EDMUND CLIFTON STONER

1899-1968

Elected F.R.S. 1937

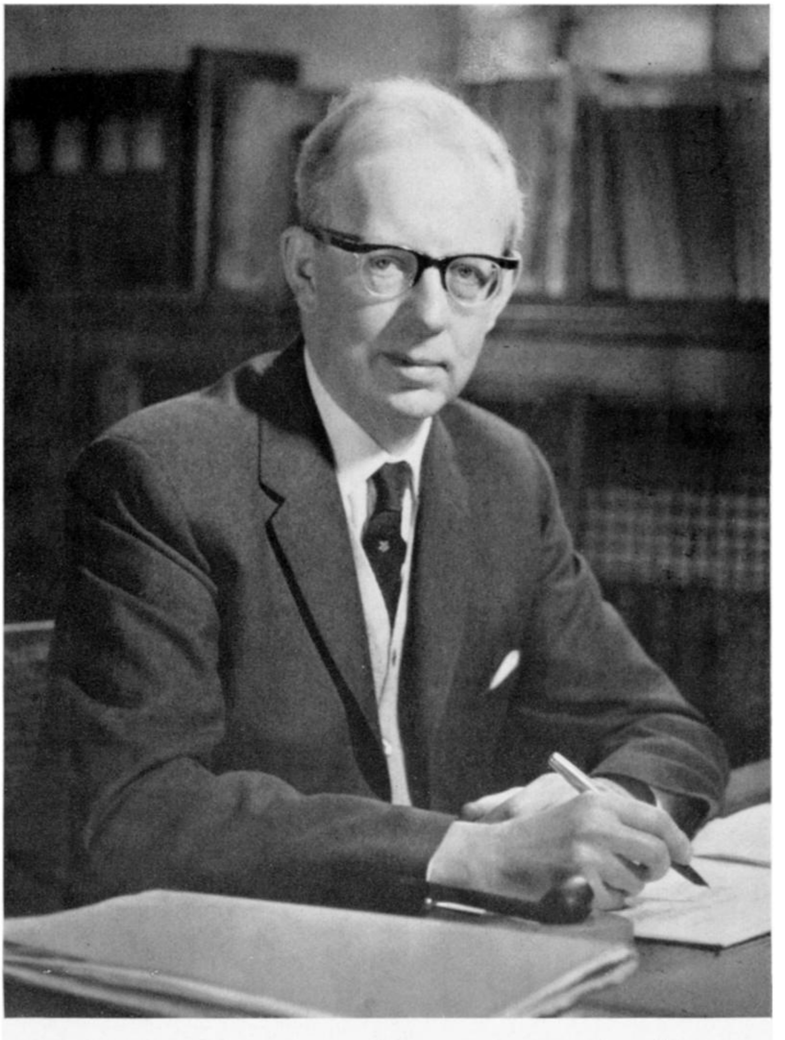
Section I. Personal details

Family and school background

Edmund Clifton Stoner was born on 2 October 1899, at 4 Marshall's Cottages, Pemberton Road, East Molesey U.D. in the district of Esher in the County of Surrey. His birth was registered in that District, Book 33, page 66. His surname Stoner was that of his father's foster-father, named James Stoner, who, with his wife Henrietta (*née* Palmer) brought up their foster-child from the age of a few weeks. James Stoner lived at Streatham, where he had a china shop and a smallholding. While at school the foster-child was known as Arthur David Hallett, but on leaving school his foster-father asked the boy to take the name Arthur Hallett Stoner, which he thereafter used. There is no reason to doubt that he was born on 11 May 1870, or that his father's name was Hallett, but he had no other knowledge of his parentage or ancestry, or of the circumstances of his adoption, and there is no other reliable information concerning his father. James Stoner died a few years after his wife and a few months before Edmund Stoner was born. As registration of birth was not compulsory before 1875 the lack of knowledge of more details of Arthur David Hallett's birth is readily understood.

Edmund Stoner's maternal grandfather, Thomas Robert Fleet, and his grandmother Caroline Clifton, only daughter of William Clifton, a gardener, came respectively from Bierton, near Aylesbury, and Cowden in Kent, and from the time of their marriage they lived in Streatham, London, S.W., where Edmund's father and mother also lived until they married. It would appear that the only known living descendant of the Fleet family is Carrie Helena (Fleet), Mrs Leonard Nelthrop, a cousin with whom Edmund remained in touch until his death. Arthur Hallett Stoner married Mary Ann Fleet (born 6 June 1868) on 27 October 1898, at St Leonard's Church, Streatham; Edmund was their only child.

In order to present a satisfactory picture of Edmund Stoner's early life and training, it is necessary to say more about his parents. His father was good at schoolwork and had hoped to remain at school in order to become a teacher, but financial difficulties and a desire for a more free and outdoor life probably prevented entry to the teaching profession. Consequently, on



Edmund C. Stoner.

leaving school he helped his foster-father on the smallholding; later he helped the 'professional' on the adjoining Streatham cricket ground, where he showed such promise as a cricketer that he was appointed second 'professional' at Streatham in 1890. He played for two seasons with Streatham and one with Partick and the West of Scotland; he was 'professional' to the East Molesey Cricket Club from 1893 to 1900, and until his marriage in 1898, Streatham was his home. While with East Molesey he played for Surrey 2nd XI, and was invited to join the staff at the Oval, but accepted instead an attractive offer which took him to Philadelphia, 1901-1903, and to South Shields, 1904-1909, both in the Durham League. When playing for Durham against the South Africans in 1907 he took 8 wickets in one innings. He later became 'professional' with Tonge, a Bolton (Lancashire) League Club, 1910-1912, and with Penarth (Glamorgan), 1913-1914.

Following war service with the Civilian Army Pay Corps and in the Borough Treasurer's Department at Bolton, he was cricket coach on the ground of Mr J. C. Gould at St Mellons, near Cardiff, a post which ended in 1922 with a shipping crash involving the Gould firm. Subsequently, he obtained part-time employment as a bookkeeper which allowed him to play cricket during the season, and he was coach for some years at Exeter School, umpire for the Minor Counties, for Oxford University, and the Authentics. In fact, it was in Scotland in 1938, while on tour with the latter club, that there began the illness from which he died on 25 September in that year. The reason for treating these matters in such detail is because of their importance in Edmund Stoner's upbringing. Firstly, the career of a professional cricketer was precarious, with its long off-season periods, difficulties of part-time employment and the vicissitudes of the game. Secondly, there were the frequent changes of post and absences from home when playing away. His family lived for 1½ years at East Molesey, 3 years at Fence Houses, Philadelphia, Durham, 6 years at South Shields, and 8½ years at Bolton.

On his mother's side there was no wealth, for when Fleet died his widow had to bring up a son aged 8 and Edmund's mother, who was then a girl of 13 years, by her earnings from daily work and needlework. His mother, as soon as she left school went into domestic service as nursemaid, parlour maid and later companion-help to what would then be termed a 'good' Streatham family. In these circumstances she must have acquired those qualities of independence and self-reliance which her son so much admired and himself possessed in high degree.

Obviously, Edmund Stoner's education depended entirely on his ability to win scholarships. He started in April 1905, in Mortimer Road Council School, South Shields, moving to Tonge Moor Council School, Bolton, in 1910, where he gained an open scholarship to Bolton Grammar School (renamed The Bolton School, Boys' Division, in 1915). While he seems to have been very happy at the South Shields school, he was much less happy at Bolton, where his parents' financial situation in the 1914-1918 war years must have contributed much to his discontent. Indeed, in this era most of his schoolfellows worked half-time in the mills before they finally left school at 14, and Stoner sometimes felt self-conscious that he was not subjected to the same hard treatment. One is glad to know that when he revisited Bolton some 30 years later, he was favourably impressed by the town and its inhabitants. He records that he made two good friends at Bolton. One was Cecil Chapman, who died suddenly in 1918, and was the son of the Vicar of Bolton who later became Bishop of Colchester; the other was Bryce McKelvie, now a Manchester surgeon, with whom he afterwards kept in touch.

Towards the end of his Bolton school period, he suffered from some degree of cardiac overstrain and dilatation, which explains why he was not called up for military service, and why he dropped games and strenuous exercise. But, he learned to play the piano and later derived much pleasure in 'keeping it up', playing solos, duets and accompaniments. (Professor W. Sucksmith, F.R.S., told me that when he sailed with Stoner to New York en route to the Washington Conference on Magnetism, 1952, Stoner acted as accompanist at the ship's concerts.) He also acquired an abiding interest in astronomy which probably led to his contributions to our knowledge of white dwarf stars. He read, rather widely, works of fiction, plays, essays, history and books on the arts. He had a taste for writing essays and sketches, short stories and even plays and poems. One would like to see some of these early efforts to trace, if possible, how his style developed.

Section II. Cambridge

Undergraduate period

In 1918 Stoner went to Cambridge. He was awarded a Thomasson University Scholarship, a Popplewell School Leaving Scholarship and an Open Exhibition in Natural Sciences at Emmanuel College. I have had available to me his own typewritten account or narrative* (some 20 pages of quarto, single spacing) of his life in Cambridge 1918-1924. I found it extremely interesting and I have drawn upon it very freely in writing the following account of this period of his life. Some parts of the narrative appeared to me to be of such great interest that I quote them almost without alteration; I have, in general, followed Stoner's own lay-out.

The narrative first deals with financial matters. On entering Emmanuel College, his total income was £190 per annum, which rose to £200 per annum when he was awarded a Scholarship at the end of his first year. His College bills for his three years of undergraduate residence aggregated to just over £500, leaving an average sum of about £30 per annum towards books, clothes, travelling and incidental living expenses in Cambridge and at home in Cardiff during vacations. It must have meant severe economy on his part and austerity for his parents. Stoner coldly comments that his

* Mrs Heather Stoner has kindly deposited this document with the Royal Society.

financial difficulties, in spite of his high scholarship income for those days, were due to a very large, short duration, rise in the cost of living, following the first World War. In the 1920 Natural Sciences Tripos, Part I (Botany, Chemistry, Physics) he was awarded Class I, and in the 1921 Part II (Physics) he was also awarded Class I.

One can only express amazement that there were no mathematics examinations in this scheme of things; by what miracle then did he become a Professor of Theoretical Physics? Stoner often referred to the situation as absurd. Further reference to it is made below. It should be taken as a good illustration of his very remarkable abilities. In his 39th Guthrie Lecture of the Physical Society (*Phys. Soc. Year Book*, p. 24, 1955) he states:

'Undergraduates reading Physics, whether for Part I or Part II, were not expected, or encouraged, to attend any formal classes in mathematics, nor, in the normal course, could they readily have done so. It might have been supposed that mathematics was quite incidental in physics, to be picked up and used, when necessary, as casually as a soldering iron. Some of those with a modest flair, and a good school background, did in fact pick up a good deal of mathematics; but the lack of any formal teaching by mathematicians for young physicists in their receptive undergraduate period was a considerable drawback to me later, as it must have been to many of my fellows. The odd arrangement about physics and mathematics which existed during my period at Cambridge is the more surprising in view of the character of the work of the earlier great physicists there, Maxwell, Rayleigh and Thomson, not to go back to Newton. I have mentioned this matter, although its interest may seem somewhat localized, because it connects with a more general question to which I shall refer later, namely the relation between experimental and theoretical work in physics.' (See p. 233.)

