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PAUL ADRIEN MAURICE DIRAC

8 August 1902—20 October 1984



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Elected F.R.S. 1930

By R. H. DALITZ, F.R.S., AND SIR RUDOLF PEIERLS, F.R.S.

PAUL ADRIEN MAURICE DIRAC was one of the greatest theoretical physicists of the century, whose work made a profound impact on modern physics. Although he took note of the work of others, his inspiration was always his own. Although he influenced many others, he had few pupils and he did not engage in team work.

FAMILY ORIGINS AND BACKGROUND

Paul Dirac was born on 8 August 1902 in Bristol. His parents were Charles Adrien Ladislas Dirac and Florence Hannah (*née* Holten), who had married in the Portland Street Methodist Chapel at Kingsdown, a suburb of Bristol, on 22 July 1899. His father's family had a French background. Paul's grandfather's great-grandfather, Pierre-Louis, was born in 1748 at Thonon on Lake Geneva; during the period of the Napoleonic Wars, he and his family moved to the Valais in Switzerland where they settled at Saint-Maurice, where Paul's grandfather, Louis, was born in 1836. Louis was a minor poet of the Valais, whose poems are included in anthologies of Valais verse, and worked initially as a primary school teacher. Later on he entered the railway service and became 'chef de gare' at Monthey, Valais, where Charles was born in 1866. After completing local college Charles left Monthey to live at Geneva where he took the matriculation degree Baccalauréat-ès-Lettres awarded by the University of Geneva on the basis of an examination covering his college studies. He attended lectures in the Faculté des Lettres as an 'Auditeur' during the academic year 1887-88 and left soon afterwards for England, where he supported himself by tutoring in French. When Louis Dirac died in 1895 at Monthey his family were living in Geneva where his other two sons, Frédéric and Roger Adolphe Claude, later established small businesses, and where his wife lived until her death in 1926.

The earliest forebear known of this Dirac family is Didier Dirac, a

sergeant in the Regiment de Poitou, whose son's birth was registered at Noyers-sur-Jabron (Basses-Alpes) in 1721. The name Dirac is of Gallo-Romanic origin (Longnon 1920), the -ac ending being an abbreviation of the Gallo-Romanic ending-acos (Latinized form, -acus). Names with this ending occur all over France, but most thickly in the Département de Charente and the departments adjacent to it. There is indeed a village named Dirac situated in the Forêt-de-Dirac about 10 km south of Angoulême, in the Département de Charente. The history of this village name shows (Dauzat & Rostaing 1963) that it stems from 'Atiracos', so that this place was once associated with some person named Atirius, and that it has been abbreviated to Dirac over the centuries, the phrase 'de Diraco' being known from a record dated A.D. 1110, for example. There is no evidence to suggest that Paul Dirac's family had any connection with this village, of course, because their name could just as well have arisen elsewhere in some other context involving another person named Atirius. However, it is interesting to note that the Département de Charente is immediately south of the former province of Poitou, which gives some support to the belief that the family and the family name did both stem from this part of France.

Paul's mother was born in 1878 at Liskeard, Cornwall, her parents being Richard Holten, a sailor of East Looe, and Mary Grace Uren of Liskeard. The Holten family moved to Bristol about 1880 when her father took up a post as Master Mariner on a Bristol ship. Florence met Charles, then a French teacher, when she was working in a library. She was said to be a beauty and a very simple, kindly woman.

By 1902 they had settled into a house at 15 Monk Road in Bishopston, Bristol, which they named 'Monthey' after Charles's birthplace. They had three children, the eldest being Reginald Charles Felix, who was two years older than Paul, having been born on 15 April 1900, and the youngest being Beatrice Isabelle Marguerite Walla, who was born on 4 September 1906. The three children were registered at birth as Swiss citizens of the commune of Saint-Maurice in the canton of Valais, but in 1919 the father gave up his Swiss citizenship and that of his children, thus releasing them all from their rights and obligations under Swiss law. Charles acquired British nationality later in the same year.

Charles had been appointed in 1896 to teach French in the Merchant Venturers Technical College at Bristol. At that time this provided teaching at the primary level (the primary school closed in 1908, after a fire), the secondary level and the Technical College level. In 1909 the University College of Bristol received its University Charter, becoming the University of Bristol. The University College had had only a small Engineering Department, much overshadowed by the large and effective Engineering Departments in the Technical College not far from the University. It was decided that the University would do best to combine its Engineering Department with those of the Technical College, so that

they became together the Engineering Department of the University, the Head of the Technical College being automatically the Dean of the Engineering School of the University, the senior teachers of the College becoming engineering professors of the University. Because Charles taught French in the Technical College he became a Recognized Teacher in the University of Bristol for the next ten years. In 1919 the secondary school became independent of the Technical College, moving to a site at Cotham (Bristol) where it became known as the Cotham Secondary School. Since 1945 it has been known as Cotham Grammar School. Charles moved with the secondary school to its new site close to his family home, and remained its senior French teacher until his retirement in 1931. He continued to teach evening classes in the Technical College until his death in 1936.

STUDENT YEARS AT BRISTOL.

Paul Dirac's mathematical ability became apparent at the local Bishop Road primary school. He entered the secondary school of the Merchant Venturers' Technical College, where his father taught, at the age of twelve, the normal age for entry there. At this school academic standards were high, but the teaching had a practical orientation. Modern languages were taught for use, metal work and shorthand were in the syllabus, and there was some history and geography, but no classics or literature. The secondary school was particularly strong in mathematics and science because the laboratory facilities of the Technical College were available, and it shared some teachers with the College. During the 1914-18 war the younger boys could make more rapid progress because many of the older boys were called up, leaving room in the science laboratories. Paul was soon far ahead of his class in mathematics and was able to work largely on his own. In Dirac's own words in *The Old Cothamian* (198):

'The M.V. was an excellent school for science and modern languages. There was no Latin or Greek, something of which I was rather glad, because I did not appreciate the value of old cultures. I consider myself very lucky in having been able to attend the School.

