Council. But in the early 1930s, before the trouble of the Tizard Committee started, Lindemann is said to have spoken of "… that insufferable little man" (23.8).
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Once Churchill had come to power in 1940, bringing Lindemann with him as his personal scientific adviser, Tizard's future was in question and his feelings towards Lindemann began to affect his judgement. Finally, in June 1940 after a meeting with the Air Ministry at which he had thrown doubt on the existence of guidance radio-beams set up by the enemy, he resigned. Curiously, only a few hours later these were confirmed to exist and to intersect over Derby, the headquarters of the Rolls Royce aero-engine works.

Committees having become a way of life even in universities, the fundamental reasons for the dislocation of the Tizard Committee by Lindemann are easier to pinpoint. A committee can work smoothly if all its members have a fair understanding of the various components of its business. With a body composed of specialists in distinct branches of one science, and yet more if it comprises specialists in different sciences, there is certain to be conflict unless there is a high degree of trust between the members. In the case of the Tizard Committee, this could never have been hoped for once Lindemann was a member – he had become accustomed to no opposition after seventeen years as a professor and must have had little confidence in commanding the understanding of a chemist, a biologist and an engineer, nor the support of a left-wing physicist from Cambridge.

It came as a relief to Tizard to accept the offer of his old college, Magdalen, to return in late 1942 as President. It was an unpropitious time to make a success of such a job and when, after the fall of the Churchill government in 1945, he returned to London in 1946, it was generally agreed that his four years as President had not advanced the cause of the scientist head of college. Indeed, it was to be 22 years before there was another such appointment leaving Dr (later Dame) Janet Vaughan (FRS 1979) in solitary splendour as the Principal of Somerville College. Then the dam broke and four more followed within eight years.

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J.H.E. Griffiths, President of Magdalen College 1968-1979

R.E. Richards (physical chemistry), FRS 1959, Warden of Merton College, 1969-1984

A.G. Ogston (biochemistry), FRS 1955, President of Trinity College, 1970-1978

A.H. Cooke, Warden of New College, 1976-1985[47]

It comes as a relief that, after the high drama of the years 1936-42, Tizard's diary reads under November 1946 "Cherwell to dinner", and to hear[48] that his last afternoon in Oxford was spent on a walk round Christ Church Meadow with his one-time adversary.

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Chap. 23

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23.6 Birkenhead, op.cit.

23.7 The Times, 6 April 1961

23.8 Birkenhead, op.cit.

[47] And two more: W. Hayes, President of St John's College and J Moffatt, Provost of the Queen's College

[48] from Dr A. von Engel 1982

There is a story of unknown date about someone's saying to Lord Cherwell, "I hear that you and Tizard have buried the hatchet". There was a thin smile with the reply "Not very deep".

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PART VI

Chap. 24 Radar

Lindemann is said by Birkenhead to have speculated in the early 20s (24.1) about using radio waves for the detection, measurement of distance and possibly also position of distant objects, eg. ships and aircraft. The first experiments using such methods (in the world) were those of E.V. Appleton (1892-1965, FRS 1927) and M.A.F. Barnett (24.2) in 1924. Appleton persuaded the British Broadcasting Company (sic) to apply frequency modulation at 0.033 to 0.2 Hz to their 386 metre transmitter at Bournemouth after programmes had finished. In Townsend's Electrical Laboratory at Oxford (by arrangement with E.W.B. Gill) he picked up the direct signal and timed the beats with the roughly equal signal from the E (later Heaviside) ionised layer. The receiver at Oxford had two triode amplifying stages with four tuned circuits, using resistance wire for the inductances to provide a sufficiently wide frequency response. The height of the E layer was found to be variable at about 100 kilometres. Soon after, G. Breit and M.A. Tuve in the US used a pulse technique as later applied to radar and Appleton subsequently followed suit.

Ground-based Radar

The history of radar – a term imported in 1944 – goes back to a very well-worked-out UK patent (24.3) of 1928 which got no further. Having achieved the modest success of a reflection from a sheet of galvanized iron at 100 yards, Alder abandoned his experiments. Meanwhile, the War Office were doggedly pursuing acoustic methods for detecting aircraft, running to 200-foot concrete mirrors.

Curiously, it was a nonsense enquiry to R.A. Watson-Watt, then working at the Radio Research Station of the NPL site at Slough, about the possibility of a "death ray" which started him off on radar. H.E. Wimperis, the Air Ministry member of the so-called Tizard Committee (see p.[138-142]) had made the enquiry and Watson-Watt passed it on to an assistant, A.F. Wilkins, who in half
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an hour confirmed the common-sense view about death rays – particularly with the relatively feeble RF sources of the day – that gross damage to a distant object was not feasible. But Wilkins went on to calculate the signal strength of a radio pulse reflected by a distant aircraft and this looked surprisingly promising. Watson-Watt was equally surprised and there was some desperate searching for order of magnitude mistakes. He sent the paper to Wimperis, begging him not to get too excited, but the calculations proved to be right and Watson-Watt was able to submit a detailed confirmatory memorandum on 12 February 1935. Following a successful demonstration on 26 February using a 50 metre BBC transmitter at Daventry and an aircraft eight miles away, the Tizard Committee gave Watson-Watt the go-ahead and, incredibly, he had a prototype of the Home Chain radar station working at Orford Ness by 15 June to a distance of 17 miles and in July to 40 miles. By the end of the year the Treasury had sanctioned five similar stations down the east coast. The full chain of 20 with its communications system was operational in the spring of 1939, extending from the Orkneys and the Firth of Tay to the Isle of Wight. These enabled the RAF to concentrate their slender numbers of fighter aircraft, with the result that we won the

Battle of Britain in 1940. The transmitters used 250 kilowatt Metropolitan-Vickers continuously-pumped tetrodes and the horizontally disposed dipole aerials were supported on 350 foot towers. The wavelength was variable between 6 and 12 meters to overcome possible jamming by the enemy. These radar sets had a range of at least 100 miles in poor conditions and sometimes up to 150 miles. They were able to give range, direction, height and an adequate indication of numbers. By the outbreak of war, an identifying signal could be impressed on the return signal by our own aircraft.

However, effective as the Home Chain continued to be throughout the war, an urgent need became evident for a parallel chain of radar sets able to see ships and low-flying aircraft at shorter range and the CDU (Coastal Defence Units) and CHL (Chain Home Low) sets, working at 1.4 metres, were rushed into use.

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> D. Roaf recalls setting one up two miles from Craill near Dundee with J.H.E. Griffiths and B.V. Rollin. They had no handbook to work from and were linked to Fighter Command only via the PO telephone exchange at Fort Douglas. Diagnostic techniques were crude – Roaf remembers Rollin clambering about the aerial looking for standing waves with a neon tube. But they got their radar set working and were able to confound Fighter Command with reports of aircraft which showed no lateral motion – they were aircraft carrier fighters practising take-off and landing.

Members of the Clarendon Laboratory were only marginally involved with ground-based radar, although it was an Oxford physicist who mistakenly set off the sirens which were audible through Neville Chamberlain's broadcast announcement that war had broken out. The Cavendish Laboratory staff had been allocated to the stations up and down the south and east coasts - a deal struck between Tizard and J.D. Cockcroft at luncheon in the Athenaeum early in 1938. What might have been construed as a snub to Oxford turned out to be a blessing. We were able to play a decisive part in the development of centimetric radar.

<u>Early air-borne radar</u>

People laughed at Watson-Watt when he forecast in 1936 that it would not be long before radar sets were fitted in aircraft. But by 1939 the first AI (Air Interception) radar sets working on 1.5 metres were installed and, although it took an operator of D.A. Jackson's class to get the best out of them, it was a start. The reflection from the ground – unavoidable with virtually non-directional aerials – was their principal enemy and, in spite of many modifications, they had their limitations. Such sets were also used in the Royal Navy when they were called ASV (Air to Surface Vessel) but the lack of directionality was compounded by reflections from waves.

Preparations for microwave radar

It would have been obvious to Lindemann that a reduction of the working wavelength to about 10 centimetres would bring the decisive advantage that a narrow beam could be used, resulting in both the concentration of transmitter power and the elimination of unwanted return echoes. There would also be a reduction in the minimum size of reflecting objects and there was always the possibility that submarine periscopes might be picked up.

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The launching of a programme resulting in the sinking of a U-boat in April 1942 by the use of a 10 centimetre sea-borne radar set (24.4) was, most fortunately for the country, pushed through in the nick of time by two unusual Civil Servants. Sir Frank Smith (1876-1970, FRS 1918) was a physicist from RCS* who, from 1911 to 1919, had headed the electrical division of the NPL, when he succeeded Tizard as Secretary of DSIR. In the Minutes of a meeting of the Air Defence Research Sub-Committee of the Committee for Imperial Defence on 20 December 1938 we read[49]

"The Sub-Committee agreed

a) To take note that Sir Frank Smith had taken the necessary steps with the Radio Research Board of the DSIR for the construction early in the New Year of experimental valves for use with very short waves."

This meeting happened to be the one referred to earlier (p.[141]) at which Lindemann made his return after two years. It must have been deeply satisfying to him, especially with Tizard present, that the Minute continues:

b) to avail itself of Professor Lindemann's offer to conduct research at the Clarendon Laboratory, Oxford, into valves for use with very short waves."

(These minutes are marked "TO BE KEPT UNDER LOCK AND KEY".)

This offer was taken up when C.S. Wright[50], Director of Scientific Research, Admiralty, and Vice-Admiral J. F. Sommerville[51] visited the Clarendon Laboratory in 1939. Meanwhile, Sir Frederick Brundrett (1894-1974, KCB 1956) was negotiating arrangements with the General Electric Company at Wembley and the Universities of Birmingham and Bristol.

Sir Frank Smith had been able in the teeth of Air Ministry opposition to achieve a long cherished ambition – a cooperative body on which all three services sat, the Coordination of Valve Development Committee. (When it came to electronic components, three quite distinct organizations – one for each Service – remained and the trouble this caused reminded people of the country's great fortune that valves at least had been rationalized in time.)

Sir Frederick Brundrett had spent nearly twenty years at the Admiralty Signal Establishment, Portsmouth. In 1937 he was moved to London where one of

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his jobs was to be Chairman of the CVD committee. Early in 1939 he went to see Lindemann and, no doubt, plans were made to enlist the support of the Clarendon Laboratory when the time arrived.

<u>Radar in the Second War</u>*

On the outbreak of war in September 1939, research in the Clarendon Laboratory was stopped and a group was set up under T.C. Keeley who were allowed to do what they liked in the field of short-wave radio to familiarize themselves with the tasks that would be set them when work for the Admiralty started. Some heroic projects saw the light of day including a generator of a rich mixture of high frequencies up to 1mm wavelength in which a beam of electrons was fired through an assembly of 1/64 inch ball bearings in oil. There was also a free-electron device conceived like a siren which delivered 3-6 watts at 6 centimetres. But for the breakthrough with the magnetron at Birmingham it might have had a future.

However, the most notable achievement was B.V. Rollin's invention of the* reflex klystron. The two-resonator klystron developed in the US used a stream of electrons passing from one resonator to the other. Rollin's breakthrough was to make the electrons turn through 360 degrees and with negligible loss of coherence cause an in-phase resonance in the single cavity at about 10-centimetres. (The reflection could be caused either by

[49] Martin Gilbert, Winston Churchill Companion Vol.3, 1936-39
[50] Sir Charles Wright, KCB 1946
[51] Admiral of the Fleet, Sir James Sommerville, 1945, GCB 1944

secondary emission from a positively-charged electrode or the same electrons could be required to make the return journey by a negative charge on it.) This work was completed by the end of 1939 – before the Clarendon Laboratory was officially working for the Admiralty (see Fig. 37 in which B.V. Rollin's own sketch is shown, from an internal report). Rollin's klystron had to be continuously pumped but it was eminently suited to development into a permanently pumped-out plugged-in component. This work was carried out by R.W. Sutton at Bristol, who was an expert in copper/glass seal techniques. Apart from the obvious mechanical advantages, this single resonator klystron showed greater tunability* than its two-resonator predecessors.

The astounding success of the first cavity magnetron designed at Birmingham University by J.T. Randall and H.A.H. Boot (24.4) is well known and R.A. Watson Watt (24.5) gives a particularly exciting account of it. It is not
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often that a new device shows an improvement in performance of two orders of magnitude. However, one must remember that the cavity magnetron was conceived from the first as a pulse device - originally about about 10^3 pulses per second lasting 10^{-6} [seconds] each.)

These two inventions very much put in the shade M.L.E. Oliphant's[52] bigger and better pumped klystrons which he claimed could be scaled up ad lib – provided that you could accept the trailer-load of machinery which they needed.

Early in the spring of 1940, a few weeks after the Birmingham magnetron had first worked, a car-load from Oxford went over to see it. C.H. Collie remembers saying on the way home, "They've got the transmitter. We've got the local oscillator. All we have to do now is build the rest of the receiver".

The solution of the detector problem proved to be in principle a return to the crystal and cat's whisker of 20 years earlier using silicon – in those days not of very high purity – and a tungsten wire. A great deal of work on purification was carried out – see for example Crowther and Whiddington (24.6) with the Clarendon Laboratory playing an essential role in the measurement of electrical properties (24.7).

A crucial demonstration of a complete 10 centimetre radar set was mounted at Swanage in the summer of 1940. The results were so impressive – e.g. a submarine conning tower at 4 miles – that the decision was made without delay to form a manufacturing team under S.E.A. Landale at the Admiralty Signal Establishment. The aerial system of this first 10 centimetre wave radar set, type 271, appears in Fig. 33. It will be seen that two aerials were used, no disadvantage for marine use but further development was needed before microwave radar could become airborne.

The problem of working with a single aerial was solved in the Clarendon Laboratory by A.H. Cooke (24.8). What was needed was a T/R (Transmit/Receive) cell which, while insulating the receiver from the transmitter while the latter was supplying power to the aerial, would conduct the return pulse to the receiver. Many gases at various pressures were tried. In the end the ideal substance proved to be water vapour.
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The War at Sea*

The story of the war at sea illustrating the outstanding contribution to victory of the early use of centimetre radar runs as follows. 1.5 metre radar, AI (air interception) to the RAF and ASV (air to surface vessel) to the Royal Navy, had limited success until Jun 1942 when the Leigh searchlight was introduced, but by the early winter of 1942 U-boats had

[52] B. 1901, FRS 1937, graduated Adelaide 1926, 1927-37 Cavendish Laboratory, Professor Birmingham 1937-50, 1950-67 appointments in Canberra, 1971 Governor of South Australia.

equipment which enabled them to hear the radar transmissions of approaching ASV sets, giving them plenty of time to dive. However, as type 271 10-centimetre radar sets came into more widespread use – 25 had been fitted by July 1941 with another 350 on the way – shipping losses decreased until by August 1943 they were down by a factor of 10. A major contribution to the success of microwave radar was that the enemy could not hear us approaching and indeed, when deceived by a captured pilot into believing that we could hear their receivers, they wasted valuable time modifying them.

There was reluctance to allow 10-centimetre equipment into the air, especially over the Continent, because the copper anode block of the magnetron could not be sufficiently damaged by the usual explosive charges should the plane be brought down. But this ban was lifted in 1943, when the aircraft of Coastal Command were able to contribute decisively to U-boat sinkings.

The final salute to the 10-centimetre magnetron came in an unusually accurate statement from Adolf Hitler. In a speech at Weimar in 1943, he attributed his country's "setbacks" in the Atlantic to a single British technological invention. Admiral Doenitz went further: "It was not superior strategy or tactics which gave him (ie. us) success in the U-boat war, but superiority in scientific research".

Magnetrons – 3-Centimetre and Below

Another major achievement of Oxford radar research was the development of the cavity magnetron for wave-lengths less than 10-centimetres. It had been found that there were difficulties in scaling down the very successful Randall and Boot design. This was partly due to the gap of only 1mm required between cathode and anode but also because of fabrication difficulties. Rollin (Fig. [38]) was successful with a new design in which the cavities are slots cut by
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broaching. Correct operation was achieved by Rollin with pieces of dielectric at intervals round the anode, and at MIT by staggering the radial length of the teeth of the anode. The appearance gave rise to the description "rising-sun". Successful magnetrons were produced for wave-lengths down to 1.25 centimetres.

But the Germans had a solution to the night-time surfacing problem – the "schnorkel". This was nothing but a vertical extension of the exhaust pipe which projected some two feet above the surface of the sea, making a better radar target than the periscope 1 to 3 feet high[53] but not very much. By this time, 1944, we were ready with a 3-centimetre radar set. In good conditions this could pick up a schnorkel but it also picked up flotsam and the enemy were able to detect it under the best conditions. This radar set was an all-Oxford production.

There is an interesting letter from Lord Cherwell (24.7) to Sir John Anderson[54], one of the very few scientifically educated politicians of those days. This is how he summarizes Oxford's achievements in radar in 1943:

> "As arranged, I am sending you the names and other particulars of the scientists employed on Admiralty work at the Clarendon Laboratory (no copy survives) whose salaries are in my view inadequate.

Note of the work done:

> "The Research Group at the Clarendon Laboratory has been very successful. Not only has it sound pieces of academic research like the absorption by rain of millimetre waves and the absolute measurement of receiver sensitivity to its credit

[53] Acknowledgements to Capt. H.M.C. Ionides, RN Retd
[54] Later First Viscount Waverley, cr. 1952 (see p.[103])

but nearly all the devices essential to centimetre RDF were first thought of or shown to work at the Clarendon Laboratory. Thus, this group was the first to

a) show that single resonator tubes could yield useful powers thus enabling ASE to develop the very successful NR99,

b) show that the heterodyne techniques could be used at centimetre wavelengths and had approximately the theoretical sensitivity,

c) show that water vapour discharge tubes could be used for common T and R and work out the conditions for application,

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d) show that the harmonic type of resonator (proposed by EMI) could in fact be maintained in oscillation thus making 3-centimetre heterodyne reception possible.

e) develop the whole of the transmission and reception valves for 1.5 centimetre.

The standard of pay bears very hardly on them and greatly increases the temptation to drift into better paid administrative positions as one of them[55] has already done".

The Clarendon Laboratory continued its development work up to and, in some cases past, the end of the War. The 1.25 centimetre radar sets turned out to be of no practical use because there is a strong absorption band by water vapour at this wavelength. A feature of the work was the "institution" of "pre-production" runs for radar valves of various types in which a few hundred of any one type might be made for use by development laboratories here and in the US.

Summary

1. Early application to microwave radar came as a result of Lindemann's advocacy.

2. Birmingham and Oxford made the crucial discoveries and solved many of the peripheral problems, which results in the first U-boat kill attributable to centimetre radar in April 1942.

3. The main reason for the subsequent success of 10-centimetre radar in tracking down U-boats was due to the fact that they could not hear them coming, unlike the earlier 1.5 metre sets.

4. Centimetre radar was accepted by the Germans as well as by us as the key factor in our winning the Battle of the Atlantic.

5. In the air centimetre radar was not regarded as worth its weight[56]. Oboe and G* were much more accurate when it came to navigation.

Much of the above is derived from Crowther and Whiddington, Science at War, and Hartcup, The Challenge of War (24.10).

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PART VI

Chap.25 Tube Alloys

The team working on radar on the north side of the Clarendon Laboratory knew that work of some kind was going on under Dr Simon on the south side – a group of physicists under N.Kurti and H.G. Kuhn – but had no idea what it was (K. Mendelssohn was working on medical physics). Everybody else, including the undergraduates, were kept out of the north side by little wooden barriers with naval pensioners scrutinising passes and had to make their way along the south side, where they too peered in, wondering why these people were not also doing something important. Only after the uranium and plutonium bombs were dropped in August 1945 did the real business of these people come out. Naturally, it was a big joke that this very highly secret work had been carried out by aliens or one-time aliens who could not be put on the highly secret radar work.

The part played by Oxford in the formidable procedure of separating the uranium isotopes (Tube Alloys – a cover name) has been very thoroughly dealt with by Professor Margaret Gowing (25.1). It seems unreal after the events of forty years ago that a group of academic scientists in a university laboratory should have undertaken preliminary work on what was even at the time so clearly a huge industrial undertaking directed towards decisive military purposes. In his book Alsos (25.2), S.A. Goudsmit recounts the activities of the scientific mission which followed hard on the heels of the US Army in September 1944 as it pushed inexorably into Germany. It was looking for evidence of industrial installations for isotope separation, or at least proto-types. It came as a total surprise that there was no trace of any. There had been reports of interest in heavy water and uranium ore, of feverish research work and orders for suitable machinery for a diffusion plant (25.3) and there was no reason for disbelief in 1940. Great Britain was on her own then
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and the possibility of a German victory very real. No government believing that the enemy might be preparing for nuclear war could possibly have neglected to make similar preparations.

But Pearl Harbour altered the position and the mammoth Manhattan project started up in the USA. The fruits of the work in the Clarendon Laboratory were, of course, available to our new allies but an unusual degree of chauvinism prevailed, to say nothing of a sharp eye on future commercial interests (25.4).

One of the Tube Alloys team was R. Berman who, in 1946, put in a thesis for the D.Phil. This was lodged in the Bodleian Library, according to the normal practice and, as is sometimes the way with theses, its issue to readers was at the discretion of the author who, in the case of secret work, could be relied upon to refuse. These arrangements had worked perfectly for nearly forty years when one heard strange tales of armed toughs in armoured vans, who bore Berman's thesis away to where it could be guarded twenty-four hours a day. It is not everybody whose work gets more secret as time goes on.

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PART VI

Chap. 26 Under-sea Weapons and "Window"

Jackson – in the teeth of opposition from Lindemann – joined the RAF in 1940 where his abilities were well used throughout the Second War, particularly in the use of conducting strips for confusing enemy radar ("WINDOW"). This and other activities are fully documented in H.G. Kuhn's Royal Society obituary (26.1).

M.A. Grace (Fig. [40]) appears later as one of the nuclear physics team working on the famous nuclear orientation experiment of the early '50s. In 1940, at the age of 20, he was picked out in a recruiting drive conducted by Admiral Sommerville (see p.[148]) and sent to HMS Vernon, the shore establishment in Portsmouth concerned with mines. For the first year he was developing the calculations involved in the translation of magnetic measurements on ships; into the remedial action known as "degaussing". It soon became routine and so Grace moved on to a research team headed by Francis Crick, subsequently renowned for the elucidation of the genetic coding of DNA. The work was on acoustic mines of a sophistication not generally known. It was to be of importance during the Normandy landings that the allies should be able to lay mines outside the Seine estuary which would bottle up the motor torpedo boats (E-boats) but not be set off too easily by minesweepers. Clearly an acoustic mine was needed which was insensitive to low frequencies but sensitive to the higher frequencies put out by internal combustion engines. There may have been a temptation to do it electronically but this would have meant valves and their power supply. A much better solution was worked out involving a reed tuned to a frequency in the kiloHz region. Even better, a differentiating RC circuit was included so that the mine exploded only when the signal had reached a maximum and was beginning to fall off again – you were then sure of the mine going off under the centre of the boat. Another type was designed to wait until several ships had passed over before exploding in the hope that the minesweepers had done their work
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and that the next ship was a more desirable target. As in the case of "pre-production" batches of klystrons from the Clarendon Laboratory, so were special mines manufactured by the hundred in HMS Vernon.