During his postgraduate period, 1921-1924, he held a D.S.I.R. maintenance grant which was slightly greater than his previous total scholarship income and was supplemented by the usual modest casual earnings from laboratory demonstrating and tutorials, while his University and College fees were now less—at last he was self-supporting.

The second part of his narrative deals with important health matters. During his first term in Cambridge his health was not good, and in the succeeding Long Vacation term he experienced certain unpleasant symptoms which he, in his usual independent manner, attempted to interpret by referring to medical books in the College Library; he concluded that he was suffering from diabetes. Accordingly, without of course disclosing his own tentative diagnosis, in July 1919, Stoner consulted Dr Lloyd Jones, who made a firm diagnosis by applying the usual tests. There was no previous history of the disease in the Stoner family, and it had not been evident in a thorough medical examination which he underwent in 1918; its causation remained a mystery.

I was a research student in the Cavendish Laboratory during the years 1922-1924, and although I did not know Stoner very well, I heard statements or rumours about his condition which now appear to have been quite incorrect. I therefore reproduce his own, unaltered, account:

'The only treatment that could then be suggested was a drastic reduction in carbohydrate intake, with regular tests and weighing. The initial improvement was very marked; and surprisingly, as it seemed to me later, with the badly balanced, restricted and rather casually adjusted diet, my general condition remained fairly satisfactory for some three years. The diet was one to which I adapted myself without undue difficulty; but it was troublesome in the choiceless common meals in college; and it was relatively costly. Moreover, I tended to avoid those friendly casual social occasions of which a normal accompaniment is the partaking of meals or refreshments rich in carbohydrates. Being different from others in matters of food and drink was bad enough; but being forced into explaining why to casual acquaintances was acutely embarrassing.

'The work of Banting and Best in 1921 initiated the use of insulin in the treatment of diabetes, but it was not until a year or so later that I had any firm information about it, mainly from friends working in that field at the Biochemical Laboratories at Cambridge. I was naturally anxious that its possible use in my case should be examined. This I found could be done only under approved hospital supervision. It happened that in the early months of 1923 my diabetic condition had somewhat deteriorated after an attack of influenza, and it was eventually arranged that I should enter Addenbrooke's Hospital, Cambridge, on 13 March 1923. Insulin was used to a limited extent, but since it would not have been available to me later, the general aim was apparently to build up, by the then sequence of "ladder" processes, a diet which was within my carbohydrate tolerance limit, and which, without insulin, should be as well balanced as possible. I was led to expect that I would have to be in hospital for a few weeks. I was actually there for over three months, and did not leave until 30 June 1923.

'The long period in hospital was frustrating and disappointing. I never discovered who was in charge of my treatment, and never had any proper discussion with the doctors concerned. It was only through the Sister that I learned anything. Even now, with a greater knowledge of the difficulties, I still think my treatment at the hospital was badly managed. The positive outcome was a better balanced diet scheme, essentially with relatively more carbohydrate. This was more troublesome than before in that it involved more careful weighing of food and more frequent testing, with at least temporary reduction in the diet when the tests were unsatisfactory. Nevertheless, the new diet scheme and the more rigorous control were almost certainly beneficial. The overall diet, however, was very low for an even moderately active life. For the rest of my time at Cambridge (until August 1924) my general physical condition was in fact quite good, but there was almost inevitably a gradual deterioration. To anticipate, this became very marked in 1927, but by that time insulin had become generally available, and better rules of treatment had been worked out. When I came home to Cardiff from Leeds in the summer vacation of 1927 the change was made to a proper insulin régime, and this resulted in a real and long-lasting improvement.

'It is proper that I should add that, in spite of the sad and grim episodes and the disappointments inevitably associated with hospitals, there were at least for me many compensations. I was in a large ward and, not being ill in the acute sense, I could talk with other patients of widely differing experience and outlook, which I much enjoyed; I could often be helpful to them and to the nurses in connexion with the usual hospital routines; and I could see from the inside how a hospital was run, with its then tight and elaborate hierarchical system. I still remember the delight at the first visit my mother was able to make to me after what seemed the endless waiting time of ten weeks, and the relief at having someone with whom my feelings could be freely shared. I remember, too, with gratitude, the many Cambridge associates who visited me expectedly and unexpectedly with friendly conversation and news and books. At that time I did a great deal of reading, both solid and light-hearted; and in my usually somewhat bare diary I wrote at length about people, including my fellow research students at the Cavendish Laboratory, books and ideas.'

To this portion of his narrative, I think it of interest to add some comments taken from his Personal Record for the Royal Society: 'With the adequate balanced diet which was then [1927] worked out, and two insulin injections a day, . . . with a few minor modifications in diet and insulin, and except for occasional mild illnesses of an ordinary kind, I have been able to lead a more or less normal and reasonably active life.

'Without the discovery of insulin I am sure that I would not have continued very long after 1927 in any career at all; but a question which might be asked is whether the necessity of keeping very carefully to the diabetic routine affected the course of my scientific career after that date. In secondary ways it may have done so. In my experience even the well-controlled quasi-normal diabetic is not quite normal: there are certain occupations or occupational activities which have to be avoided as they may involve physical exertion of such an extent or character as to give rise to reactions which might be harmful in their effect on the diabetic himself or on others; and the necessity for a regular routine in meals and injections makes some kinds of travelling, both of short and long duration, very troublesome to arrange and sometimes quite unmanageable.'

I now omit the third section of his narrative which deals with vacation activities, and which is of interest because he states that he once wrote a book about school life, and pass to the fourth section which deals with his Cambridge life and his friends, beginning with his undergraduate life, 1918-1921. In his first year he occupied a set of attic rooms in Emmanuel Old Court, and in his second and third years he shared a spacious set of rooms in Front Court with a fellow undergraduate, Robin Hill (Robert Hill, F.R.S.) who became a lifelong friend; he represented the Royal

Society at the funeral service at Lawnswood Crematorium, Leeds, on 1 January 1969.

Stoner seems to have enjoyed a reasonably pleasant social life in his College. He was Secretary and later President of the Emmanuel Scientific Society, and was able to enjoy himself on the tennis courts and on the Cam. He refers with affection to Robin Hill, Malcom Dixon (F.R.S.), and Eric Mobbs who joined the Indian Forestry Service and later worked in Bangor. He likewise refers to his Tutor, Alexander Wood, whom he describes as an excellent 'straight' lecturer in physics whose researches on the acoustics of buildings were secondary to his teaching, but nevertheless were distinguished. He also comments on Wood's political and religious activities and views which were sincerely held but not universally popular. [I, too, remember Wood with affection, for I demonstrated with him in the elementary laboratories in the Cavendish. I also remember his views and that in the dark days of the war (1939-1945) Wood was advertised to give an evening talk in Nottingham in support of sending food to Greece. I found, as I suspected, that he was preparing to undertake a very late overnight return journey to Cambridge, which would have been rather appalling; and he was persuaded to stay the night with us. L.F.B.]

In order to preserve what I think is a desirable sequence, I now pass to the fifth section of Stoner's narrative, and reproduce it almost completely, the only omissions being a redundant opening sentence and a sentence which might, if included, possibly be hurtful to a living person. His remarks concerning mathematics should be particularly noted, because of the mystery of how he acquired the mathematical knowledge which led him to such eminence in theoretical physics.

'The subjects I read for Part I of the Natural Sciences Tripos were Botany, Chemistry and Physics. I was a little surprised that mathematics as such was not among the suggested choice of subjects, but at that time mathematics could not be included even as a half subject in that Tripos. I took the first-year examination, "Mays", in 1919, and did sufficiently well for my College status to be changed from Exhibitioner to Scholar. In the following year I gained a First in Part I, and went on in my third year (1920-1921) to Part II in Physics.

'Some of the lectures in Physics in both Part I and Part II were fairly mathematical in character, but, like many others, I had to fill in the necessary mathematical background by *ad hoc* study on my own. This has left me with considerable scepticism about the need for long formal courses for those who wish to learn about anything, but at the same time I feel that the casual attitude about mathematics was a serious defect in the Cambridge teaching of intending physicists at that time. For myself, my resulting "non-professionalism" in mathematics has been a drawback, and I have always been at some pains to try to ensure that the young physicists with whose teaching I have subsequently been concerned do not suffer from the same disadvantage.

'The other general matter on which I wish to comment is connected with the tutorial method of teaching, which is often regarded as a hall-mark of excellence of the older Universities. During my first term a fellow undergraduate and I wrote short essays (little more than answers to actual or possible examination questions) on topics in physics, and at weekly meetings of less than an hour with our tutor (as "supervisor in physics") these were commented on briefly, and there followed some rather perfunctory discussion on such points as we could think of as suitable to raise. This was the whole extent of the academic supervision I had (in all subjects) over my first two years. In each of the three subjects there were usually three lectures per week (to large groups, of the order of 200) and six hours laboratory, so that during the two years of study for Part I of the Tripos less than 1 per cent of the class time was occupied by individual or small group tutorial sessions.

'There were, of course, occasional discussions with demonstrators in the laboratories, usually about the experimental work in progress, and [were] helpful; and the College Tutor (as director of studies) was responsible for advising about courses to be taken, and also (as Tutor) for helping, if approached, on more personal problems. The point of special interest is that individual teaching played a very small part indeed in the teaching at Cambridge, at least in the Natural Sciences. I could never understand in later years how the myth had arisen that it was otherwise.

'In the Part II year a fellow-undergraduate and I had, by arrangement, some eight periods of supervision from one of the staff members of the Cavendish Laboratory, thinking that we might get some useful hints about examination "technique" and the like; but the supervision was quite useless in this respect and not very helpful in any other. The general distribution of class time between lectures and laboratory was much the same as for Part I, but the overall total was less. The classes were much smaller, and this gave more opportunity for getting to know and talking to individual members of staff. The most useful discussions, however, were those with fellow-students also reading for Part II, and I still remember in particular pleasant evenings with A. C. G. Menzies (who was to precede me at Leeds), when the sorting out of awkward problems in physics was happily combined with lighter entertainment.

'Of the subjects available in Part I other than Physics and Chemistry, Botany was my own first choice, for I wanted to correct for the absence of biology in my school teaching, and I already had an interest, quite unscientific, in plants and flowers. The basic lectures were by the Professor, A. C. Seward [F.R.S.] who, though not exciting in manner, was clear and fluent, and drew many illuminating sketches on the blackboard quickly and well; his comprehensive survey started with algae and fungi, about which I knew virtually nothing, so that a new world was opened at once. The associated laboratory work was concerned with examining plants of all the main groups by eye assisted by lens and microscope, cutting and staining sections, and making careful drawings of what was observed; all of which, with so much new, I found most interesting and enjoyable. In another group of lectures on Genetics, by R. C. Punnett [F.R.S.], I was introduced to the exciting and relatively new work on chromosomes and heredity by an active worker in the field.