'I was at the M.V. during the period 1914-18, just the period of the First World War. Many of the boys then left the School for National Service. As a result, the upper classes were rather empty; and to fill the gaps the younger boys were pressed ahead, as far as they were able to follow the more advanced work. This was very beneficial to me: I was rushed through the lower forms, and was introduced at an especially early age to the basis of mathematics, physics and chemistry in the higher forms. In mathematics I was studying from books which mostly were ahead of the rest of the class. This rapid advancement was a great help to me in my later career.

'The rapid pushing-ahead was a disadvantage from the point of view of Games—which we had on Wednesday afternoons. I played soccer and cricket, mostly with boys older and bigger than myself, and never had much

success. But all through my schooldays, my interest in science was encouraged and stimulated.

'It was a great advantage, that the School was situated in the same building as the Merchant Venturers' Technical College. The College "took over" in the evenings, after the School had finished. The College had excellent laboratories, which were available to the School during the daytime. Furthermore, some of the staff combined teaching in the School in the daytime with teaching in the College in the evenings.'

Dirac's schoolmates remember him as silent and aloof. One of his contemporaries described him (Phillips 1947) as follows: 'He was a slim, tall, un-English looking boy in knickerbockers, with curly hair. He haunted the library and did not take part in games. On the one isolated occasion I saw him handle a cricket bat, he was curiously inept.' However, he did serve as a prefect, although thought to be a somewhat peculiar one, in his last year at school. Also Dr J. L. Griffin (1979) recalls that: 'Even in those days (1917/18), he was recognized by the whole class as a boy of exceptional intelligence. I remember an occasion when he politely and gently corrected a statement by the Chemistry master, Dr Davidson; and it was accepted with grace. This enhanced his standing with all the other boys.'

In 1918 he became a student of Electrical Engineering in the University of Bristol. His favourite subject was mathematics, but he did not realize one could earn one's living by mathematics, except as a school teacher, a career that did not appeal to him. In choosing engineering he was following in the footsteps of his brother, who had been persuaded by his father to follow this course although he would have much preferred to go into medicine.

Because the Engineering Department of the University was part of the Technical College, he continued his studies in the same building in which he had done his school work. He commented later that the engineering training was valuable in showing him the merit of an approximate approach to problems that are too complex to be handled rigorously. He had no contact with the Physics Department at Bristol, nor with its professor (A. M. Tyndall), because that was in a different part of the University, up on the hill at the foot of which the Technical College was sited.

He did excellent work in the Engineering Department and graduated with first class honours in 1921. In a summer vacation he worked as a student apprentice in the engineering works of Thomson-Houston in Rugby, but did not find this work challenging. A report from the firm about his work was unfavourable. After graduating he did not succeed in finding a job.

There was then much interest in the theory of relativity, following the verification of its predictions by observations during a solar eclipse a few years before. Paul had some trouble finding out the details of this theory.

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He attended a series of lectures by C. D. Broad, the professor of philosophy at Bristol University, but these did not satisfy him until Broad wrote down the formula for the Lorentz metric, which Paul found unexpected, and which gave him the clue to understanding relativity.

By this time mathematics had become his real interest. This included relativity (then regarded as applied mathematics). He tried to go on to Cambridge University where the mathematics teaching was far superior to that at any other university, but this thought came too late for him to take the Cambridge Entrance Examination in December 1920, the examination on which the awards of major scholarships for undergraduate studies were based. He did take the examination in June 1921 and was then awarded an exhibition (a minor scholarship) by St John's College, the best available from that examination, but this was not sufficient to cover the cost of study in Cambridge. Although his father had been resident at Bristol for more than 25 years, his local authority refused to provide Paul with the customary financial support for an undergraduate at Cambridge University from Bristol, on the ground that his father had become naturalized as British only in 1919.

The staff of the Mathematics Department at Bristol had been disappointed when Paul chose engineering rather than mathematics. They now proposed that he take the mathematics lectures as an unofficial student without paying fees. He accepted and became the best student in his year. Among his teachers there he remembered with particular enthusiasm Peter Fraser, an inspiring teacher, who introduced him to the beauty of mathematical rigour, and to projective geometry, which attracted him greatly. In the final year there was an option to specialize in pure or in applied mathematics. The only official mathematics student in his year, Beryl Dent, chose the applied option. To save the need for two separate courses Paul had to do the same. In 1923, after two years, he passed the final examination with first class honours.

DIRAC'S FATHER, CHARLES

It is worthwhile to step aside and look briefly at Dirac's father Charles because he was a major influence in his sons' early development, as Paul himself recognized. Charles's father, Louis, has been described as a highly emotional man who led a rather disturbed and difficult life. Whatever the cause, his eldest son Charles came to feel alienated from his family; he ran off to Geneva and thence to England, not informing them where he was going nor what became of him. He did not even inform them of his marriage until some years after the event, probably at the time when Charles and his family visited his mother in Geneva in 1905, a visit that Paul remembered all of his life, a memory no doubt kept alive in him by his mother.