The Prof's Ennoblement

It is, of course, a well-established fact familiar to everybody with any historical knowledge of the Second War, that Lindemann was more than Winston Churchill's scientific adviser – he was a close and continuous confidant. What there was in the natures of the two men to account for this rapport, born as it was of nearly twenty years of frequent domestic visits, will probably never be wholly understood. That it was important in Churchill's life at the time is reflected in an anecdote of the writer's father. A conference with certain of the generals had run into the small hours. Eventually they spilled out roaring "The old man's impossible. For God's sake send the Prof in". Lindemann was found and sent in. When the conference resumed, all was smiles and accord. Somehow or other Lindemann had the secret of relaxing Churchill and perhaps the historians of the future will be able to tell us the contribution this made to the conduct of the war.

This same value placed on the company of the supposedly haughty and frosty Lindemann was to be noted in Christ Church Common Room when the writer became a member in the early '50s. Lord Cherwell, as he had become (see below), rarely ate dinner but used to appear in the Common Room afterwards. As soon as the bowler hat and stick were laid to rest, the company seated round the table would at once make room for him – not reluctantly in one place as was sometimes the case with other people but eagerly in many places. Everybody wanted to be next to him or at least within earshot.

When the Second War started in September 1939 Winston Churchill returned to the office of First Lord of the Admiralty[57] and Lindemann became his scientific adviser straight away. When the Chamberlain government fell in early 1940 and Churchill took charge, he needed Lindemann in the Cabinet and,
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as a seat in the Commons was out of the question, Lindemann was made a baron and chose the name of the river close to his laboratory. The serious state of affairs in the country did not stem the flood of jokes – notably by the chemists – about other possible titles such as Lord Christ of Church but it was his colleague, H.R. Trevor-Roper[58] who produced

Easter Hymn 1941

Lord Cherwell when the war began
Was plain Professor Lindemann.
Now Christ Church men with one accord
Salute their newly-risen Lord.

Churchill was to recall Lord Cherwell to his 1951 government for which purpose the University were happy to agree to two years' leave of absence but they resolutely refused an extension. In 1956 Lindemann became First Viscount Cherwell, CH.

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[57] Previously held by him in 1911-1915
[58] Lord Dacre, 1979, Master of Peterhouse College, Cambridge 1980

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PART VII – OXFORD PHYSICS AFTER THE SECOND WAR

Chap.27 F.E. Simon (1946-1956)

The end of the Second War found F.E. Simon with a greatly increased scientific reputation and with more influence in public affairs – for instance, he became Scientific Correspondent of the Financial Times in 1948 and contributed several articles himself. He had become FRS in 1941, was awarded the Rumford Medal in 1948 and was a member of the Council of the Royal Society from 1948 to 1950. He was awarded the CBE in the first post-war Honours List of 1946 and was to become Sir Francis Simon in 1956. ("I accept it only because it will please my wife, you understand".) If he was quick to accept invitations to join committees it was because he could see the positive advantages to his interests. A member of a committee of the late '40s to promote cooperation with AERE Harwell observed[59], "Do you know what Simon means by cooperation? Sitting at a table and going away with £5,000 for the Clarendon".

The major achievements of Simon's low-temperature group were the first positive nuclear orientation experiments of 1952 and the realization of microdegree nuclear spin temperatures of 1956. Other new lines were the measurements of dielectric thermal conductivities by R. Berman[60], from which it became possible to see for the first time the relative contributions of the two effects, the so-called Umklapp Process predicted by R.E. Peierls (27.1) and the other discovered at Leiden in the '30s (27.2). It was also with Berman that Simon was able to determine the equilibrium conditions for the synthesis of industrial diamonds.

This work was naturally of great interest to the industrial diamond synthesis industry and Simon was able to persuade de Beers to support his work, including the Diamond Conferences which have been held for the last 35 years successively in Oxford, Cambridge, Bristol and Reading.

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Simon's principal public crusade was against the waste of energy. This he carried over the widest possible spectrum – from the heat that goes up the chimney from the domestic grate or stove to the same phenomenon in power stations. But, of course, there was the subtler thermodynamic waste when electrical energy is squandered by using it to produce heat in a resistive heater such as a domestic electric fire, rather than using it to power a heat pump in which several times more heat is produced for the same electrical energy consumption.

One of Simon's most interesting public initiatives was the foundation in 1954 of the National Industrial Fuel Efficiency Service which by lectures, publications, conferences and the like brought the engineering realities of fuel efficiency right to the door of the many offenders. NIFES was an early candidate for privatization and in 1971 became a consultancy with headquarters in Altrincham. At the time of writing it has a staff of 200 spread among eight offices.

Nuclear Orientation

[59] Nancy Arms, A Prophet in Two Countries, Pergamon 1966

[60] Fellow of University College 1955-1984

It had been suggested by C.J. Gorter (27.3) in 1934 and by Kurti and Simon (27.4) in the following year that it might be possible to orientate certain nuclei and enable the nuclear physicists (27.5) to perform new experiments and, in particular, on radioactive nuclei. Further, it should provide a means of exploring still lower temperatures. However, it was all too evident that there would have to be a breakthrough in the production of much higher magnetic fields in the laboratory – 1 tesla (10^4gauss) was not out of the way but fields even five times stronger called for major engineering installations. The field required for a single-stage nuclear demagnetization might be around 100 tesla, which is still unattainable today by a factor of about 5. What became known as the "brute force" approach clearly had to give way to something subtler.

J.A. Spiers (see p.[181]) had set out the nuclear physics case for attempting nuclear orientation and it was not long before the reality was being planned at Leiden and at Oxford. Bleaney (27.6) put forward three ways of using the fields in solids to produce what was required internally.

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1. Pound (27.7) had proposed a procedure which relies upon electric fields and which would produce alignment though not polarization of the nuclei, the distinction being that in the latter case all spins would point the same way while in the former they would point either one way or the other. In any case, this procedure was not of interest because it did not involve a paramagnetic salt.
2. Gorter (27.8) and, independently, Rose (27.9) proposed the use of a paramagnetic salt in which the internal magnetic fields of the electrons and the nuclei would be mutually lined up by the large internal magnetic field of 10 to 100 tesla and, if a moderate field were left on after demagnetization, this would keep both sets of spins polarized.
3. Bleaney's (27.10) method was not dissimilar but included the use of one of the highly anisotropically paramagnetic salts discovered by Cooke et al (27.11)* such as the double sulphate of copper and rubidium, diluted with the diamagnetic zinc rubidium sulphate, with the addition of a small amount of the radioactive cobalt-60. Using this one can demagnetize to zero field and thus reach a lower temperature.

Leiden and Oxford pursued their different paths. Gorter had been able to publish only an inconclusive result in 1949 (27.12). A self-contained liquid helium system was built by E. Ambler in one of the new Simon Building rooms. The magnetic field was provided by a solenoid of French manufacture running from the 2MW generator and giving a field of 4 tesla. Geiger-Muller counters were used to detect gamma rays axially and equatorially. In a later paper (27.13) B.Bleaney, J.M. Daniels, M.A. Grace, H. Halban, N. Kurti, F.N.H. Robinson and F.E. Simon report an experiment in which there was an anisotropy at demagnetization of 1:3.2 falling to 1:0.04* after nine minutes. That these were experiments of value to the nuclear physicist is shown by the fact that under N.J. Stone they are still in progress after more than thirty years, although the helium dilution refrigerator has done away with the need for the difficult and time-consuming demagnetization process.

This experiment caused a considerable stir at the time but, although its outcome was of interest, it was not unexpected. But when a similar experiment

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was carried out at the National Bureau of Standards in 1957 (27.14) in which beta as well as gamma rays were detected, the result shook the physical world to its foundations. The concept that right-or-left-handedness is not preferred in nature had become so much a part of nuclear physics that it had the status of a law. Lee and Yang in 1956 wrote (27.15) "…

for the weak interactions … parity conservation is so far only an extrapolated hypothesis unsupported by experimental evidence". The implications of the Salam-Weinberg view of the connection between this theory and electro-magnetic forces is exercising theorists and experimentalists, including the Sandars group in the Clarendon Laboratory, up to the present time.

Nuclear Cooling

Having seen through the successful nuclear orientation experiments just described, Kurti and his collaborators turned in 1953 to the achievement of nuclear spin temperatures in the micro-degree region by a two-stage demagnetization process using the magnet specially designed by Daniels (27.16) (see p.[171]). The design of their experiment bypassed two fundamental problems in a boldly simple way. There needs to be a thermal link between the two stages (which were 23cms apart) and the problem of the avoidance of heating due to eddy currents they solved by making the link of 1540 insulated copper wires of 40 SWG (0.12mm). The serious difficulty of heat contact between this link and the nuclear specimen they sidestepped by making the specimen of the same wires as the heat link folded back upon themselves four times to produce a mass 7cm long. This leaves the problem of heat contact between the copper wires and the electronic demagnetization specimen of chromic potassium alum at the top. Robinson (27.17) discovered that a slurry formed of the alum with water and glycerol set to a glassy solid not far below room temperature which provided ideal thermal contact between the alum and the copper wires. The paper of Kurti, Robinson, Simon and Spohr (27.18) describes the steps taken to secure low heat-leak and the experiments made to prove their effectiveness and the procedure which led to measurements of nuclear spin temperatures of 2-3 microdegrees K, the two stages reaching equilibrium in about 2 minutes. What, it was asked, was this 2-3 x 10^{-6}K the temperature of? All that could be said

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was that it was the temperature of the nuclear spins of the copper specimen and that the time required for thermal equilibrium with the lattice was likely to be several orders of magnitude longer than the time for which these low temperatures persisted.

Low-temperature Physics

After the Second War, K. Mendelssohn had to convert his laboratory space in the Clarendon Laboratory from medical physics to low temperature physics. He was lucky in getting D.K.C. MacDonald (1920-1963, FRS 1960) to join him in 1946. MacDonald was a Scot of sometimes overpowering vitality and with high levels of ability both in mathematics and practical physics. He came with a doctorate from Edinburgh and at once signed up for another one in Oxford. His main interest was in metals and his aim was to develop the understanding of them by measuring their electrical resistance from 4K up to room temperature. With a Simon liquefier in which specimens could be changed until the liquid helium ran out, he made measurements on the alkali metals, their magneto resistance and their size effect. He was also interested in the sodium-ammonia solutions which were, at the time, reputed to show superconductivity at liquid air temperature. While pressing through this programme of experimental work, MacDonald was writing about fluctuations and noise. In 1951 despairing of a permanent job in Oxford, he moved to the National Research Council in Ottawa to start up a new department.

Mendelssohn revived his work on super-fluidity with J.B. Brown, G.K. White (see also p.[226]) and B.S. Chandrasekhar, D.F. Brewer, R. Bowers and D.O. Edwards, all of whom have made a name for themselves away from Oxford, as has J.L. Olsen who, in parallel with MacDonald's resistance measurements, set up a thermal conductivity cryostat on which he made a series of measurements of lead-bismuth alloys. This work was continued by H.M. Rosenberg, who has stayed in Oxford. Successors who have made their mark include H. Montgomery and N.G. McCrum.

By the middle of the '50s, Mendelssohn was beginning to dilute his interest in physics with journalism and authorship and became an enthusiastic and wide-ranging traveller. (See particularly his excellent biography of Nernst, entitled The Rise and Fall of German Science (27.19).)

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Some Un-done Low Temperature Experiments

In the course of a taped conversation in 1983 (27.20), B.S. Chandrasekhar[61] discusses the notable absence of the Clarendon Laboratory from four keenly pursued topics in low temperature physics of the 20 years 1955 to 1975, and tries to draw morals about the virtues of closer contact between theoreticians and experimentalists.

Chandrasekhar points to the Bardeen, Cooper and Schrieffer theory of super conductivity of 1957 as one of the most comprehensibly written papers (27.21) within living memory which also explained all the mysterious phenomena shown by superconductors. There was at once a near-universal rush of experiments in an atmosphere of "crackling excitement" to test the new theory – Illinois, Rutgers, Westinghouse, Cleveland, Grenoble, California, Cambridge, Moscow, Leiden, but not Oxford.

Again, in 1961 the discovery by Kunzler et al (27.22) of the behaviour of niobium-tin with its astonishingly high transition temperature (18K) and threshold current of 10^5 amps cm^{-2} at 8 tesla. This was for a short sample of the alloy and, although drawing this into a mile of wire was obviously going to involve metallurgical problems, these need not have taxed the resources of W. Hume-Rothery's laboratory unduly. An experiment on such a wire would have been well within the Clarendon's scope.

Similarly, when the Josephson Effect (27.23) was predicted at the Cavendish in the mid 1960s, another flurry of experimental activity opened up in which Oxford did not figure.

Finally, Chandrasekhar notes the experimental observation that liquid He-3 undergoes a phase change at about 2mK by Osheroff and others (27.24) at Cornell. The theoreticians were on to this straight away and had it wrapped up in a matter of months. "Rewriting history" says Chandrasekhar "it could just as well have been done in Oxford."

It is, of course, true that the people in Oxford who could have

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contributed to these four debates were all doing something else and, after all, a phenomenon that turns on one physicist will not necessarily turn on another. But Chandrasekhar draws attention to the spectacular collaboration between experimenters and theoreticians in the Clarendon that, in the late '40s and in the '50s, put microwave spectroscopy so definitively on its feet. He cannot help wondering whether the very close association of theoreticians and experimentalists found in many laboratories in the US might not have resulted in more participation by the Clarendon Laboratory in some of the causes celebres of low temperature physics in the two post war decades.

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PART VII

Chap. 28 Magnets

The magnets of the pre-war days remained useful for various purposes but it was the case that nothing would go above 1 tesla and that only for a limited time. After the War, we have seen* that A.H. Cooke, with the help of the Royal Society, was able to buy an iron-cored magnet wound with water-cooled copper tubes, capable of a steady field of 1.5 tesla in a gap of 5cm diameter and 5cm wide. With a total weight of 2 tons, the original idea of serving several rooms proved unrealistic and, in the event, it was used in only two inter-communicating rooms. The ironwork of the magnet of similar design from before the War used by A.H. Cooke was fitted with new coils, wound from copper strip insulated by a nylon mono-filament and wound into pancakes by R.P. Hudson (28.1). This magnet achieved its design figure of 2 tesla with coned pole-pieces and a 3 centimetre gap or 1.2 tesla with flat pole faces and 5 centimetre gap but, like other metal-cased magnets, it ran into trouble due to electrolytic action. When the brass cases were replaced by tufnol, it was used successfully for many years.

> The horizontal part of the magnetic circuit would have been made of timber, if the contractors had had their way – it would have helped to lower the centre of gravity, they said.

The work of N. Kurti's team who were planning the two-stage demagnetization of 1956 called for an iron-free solenoid-type magnet with vertical field and with very low stray field. J.M. Daniels (28.2) applied himself to this requirement and was able to design such a water-cooled magnet to match the laboratory's 100kW generator (originally installed by Egerton in the old Clarendon Laboratory). This magnet gave 1.47 tesla in a 4 centimetre diameter central hole, with homogeneity to 0.5% over 6 centimetres. It was very much used in other work as well as in the two-stage demagnetization and was a useful curtain raiser to the generation of high-field magnets for use with the
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2MW generator. These were designed by M.F. Wood, the Cambridge engineer who joined us in 1956 and are described below.

There was a demand for magnets of lower field, air-cooled, under 0.5 tesla for the microwave spectroscopists and others and what became known as the TRE or Tickford or Newport magnets were widely used and, indeed, their successors still are. Just after the Second War, word reached Oxford that the Telecommunications Research Establishment[62] at Malvern had spare workshop capacity. J.H.E. Griffiths and N. Kurti went down by train and were enjoying the opportunity for an uninterrupted chat. When they noticed that the train was at Evesham – according to the timetable 50 minutes away from Malvern – they remembered that they were supposed to be turning up at TRE with a specification. An old envelope was found and the specification worked out in the available time. These magnets were commercially produced later. They gave 0.6 tesla continuous with 5 centimetre gap. Scaled-up versions are still in production giving 1.5 tesla with the same gap. The Clarendon Laboratory received regular royalties – in kind.

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[62] Subsequently RRE and now RSRE

2MW Generator

On 24 March 1939 Professor Lindemann had written to his former colleague, Prof. A.C.G. Egerton (see p.[83]) of Imperial College and Secretary of the Royal Society seeking financial support for a DC generator and magnet with the objective of reaching 100 kilogauss (10 tesla). (It is curious that Lindemann states temperature objectives in the form "1/10,000 of a degree" rather than "0.0001 degrees" or even "10^{-4} K".)

The sums attached to the various items, even allowing for inflation over 45 years, seem a little odd. He puts forward the following "extremely rough" figures

1,000 KW generator second hand		£1,000
building to house it		£ 200 (sic)
installation, switchgear etc		£ 200
water-cooling arrangements	less than	£ 200
magnet	less than	£ 200
(to be built by the Clarendon Laboratory)		

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He therefore applies for a grant of £2,000 hoping not to use the whole sum. In a postscript Lindemann writes "I am afraid there is little hope of getting help from Lord Nuffield. He appears to draw a very definite line between physicians and physicists".

On 10 July 1939 the Assistant Secretary wrote to Lindemann to say that a grant of £1,500 had been made from the Messel Fund for the purchase of "a generator and a large magnet". Meanwhile, Lindemann had approached Col. Fraser, the Oxford City Electrical Engineering Manager, who consulted the British Electrical Development Association Incorporated of Savoy Hill, London. Their advice was to try the various city authorities known to be moving away from trams. One of these was Manchester Corporation, who had two generators available, both rated at 2 megawatts and offered at £1,000 each. Using established contacts at Metropolitan Vickers, Lindemann was able to get a manufacturer's view of their condition in August 1939, as a result of which No. 4 Generator Set was identified as being the one for Oxford. Naturally, the outbreak of war put a stop to these plans but, when in June 1945 they were resurrected, it was found that the machine was still there and still available to us at the same figure of £1,000 and that the grant from the Royal Society was still available. A further inspection was made by Metropolitan Vickers who reported that the commutators were only worn by 1/16 inch in 28 ½ inches and had not been stoned and that, although it had been supplied in 1919, it had been very little used. The original cost was £9,856. The generator which eventually came to us was one of four in Manchester Corporation's Dickinson Street depot. Their output of 400 volts DC was used both to power the Manchester tramway system and in the tungsten street lighting[63].

The generator set (Fig. 41) is in three parts: a 2,900 horse-power 6.5KV slip-ring motor driving two generators each rated at 2,200 amps at 460 volts, with 3 pedestal bearings. When the generator set was first run up, it was

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found that there was serious instability between the two generator sections – at times one would tend to bear more than its share of the load. Metropolitan Vickers were called in and cross-connected shunt-wound supplements to the field windings were prescribed. This was going to cost £4,000 and raising this was going to be a serious problem when T.H. Carr, who had been looking after our electrical affairs since 1946, remembered that he happened to have a drum of 1,000 amp cable and it was not long and at no cost at all before the generator set was running properly.

[63] The author's thanks to Professor N. Kurti who established this interesting dual usage on site in 1985.

High Magnetic Fields

In 1957 the liquid helium dilution refrigerator was still a decade off as a readily available laboratory tool and the two-stage demagnetization process was the only route towards temperatures below about 1 milli-degree. In the absence of a commercial source of solenoids matched to the 2MW generator, there seemed no alternative to making them on the spot. This being a long-term engineering development programme, the Clarendon Laboratory took the step of appointing M.F. Wood, a Cambridge graduate who had spent his National Service years in the coal industry. In a short time he had produced an aluminium-alloy-clad polyhelix type solenoid but a change to glass-reinforced polyester for the case soon showed many advantages and these became the standard.

Meanwhile, “hard” superconductors were coming in and by 1962 cryostats containing superconducting solenoids giving fields up to 6 tesla in a 1.5cm hole were being designed and built in the Clarendon Laboratory. The high point in the laboratory’s development of large superconducting solenoids came in 1976 with a record-breaking 16.5 tesla in a 2.5cm hole. This magnet has been in regular use by several of the laboratory’s solid-state physics and spectroscopic groups.

For groups needing high fields in a larger diameter hole, the so-called hybrid system was designed – an outer superconducting solenoid producing 6.5 tesla surrounding a resistive copper solenoid of the polyhelix type, producing 9.0 tesla so as to give a total field in the central 5 centimetre hole of 15.5 tesla.

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Building Changes from 1946

Soon after the end of the Second War, the Department was able to acquire a very large amount of surplus components and, in particular, a railway truck full of valves, not all of which were useful but many types were to keep the laboratory going for many years. All this loot was dumped in the as-yet empty High Tension room and before the nuclear physicists could even get back to the position in the old laboratory of six years earlier, this valuable material had to be shifted. Two Nissen huts – possibly left over from the First War – were put up in the light well with shelving from the so-called static water tanks. By some sleight of hand, a maintenance workshop was built to allow free access from the Clarendon Laboratory to and from the Electrical Laboratory.

But the highest priority was, of course, to put up a building to house the 2MW generator from Manchester and two research rooms for high-powered magnets and a few others. This is the part of the present buildings subsequently called the Simon Building. It is built of stone because only this material and the specialists in laying it happened to be available in the very difficult post-war years. The new building work wrapped itself round the NE corner of the Townsend Building with which it communicated by two passages through former windows. A bridge was constructed to provide first floor access and, whether by good luck or good forethought, the floor levels were identical. On the ground floor, however, we had an example of the impracticality of the Prof. Because of some unresolved misunderstanding about access by fire engines, communication at ground floor level was at first in the fresh air and possible at the far end only by walking through one of the new research rooms or by going down a step into the workshop and up again. Although folding doors were soon installed, this much frequented route remained highly inconvenient for many years. But still odder, having united the departments first administratively, then for traffic structurally, the Prof took fright – Townsend’s relicts would come over in the night and steal his people’s instruments. (Of course, there were more of “his” people in

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Townsend's former premises but this seemed not to count.) So a pair of lattice gates, as commonly used on lifts, was installed where the bridge joins the original building and these were to be padlocked at night. They never were, but remained as a sort of monument for over 30 years.