'Chemistry, which I had liked at school, I found disappointing at Cambridge. The lectures on Organic Chemistry, by Pope [Sir William Pope, F.R.S.], covered a great deal of ground efficiently, but they were formal, factual and dull; and the associated laboratory work was trivial. In contrast, the lectures on General and Physical Chemistry given by [H. J. H.] Fenton [F.R.S.], who was acute and critical and a master of his subject, were among the best I had in Cambridge. The laboratory work was more varied than that in organic chemistry, but it did not match the level of the lectures.

'In Physics the lectures of Wood on properties of matter, light and sound, and of Whetham [Sir W. Dampier Whetham, F.R.S.] and J. A. Crowther [later Professor of Physics at Reading] on heat and electricity respectively gave a good general survey of the field of classical physics. Partly, I suppose, because in it I had my first experience of a really good lecturer, Wood's course in my first term on properties of matter still seems to me the best extended course of lectures I have ever attended. There was history in them, a sense of development, good demonstrations, and many touches of unforced humour. Most matters of importance were dealt with in a stimulating way, but without elaborate detail. (It is worthy of note that Alec Wood had already given essentially the same lectures for a considerable number of years and was to give them for many more.) The other lectures in this basic group were good in presentation and material but they were more ordinary. A series of some twenty lectures by Rutherford, which dealt with properties of matter more from an atomic standpoint, were unsystematic, and often casual and ill-prepared, but they were stimulating when he could bring in themes which connected with his current or earlier research.

'The laboratory classes for Part I Physics, under the very active direction of G. F. C. Searle [F.R.S.], were very well organized. His methods and experiments have been widely followed in other physics teaching laboratories. Many experiments were available (different groups in different terms), nearly all well thought out, and some very ingenious. They were normally designed to occupy one two-hour period, and fairly detailed instructions were given in a "manuscript". In consequence the experiments could usually be done "satisfactorily" without much thought or lasting impression, but they inculcated a feeling for care and accuracy in observation, and provided a means of gaining a wide experience of basic experimental methods and procedures. [I seem to remember one such manuscript on the Clement and Desormes experiment with one half-page of experimental directions and nine pages of suggested corrections to the observations. L.F.B.] I came to have a real liking for Searle, little though I could agree with many of his provocatively expressed views. With his early distinction in research, his later unquenchable enthusiasm for laboratory teaching, his fund of surprising anecdotes, and his strange mixture of roughness and kindness, he will long be remembered.

'The lectures on Physics for Part II (1920-1921) were an odd mixture, and there was no sign of any attempt having been made to develop a coherent programme. Searle's lectures on Heat and Electricity had obviously once been prepared with care and thoroughness. There were elaborate mathematical and experimental details of standard work carried out in the past, but no impression was given of Physics as something developing freshly now. The only other long series of lectures was that by C. T. R. Wilson on Atmospheric Electricity. On the rather specialized theme the material was good and up-to-date, but Wilson, though one of the nicest of people, was the worst lecturer among those giving long courses that I have ever experienced: halting, nervous, and so quiet as often to be inaudible from the front row. [In spite of this, Dr R. Hill says that Stoner attended until the end of the course.] All the other courses were short (4 to 10 lectures), and although they were fairly good in themselves, it was nobody's business to provide the links, and they were just a collection of snippets. The Part II programme of lectures on Physics, in short, was not a programme at all. [I would like to add to Stoner's comments about C. T. R. Wilson. It happens that I voluntarily attended this course in 1922-1923. The lectures reminded me of the story of an Australian local preacher who, when asked why he was so successful in the pulpit, replied that he took his text and divided his sermon into three parts; in the first he told his congregation what he was going to tell them, then he told them, and then he told them what he had told them! But, those somewhat halting lectures opened to me a new realm in physics. Wilson spoke of much that was not readily accessible in books, and he gave me an interest which I have never lost. I suspect that Stoner did not read the one French and the one German book which Wilson recommended us to read. L.F.B.]

'The laboratory classes for Part II Physics were conducted by H. Thirkill [Sir Henry Thirkill, C.B.E.], then a Fellow and Tutor of Clare, with Appleton [Sir Edward Appleton, G.C.B., F.R.S.] and others as demonstrators. . . . I have lost the record of my laboratory work, but I remember enjoying some of the experiments on optics and on radioactivity. Most of the experiments, however, seemed to have their *raison d'être* in some mild constructional work involved, or to have been built round miscellaneous pieces of equipment which had been haphazardly accumulated in the passage of years. The whole thing was casual, and nobody seemed to be responsible for any systematic development of the laboratory.

'Although the organization of lectures and laboratory classes for the more advanced work in Physics at Cambridge seems to me even more haphazard in retrospect than it probably did at the time, it was probably no worse than in other universities, and may well have been better. The necessary sorting out and linking together was no doubt done to some extent in the way implied by the old phrase "reading Physics"; it was expected that at least those taking Part II would read on their own fairly extensively, and most of them did. Moreover, most of their "teachers" were making or had made interesting contributions of their own in the field of physics; and through them or others some awareness got through of the exciting research in progress at the Cavendish. At any rate, as the year went by, my interest in physics increased, and I was very anxious, whatever I might do later, to spend some time "doing research" at the Cavendish.

'I still remember the sense of freedom that I felt at the thought, when writing the last sentence of my answers to the final Part II Physics paper, that this was probably the last formal academic examination I would have to take. Although I was acutely conscious of glaring shortcomings in dealing with some of the questions, on the whole I had enjoyed the examination, and felt that a fair proportion of my answers were reasonably good. Though not entirely surprised, I was elated to find my name in the First Class group; for Rutherford, whom I had rather nervously approached about staying on, could hardly decline now to recommend me for a grant for research.'

Cavendish period

Let us now turn to that fourth part of Stoner's narrative which deals with his experiences in the Cavendish. He lived in College during the Long Vacation term of 1921, when he started his first research work with Gilbert Stead, who later became Professor of Physics at Guy's Hospital, London, S.E. He then moved into lodgings with Mrs. Chiddenton in Warkworth Street, about half a mile from Emmanuel College, where he lodged for the rest of his time in Cambridge. He found the research group of about 30 in the Cavendish to be a friendly one, and of such a size that he came to know most of them; he continued to maintain happy contacts with many of them over the years. He gives interesting, detailed descriptions of three persons—chosen because of the character and extent of his personal relationships with them—Ernest Rutherford, Peter Kapitza and Nazir Ahmad. I think it appropriate to give his comments on Rutherford *in extenso.*

'I had an enormous admiration for Rutherford, but I could add little new to all that has been said and written in proper praise of him and his achievements. I give only a brief indication of my own personal rather than scientific impressions. To balance the over effusiveness of some of the accounts of him I should perhaps say first that he was not invariably as helpful and stimulating to the young research student as is generally supposed. He was my official supervisor, but it was only after an initial period of about a year (during which I was under the immediate supervision of Mr G. Stead, in whose work on thermionic tubes Rutherford arranged that I should join) that I became directly responsible to him. He approved of a proposal which I submitted for a systematic quantitative investigation of X-ray absorption, and arranged for me to have the necessary basic apparatus. After that I was left very largely on my own.

'There was no closely similar work in progress in the laboratory, and the technical difficulties, with the resources available, were more formidable than I had anticipated. Progress for some time was inevitably slow, but I can recall no helpful suggestions from Rutherford, nor indeed any proper discussion with him about the course of the work. It was his practice to make occasional rounds of the research laboratories and to spend a short time, himself doing most of the talking, with each research student. He could be genially complimentary when manifest progress was being made. When things were going badly, however, he could make the most devastating comments in his naturally loud voice which could be heard far away. There was little opportunity to counter these comments, which often seemed to me extremely unfair and which, so far from acting as a stimulus to further effort (which may have been the intention), could be discouraging of any effort at all. I could never accustom myself to Rutherford's "bark", nor to his forceful dominance in discussions; and except once after my research student period, when I was talking to him in his own home, I never found conversation with him easy.

'All this, however, was only one side of Rutherford. He was very kind to me when personal difficulties arose, as in connexion with my period of illness and treatment at Addenbrooke's Hospital in 1923; and he saw that my X-ray work when I returned would be enormously facilitated by an associated observer, and spontaneously arranged for me to be joined by L. H. Martin [Sir Leslie (Harold) Martin, C.B.E., F.R.S., University of New South Wales] from Australia. He was, too, most generously appreciative of what he considered to be the good work I completed in this and other fields toward the end of my Cambridge period, and afterwards, I continued to regard Rutherford as of outstanding greatness in the field of the physical sciences; and to feel that his judgements of people and his views on academic and social problems, though often ill-considered in expression, were more often more nearly right in essentials than those of most other great scientists.'

[I, too, knew Kapitza and Ahmad; both were very dedicated physicists whom Stoner obviously greatly admired, but this is not the place to deal further with his kind remarks concerning them. One point intrigues me; the observant Stoner does not mention an extraordinary feature of Cambridge life in his time at the Cavendish; namely that it was 'not done' to acknowledge one's laboratory (or College) companions in the streets, etc. Those, like me, who were 'hybrids', i.e. graduates from overseas and provincial universities, did not appreciate this fashion or ever got used to it. But, I should add that I cannot ever remember passing Stoner in the street! L.F.B.]

Turning now to his remarks on postgraduate research in the sixth portion of his narrative, we have already mentioned work with Gilbert Stead. The researches on low-voltage glow in thermionic tubes containing mercury were successfully completed and the results were published in the *Proc. Camb. Phil. Soc.* Stoner describes Stead as a good physicist and he admired his great skill in glass-blowing—a skill which at that time was often demanded of Cavendish men. Professor Stead has kindly written that he quite soon realized that Stoner was a man of exceptional ability, whose friendliness and personal charm endeared him to the Stead family, so that he soon became a welcome visitor to their house, and on a number of occasions they visited Stoner in his rooms. He was very fond of children and he and their elder daughter—then about 3 to 5 years old—became firm friends. Stead remarks on Stoner's attack of diabetes and unfortunate lack of response to insulin, which, resulting in a very strict low-calorie diet, might well have ended his career as a physicist but for his determination to concentrate the whole of his limited stock of energy on his work and let everything else go. Stead mentions a comment by Whiddington [R. Whiddington, C.B.E., F.R.S.] that Stoner could get through twice as much work as anyone else because he needed no exercise! At first encounter Stoner in his young days gave the impression of being rather quiet and even shy, but one soon came to realize that he had strong views about such matters as the duties and responsibilities of citizenship, and he was always ready to speak out firmly in defence of his principles, and when doing so was no respecter of persons; it was his physical disability which brought out his full strength of character. When Stead met him after he went to Leeds, Stoner was always just the same, and his outstanding achievement in spite of his physical disabilities filled Stead with admiration.

Stoner seems to have suffered some impatience with the tribulations of experimental work, or at any rate with glow discharges, and he tentatively asked Rutherford if he could work on the absorption of X-rays by a chosen set of elements, a topic which had occurred to him during his reading of Sommerfeld's *Atombau und Spektrallinien*, and Rutherford provisionally agreed. The work started in August 1922, but it was seriously interrupted as we have seen by Stoner's illness in 1923. His idea was to measure the absorption of a range of approximately monochromatic X-ray beams by a chosen set of elements, and most of the apparatus was collected and assembled before his illness. Soon after he returned from hospital the apparatus was completed with the help of L. H. Martin, and the long series of measurements was eventually completed. Stoner considered it to be a useful piece of work but that it did not come up to his expectations for the elucidation of problems of atomic structure. The published account of the work, of course, contains no reference to the tribulations which he suffered in making a sensitive Compton electrometer work. Stoner also co-operated with Ahmad on related work with gamma rays, and he was present when Ahmad read their joint paper before the Royal Society.