Charles did not reject his background as a French-speaking Swiss

citizen. He wished his children to speak French, the language of their Dirac forefathers, and they were required to speak French to him at home as far as possible. At the dinner table he required them to speak only French, and grammatically correct French at that, or they would be punished. In spite of his own revolt against parental authority, Charles became a strict disciplinarian himself. He is remembered in the school for his strictness, as we shall see below, and it was the same at home. Paul often said that his reticence in talking was most probably due to this experience. As Paul has recollected (Kuhn 1962, Salaman & Salaman 1986) it became the regular arrangement that he ate in the dining room with his father, while the other two children ate in the kitchen with their mother, presumably because Paul's brother and sister were unable to meet their father's requirement. Their mother could not speak French, so that their father's requirement made it difficult for her to be at the dinner table. Indeed, it has been reported (Salaman & Salaman 1986) that Paul said that he never saw his parents have a meal together; this must surely be an exaggeration, even if it were the normal situation.

Charles Dirac was a man with a dominating personality who saw only one way to achieve his desires for his children. Paul did become able to speak French correctly and fluently—he lectured in this language on more than one occasion in Paris—but the father destroyed the relationship between himself and his son, and Paul did not come to associate any pleasure with his use of the French language. This situation gives special point to an old story about Dirac, who shared his cabin with a Frenchman on one journey across the Atlantic in an English ship. This Frenchman had great difficulty with the English language and had to struggle incessantly in communicating with his cabin mate. On the last day of their journey the Frenchman suddenly realized that Dirac could understand French and asked 'Why didn't you tell me that you could speak French?', to which Dirac replied 'You didn't ask me'. In consequence of his upbringing, Dirac did not seek to speak French, doing so only when it was absolutely necessary. For example, Joan Thomson once recalled the visit of Maurice de Broglie to the Lodge at Caius College, Cambridge, when her father was Master there, during which de Broglie and her father spoke French, but Paul only English. When de Broglie asked him 'Don't you ever speak French?', Paul replied laconically 'Sometimes'.

As a teacher at the Merchant Venturers' Technical College and later at the Cotham School, Charles Dirac was highly regarded, described by his colleagues as an excellent teacher and a strict disciplinarian. Dirac gave the boys' view of him (198):

'My father, C. A. L. Dirac, was French master in the same school. He was somewhat strict, and would frequently give the boys a test which was not announced beforehand, so that they were unable to prepare for it. He expected them to be always ready for any sort of test. He was thus not very popular with the boys, but he was very successful in getting them through their exams, for which they were glad. He was nicknamed "Dedder".'

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The boys in the lower forms were told by their seniors: 'Wait until you get old Dedder. You will learn more French from him in a term than you ever learned before in your life.' He was feared for these unexpected class tests, introduced by his announcement 'Tak'a pice of papper', with severe penalties to those who made mistakes. When Hebblethwaite gained the top marks for homework translation for the first time ever, Dedder's response was 'Hebwhite, you never wrote this. Saturday four for cribbing', meaning four half-hour detentions on Saturday afternoon. He was not disliked by his students, who recall him affectionately in later life as a man who was fundamentally fair and kindly. Besides being the senior French teacher he was housemaster to 'Dirac's House'. Although little interested in games he always showed pleasure to hear of his House's success. He was considered a successful housemaster.

To sum up, we may quote Professor W. R. Niblett (1985) who was a student at Cotham Secondary School in the early 1920s: 'Characterful, precise, with a faith in sound grammatical teaching, he secured excellent results for his boys in the school certificate examination.' On a more personal level, Dr L. R. Phillips (1947) described him as follows:

'Charles Adrien Ladislas Dirac, called 'Dedder' behind his back by the boys, was a slow moving, thick-set Frenchman with hunched shoulders, a very short neck and a great dome of a head. I have never understood why he should have been a Frenchmaster in a not-particularly-well-known school. He was *the* disciplinarian in the school, precise, unwinking, with a meticulous, unyielding system of correction and punishments. His registers, in which he recorded all that went on in a class were neat and cabalistic: no scholar could possibly understand their significance. Later, as a senior, I began to realise the humanity and kindness of the man.... But to us in the junior school he was a scourge and a terror.'

D. C. Willis (1985), a student in Charles Dirac's time and a staff member after Charles had retired from the school, described him with the words:

'He had, next to Archbishop Temple, the largest cranium in Christendom', and reported that his father, E. D. Willis (a staff member soon after Paul Dirac graduated from the school), although not a close friend of Charles Dirac, had a very high opinion of him as a person of high integrity. He also noted that 'He was a brilliant linguist, being able to speak eight or nine languages—it was said that he learned a new language every summer holiday.' Charles Dirac was indeed unusually interested in languages and able with them; he was also a leading light of the Esperanto Society of Bristol, becoming its President in due course, and active in the British and Universal Esperanto Association. It is clear that he was highly regarded in general, while being somewhat of an eccentric, a foreigner, someone always outside the common mould, and almost a landmark in Bristol. His funeral service at St Bonaventure's, the Roman Catholic Church not more than 200 metres from his home, was quite a large affair, not really surprising for a man who had taught for almost

40 years in several major educational institutions in the city of Bristol, and who had shared common interests and activities with all those interested in languages in the wider community outside.