The 1947 Simon Building had a southern face for a future extension and it was to this that a building was added in 1962, comprising a first and second floor of high magnetic field laboratories, known as the Mullard Cryomagnetic Laboratory, in recognition of the generous contribution of that company towards its cost. At ground floor level there was a workshop extending to the south-east corner of the Townsend Building. The upper floors were supported on eight legs, one of which, not very conveniently, was inside the workshop. (This workshop was converted to a library in 1983.)

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Chap. 28

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PART VII

Chap. 29 Theoreticians at Last

Impressive as the Prof.'s record was in building up experimental physics between the wars, it must be recognised that his attitude to theoretical physics was inadequate and eccentric. He took the lofty view that any experimental physicist worth his salt should be up to mastering the mathematics. In short, he did not see the need, let alone the value of non-experimental theoreticians. This attitude is the more difficult to understand in the light of his enthusiasm for Heisenberg's development of the quantum theory. On the other hand, Lindemann could take comfort from the example of Rutherford who was clearly getting along very well without anyone to do his mathematics for him (although it is said that F.W. Aston, of mass spectrometry fame, and C. G. Darwin helped him at various times). To compound his cavalier attitude towards theoretical physics in general, Lindemann allowed himself to fall behind – around 1926 he failed to read Schrödinger's new wave mechanics or Dirac's relativistic treatment in 1932. When Schrödinger came to Oxford to lecture on his wave mechanics at this time (29.1), it was embarrassing that no-one was familiar with concepts which had become part of everyday life in other universities.

> When F.E. Simon came to Oxford from Breslau in 1933 he asked how he could meet the senior theoretician. It was explained to him that the nearest to the description was the Sedleian Professor of Natural Philosophy, A.H. Love, the discoverer of Love waves, who lectured in Townsend's Electrical Laboratory. Simon arranged to be at hand on a day which happened to be when the Professor lectured on the gyroscope and the sight was, therefore, presented of an aged and bearded gent extracting himself and two bicycle wheels from a taxi. Simon was deeply moved.

In 1930 the Rhodes Trustees were able to secure A. Einstein to give the Rhodes Memorial Lectures in the following year and these he duly delivered on the Theory of Relativity on three successive Saturdays in May 1931 at noon in Rhodes House, in German. Christ Church, no doubt prompted by Lindemann, made him a Research Student (ie. Fellow) for five years. For some of 1931 he lived
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in college, in the rooms usually occupied by the famous R.H. Dundas (29.2) who was doing a world tour for nearly all of 1931. Einstein delighted Christ Church and, indeed, the whole University.

> There are many anecdotes about the year when the great man was in Oxford. Perhaps these are relatively unfamiliar. On being asked whether he would sign a blackboard which he had used on an earlier visit and which was afterwards quickly glazed and framed: "This I will not do. What I thought then I do not think now."
>
> While being shown round Winchester College by J.G. Griffith[64], he noticed that a Chamber converted from housing a group of studies to a changing room still had names inscribed at high level. This puzzled him but, when he had rationalized it,

[64] Son of I.O. Griffith, Fellow of Jesus College 1938-1980, Public Orator 1973-1980.

he said: “Jetzt versteh’ ich. Der Geist der Verstorbene geht in den Beinkleidern der Lebender”.

By Lindemann’s influence with ICI, E. Schrödinger (1887-1961, For.Mem.R.S. 1949, Nobel Prizewinner shared with P.A.M. Dirac 1933) was installed in Oxford in 1933 as a Fellow of Magdalen College. He was given no accommodation by Lindemann and he never really settled down. In 1936 with little political shrewdness he moved to Graz and, when Hitler invaded Austria two years later, to Rome. It was there that Schrödinger was persuaded by the Prime Minister of the Irish Republic, E. De Valera, to join the staff of the ambitious Institute for Advanced Studies in Dublin. Although the scheme did not work out on the scale hoped for, Schrödinger stayed there as Professor for Theoretical Physics until his retirement.

An Advisory Committee was set up at the request of the University Chest at the time in 1937 when the decision had been made to go ahead with Lindemann’s new building. W.L. Bragg (29.3) and Professors Lindemann and Townsend gave evidence. Bragg’s advice was eventually to unite the two departments under an experimental professor and to convert the Wykeham Professorship to a theoretical one. This is, of course, what happened in 1946. But when in 1943 the question came up of filling Townsend’s vacant chair, although this was postponed until after the war, it became evident that the recommendations of 1938 had included no provision for theoretical physics other than the view that “… the Sedleian Professor ought to cooperate more closely with the work of the Clarendon Laboratory”.

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The electors of the first Wykeham Professor of Theoretical Physics in 1946 faced an unusual task – they had to find someone undismayed at coming to a non-existent department which was subordinate to Professor Lord Cherwell who, it was well known, was somewhat out of sympathy with theoreticians. A bold initiative would have been to find accommodation away from the Clarendon Laboratory, set up a new and independent department and invite R.E. Peierls of the University of Birmingham to head it. (When this possibility fell through, F.E. Simon is said to have said “It is just as well – we can’t do with too many bloody foreigners about the place”.) But looking at the Electors, one sympathizes with their position: the Vice-Chancellor (Sir Richard Livingstone, President of Corpus Christi College), Sir Lawrence Bragg (the outsider), The Warden of New College (A.H. Smith), Professor H.H. Plaskett (New College), Professor C.N. Hinshelwood (29.4), E.W.B. Gill (29.5) and Professor Lord Cherwell himself. Luckily they were able to appoint M.H.L. Pryce (Fig. [42]), then aged 33 (FRS 1951).

> Maurice Pryce has always been remarkable for his cherubic countenance and this led to misconceptions about his age and status. Lord Cherwell for one as well as distrusting him as a theoretician did not take him seriously. In those days it was still contrary to the Proctors’ Rules for junior members to frequent licensed premises and solemn processions of a Proctor and two attendant bull-dogs made their way round the Oxford pubs to bring offenders to book. Just after the War, undergraduates tended to be older than normally so it was not surprising that this jolly young man was questioned in the Eastgate Hotel bar,
>
> “Are you a member of the University, Sir?”
>
> “Yes.”
>
> “Your name and college for the Proctor, Sir?”
>
> “Maurice Pryce, New College – er, Wykeham Professor of Theoretical Physics.”

For two years before the Second War and for a year after he had been Fellow of Trinity College, Cambridge and for the five years 1940-5 Reader in Theoretical Physics in the University of Liverpool, though in fact engaged on both radar and atomic energy research.

Pryce was duly installed in the sunny but at the same time somewhat bare room next to the original library on the south west corner of the new laboratory. For three years he was the sole spear-point of the theoretical physics empire. To us undergraduates of the day the new professor was a model of lucidity, although the weaker brethren got a little breathless. We did not realize, of course, that this was something quite new in Oxford – an ingredient which had been sadly lacking for many generations.

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The advent of M.H.L.Pryce coincided with the attack on paramagnetic solids at a variety of temperatures with the sources of centimetre and millimetre waves left over from the Second War.

Bleaney, Cooke* and Griffiths with their collaborators were beginning to amass spectroscopic data and M.H.L. Pryce was eagerly providing the theoretical background – sometimes that way round and sometimes suggesting such-and-such substances in such-and-such conditions. This work was to expand greatly when A. Abragam joined the group in 1948 and in the following year R.J. Elliott (later to become professor himself in 1974). In 1949 K.W.H. Stevens joined Pryce in this field as a Pressed Steel Fellow and we shall see how the experimental work expanded. In 1948-50 Pryce published seven papers, one with his colleague, K.W.H. Stevens, all but one being concerned with the RF spectroscopy of paramagnetic substances. But he was at the same time maintaining his grip on nuclear physics and gradually building up a wide spread of interests in his sub-department. As to permanent University staff, Pryce was able to appoint G.S. Rushbrooke in 1948 to a post then called Senior Lecturer and, in the following year, C. Domb to an ICI Research Fellowship – they were accommodated in what had been Townsend's room on the south west first floor corner of the Electrical Laboratory (soon to be called the Townsend Building). The ladies lavatory outside was converted to the uses of the theoreticians as in subsequent years their numbers grew. They made important contributions in statistical mechanics and particularly series expansions. Then, in 1951-2, both of them left, Domb to a Professorship at King's College, London, and Rushbrooke to a Professorship at Newcastle. This was a difficult time because Pryce had to take a year off with, as he put it, "twins and pneumonia". Lord Cherwell, to the natural irritation of Pryce, exercised his constitutional right and, though without consultation, appointed J.A. Spiers to the Senior Lectureship. (Spiers came with a very distinguished military record and at once impressed with a paper (29.6) which paved the way to nuclear orientation.) The consequence was that K.W.H. Stevens left Oxford for Nottingham, leaving Pryce less well-provided for in the paramagnetic solids work. R.J. Blin-Stoyle (FRS 1976) succeeded to his Pressed Steel Fellowship*.

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After a year in Birmingham, he returned to take up a new post of Senior Research Officer (nearly equivalent to University Lecturer). This appointment brought strength to the nuclear physics side until 1962 when he moved to the University of Sussex.

It was a blow to Oxford physics when M.H.L.Pryce resigned in 1954 to take up the H.O. Wills Chair at Bristol in succession to N.F. Mott. The advantages of running an autonomous department were obvious and no doubt the prospect of anything of the sort in Oxford seemed bleak, although the Prof's retirement could not be far off. During the

interregnum and with the support of F.E. Simon, D. ter Haar, whose interests in low temperature physics recommended him to Simon, was appointed as Lecturer and, in 1959, Reader. He also introduced astrophysics and cosmology and forged links with the University Observatory. The Electors were changed only by the succession of Prof. C.A. Coulson (Rouse Ball Professor of Mathematics) to E.W.B. Gill's place because the Warden of New College had become Vice-Chancellor. This body of men were able to pull off a tremendous coup for the University in filling the Wykeham Professorship with no less than a Nobel prizewinner, Willis Eugene Lamb Jnr, then aged 43 and Professor of Physics in Stanford University, California. Lamb and Retherford in 1947 applied war-time techniques developed for radar to the measurement of hyperfine structure in the hydrogen spectrum, predicted by Dirac but not yet observed. Their results were closely in agreement with theory and they were able to advance the growing subject of quantum electro dynamics. Many reasons were advanced for this unlikely move to Oxford, from trouble at Stanford over car-parking, to the superior library facilities in Oxford for Mrs Lamb's work in Spanish history. Lamb continued the tradition of covering a wide field of physics. He was able to add to the permanent staff in 1957 three combined college and University lecturers – M.C.M. O'Brien, Fellow of Lady Margaret Hall, C.A. Caine, Fellow of St Peter's College and D.M. Brink of Balliol College and, in 1959, R.J. Elliott, Fellow of St John's College since 1957, was made full University Lecturer (Fig. 43). W.E. Lamb returned to the US in 1962 as mysteriously as he had come, to everybody's regret but with thankfulness to have had him here as long as we did.
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Now – and some would say and about time too – the grand old man of British theoretical physics came to Oxford. With his wide experience in particular covering solid state physics and nuclear physics, R.E. Peierls (Fig. [44]), who had been Professor of Theoretical Physics at Birmingham since 1937, could not have been better suited for the job. (The Electors this time were the Vice-Chancellor, Dr W.F. Okeshott, Rector of Lincoln College, Sir William Hayter, Warden of New College, Professors Bleaney, Coulson, Hinshelwood and Sir Nevill Mott and Dr A.H. Cooke.)

There were some who were a little nervous that this Great Panjandrum from Birmingham might want to run Oxford theoretical physics in the same way as he had been running the pyramidal structures at Birmingham all those years. But the reality was very much the continuance of the role of chairman of four more or less independent groups – solid state under R.J. Elliott, nuclear structure under D.M. Brink and B. Buck, astrophysics under ter Haar, with the new Royal Society Professor, R.H. Dalitz (FRS 1960) and J.C. Taylor as Reader to expand elementary particle physics. (The experiment of appointing a theoretical plasma physics professor, W.B. Thompson, was doomed to last only for two years.)

The growing Department was by now ripe for independence and this was clearly the time to face the accommodation question. It could have gone two ways: either the nuclear physics theoreticians could have been accommodated in the new nuclear physics laboratory, the solid state theorists in the Clarendon Laboratory etc. or, if suitable space were available, the new department could be pulled together into a unity though at the expense of some geographical separation from the experimentalists. The plans for the eastern end of Keble Road included a unit designed for theoretical physics and undergraduate physics laboratories and, while all the Keble Road houses would have to vanish, 12 to 14 Parks Road could remain temporarily. In the event, the axe of 1974 caused this plan to disappear into the indefinite future and Professor Sir Rudolf Peierls (since 1968) retired in 1974 with his department somewhat cramped but at least together in 12 to 14 Parks Road and one floor of 10 Keble Road.

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The Electors followed the obvious course of appointing R.J. Elliott to the Wykeham Chair. Clearly there was no hope of new buildings for at least two decades so an arrangement was come to whereby Theoretical Physics was to be housed in a totally restructured 1 to 4 Keble Road – close to Nuclear Physics but slightly further from the Clarendon Laboratory. These geographical considerations undoubtedly affected the very desirable liaison between theoreticians and experimentalists which was so beneficial in the early days in the Clarendon Laboratory. But this is compensated by the interaction between those researching in different disciplines which allows the Department to mount integrated courses across all of theoretical physics and individually helps hold together Oxford Physics as a whole.

The present department has become very much comparable in scale with the other three large theoretical groups in the country, namely at Cambridge, Manchester and Imperial College, numbering a total of a hundred souls and turning out some 12 D.Phils every year.

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Chap. 29

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29.2 H.R. Trevor Roper, Spectator (9 June 1967), A Brief Life: R.H. Dundas by John Aubrey, FRS

29.3 Then Director of NPL, Professor of Experimental Physics, Cambridge, 1938, Sir Lawrence Bragg, 1941, son of W.H. Bragg

29.4 OM 1960, Nobel Prize 1956, Dr Lee’s Professor of Chemistry 1937

29.5 Fellow of Merton College, former colleague of J.S.E. Townsend

29.6 J.A. Spiers, Nature (1948), 161, 807

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PART VII

Chap. 30 Microwave Spectroscopy

We have seen (p[153]) that the 1.25 centimetre radar sets developed in Oxford and on a large scale at MIT turned out to be of no practical value because of the absorption due to water vapour. B. Bleaney[65] (Fig. 45) became interested in making measurements of the very much stronger absorption in ammonia and soon after the end of the war, in 1945, he set out to test the recently published van Vleck-Weisskopf (30.1) line-shape formula on ammonia.

The ammonia molecule has the shape of a very flat tetrahedron and it is the nitrogen atom flipping from side to side which is in resonance with the 1.25 cm radiation. Working with R.P. Penrose (1921-1949), Bleaney spent the years 1945 -48 making a thorough spectroscopic study of this gas (30.2). By this time these phenomena in a variety of compounds were becoming very much worked upon in the US, so Bleaney decided to join J.H.E. Griffiths in microwave spectroscopy of magnetic solids and, in particular, metallic salts. Griffiths had meanwhile in 1946 put behind him his pre-war interests in nuclear physics and, like Bleaney, interested himself in the absorption by solids of centimetre waves. Discoveries are of two types. Sometimes an experimenter thinks up a theory or a theoretician does it for him and then an experiment proves the theory right or, of course, not. Less often an experimenter tries an experiment out of the blue, looking for one phenomenon but finds something else. Griffiths's discovery of ferromagnetic resonance was of this sort.

He was interested in the behaviour of the permeability at micro-wave frequencies. Its behaviour at the lower radio frequencies was well-known, as was its disappearance at optical frequencies – what would it do at radar frequencies? Griffiths had three klystrons available, working at 1.22, 1.43 and 3.18 centimetres. These he applied in turn to resonant cavities, one wall
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of which had been plated to a thickness of 0.025 mm with nickel, iron and cobalt. He put a magnetic field parallel with the surface of the specimen and looked at the power supplied by the klystrons. What he found (30.3) was the typical curves for resonances. When he tried to formulate a theory, he found himself in difficulty with the demagnetizing correction and it was not until C. Kittel's (30.4) paper of 1948 that the right way of working it out became apparent. (Griffiths's first research student, K.J. Standley, in an appreciation (30.5) of him recalls that his first reading of this paper was a rare occasion of his being thoroughly put out.)

While Griffiths had been busy with ferromagnetic resonance, Bleaney's* research student, D.M.S. Bagguley[66], had begun in 1946 looking for paramagnetic resonance in $CuSO_4 .5H_2O$ at room temperature. He made two discoveries. One was that the lines were unexpectedly narrow – certainly narrower than those in chrome alum. This effect was

[65] Fellow of St John's College 1945-1957, FRS 1950, CBE 1965, Dr Lee's Professor of Experimental Philosophy, 1957-1977

[66] Fellow of Brasenose College 1950

explained by Gorter and van Vleck (30.6). The second discovery was that where two lines should be seen owing to anisotropy in the crystal only one appeared. This again turned out to be due to the very strong exchange forces between neighbouring copper ions (30.7).

But the results from other paramagnetics at room temperature were disappointing and Bleaney and Penrose, therefore, built a cryostat usually used with liquid/solid hydrogen down to 12K. They soon found resonance in many compounds of elements of the transition group. The results of the Bleaney and of the Griffiths groups are summarized in joint papers (30.8) of 1948. It was in the course of this work that the anisotropy in cobalt salts was discovered and this was to be important in nuclear orientation (see p.[164]). In this work the Clarendon Laboratory was well ahead of the rest of the world. In the USSR some work was going on but not above 120MHz – we were able to work at 30-300 times higher in frequency.

In 1948 Penrose[67] went to Leiden on a visit where he discovered what had every appearance of being hyperfine structure in a dilute copper double
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sulphate (30.9). However, this prompted a letter from M.H.L. Pryce and A.Abragam (30.10), who pointed out that the anisotropy observed was not that expected*. It took a considerable experimental programme in Oxford before Pryce and Abragam were able to report the rationale (30.11).

In 1949 Bleaney was able to spend six months at Harvard where he learned some quantum mechanics (the great guru of the subject, J.H. van Vleck, the author of <u>Electric and Magnetic Susceptibilities</u> (30.12), was away on sabbatical)*. This enabled him to cope with the theory and accounts for his remarkable subsequent successes, including of course his co-authorship with A. Abragam of <u>Electron Paramagnetic Resonance of Transition Ions</u> (30.13).

The possibility of observing the h.f.s. promised a new purpose behind paramagnetic resonance work – that of being able to determine hitherto unknown nuclear spins. For example, by diluting neodymium with the inert lanthanum, Bleaney was able to see not merely the nuclear spin but the presence of two isotopes. Naturally, his group were soon measuring many other of the rare earths (30.14). Meanwhile, Griffiths and J. Owen were looking at the heavier elements such as iridium. Bleaney also turned to the heavy elements in the actinide group and was able to measure the nuclear spin of uranium, plutonium, americium and neptunium (30.15). These nuclei are often far from spherical and a study of their h.f.s. gives valuable clues to the structure of their nuclei.

The above has drawn on Contemporary Physics, Volume 25, 1948, where a full account by Bleaney is to be found.

The torrent of papers with the name B. Bleaney on them – 63 papers, plus the first edition of the renowned <u>Electricity and Magnetism</u> (30.16)*, in the twelve years after the Second War – naturally subsided under the pressure of administrative work when Bleaney became Dr Lee's Professor in 1957, although the successor to J.H. van Vleck's <u>magnum opus</u> mentioned above was under way. But groups in their own right were becoming established of the Professor's former collaborators. J. Owen[68] and J.M. Baker[69] were the specialists in electron spin resonance and later electronic/nuclear double resonance (or ENDOR). A group
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[67] Died soon after in Leiden of a cerebral tumour
[68] Fellow of Lincoln College 1954
[69] Fellow of Merton College 1956

under W. Hayes[70] looked after magnetic resonance techniques in the investigation of defects in solids.

The work of these groups was greatly advanced by the coming in 1958 of G. Garton[71] on a US Airforce contract concerned with masers. He was an Oxford inorganic chemist whose first job was to make crystals of dysprosium aluminium garnet. This he was able to do in a matter of months and naturally he was besieged by eager microwave spectroscopists with demands still unexhausted after nearly thirty years for crystals of all fourteen rare earths in varying compounds yet purer, yet more optically perfect, sometimes with a trace of one of the other rare earths and at varying concentrations and, of course, with their crystallographic axes determined. There is also a steady line in fluorides and in oxygen-17 substituted oxide specimens for nuclear resonance. There are many crystal-growing groups around but none with this one's wide repertoire.

The above, together with the specialist theoreticians, made decisive inroads into the theory of the rare earths. At the same time, other groups were interesting themselves in other aspects of solid-state physics. D.M.S. Bagguley, R.A. Stradling[72] and B.V. Rollin in semi-conductors, H.M. Rosenberg[73] in composites, J[74] and E.M. Wilks in diamonds. Increasingly, as the years went by, people asked themselves what had happened to low temperatures. R.W. Hill was certainly finding continued interest in specific heats as purer specimens and lower temperatures became available. K. Mendelssohn's group remained in being until his retirement in 1973.

In 1973 Bleaney was seized with a new idea called enhanced nuclear magnetism. In a paper (30.17) he sketched out the situations in which it might be possible in some of the rare earths in appropriate conditions for the nucleus
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to be subjected to a strong field due to the electron. A typical forecast of this enhancement was a factor of 175 in field strength. It was some time before it was realized that a likely candidate for a successful experiment was holmium vanadate. Bleaney obtained one of the Garton group's crystals on which no measurements had been made because of its unpromising electronic constitution (a singlet ground state)*. But it was exactly that which made the substance ideal for the trial experiment which was so successful that it led to widespread interest in this material and the Holweck prize. No doubt other forces were at work as well but it is said that once this experiment was successful, the decision to resign the professorship followed very quickly, and it is a continuing delight to everybody in the Clarendon Laboratory to see the results and new ideas pouring out in the way that we had got used to in the ten post-war years.