I now quote again from his narrative, first adding that I was present when R. H. Fowler [Professor R. H. Fowler, F.R.S., Rutherford's son-in-law] subsequent to the meeting mentioned below, made a remark to Stoner: 'The other day you practically convinced me that the inner electrons in the atom must be distributed in a way quite different from what other people have imagined, but now I begin to have some doubts.' I could not see Stoner's face as he replied, very quietly, that the X-ray evidence supported his schemes. His narrative reads:

'My reading in connexion with the X-ray and the earlier work had made me familiar with the nomenclature of the X-ray and optical levels in atoms and their quantum specifications, and I gave a great deal of thought to the associated problem of the distribution of electrons among these levels. One night in May 1924, a distribution scheme occurred to me in which the numbers in full levels were simply related to the quantum numbers specifying them, and which seemed free from the (usually admittedly) arbitrary and unsatisfactory features in schemes previously proposed. I was very excited about this, and in the next few days I satisfied myself that it was consistent with the major relevant experimental findings. I wrote a brief note about the scheme for Rutherford and, in his absence, left it on his desk. He must have passed it on to R. H. Fowler (with whom, at this period, I had several most helpful discussions on theoretical points), for soon afterwards Fowler asked me to call on him to discuss it. He was favourably impressed and suggested that I should write a full and detailed paper about it. This I was only too pleased to do, and in July a paper on "The distribution of electrons among atomic levels" was completed. It was communicated by Fowler to the *Philosophical Magazine*, and appeared in the issue of October 1924.

'Probably no other single paper of mine has attracted so much attention. This is hardly surprising, for the theme was one of both basic and topical interest to the chemists as well as physicists, and in essentials it has stood the test of time. Later it would have been presented differently, for at the time of writing the paper neither electron spin nor quantum mechanics had been born. It is of interest to note, however, that an explicit statement is effectively made of what later became known as the Pauli exclusion principle, though it is presented more as having been arrived at inductively from experimental findings rather than as a basic axiom for a deductive treatment of electron distribution as in Pauli's paper in the following years.'

Thus, by the end of his final year at Cambridge, Stoner rightly felt that he had made a definite contribution to physics and he seems to have realized that his life's work would be on the theoretical side; he now began to think more about his future career. Industrial or school-teaching posts were clearly unsuitable for him, and there remained only the possibility of a university post of some kind outside Cambridge, as it appeared to him that the necessary financial support to enable him to remain there without undertaking a depressingly heavy load of coaching and supervision was unlikely to be forthcoming. To quote his own words:

'By this time I had in fact quite firmly decided that I did not wish to become a Cambridge don. There were many features of the general set-up at Cambridge which I had gradually come to feel were unsatisfactory: among them the peculiar dichotomy of the College, and the University; the obscurity of the interrelations between the "Departments" and both the University and the Colleges; and the lack of responsibility of the lecturers, except by chance, for any other teaching or even advising of those who attended their lectures, or for acting as examiners. Connected with this the salary arrangements seemed curious: except for those with the more remunerative University or College appointments a reasonable salary could be obtained only in a piecemeal way from a combination of minor appointments supplemented, if necessary or desired, by supervision and coaching.'

But, I would like to end this account of his views on the Cambridge scene by recalling that he wrote a very topical song which was published in *The Post-Prandial Proceedings of the Cavendish Society*, a collection of topical verse on modern physics and other matters written by members of the Physics Research Society of the Cavendish Laboratory (published by Bowes and Bowes, Cambridge). I believe that the song was entitled *Isotopes*, and was sung to the tune of the Grand Inquisitor's song from *The Gondoliers*, the popular Gilbert and Sullivan opera, which begins: 'I stole the prince, and I brought him here . . .' I quote the first verse from memory, for it always gives me much pleasure to remember this light-hearted and happy side of Stoner, and the occasions on which it was sung at the Annual Cavendish Dinner, including that at which we celebrated J.J.'s [Sir John Joseph Thomson, F.R.S.] 70th birthday:

'Since J.J. on the game began
By analysing Neon
Many a man had thoughts which ran
Beyond a paper's rightful span.
So did we all agree on.
It needs a man both strong and stout
These isotopes to sever.
Of that there is no manner of doubt—
No probable, possible shadow of doubt—
No possible doubt whatever.'

Section III. Leeds

Early work

Coming now to the final section of his narrative, having resolved to seek a post in a provincial university, he applied for posts at Durham and Leeds and was eventually appointed Lecturer in Physics at the latter University, with 'special conditions' attached to the appointment in view of his medical history. These conditions amounted to relieving the University of the obligation to pay his salary during absence from duty due to ill-health for more than specified periods related to the number of years of his service; happily, these conditions were never invoked, and he remained in the service of the University until his retirement in 1963. Happily, again, in 1928 Emmanuel College awarded him a Research Fellowship; this did not require residence in Cambridge but gave him the valued opportunities of making frequent visits, and long stays in Cambridge during long vacations. On joining the Leeds staff he resided as the only lodger or paying guest with Mr and Mrs Fletcher at 6 South Parade, Headingley, Leeds 6, until 1932, when he bought a house, 10 Winston Mount, Headingley, where his parents joined him from Cardiff in that year.

The contemporary Cavendish Professor of Physics and Head of the Department at Leeds was Richard Whiddington, F.R.S., who had been appointed in 1919. Under his guidance the honours school had flourished. An excellent short history of the Leeds Department from its beginnings in 1874 under A. W. Rücker [later F.R.S.] was written by Stoner for *The University of Leeds, The First Half Century*, by A. N. Shimmin (Cambridge University Press, pp. 139-144, 1954). In it Stoner gives a brief account of the research work in progress during his early years in Leeds. It was then that he began his distinguished work on the interpretation of the magnetic properties of many types of materials by his pioneer application of quantum mechanics, thermodynamics and statistical mechanics. Further information about the Department is given in an article which Stoner wrote on the occasion of Whiddington's retirement, published in the *University of Leeds Review*, **2**, 342-351, 1951. This is not the place to deal exhaustively with that article, but one cannot fail to note, in view of Stoner's own experiences, that Whiddington, sometime Fellow of St John's College, took Part I of the Tripos in Physics, Chemistry, Geology and Botany in 1907 and Part II in Physics in 1908—no Mathematics!

From Stoner's papers and articles one learns how excellent were the relations between Whiddington and himself. They obviously 'took to one another' straightaway, and this is amply confirmed in the letters which Richard and Catherine Whiddington have kindly written. Whiddington mentions the difficulty, in those days of acute financial stringency, of starting Stoner at Leeds with the salary and status which he truly deserved. He tried to protect Stoner from over-involvement in university administration and lecturing; but Stoner was such a good lecturer and really liked administration, and since later on Whiddington worked away from Leeds in his periods at the Admiralty, the protection was not great. However, they happily shared a common interest in the war-time development of the magnetron, in which Stoner gave considerable help on the theoretical side. Stoner became a firm family friend of the Whiddingtons; he was a frequent visitor to their cottage at Holme-next-the-Sea by King's Lynn, and he, an only child, seemed to have an instinctive knowledge of the games and books which would appeal to young children; and as he was a keen and knowledgeable naturalist his visits were much appreciated by the Whiddington children.

Stoner's advancement in Leeds was fairly rapid. He was promoted Reader in Physics in 1927 and Professor of Theoretical Physics in 1939; he was elected F.R.S. in 1937, and awarded the degree of Sc.D., Cambridge, in 1938. During the years 1940-1945 when Whiddington was seconded to government service away from Leeds, Stoner was Acting Head of the Department and shouldered the greatly increased load which then fell upon every Physics Department owing to service demands for radar officers under the State Bursar scheme. Incidentally, Dr C. P. Snow (Lord Snow), and his colleague H. S. Hoff who used to visit the Universities and University Colleges to interview their bursars, always referred to Leeds, Sheffield and Nottingham as the Magnetic Belt. At the end of the war Stoner was very anxious that some effective experimental work on the low-temperature magnetic and related properties of transition metals and alloys should be undertaken at Leeds, and he initiated a programme of work under the general supervision of the late Professor F. E. Hoare. After Stoner became Head of the Department he devoted much time and energy to raising the necessary funds for its support, and he was involved in some very protracted negotiations in obtaining a Collins helium liquefier for the Department. The provision of this machine proved invaluable in the development of important work on many aspects of solid state physics. I should like to add that it had important repercussions in Nottingham, where a Leeds graduate took a leading part in establishing low-temperature work there.

One can feel sure that he thoroughly enjoyed the whole range of departmental duties. He writes in his Record for the Royal Society:

'I enjoyed most aspects of the very varied university life and work, including teaching, research, the sorting out of troublesome problems in Departmental and University planning and administration, and the easy, friendly associations with colleagues and students. During a period of nearly forty years it was inevitable that there were occasional disappointments, though it was unfortunate that the latter part of my career, with its exciting promise and eagerly accepted challenge, should have been rendered less effective than it might have been by sequences of episodes of a Departmentally frustrating and personally hurtful kind; but on the whole my University life was a very happy one.' He made a similar remark to me, verbally, a few years ago, and I sensed that he much resented the difficulties which so severely curtailed the time and energy which he could have devoted to his own scientific work; one cannot fail to note that his major publications ceased in 1955. I am unable to add any details of these difficulties.

One is impelled to ask the question: did Stoner ever wish to return to Cambridge? The only evidence that he did, came from F. I. G. Rawlins,* who used to discuss rare earth problems with him and related that he happened to meet him in 1929 just after the appointment of a Professor of Theoretical Chemistry, when Stoner remarked that if he had known that the electors were 'going outside the list' he would have applied himself.

He spared no effort in furthering the welfare of all the members of his Department—academic and technical staff, undergraduates and research

* Obit. 2 May, 1969; see *The Times*, 7 May.

students. As Professor he took a full share—probably more than a full share—of departmental teaching duties in the laboratories and the lecture rooms. One of my colleagues at Nottingham, Dr Colin Matthews, himself a Leeds graduate, kindly confirmed this impression. He said: 'A major part of the Leeds honours course at that time was given by Stoner personally, for he gave courses on (1) Thermodynamics, (2) Statistical Mechanics, (3) Wave Mechanics, (4) Atomic Physics, and (5) Magnetism and Matter; the course was dominated by Stoner, and rightly so, for he was an ideal university teacher. In his rather quiet voice he spoke individually to one in lectures and not as if he were addressing a class. He was a most inspiring teacher. His office [at that time] was immediately outside the third year teaching laboratory, and one felt that the door was always open.' [I hope that the last remark was prompted by the knowledge that during the whole of the time that I occupied a Chair, my door, too, was always open! L.F.B.] 'Given a louder voice, better health and a wish to "blow his trumpet" he would have been more widely recognized for his work on metals, but he would have been quite different from the man we knew!' Professor R. S. Tebble of Salford University who carried out research at Leeds with Stoner some years ago, told me that his chief memory of working with Stoner was his infinite patience in explaining his ideas to those who worked with him; and one wondered, if in fact, progress would have been more rapid if he had done things himself—but he was tied up with the development of a General Studies Degree course and other administrative affairs.