It is apparent that Charles Dirac cared about his children and their futures. He was often seen walking to school with his daughter, of whom he was clearly very fond. However, he alienated his sons. His first son, Reginald, wanted to be a doctor, but he caused him to study mechanical engineering at Bristol University. Reginald gained only a third class degree in 1919 and took a job as a draughtsman with an engineering works at Wolverhampton, but he committed suicide in a field near Much Wenlock in Shropshire when he was 24 years old, for no known reason according to the newspaper reports at the time. Paul has described the severe reaction that Reginald's action had on Charles, even fearing for a time that his father might lose his sanity, and resolving to himself that he would never take any similar action, no matter what the circumstances. Paul's relationship with his father became chill and they had little personal communication. When he was awarded the Nobel Prize in 1933 he was told that he would be permitted to invite his parents to accompany him to Stockholm for the award ceremony, but he chose to invite only his mother. Yet Charles was proud of his son's success and interested to try to understand what he did. Paul has recorded (Kuhn 1962) that his father did encourage him to take up the offer of the mathematics department in 1921, and that he was later grateful to his father for this support. Charles was always concerned about Paul's work and his progress with it. D. C. Willis (1985) recalls that during the year 1929-30, when he was receiving special tuition in French from Charles, he was often sent on errands to the Dirac home (then at 7 Julius Road, Bishopston) during the dinner hour to have news of Paul, who was at that time working continuously in his bedroom there, not coming out except to collect his food and use the lavatory. Also, Professor S. Chandrasekhar has told us of the recollection of Professor A. M. Tyndall, head of the physics department at Bristol University for three decades, that, when he gave a course of popular evening lectures on modern physics at the University in the early 1930s, he noticed a regular listener in the front row, a man much older than the others there, who was taking careful note of all that he said. At the end of the last lecture of the course this old man came up to Professor Tyndall to thank him, saying 'I am glad to have heard all of this. My son does physics but he never tells me anything about it'. He was Charles Dirac.

Paul did not seek to visit his relatives in Switzerland. Indeed, he avoided the possibility of setting foot in Switzerland, the country that he associated with his father. In 1952, when Dirac made his first visit to the Nobel Laureates' meeting at Lindau, his cousin, Hugues Dirac, visited Paul and his wife there, and persuaded them to visit his home at St Gallen after that meeting and to stay there some days. His first and

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only visit to Geneva after 1905 was at the beginning of July 1973 (Mehra & Rechenberg 1982) when he visited CERN.

CAMBRIDGE: SUCCESS IN RESEARCH

After his excellent performance in the mathematics examinations at Bristol University in 1923, Paul Dirac was awarded a research studentship by the Department of Scientific and Industrial Research (D.S.I.R.) so he was now able to go to Cambridge as a postgraduate student. (In 1925, through a competition, he was awarded the more prestigious and more valuable Senior Studentship of the 1851 Exhibition.) He was also given a grant of £5 by the Bristol Education Authority to tide him over until his D.S.I.R. grant was paid. Even in those days £5 was not much, and he had to live very frugally for a time. He was hoping to have Ebenezer Cunningham assigned as a supervisor, because he knew Cunningham was working on relativity. But Cunningham did not accept any more students, and Dirac was assigned to Ralph H. Fowler.

Fowler was then the leading theoretician in Cambridge, well versed in the quantum theory of atoms; his own research was mostly on statistical mechanics. He recognized in Dirac a student of unusual ability.

Under his influence Dirac worked on some problems in statistical mechanics. Within six months of arriving in Cambridge he wrote two papers on these problems (1, 2)*. No doubt Fowler also aroused his interest in the quantum theory, and by May 1924 Dirac completed his first paper dealing with quantum problems (3). Four more papers were completed by November 1925 (4-7).

Dirac did not make many friends among the students. He was very diffident, and did not make acquaintances easily. His contemporary, Robert Schlapp, recalls that Dirac often chose to sit next to him in Hall. When he told Dirac of the problems he was working on under Larmor, Dirac said 'You ought to tackle fundamental problems, not peripheral ones'.

He was now thinking about the Bohr-Sommerfeld quantum theory, which at the time was the best available theory of atomic phenomena, but he was very conscious of its shortcomings and contradictions. He attempted to find ways of improving the theory, but without success.

In the summer of 1925 Heisenberg came to give a talk in the Kapitza Club, the Cambridge forum for discussions on modern physics. He had then already written his pioneering paper that started modern quantum mechanics, but the main subject of his talk was something else rather less exciting. At the end Heisenberg mentioned his new ideas briefly, but Dirac says (162) that he did not take this in, and in fact did not remember afterwards that these ideas had been mentioned.

* Numbers in this form refer to the entries in the bibliography at the end of the text.

Even when Fowler received proofs of Heisenberg's paper and sent them to Dirac for his comments, their significance did not sink in on the first reading, and Dirac put the paper aside. But when he looked at it again a week later he saw that it was an important new departure, capable of resolving the difficulties of the old quantum theory. He was at first puzzled by the appearance of non-commuting quantities, i.e. that by the multiplication rules Heisenberg had been led to, the product of two quantities depended on their order, so that AB did not equal BA . This result had also worried Heisenberg. But then Dirac realized that this was the essence of the new approach.

He commented later (130, 133) that scientists who propose a new idea tend to have an emotional attitude to it, and fear it may yet prove wrong. 'Lorentz did not have the courage to express relativity, and Heisenberg had the fear of non-commutativity...the originator of an idea is not the best person to develop it'.

It was for him a big step to see that the commutators were the analogue in quantum theory of the Poisson brackets of classical mechanics. This thought occurred to him during a walk in the country. He had developed the habit of relaxing during weekends by going on long walks, and not thinking about his problems, but on this particular occasion he kept thinking about the problem of non-commuting variables, until the similarity with Poisson brackets occurred to him in a flash. He did not remember the theory of Poisson brackets in detail, and he waited impatiently until Monday morning when he could check the details in the library.