<u>A.H. Cooke</u> (Fig. 46)

While A.H. Cooke[75] was still an undergraduate at Christ Church, where his tutor was C.H. Collie, he was included with F.E. Simon and H. Pearson as an author of a paper (30.18) on a remarkable bid to apply the adiabatic expansion principle which worked so dramatically for helium to the liquefaction of hydrogen. The thermodynamic data for hydrogen at temperatures below those of liquid air and at pressures above 100 atm. were unknown at the time. Simon was always an optimist and would have wanted to do an

[70] Fellow of St John's College, 1962
[71] Fellow of Wolfson College, 1967
[72] Student (Fellow) of Christ Church 1965, Professor of Solid State Physics, St Andrew's University 1978, Professor of Physics, Imperial College 1985
[73] Fellow of St Catherine's College 1970, University Reader 197[9]
[74] Fellow of Pembroke College 1958
[75] Fellow of New College 1946, Warden 1976-85

experiment, however modest the projected yield. A major contributor to the success of the helium expansion liquefier was the very low specific heat of the pressure vessel at the temperature of liquid hydrogen and it therefore did not matter how heavy it was. At around 50K the situation is quite different and Simon's approach was, therefore, to look for a vessel of maximum tensile strength with minimum weight. He had been in dialogue with Hadfields and, when they came up with a vessel made from their AMF steel guaranteed safe at 15 atm. and liquid hydrogen temperature, it seemed that the prospects were optimal. Expanding from 45.3K and 180 atm. a yield of 61% of the volume of the pressure vessel was achieved. It was only afterwards that it was revealed that, while in the workshop, the vessel had collapsed in a three-jaw chuck and had quickly been blown out again on the hydraulic pump.

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Between the completion of his D.Phil. thesis and the outbreak of war in 1939, A.H. Cooke and B.V. Rollin built the first expansion helium liquefier from which the liquid could be withdrawn – indeed, the whole expansion was performed through the external vessel, providing a useful degree of pre-cooling. The pressure vessel had a volume of 450cc and was made from Vickers-Armstrong's Vibrac steel which has been in continuous use in the Clarendon Laboratory over the 40 years up to the present time. Expanding from 135 atm. and 11.6K, they found that 205cc of liquid were left in the vessel.

For his D.Phil. Lindemann gave A.H. Cooke an ambitious project which combined nuclear physics and low temperature physics in the way which Lindemann had sought for many years. There had been difficulty for some time in resolving the energy balance in beta-activity and this had led Pauli to postulate the existence of the neutrino. Lindemann hoped that, with the very sensitive heat energy measurement presented by a demagnetized paramagnetic specimen, one would be able to determine these energies. In the event it turned out that the measuring methods of the time, notably of temperature by measuring magnetic susceptibility, were unequal to the demands made by the very small quantities of heat energy. Cooke's thesis title therefore had to be changed and he became a member of F.E. Simon's low-temperature group.

Over the years 1936-8 Cooke redesigned the Simon expansion liquefier plus cryostat, and carried out a series of experiments leading to the establishment of the absolute thermodynamical temperature scale for iron ammonium alum. These experiments departed from the previous Oxford practice of measuring susceptibility and hence temperature by means of a ballistic galvanometer. There had been reports from other laboratories that AC methods led to unacceptable heating but Cooke was able to show first by theory and then by experiment that the use of a low-frequency, ie. 50Hz, led to entirely acceptable heat inputs. These methods were to find application in his work for many years after the Second War. (An account of this part of Cooke's thesis appeared as a paper (30.19) with R.A. Hull.)

After the end of the war, Cooke assembled all the ingredients needed to explore magnetic properties at low temperatures. He was able to

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improve his pre-War magnetic susceptibility measurement methods with the electronic techniques developed in the War. The Royal Society financed a 1.5 tesla iron-cored water-cooled magnet built by Savage and Parsons of Watford. Thus, he was now set up to measure susceptibility over a wide range of temperature, field and frequency, specific heats at low temperatures, adiabatic demagnetization conditions and paramagnetic relaxation. At times in this first decade, when he had been fitted out with a 3cm klystron, his work complemented that of the Bleaney and Griffiths groups, who were not then equipped to make paramagnetic resonance measurements down to liquid helium temperature.

Rare Earths

In a conference paper of 1955 (30.20), Cooke summarizes the data known about the ethyl sulphates and the double nitrates of ten of the fourteen rare earths. The case which stands out is cerium magnesium nitrate (30.21). The cerium atom has no nuclear spin and this means that on demagnetization from 1K and a field of 0.71 tesla, a temperature of 6mK is reached and Curie's law holds to within 2% down to this temperature. What is more, the crystal is highly anisotropic so that the temperature measurements are unaffected by external fields provided that the two fields are at right angles.

Paramagnetic Relaxation

This phenomenon concerns the rate at which thermal equilibrium is established between magnetic dipoles in a paramagnetic substance, firstly with each other ("spin-spin") and secondly between the dipole and the crystal lattice ("spin-lattice"). Fortunately, the time constants for the two phenomena are widely different, being roughly in the radio and audio-frequency ranges respectively. Measurements are made of susceptibility in varying frequencies of applied alternating magnetic field. Clearly the susceptibility will be different when the dipoles are interacting with the lattice (to take the spin-lattice case only) from when they are not. Measurements of specific heat are also possible (see Cooke (30.22)) by this means.

The experimental system was, therefore, elaborated to include the facilities for paramagnetic relaxation time measurements and, in the decade
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up to 1956, several papers were published mainly with R.J. Benzie (30.23).

In a series of experiments by Cooke and Benzie (30.24) on copper double sulphates where the other ion was in turn ammonium, potassium, rubidium and caesium, and also in which the copper was successively diluted with zinc, it was possible to separate out the three contributions to the specific heat between 1 and 4.2K ie. dipole-dipole and exchange interaction and nuclear hyperfine splitting. The results were consistent with the microwave spectroscopic results of Bleaney et al (30.25).

The cornucopia of G. Garton's crystal growing group benefited Cooke's magnetism group as much as it did the microwave spectroscopists – for one thing, the two groups often worked on different properties of the same substance. Their wide scope is well-illustrated by a paper of 1970 (30.26) on dysprosium vanadate – an antiferrimagnetic with its TN* at 3K which undergoes crystallographic transition at around 14K. The techniques were spectroscopic (optical and infra-red), specific heat and magnetic moment measurements. The problems in measurements on specimens of this kind are shown by the words "measurement of any crystal property with a magnetic field along the b axis is, therefore, difficult since the crystal tries to avoid this configuration".

Cooke has always been lucky in his research students. W.P. Wolf remained until 1963 when he was appointed Professor at Yale. In later years, D.T. Edmonds[76] was a notable successor, diluting his pure physics interests with work in the biochemical field. M.J.M. Leask[77] also came into the group (see p.[ ]).

A direct consequence of the availability of tailor-made crystals of high optical quality was the branching-off from the Cooke group of the magneto-optical spectroscopic work of Leask in the mid-1960s.

[76] Fellow of Wadham 1965
[77] Fellow of St Catherine's 1965

There are a great many stories about Arthur Cooke's witty remarks, though the Clarendon Laboratory was not the scene for those which must have been an ingredient in his ascent into the top flight of University life. I treasure these two.

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In the 1950s we all used big glass bucket dewars to carry liquid oxygen about in and it was not unusual, though always interesting, to hear the impressive sound of one breaking. This sound was coming from room 016 and within seconds the occupants of 015, 017 and 018 were piling into the empty room 016. Very shortly the Doctor came to join the fun and as we had worked out what had happened we explained it to him – a rise of pressure in a glass system had pushed the bung of a tap out, this had dislodged a huge brass nut which must have been near the top of the cryostat and was now lying among the broken glass. "Ah – I see", he said. "What a neat arrangement."

Then there was the time when solid-state circuitry was coming in and cathode ray oscilloscopes and the like suddenly became smaller. Around that time the Lindemann Building's gas meter was being changed and, as our consumption had never reached the level predicted in 1939, a much smaller one was waiting in the front hall ready to go in. When A.H. Cooke saw it, he said "Transistorized model?"

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PART VII

Chap. 31 Solid State Physics

F.E. Simon liked to think of himself as well-entrenched in the field of solid-state physics – a term which according to the OED came into use in 1866. However, owing to the dazzling feats achieved by his group he was in his time considered as more of a low-temperature specialist. The work of B. Bleaney, of J.H.E. Griffiths and A.H. Cooke and their associates was thought of in the context of the various magnetic effects but equally they can be regarded as contributing to the growing attention paid in Oxford to solid-state physics.

Cyclotron resonance

The topic of greatest interest in the outside world has become the physics of semi-conductors and we must trace its continuous development in Oxford over thirty years. The role of D.M.S. Bagguley[78] as a collaborator with J.H.E. Griffiths after* the discovery of ferromagnetic resonance has already been noted (p.[187]). In 1953 cyclotron resonance was observed by G.Dresselhaus, A.F. Kip and C. Kittel at Berkeley (31.1) following its prediction in 1952 by R.B. Dingle (31.2) and by W. Shockley (31.3). (The term had been coined before the Second War to describe the motion in the ionosphere of electrons in the earth's magnetic field.) These experiments were of particular interest in that from them the effective mass of the carriers – electrons and holes – could be determined, a possibility first suggested by H. Suhl[79]. Bagguley was already equipped for such measurements. Specimens of single crystal germanium and silicon available in the UK, eg. from Marconi Instruments Ltd, were of lower purity than those used by Kittel et al and cyclotron resonance would have been swamped by the normal conduction. But by doping his specimens initially with copper and subsequently with cobalt or gold Bagguley was able to reduce

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the conduction to such a low level that not only was cyclotron resonance observable at 4K but at higher temperatures ie. up to 90K (31.4). (This work was done with the magnet installed by A.H. Cooke, p[192]) Later work also confounded the pundits by enabling cyclotron resonance to be observable for holes as well as for electrons (31.5).

Magnetophonon effect

In 1958 Bagguley was joined by R.A. Stradling[80] who consolidated this work, going down to 4mm for the RF source and pushing the maximum temperature up to 120K.

The region of the spectrum between the lowest attainable radar frequencies – about 4mm – and the longest wavelengths of conventional infra-red spectroscopy was opened up by the technique using electrical-discharge gas lasers developed by a group at the Francis Bitter National Magnet Laboratory. This so-called far infra-red spectroscopy has been applied in Oxford to the investigation of MOS* devices of what has become conventional electronics.

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[78] Fellow of Brasenose College 1950

[79] A pupil of M.H.L. Pryce

[80] See p.[189]

In 1961 Russian theoreticians (31.6) predicted a possible effect not unlike cyclotron resonance but in which the role of the microwave radiation might be taken over by phonons of "optical" energy, thus becoming known as the magnetophonon effect. Two years later this was confirmed experimentally (31.7) by Puri and Geballe at Bell Telephone Laboratories.

R.A. Stradling was able by this means to confirm earlier unpublished work on the n-type III-V semiconductors indium antimonide and indium arsenide and to find values of the effective mass and of the band structure of gallium arsenide. These experiments did not extend below 60K and it has therefore been possible to design an experiment for third-year students which enables them to observe this quantum-related phenomenon with relatively simple resources (31.8).

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Hetero-junctions

But the most exciting development in semi-conductor physics has been the development of the so-called hetero-junction. This can be likened to a sandwich in which one half is white bread, the other brown and a very thin layer of jam in the middle. The actual constituents of a typical hetero-junction of this sort might be gallium arsenide, a thin layer of aluminium and indium antimonide. The thinness of the middle layer may need to be of the order of 10nm – ie. 1/20 of a wavelength of visible light – and the deposition of this presents formidable practical difficulties. The theoretical treatment of semi-conductor devices incorporating such very thin layers is an application of what has become known as low-dimensional physics. The high magnetic fields which confine the electrons in cyclotron resonance to flat orbits are clearly one established way of confining them to a two-dimensional state and these very thin films are another and both, appropriately used together, can clearly result in a one-dimensional state. These devices treated by such theoretical techniques present a wide prospect of tailor-made electronic devices - eg. the solid state photomultiplier of very high sensitivity. Further, the intriguing possibility that fractional charge may show up is one of the aspects of new physics which is opening up.

The winning of a £600K contract from SERC to buy, install and staff an MOCVD (metal-organic chemical vapour deposition) plant has put the Clarendon Laboratory in the position of working in this field independently of reliance on the semi-conductor industry for samples.

Defects

Interest in defects in solids was aroused by observations such as those of Brown and Thomas (31.9) of GEC, Wembley who found that natural quartz or impure synthetic quartz were reversibly darkened by 30 kV X-rays. while pure synthetic quartz was not. The first experiments on magnetic resonance in similar defects in diamond were carried out by A.H. Cooke (unpublished) and subsequently J.H.E. Griffiths, J. Owen and L.M. Ward (31.10, 11) made a systematic study of neutron-irradiated diamond and smoky quartz. Using the theoretical treatment set out in a paper of B. Bleaney (31.12) they were able to relate the resonances at 1.2 and 3.15 cm to the crystallographic axes.

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Beginning with the work for his D.Phil. thesis published in 1959 on defects resulting from impurities and from X-irradiation of crystals of alkali halides, which he also submitted to irradiation by polarized light at low temperature, W. Hayes led an attack on this method of studying defects which he sustained for nearly thirty years.

Paramagnetic resonance turned out to be a powerful means of investigating defects in solids which have been irradiated by X-rays, electrons, neutrons etc. This discovery was shown up in 1957 in an experiment by B. Bleaney, W. Hayes and P.M Llewellyn (31.13) aimed at measuring the nuclear states of the two radioactive nuclei Am (half-life 470 years) and Pm (half-life 2.6 years). No h.f.s.was shown by either ion but the radiation damage resulting from 5.5 MeV alpha particles in the first case and 0.22 MeV beta rays on the second appeared, which could be interpreted as the N14 spectrum arising from the radiation damage to the nitrate ion.

These experiments led to much further work on a wide variety of solids after damage by a variety of sources, notably 50KV X-rays. A marked increase in the available energy was achieved by the installation (appropriately in the Townsend Building) of a 1MV Van der Graaff set (31.14, 15).

As lasers were developed, they presented increasing possibilities as sources of high radiative energy over an increasingly wide range of frequencies. But a valuable feature was the availability of very short pulse lengths so that it became possible to determine very short recombination times. In 1979 a substantial grant was to be obtained from SERC for lasers and their associated equipment to work in picosecond (10^{-12}s) phenomena. This group under the leadership of R.J. Ryan[81] were to investigate the behaviour of charged carriers, notably in semi-conductors.

A further step towards a more complete solid-state physics laboratory came with the establishment in the Clarendon Laboratory in 1976 of a crystallographic group under A.M. Glazer[82], who came from the Cavendish Laboratory, Cambridge, where he was the last of the long line of
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crystallographers started by W.H. Bragg. His group, numbering about twelve, have looked into many problems – about six at any one time – mainly concerned with topics of interest to the communications industry such as the mechanism of the piezo-electric and electro-optical effects in quartz, mercuric sulphide and bismuth germanium oxide ("BGO"). They are also active in the development of instruments, one of which greatly improves the accessibility for crystallographic work of the temperature range 80K to room temperature.

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PART VII

Chap.32 Physical Techniques in Archaeology and Art History

One of Lord Cherwell's happiest initiatives was in setting up the highly successful Research Laboratory for Archaeology and the History of Art which flourishes to this day in 5-7 Keble Road. It all came about in what was almost a caricature of the way in which everything is supposed to happen in the older universities – over dinner and the subsequent observances now rapidly vanishing into history.

Professor C.F.C. Hawkes had read Greats at New College and, in 1928, was examined by, among others, R.H. Dundas the ancient historian at Christ Church. In 1950, after he had been back in Oxford for three years, he was asked to dinner by the latter to meet Lord Cherwell – something of an event in itself because of the Prof's dietary eccentricities. With the fish the Prof moved into the attack – why had the archaeologists not made more use of the techniques offered by the physicists? Was Hawkes interested? The latter replied that he certainly was, whereupon the Prof said then why had he done nothing about it? After some quick thinking, Hawkes replied that a first-class laboratory would be needed manned by a first-class scientist with the appropriate interests and he thought one would never find one such prepared to devote himself to such work. The Prof's answer was to introduce Hawkes to his neighbour on the other side, who was of course , E.T. Hall[83] of New College, who had been working for a D.Phil. in the Clarendon Laboratory since October 1948 on X-ray fluorescence spectroscopical analysis of metals and alloys. Conversation continued after dinner and included C.H. Collie. Professor Hawkes was borne home in E.T. Hall's fast car to the latter's flat in the Banbury Road where, in due course a substantial breakfast was eaten. Towards dawn, Hawkes was returned to Keble. The Prof's next move was to prevail upon T.C. Keeley to activate Sir Maurice Bowra, Warden of Wadham and Vice-Chancellor of the day. A Committee was constituted, evidence heard and blessings bestowed. The necessary money was allocated by Nuffield and by the
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Wenner-Gren Foundation of New York. By 1955 the new laboratory was established in 6 Keble Road with E.T. Hall and S.A. Young in charge. The X-ray work continued and also a new method – the optical emission from bronzes. There was also the dating of pottery from China by determining the direction of the remanent magnetic field and then finding out when the earth's magnetic field lay in the same direction. After two years Young moved to spend several years with Richardson-Westgarth in the construction of nuclear reactors and thence to South Africa to mine uranium. His place was taken by M.J. Aitken[84] (FRS 1984) who was able to extend the scope of the magnetic measurements by applying the proton magnetometer (see also p.[ ]). The sensitivity became sufficient for the detection at ground level of kilns several feet deep. This work continued over the years 1960 to 1970 but in recent years the study of pottery has been revived by the advent of the SQUID. This has enabled remanence measurements to be extended to cases where only very small samples can be made available.

[83] Fellow of Worcester College 1969, Professor 1975
[84] Fellow of Linacre College 1965, Professor 1986

In 1963 M.J. Aitken began applying the phenomenon of thermoluminescence to the dating of pottery and ceramics. The absorption of natural radioactivity by these materials results in the displacement of electrons which remain trapped. When such material is heated to about 350°C, the stored energy is released in the form of light and the quantity of this light is simply related to the time over which the natural radio-activity has been absorbed. A measurement of the natural radioactivity is, of course, necessary. The impact of this method of dating was very considerable. There were several two-headed drinking vessels from south-west Turkey in major museums and sixty-six statuettes of female goddesses which were reputed to be seven thousand years old but all of which were found to be fakes made within the last few hundred years. (The only genuine example of the double-headed vessel was in the hands of a Swiss dealer.) This early triumph established an immediate reputation for the Keble Road laboratory and for the next ten years they had as much work as they could handle and no competition.

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Oxford has not offered a service for carbon-14 dating until recently when the possibilities offered by high-energy mass spectrometry have become evident. The advantages are that specimens weighing milligrams rather than grammes are needed and that the procedure is much quicker though slightly less precise*.

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PART VII

Chap. 33 Some Individualists

The years after the Second War were very much a time in which individualists could flourish. Two adventitious causes had led to useful amounts of new space: thanks to Simon's freedom with the use of the words "atomic energy", a new building had sprung up at a time of general privation and much of Townsend's Electrical Laboratory – soon to be named after the deposed chieftain – was yawning for occupation. Further, there were a number of bright young men now not so young left over from the radar or Tube Alloys work during the Second War. Some of these naturally joined the established groups but some had ideas of their own. Lord Cherwell had always been sympathetic to someone with a promising idea and with the space which was becoming available and in a time when money was less of a problem than it was to become, the individualists had their chance.

B.V. Rollin

We have seen that B.V. Rollin scored his first success in the discovery of the helium film (p [116]) and that before radar work in the Clarendon Laboratory had officially started he had invented the reflex klystron (p.[149]) and produced the first millimetre wave magnetron soon after (p.[152]). This remarkable man – an individualist if ever there was one – followed these three coups with a further three over the twenty years after the Second War – the first NMR experiment in the UK, the first two-stage demagnetization and the use of indium antimonide as an infra-red bolometer.

B.V. Rollin graduated from Wadham College, where he had been a pupil of T.C. Keeley, in 1933 and joined F.E. Simon's low-temperature group. In those days recruits were put to the development of cryogenic equipment as a preliminary to its use to make measurements at low temperatures. F.E. Simon had a strong interest in what A.H. Cooke has called the "missionary" – he wanted to show that any physics laboratory, however modestly equipped, could
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work at low temperatures. Rollin was given a project which only someone of practical genius could have hoped to get working in those days – a combined Linde hydrogen liquefier with a Simon expansion helium liquefier all contained within a single glass dewar and working from a cylinder of hydrogen gas and another of helium gas. One cc of liquid helium could be condensed into a glass vessel at the bottom and it was with this that Rollin was able to discover the phenomenon now known as the "Rollin Film". This was a quite unexpected additional feature of the phenomenon of superfluidity which K. Onnes discovered in 1928. (He made the observation of the vessel that fills and empties, but never followed it up.)

Rollin went to the University of California in 1937 on a Commonwealth Fund Fellowship where he worked on lead-tin alloys, which exhibit an appreciable change in resistivity between 20 and 60K, with a view to their use in infra-red bolometers. Later he moved from the Chemistry Department to the Radiation Laboratory, where he worked on the application of nuclear physics to solid-state problems, especially the measurement of the self-diffusion coefficient of copper. In 1939 he returned to the Clarendon Laboratory.

The discovery of the Rollin film put Simon in a difficult position. There had been some sort of agreement in their early days in Oxford that Mendelssohn would work on superconductivity and superfluidity while Simon and his immediate colleagues would work on magnetic phenomena including cooling by demagnetization, specific heats etc – very roughly a division between fluids and solids. Rollin was one of Simon's people and here he was discovering a phenomenon arising from superfluidity. This was very much the kind of controversy which was apt to arise and which gave great distress to Simon.

Nuclear Magnetic Resonance

After the War, B.V. Rollin combined his pre-war experience in low temperatures with his war-time knowledge of electronics in looking at nuclear magnetic resonance. This phenomenon had been looked for by C.J. Gorter at Leiden in 1942 but, for technical reasons, nothing was seen. Bloch (33.1) and Purcell et al (33.2) at Stanford and Harvard respectively had been able to detect NMR in water and paraffin. With J. Hatton (see p.[209]) Rollin was able to follow up with a paper (33.3) describing measurements on liquid and solid hydrogen and on heavy water with transverse fields up to 0.4 tesla and RF signals of 2 to 5 MHz.

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In subsequent years there was an understanding that NMR belonged to chemistry and it was not further pursued in the Clarendon Laboratory – mistakenly, some people think.

Two-Stage Demagnetization

This experiment arose out of the recent discovery that the thermal conductivity of a superconductor such as lead is much lower in the superconducting state than when it is made normal by applying a magnetic field (Hulm (33.4)). Heer and Daunt (33.5) and Mendelssohn and Olsen (33.6) were quick to see that this provided the possibility of a thermal switch between the two stages of a demagnetization experiment. With Hatton, Rollin designed an experiment in which a pill of iron ammonium alum was vertically spaced from a pill of 10% copper potassium sulphate in zinc potassium sulphate with a very pure lead wire between them. The experiment was conducted in 5 stages.