Stoner served on several committees of the Royal Society, but the difficulties of travel and the responsibility of looking after his mother at times prevented that active participation in what is termed public work that I know he desired. He was a Member of the Board of Visitors to the Royal Observatory, 1952-1956; Member of the Advisory Council on Scientific Research and Technical Development, Ministry of Supply, 1955-1958; D.S.I.R. Visitor to the Wool Industries Research Association, 1955-1961; Member of the Postgraduate Training Awards Committee and Chairman of the Physics Sub-Committee, D.S.I.R., 1957-1962; Member of the Panel of Equipment Assessors for Physics, University Grants Committee, 1958-1964. [I had some experience of his work as an assessor and I considered it to be rather severe. L.F.B.] Although he felt that it would be out of proportion to comment on the above committees on which he served, he found the work to be so very diverse in extent and character that he recorded the following illuminating general impressions:

'There were some members of these committees who were of sound judgement and high integrity, but they often tended to be dominated by others who seemed to me to be surprisingly irresponsible and so self-centred (or "group centred") as to be unable to see that there were any views other than their own that were worthy of consideration. The agenda for the meetings were sometimes rather thin, but usually points of interest came up for discussion; and it was enjoyable to make or renew contacts with people whose primary duties and occupations covered a field wider than, or different from my own.

'Conscientious membership of a committee almost invariably involves a fair amount of "homework". In some cases, when any "special duties" were undertaken, the work involved was not only responsible but also, in the periods in which it was concentrated, very demanding. In one case in particular I found this work as worth while and as satisfying as any that has ever come my way. I did not seek committee work as such, but I was very disappointed that I was never invited to serve on one or two particular central committees whose work seemed to me so important and to which I felt I could have made a more than averagely useful contribution.'

Magnetism and Matter

The early experimental researches with Stead have already been mentioned, as well as the theoretical work on the scheme of electron levels in the atom which were later encompassed in Pauli's statement of the exclusion principle. It seems only natural for one to presume that Stoner's interest in electron energy levels should lead him to a deep study of the magnetic properties of atoms. As he mentions in the autobiographical note which he wrote for the McGraw-Hill *Modern men of science*, **2**, 525, 1968, he was struck with the huge quantity of experimental data on diamagnetic and paramagnetic properties of matter, data which were ignored in the then current textbooks. The result was that he felt constrained to write his first book *Magnetism and atomic structure*, which was published in 1926. I remember that I reviewed *Magnetism and atomic structure* for *Science Progress*, and in my review I described it as a very important book which would undoubtedly prove invaluable to all interested in magnetic research. I found it very useful in courses on magnetism given to honours physics students at University College London. I might add that my copy of this book was stolen, and that is one way of assessing its value. It definitely broke new ground by introducing quantum ideas in the elucidation of the magnetic behaviour of matter. About the same time, Stoner published a number of papers—some quite short—in the *Proc. Leeds Phil. Soc.*, which were of great value to those working in the experimental field. I believe that there is nobody who has done research in magnetism since 1926 who has not felt indebted to Stoner at some time or other for this pioneer work. After 1926, it was almost impossible to open any up-to-date book on magnetism which did not contain references to Stoner's original works.

To the same volume of *Science Progress*, 1926, Stoner contributed a very clear and up-to-date article on recent developments in magnetism which nevertheless contained the following statement: 'The magnetic properties of atoms are little elucidated by the spinning electron hypothesis. To account for the Landé splitting factor g, it is necessary to assign a double magnetic moment to the electron, and a number of artificial features have to be introduced. As an illustration of this artificiality, the unit magnetic moment of alkali atoms is to be regarded (formally) as arising from a zero contribution from the orbital motion of the electron, and unit moment from the spin (that is, double the angular moment). The gyromagnetic anomaly is still unexplained unless the effective ions in all the ferromagnetics examined are in an S state.' Stoner very rarely made an incorrect deduction, and the foregoing quotation, I think, really emphasizes his cautionary approach to new and revolutionary ideas. Later on, of course, he made full use of those ideas and of Fermi-Dirac statistics in his explanation of the fact that the atoms of ferromagnetic metals do not exhibit integral numbers of Bohr magnetons at low temperatures.

Stoner also wrote the Magnetism section for the *Encyclopaedia Britannica*, 14th edition, **14**, 636-667, 1926, and *Magnetism*, a Methuen Monograph on Physical Subjects, which ran to four editions and appeared in a Russian translation by J. Dorfman, 1932, and in an Italian translation by Margherita Bernini, 1955.

Magnetism and atomic structure did not have a very long life. The concepts of electron spin and of spatial quantization, and a series of theoretical investigations, particularly those of Van Vleck on electric and magnetic susceptibilities, and the gradual replacement of the Weiss by the Bohr magneton, meant that the book had either to be substantially revised and enlarged or be entirely rewritten. He made the latter choice, and the result was the publication in 1934 of *Magnetism and matter* (pp. xvi+576, Methuen, London)—a book with a much wider content than its predecessor. [I also reviewed the new book for *Science Progress*, and I have preserved my copy!] It is a book which for a long period stood in a class by itself and can still be read with profit. Professor D. H. Martin, himself the author of a recent high-standard book on magnetism, has written that Stoner's book *Magnetism and matter* (forming a triumvirate with Van Vleck's and with Bates's) made sense of the subject and its basic importance for many physicists and for many years. Some time before the end of his life, on one or two occasions, I tentatively enquired about the possibility of a second edition; he was very definite that he did not wish to take on such a heavy task and preferred to make other contributions to the subject. Such are to be found, I think, in his two articles in *Reports on Progress in Physics*, 1948 and 1950, and in the 39th Guthrie Lecture. These articles were eminently suitable for young physicists coming new to magnetism after the war, since from them they could get a good appraisal of how much of importance was known in magnetism and how much was not.

Work on white dwarf stars

It does not appear to be very widely known that for several years Stoner maintained an active interest in astrophysical theory, which no doubt accounted for his pleasure in serving for a time as a member of the Board of Visitors of the Royal Observatory. Apart from an early paper in which he discussed some current hypotheses about energy generation and the origin of cosmic rays (*Proc. Leeds Phil. Soc.* **1**, 349, 1929), Professor W. H. McCrea [F.R.S.] informs me that his astrophysical work was on dense stars, to our knowledge of which he made significant contributions. He was the first to give the accurate formula for the internal energy of a completely degenerate electron gas, taking account of relativistic effects (*Phil. Mag.* (7) **9**, 944, 1930). This is required for the study of white dwarf stars, and in the same paper (*vide* also *Monthly Notices, Roy. Astron. Soc.* **92**, 662, 1932) Stoner announced his discovery of the existence of a maximum possible mass for such a star; W. Anderson independently made the same discovery about the same time. This was in effect the famous 'Chandrasekhar limiting mass', so-called because in 1935 S. Chandrasekhar [F.R.S.] was the first to isolate it in a full treatment of the problem according to the theory of stellar evolution. The work of Stoner and McDougall on Fermi-Dirac functions, mentioned below, was important for the study of certain problems of stellar structure and was later used therefor by Chandrasekhar.

About this time (1934) Stoner became interested in the specific heats of ferromagnetic materials in the neighbourhood of the ferromagnetic Curie point, and some interesting experimental work by Grew on nickel came from the Leeds Department; it bore the marks of Stoner's analysis. [I also noted that he included some remarks on my work on the specific heat of the ferromagnetic substance, manganese arsenide, in *Magnetism and matter*.] He was keenly interested in the quantitative behaviour of the intrinsic magnetization of ferromagnetic metals with temperature—known as the law of corresponding magnetic states—because he wanted to determine what spin value $\frac{1}{2}$, 1, $\frac{3}{2}$, etc., should be attributed to an atom in a given ferromagnetic metal, and there again came from Leeds some interesting results which have frequently been quoted and used in discussion.

In March 1937, there was communicated to the Royal Society and later published in its *Transactions* a very important paper on 'The computation of Fermi-Dirac functions', by Stoner and McDougall. Its opening paragraph gives the *raison d'être* of the paper: 'The quantitative application of Fermi-Dirac statistics involves the evaluation of certain integrals which have not previously been tabulated. In this paper, tables are given of the values of the basic integrals most frequently required, with a view to placing Fermi-Dirac statistics on as firm a numerical basis as is Maxwell-Boltzmann statistics.' The heavy numerical work was successfully carried out with the aid of Brunsviga calculating machines. The Summary at the end of the *Transactions* paper (**237**, 104, 1938) is now reproduced in order to give an idea of the magnitude of the work involved in it:

'The application of Fermi-Dirac statistics to physical problems (examples of which are indicated) requires the evaluation of integrals of the form $F_k(\eta) = \int_0^\infty \{x^k/(e^{x-\eta}+1)\}dx$, especially for $k=\frac{1}{2}$ and $k=\frac{3}{2}$, and of a number of related functions.

'This paper is primarily concerned with the evaluation of $F_{\frac{1}{2}}(\eta) = F$, from which the other functions may be obtained, for a wide range of values of the argument η. Series expansions, which are available for $\eta \gg 1$ and $\eta < 0$, corresponding to $\epsilon_0/kT \gg 1$ and approximately $\epsilon_0/kT < 1$ (ϵ_0 being the maximum particle energy in the Fermi-Dirac distribution at absolute zero), are studied in detail and are employed in the calculation of F for $\eta \geqslant 16.0$ and $\eta < 0.0$. The determination of $F(0)$ is carried out by means of a relation between the functions $F_k(0)$ and the Riemann zeta functions. For values of η between 0 and 16, the computations are made by numerical integration methods, supplemented by the use of series for the initial and final parts of the x range. A direct method is used for $0.0 < \eta < 3.0$, but for $3.0 \leqslant \eta \leqslant 16.0$, a modified procedure greatly reduces the work of computation.

'From the $F_{\frac{1}{2}}(\eta)$ table so obtained, values of $F_{\frac{3}{2}}(\eta)$ are found by numerical integration, and of the derivatives F', F'' and F''' by numerical differentiation. The final table gives, at tabular intervals $w = 0.1$, the values of the functions $\frac{2}{3}F_{\frac{3}{2}}(\eta)$, $F_{\frac{1}{2}}(\eta) = F$, wF', w^2F'' and w^3F''', to six decimal places for $-4.0 \leqslant \eta \leqslant +4.0$, and to five decimal places for $4.0 \leqslant \eta \leqslant 20.0$. Convenient methods for direct and inverse interpolation are described.

'Some properties of the $F_k(\eta)$ functions, defined only for $(k+1)$ positive, are discussed, and an analytic continuation of the functions, obtained in an Appendix, enables these properties to be established for a wider range of k values.'

And, again, there arises the question, how did this very remarkable man acquire his knowledge of mathematics?

The tabulated values of the Fermi-Dirac functions given in the paper which has just been described, were used in two very important papers by Stoner on Collective Electron Ferromagnetism described below, as well as in detailed papers on the thermodynamic functions for a Fermi-Dirac gas. The functions have also been extensively used by theoretical workers on semiconductors.