Dirac's first paper on quantum mechanics (8) parallels much of what was being done at the same time by Born, Heisenberg and Jordan in Göttingen, but expressed in his own characteristic style. This was followed by a series of papers developing, generalizing and applying the new theory. This work immediately attracted the attention of theoreticians everywhere, particularly in Copenhagen, Göttingen and Munich, then the main centres of research in quantum theory.

A thesis entitled *Quantum mechanics* (12) was just a by-product of this work, and he obtained his Ph.D. in 1926. Shortly after that Fowler arranged for him to spend some time in Copenhagen and then in Göttingen, still supported by the 1851 Exhibition Studentship.

He went to Copenhagen in September 1926. There he completed his paper on transformation theory, which shows the Schrödinger wave equation and Heisenberg's matrix equations to be special cases of a more general formulation. He comments in reference 162 that this work gave him more pleasure in carrying it out than any other paper he wrote on quantum mechanics before or after. In this paper he also introduces a notation that has become standard for most work in quantum mechanics.

He enjoyed the informal and friendly atmosphere in Copenhagen and had many long conversations with Niels Bohr. He respected Bohr greatly

for his depth, but says (162) that he does not know whether Bohr had any influence on his work, because Bohr tended to argue qualitatively, whereas Dirac liked to think in terms of equations.

In Copenhagen he started working on the problems of the emission and absorption of radiation, and this was continued in Göttingen. In his early papers he introduced the method of second quantization for boson fields. He also derived from quantum mechanics the expressions for the *A* and *B* coefficients introduced by Einstein in the laws of spontaneous and induced emission and absorption of radiation.

He moved on to Göttingen in February 1927. There he interacted particularly with his fellow student, Robert Oppenheimer, and he had many discussions with Max Born, James Franck and Igor Tamm. The latter was a visitor from Russia, with whom a lasting friendship developed.

By now he was internationally recognized, and he was invited by Ehrenfest to stop for a few weeks in Leiden in Holland on his return journey from Göttingen. This was the first of many scientific visits; he became an inveterate traveller.

In 1927 he was elected a Fellow of St John's College, Cambridge. This type of fellowship was competitive, and candidates had to submit a thesis for the purpose. Not surprisingly the college had no doubt about his merit. When in 1929 he was appointed a University Lecturer, the college was anxious to retain him without burdening him with teaching or administration; they therefore made him Praelector in Mathematical Physics, a post with nominal duties, which entitled him to an additional stipend.

It was also not surprising that he was invited to the Solvay Conference in October 1927. These conferences, held in Brussels every few years, gathered the élite of physicists (see Mehra 1975). Here he made important contributions to the discussion (22) and had the opportunity of meeting Einstein and Lorentz.

Recognition did not change his habits greatly; he continued working intensely, mostly in his college room (New Court A4, later Second Court C4), and largely following his own thoughts. He kept looking for a relativistic theory of the electron, and in the winter of 1927–28 he found the right equation, now known as the Dirac equation, probably his greatest contribution to modern physics. This equation not only gave a relativistic description of the electron, but showed it to have a spin of half a unit, as was known empirically, and associated with this spin a magnetic moment of correct magnitude.

A comment by Mott (1986) is typical of the impact of this paper on physicists: 'This seemed, and still seems, to me the most beautiful and exciting piece of pure theoretical physics that I have seen in my life time—comparable with Maxwell's deduction that the displacement current, and therefore electromagnetism, must exist.'

The energy levels predicted by Dirac's equation were the same as those given by Sommerfeld's formula, which agreed well with observation.

The equation had, however, a serious flaw in that it allowed unphysical solutions in which the electron moved with negative energy. Dirac gave much thought to attempts at avoiding this trouble, and in 1930 hit on the idea that all negative-energy states might in nature be filled, thus preventing, by Pauli's exclusion principle, any further electron going into any of these states. A vacant place, or 'hole', would then appear as a particle of positive charge, and of the same mass as the electron. Such a particle had never been seen, and Dirac decided that if it existed it could not have escaped detection. The only known positively charged particle was the proton, and for a time Dirac believed that the 'holes' were protons. In that case their very much larger mass would have to be attributed to the Coulomb interaction between charged particles, which is difficult to evaluate. However, he had to abandon this hypothesis, and by 1931 he came to consider seriously the possibility that there was a new, as yet undiscovered particle, which he called 'anti-electron' (33). This idea was indeed confirmed when the positron was discovered in 1932. In the autobiographical interview with T. Kuhn (Kuhn 1963) he says that he had forgotten he made this remark, and it is not generally realized that he was the first to speak of such a particle.

Further honours and appointments followed. He was elected to the Royal Society in 1930, on the first occasion after being proposed, which is quite unusual. In 1932 he was elected Lucasian Professor of Mathematics in Cambridge (in the Cambridge tradition of treating theoretical physics as a branch of mathematics) only one year after the election of his teacher, R. H. Fowler, to the Plummer Chair of Mathematical Physics. In 1933 he shared the Nobel Prize for physics with Schrödinger. At first he was inclined to refuse the prize because he did not like publicity, but when Rutherford told him: 'A refusal will get you much more publicity', he accepted.

Meanwhile, besides a substantial output of research, he completed his book *The principles of quantum mechanics*, of which the first edition was published in 1930. This, and the three later editions, which were substantially revised, have helped generations of physicists to learn the spirit of the new physics. It reflects Dirac's very characteristic approach: abstract but simple, always selecting the important points and arguing with unbeatable logic.