1. A horizontal field of 0.42 tesla was applied symmetrically so that both pills were in it. Heat was conducted through the usual exchange gas into the surrounding liquid helium bath at 0.9K.
2. The exchange gas was pumped away.
3. The magnet was lowered so as to demagnetize the upper pill. The stray field, however, was high enough to keep the lead wire normal and heat was therefore able to flow from the lower pill to the upper.
4. An auxiliary compensating magnet was then brought up so that enough of the wire was in a field less than the critical field of 0.08 tesla for the two pills to be virtually thermally isolated from each other.
5. The main magnet was then taken away so that the lower pill was demagnetized.

A steady temperature T+ = 10mK was reached for five minutes, the estimated heat leak to the lower pill being 100 ergs per min.

In a second experiment (33.7) this figure was reduced to 1 erg per minute and the final temperature T+ to 3mK.

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We all have our own lists of physicists who should have got themselves an FRS. Bernard Rollin stands at the top of most of them.

J. Hatton

During the early 1950s, J. Hatton was pursuing other lines, sometimes in collaboration with Rollin and sometimes not. Before the US hydrogen bomb programme, helium-3 was very scarce indeed. Simon persuaded BOC to let us have a cylinder of atmospheric helium (containing one part in 10^5 of helium-3 – 10 times more than in helium from the oil-fields.) Hatton set up a thermal column running between two floors giving a vertical height of nearly 20 feet. After a great deal of time and effort, he managed to end up with 100cc of 1% helium-3 and this was looked upon as riches indeed. He first tried it for NMR but without result. A theory of the time suggested that there might be an excited state of the alpha particle. With his nuclear physics experience behind him, he was able to make an ion source and use it on the 400KV HT set with a target of solid deuterium but, again, no reaction could be detected. At this point Simon asked him to restart the high-pressure work of some six years earlier but it did not prosper and, after an unpleasant though non-injurious explosion, he gave it up and returned to NMR.

P.C. Thonemann

While in the late '30s the world's nuclear physicists were puzzling about what came to be called nuclear fission, an undergraduate in Melbourne was thinking hard about nuclear fusion. The method which seemed to P.C. Thonemann the most promising was to create a pulse of high current in an RF gas discharge contained by a toroidal magnetic field and to so let it constrict itself by its own magnetic field that very high temperatures resulted along the axis. As to the best shape, he quickly came to see that the torus offers the best option. How the pulse of current in the gas is to be induced can be answered by making the torus the secondary of a pulse transformer. This conformation is not the only possible route to the successful extraction of useful energy from nuclear fusion but it is impressive that Thonemann's line of thought is the basis of most of today's ventures forty-five years later, including the JET project at the Culham Laboratory near Oxford.

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The mathematics of the motion of ions and electrons in imposed and self-generated electric and magnetic fields was beyond even the ablest undergraduate and Thonemann was lucky in being able to call upon the help of Laurie Alexander, who was an authority on the ionosphere. On the outbreak of war in 1939 Thonemann found himself working in munition factories but, when he was able to move to the University of Sydney in 1945 and do an MSc, his ambitions were unchanged. With his MSc near completion, Thonemann wrote in 1946/7 to Trinity College, Cambridge and – with a nice sense of symmetry – to Trinity Oxford. The latter passed the letter to the Clarendon Laboratory and, after a letter from Prof. von Weller at Sydney, Lord Cherwell wrote back with an offer. Trinity College, Cambridge was slower to cut corners and Thonemann found himself installed in Oxford as an Advanced Student at Trinity College under the supervision of D. Roaf. Naturally, he put forward his ideas about nuclear fusion but, in the interests of getting something completed within three years, he was put to developing ion sources for the 1.2MeV accelerator – a topic not too far removed from his main interest. But – as at Sydney – he slipped in on the side occasional preliminary items on his fusion agenda.

The ion source prospered and in the following year Thonemann was given space in the then somewhat insalubrious SE corner of the Electrical Laboratory (later called the Townsend Building). There his needs were given the highest priority and before long he was working on a ring discharge in the glass torus shown in Fig. 47. The winding can be seen which produced the toroidal magnetic field required to contain the continuous ring discharge excited by an RF generator.

Fig. 48 also shows a later (1949) apparatus in which the helical winding round the torus was made from copper tube through which cooling water could be passed and with a current of 1000 amps from a plating rectifier a field of 0.1

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tesla could be produced. Part of the iron circuit of the pulse transformer can be seen – all of the latter had to be made up in the Clarendon Laboratory.

But he had to pay for the Prof's support by giving a Friday afternoon Colloquium with M.H.L.Pryce, D.A. Jackson and H.G. Kuhn – all known to be sceptical – lined up in the front row. When it was all over, Lord Cherwell said to him – "Well, - you had a rough ride but you stayed on your horse". Thonemann at about this time also had the unnerving experience of explaining what he was doing to someone clearly of great distinction but of unknown identity, who subsequently turned out to have been Professor George Thomson[85]. As the work progressed the Thonemann group were visited regularly by J.D. Cockcroft[86] and, even more alarmingly, by Henry Arnold, the top security man at AERE, who had tracked down Klaus Fuchs. (Arnold came in for a good deal of light-hearted teasing.) Leading luminaries in the nuclear energy field also appeared including Teller. On one of his Sunday morning visits in early 1951 when he would creep in and be suddenly found by Thonemann's group, standing behind them, Cockcroft suggested a trip to America and Thonemann found himself visiting nuclear energy pundits including E.O. Lawrence of cyclotron fame. He also found himself shadowed by FBI men – development of the hydrogen bomb was just starting up and fusion research was delicate ground. At Berkeley he was offered a job on the spot by du Bridge and Loeb but, owing to an embargo on making appointments placed by the High Court, who were involved with legislation resulting from the McCarthy regime, no appointment could be made and Thonemann returned home.

At this point it was realized that, if a metal toroidal gas discharge chamber was substituted for the glass one, the currents induced in it by the current pulse in the gas should help to keep down the inherent instabilities. This indeed proved to be the case and all subsequent toruses have been made of metal. (Naturally, there has to be a gap and insulation failure due to transient induced voltages of up to 1KV proved troublesome later.)
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By 1949 Thonemann was able to demonstrate to Cockcroft and the Prof a stabilized pinch discharge. In 1951 Thonemann's work became "Classified" (meaning "classified secret") and Thonemann with his colleagues P.A. Davenport and W.T. Cowhig from RRE Malvern were moved to AERE Harwell.

The performance of a system of this sort can readily be shown to be intrinsically dependent upon scale. Thus, when the next torus was built on the same principle, it was very much larger – 1.0m diameter by 3m between centres and was called ZETA (Zero Energy Thermonuclear Assembly). The toroidal magnetic field was 0.04 tesla, the stored energy released at every pulse was 5 x 10^5 joules and pulses up to 2 x 10^5 amps lasting 4m.sec. could be repeated every 10 seconds.

In a paper of January 1958 Thonemann and eleven others (33.8) report that the system is "… adequate for the detailed study of magneto-hydrodynamical processes …" They conclude "To identify a thermonuclear process it is necessary to show that random collisions in the gas between deuterium ions are responsible for the nuclear reactions. In

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[85] Son of J.J. Thomson, and professor at Imperial College since 1930

[86] Then the Director of AERE Harwell

principle, this can be done by calculating the velocity distribution of the reacting deuterium ions from an exact determination of both the energy and direction of emission of the neutrons. *The neutron flux so far obtained is insufficient to attain the desired accuracy of measurement*." (Author's italics.)

J.D. Cockcroft was at the time Director of AERE Harwell and he called a press conference to coincide with the publication of this issue of Nature with copies on sale on the spot. On the way over, he asked Thonemann whether he thought that the neutrons were thermo-nuclear. Thonemann remembers (33.9) replying "I don't know but I think so. However, there are some features which make it rather doubtful". To the journalists Cockcroft, allowing for caution on the part of Thonemann, said he was 90% certain that thermo-nuclear neutrons had been produced. The press did what comes naturally and came out in headlines like "UNLIMITED ENERGY FROM SEA-WATER". Later in the same year experiments by the Nuclear Physics Group showed (33.10) that the neutrons were not thermo-nuclear, whereupon the headlines said "ZETA A FLOP". (The non-thermo-nuclear property of the neutrons was very readily demonstrated by reversing
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the direction of the plasma current in the torus.) The blame for this fiasco lies with Cockcroft – someone of his eminence should know the ways of the press and keep to the letter of the printed word.

Today huge successors are being built of Thonemann's torus of nearly forty years ago. Recognition has yet to follow.

R. Kompfner

R. Kompfner (1909-1978) moved from Vienna to London in 1934 and worked in the profession for which he had been trained, that of an architect. (One must remember that on the Continent a much higher standard in engineering is required of architects than it is here.) In the brutish way customary in wartime, Kompfner was interned on the Isle of Man for six months in 1940. Then he found himself in Birmingham working on that most secret and successful device, the magnetron. It was there that he started working on his ingenious and successful travelling wave tube – a broad-band, low-noise amplifier for microwaves. An employee of the Admiralty, he was sent to the Clarendon Laboratory, as being specialists on centimetre receiving techniques, where he spent a fruitful seven years before departing to Bell Labs where he was to produce clever ideas ceaselessly.

In Oxford he had rewarding collaborations with F.N.H. Robinson and, for a time, with J. Hatton.

J.H. Sanders

It will be remembered that J.H. Sanders with J. Hatton went with D.Roaf to the Canadian reactor site known as Chalk River. They had been recruited by J.D. Cockcroft and, when they got there, they thought about what they would do after the war and did experiments which had some bearing on that. When he came back in 1946, Sanders was involved with P.C. Thonemann and J. Moffatt in the construction of a 50kV accelerator to investigate the D+D reaction at very low energies, the results of which were needed for theoretical work on thermo-nuclear reactions. In 1949 he went back to Chalk River on a post-doctoral fellowship and worked with K.W. Allen on the T+T reaction and the search for the di-neutron. Returning in 1950 as a University Lecturer, he thought it wise to have a specific research project to put forward and it is
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significant that he felt able to take the initiative himself and that the Prof was happy about letting him have space and apply for funds – there was no "Oh well, you'd better have a talk to So-and-So". This is another example of Lord Cherwell's ready sympathy with initiatives.

L.W. Alvarez and F. Bloch in a classical paper (33.11) described experiments at Berkeley to determine the magnetic moment of the neutron. They used a method applicable to any particle, in which a resonance experiment and an inverse cyclotron experiment can be performed in the same magnetic field so that a result can be obtained by measuring the frequencies of the two RF fields only – constancy and homogeneity of the magnetic field is all that is required and not its absolute value. This principle was applied by two groups to the determination of the magnetic moment of the proton expressed in nuclear magnetons – an accurate value of this dimensionless quantity being one of the sources of data from which the fundamental constants are calculated (33.12). C.D. Jeffries (33.13) and Hipple et al (33.14) arrived at conflicting results, namely

P = 2.7924(5) [+/-] 0.0002 nuclear magnetons (Jeffries)
2.79276 [+/-] 0.00006 nuclear magnetons (Hipple et al)

Sanders set out to resolve this discrepancy and, if possible, to increase the accuracy by an order of magnitude. This he was able to do by improving upon Jeffries's experimental design. The final figure quoted by Sanders and Turberfield (33.15) is

2.79277 [+/-} 0.00005 nuclear magnetons

The order of magnitude improvement in the accuracy was achieved mainly by a re-design of the cyclotron part of the apparatus. Sanders arranged for the protons to pursue as many orbits as possible on the same circular trajectory rather than letting them spiral inwards.

When in 1960 this work was complete, R. Kompfner (see p.[213]) invited Sanders to spend two sabbatical terms at Bell Labs where the first paper had just been published on what was then known as the optical maser but subsequently the laser. (While there, Sanders claims he established a record as the first person to make a gas laser that didn't work.)

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F.N.H. Robinson

The observant will have noticed that whenever an important experiment is done in the Clarendon Laboratory, F.N.H. Robinson has something to do with it. This lively, and in those days rare, import from Cambridge brought to Oxford a passion for DIY electronics and what he calls his speciality as an experimental mathematician.

In recent years Robinson has developed his interests in the solving of problems in applied physics and engineering where non-linear differential equations are involved. His publications include many papers on noise in a variety of electronic components. He has contributed to the Encyclopaedia Brittanica under "Electricity" and has had several books published. His method is to mimic the parameters with analogue electrical quantities and, having watched their dynamic behaviour, to digitize them for feeding into a computer. Recently he has secured finance for his work and has strengthened the mathematical contributions of his co-workers.

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A von Engel

In the course of Townsend's tenure of the Wykeham Professorship of Physics it became clear, although not perhaps to him, that the phenomena of discharge in gases were not going to lead to any spectacular breakthroughs in the understanding of pure physics. On the other hand, there has been a continuous stream of applications in applied physics in which Townsend's doctrines have been fundamental. A. von Engel had quarters in the Clarendon Laboratory until 1969 when he moved to the Department of Engineering Science where he is still scientifically active in his 80s. One of the mysterious requirements of wartime security demanded his moving from London to somewhere about 50 miles out. The pin came down on Kidlington but he chose to regard Oxford as within experimental error and 1941 saw him teaching for Christ Church, Exeter and several other colleges. Townsend had disappeared but E.W.B. Gill found room for him in what was then the Electrical Laboratory and he got to know Townsend well outside the University. As a practitioner of an unfashionable line of work, von Engel was unlucky in never having a permanent University or college appointment (though now an Honorary Fellow of Keble College) but, for 45 years, he has kept afloat. More to the point, he has shepherded some sixteen research students through their D.Phils and, mostly with them, published 48 papers up to 1969. He wrote notable obituaries of Townsend for the Royal Society (33.16) and for Nature (33.17). In 1955 he published his magnum opus Ionised Gases (33.18).

One of the topics in discharge phenomena which has become important since Townsend's time is, of course, plasma physics, a leap of around 10^{10} in current. Keeping up with this escalation has presented von Engel with no insuperable barriers – see his review article of 1961 (33.19). His former research students have a way of doing well, notably R.J. Bickerton, now Deputy Director of the Joint European Torus.

J.H. Sanders tells me that he has more than once, when in the US had the reaction to telling people where he came from, "Oxford? Why, you must know Dr von Engel".

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PART VII

Chap. 34 Beams and Lasers

G.K. Woodgate, a former research student of H.G. Kuhn, started the atomic beam group in 1952 after having spent a year at Columbia University under Prof. I.I. Rabi. With the coming of P.G.H. Sandars in 1956, the newly-formed beam group took on a new level of distinction. In the course of working for his D.Phil. thesis, P.G.H. Sandars was measuring the hyperfine structure of the europium atom. Because the 4f shell is half-filled with 7 electrons, there should be no hyperfine structure. The discovery that there was plenty of it set Sandars to working out a method of calculating the consequences of treating the electrons relativistically. With J. Beck (34.1) he was able to develop a tensor-operator treatment of the many-electron atom in which it was possible to turn a relativistic problem into a non-relativistic one. This treatment has been widely taken up and quoted.

In the five years from 1968, the group started on a programme of measurements of the Stark Effect in atomic beams, studying various rare earth elements and aluminium. At the same time, work was under way on measuring the quadrupole moment of the aluminium atom, the experiments being of the greatest difficulty, involving a complicated programme of switching electric fields. An early form of inter-active computer was built by J.R.P. Angel (34.2). Later, a PDP-8 was used to run the experiment. These techniques were to prove their value when it came to still more exacting requirements.

In the 1970s, the atomic beam group moved into the testing of fundamental theories, the first being to set an upper limit to the electric dipole moment of the electron. The outer electron of caesium was an obvious candidate but, for experimental reasons, the outer electrons of the xenon atom were found to be the most favourable. The result was that the dipole moment, if it exists, is less than 2.2×10^{-24} e.cm. This result disposed of a number of theories of the time but one understands that there are always plenty of eager
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theorists waiting for information about the possibilities of an even smaller dipole moment. The next experiment was mounted for a similar measurement for the proton. The search for a favourable element or compound involved much more arcane considerations. In the end, thallium fluoride was chosen – not happily from the toxicity point of view. The beam tube was 70 feet long and yet more complicated computer control was resorted to. The limits to the departure from zero were found to be similar.

Laser physics at Oxford started when J.H. Sanders returned from his visit to the Bell Telephone Laboratories to look into the new optical maser (see p.[214]).

Meanwhile, C.E. Webb[87], who was in his last year at Nottingham University, was doing vacation work at RSRE, Malvern, on 10cm masers. Seeing optical masers in the booklet about research at Oxford, he put in for and got a place in research in October 1960 – one of the only two working on optical masers in the world. The first helium-neon gas laser was made at Columbia and J.H. Sanders brought back to Oxford the practical expert who had been in charge, E. Ballik. (A rare example of UK employment conditions being more attractive than those reigning at the time in the US.) With his help a similar laser was built

[87] Fellow of Jesus College 1973

in the Clarendon Laboratory. Because we had made a machine for measuring reflectivity and had made it available to our competitors at SERL, Baldock, they beat us to it by five days. This laser had plane-plane mirrors and needed an auto-collimating telescope developed by J.H. Sanders to set it up. The concave mirrors of later years made this very much easier.

C.E. Webb, having got his D.Phil. went to the Bell Laboratories and returned here in 1968 on an AEI fellowship. An intensive programme started to find as many possible reactions as they could. By the early 1970s, they had amassed 40. At the same time, J. Piper was developing designs for laser tubes. A successful outcome was the copper iodide laser producing 70 watts mean power of green light. Subsequently, work was largely on the so-called excited dimer, in which the excited state is a molecule of, say, krypton and fluorine which has a life-time of 10 nano-seconds. On radiating and dropping
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back to ground state, the molecule dissociates in one pico-second. The Oxford excimer laser was not a world first but the design proved easier to scale up and has become generally adopted.

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PART VII

Chap. 35 Infrastructure

One of the strengths of Lindemann and even more so Simon was the high value which they put upon the contributions of the essential non-academic members of their department. In the days before the Second War, an element of the amateurish was appropriate but very often, such as in the case of Lindemann's glassblowing, carried to professional standards.

The University of Oxford is governed entirely by amateurs – its Vice Chancellor for four years and all its myriad committees by able people taking varying amounts of time off from the academic work. They are, of course, kept in order by a number of officials with training and experience from outside universities and this number has grown vigorously in the last forty years. But in the '30s the Clarendon Laboratory was run by I.O. Griffith, who was the honest broker who, we have seen (p.[121]), got the Clarendon Laboratory built. On his death in 1941, the torch passed to T.C. Keeley, who managed the department in his quiet but firm way for the six years of the Second War and for the years 1951-53 when Lord Cherwell returned to the second Churchill government. Up to the end of the Second War, the Clarendon Laboratory was able, with the help of the redoubtable Miss Chapman, to manage its own affairs but in 1946 a sizeable error in the financial paperwork, which could not be found without professional help, gave the University Chest the opportunity which they had been waiting for and they put in one of their accountants with the idea that the Clarendon Laboratory's independence was totally ended. The University Chest's ploy totally misfired – the good Mr Butt loyally protected our interests for the next 35 years. In May 1952 the then Secretary of the University Chest, H.H. Keen, wrote to Lord Cherwell complaining that many books were not up to date. From August 1948 for a year Butt had been on his own and, thereafter, had one assistant. Lord Cherwell, in June 1952, instructed Price Waterhouse to investigate. In their report
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dated 10 September 1952 they completely exonerated Mr Butt, spent 220 hours bringing the books up to date and recommended <u>two</u> assistants. Game, set and match to the Prof.

After the War, the Prof felt that there was a need for a good reliable practical man to fill a rather ill-defined role as a sort of works engineer, though called maintenance officer. A charming ex-RN Captain (E) was appointed but the poor man had not been the same since running a Wrennery in Aden. After 5 years of keeping us all amused by darning and washing his socks in his office, he was retired and it must be clear by now who slipped into his shoes.

We have seen that Lindemann's predecessor, R.B. Clifton, was no great devotee of workshops. But Townsend had much of his teaching and research apparatus made on the spot. Lindemann has left even more positive view on record "If an army marches upon its stomach, a physical laboratory progresses upon its workshop". In the 1930s his workshop comprised A.H. Bodle, a former member of Townsend's staff, W.W. Stonard, who succeeded him in 1947 and represented the end of what might be called the Chatterton's compound era, the grave F.W. Etheridge and the ebullient J.J. Milligan. The varied skills of these four allied to a few versatile and high quality machines, saw through the increasingly demanding requirement of the developing research programmes. The new Clarendon Laboratory secured a workshop of, for the time, very generous proportions and A.H. Bodle at the worst possible time, early 1940, was able to fill it with some machines of high quality which were to prove their worth in the days before the generous equipment grants started in the early ’60s.

Soon after the end of the Second War, we had a staff of nine in the machine shop forming part of the new Clarendon Laboratory and these had been augmented by Admiralty staff supporting the radar work. Townsend’s workshop (two technicians) was by now part of the Clarendon Laboratory and into this were moved the six Tube Alloys technicians from the Jesus College Laboratory.

In 1962 the Department was able to pull off an administrative coup which would have been impossible later. The increasing microwave work was calling for machining standards to an accuracy hitherto unknown. The chief expert at
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this work, who brought to it a scientist’s academic understanding of the physics, was E.J. Jenkinson[88] who had come to us after the Second War from Boscombe Down. It seemed a good idea to make him and W.W. Stonard joint heads of what had become known as the Lindemann Workshop. Unbelievably, the University fell in with the idea and this happy arrangement persisted for six years. Meanwhile, E.R. Tilbury had succeeded J.J. Milligan as head of the Low Temperature Workshop in the Townsend Building and it was his inventiveness and careful techniques which resulted in the many cryostats and the two liquefiers (see below) of the twenty years around 1955-75.

The essential work of getting the new building going in 1939 was put in the very capable hands of G.E. Topp, a one-time London bus driver who had for some years been Lindemann’s chauffeur. He secured T.H. Carr from the staff of Lowe and Oliver, who had from time to time seen through major electrical projects in the ’30s. With a number of assistants, notably K.S. Rumble, today’s successor, they organized the installation of the major projects of the nuclear physics and low temperature groups. (T.H. Carr’s important contribution to the 2MW generator has been mentioned on p.[174].) G.E. Topp also brought about one of the Clarendon Laboratory’s unique ventures, a small team of builders under the vigorous leadership of A.G. Humphries, who over the years carried out major internal building operations in Old Physiology, Old Zoology and houses in Keble Road and Parks Road.

Thus, the Clarendon Laboratory was able to call upon a considerable work force at a time when it was most needed – these were very much the days of “DIY” and the successes of 1952-6 owed much to both the quality and number of technical staff. On the other hand, we were light on research techniques and in electronics. G. Frater struggled manfully with the latter but unhappily died in 1960 when we were quite soon able to assemble a new generation of up-to-date electronics specialists.