The collective electron theory of ferromagnetism

The papers on collective electron theory were published in the *Proc. Roy. Soc.* for April 1938 and February 1939. In the first paper, by using Fermi-Dirac statistics he obtained general equations for the magnetic moment M of a number N of electrons each of moment μ in an unfilled band of standard parabolic form, for which the interchange interaction effects give rise to a term in the energy expression for a ferromagnetic which is proportional to the square of the magnetization. He treated the relative magnetization ζ (or $M/N\mu$) at a stated temperature as an implied function of the reduced field, $\mu H/\epsilon_0$, reduced temperature, kT/ϵ_0, and an interaction energy coefficient, $k\theta'/\epsilon_0$, where ϵ_0 is the maximum particle energy at absolute zero.

Extensive computational work, based largely on the use of the Stoner and McDougall tabulated tables of Fermi-Dirac functions, enabled him to obtain a series of values of kT/ϵ_0 as a function of ζ for a series of values of

$k\theta'/\epsilon_0$. He found that the character of the dependence of ζ on kT/ϵ_0 (or on T/θ) depended on the ratio $k\theta'/\epsilon_0$, the classical results being obtained when $\epsilon_0/k\theta' \to 0$. He found that a necessary condition for the existence of ferromagnetism was $k\theta'/\epsilon_0 > 2/3$, while for $k\theta'/\epsilon_0 < 2^{-\frac{1}{3}}$ (i.e. 0.793 701) the relative magnetization ζ (i.e. $M/N\mu$ at 0°K) was less than unity. For small values of ζ, the magnetization-temperature curve closely followed the equation $(\zeta/\zeta_0)^2 = 1-(T/\theta)^2$, but it did not change monotonically to the classical curve as $k\theta'/\epsilon_0$ increased. [There are many other interesting features of the curves which cannot be discussed here.] Important expressions for the variation of magnetization at low temperatures and near the Curie point were derived; and there disappeared that major difficulty of the classical theory; viz. the lack of agreement between the value of the saturation magnetic moment of a metal atom derived from its paramagnetic behaviour above the Curie point and that derived from its low temperature ferromagnetic behaviour. There is good reason to believe that Stoner really enjoyed computing. Indeed, when he started on the important joint work with Wohlfarth which is described later in this memoir, he was somewhat disappointed that Wohlfarth obtained analytically so many results for which they did not need to use the old Brunsvigas.

The second paper on collective electron ferromagnetism is a treatment of the energy relations, corresponding to that for the magnetic characteristics in the first paper. Consideration of the energy relations was particularly important because of the difficulty of deducing the intrinsic magnetization from the experimental measurements of apparent magnetization, because to these latter results must be added complementary information derived from magneto-thermal effects—such as the behaviour of the specific heat of the ferromagnetic above and below its Curie point—and its magneto-caloric behaviour. The replacement of the classical by Fermi-Dirac statistics gave a remarkable modification in the theoretical specific heat relations, especially in their limiting forms for the specific heat of a ferromagnetic at the Curie point. The long-known discontinuity was shown, in general, not to depend on the (Weiss) 'magnetic' contribution alone, and therefore it alone could not give an immediate, direct measure of the interchange interaction effect as was previously thought; and comment was made in the paper on the (then) absence of a satisfactory explanation of the 'tail' in the specific heat-temperature curve close to but above the Curie point. He dealt with many of these matters in his Kelvin Lecture, 1944.

Strasbourg, 1939

At a Réunion sur le Magnétisme held at the University of Strasbourg, 21 to 25 May 1939, the officially invited British members were Mott, Simon Stoner and Sucksmith; I was very glad to be present as a kind of supernumerary as the guest of Professor Pierre Weiss. I need hardly add that overseas conferences were very rare events between the wars, and this was the first of a series of truly international conferences on magnetism. It saw the demise of the Weiss magneton, the introduction by Louis Néel (For. Mem. R.S., 1966] of the concept of fluctuations in the neighbourhood of the Curie point, and the concept of antiferromagnetism. I did not at the time fully appreciate the importance of Stoner's contribution to the collective electron theory which had only just been published, and he did not read a paper at Strasbourg, presumably because he had been so busy writing papers for our *Proceedings*! But, I find that in the discussion of Néel's very comprehensive paper 'Champ moléculaire, aimantation à saturation et constantes de Curie des éléments de transition et de leurs alliages' (*Le Magnétisme*, Vol. II, p. 158; Institut International de Coopération Intellectuelle, Paris, 1940)* the Rapporteur wrote a section beginning: 'Le Prof. Stoner expose ses travaux sur les bandes paraboliques; il rend compte des calculs précis qu'il a fallu faire pour avoir des resultats exacts. Il a eu l'idée d'appliquer la statistique Fermi-Dirac et de la comparer avec la statistique classique . . .' Later in the same section, Stoner continued: '. . . les courbes expérimentales qu'on calcule avec les statistiques Fermi-Dirac pour l'aimantation au-dessous du point de Curie sont toutes au-dessous de la courbe expérimentale et les differences sont notables près du point de Curie. Il faut donc avoir une autre explication de ces anomalies; celle du Prof. Néel avec les fluctuations paraît très bonne.'

Stoner could be a very tough opponent; once he had taken up a position which he thought correct, it was almost impossible to dislodge him. In *Le Magnétisme* (*vide* Vol. III, p. 304) there is a long section from which I quote for reasons which I think are obvious:

'Le Prof. Stoner voudrait dire quelques mots sur les parties du rapport du Prof. Foëx qui commencent aux pages 189 et 232.

'A la page 232, on trouve une courbe pour le platine, pour l'inverse des susceptibilités, dont l'allure est à peu près la suivante.' [I omit a roughly drawn curve—not a straight line—or graph of $10^{-6}/\chi$ against T °K. L.F.B.]

'Le Prof. Stoner se propose de discuter cette figure du point de vue des bandes électroniques.'

'Quant aux résultats expérimentaux, on peut dire qu'ils sont répresentés par une courbe, avec des écarts, ou même, comme dans la figure (39), par une série de droites, avec des écarts. Avec les droites, on fait les extrapolations, on trouve des points de Curie négatifs, et l'on calcule les moments. En conséquence de cette méthode de discussion, le Prof. Foëx explique qu'il y a une série de variétés de platine, chacune avec un champ moléculaire très grand et négatif. Du point de vue des bandes, cette conclusion semble tout à fait incorrecte.

'En effet, aux basses températures la chaleur spécifique dépend des mêmes éléments que la susceptibilité paramagnétique. Alors, si l'on connaît la valeur de la chaleur spécifique, on peut calculer la valeur de la susceptilbilité pour les mêmes électrons, sans interaction. On a pour les basses

* I understand that copies of this book, which arrived in England in 1945, are extremely rare; e.g. there is no copy in the National Library of France, Paris.

températures, la formule: $C_A/\chi_A = (\pi^2/3)\ (k/\mu)T$, d'où $\chi_A \times 10^6 \approx 5{\cdot}6\ (C_A/T) \times 10^4$. Pour le platine, on a le résultat expérimental, $(C_A/T) \times 10^4 \approx 16$, et l'on calcule $\chi_A \times 10^6 \approx 90$. La forme générale des courbes $(1/\chi,\ T)$ est représenté dans la figure suivante. [Fig. 49, a gentle curve, concave upwards, is omitted here. L.F.B.] Or, la valeur observée pour la susceptibilité du platine aux plus basses températures est $\chi_A \times 10^6 \approx 240$, c'est-à-dire que la valeur est plus grande $(1/\chi$ plus petit) que celle calculée pour les électrons sans interaction. On peut donc conclure que pour le platine le champ moléculaire n'est pas négatif, mais, au contraire, positif et grand (voir la figure).

'Si l'on fait une extrapolation d'une droite tangente à la courbe, on trouve un point de Curie négatif, mais on ne doit pas conclure que le champ moléculaire soit négatif. Si l'on considère les différentes parties de la courbe on trouve évidemment les valeurs de θ différentes. Le Prof. Stoner pense, dans ces conditions, que c'est une méthode tout à fait artificielle. Il est évident que la forme exacte de la courbe dépend de la forme des bandes, mais les valeurs aux basses températures ne dépendent pas de la forme des bandes et les formules au-dessus sont indépendantes de la forme exacte de la courbe. [Since Foëx had attributed the paramagnetism of metallic chromium and metallic manganese to antiferromagnetic coupling, the report continued.] 'Il pense que la même façon de parler s'applique également aux autres métaux, par exemple, aux métaux de transition, comme le manganèse. Malheureusement, quand on ne possède pas la valeur de la chaleur spécifique on ne peut faire de semblables calculs. Avec le palladium, il en est de même. On calcule de la chaleur spécifique du palladium, une valeur pour la susceptibilité qui est beaucoup plus petite que celle qui est observée.

'Le Prof. Stoner n'est pas convaincu qu'on ait trouvé, parmi les métaux, un seul cas de ce qu'on appelle l'antiferromagnétisme; il ne croit pas que les substances dites antiferromagnétiques le soient on réalité.' [Stoner then made some remarks about the special case of gadolinium and the complicated nature of nickel.]

The Chairman (Pierre Weiss) indicated that Stoner's intervention necessitated an immediate discussion and called on Foëx to reply. Foëx maintained that his treatment of the curves was legitimate because it was 'imposé par l'expérience'. The report continues (p. 309) and we read:

'Le Prof. Stoner pense qu'il est très difficile de discuter les faits expérimentaux trouvés par le Prof. Foëx. Quand on voit beaucoup de lignes droites comme dans la figure de la page 233, cela paraît artificiel. Il ne pense pas que cette courbe signifie qu'il y ait beaucoup d'espèces de platine.

'Le Prof. Foëx pense que c'est une manière de dire qu'on obtient des décompositions en droites, chacune étant bien caractérisée et cela lui paraît incontestable du point de vue expérimental.

'Le Prof. Stoner est d'avis qu'on a cherché des droites avec trop d'enthousiasme. Il ne peut pas s'expliquer tout ce que les expérimentateurs ont découvert. Surtout la foule de variétés de platine décrites par le Prof. Foëx ne lui semble pas vraisemblable.

'Le Prof. Foëx dit qu'il est très facile de nier les expériences qu'on n'a pas faites.

'Le Prof. Stoner ne nie pas les expériences, mais il ne peut pas expliquer les droites que l'on trouve autrement que par des effets secondaires. Pour lui, on n'a pas su voir la forêt à cause des arbres.

'Le Prof. Foëx indique que si le cas était isolé, il serait de l'avis du Prof. Stoner, mais il s'agit de phénomènes que l'on retrouve souvent.

'Le Prof. Stoner maintient son point de vue et sans nier l'existence des droites, pense qu'il faut les expliquer d'une autre manière.

'Le Président pense qu'il ne faut pas douter d'un phénomène parce qu'on ne le comprend pas.

'Le Prof. Néel est d'accord avec ce que vient de dire le Président mais il est également d'accord avec le Prof. Stoner dans ce sens que la multiplicité des droites n'a pas de signification fondamentale.' [Néel then went on to discuss the sign of the internal magnetic field.]