He was, of course, very much in demand as a lecturer, and he liked to travel. His trips included visits to the Soviet Union, where he attended several conferences. On one of these visits, probably the first in 1928, he arrived by a different route from that specified on his visa, not realizing that a Soviet visa is valid only for one particular point of entry. He had to wait in a tiny border village until the problem was sorted out. He stayed overnight in a peasant's cottage, where the room was so infested with

bedbugs that he spent the night sitting on a chair placed on the table. In spite of this experience he enjoyed the visits to the Soviet Union and came again each year, except in 1931, until his last prewar visit in 1937.

During these visits he made friends with Soviet colleagues. I. E. Tamm, his friend from the Göttingen days, was a passionate mountaineer, and proposed several times joint climbs in suitable mountains. After various practical difficulties, at least one of these trips materialized in 1936. Dirac had joined an expedition to observe the solar eclipse that was total in the Caucasus on 19 June. However, the death of his father on 15 June made him return to England, so he missed the eclipse. He came back to the Caucasus after to walk and climb. It was most probably on this visit that he joined a party to climb Mount Elbruz, the highest mountain in the Caucasus. This proved too much of a strain for him; he collapsed at a high altitude and had to rest there for 24 hours before returning.

With other Soviet physicists, including V. A. Fock, his contacts were more on the scientific side, and at least one important paper (37) resulted from that collaboration.

In Cambridge Dirac had become very friendly with Peter Kapitza, a Russian experimentalist who had worked in Cambridge since 1921, and for whom the Royal Society Mond Laboratory had been built. When he went home to Russia during the summer vacation of 1934 he was prevented from leaving the U.S.S.R. because his services were needed there. Dirac was greatly perturbed by this development, which affected him for the rest of his life (183). In the summer of 1935 he visited Kapitza in Moscow to give him moral support, and to advise the Royal Society and the University of Cambridge about ways of helping Kapitza to continue his productive research, in Russia. He visited Kapitza again in 1936 and 1937. After that it became inconvenient to obtain a Soviet visa.

Dirac's first visit to the United States was in 1929; after lecturing in the universities of Wisconsin and Michigan he crossed the Pacific in the company of Heisenberg, lectured in Japan, and returned on his own by the trans-Siberian railway.

He spent much of the academic year 1934-35 at the Institute for Advanced Study, Princeton, an institution to which he was to return many times. There a close friendship developed with Eugene Wigner, a professor at Princeton University, whom he had already met in Göttingen and elsewhere. He met Wigner's sister, Margit ('Manci'), who was visiting from Budapest, and in January 1937 they were married in London. She is in temperament quite unlike Paul; spontaneous and impulsive, with great warmth and with strong likes and dislikes.

Paul abandoned his bachelor quarters in St John's and they moved to a house in Cavendish Avenue, Cambridge, which remained their home until his retirement in 1969. They were joined by Manci's two children from her first marriage, Judith and Gabriel Andrew, who both adopted

the name Dirac. Gabriel later became a pure mathematician of distinction. He was Professor of Pure Mathematics in the University of Aarhus, Denmark, when he died a few months before Paul.

Paul and Mani had two daughters: Mary Elizabeth, born in 1940, is now Mrs P. Tilley. Florence Monica, born in 1942 is an Oxford B.A. and Cambridge Ph.D. in geophysics; she married the geophysicist R. L. Parker, and has a son and a daughter. The son, named Paul, has a strong likeness to his grandfather. Dirac's mother lived with the family at the end of her life and died there on 21 December 1941.

Although in the 1930s the quantum mechanics of atoms and systems of atoms was complete and well understood, in no small part due to the work of Dirac, the quantum theory of the electromagnetic field was still giving trouble. To many questions the theory gave infinite answers. Dirac was unhappy about these difficulties and made numerous attempts to eliminate them, but without success.

At the same time he continued working on new applications and new methods. In addition he produced two quite revolutionary ideas not directly connected with the search for an improved quantum electrodynamics.

One of these was the magnetic monopole. He showed that the equations of physics could consistently accommodate a magnetic pole, not previously regarded as possible, provided the product of its strength and the charge of the electron was an integral multiple of $hc/2$. An interesting implication of this result is not only that the pole strength of any magnetic pole would have to be a multiple of $hc/2e$, where e is the electron charge, but that if there exists a pole of strength $nhc/2e$, the charges of any particle would have to be multiples of e/n . This would account for the quantization of charge.

The other idea was what he later called the 'large-numbers hypothesis'. This hypothesis, first put forward in 1937 (50), starts from the belief that the laws of nature should not contain fundamental dimensionless constants of enormous magnitude, and that, where such numbers appear, they are not constant but related to the present age of the universe, which, measured in atomic units, is also a very large number.

Both these ideas attracted much attention and were discussed in many papers besides Dirac's own further work. On their reality there is as yet no final verdict; no certain experimental evidence for magnetic poles or for the variation in the planetary orbits predicted by Dirac has been found, though there are some positive indications.

WARTIME PREOCCUPATIONS

In 1933 Dirac started some experimental research. He had invented a method of isotope separation that consisted of forcing a stream of gas to follow a helical path. The heavier molecules, with their greater inertia,

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would tend to be on the outside of the rotating mass of gas and the lighter ones on the inside. In effect this is like a centrifuge without moving parts. Kapitza encouraged him to try out the method himself; a simple apparatus was made in Kapitza's workshop and a compressor made available to drive it. Dirac made some progress with the device but had not got far enough to establish that it could separate a gas mixture. He did notice a marked difference in temperature between the two emerging fractions, which he attributed to the effect of viscosity. (A more likely explanation is that the rotational motion also separates the faster from the slower molecules.) The experiment was abandoned when Kapitza was detained in the Soviet Union, as Dirac did not feel like continuing on his own.