The concept of a Research Workshop has moved on very considerably over forty years. There was a time when a very few research workers were
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grudgingly allowed to make occasional use of the superannuated lathe. But in 1953 it was decided to make the instruction of users and the care of a larger research workshop a job for one of our very best instrument makers. The first incumbent was F.W. Etheridge on whose

[88] Awarded Hon. MA 1978 after thirty-two years’ service

retirement in 1955 we installed the equally patient H.S. Lock[89]. In the middle '60s it was possible to open up a much larger research workshop in the basement and on occasion – and unofficially – rush jobs have been pushed through by research students who have been coached to a high standard.

In the forty years since the Second War, we have seen two cycles of private pay scales to national pay scales and back again. 1972 saw the end of these oscillations and the national scales which we are now tied to do at least reflect inflation to some extent. There were periods when the University's salaries fell behind those of Harwell, when there was a technician exodus but sooner or later they picked up again, when the flow was reversed.

During the '50s the need was growing up for laboratory technologists – people who could keep the increasingly complicated technical services going for research students to draw upon. The mass spectrometer leak detector is an obvious example. The University was able in the late 1950s to attract well-qualified school leavers who, after some five years' part-time study, could attain a recognized qualification. But the University had no salary scales for staff on their way up the examination ladder nor positions to offer them when qualified and, consequently, the promising ones moved off to AERE Harwell and the like. Professor Bleaney attacked the problem with vigour and soon the University formed a small committee with him as Chairman. In a few months a new category of Departmental Research Assistant was formed and suitably qualified technicians were transferred into it. The Department soon had many DRAs at the three levels established and the technical requirements of electronics, cryogenics, crystal-growing, thin optical film production and high magnetic field technology were being mastered to a very high degree of competence.
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Liquid refrigerants

After the Second War the old hydrogen liquefier was found to be too corroded to be used and a team led by G.O. Jones quickly got a new one in operation (35.1). Simon had managed to acquire a sizeable hydrogen compressor of the transportable kind used for balloon barrages. The electrolysis plant was enlarged and, by 1947, sufficient liquid hydrogen was available to enable several groups with liquefier cryostats to run experiments at liquid helium temperatures and below. Realizing that there would soon be a demand for "free" liquid helium, Simon took on two physicists interested in practicalities and set them to build liquefiers from which the liquid helium could be drawn off – G.K. White to make a Linde liquefier and the writer a Simon liquefier. One of the basic axioms was that unacceptable losses had been experienced in other laboratories when liquid helium was transferred through an inverted U transfer line, popularly though incorrectly known as a siphon. It was not possible to use glass dewar vessels and the internal parts of the liquefiers therefore had to be in high vacuum. In those days constructional techniques for leak-free components at low temperature had not yet been learnt and the choice of materials was very limited – adequate stainless steels for example did not come into use for another fifteen years and we were without leak detectors. Both liquefiers just about functioned but clearly the thing to do was to make the best possible siphon (35.2) and try it. This was done and the losses found to be trivial. Now extraction of the liquid from the top was possible and we could return to the use of large glass dewar vessels. A small Linde helium liquefier was established (35.3) and a Simon expansion liquefier (35.4), which produced one shot of one litre of liquid in about an hour. In those days it was usual for at least three experiments to share one litre of liquid helium and terrifying was the strife if one of them took more than the allocation. These liquefiers were clearly unsuited to the more intensive operation

[89] Awarded Hon. MA after fifty-two years' service

expected and there was discussion in the early 1950s about how future needs were to be met.
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> It was an occasion when there was an example of the tendency of F.E. Simon to allow emotion to compete with practicalities. The Simon liquefier is excellent when quantities are small, and time and thermal efficiency are unimportant. The large Simon liquefier which we had running was demonstrably thirsty for liquid hydrogen - for example, when one was expanding, air from the atmosphere in the room could be seen condensing on the helium pipe and dripping off. (Visitors used to think we had a leak.) The management of the system called for close attention by an experienced person. These and other arguments weighed little with Simon. He wanted a bigger one and not a Linde liquefier – the expansion liquefier was "ours" and the writer was being pig-headed. (It was at about this time that F.E. Simon lost interest in the possibility of our raising the money for a Collins liquefier, their representative having advised him that an establishment of our size would need two because their reliability was not then as high as it became later.) But in 1955 opposition suddenly crumbled and there was enthusiasm to build the Linde liquefier which survived into the turbine age (1979).

Strange as it now seems, the Clarendon Laboratory had to rely upon bulk supplies of liquid oxygen up to 1961. Liquid nitrogen was quite unobtainable. A plant (35.5) was made to condense atmospheric air using liquid oxygen but, owing to the tendency of the nitrogen to boil off preferentially, we had continuing trouble from explosions in the exhaust of rotary vacuum pumps from oil vapour being ignited by the oxygen-rich mixture being pumped.

> The vacuum pump on the large Simon helium liquefier suffered explosions at its exhaust so regularly that F.E. Simon asked whether we could not dispense with its motor and use the wasted chemical energy to drive the pump.

This was the primary cause of an explosion and fire in the 1946 hydrogen liquefier which occurred in April 1953. (See Fig. 49 in which Prof. Simon is talking to the writer.) The operator, A.Hazel, fortunately escaped with only slight permanent damage to his eyesight.

However, by 1961 the demands of the steel and food industries changed the position and it was suddenly possible to buy liquid nitrogen by the ton. The changing scale of the operations can be judged from the capacities of the original home-made liquid oxygen vessel at 400 litres (35.6) to the present liquid nitrogen vessel installed in 1961 which holds 5,500 litres. The original indoor helium gasholder held the equivalent of one litre: we now manage with one holding 20 litres but only with automatic pump-down. From a few litres per week thirty-five years ago, we now on occasion produce and use 2,000 litres.
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Liquid nitrogen has its triple point at 63K – considerably higher than that of the liquid air which we had been using for our hydrogen liquefier. This would have given us a yield too low to keep our helium liquefier going. We, therefore, decided to build a new hydrogen liquefier and this was done at little expense and without undue inconvenience (35.7). It incorporated some interesting automation and later an overhead liquid hydrogen transfer line to the helium liquefier was added (35.8), and the whole system automated. The hydrogen liquefier ran for the next twenty years and for a time is believed to have been the only one in the UK. In 1966 the helium liquefier was fitted with a larger compressor and one of the heat exchangers was replaced by a new type (35.9) so as to give a continuous liquefaction rate of 15 litres per hour into an internal liquid helium vessel by means of a servo system. We thus had a completely automatic system which it was a joy to watch working.

In the middle 1960s, a fleet of fifteen 50-litre gas-cooled super-insulated liquid helium vessels (AMANDA, BELINDA, CHRISTINA, et seq) came into use with hydraulic lifting trolleys to deliver liquid helium round the laboratory. These are still going strong after 20 years with re-pumping every 5 years. Six have been repaired after more or less gross mechanical damage. The makers, BOC Ltd, put a life expectancy of only 5 years on them!

There was, since the early 1960s, a change in internal policy away from "do-it-yourself" to reliance on commercial sources of equipment. This was due to the coming of Equipment Grants and, although ordinary money was getting very tight in the late '70s, it was possible by emptying every available coffer and, with a generous contribution from the Science Research Council, to find the £80K required for a turbine liquefier in 1979. (On learning that there would be a saving of £2000 p.a., T.C. Keeley observed "It'll pay for itself in 40 years then".)

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Spin-off

The Clarendon Laboratory had none of the early distinction of the Cavendish in the establishing of successful local firms – the once-famous Cambridge Instrument Company and the still familiar name Pye are obvious examples. We have seen that the original Oxford Instrument Company (p.[80]) was set up in a small way between the wars but with the emergence as a prosperous and highly successful business the new Oxford Instrument Company and its satellites have pushed Oxford technology into the limelight. M.F. Wood[90], who came to the Clarendon Laboratory in 1956 to build high-field magnets moved on from satisfying the needs of a few physics research laboratories in his spare time to leaving the University in 1961 to develop his business interests.

On a smaller scale, the Littlemore Scientific Engineering Company has flourished under E.T. Hall (see p.[204]) and Telsec, now Telsec Process Analysis Ltd of Peterborough, under H.J. Lucas-Tooth[91]. There have, of course, been other small engineering firms run by former Clarendon Laboratory staff and the total number of these stands at six. In recent years though not within the limits of this book we have seen Crystalox, Oxford Lasers and Solid State Logic.

Social life

It was always the Prof's policy to try to get as many as possible of the Arts members of the University to see what went on in his laboratory and to that end he arranged for Open Days in the summers of 1949 and 1954. With the prospect of wine on the (then) flat roof, substantial numbers turned up and most were impressed.

Somehow it was usually Simon's group who got parties going and this was certainly so in the case of the Christmas party. Fig. 50 shows the Prof and T.C. Keeley putting in an appearance. At one of these there was a poster headed "SIMON TEMPERATURE SCALE" and below a mercury thermometer with three points marked in descending order CAP POINT, under it MUFFLER POINT and GOING HOME POINT. Lady Simon was sent to ask for it next day.

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[90] OBE 1982, Knighted 1986, FRS 1987

[91] Sir John Munro-Lucas-Tooth, Bart, 1986

The Clarendon Laboratory has for some 35 years run a cricket club which has usually played the Cavendish Laboratory. The opening stand of 51 by B. Bleaney and R. Berman stood for some 20 years.

Another survivor has been the singing of carols at Christmas in Wadham College Chapel – first suggested by E. English who combined instrument making with being a church organist. The first was held in Keble College but Wadham College had closer ties. One year we were able to raise a semblance of an orchestra.

The design of the 1939 buildings had never provided for drinking tea or coffee whether by the staff or by the academics. The room next to the Library (room 120) served during the Second War and, for a time, the original Small Lecture Room (room 103) was used. But for the next fifteen years the strange procedure ruled that we recruited ourselves from a trolley on the first floor bridge between the buildings and stood about in this near-one-dimensional space. (Our primitive resources are recognizable in The Sapphire Conference (35.9).)

It became possible in 1964 to retire the trolleys and open a hatch into the bridge by converting a battery-charging hut into a kitchen. In recent years a common room has been built over part of the Lindemann Workshop and the kitchen rotated clockwise by one right-angle.

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References

Chap. 35

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PART VII

Chap. 36 Changes at the Top

Retirement of the Prof

It is perhaps never wise to take too much interest in one's successor but when, after 37 years, Lord Cherwell decided to retire in 1956 at the age of 70 – the age provided for in the old Statutes – he naturally felt moved to use his considerable influence in the best interests of the Clarendon Laboratory. Although Lindemann had opted for life tenure without pension, he retired at the due age under the retirement-with-pension option. This was typical of his extreme punctiliousness in such matters. Unfortunately, he made a miscalculation which was to cause something of a rift between him and F.E. Simon. He thought that Simon was so well placed with his own quarters and virtually independent that he would have no wish to occupy the Dr Lee's Chair. In this he was quite wrong. "He has lost interest in physics for many years. Why does he now want to appoint his successor?" Simon must have known what was happening at Cambridge. N.F. Mott had moved to Cambridge from Bristol in 1954 and had lost no time in closing down nuclear physics in the Cavendish Laboratory. This left D.H. Wilkinson high and dry and a move to Oxford would have solved two problems. Lord Cherwell could not have been at all sure that the evolutions that actually took place would work out as they did. However, in the event, Simon stepped into the Dr Lee's Professorship on 1 October 1956, though sadly only to die by the end of the month.

Lord Cherwell stayed on in Christ Church and seemed very much himself but died in August 1957, partly as a result of having contracted diabetes for which he refused treatment. His funeral was in Christ Church and attended by Winston Churchill, then aged 83, according to Cranmer's liturgy and followed by burial in Wolvercote cemetery (in the angle between Five Mile Drive and the Banbury Road).

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People often asked what was the Prof really like. One answer was that it depended on the time of day: pre-7.00 pm he could seem aloof and arrogant, after 7.00 pm relaxed and genial. It may be more to the point that he was one of the fairly small proportion of people who, far from betraying their natures, actually give quite unconsciously a contrary indication. We had such a one among us for more than 30 years in the shape of Bernard Rollin, a man who went about looking totally pleased with life and he certainly had every reason for being so. That the reverse was the case was known to his few intimates and was proved by the sad manner of his death. Cherwell was in the opposite case. All the evidence is that his was a gentle nature but, for reasons perhaps deriving from his unhappy home life, he wore the stern patrician countenance that puts people off and perhaps provokes them to say silly things which earn a sharp retort, if they get one at all.

> Talking to a lot of Oxford people about the Prof, the writer has heard no instances of ill temper, scorn, sarcasm or the like. One must remember the unexpected geniality with which a report that a master-key had gone missing was received. There are many stories of unobtrusive rescue of employees in personal straits. That he has been known to put himself out for a trapped bird is at first

surprising, until one remembers that he was brought up in the country. No-one who knew the Prof in Oxford can doubt his real nature but it remains a puzzle why he bothered to be so successful at concealing it.

On the death of Sir Francis Simon after just a month in office, the Electors were probably able to exclude any claims from outsiders – when an institution is booming, as the Clarendon Laboratory had been, one looks for a local. On the other hand, they must have had to reconcile the claims of the generations – the contemporaries of F.E. Simon or his pupils. In the event, the wisdom of the election of Brebis Bleaney was self-evident to all and a delight to most. He was 42 at the time, had been a fellow of St John's for ten years and had been FRS since 1950. His command in the field of microwave spectroscopy was already legendary, with plenty of unconquered territory ahead. But above all, was his contribution to the nuclear orientation success of 1952.

The financial climate when BB took office was buoyant – his problems were how to provide for the rising tide in the number of physics undergraduates. Sixty-one physicists had graduated in 1955 and by 1960 the number was 138.
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Expansion continued in later years but more slowly – by 1975 there were 160. These were the years immediately before and following the Robbins Report.

Meanwhile, physics had been blessed with the same expansionist spirit in the high levels of University administration. In 1959 was the foundation of a third chair, the Professorship of Experimental Physics, tenable with a Studentship (fellowship) of Christ Church. (This in effect made statutory the <u>ad hominem</u> title of professor accorded to F.E. Simon.) The election to the new chair must have been something of a formality. D.H. Wilkinson of the Cavendish Laboratory, who had been in Oxford since 1957 in an unestablished professorship, had been made FRS in 1956 after distinguished work on the photo-disintegration of the deuteron, the giant dipole resonance and the early application of the new concept of isotopic spin to the structure of light nuclei.

By 1976 the increasing lack of money was making itself felt but the Clarendon Laboratory was steaming along confidently with nothing worse in view that the retirement of A.H. Cooke in 1980. Then we received two nasty jolts. As we saw, Brebis Bleaney had had the idea of enhanced nuclear magnetism in 1973 and three years later it spectacularly worked. With the aid of a Warren Fellowship* from the Royal Society he found that he could afford to take early retirement from his university post and he resigned in 1977 after a year's sabbatical leave. The usual conventions about not working in your old department were relaxed to the extent of allowing him to occupy an office in the Old Zoology building and creep in from time to time to experiment inconspicuously in Inner Halbania (see p.[105]). When we saw how the years rolled off him in subsequent months we all realized what a thoroughly good thing this unexpected move was.

But there was another shock to come – Arthur Cooke was elected Warden of New College with effect from 1976 when Sir William Hayter retired. Of course, physicists know that they are uniquely gifted for nearly every alternative occupation and certainly for managing a large college. But when a collection of assorted academics take the same view, it says something about the person elected. The journal <u>Oxford</u> described the future Warden as "deft,
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witty and brisk", and this very accurately describes the man whom we were saying goodbye to as an outstanding physicist of his time.

In the event, Warden Cooke held office for nine years and the satisfaction in the college with the success of electing one of their own number to be Warden was reflected in the choice of another, H. McGregor.

J.H. Sanders was appointed Deputy Professor for the year 1976-77 and for a further year as Acting Professor.

The Electoral Board in 1976 consisted of Dr R.E. Richards, FRS[92], Chairman, as deputy for the Vice-Chancellor, Stuart Hampshire, Warden of Wadham College, D.T. Edmonds, Fellow of Wadham College, Prof. D.E. Blackwell, Savilian Professor of Astronomy, Prof R.J. Elliott, FRS, Wykeham Professor of Theoretical Physics, Sir Denys Wilkinson, FRS, Vice-Chancellor, University of Sussex and Sir Brian Pippard, FRS, Cavendish Professor of Physics. The view must have been that low-temperature physics was played out although a few present and former members of the Clarendon Laboratory have made distinguished contributions to the field in the last decade. However, at the time no one could have argued that solid state physics had become the field in which our present and future strength lay. In particular, the application of laser physics to solid-state experimental work was wide open to us. The front runner was unhappily committed to seeing out a short-term job on the Continent and a proposed union with another Oxford professorship fell through, partly because of the recent death of a relatively useful science professor. The three FRS members of the Electoral Board were then seized with the idea that Bill Mitchell of Reading University might be the ideal man – a solid-state physicist through and through with outside administrative experience in his having been (part time) Chairman of the Physics Committee of the Science Research Council. It is too soon to go further into the pros and cons of this as seen at the time but one important legislative consequence arose – all scientific Electoral Boards were augmented by two members drawn
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from the departments which included a laboratory. Thus it can no longer happen that members plus the Chairman (usually the Vice-Chancellor) can outvote three members plus the two departmental members. Time will tell.

Mitchell had been a lecturer at Reading University since 1951 and a professor since 1961. His papers, numbered 66 (counting up to his appointment in Oxford in 1978): semiconductors 23, dielectrics 20, radiation damage (defects) 10, neutron scattering 7, metals 1, techniques 5.

[92] Warden of Merton College, 1969-84, Dr Lee’s Professor of Chemistry 1964-1970 and subsequently Vice-Chancellor 1977-81.

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PART VII

Chap. 37 Numbers and Structure, 1919-1978

The near total eclipse of physics in the First War has already been noted - R.B. Clifton having clocked up fifty years finally retired in 1915 and James Walker saw to the instruction of one or two undergraduates in the Clarendon Laboratory. We have seen that Lindemann's first few months as Professor led to an astonishing flowering of many lines of research (p.[77-78]) but how did the undergraduate numbers build up?

Between the Wars

The numbers of physics undergraduates were 20 ± 8 through the interwar years and the numbers of permanent academic staff teaching them were in the Clarendon Laboratory 5 ± 2 and in the Electrical Laboratory 6 ± 2. (These figures include Professors and Readers who have relatively heavy lecturing and sometimes demonstrating obligations but no responsibility for college teaching.)

After the Second War

The numbers of graduates after the Second War were at once some three times the pre-war figure. It must be admitted that the Professor was slow to press the University for more permanent academic appointments. It was C.H. Collie who bullied him into making a demarche in 1947; its effect can be seen in Fig. 5[1].

The coming of Theoretical Physics was met in 1952 by the introduction of a special examination paper, an indication on the Class Lists and the remission of the obligation to attend the practical course in one's last year.

The obligation to attend practical classes has always been taken seriously in Oxford and the mixed squads of demonstrators – senior permanent academic staff alongside research students – have been of the greatest value to both sides. Over the years there has been an increasing tendency to favour experiments which put over their message economically – a few difficult
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experiments are all very well but in the 1950s there were perhaps too many of them.

Before 1971 undergraduates did three or two days practical work per week but after that only in four weeks per term. This left more time for library reading and solved incidentally the space problem brought about by rising numbers and failure, because of Government cuts, of plans for new buildings in Keble Road.

Research degrees

Until just after the First War the only research degree was Bachelor of Science (minimum two years) converted recently to Master of Science. But the Rhodes Trustees were among the early proposers of a degree comparable with those offered by many other universities and the Doctor of Philosophy degree (minimum three years) was first conferred in 1921. (The D.Sc. had been established in 1900 for grandees with a lifetime's accumulation of published work, on the same lines as the D.Litt.) The B.Sc./M.Sc. degree became a rarity when government support for D.Phil. courses came in in the late 20s.

Decentralization

When Lord Cherwell retired in 1956 his Clarendon Laboratory embraced all topics in physics except astrophysics. This happy state of affairs was doomed to come to an end, firstly because new nuclear physics was certain to call for new apparatus in specially designed buildings. We have seen that a Department of Nuclear Physics was formed (see p.[107]) in new specialized buildings at the western end of Keble Road. Theoretical Physics moved out into 12-14 Parks Road and a separate department was created for Prof. R.E. Peierls in 1964. Atmospheric Physics too split off and in 1970 moved into the old Zoology Laboratory immediately to the south [of] the Townsend Building. This centrifugal tendency was well suited to those heady days of relative affluence but it is not surprising to find twenty-five years later that concentration has become the pattern for the future.

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PART VII

Chap 38 Cambridge

It is a truism that procedures and practices at Oxford and Cambridge, whether past or present, either run on parallel lines or are poles apart. Familiar examples are the permissibility or not of persons being simultaneously Head of a college and professor in a University department and the policies regarding taking books out of University libraries.

It is an unfortunate fact of life that the Cavendish Laboratory has spawned (up to 1985) seventeen Nobel Prize winners while the score at Oxford remains obstinately at zero. The tremendous rise of Cambridge physics in the first half of this century was, of course, largely due to J.J. Thomson and E. Rutherford. But it is possible to identify two pieces of sleight of hand which sped them on their way. In 1895 Cambridge University passed legislation which allowed graduates of other universities to be given a Cambridge degree automatically after two years' work as a post-graduate student and it was by this arrangement that Rutherford and Townsend were able to start work in the Cavendish Laboratory in 1896. It has been said that J.J. Thomson opened the doorway to the new physics with his work on the electron but it was Rutherford who went through it. The succession would never have come about without manoeuvring in 1919 which amounted to the bamboozling of J.J. Thomson. The outcome was that Rutherford succeeded him as Cavendish Professor and Thomson became Master of Trinity College with rights to space but no authority in the Cavendish Laboratory.

But going back to 1850, the position of physics in Cambridge looked quite different – for a time not much less inglorious than at Oxford. When the Natural Science Tripos was established in 1851, physics did not appear as a subject and it was not until 1860 that it did and then as a branch of chemistry. In 1876 chemistry appeared as a branch of physics and this strange position was put straight in Part 1 in 1883. There were continued heart-searchings in Cambridge about the relative merits of width and depth

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and in 1851 width was so much in vogue that it is fairly certain that candidates were expected to answer questions from six subjects, viz. anatomy, physiology, botany, geology, mineralogy and chemistry. In 1854 one candidate certainly achieved distinction in 5 subjects. The width became eaten away in the course of many fiercely contested debates but it was not until 1902 that physics became a single subject by a vote of 76 to 73.