In Strasbourg, Stoner and I stayed at La Maison Rouge, Place Kleber. At our first breakfast—where Stoner asked for 'deux œufs plats sur une assiette'—as we had not really met for some time we exchanged a lot of news and stories, and I noticed that four young waiters gathered round our table, obviously within earshot. On leaving the hotel, we were accosted by an Englishman who introduced himself as the British Consul and asked us how the Réunion was progressing. I took the opportunity of asking him why the young waiters seemed to be so interested in our conversation, and he replied—'Well, they are English! They have come here to learn the French language and hotel arrangements, and four French boys are likewise in London on an exchange basis.'

Grenoble, 1950

Stoner attended the Second International Conference on Magnetism, held at Grenoble from 3 to 6 July 1950, when the British invited participants were Hoselitz, Kurti, Roberts, Stewart, Stoner, Sucksmith and myself. It was here that he gave an excellent survey of collective electron ferromagnetism in metals and alloys (*J. Phys. Radium, Paris*, **12**, 372-388, 1951; Stoner's paper is in English). It was remarkable for its very clear, simple statement of the fundamental premises of the basic treatment of his theory and the excellent résumé of the general character of the theoretical treatment, together with a penetrating account of its applications to the behaviour of metals and alloys. Here, he also dealt with a refinement of the theory due to Wohlfarth (*Proc. Roy. Soc.* A, **195**, 434, 1949), which takes account of a 'transfer' effect. In Stoner's original theory a single band was considered and the number of available electron spins was assumed to be fixed. In nickel, however, the total number of electrons is just sufficient to fill the *d* band, but owing to the overlapping of the *d* and *s* bands, there are a number of 'holes',

N_d, in the d band which must be equal to the number of electrons, N_s, in the s band. At absolute zero the number per atom is close to 0.6, but with increase in temperature the electrons move to higher energy states, which in the s band are virtually unlimited; consequently, the number in the s band increases and so does the number of 'holes' in the d band. The 'holes' in the d band are held responsible for the ferromagnetic characteristics, and the relative contributions of the s band electrons to the paramagnetic susceptibility are treated as small, but neglible. The result is that the inverse susceptibility should increase more slowly with increase in temperature than is predicted by the single band treatment, and this removes the remaining differences between the experimental $(1/\chi, T)$ curves and the theoretical ones.

The true importance of all this work is that it provides an alternative model—called the collective-electron or itinerant-electron model—to the Heisenberg localized-electron model of a ferromagnetic. The two models have recently been critically compared by Professor D. H. Martin (*vide Magnetism in solids*, Iliffe Books Ltd, London, 1967, chapter 4). The localized electron model seems to be the more appropriate in dealing with the rare earth metals and non-metallic solids; and the Stoner model to be so with the iron group metals. However, recent work on electron band parameters, the excitation of spin waves, and extensions of the simple itinerant-electron model by the introduction of spatial correlations to it, indicate that the latter model has an assured future, and one now finds Stoner's name occurring in the current literature even more frequently than at any previous time. Moreover, Rhodes & Wohlfarth (*Proc. Roy. Soc.* A, **273**, 247, 1963) have developed a criterion, based on Stoner's fundamental concepts, for deciding which of the two models is the more appropriate for particular materials. Their treatment requires the determination from experimental data of the ratio (q_c/q_s), where q_c and q_s are the effective numbers of the magnetic carriers per atom as deduced from the $(1/\chi, T)$ curves at high temperatures, and from the low temperature saturation magnetization, respectively. When (q_c/q_s) is significantly < 1 the itinerant electron model is the more appropriate. In this case, Rhodes & Wohlfarth have further shown that both theory and experiment show that the value of the ratio tends to be large for materials with low Curie temperatures. [During the discussion on Stoner's Grenoble paper, W. Shockley, then at Bell Laboratories, Murray Hills, U.S.A., showed an interesting mechanical model which enables one to visualize the distribution of electrons between two bands. It is described at the end of the published paper, *loc. cit.*; but I have not seen a description of it elsewhere. L.F.B.]

It should be emphasized that one of the great advantages of the Stoner treatment of ferromagnetism is that it lies within the general framework of the electron theory of metals, so that the magnetic properties of metals and alloys may be correlated with other properties such as specific heats, electronic energy band structures, Fermi surfaces and transport properties. For many years after Stoner's original formulation his treatment was not widely accepted, but recent experimental work on Fermi surfaces and related topics has fully supported his approach. The importance of a further concept introduced by Stoner, that of exchange enhancement of spin susceptibility, has also recently been widely recognized. While Stoner was concerned primarily with the static susceptibility of metals, current work reveals the importance of this concept in dynamic properties, such as those involved in the treatment of 'paramagnons' and the inelastic scattering of neutrons.

Domain size effects

Partly as a result of war-time association in the development of magnetrons, Stoner became interested in the behaviour of modern ternary alloys which possess the high remanence and the high coercivity needed for the manufacture of permanent magnets. His particular urge was to find a mechanism which would account for or describe their remarkable magnetic properties. Actually, he had the germ of an idea when he was studying domain size effects in magnetization processes as early as 1936 (*Phil. Trans.* A, **235**, 165, 1936), and he first drew attention to some important magnetic effects which must arise when only a small fraction of a ferromagnetic specimen is composed of particles containing merely some 10^3 or 10^4 atoms, when these small particles act as a trace of an impurity of very high magnetic moment. Such particles are now termed 'superparamagnetic'.

After the war, he realized that within such particles there would be no domain boundaries or walls which could be displaced by an applied field. Consequently, such particles would possess very high coercivities if by reason of their shape or magneto-crystalline properties, it was, in addition, very difficult to rotate their magnetization vectors through 180 degrees. With the collaboration of E. P. Wohlfarth he made a series of calculations which were embodied in their paper on a mechanism of magnetic hysteresis in heterogeneous alloys, published in 1948. The theoretical mechanism described in that paper is the only one known to us at the present day which definitely describes or requires magnetic hysteresis.

For present purposes, while not strictly following the Stoner & Wohlfarth treatment, we may take the simple case of a single particle in the shape of a prolate ellipsoid of revolution. We assume that its crystalline and strain anisotropies are very small, and we consider what happens as we turn the ellipsoid about a short axis in a magnetic field, H; we further assume that its saturation magnetic moment M_s is not field-dependent and changes only in direction as we turn the ellipsoid. The demagnetization factors D_a and D_b for directions respectively, along and perpendicular to the long axis, are now of great importance. It is convenient to work with a normalized or reduced field $h = H/[M_s(D_a-D_b)]$; and it is found that the equation $\frac{1}{2}\sin 2(\phi-\theta)+h\sin\phi = 0$ must be satisfied, where ϕ is the angle between the vectors M_s and H, and θ is the angle between H and the long axis.

This important equation for ϕ, which gives us the magnitude of the normalized magnetization component M_H/M_s ($=\cos\phi$) along the H direction, was laboriously solved by Stoner & Wohlfarth before the advent of computers.

There are two special cases—case (*a*) when $\theta = 0$, on plotting M_H/M_s against the reduced field h, we get a square hysteresis loop with irreversible magnetization only, and case (*b*) when $\theta = \pi/2$, we get in place of a loop a line with reversible rotation only. These two special cases are currently of tremendous importance in the discussion of the magnetic behaviour of thin ferromagnetic films. For another case, (*c*) when $\theta<\pi/4$, the critical field at which the magnetization suddenly rotates, or 'switches', is the coercive field. For a further case (*d*) when $3\pi/4>\theta>\pi/4$, the critical field is greater than the coercive field, giving a loop of an intermediate and often peculiar shape. There is a very wide range of loop shapes which are well worth study either in the original paper, in Stoner's article on Ferromagnetism in Vol. 11 of *Reports on Progress in Physics*, 1948, or in some of the advanced books on magnetism published during the last five years or so; the theoretical magnetization and hysteresis loop for an assembly of a large number of non-interacting, randomly-orientated, prolate ellipsoidal particles is extremely interesting.

This idea for a possible explanation of high coercivities has been a very fruitful one. It has been much used by Wohlfarth and others (*vide* 'Hard magnetic materials', *Phil. Mag.* **8**, 87, 1959) and there is good experimental evidence in its support (*vide* L. F. Bates & A. W. Simpson, *Proc. Phys. Soc.* B, **68**, 849, 1955, mentioned below). There is Bitter figure evidence that in high coercivity materials there are elongated single domain (E.S.D.) particles which may form, by magnetic interaction, coherent regions or assemblies which are termed magnetic interaction domains.

The idea must have been much in Stoner's mind when he made a very fundamental contribution to the thermomagnetic behaviour of ferromagnetic substances in weak and moderate fields. In 1938, I devised a new technique for measuring the very small adiabatic changes in temperature which occur in a soft ferromagnetic material when small changes are made in the strength of a steady magnetic field to which the material is exposed. With the help of J. C. Weston, many measurements of the small temperature, or rather, the corresponding small heat changes, ΔQ, were made with such materials as a magnetic field was slowly changed, step-by-step, over partial or complete hysteresis loops. We obtained a large number of what we considered to be interesting $\Sigma\Delta Q$, H and $\Sigma\Delta Q$, I curves, I being the intensity of magnetization. Several of my friends were somewhat critical of the value of this work; but not Stoner. With the collaboration of P. Rhodes, he showed that many of the interesting features of the Bates & Weston results could be resolved by a relatively simple thermodynamic analysis. They showed that it was necessary first to separate the effects of heating and cooling due to two basic processes—one (*a*) an effect due to change in intrinsic magnetization, and another (*b*) due to the rotation, in an anisotropic crystalline field, of the domain magnetization vectors towards the direction of the applied magnetic field whose change was responsible for the observed thermal changes.

They deduced the important equation

$$\Delta Q' = a\int d(IH) + b\int H dI,$$

where $\Delta Q'$ is the heat liberated per cm^3, $a = -(T/I_0)(dI_0/dT)$, $b = (T/K)(dK/dT)$ and K is an anisotropy constant which measures the potential energy of the system as a function of the orientation of the intrinsic magnetization I_0 with respect to the crystalline axis. The first term on the right-hand side of the above equation represents the contribution arising from effect (a), while the second term represents that from (b). Hence, for adequate testing of the Stoner & Rhodes theory we need data for the same specimen of the material for I_0, (dI_0/dT), K and (dK/dT).

Stoner & Rhodes further showed that effects due to internal stresses and reversible boundary movements could also be taken into account by modifying the coefficient b, and those due to the formation of new domain boundaries by modifying the coefficient a. On subtracting from the observed heat changes those which could reasonably be accredited to changes in spontaneous magnetization, it is found that

$$Q'' = \Sigma dQ' - a\int d(IH) = \int b'' H dI_0$$

In this equation b'' formally corresponds to b, but it was placed within the integration sign in case it should ever be found to be a variable coefficient which had to be calculated from experimental results. Hence, $b'' = (1/H) \times (dQ''/dI)$, or, if we wish to make perfectly clear what is done with the experimental results, we write $b'' = \Delta Q''/\Delta\int H dI$. The quantity b'' was therefore of considerable interest, since one could now hope to find how it varied over a hysteresis loop, always remembering that it was expected to be equal to the calculated value, b. One would not expect exact agreement between b'' and b, because the value of the latter could not be known accurately, and at that time there were no experimental methods of separating reversible from irreversible heat changes, or of making allowance for the effects of internal stresses due to magnetostriction, etc. However, b'' could reasonably be expected to show discontinuities in regions where irreversible processes occurred—e.g. in the hysteresis region between coercive points. Many values of b and b'' were calculated by Stoner & Rhodes.