Dirac's experiment was remembered, however, when during the war isotope separation became an urgent problem for the atomic energy programme. Dirac visited F. E. Simon's group at Oxford early in 1941, and their discussions led Dirac to propose several simple designs for an isotope separator that involved forcing a gas stream to turn a corner. A team in Oxford set up an apparatus to one of his designs and showed that it did indeed separate isotopes. They concluded, however, that its performance could not compete economically with gaseous diffusion. Dirac took a very close interest in this work, which went on until 1945, and in visits to Oxford and in letters made numerous practical suggestions and comments.

This was not his only connection with atomic energy problems. He became an informal consultant to the theoretical group in Birmingham, and looked at a number of problems of interest to them. One of these was discussed in a report called 'Theory of the separation of isotopes by statistical methods' (60). Here he introduced the concepts of 'separative energy' and 'separative power', which give a measure of the minimum effort required to obtain a given amount of separated isotope, and the contribution made to this by a particular device. These quantities, which are helpful in discussing plant design, are now used widely. They are quoted, for example, in Karl Cohen's book (1951) and in the recent review by Whitley (1984).

Another note (63) concerned isotope separation in a self-fractionating centrifuge. This concerns a centrifuge in the form of a long cylinder spinning about its axis, in which gas is made to flow axially close to the wall, and in the opposite direction closer to the axis. This 'counterflow' arrangement makes one such centrifuge the equivalent of many stages. Dirac's paper shows that it is possible to maintain a stable flow in such a machine, and the very successful uranium separation plant now operated by URENCO, a British-Dutch-West German consortium, does follow the principles investigated by Dirac. His calculations were probably done in 1941, but we have been unable to trace what precisely motivated them.

A study made for the Birmingham group proposed and evaluated a method to determine the critical size of a mass of ^{235}U of non-spherical shape (68, 69). Several reports (64, 65, 66) were concerned with an approximate method to determine the criticality and the explosive yield of a sphere of ^{235}U allowing for conditions varying with the distance from the centre, because the U sphere is surrounded by a reflector, or because of the incipient expansion. The contributions mentioned above are discussed in more detail elsewhere (Dalitz 1986).

Other proposals connected with the war did not lead anywhere. There was a suggestion, put to Dirac by J. G. Crowther, who was then head of the Science Department of the British Council, that Dirac accompany the British Ambassador to Moscow in early 1943 to make contact with Soviet scientists. Dirac was interested, but in the end did not go. We do not know whether the reason was Dirac's contact with the secret atomic energy work, or whether the Soviet authorities did not approve.

During the war there was little opportunity for foreign travel, but Dirac paid several visits to the Dublin Institute for Advanced Study, where Schrödinger was his host.

In 1945, after the end of the war in Europe, the Soviet Academy of Sciences celebrated an anniversary, to which they invited many foreign scientists, including Dirac. He would have liked to go, but was not allowed to travel. The government stopped all those scientists who had been connected with atomic energy work, to prevent any leakage of information to the U.S.S.R. They also stopped a number of other scientists to make the reason for the ban less obvious. It is not clear whether Dirac was regarded as belonging to one or the other group.

With all these preoccupations his rate of publication slowed up somewhat during the war, but he continued working at the unsolved problems. He also continued teaching. Because of the wartime shortage of staff he was called upon to take part in some first-year undergraduate examinations, a task he carried out dutifully but not too happily. Lady Jeffreys remembers that when the examiners were arguing about a percentage point or two on some candidates' marks Dirac asked with surprise 'Can you examine to that accuracy?'

Staff shortage during the war years led also to pressure to take on the supervision of research students. Previously Dirac had been reluctant to become involved in this responsibility, though he was always kind to students who came to him with questions. Now he became the supervisor of a few students. This continued for some years after the war, after the teachers had returned, because the number of research students had increased very substantially.

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RESEARCH STUDENTS

In general, Dirac did not seek to take on research students, for he did not like to take responsibility for the success of their research projects. Nor did he seek to find collaborators in his own research, although such collaboration did come about on occasion, in some natural way. For example, in 1932 he published some work (35) on calculation of photoelectric cross sections, a topic of interest to experimenters in the Cavendish Laboratory, carried out jointly with J. W. Harding, who was officially a student of Rutherford. At about the same time, H. R. Hulme, a student of R. H. Fowler, calculated internal pair conversion rates and benefited greatly from the advice and guidance of Dirac, whom he considered to be effectively his supervisor.

When Fowler went abroad on sabbatical leave in 1930-31 he requested Dirac to take care of his student, S. Chandrasekhar, newly arrived from India. No formal arrangement was made, for the University records show Fowler as his supervisor all through this period. Chandrasekhar recalls that Dirac was very helpful to him. They met in Dirac's room about once each term but they also met frequently elsewhere, at tea, in the library or on the street, where they discussed the progress of Chandrasekhar's work. Dirac always read thoroughly the papers Chandrasekhar proposed to submit for publication, giving useful comments. Chandrasekhar was concerned with the theory of white dwarf stars, at that time; Dirac expressed considerable interest in the research but told Chandrasekhar that if he were to become interested in astrophysics, he would prefer to work on general relativity and cosmology. Dirac continued to act like a supervisor towards Chandrasekhar for long after Fowler's absence abroad, and it was he who proposed and arranged for Chandrasekhar to work for a time at Copenhagen after he had taken his Ph.D. degree.

Dirac's first formal responsibility for the supervision of research students came in 1935-36, when Andrew Lees and Paul Weiss were passed on to him from Max Born, who was absent from Cambridge for two terms in that academic year and then took up his Chair at Edinburgh in September 1936. Both of them completed their Ph.D. theses under Dirac's supervision.