How then did Cambridge physics get into the decisive lead? The answer of course is that mathematics at Cambridge had a closely similar place to Literae Humaniores in Oxford over the first half of the nineteenth century back to the time of Newton. It is not generally realized that before 1827 it was not possible to qualify for a degree by reading classics at Cambridge – "The Tripos" was the Mathematical Tripos of later years. The consequence was that the disparity between the annual number of Wranglers at Cambridge and Class 1s in mathematics at Oxford was of the order of – to take 1860 as an example – 38 to 1. It was therefore likely, merely on statistical grounds, that when the problems arising from physical discoveries proved to require mathematics for their solution it would be Cambridge mathematicians who solved them. In the event, the first three Cavendish Professors were either Senior or Second Wranglers – J. Clerk Maxwell (Second in 1854), W. Strutt, later Lord Rayleigh (Senior in 1865) and J.J. Thomson (Second in 1880) as were Stokes (Senior in 1841) and W. Thomson, later Lord Kelvin (Second in 1845), although it is said that they had to be taught a deal of practical physics. The rationale of this strange story is that from the 1830s as discoveries in physics were made they were incorporated into the mathematics syllabus until 1849 and 1850 when there was a comprehensive purification by Whewell. (It was while this was in force that Clifton took his Tripos in 1859 – see p.[34]). Largely as a result of an initiative by Maxwell in 1866, questions involving physics reappeared in the Mathematical Tripos. (Both at Cambridge and at Oxford it seems that there was uncertainty about the nature of physics and it was a few decades before it was accepted that it is essentially a mathematics-based subject and not, as it had originally seemed, a descriptive subject.)

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<u>Appendix A</u> – THE OXFORD DRY PILE

Our dry pile (See Fig. [52]) ceaselessly ringing its bells for more than 170 years, is the subject of a steady stream of enquiries from all parts of the world and we are kept busy explaining that there is no perpetual motion, that it will one day stop, that the voltage is about 2KV, the current about 1 nano-ampere and that if it really were dry it wouldn't work. Its internal constitution is unknown but we think it likely that it follows Singer's recipe embodying about 2000 pairs of discs of zinc and "silvered" paper.

We do not know when or where it was made but it is closely similar to an illustration in <u>Elements of Electricity and Electro-Chemistry</u> by G.J. Singer, published in London in 1814 and thought to be the first text book on electricity. An important difference is that our dry pile has a coating of molten sulphur and it is this which seals in the right amount of water to provide the electrolyte without causing short-circuiting or unwanted chemical action.

The dry pile was important in its time because it showed that the high voltage electrostatic experiments and the low-voltage experiments of Volta were in reality concerned with the same phenomenon. Over the 15 years from 1800 workers in many countries were trying out a great variety of recipes and reporting on the role of humidity. Contemporary sources point to Jean de Luc, FRS, as the first in the field. (Nicholson's Journal for June 1810 states that he was experimenting in 1800.) His work was first published in his <u>Traité Elémentaire sur le Fluide Electrico-Galvanique</u>, pub. Paris and Milan 1804. Other notable workers in the field were Maréchaux, Behrens and Zamboni and exhaustive accounts of their research appear in Gilbert's <u>Annalen</u> over the years 1803-20. Interest was not confined to inorganic substances and there are accounts of the performance – usually but not always negative – of piles made of slices of such substances as walnut wood, beetroot, radish etc.

The Oxford dry pile played a curious part in the scientific history of the 1939-45 war. An infra-red telescope using an image converter tube with a lead sulphide cathode - later to be succeeded by lead telluride – was developed in the Admiralty Research Laboratory and this called for a portable battery
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giving about 3KV at a very low current. Dr A. Elliott, an Oxford physicist, remembering the dry pile in the Clarendon Laboratory, followed the recipe given by Charles E. Benham in the <u>English Mechanic</u>, February 1915, and a considerable number were produced.

Meanwhile, the activity of our dry pile continues and on present form the clapper seems more likely to wear out than the electro-chemical energy driving it.

In an extant MS letter to Robert Walker dated 1 April 1841 Francis Watkins, of the firm of instrument makers in Charing Cross, Watkins and Hill, writes "I fear the de Luc's columns mounted on bells will not be of any service in showing the alternate attraction and repulsion of the clapper – the residual electrical power sufficient to keep up the ringing of the bells seldom lasts longer than three or four years and I should conclude your apparatus was constructed by Singer, who has been dead more than <u>20</u> years … Will it not make a show among your Electrical apparatus, it is a pretty apparatus but alas very transient in its working powers … If you would like to have the de Luc's columns back …"

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Appendix B – transcribed from a privately printed pamphlet dated 6 February 1868[93]

Notes by an Oxford Chiel

"A Chiel's amang ye taking notes
And, faith, he'll prent it

Facts, figures and fancies, relating to
The Elections of the Hebdomadal Council
and Offer of the Clarendon Trustees
and The Proposal to convert the Parks into Cricket Grounds
……….

THE OFFER
of
THE CLARENDON TRUSTEES

'Accommodated: that is, when a man is, as they say, accommodated or when a man is – being – whereby – he may be thought to be accommodated, which is an excellent thing.'

DEAR SENIOR CENSOR

In a desultory conversation on a point connected with the dinner at our high table, you incidentally remarked to me that lobster sauce, 'though a necessary adjunct to turbot, was not entirely wholesome'.

It is entirely unwholesome. I never ask for it without reluctance: I never take a second spoonful without a feeling of apprehension on the subject of possible night-mare. This naturally brings me to the subject of Mathematicus and of the accommodation provided by the University for carrying on the calculations necessary in that important branch of Science.

As Members of Convocation are called upon (whether personally, or, as is less exasperating, by letter) to consider the offer of the Clarendon Trustees, as well as every other subject of human, or inhuman, interest, capable of consideration, it has occurred to me to suggest for your consideration how desirable roofed buildings are for carrying on mathematical calculations: in fact, the variable character of the weather in Oxford renders it highly inexpedient to attempt much occupation of a sedentary nature in the open air.
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Again, it is often impossible for students to carry on accurate mathematical calculations in close contiguity to one another, owing to their mutual interference, and a tendency to general conversation: consequently these processes require different rooms in which irrepressible conversationists, who are found to occur in every branch of Society, might be carefully and permanently fixed.

It may be sufficient for the present to enumerate the following requisites: others might be added as funds permitted.

[93] Bodl. Library G.A. Oxon 8° 161, 23

A. A very large room for calculating Greatest Common Measure. To this a small one might be attached for Least Common Multiple: this, however, might be dispensed with.
B. A piece of open ground for keeping Roots and practising their extraction: it would be advisable to keep Square Roots by themselves as their corners are apt to damage others.
C. A room for reducing Fractions to their Lowest Terms. This should be provided with a cellar for keeping the Lowest Terms when found, which might also be available to the general body of Undergraduates, for the purpose of 'keeping Terms'.
D. A large room, which might be darkened, and fitted up with a magic lantern, for the purpose of exhibiting Circulating Decimals in the act of circulation. This might also contain cupboards fitted with glass doors, for keeping the various Scales of Notation.
E. A narrow strip of ground, railed off and carefully levelled, for investigating the properties of Asymptotes, and testing practically whether parallel lines meet or not: for this purpose, it should reach, to use the expressive language of Euclid, 'ever so far'.

This last process of 'continually producing the Lines' may require centuries or more: but such a period though long in the life of an individual, is as nothing in the life of the University.

As Photography is now very much employed in recording human expressions, and might possibly be adapted to Algebraical Expressions, a small photographic room would be desirable, both for general use and for representing the various
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phenomena of Gravity, Disturbance of Equilibrium, Resolution and c, which affect the features during severe mathematical operation.

May I trust that you will give your immediate attention to this most important subject.

Believe me,
Sincerely yours,
MATHEMATICUS

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Appendix C Contributed by Prof. F. Llewellyn Jones, Professor of Physics, University of Wales and Head of Department of Physics, University College, Swansea, 1945-65, Vice-Chancellor 1969-71, Principal 1959-60; 1965-74

Distribution Functions in the Monatomic Gases

I In ideal Maxwell-Boltzmann gases, with no inelastic collisions and no external field, all energy gains or losses occur only at collisions. At constant temperature, the rate of gain of any group of atoms = rate of loss on the average; equipartition or statistical equilibrium occurs and the energy distribution is Maxwellian. In its non-dimensional form, where y = Ex/E1, where Ex is the energy of a group dn, when the mean of the total number of atoms N is E1, the distribution is expressed:

$$dn/N = A.y^{1/2}\exp - 3y/2dy \qquad (1)$$

This probably also holds at very high temperatures even with inelastic collisions, provided all radiation is still absorbed in the plasma.

II Consider the case when the two species of "gas" considered are the cloud of electrons moving through a cloud of ideal gas atoms under an external electric field Ex, and the mean energy of the electrons is much greater (say > 100) than that of the ideal gas atoms, which may then be regarded as stationary. In this case, there is a mean average fractional loss of energy per collision of the light m electrons to the M heavy atoms (~ 2m/M). However, there is also a mean gain of energy by the electrons through moving down the field Ex; the gain being eExl, where l is the free path, per collision ($\simeq$ eEx/α where α is the cross-section).

Equilibrium occurs when these two averages are equal and a steady energy distribution function is set up. This was first realized and stated independently by Townsend (Ap. 1) and by M.J. Druyvesteyn (Ap. 2). However, Townsend worked it out only approximately, getting

$$dn/N = \pi^{-1/2}\exp\{-(y-0.85)^2\}\,dy$$

while Druyvesteyn gave the exact and correct formula:

$$dn/N = 1.04y^{1/2}\exp(-0.55y^2)\,dy. \qquad [(2)]$$

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The most elegant and useful proof is due to P.M. Davidson ([ ]) which readily permits the variation of cross-section with electron energy to be taken into account.

Note that the y^2 in the exponent gives a narrower form of the distribution function than the Maxwellian. Also, this form holds only provided l (or α) is constant, independent of Ex. When this is the case, all gases have the same distribution function when inelastic collisions can be neglected. With mean electron energies ≤ 6eV, as in glow discharges in wide tubes at low pressures in cold gases, inelastic losses can be ignored compared with the more numerous elastic losses (2m/M).

III In general, two effects change this conclusion:

i) when the mean electron energies are high, ≳10eV, or at high gas temperatures, inelastic collision may not be neglected, and

ii) any variation of elastic cross-section with electron energy greatly changes the rate of elastic losses as well as the rate of gain from the external field. The form of energy balance as f(E) is then changed and the final solution does not give the exact Druyvesteyn form (2) above). This is best seen from the Davidson proof. Integration of equation (2.30) p.28 of the Monograph (Ap. 3) now will depend on the precise form of the cross-section as a function of and [sic] energy = f(E).

Experiment and theory have given these functions for most atoms, and since they are different for different gases, then no two gases will have exactly the same distribution function in low pressure glow discharges, because of the Townsend-Ramsauer Effect giving variation of cross-section with energy.

IV Electron distribution in Helium

[Here Llewellyn Jones inserts a graph of α(E) against E.]

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This gas is interesting because of the particular shape of the {α(E), E} curve. Up to about 4 eV, [α(E)] is fairly constant but afterwards [α(E)] is nearly inversely proportional to E.

Thus, from equation 2.3 (Ap.3) the slower electrons with energies around the mean energy, which incidentally are about >80% of the total number, would tend to have a distribution not too far from the Druyvesteyn in that region. On the other hand, the higher energy electrons >15eV (while only being a small fraction of the total, nevertheless are responsible for all the inelastic losses of excitations and ionization) have a distribution determined by the increase of free path with energy.

Formally, this can be seen easily by substituting

l = lo/u u = electron speed

in equation (2.30) (Ap. 3) and then integrating. This gives a form for this group of high energy electrons

$dn/N = 1.\ y^{1/2} \exp -Dy\ dy$

where the exponent is now y and not y^2 so this form is similar to Maxwell's. Thus, the form of the energy distribution curve as far as the higher energy electrons is concerned <u>appears</u> to be Maxwellian.

The complete distribution function is, of course, not Maxwellian, as the shape near the mean energy is broader. Thus, properties dependent on the majority of electrons, such as transport processes, would be dependent on these slower electrons because they are in the vast majority. On the other hand, properties dependent only on the higher energy electrons, such as excitation and ionization, are determined by the small fraction whose distribution is similar in form to the "tails" of the Maxwellian distribution (Ap. 4). This does <u>not</u> demonstrate equipartition but only the operation of the Townsend-Ramsauer Effect.

Based on experimental data on spectral line intensities and mobilities, an empirical formula can be found for the <u>full</u> distribution in helium (Ap. 5).

<u>Case of Neon</u>

In this gas, the cross-section for many purposes can be regarded as constant, so that the distribution function is practically the Druyvesteyn form in glow discharges.

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<u>Argon</u>

Here the Townsend-Ramsauer Effect is very pronounced (it was for this gas that the Effect was first formulated!). There is a very sharp minimum of α = f(E), so that the electron energies tend to this value, giving a very narrow distribution. In fact, for many purposes one can take a constant value E1 for all the electrons.

<u>General case</u>

In general, with an assembly of different species, with possibly complicated reactions involving inelastic collisions, exact calculations of distribution functions must take into account all inelastic losses. When the cross-sections are known and the values put into the generalized Boltzmann Equation of continuity, such calculations are relevant in studies of high-energy, high density plasma, as well as for stars, etc. and this accounts for the modern strong interest in formal and numerical methods of solution of the Boltzmann Equation, as discussed in the NATO Advanced Study Institute 1981 (Ap. 6).

Historically, this question of distribution during the years prior to 1934 was almost completely ignored outside Oxford. Langmuir did not indicate that his probe depended on the form of the distribution, and calculations on positive columns at that time all took the Maxwell form as natural. Since then many writers assume a Maxwellian form just because it can readily be integrated formally in the collisional equation!

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References

Appendix C

Ap. 1 J.S.E. Townsend, Phil.Mag. (1930), 9, p. 1145

Ap. 2 M.J. Druyvensteyn, Physica (1930), 10, p. 61

Ap. 3 F. Llewellyn Jones, Ionization and Breakdown in Gases, London: Methuen Monographs, 2nd Ed. (1966), p. 27-29

Ap. 4 J.S.E. Townsend and F. Llewellyn Jones, Phil.Mag. (1931) XII, p. 815 and F. Llewellyn Jones, ibid, XV, (1933), p. 958

Ap. 5 F. Llewellyn Jones, Proc.Phys.Soc., LVI (1944), p. 239

Ap. 6 Electrical Breakdown and Discharges in Gases, Pt. A., Fundamental Processes, Ed. E.E. Kunhardt and L.H. Luessen, NATO ASI Series, New York: Plenum Press and NATO Scientific Affairs Division, 1983

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<u>Appendix D</u>

The village* of Clarendon, two miles to the east of Salisbury, has now disappeared leaving behind only the name Clarendon Park and the house. Edward Hyde (1609-1674) was born into the minor country gentry at Dinton, six miles to the west of Salisbury. He came up to Magdalen Hall in preparation for entering the legal profession but his father felt that he was wasting his time in Oxford and, after he had taken his BA in 1626, sent him up to London. He rose rapidly and by 1643 had become Chancellor of the Exchequer and legal adviser to King Charles the First. He saw much of the action in the Civil War and, in due course, was able to write its history in his famous "The History of the Rebellion and Civil Wars in England", published posthumously by his sons in 1702-4. He became Lord Chancellor in King Charles the Second's secret government and became virtually the head of it at the Restoration in 1660. But he fell from favour and in 1667 was obliged to live in exile in France until his death in 1674. It was in these years that he wrote his <u>History</u> and his <u>Life</u>, assigning the perpetual copyright to the University of Oxford, whose Chancellor he was from 1660-67. (This copyright has had a chequered history but still survives. See Carter, <u>A History of the Oxford University Press</u>.) But for his portable desk, probably of Dutch manufacture, little of the First Earl's writings would have survived (see Fig. [53]).

The History was one of the first major publications by the University press which, at the time, was housed in the Sheldonian Theatre and Fig. [54] reproduced from the title page of the <u>History</u> confirms this unlikely truth. The profits in 1713 amounted to only about £600 and, in reality, only contributed one tenth to the cost of building the "New Printing House", now known as the Clarendon Building. The Clarendon Press moved to its present site in Walton Street in 1832*. The Clarendon Trust was set up under the will of Henry, Lord Hyde (Edward Hyde's great grandson) and its activities are set out in the following "Note" by the Vice-Chancellor:

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> In a Convocation to be holden of Tuesday the 4th of February next, at two o'clock, it will be proposed to accept an offer made by the Clarendon Trustees to apply their fund to the erection of a building contiguous to the New Museum for the purpose of providing laboratories and other accommodation requisite for the Department of Experimental Philosophy.
>
> Delegates Room — F.K. Leighton
> December 9 1867 — Vice-Chancellor

It may be within the remembrance of many members of Convocation:

1. That Henry Lord Hyde, who died in 1753, bequeathed certain moneys expected to arise from the publication of the writings of his great grandfather Edward, Earl of Clarendon and of other papers "to be applied as a beginning for a fund for supporting a manege or academy for riding and other useful exercises in Oxford if the University should approve of such an institution: to such other uses as his Trustees should judge to be most for the honour and benefit of the University and most conducive to public utility".
2. That on the 13th December 1864 by which time the accumulated fund was little less than £10,700 consols, the specific object of Lord Hyde's bequest was submitted to Convocation for acceptance or rejection: and that Convocation then declined to accept it; and

3. That to the Notice by which the Vice-Chancellor convened Convocation on that occasion there was appended a suggestion "that the Interest of the Fund might not inappropriately be applied to the maintenance of the Park as a place of recreation and exercise".

That proposition was submitted to the Trustees but was declined by them in June 1865: and in the course of the correspondence they intimated an opinion that some scheme should be devised "for perpetuating the name of the Trust in connection, if possible, with some visible object": and again, "that the purpose chosen should be one as definite, tangible and complete in itself as may be".

The present state of the case is shown by the following correspondence.

I. Extracts from a letter addressed by the Vice-Chancellor to Mr Gladstone on 5th April 1867.

> "I believe that you are now the Senior Trustee of Lord Hyde's bequest: and I therefore venture to ask you, as representative of your colleagues in that Trust, to lay before them an application which I have been commissioned to make on behalf of the Hebdomadal Council. The object of my application is to ask –

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> 1. Whether the Trustees would grant the funds at their disposal in aid of the erection of the new Examination Schools. Such Schools have become absolutely necessary, as well for the accommodation of Candidates for Examination, as for the purpose of affording increased room to the Bodleian Library by surrendering to it the present Schools, which are very inadequate and inconvenient for Examination purposes.
> 2. I am to ask (if the Trustees should not approve of this application of the funds at their disposal) whether they would be disposed to grant such a sum as may be needed for adding to the New Museum Physical Laboratories and other accommodation requisite for the Department of Experimental Philosophy, in case the University should decide upon making such additions. These buildings are rendered desirable, if not necessary by the rapid development of this branch of Science since the foundation of the Museum, and the enlarged appliances which have in consequence become requisite during the last few years both for the use of students and for the purposes of adequate illustration. Such an addition to the Museum would be complete in itself and might form a distinct department and group of buildings, under the name, if it were thought desirable so to perpetuate it, of the Trust which supplied funds for its erection … No plans have as yet been obtained: nor indeed have the Council yet determined upon recommending the proposal to the University.
> With respect to my first enquiry, I may add that the new Schools are a pressing want, which must be supplied shortly. The outlay upon them will be necessarily large: and the University Chest, not rich in itself, has just at the present time many urgent claims upon its resources. A grant in aid of either of the purposes named would not only help that purpose forward, but in so doing it would also greatly facilitate the other works which are in contemplation.

II. Letter from Mr Gladstone to the Vice-Chancellor

11 Carlton House Terrace
May 3rd, 1867

> "Dear Mr Vice-Chancellor
>
> The Clarendon Trustees have met for the purpose of taking into consideration your letter of April 5, and they have had no difficulty in arriving at the conclusions – first that in their view the second of your proposals is preferable to the first: and next

that it is a proposal satisfactory in itself, agreeable to the purpose of the Trust and one which it is their duty to entertain.

They are ready therefore in concert with the University to consider of the best mode of applying the funds belonging to them for "adding to the New Museum Physical Laboratories and other accommodation requisite for the Department of Experimental Philosophy". And, in as much as time might be lost if at each step in the communications which may become necessary the members of the Trust were to be formally assembled, they have requested Sir W. Heathcote, and he has kindly agreed, to give his attention to the part of the Trust to these communications: and he will take care that the Trustees are summoned when any definite step is to be taken, so as to secure a perfect regularity in the proceedings. He will be prepared to discharge this duty on our part in concert with you or with any person or persons who may be appointed for a like purpose on the part of the University.

You will gather from what I have said that we do not deem ourselves to be discharged of our responsibility by the simple fact of your proposal and our acceptance, but that it will be our duty to be parties to the manner in which it may be deemed fitting to give effect to the

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plan of which you have made known to us the general object.

I have the honour to remain,
Dear Mr Vice-Chancellor,
Very faithfully yours,
W.E. GLADSTONE."

EDITORIAL ENDNOTES on AJ Croft's "OXFORD'S CLARENDON LABORATORY, by TMM Baker, 2022

This transcription

This is a transcription of Croft's History, typescript copies of which are held in the Clarendon Laboratory Archive and the Radcliffe Science Library (Bodleian Library). The Radcliffe Science Library's copy is a bound typescript, lacking some pages, with page numbers entered manually, and missing pages made good by copies from the Clarendon's version. The Clarendon's copy is on loose pages. The Clarendon Archive also contains several earlier draft versions of Croft's History, along with his research material: correspondence, recordings and transcripts of interviews with members of the Laboratory, and documentary sources including extensive notes by Croft's wife Margaret, and his research assistant Kirwan Angwin. The only other known copies of the History are one of the earlier draft typescripts now in the possession of Michael Wells, and a copy, referred to in the bibliography of Fox's and Gooday's "Physics in Oxford, 1839-1939", 2005, which was then in the History of Science and Technology Seminar Room of the Modern History Faculty.

As far as the editor is able to ascertain, the version of Croft's text transcribed here is the latest and most finished before his efforts to publish it were interrupted by his death.

Lacunae in Croft's text are marked with square brackets, and inferred text (usually cross-references) with square brackets. The Figures referred to in Croft's text are not present in the Bodleian's or the Clarendon's copies. An inferred list of Croft's Figures, derived from the text, follows the list of chapters. The manual page numbers of the Clarendon Archive's text have been inserted into the transcription in square brackets at the beginning of each page. These page numbers have been used to complete Croft's cross-references. Simple typographical errors (which are rare) have been corrected without comment. Croft's footnotes, marked in the manuscript with a system of asterisks, are numbered in this transcription. Asterisks are used to refer to the editors' endnotes. A small number of manual additions and corrections – usually by Brebis Bleaney – are incorporated in the text, and referenced in the endnotes.