Washington, 1952

At the Washington Conference on Magnetism held at the University of Maryland from 2 to 6 September 1952, Stoner gave an invited paper on the analysis of magnetization curves, and I presented one by G. Marshall and myself on heat effects in the magnetization of silicon-iron, to which the

Stoner & Rhodes theory was applied. Their equation formed a basis for much subsequent work at Leeds and Nottingham. Stoner gave additional details of analysis in his two long reports on the progress of research in ferromagnetism (*vide Reports on Progress in Physics*, 1948 and 1950), and also a general account of the analysis of magnetization curves, by which is meant a discussion of the nature and the extent of the several elementary changes which might occur in any part of a magnetization loop. The Stoner & Rhodes theory is particularly helpful in those cases where the separation of reversible and irreversible changes is possible by experimental means, as in the case of cobalt (*vide* Bates & Sherry, *Proc. Phys. Soc.* B, **58**, 642, 1955), and the theory adequately covers the behaviour of many high-coercivity alloys, as shown by Bates & Simpson (see p. 229).

Leeds workers subsequently made many experimental and theoretical advances in this branch of magnetism. They devised a new, indirect method of attack on the problems by measuring the small changes in magnetization which accompanied controlled, small changes in the temperature of a ferromagnetic specimen in the shape of a very long prolate ellipsoid of known demagnetization factor (*vide* Tebble, Wood & Florentin, *Proc. Phys. Soc.* B, **65**, 858, 1952). They expressed the Stoner & Rhodes equation in a more general form, based on free-energy considerations. They thus covered both reversible and irreversible changes in high fields, and this permitted the coefficient b to be replaced by a coefficient, c, which depends on the kind, or kinds, of magnetization that occur during a field change (*vide* Teale & Rowlands, *Proc. Phys. Soc.* B, **70**, 1123, 1957). Professor R. S. Tebble kindly writes: 'Although, (or perhaps because!) Stoner was a theoretician he was often deeply impressed by the work of the experimentalists. He was full of admiration for your own magnetocaloric work and I remember how delighted he was at the excellent agreement between our reversible magnetocaloric measurements and your own.' Tebble adds that Stoner was fulsome in his praise of D. J. Craik's (Nottingham) Ph.D. thesis on magnetic domains; also, despite the success of the Leeds work on domains, Tebble wondered, on reflection, whether Stoner should not have pursued his collective electron work to the exclusion of all else. I sympathize with Tebble's remark, but, somehow, I think that Stoner always knew where he wanted to go.

In the 47th Guthrie Lecture (*Proc. Phys. Soc.* **84**, 625, 1964)—delivered at the International Conference on Magnetism held in Nottingham in September 1964—on 'Magnetic processes in weak and moderate fields', I discussed the effects of closure domain configurations on the shape of the hysteresis loop, and the effects of domain nucleation and growth in determining the magnetic properties of a material, in the light of the results of Bitter figure and magnetothermal measurements. I described how the detailed examination of b'' by Teale & Rowlands of Leeds allowed us the possibility of writing the free energy of any process, say the nth, as $F_n = A_n(I/I_0)$ which would result in a cofficient $b_n = (T/A_n)(\mathrm{d}A_n/\mathrm{d}T)$—with of course $b_1 = (T/K)(\mathrm{d}K/\mathrm{d}T)$; and I reproduced a table of mathematical expressions for b_n for some seven different processes. I showed that much information might be obtained by plotting the product $(b''-b_1)\,H$ against H; for, when only two processes are superimposed, the graphs have a very simple form. This work permitted the study of magnetothermal effects to be related to the demands of a number of modern theories of coercivity (*vide* Bates & Pacey, *Brit. J. Appl. Phys.* **15**, 1391, 1964; and A. J. Pacey, Ph.D. Thesis (Nottingham, 1963)).

I think that in some respects Stoner was very sensitive and very easily wounded. He could not easily forget a personal slight, and I think that what he regarded as a personal slight would not always be so regarded by his peers. On the other hand, he was capable of administering a sly 'dig' or sharp riposte. For example, when he proposed a vote of thanks to Professor Wohlfarth at the end of his Inaugural Lecture as Professor of Magnetism at Imperial College, Stoner said 'Wohlfarth was at one time my research assistant, which means that I sometimes gave him assistance.' Wohlfarth has recorded that Stoner was always guided by a sense of what is simple and what is good physics. To his many friends he was kind and loyal; but, he could be severely critical of anything which he regarded as second-rate.

Stoner's conscientious service to the University of Leeds has already been described. I would like to mention other services which I think were valuable. He was a member of the Association of University Teachers and was for a period Chairman of the Leeds Local Association, and I remember attending some Council meetings at which he was present. Another important service was as referee for papers submitted to several scientific societies. He must certainly have refereed a large percentage of the papers published from Nottingham. Of course, a referee is supposed to remain anonymous, but, Stoner's style and kindly touch were unmistakable. One sometimes read the words—'and, with respect, the referee suggests that the results might be discussed advantageously with reference to . . .'—and then there would follow a reference to a publication which had just appeared or to one little known and overlooked. On one occasion, when what I thought was a great 'brainwave' ended with a negative result to be described in a two-page paper in the *Proc. Phys. Soc.*, I felt sure that Stoner was the referee, for he wrote quite a long report giving reasons why papers which reported negative results should occasionally be published, and the special reasons why the present communication, '*so commendable in its brevity*', should be published. I like the phrase in italics; for once when I drew the attention of one of our very distinguished French colleagues to one of Stoner's papers, adding that he would probably find it rather long, he replied: 'Mais, si c'est Stoner, c'est longue!'

I hesitate to add any comment about Stoner's services as an examiner of candidates for higher degrees, but he seemed to be so different from other examiners. His judgement on a thesis was always meticulously written before the oral examination, but in the latter no account was taken of time. One could never predict how long an oral would last or what caused the duration of one oral examination to be so much different from another. I remember one oral which lasted about four hours; while Professor D. H. Martin (Queen Mary College) reminded me that most of his Ph.D. oral examination took place while Stoner was waiting for a train on the platform of the Midland Railway Station at Nottingham. But, no candidate to my knowledge ever 'got away' with imperfect experimental work or suspect theory, and I regret that I shall never again see the lovely smile which suffused Stoner's face after he had finally watched the departure of a candidate, and he murmured, 'Well! That was a nice bit of work, wasn't it?'

In Section II of this Memoir (p. 204) there is a remark concerning the relation between experimental and theoretical work in physics. I now quote from the penultimate section in Stoner's 39th Guthrie Lecture:

'It seems to me that in the field of magnetic properties of matter, and possibly in many other branches of physics, an unfortunate dichotomy is developing. There are many papers giving "theories" of magnetic properties and behaviour, the results of which seem to be widely at variance with the experimental findings which they purport to explain; and there are many papers, giving the results of lengthy and painstaking experiments, in which there is no clear indication of the purpose of the measurements: the "discussion", if any, is perfunctory, and the presentation of the results themselves is in such a form that a further extensive investigation is necessary if any significant information is to be extracted from them. I am not thinking here of work which is primarily mathematical, or primarily technical, but of work which is physical, that is work which aims at adding significantly to knowledge and understanding of the physical world. The direct personal work of the individual physicist may be, and indeed almost must be, primarily theoretical or experimental; but experiment without appreciation of theory, no less than theory without appreciation of experiment, avails little. It is the two together which constitute science; to quote Whitehead slightly out of context, science in its modern form, springs from a "passionate interest in the relation of general principles to irreducible and stubborn facts". In the study of the magnetic properties of matter, and in other branches of physics and of science, the principles and the facts must be sought and constantly kept in view together.'

Stoner was obviously very greatly attached to his mother. She lived with him after his father's death in 1938 until her own death, at the age of 87, in 1955. I find it hard to believe that this devotion and attention did not have an adverse effect on his work, but in the Lawnswood Book of Remembrance he expressed his views of the character of her life in the words: 'Dear wife and mother. Clear-eyed, loving, giving.'

In 1951, he married (Jean) Heather Crawford. It was a very successful marriage which gave much pleasure to those who knew him. He had known Heather since she became the Departmental Secretary some two years earlier, after being for some years previously the private secretary to a distinguished Leeds surgeon. With her youth, charm and efficiency, as well as a large and happy family circle, she brought great and unhoped for happiness into his life for the seventeen years of their marriage.

There are two photographs of Stoner in the possession of the Society. One was taken some years ago by Stoneman of Baker Street, W.1, and the other by Lonnigan, Leeds, on 6 May 1952. Both are good photographs; but in my opinion they do not make clear the kindly nature of the man which I remember and wish to be remembered. I therefore asked Mrs Heather Stoner if any other photographs of him were available; the one reproduced with this memoir was taken on 24 January 1964, in his room in the (old) Department of Physics, Leeds. Mrs Stoner and I both think it an excellent likeness.

Stoner was admitted to the General Infirmary at Leeds on 20 December 1968. On 27 December, I received a Christmas card on which his wife stated that he had been admitted for a heart condition and diabetic treatment, and although he was very annoyed about it and hated doing nothing in bed, she could already see some improvement and hoped that he would be home at the end of the month. The end came suddenly and unexpectedly in the early evening of the 27th. I suppose that when each one of us comes to go down to the darkness which men call death he hopes to leave a little light behind him; E. C. Stoner has left a great light.

It is obvious that in writing this memoir I have drawn extensively on his own carefully, meticulously prepared records; and I gratefully acknowledge the help which I have received from Professor S. Chandrasekhar, F.R.S., Dr R. Hill, F.R.S., Professor W. H. McCrea, F.R.S., Professor D. H. Martin, Dr Colin Matthews, Dr P. Rhodes, Mr F. I. G. Rawlins, Professor Gilbert Stead, Professor W. Sucksmith, F.R.S., Professor R. S. Tebble, Professor R. Whiddington, F.R.S. and Mrs Whiddington, Professor E. P. Wohlfarth and Mrs Heather Stoner.

Finally, in his Guthrie Lecture, Stoner stated: 'Most scientific writing, in original papers, review articles and books, is severely impersonal in form, following a generally accepted convention; a convention reflecting what Born has referred to as "the disinterested, objective description and explanation which is characteristic of the modern epoch" in science. Scientific research, however, is intensely personal, and the impersonality convention, valuable though it may be in limiting irrevelancies, can be misleading in its implications as to the character of scientific work. On this special occasion, therefore, it may not be out of place for me to deal with some of my own work and with some small part of the wider field in which it lies in a rather more personal way than usual.' If, therefore, this memoir should appear to my colleagues in the Society and elsewhere as not sufficiently impersonal, I plead that I am only following here the example set by the very great and remarkable man of whom I have written, and that, try as I would, I could not make the memoir impersonal.

L. F. Bates

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