He took no further students until war time, when C. J. Eliezer came to Cambridge from Ceylon (Sri Lanka) in 1941. Eliezer (1987) has described how Dirac guided and encouraged his work, which was about Dirac's classical equation for an electron interacting with its own radiation field. He took his Ph.D. in 1946. In January 1945 Sonja Ashauer from Brazil and S. Shanmugadhasan from Ceylon came under Dirac's supervision. Miss Ashauer worked on further aspects of the same topic, and Shanmugadhasan on the electrodynamics of spinning particles, and both completed their theses in good time. According to Shanmugadhasan's account (Shanmugadhasan 1987) he worked largely on his own and had

to take the initiative in seeking out Dirac when he wanted to talk to him, but he received all the advice and encouragement he needed. He says 'Despite his sink-or-swim attitude towards his students, I firmly believe that Dirac was the best kind of supervisor to have.' In Michaelmas term 1945 Harish-Chandra arrived from India, where he had worked with H. J. Bhabha at Bombay. He venerated Dirac but became persuaded by his experience at Cambridge that he was not suited to theoretical physics. As to his reason for abandoning physics, he mentioned a conversation with Dirac in which he said that he had discovered a lack of rigour in Dirac's work on the Lorentz group. Dirac replied 'I am not interested in proofs but only in what nature does.' Harish-Chandra added 'This remark confirmed my growing conviction that I did not have the mysterious sixth sense which one needs in order to succeed in physics, and I soon decided to move over to mathematics.' However, Dirac did suggest the topic of his thesis, the study and classification of the irreducible infinite-dimensional representations of the Lorentz group, a topic that had been opened up by earlier work of Dirac (70). This subject led Harish-Chandra naturally into the area of mathematics where he flourished and that he made his own in later years, the study of the infinite-dimensional representations of semi-simple Lie groups. Thus, in the immediate postwar period, Dirac had formal responsibility for the research work of four students, all of whom completed their Ph.D. theses satisfactorily.

Later postwar students who were supervised in their research work by Dirac for two or more years were R. J. Eden (1948-50), who worked on what is now called constrained dynamics, H. J. D. Cole (1948-50), S. F. B. Tyabji (1951-54), an Indian with a B.A. (Bombay) in mathematics who had come back from a career in law, and B. McCormick (January 1951-September 1953) from Harvard, but they did not all achieve their goals at Cambridge. Many other students spent three or four terms under Dirac's supervision in this period, D. W. Sciama (1952-53), M. Cini (1951-52) from Italy, and P. A. D. De Maine (1955-56), a South African who had come to Cambridge from Canada to study electromagnetic theory and who held already a research degree in experimental chemistry. The last two had to cut short their studies at Cambridge for financial reasons. J. E. Roberts (1962-63) spent two terms under Dirac before moving on to another supervisor and to later studies abroad.

Probably the only student who came to research under Dirac by invitation was R. J. N. Phillips, who did so in 1954 and who has recently related the circumstances (Phillips 1987) at the Memorial Meeting held for Dirac at Cambridge in April 1985. His happy supervision by Dirac came to an end after two terms when Dirac left Cambridge on his 1954-55 sabbatical leave.

In general Dirac was discouraging in his initial contacts with prospective research students. Mott's verdict (Mott 1986) was 'Dirac

is unapproachable and he spends so much time abroad'. In fact, the Degree Committee sent many students to approach Dirac. M. H. L. Pryce was told 'Thank you very much, I do not think I need any help at the moment'. Shanmugadhasan (1987) has related how Dirac suggested to Miss Ashauer in 1945 that 'we should go to see A. H. Wilson, for I think he will have a suitable problem'; she did not comprehend his meaning and continued to question Dirac, who then made the best of the situation and accepted her as his research student. In 1936 H. C. Corben met with a cold response in his first and only interview with Dirac: 'Would internal pair creation be a suitable area for some calculations?' 'Yes', came the reply. 'Have there already been calculations on these processes?' 'No', came the reply, and nothing further came, until Corben decided that it would be best to leave quietly and to seek another supervisor. In 1950 A. C. Hurley lasted a term before he decided to find a successful career in chemical physics under J. Lennard-Jones.

The total number of students successfully supervised by Dirac was not at all negligible, more than a dozen if the early informal supervisions are included. These students generally considered him a good supervisor, making suggestions sparingly and only when they were needed, but giving adequate guidance. Of course, many other students, more than we can name here, approached Dirac with questions arising out of the lectures or from their own research, and he treated these questions gently and seriously. It was, however, high praise when he said to Sciama (not yet his student), after reading a paper that Sciama wished to submit for publication: 'Thank you for showing me your paper. It was more interesting than I expected.' Similarly, when M. H. L. Pryce returned to Cambridge in 1936, after two years at the Institute of Advanced Studies at Princeton where he had been working on questions closely related with Dirac's current work, he gave his first seminar at Cambridge on this work with some trepidation. It was one of the high points of his life when, at the end of his seminar, Dirac came over to him to say 'May I please communicate your paper to the Royal Society?'

FAR FROM THE MADDING CROWD

In his own research Dirac continued to follow his own ideas, not joining the mainstream of theoretical research and not afraid of holding minority opinions. His work in the immediate postwar years was dominated by many determined attempts to rid quantum electrodynamics of the infinities. Even when the work of Schwinger, Feynman and Dyson showed how to obtain finite answers consistently from the 'renormalized' theory, and these answers agreed with experiment to an impressive accuracy, he refused to regard the theory as satisfactory. He never changed this view.