The author, AJ Croft

A good summary of Croft's life, and of the genesis of his History, is the Obituary written by Nicholas Kurti for the Physics Newsletter on 9th March 1988:

'Antony Julian CROFT, M.A., D.Phil., F.Inst.P., M.I.E.E. (1925-1988)

"A.J." Croft, the low temperature physicist, who died on 27th February 1988, was a well-known figure in the Clarendon Laboratory and in Oxford. He was born on 9th January 1925 and his congenital diabetes had a strong effect on his career and his life. He went from Westminster School with an exhibition to Christ Church and I first met him in 1946-47 during the 3rd year practical physics course. He threw himself with great gusto into the work and it was obvious that he hoped to continue with physics after graduating, preferably in Oxford. However his finals results were not good enough for him to embark straight away on

D.Phil. work. On the other hand his performance in the practical was impressive. Not only was he a good experimenter with a flair for designing and skill in constructing apparatus – his training in workshop practice and engineering at school stood him in good stead – but he also had an almost uncanny ability to overcome difficulties, which so often slow down or halt experimental work. I remember that when some piece of equipment was missing or some material was needed to carry out repairs I did not have to look for the course assistant, I could rely on A.J. to get what was needed. He seemed to know instinctively where to find things or whom to ask – in fact he, a 3rd year undergraduate, knew better how the Clarendon ticked than I did after working there for 15 years!

It was these qualities that persuaded Simon (F.E., later Sir Francis Simon, Professor of Thermodynamics) to engage A.J. as a Research Assistant to help with the provision of enlarged cryogenic facilities for the Clarendon Laboratory in its new home. In 1949 A.J. embarked on his D.Phil. work with a DSIR grant and obtained his degree in 1952.

To understand the purpose and the content of most of A.J.'s work up to 1960 and even beyond one must realize that the Clarendon 40 years back was a different place from what it is today. Much of the equipment – or even materials – which nowadays one can obtain commercially were not available in those days. And even if they were, shortage of funds often forced us to manufacture them "in house". Characteristically A.J.'s first two publications were short notes in the Journal of Scientific Instruments on two simple gadgets, one to protect diffusion pumps when the water cooling fails, and the other to prevent oil from rotary pump flowing back into the apparatus should the pump accidentally stop.

Many readers today will find rather baffling another of A.J.'s publications from the same period: "A laboratory plant for making liquid air from liquid oxygen", but there was a good reason for it. Liquid nitrogen was in those days an expensive commodity whereas liquid oxygen was regularly being delivered in bulk to the Pressed Steel Works in Cowley, that being the cheapest supply of oxygen needed for welding purposes. In 1961 Croft designed and built a new and highly successful hydrogen liquefier which was connected by a 14m long transfer line to the helium liquefier. This liquefier was perhaps the best laboratory-sized installation not only in Britain but in the world.

The 1950s saw the gradual replacement of the individual small Simon liquefiers, each one forming an integral part of the experimental apparatus, by a single central liquefier, from which liquid helium could be transferred. The first of these, designed and built by A.J., was a large single expansion liquefier capable of producing just over 1l of liquid helium per expansion and having a repetition rate of about one per hour. This was clearly insufficient for the Clarendon's needs and by 1955 it was decided to build a helium liquefier capable of producing about 50 litres in a working day. Since the laboratory had a large hydrogen liquefier, a Linde-type He liquefier was designed by Croft and Bligh and commissioned at the end of 1956. This liquefier was eventually connected to the hydrogen liquefier by a 14m long liquid hydrogen transfer line also designed by Croft.

Several publications on new types of heat exchangers, on methods of storing and handling liquefied gases, etc., one with the evocative title: "Cryogenic Difficulties" and an excellent little monograph "Cryogenic Laboratory Equipment" are proofs of A.J.'s good grasp of cryogenics and of his desire to let other people profit from his knowledge and experience.

Mention should be made of an attractive piece of work in cryophysics rather than cryogenics which A.J. did with his D.Phil. student, Robert H.Exell (now Professor at the Asian Institute of Technology in Bangkok). It consisted in the direct measurement of the entropy of magnetization of a paramagnetic substance (iron methyl-ammonium alum in this case) whose behaviour is too far removed from ideal for the entropies to be calculated. The sample is magnetized at, say 1°K, thermally insulated from the surrounding bath of liquid helium. Heat is then introduced at a constant rate and the magnetic field H is reduced in a way to keep the temperature of the sample constant. Since the quantity of heat introduced is proportional to time and the demagnetization is isothermal the H vs time plot represents the entropy as a function of the magnetic field.

By 1959 A.J. was appointed a "Graduate Assistant", a position on the same scale and status as a University Lecturer or Senior Research Officer. He had already had an important role in various aspects of the laboratory's administration and with this appointment his responsibilities increased. And then, in the summer of 1964, came the blow; he lost his sight and his activities in the lab gradually but inexorably had to change direction. Not surprisingly he became interested in how new developments in electronics could help blind people, especially those with residual vision who, in the U.K., account for 85% of the registered blind. In a survey article on "Scientific Aids for the Blind" (Contemporary Physics, Vol. 18, pp 73-80, 1977) he describes various new technologies and gadgets and urges seeing people who develop these instruments to cooperate with the blind and try to understand their problems and their needs. As a corollary he also pleads with the blind who have the necessary technical knowledge to work in this field. He could do so with a good conscience having developed, with David T. Smith of the Clarendon Laboratory, an electronic calculator for the blind. The article, written with sensitivity, also warns against regarding novel gadgets as panaceas and insists, for instance, that "the long stick or a guide dog or both are superior to any science-based device" – strangely A.J. never used either of them!

Two major projects occupied much of the last 15-20 years of A.J.'s life: the University's telephone system and the history of Oxford Physics. As Executive Secretary of the Telephone Sub-Committee of the University's Buildings Committee he worked tirelessly on establishing the central telephone system embracing Departments, Colleges and Administration, leading the way eventually to the new system which came into operation in January 1987.

A.J. was always interested in scientific hardware and techniques. A visible proof of this is his creation of the display in the Clarendon which includes the Lindemann-Keeley Electrometer, an early Simon expansion liquefier, Thonemann's laboratory apparatus from which the Zeta machine for nuclear fusion was developed, early specimens of helium film flow, nuclear orientation and nuclear cooling experiments, etc., etc.

As time went on he became more and more involved in the history of physics in the Clarendon and more generally in Oxford. In about 1978 he embarked on an ambitious project, namely the history of physics in Oxford from the early beginnings to the present. An enormous amount of archival material was assembled, including long tape-recordings of many old Clarendonians, and the MS was completed. It is to be hoped that all the material will be preserved and that a publisher will be found for the book.

To engage in such manifold activities A.J. obviously needed help of various types, such as technical assistants and, more importantly, secretaries, among whom I want to mention particularly Marion McClintock, Anne Peacock and Jane Davis, all of whom adapted themselves with patience and good humour to A.J.'s sometimes odd ways and attitudes. And there was of course the constant companion, his wife Margaret (née Nicklin) a fellow-undergraduate (St. Hugh's, Modern Languages) whom he married in 1948. All who knew the Crofts admired the ways in which Margaret helped A.J. She gave him unstinted support, she protected him and her loyalty and devotion, often under difficult circumstances, were not far from miraculous.

I have known A.J. for just over 40 years and have many happy memories of his earlier days. He was gregarious, enjoyed swapping stories and had many interests, particularly music: he played the organ and sang for many years with the Bach Choir. He cultivated an old-fashioned appearance and behaviour, always dressed formally, wearing a suit with a waistcoat and had strict views about etiquette and social classes. I recall how on one occasion he was holding forth about which among the young female graduates could be termed "ladies". I could not help feeling that his snobbishness had an element of self-mockery in it. He seemed to have sensed that some of his views were slightly ridiculous but he nevertheless indulged in them. His reluctance to travel abroad was near-pathological. I believe he only once ventured outside England, when he had to go to Scotland on family business. He complained about having to travel to a country where policemen wore peaked caps with checkered bands.

The loss of his sight took its toll. I remember that in the spring or summer of 1964, when I was away on sabbatical he wrote to me that he was going blind but that he would go on as before, emulating the example of the then Bursar of Balliol (Sir Theodore Tylor) who was totally blind. Perhaps he was trying too hard to do the impossible, pretending and wanting others to believe that things were the same as before. I wonder whether that is why he never carried a stick, never learned Braille and, although he always had a dog, it was never a guide dog. In fighting for a status quo or a semblance of it he became less tolerant of others, less flexible in his views, less open to persuasion.

To those of us who have known A.J. for a long time it was a saddening experience to witness these changes caused by his deteriorating health. But we retained many happy memories of an earlier A.J., the companionable, warm and trusted friend who did so much for cryogenics, especially in the Clarendon and who was yet another example of Oxford's "original and memorable characters".'

History of the text

Kurti's Obituary states that Croft began work on the History in about 1978. A 1981 letter from Croft to Derek Jackson in the Clarendon Archive says that the Institute of Physics asked him to write a history of Oxford physics, 1839-1976. The Archive contains subsequent correspondence with the Institute of Physics up to 1985. The original working title seems to have been "A Century of Oxford Physics". At some stage this became "Clifton, Townsend, and Lindemann", and then "Lindemann and Oxford Physics": the last of these seems to have been motivated by an intention for the Institute of Physics to publish during the Lindemann centenary in 1986.

The Institute of Physics appears to have changed its mind about publication at a late stage, at about the same time that the work assumed its final title: "Oxford's Clarendon Laboratory". Croft then marketed the book to other academic publishers, but time then ran out for him, and despite Kurti's expressed hope that the book would find a publisher, with the author dead impetus seems to have been lacking.

There was a copy of the History, albeit not a complete text, in the Radcliffe Science Library by 1991. At that stage the Bodleian Library had a correspondence with John Lloyd of the Nuclear Physics Laboratory, and with Donald Edmonds of the Clarendon (preserved with the RSL copy), about filling the gaps in the RSL copy with copies of pages from the Clarendon Archive text, which was done. At that stage Donald Edmonds was expressing anxiety that Croft's 'very interesting' text should be preserved in the light of the small number of copies that existed.

Michael Wells, who had saved Bleaney's copy of an earlier draft when his office was cleared after his death in 2006, drew Timothy Baker's attention to its existence in 2020. Baker completed this transcription in 2020-2021, drawing also on the more complete Clarendon Archive and Radcliffe Science Library copies. Stephen Blundell kindly provided access to the Clarendon Archive's copy, which forms the basis of this text.

Editorial notes on the text

Title page

Antony: Although Croft's text gives 'Antony Croft' as the author's name, Kurti's biography, and the recollections of Mike Wells, make it clear that Croft was always known to his colleagues as 'AJ', and never used his Christian name. It is not clear why Croft did so here; all his published works appeared under 'A.J. Croft'.

Chapter 4

Henry Briggs: In fact, Briggs's decimal logarithms were fundamental to the complex calculations needed for Kepler to derive his laws of planetary motion.

Medical men: Thomas Willis, Sedleian Professor, 1660, and pioneer of neuroscience, made early steps towards physical chemistry: his notion of 'sulphur corpuscles' corresponded broadly to the oxidisability of a substance.

Chapter 6

First chemical laboratory in Oxford: In fact, Charles Daubeny's laboratory opposite Magdalen College dated from 1844; the Christ Church Anatomy School, which had a chemical laboratory, from 1767, and the Old Ashmolean Museum's laboratory from 1683; earlier in the 17th century, chemistry was taught and studied in apothecaries' laboratories in the town. The Balliol Laboratory was the ancestor of the Physical Chemistry Laboratory.

Greenwich: Though Bradley's zenith telescope has been at Greenwich since 1749, he made his observations at Kew, Surrey and Wanstead, Essex.

1832: Croft's typescript has '1822', which is wrong.

Chapter 8

Walker and the University Museum: In the Bodleian and Clarendon versions, this chapter is still entitled 'Robert Walker' and still labelled in Part II. However, the Bodleian and Clarendon text's chapter list indicates an intended change of name, and a move to Part III – and Walker was indeed appointed in 1839.

Chapter 9

Henry Smith: Smith became a fellow of Corpus Christi College in 1873.

Chapter 13

First automatic pilot: The first autopilot, using gyroscope and hydraulic controls, was in fact developed by Lawrence Sperry, 1912-1914.

Chapter 16

Manuscript note at end of chapter in some versions: 'Became separate dept 1964.'

Chapter 17

Adoptive father: Derek Jackson was in fact Charles Jackson's son, and not adopted.

Chapter 18

G. Dannen: The reference is to Gene Dannen, expert on Szilard's work: see www.dannen.com.

Rothschild family: Aline de Gunzbourg (1915-2014) was descended from the Gunzbourg, Deutsch de la Meurthe, and Halphen families.

Manuscript note at end of chapter in some versions: 'Became independent 1961 or 2'.

Chapter 19

Inter-war years: This chapter started as 'Three Exports'. Croft later added Stephen Hawking, who was a post-war Oxford physicist.

Chapter 20

Third floor in 1960: The Clarendon and Bodleian text has '1910', which must be wrong. The text lower down gives the true date.

Chapter 24

RCS: Royal College of Science, South Kensington, part of Imperial College London.

Radar in the Second War, and *The War at Sea*: These sections were extensively rewritten and somewhat shortened between earlier and later versions of Croft's "History". The relevant text from an earlier draft formerly in the possession of Brebis Bleaney follows:

At Birmingham there had been much activity under M.L.E. Oliphant towards bigger and better klystrons. He had a large 50-centimetre continuously-pumped klystron mounted on a trailer ready for a demonstration anywhere and he used to say that there was no limit to the power that could be achieved by scaling it up. By some means now unknown, Sir Frederick Brundrett was able to talk to J.T. Randall, a former employee at GEC, Wembley, who was at the time working at Birmingham on a Royal Society fellowship. He was given permission to devote part of his time to working for the Admiralty and he and N. Boot, having doubts about high-power klystrons, decided to try applying the resonator principle to the design of a 10-centimetre cavity magnetron. Everybody knows that they were richly rewarded by finding quite soon that they had made a jump of two orders of magnitude in the available power. (It must be remembered that their success was due to the conception that their magnetron would be very much a <u>pulse</u> device. In fact, about 1,000 pulses per second each lasting 1 micro-second.)

Early in the spring of 1940, a few weeks after the Birmingham magnetron had first worked, a car-load from Oxford went over to see it. C.H. Collie remembers saying on the way home, "They've got the transmitter. We've got the local oscillator. All we have to do now is build the rest of the receiver".

Meanwhile, Oxford was waiting poised, although it too had had early success. When the Second War began in September 1939, research was stopped and T.C. Keeley had been asked by the Prof to start up a group and get them acquainted with short-wave techniques. They were B.Bleaney, C.H. Collie, A.H. Cooke, J.G. Daunt, J.H.E. Griffiths, R.A. Hull, C.Hurst, D.A. Jackson, D. Roaf, and B.V. Rollin. Some remarkable projects were undertaken. A source of somewhat wide-band RF power was generated by passing sparks through a channel containing oil and 1/64 inch ball bearings. Some of its output was about one millimeter in wave-length. Another interesting device was entirely up-to-date in that it was sealed up in a vacuum but it was clearly inspired by Monsieur Cagniard de la Tour's siren. But for the magnetron, it might have been a winner. It produced 3 to 6 watts at 6 centimetres.

While all this was going on, B.V. Rollin was quietly inventing one type of the reflex klystron. This is substantiated in Fig. 41 which shows a drawing labelled and dated in his own hand, being part of a report to Lindemann just before the Admiralty work started. The electrode labelled D is given a positive voltage, so that secondary electrons are emitted, which return to the resonator. Rollin's report notes that, if the electrode D is given a negative potential, the klystron works just as well. This was the way in which reflex klystrons were afterwards used, having the advantage that more of the velocity modulation of the original electrons is preserved if they are turned back on their tracks rather than being used to provoke secondary emission. Before this device could be made into a permanently pumped and sealed component, it had to go to Bristol, where R.W. Sutton was a master of the copper-to-glass seal. The great advantage of the single resonator over the double resonator type was that it was much more stable [MS note – by Bleaney: 'No! Tunable.'] – as one would expect. Before long 3.2 centimetre klystrons were being made by [MS correction

– by Bleaney: ‘B.V. Rollin’ deleted, ‘B. Bleaney’ inserted] and J.H.E Griffiths, and 1.6 centimetre by D. Roaf.

The solution to the question of the detector was surprisingly simple. The early days of broadcasting were not so distant that the idea of using a crystal and a cat’s whisker seemed laughable. But it was found that under the well-controlled industrial conditions that applied at GEC, a very high standard of mechanical reliability and low levels of electrical noise could be achieved by a piece of silicon – impure as it would have been – and a tungsten whisker. Several Oxford physicists worked on free electron devices but none were found better.

The War at Sea

In 1941 the German U-boat was taking a toll of shipping in the Atlantic which, if sustained, would have led to defeat. From April to December we lost 1.5 million tons. The only counter-measure against the U-boats was to find them during the 5-6 hours night-time period when they charged their batteries on the surface. The 1.5 metre air-borne radar set (AI Mark I) was effective when supplemented by the Leigh light – and air-borne search light. But by 1942 the enemy had installed receivers sensitive to 1.5M which gave the submarine plenty of time to dive. The U-boats were able to regain their supremacy; for example, on 1 May 1943 one U-boat sank 43,000 tons of our shipping in a single night. The first ship to be fitted was the corvette, HMS <u>Orchis</u> in March 1941 and, by July, 25 more ships were fitted (24.5).

The solution of the problem of fitting radar into aircraft was to use the same aerial for the outgoing pulse as for the incoming echo, provided that some sort of switch could be devised. (Ten centimetre magnetrons were not allowed in aircraft over enemy territory until 1943, when one was lost over Rotterdam. The Germans, as it happened, had given up centimetre radar research in the same year and it was not until 1945 that they were able to make use of what they had found.) Fig. 42 shows the separate transmission and reception aerials which were needed by the early centimetric sets. This problem came to A.H. Cooke in Oxford. It was relatively easy to arrive at the geometry of what became known as the TR (transmit-receive) cell but there was a problem in finding a gas which would conduct when the big pulse came along, while isolating the receiver. After trying helium, neon, argon, carbon dioxide and nitrogen, Cooke in despair tried water and found it a winner. 10 centimetre radar sets were then fitted to Coastal Command aircraft and, from March 1943 to August 1943, shipping losses dropped from 400,000 tons to 40,000 tons per month.

The German Navy became increasingly baffled by our success in locating surfaced U-boats since they could not now detect our aircraft. Inevitably, they captured one of our Coastal Command pilots and pressed him to disclose how we were doing it. The pilot was ready with his answer. It was quite simple – we picked up radiation from their receivers. They believed him to the extent of going to a good deal of trouble over modifying their receivers (24.6).

The final salute to the 10-centimetre magnetron came in an unusually accurate statement from Adolf Hitler. In a speech at Weimar in 1943, he attributed his country’s “setbacks” in the Atlantic to a single British technological invention. Admiral Donitz went further: “It was not superior strategy or tactics which gave him (ie. us) success in the U-boat war, but superiority in scientific research”.

One type of the reflex klystron: In a manuscript note of his copy of Croft’s “History” (see previous endnote) Brebis Bleaney amended ‘the’ to ‘one type of’. This change has been adopted.

Greater stability: In his copy of Croft’s “History”, Bleaney stated in a manuscript note that it was the tunability of Rollin’s klystron, not its stability, that was notable. This change has been adopted.

G: ‘G for Grid’, or ‘Gee’, the radar navigation system.

Chapter 27

Discovered by Cooke et al: In the Clarendon archive text is a handwritten note by Bleaney querying 'discovered', with reference to 'Bleaney, Penrose & Plumpton, Proc.R.Soc.A 198, 406 (1949)'.

1:0.04: In the Clarendon archive text is a handwritten query by Bleaney of the 1:0.04 ratio.

Chapter 28

We have seen: Chapter 28 was Chapter 32 in an earlier draft of Croft's history. The reference to the Royal Society magnet appears in Chapter 30 of this text (below), so 'we have seen' is no longer right here.

Chapter 29

Cooke: In the Clarendon archive text is a handwritten note by Bleaney inserting 'later' against Cooke's name in this context.

To his Pressed Steel Fellowship: In the Clarendon archive text is a handwritten note by Bleaney replacing these words with 'him as a Pressed Steel Fellow'

Chapter 30

Bleaney's: corrected by Bleaney in a manuscript note from Croft's 'his' (i.e. Griffiths's).

Was not that expected: In the Clarendon archive text is a handwritten note by Bleaney replacing Croft's 'should not have appeared' with 'was not that expected'. This change has been adopted.

Van Vleck, the author of Electric and Magnetic Susceptibilities: In the Clarendon archive text are handwritten amendments by Bleaney, altering Croft's 'a year' to '6 months', and Croft's 'he could sit at the feet of the great guru of the subject' to 'he learned some quantum mechanics', and adding 'was away on sabbatical leave' after '(30.12)'. These changes have been adopted.

Electricity and Magnetism: Bleaney was co-author of "Electricity and Magnetism". The main author was his wife Betty Plumpton (BI Bleaney).

Bleaney obtained one of the Garton group's crystals: In a manuscript note in the Clarendon archive text Bleaney amended Croft's 'Bleaney had one of the Garton group's crystals available but had made no measurements on it because of its unpromising electronic constitution' to: 'Bleaney obtained one of the Garton group's crystals on which no measurements had been made because of its unpromising electronic constitution (a singlet ground state).' This change has been adopted.

TN: Neel Temperature: the temperature at which the antiferromagnetic material becomes paramagnetic.

Chapter 31

J.H.E. Griffiths after: In the Clarendon archive text is a handwritten note by Bleaney replacing Croft's 'in' with 'after'. This change has been adopted.

MOS: Metal Oxide Semiconductor.

Chapter 36

Warren Fellowship: The Clarendon archive text has a handwritten correction by Bleaney of Croft's reference here to a Senior Harmsworth fellowship.

Chapter 38

Though it has the character of an Appendix, Croft includes this account as a chapter.

Appendix D

Village: Clarendon was not a village, but a royal forest, whose hunting lodge King Henry II turned into a royal palace, promulgating there the Constitutions of Clarendon, 1164. Edward Hyde acquired the old palace and its park in 1664, and took if for the title of his earldom. Eilert Ekwall (Concise Oxford Dictionary of English Place Names, 4th edn, 1960) interpreted the name as possibly Old English 'clæfren dun', meaning 'clover hill'.

1832: Croft has '1822', which is wrong.